The 'Goal-Corrected Partnership' in Attachment Theory: A Critical Assessment of the Research Programme

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Attachment Theory may be the largest research programme in human development. But the contours of the theory remain quite difficult to define. Indeed, a pressing requirement to clarify what the ‘theory’ actually contends remains. This thesis is an interdisciplinary project that brings the dual lens of history and philosophy to shed new light on the theory.

John Bowlby’s (1907-1990) goals and methods for Attachment Theory were both radical and innovative. First, he set out to ‘radically’ overhaul the entire edifice of psychoanalysis—what he would call Freud’s original metapsychology. Second, Bowlby combined three fields in a manner that anticipated today’s more integrative non-dualist, non-reductive approaches to the human mind: (1) Tinbergen’s four questions in behavioural biology, (2) questions in emotion research, and (3) a range of concepts from the cognitive sciences, especially Craik’s concept of mental models. This integrative approach marks a philosophical commitment to naturalism.

I distinguish 13 attachment constructs—the initial 12 allocated across the Tinbergen framework. This provides a helpful approach for clarifying the inherent complexity of the theory. Indeed, an historical picture beginning in the 1950s suggests how complexity has been increasingly embraced. A 13th construct—the organisational perspective—provides a plank for tying attachment to philosophical insights from Developmental Systems Theory (DST).

Bowlby’s Goal-Corrected Partnership (GCP) proposed that attachment relationships develop beyond infancy and require engagement with newly emerging cognitive skills. The thesis argues that the GCP offers an important corrective to conceptualisations that somehow narrowly conceive attachment phenomena as a purely implicit, infancy derived, affectively triggered ‘protection’ function.

Attachment Theory makes possible a reintroduction of a developmental perspective into psychiatry. Its causal credentials could also provide a breath of fresh air for a mental health arena dominated by symptoms, signs and their removal. Finally, an acknowledgement of GCP relationships, matches the growing empirical awareness that behaviour, emotion and cognition may be more integrated phenomena than current studies allow.
Acknowledgement

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Indeed, this doctoral pursuit actually stretches back some thirty plus years. Formal studies to obtain a PhD actually began in 1986 at the Katholieke Universiteit te Leuven (KUL) in Louvain, Belgium. After successfully sitting the two required degrees by 1989, initial full acceptance as a PhD candidate came in 1990. Fast forward two years: it was upon leaving Oxford University in 1992 with yet another (unmarketable) degree, that I promised myself I’d nonetheless return to academics for that elusive PhD: consciously suggesting, “next time in philosophy!” It has been a curious and unexpected journey to Sydney.

Finally, I would like to dedicate this work to my dear late friend, Stephen Happel who first encouraged me to venture to Belgium and engage the academic world. He more than anyone
helped me to see that the human intellect can be employed both to understand and to relieve human suffering. Again, less marketable, but potentially well suited for a ‘good life’!

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The ‘Goal-Corrected Partnership’ in Attachment Theory: A Critical Assessment of the Research Programme

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CHAPTER ONE: Thesis Introduction

All theories have vulnerabilities—incoherence in propositions, illogical deductions, predictions that are not distinctive, and the like. But a very successful theory faces two particular and related hazards. These are overreaching, trying to explain everything and the belief that the theory is the total explanation for any phenomenon (Sroufe, 2016, p. 997).

1.1 Thesis Précis

Attachment Theory may arguably be the largest research programme in field of human development. Despite this massive success—especially expansion over the past 10 years—the contours of the theory remain quite difficult to define. The theory demonstrates two characteristics that may sit in tension with one another. On the one hand, Gillath and colleagues (2016, loc. 930) have described the research endeavour as a unified ‘Grand Theory’, in part as it seeks to address each of Tinbergen’s (1963) four questions for understanding biological behaviour. On the other hand, Thompson (2016, p. 330) has also acknowledged divergence within the enterprise, suggesting the presence of potentially competing mini-theories. The consequence of this tension is a difficulty in establishing the core tenets for what is an undeniably complex theory—a fact not fully appreciated across the field. Indeed, the initial hypothesised question of my research—‘what might appropriately count as an ‘attachment-informed’ psychotherapy?’—, quickly gave way to a more pressing requirement to clarify what the ‘theory’ actually contends. The outcome of that shift is this thesis: an interdisciplinary project that brings the dual lens of history and philosophy to shed light on a theory that holds great promise for both psychiatry and psychotherapy.

John Bowlby’s (1907-1990) goals and methods for Attachment Theory were both radical and innovative. First, he set out to ‘radically’ overhaul the entire edifice of psychoanalysis—what he would call Freud’s original metapsychology. In doing so, Bowlby sought to resurrect Freud’s abandoned 1896 Project for a Scientific Psychoanalysis: to reinvigorate the field by setting it into an empirical framework. Indeed, the presence of a thriving field, psychoanalytical developmental research (PDR) focussed on early infant development, is testimony to the partial success of his programme. Perhaps less clearly understood has been the innovative interdisciplinary method he developed. Bowlby combined three fields in a manner that anticipated today’s more integrative non-dualist, non-reductive approaches to the human mind: (1) Tinbergen’s four questions in behavioural biology—phylogeny, ontogeny, function and mechanism—, (2) questions in emotion research—calling especially on Langer’s acknowledgement of unresolved empirical issues in gaining explanatory access to felt processes in organisms, and (3) a range of concepts from the cognitive sciences, especially Craik’s early ideas for mental models. I have described this approach as his TLC (Tinbergen+Langer+Craik) strategy, a philosophical commitment to naturalism.

Capturing the breadth of the contents of Attachment Theory will probably always be an unfinished, if not unsatisfying task. There is always more to tell. Nonetheless, philosophy and
history provide two tools for tackling the challenge. A critical identification of 13 attachment constructs—the initial 12 allocated across the Tinbergen framework—provides a helpful approach for clarifying the inherent complexity of the attachment field. Indeed, an historical picture suggests how, as the theory matured from its early days in the 1950s up to Bowlby’s more comprehensive three volume Attachment series by the early 1980s, complexity was increasingly embraced. For example, a thirteenth construct—the organisational perspective (Sroufe & Waters, 1977)—provides a plank for tying attachment conceptions with philosophical insights on complexity in Developmental Systems Theory (DST). Indeed, an individual’s global attachment regulatory capacity can be more robustly considered as a Dynamic Lifespan Personality Construct (DLPC).

Bowlby’s Goal-Corrected Partnership (GCP) proposed that attachment relationships beyond infancy required engagement with newly emerging cognitive skills, especially the capacity for turn taking. With the eventual advent of measures spanning the full lifespan, the GCP position has taken on a received status in the field. However, the notion is paradoxically under-valued, and, indeed, not acknowledged by a few key researchers. The thesis argues that the GCP offers an important corrective to conceptualisations that somehow conceive attachment phenomena as a purely implicit, infancy derived, affectively triggered ‘protection’ function. Indeed, the GCP might better be conceived as part of an ongoing dynamic lifespan development. Several arguments are offered in support of this expanded perspective for a GCP. Most notable is the analogous application of Homological discussions in emotion research on discrete emotions. The engagement of cognitive capacities within attachment may have emerged in our evolutionary history, including the emergence of adult pair-bonds. But the emergence of an individual’s specific GCPs will also be shaped by unique interpersonal and cultural development.

Three implications of Attachment Theory for both psychiatry and psychotherapy can be drawn. First, Attachment Theory offers the possibility for a reintroduction of a developmental perspective into psychiatry—something missing since the shift to the DSM-III. But this time we would possess an empirically driven lifespan perceptive. Next, the causal credentials emerging in the attachment field, could provide a breath of fresh air for a mental health arena dominated by symptoms, signs and their removal. Third, the increasing role of complexity in attachment development, especially in attachment GCP relationships, matches the growing empirical awareness that behaviour, emotion and cognition may be more integrated phenomena than current studies allow. To conclude, we currently lack an adequately articulated attachment-informed psychotherapy. However, many of the components—phylogenetic, ontogenetic, functional and mechanistic—have arguably been identified.

1.2 Why this Thesis?

Why this particular thesis on attachment? Permit me to begin making my case for a philosophical thesis on Attachment Theory with a few related anecdotes. I have been a
practicing counsellor/psychotherapist for the past 11 years with a particular interest in longer-term psychodynamic work. I began PhD studies in the beginning of 2010. Since then, perhaps every six months or so, I seem to have had ‘strange encounters’ with ideas purported to represent Attachment Theory. The source of these viewpoints could be a visiting overseas developmental researcher, or a colleague sharing notions discovered while attending a psychotherapy conference, or perhaps someone who had read a new neuroscience book covering attachments. But perhaps the most striking encounter has come not from fellow professionals but clients, and the source is not academia or clinical reflection but the popular, slightly sentimental film Good Will Hunting. ‘Attachment injury’ would appear to be relieved via dramatic therapeutic change: cathartic insight facilitated by a targeted boundary violation? Perhaps an enthusiastic film audience can be excused if they might succumb to Sroufe’s identified risks of ‘overreaching’ and ‘explaining everything’ via a single theory. But amongst professionals, those risks are compounded by two additional sources of confusion: (1) not everyone agrees on the contents of the theory, and (2) nor do people agree on how it might impact the practice of psychotherapy. Unfortunately, without minimal agreement on the former, implementing the latter may become potentially more problematic and the risks identified by Sroufe may be more easily imported into clinical settings.

Early hopes for a thesis-driven systematic review of an attachment-informed psychotherapy—a contribution to today’s relatively sparse philosophy of psychotherapy—, soon gave way to an even more daunting task: providing a philosophical assessment of Attachment Theory suitable for the some future but now delayed clinical review. My initial curiosity in 2009 centred on “Why had it taken so long for such a successful theory to gain general acceptance?” By 2017, that inquisitiveness had given way to near marvel at the exponential growth of the field. Along the way, general acceptance of the theory appears to have been gained. So I may need to repose the question, “Why a conceptual thesis about Attachment Theory?”

Perhaps the primary reason relates to the success of Attachment Theory and its promise for a more causally based mechanistic approach to mental health. First of all, the field ranks amongst the most prolific research projects in the adjacent fields of human development and clinical application. As Cassidy and Shaver note in the preface to the most recent third edition

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1 I come to Attachment Theory as both an aspiring philosopher and a mental health practitioner: counsellor, psychotherapist, supervisor and professional development educator. Before commencing PhD studies, I also successfully completed a year of psychodynamic training in the ‘conversational model’ (Meares, 2005) with the Australian New Zealand Association for Psychotherapy (ANZAP). The latter group includes psychiatrists, psychologists and counsellors. Although I do not formally practice in the ANZAP model, the training has nonetheless greatly informed my practice. So this is an informal acknowledgment of any therapeutic conflicts. I have also for the last 5 years provided professional development seminars on Attachment Theory for practitioners seeking continued education credits (psychiatrists, psychologists, psychotherapists, counsellors, social and community workers).

2 The film was released in 1997, directed by Gus Van Sant and starred Robin Williams as the therapist. The screenplay was written directly for the film by co-stars Matt Damon and Ben Affleck. (http://www.imdb.com/title/tt0119217/ accessed 21/12/16.) All three won Oscars, the latter two for screenplay. The film is now listed on-line at http://www.teachwithmovies.org/ as a suggested teaching resource for Attachment Theory, complete with a reasonably written explanatory guide. However, Robin Williams’ portrayal has also been described as an example of an unhelpful therapist stereotype in cinema—i.e. the “Motivating and Well-Intentioned” type—in a more scholarly clinical discussion of cinema and psychotherapy (Wedding & Niemiec, 2003, p. 209).
of the *Handbook of Attachment*, “Anyone who conducts a literature search on the topic of ‘attachment’ will turn up more than 30,000 entries that have appeared since the beginning of 1975 (three times the number we discovered when preparing the 2008 second edition of this volume)” (2016, loc. 149). That is approximately a 200% increase in total publications between editions in just a quarter of the time wherein the initial 10,000 were published. This period between editions also corresponds roughly to the period of my part-time research. As Sroufe has noted, success brings its own challenges. Indeed, I have described the task of assessing Attachment Theory as akin to jumping on a speeding driverless train.

What may be lacking in the midst of the theory's growth is a robust framework in which to ground the research edifice. The encounters with key philosophical questions readily engaged by Bowlby (1969/1982) would appear to have receded. Cassidy and Shaver (2016) divide the current *Handbook* into seven useful parts: (1) an overview, (2) biological perspectives, (3-4) two lifespan parts—infancy and childhood, adolescence and adulthood—, (5) psychopathology and clinical application, (6) systems, culture and context, (7) and a newly added part on ‘perspectives’. The organising principles no doubt reflect the history of the *Handbook* and appear to represent a mixture of theory, lifespan, risk, treatment and proximal/distal influences. However, critical philosophical questions implicit for the theory are less directly tackled: namely, notions such as causation; a host of dichotomies, including mind-body relations, emotion-cognition, nature-nurture perspectives; levels of analysis; and the types of integration questions raised by developmental systems theory (DST) and complexity science. Just as philosophy may assist a related discipline like psychiatry (Fulford et al., 2013), so too it may assist attachment study. One of the potentially more interesting philosophical threads in Attachment Theory is an emerging picture of developmental causation across the lifespan. Such a thread fits well within the relatively original framework proposed here: namely Bowlby’s commitment to what would now be termed *philosophical naturalism*.

### 1.3 An Interdisciplinary Project

Assessing the state of such an enormous research tradition carries with it both unique challenges and risks. There is an inevitable strain between breadth and depth of analysis—an increasingly recognised concern across academia in the face of the rise of interdisciplinarity (Frodeman, Klein, Mitchame, & Holbrook, 2010). On the one hand, engaging the theory requires a certain degree of comfort with a potentially overwhelming amount of information. Indeed, Attachment Theory has been with us for nearly seven decades now. No one individual carries the sort of comprehensive perspective sustained so extraordinarily by Bowlby. The theory has also been a rather remarkable *interdisciplinary* success. Familiarity with a potential multitude of disciplines can be truly daunting. Likewise, Attachment Theory represents one of the first serious efforts at constructing a *lifespan* perspective. Once again our understanding of the sheer diversity in human development is growing increasingly more
complex. So capturing a more general picture of the attachment landscape requires a degree of comfort with deciding when to go further and when to stop. The decisions are hardly obvious in places. On the other hand, such a broad approach may inevitably miss out something valuable. The degree of focussed granularity typical for most theses cannot be sustained with an interdisciplinary topic like attachment. Nonetheless, a degree of deeper exploration of the conceptual terrain is required, lest the thesis become a mere survey or summary. I will also note below my interest in a relatively broad audience, something that has also required a degree of depth across a few disciplines. The challenge has been to hold the tension between breadth and depth, whilst engaging as wide an audience as possible.

I study in the Unit for History and Philosophy of Science, in the Faculty of Science. Much of the research in the unit tends to naturally fall into either a historical or a philosophical format. My approach is probably best described as a hybrid project. Although predominately philosophical, this thesis has also sought to combine the two, offering both a philosophical and historical assessment of Attachment Theory.\(^2\) I would argue that it is hardly possible to adequately grasp the theory without some grounding in its historical evolution. I would suggest that this combination also supports the task for both assessment and identification of a more coherent framework. On the one hand, there are novel insights emerging from the combined approach but the thesis does not represent a completely new project. On the other hand, a partial reorientation of Attachment Theory is also offered, but again not something indiscernible from Bowlby’s original ideas.

### 1.4 Introducing the Thesis Three Part Structure

The thesis position is structured in seven chapters allocated across three parts, followed by a general conclusion. **PART ONE: Identification of Bowlby’s Original Goals and Proposed Methods** consists of two chapters. Together they provide a clear picture of Bowlby’s ambitious and innovative attachment project. **CHAPTER TWO: Bowlby’s Proposed Radical Overhaul of Freud’s Metapsychology** sets outs Bowlby’s hopes for an empirically driven alternative to traditional psychoanalysis. After a careful unpacking of the more classical psychoanalytic notion(s) of metapsychology, Bowlby’s five core tenets for renovation are identified. Evidence of his partial success is described in terms of the emerging field of psychoanalytic development research (PDR)—an empirical endeavour allied with attachment infancy research. Perhaps the most interesting contribution made by Bowlby has been his

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\(^2\) I suspect my attraction to history and multiple disciplines may also reflect my past engagement with studies in the 1980s at the Flemish speaking Katholieke Universiteit te Leuven (KUL) in Louvain Belgium and early 1990s at Oxford University. First, history of philosophy has typically been a key component in the curricula for Continental philosophy (Gutting, 2011). Indeed, I am grateful to Paul Griffiths for his words of support and observation of how Canguilhem’s (1989) *The Normal and the Pathological* offered a typical Continental approach in philosophy of science that combined history and philosophy of psychiatry. In addition to studying philosophy in Belgium, the curriculum I completed for gaining the transitional pre-PhD license degree (STL— an intense two year combined coursework and thesis degree) to teach graduate studies included four disciplines: history, biblical studies, philosophical theology and ethics. My studies at Oxford in the Oriental Faculty— Jewish Studies in the Graeco-Roman Period—followed a classics approach to ancient texts with strong emphasis on historiography. Both locales required direct study of several ancient languages and competence in major European languages. So, the breath of attachment has seemed inviting.
contribution to empirical method in developmental study. CHAPTER THREE: Bowlby’s Approach: Tinbergen Meets Langer and Craik identifies how Bowlby turned first to ethology for a model for studying human behaviour. The chapter also indicates how an implicit employment of Tinbergen’s quadripartite approach in ethology was further integrated with questions raised in both philosophy of emotion—particularly in the later work of Suzanne Langer—and the cognitive sciences—Craik’s mechanistic notion of mental models. I have coined this Bowlby’s TLC strategy. The position is framed within philosophical discussions of naturalism.

The thesis shifts perspective in the next part, moving to a more formal assessment of the theory. PART TWO: A Philosophical and Historical Assessment of Attachment Theory proceeds in three steps: one analytical/philosophical, the second historical and the third conceptually creative. An assessment of the content of attachment research is conducted through a reapplication of the Tinbergen framework. CHAPTER FOUR: Identification of Constructs of Attachment Theory differentiates thirteen possible constructs and surveys current knowledge by construct. A series of hypotheses and unanswered questions are also considered. Step two seeks to situate today’s theory in an historical picture, capturing important theoretical evolution. CHAPTER FIVE: Understanding the Conceptual Evolution of Attachment Theory assesses the relative state of maturity of the theory. The chapter traces the emergence of Attachment Theory across four periods from Bowlby’s earliest clinical experience through today’s current period of expansive growth. A final third step, takes up the thirteenth construct—an organisational perspective—providing several possible expansions. CHAPTER SIX: The Attachment Organisation Perspective: DLPC, DST and Implications for Psychiatry introduces a re-conceptualisation of both attachment’s global and lifespan dimensions. A Dynamic Lifespan Personality Construct (DLPC) is proposed, suggesting a possible integration of the attachment global regulatory capacity and unique attachment style into personality psychology. The lifespan developmental dimension of attachment is also analysed in terms of Developmental Systems Theory (DST). Finally implications for psychiatry are drawn, with a particular focus on possible causal contributions.

The task or conceptual rearticulation is extended to adult romantic relationships in the final part of the thesis. PART THREE - A Rearticulation of Attachment Theory: The Goal-Corrected Partnership Revisited offers a deeper look at the theory’s most recognisable construct, the attachment relationship. CHAPTER SEVEN: The Goal-Corrected Partnership across the Lifespan explores the employment of the GCP across the field and argues for its received status in the theory. However, note is drawn to cases where influential researchers have failed to take up the concept. Particular focus is given to describing possible relationships between Phase III pre-linguistic and the more cognitive influenced Phase IV capacities. The chapter also suggests how the GCP might better be seen more dynamically in terms of lifespan development, especially for adult romantic relationships. A possible justification for an
expanded GCP is taken up next in following chapter, arguing for a constitutive role for cognitive capacities in attachment. **CHAPTER EIGHT: Grounds for Updating Bowlby’s Cognitively Enhanced Lifespan GCP** provides a reapplication of his TLC strategy. An analogy is developed from the employment of phylogenetic and developmental homologies in discussions on discrete emotions, arguing for a more unified dynamic lifespan developmental depiction of Phase III and IV. These cease to be considered as ‘one off’ discussions associated only with the development of first attachments, but can be understood to be applicable across all attachments. Finally, interesting cognitive research on security priming in adult attachments provides the final piece of evidence for concluding a more lifespan considered GCP.

1.5 For Whom? The Proposed Audience and Suggestions for Reading

As one might expect with an assessment of Attachment Theory, the thesis is an interdisciplinary project and thus should be suitable for a broad audience. At least four general groups can be identified. I would envision the thesis to be of interest to **philosophers** working within multiple areas of philosophy: including philosophies of biology, development, mind, emotion and psychiatry. Equally, **researchers** from a broad set of research fields and disciplines—psychiatry, psychotherapy, developmental psychology, personality and social psychology—might find this exercise helpful. A third group would be clinical practitioners of various backgrounds and working with both children and adults. Indeed, participants in the field of contemporary psychoanalysis, broadly defined—a group that has traditional coupled theoretical and clinical discussion—, might also find this discussion valuable. Finally, historians of psychiatry and psychology might also find this interesting. Nonetheless, as an interdisciplinary project in such an increasingly large field as Attachment Theory, a few inherent challenges exist. Engaging a significant volume of relevant material might benefit the provision of a few roadmaps that might consider differences in the audience.

First, the volume challenge! A few comments on my writing style—a partial reflection of the scale of research conducted—and a few possible tips for approaching the thesis might be in order. Hence, note should be made of the breadth and depth of referencing throughout the thesis. Knowing the historical controversies that have surrounded the theory, I have erred on the side of caution in providing thorough support for most points. There are also many complex issues that require space for adequate consideration. Similarly, mention should be made of the employment of footnotes, some occasionally quite involved. Both the reference breadth and footnote details reflect in part the seven years devoted to the project. There literally is an immense mountain of material to be engaged. Valuable publications in the field over 2015 and 2016 have also emerged at a truly ‘fast and furious’ pace. Setting and retaining adequate boundaries in this thesis has been challenging with such a vast and changing landscape. Hopefully, the judicious employment of footnotes has also helped to manage some of that volume. However, managing this material is the task of the thesis. Indeed, establishing a thorough presentation for the contents for Chapter Four has proven particularly
challenging. The construct identification in Chapter Four provides an original response to engaging the theories weight and complexity. As such it offers a sort of foundation from which future discussion might be continued. As stated above, with Attachment Theory there is always more to tell!  

Next, having acknowledged both an extensive employment of footnoting in several places and an intentionally thorough—though hardly comprehensive—assembly of references, a few tips for reading may be in order. First, I might suggest three possible ways for approaching sections of the thesis or particular chapters: (1) a full engagement with text and all footnotes, (2) a predominant read of the main text without footnotes, and (3) a more directed journey through selected material, where the main points can be engaged without a dip into extensive detail. The choices here—especially between one and two—will partly depend on a reader’s degree of familiarity and interest with a particular topic and can probably be arrived at pretty quickly.

However, permit me to make a few specific suggestions for engaging three specific chapters where a more directed journey might be more judicious: namely, Chapters Two, Three and Four. These suggestions might be described as user-friendly road maps for safely traversing interdisciplinarity across the thesis. (1) For readers less familiar with Attachment Theory, an initial read of Chapter Five might be an appropriate starting point, before commencing the thesis order. When arriving at Chapter Four, the longest and most detailed in the thesis, Sections 4.1 and 4.2 provide a solid introduction and 4.7 will prepare for later chapters. Discussions of individual constructs in Sections 4.3-4.6 might also be considered in terms of interest in Phylogeny, Ontogeny, Function or Mechanism. However, Section 4.4’s look at ontogenetic constructs is most crucial for the remainder of the thesis. (2) For those particularly interested in attachment and psychoanalysis—past and current—Chapter Two should provide a quite thorough discussion. However, for those with less interest or familiarity in what can be less accessible Freudian details, a focus on Section 2.3 as well as subsections 2.4.1 and 2.4.3 might be recommended. These will provide an adequate picture of Bowlby’s goals and historical success within psychoanalysis. Finally, for those with particular interest in philosophy of biology and ethology, Chapter Three offers an in-depth look at Bowlby’s turn to ethology and philosophical naturalism. A more focused read of Sections 3.5 and 3.6 will provide a clear picture of Bowlby’s innovative TLC methodology. This framework will be reemployed throughout the remainder of the thesis. Finally, Chapters Five through Nine should be relatively accessible to all audiences.

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4 Indeed, in consultation with my supervisors, my word count has focused on the actual text of the thesis and has not included reference lists and footnotes.
5 Where possible, I have endeavoured to keep chapters current with through 2016.
6 Obviously the Introductions and Conclusions for each of the three chapters are required.
CHAPTER ONE REFERENCES


PART ONE: Identification of Bowlby’s Original Goals and Proposed Methods
CHAPTER TWO: Bowlby’s Proposed Radical Overhaul of Freud’s Metapsychology

Freud posed the following question: how, in the course of the analysis, can an instinct be “tamed” ... “It is not easy to find an answer”, he wrote “We can only say: ‘So muss denn doch die Hexe dran!’”—the Witch Metapsychology. Without metapsychological speculation and theorizing—I had almost said ‘phantasying’—we shall not get another step forward. Unfortunately, here as elsewhere, what our Witch reveals is neither very clear nor very detailed” (Perron, 2005, p. 1868).

[Attachment Theory] is designed . . . to offer an alternative to the traditional metapsychology of psychoanalysis and to add yet another to the many variants of the clinical theory now extant. How successful these ideals will prove only time will tell (Bowlby, 1988, pp. 25-26).

2.0 Chapter Introduction

This chapter—the first of two—launches Part One’s identification of Bowlby’s aspirations for psychoanalysis. Bowlby’s ‘Radical Overhaul of Freud’s Metapsychology’ will be articulated in a manner that captures his ‘ideals’ for psychoanalysis noted in the epigraph above. Bowlby’s Attachment Theory emerged ultimately as an ambitious reform programme for psychoanalysis. The focus was predominantly ‘classical’ psychoanalysis (see Section 2.1). However, a review of the place of Bowlby’s programme within more ‘contemporary’ psychoanalytic practice (see Section 2.4) will also provide initial evidence for possible ‘success’ for his alternative metapsychology some 25 years later. As indicated in the thesis introduction, I have intentionally retained Freudian terminology. I must now demonstrate the method in such a potential distraction, especially as regards the notion metapsychology.

Several specific proposals for change will be proposed in this chapter that capture Bowlby’s aspirations, matters he would come to describe as ‘radical’.8 These modifications may be considered radical from at least two perspectives: scale and method. The initial one, the focus of this chapter, reflects the immense scope of Bowlby’s proposal. He sought to alter nearly the entire conceptual foundations of Freud’s theoretical edifice of psychoanalysis. Unlike most other psychoanalysts of his day, Bowlby also offered no pretence for sustaining unhelpful continuities with the entirety of Freud’s sometime convoluted work. Nonetheless, Bowlby (1991) considered his proposed changes to be natural extensions of Freud’s much discussed scientific project for psychoanalysis. The second

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7 The English translation, “We must call the Witch to our help after all!” is provided in the footnote of Analysis, Terminable and Interminable [1938 Standard Edition Volume 23]. The German phrase is actually a quote from Goethe’s Faust and appears first in the original 1937 German article, Die endliche und unendliche Analyse. Freud chose to retain the German witch [die Hexe] metapsychology in the initial English version (Perron, 2005). However, a parallel translation of the same article published in the International Journal of Psycho-Analysis places the German phrase in the footnote and renders the German term Hexe as ‘magic’ (S. Freud, 1937, pp. 380-381).

8 Bowlby regularly employed the adjective/adverb “radical”/“radically” when describing the differences between his approach and that of traditional psychoanalysis. In his first major volume on Attachment, he indicates his intention to reverse the reliance on adult clinical data for theory building and to rather study very young children to extrapolate forward. “The change in perspective is radical” (Bowlby, 1969/1982, p. 4). In his last major publication on Attachment Theory, Bowlby (1988) would employ the adjective radical to describe at least six key differences to be found in his work: (1) between his proposed view of cooperative infants, parenting and human nature underpinning those features and those held in Western societies (p. 9), (2) the availability of useful developmental concepts in worlds of science in Freud’s day and Bowlby’s day (p. 34), (3) his proposed theory of infant motivation and that held by Freud’s drive theory (p. 65), (4) his own notion of developmental pathways and Freudian oedipal derived approaches (p.65), (5) the nature of an infant bond he will adopt from ethology and its difference from any considered by either psychology or psychiatry (p. 131), and (6) in terms of consequent differences in the practice of psychotherapy (p. 165). I will condense these into more specific conceptual ones addressed here in Chapter Two and more methodological/strategic ones addressed in Chapter Three.
radical perspective, something to be considered in Chapter Three, is Bowlby’s deep methodological commitment to scientific process and philosophical naturalism. Historically speaking, psychoanalysis is also a moving target. Hence, grasping the radicalism of his scope and method requires a degree of engagement with Freudian theory, in Freud’s times, in Bowlby’s era and today.

Freud’s 1938 employment of Faust’s witch metaphor in the epigraph recognises how a key portion of psychoanalysis, especially its systematic explanatory metapsychology, has been notoriously indeterminate. Indeed, both of these Freudian terms—psychoanalysis and metapsychology—require some definitional clarification (see below). Freud’s multi-faceted views evolved but probably never quite in the direction of a coherent unambiguous system (for a relatively ‘even’ treatment of the historical development of Freud and his ideas see Makari, 2008). Adding to this ambiguity is the presence of Freud’s numerous sub-theories whose potential import in his overall framework seems to have also varied across his career. Consequently, recovering a definitive answer as to which individual sub-theory (or sub-theories)—repression, free association, the topographic model, the drive theory, the pleasure principle, the dual drives, Oedipal development, etc.—was more pivotal for Freud may therefore be elusive. Not surprisingly, both Freud’s immediate heirs and more contemporary psychoanalytic scholars have also disagreed as to what role a metapsychology—however, defined—might play. Equally, the role given to the individual sub-theories and their possible relationship with one another may vary widely. However, instead of tackling a seemingly insurmountable task of specifying the full breadth of this indeterminacy, I will allow Bowlby to do much of the work for me.

I want to stress that the views of psychoanalysis taken in this chapter will be critical, informed, but also not unfairly harsh. Approaching the volatile topic of psychoanalysis requires both historical perspective and a degree of conceptual awareness lest one succumb to the risks of intellectual laziness or doctrinaire inspired neglect. In particular, potentially less may be known about the sophisticated empirical developmental study conducted within psychoanalysis, what I will describe as Psychoanalytic Developmental Research (PDR). Knowledge of such work also suggests how poorly informed generalisations that psychoanalysis is inherently and always anti-empirical can be avoided.

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9 Makari’s views are particularly refreshing for an historian, psychiatrist and psychoanalyst. He avoids unnecessarily falling into the trenches of the seeming interminable Freudian skirmishes. The book has been broadly well received by therapeutic insiders (Karasu, 2008) and outsiders (Aranda, 2008) as well as historians and commentators of varying persuasions (Anderson, 2010; Appignanesi, 2008; Zaretsky, 2010). Makari’s perspective stands in sharp contrast to Sandler’s (1969) proposals that seek to minimise the impact of any suggested indeterminacy describing Freud’s ideas in terms of an evolving ‘basic model’.

10 I want to suggest that establishing clearly in a historical-critical manner what counts as psychoanalysis from within its complex traditions and differentiating what follows need not be dismissed out of hand as revisionist, unscientific or nostalgic. At the same time, assessing the relative coherence of those traditions should also not be rejected in quasi-fundamentalist fashion as reactionary, prosecutorial or ‘unconsciously defensive’. Distinguishing helpful and clearly argued positions from the more overtly polemical ones is rarely easy in the Manichaean-like field of Freudian study. The former—i.e. well argued positions—may often, if not inevitably sit within the latter—deeply polemical disputes. An example might be Borch-Jacobsen and Shamdasani’s (2012) recent publication, Freud Files: An Inquiry into the History of Psychoanalysis and the relatively dismissive review by the Cambridge philosopher of science, John Forrester (2012). Bringing to bear new evidence, Borsch-Jacobsen and Shamdasani offer stinging critiques of the impact of less helpful behaviours of Freud and his troops on claims made by psychoanalysis to be a scientific and coherent enterprise. In review, Forrester quickly diagnoses a biased forensic investigation that ignores the breadth of cultural and social impact brought about by psychoanalysis. In ways, both make valid critical points but would also appear to be concerned with differing matters: Freud as psychological scientist in the former and Freud as influential cultural phenomenon in the latter review. Vitriol and emotive polemic are equally as much a part of psychoanalytic tradition (see relevant discussions in Casement, 2004) as they are in the Freud wars that pit insider supporters and outside critics (Gomez, 2005). As will be noted in Chapter Five, Bowlby was a near casualty within those internal psychoanalytic wars.
However, such familiarity might also permit an importantly critical question: why might PDR seem also to never venture beyond the timeframe of early attachment formation in the first two years of life?

Chapter Two’s introduction of Bowlby’s metapsychological reforms for psychoanalysis will proceed in five steps. Section 2.1 will offer a conceptual description of psychoanalysis, focussed on what can be labelled ‘classical’ psychoanalysis—Bowlby’s primary target for reform. Next, Section 2.2 will identify four candidate definitions for Freud’s ‘metapsychology’ with emphasis on connections to etiological explanations in natural science. It is from this conceptual terrain that Bowlby’s proposed reforms emerged. Section 2.3 will identify five major components comprising Bowlby’s radical overhaul. Section 2.4 will then show how Attachment Theory might be untangled from at least three threads of psychoanalysis: (1) ‘classical’ psychoanalysis, his intended focus, (2) more ‘contemporary’ renditions that may have integrated Attachment Theory within, and (3) the previously noted Psychoanalytic Developmental Research’ (PDR) discipline that has also embraced the theory. Section 2.5 will provide a brief conclusion to the chapter and direct attention to the next chapter’s radical naturalist method.

2.1 Identifying ‘Classical’ Psychoanalysis

2.1.1 Why Bother with Psychoanalysis?

Why devote a portion of a chapter rambling through these thickets? Why not do as Rutter (1995, pp. 560-561) has advised: pay respect to the historical contributions of the psychoanalytic traditions, leave the distracting details behind and study Attachment Theory as contemporary empirical developmental endeavour? I will indeed suggest in Chapter Seven how Attachment Theory might be better considered as not a fundamental psychoanalytic enterprise but a lifespan developmental one.

I offer three simple reasons for retaining some initial focus on psychoanalysis in approaching Attachment Theory. First, it is hardly possible to adequately understand Bowlby’s proposals, especially his hopes for a new metapsychology, without some vision of the world from which they emerged. Second, psychoanalysis is still with us albeit in vastly diverse and at times hardly recognisable contemporary forms (for a good historical survey see Mitchell & Black, 1995) see discussion below in Subsection 2.4. As indicated, Attachment Theory plays a fairly significant role in many of these more contemporary schools. See especially two helpful surveys that take different perspectives on the relationship between the Attachment Theory and psychoanalysis: one complimentary (Fonagy, 2001), the other more incompatible (Eagle, 2013). Differentiating Freud from the ideas of his later followers whilst also distinguishing Bowlby from each is required for coherence sake. Finally, this additional clarity may also anticipate and deflect strategies that might prematurely dismiss Attachment Theory in a guilt-by-association manner as being merely psychoanalytic.

2.1.2 Freud’s Indeterminacy with Definitional Matters

It is worth noting that many of the important critical and philosophical discussions of psychoanalysis by such notable writers as Grünbaum (1984), Ricoeur (Ricoeur & Thompson, 1981) and Habermas (1981) have generally focussed on what is now commonly called ‘classical’ psychoanalysis, and may therefore be less applicable to the more contemporary articulations. But see also Grünbaum (2004, pp. 297-301). Most of these discussions have revolved around possible empirical status of interpretive insight in the clinical setting.
Now, distilling a definition of psychoanalysis from Freud’s corpus is hardly simple. Indeed, the task may have proved difficult even for Freud. In an unfinished 1938 work he sought to provide just such a definition. Freud stated, “Psychoanalysis is part of the mental science of psychology” (1963, p. 224).  

But he also prefaced this partial unspecific definition acknowledging the inherent difficulty in introducing psychoanalysis; he advised the adoption of two possible definitional methods. He described the first as genetic—a more inductive route that invites an enquirer to take part in “building up a new theory about the subject . . . [an approach that] has the defect of not making a sufficiently striking effect on the learner”. A second method he termed dogmatic, in which “something that is presented [to a listener] ready-made as an apparently self-consistent whole” leaves the critical hearer wondering “where does the fellow get [the definition] from” (S. Freud, 1963, pp. 223-224).

Unfortunately, Freud died before applying either approach to the completion of his task at hand: hence, the need to turn to others for assistance. But to reiterate what will appear more obvious is that with psychoanalysis—Freud’s and that which follows—, simple exact definitions may prove difficult to locate. Nonetheless, useful characterisations can be identified from commentaries focused on Freud’s work.

2.1.3 Constructing a Definitional Framework for ‘Classical’ Psychoanalysis

A relatively common view associates ‘classical’ psychoanalysis with ideas from a specific timeframe beginning in Vienna. Included here are Freud’s positions emerging from around 1895—when he broke with hypnosis and his collaboration with Josef Breuer (1842-1925)—up to the time of his death in London in 1939 (Eagle, 1984, 2011; Erwin, 1997; Mitchell & Black, 1995). From a conceptual point of view, a simple timeframe is obviously inadequate for elucidating complex ideas within psychoanalysis. As a complement to the post-1895 timeframe, I will construct a task-based definitional framework that includes: five superordinate tasks, Freud’s range of theories that underpin these five commitments and, finally, a less visible metapsychology to be discussed in Section 2.2.

Table 2.1 outlines three contrasting approaches taken in depicting psychoanalysis: Grünbaum (2004), Eagle (2011) and Fonagy & Target (2003).

12 Freud appears to be constructing an introduction to psychoanalysis for a general or lay audience that may have been intended to complement the previous unfinished effort aimed at ‘practitioners’, namely the 1938 Outline of Psycho-Analysis (S. Freud, 1989). Rieff suggests Freud’s Some Elementary Lessons in Psychoanalysis, “appears to be the beginning of a fresh version of Outline of Psycho-Analysis” (S. Freud, 1963, p. 223 n.221). Lessons appeared originally as a footnote within the early publications of the Outline, but was subsequently omitted from later editions. The Outline also offers no definition for psychoanalysis but rather a series of topical discussions within three parts: The Mind and its Working, The Practical Task and Theoretical Yield (S. Freud, 1989).

13 See discussion in Appendix 1 of possible meanings for Freud’s curious employment of this ambiguous term, dogmatic.

14 I adopt the term ‘classical’ along with the accompanying time frame as used by both Mitchell & Black (1995) and Eagle (2011) that hives off Freud’s initial employment of hypnosis in the context of his early but discarded affect trauma model. Fonagy and Target describe more neutrally Freud’s first theoretical ‘phase’ (2003, pp. 33-34). This break was probably more than a mere shift. This pivotal time also acknowledges Freud’s abandonment of his childhood seduction theory, a vociferously debated topic from the 1980s (for an insightful discussion see Glymour, 1993; for an interesting philosophical interpretation, see as well Gould, 1987). Eilenberger sees this as the beginning of a turn to a deeper psychology and a theoretical exploration of the implications of free association (1981, p. 498). Solloway also sees the introduction of Freud’s new theories on sexuality in the midst of the split as crucial here (1992, pp. 97-100). Grünbaum’s (1984, 2004) important critiques of psychoanalysis do not reflect this precise timeframe nor theoretical differentiation. However, although his work was less historical he nonetheless acknowledges Freud’s post-Breuer shift. The provision of the term ‘classical psychoanalysis does not imply that practices aligned with this theoretical orientation are no longer conducted. That is not the case.

15 Grünbaum is primarily interested in what I will describe as ‘classical’ psychoanalysis. Eagle is interested in systematically comparing ‘classical’ psychoanalysis with more ‘contemporary’ renditions. Finally, Fonagy & Target like Eagle are interested in
Before examining the contents of Table 2.1, I will first describe its construction. I start with a relatively uncontroversial observation offered by the historian Sulloway: “Psychoanalysis may be conveniently introduced in terms of three independent accomplishments by Freud: (1) a [therapeutic] method, (2) a theory of neurosis and (3) a theory of the normal mind” (1992, p. 11). What may make psychoanalysis potentially unique and at the same time problematic for study is how these three might be seen to interrelate.\(^{16}\) As will be demonstrated in several places below, psychoanalysis did not emerge as a simple distinct theoretical endeavor followed by applied science—i.e. Sulloway’s points (2) theory about the mind’s disorder, and (3) theory about the mind did not precede his (1) theory about therapy. Rather, the three accomplishments no doubt emerged in a more iterative interactive manner. In a similar vein to Sulloway, Eagle (2011) also accepts these three tasks within his list of generic psychoanalytic topics are observable not just in Freud, but also in post-Freudian psychoanalysis. Eagle also adds a fourth, fundamental topic, (4) ‘notions of object relations’—i.e. understanding the role of early relationships for development. Finally, in a comparable manner, Fonagy & Target (2003, pp. 5-7) have also singled out a fifth topic or assumption—one that partially subsumes (2) and (4), namely, (5) specific notions of developmental continuity and their conceptual consideration for psychopathology.\(^{17}\) Although the proposed theoretical contents of these five topics or tasks—highlighted in yellow in the table—may vary, I would suggest that they remain generic enough to be acceptable by most commentators.

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\(^{16}\) For a good discussion of Freud’s approach to the mind, especially as regards his Project, see Wollheim (1991, pp. 42-65).

\(^{17}\) Fonagy and Target’s approach for integrating the study of the normal and abnormal may also capture the spirit of principles elaborated by the discipline of developmental psychopathology.
Table 2.1: A Definitional Framework for ‘Classical’ Psychoanalysis

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<th>Five Proposed Fundamental Topics [ &amp; Author Identified Key Pivotal Theory ]</th>
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<td>“Distressing mental states induce the operation of a psychic mechanism of repression, which consists in the banishment from consciousness of unpleasant psychic states” [italics in original] (p. 275).</td>
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<tr>
<td>“Once repression is operative, it not only banishes such negatively charged ideas from consciousness but plays a further crucial multiple causal role: it is causally necessary for the pathogens of neuroses, the production of our dreams, and the generation of our various sorts of slips (bungled actions)” [italics in original] (p. 275).</td>
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<tr>
<td>“The ‘method of free association’ can identify and lift (undo) the patient’s repressions; by doing so, it can identify the pathogens of the neuroses, and the generators of our dreams, as well as the causes of our motivationally opaque slips; more-over, by lifting the pathogenic repressions, free association functions therapeutically, rather than only investigatively” (p. 275).</td>
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sexuality, instinctual drives, the oedipal complex, psychic conflict, aggressive drive and the shift to a structural model (1995, pp. 2-21). What is challenging here is that this list does not match theory for theory with those stipulated in the three examples provided in Table 2.1. Indeed, Mitchell & Black’s list does not include the three identified key Freudian theories provided by each critical discussion [highlighted in magenta]: Grünbaum’s “avowed” cornerstone repression, Eagle’s constancy principle and Fonagy and Target’s genetic-developmental proposition. Now, I will argue below that the task of integrating these potentially disparate matters was envisioned by Freud to be taken up within his metapsychology. Such an endeavour would seek to systematically address both how the parts of the whole of the edifice interrelate and what might count as the more general relation of psychoanalysis to causal explanation in the natural sciences. As the epigraph above suggests, Freud’s Hexe struggled to deliver. However, as Eagle (2011, pp. 43-45) has also noted, changing commitments to such systematic metapsychological tasks might differentiate ‘classical’ aims with the more ‘pragmatic’ aims of post-Freudian psychoanalysis (see Section 2.4).

2.1.4 Bowlby’s Position within Psychoanalysis

A brief note on context may help conclude this initial look at psychoanalysis. Bowlby was an active participant in the world of psychoanalysis. No one reasonably disputes the simple fact that his innovative notions for understanding early emotional development—i.e. Attachment Theory—emerged from within the mid-20th Century’s first generation of post-Freudian psychoanalysis. Bowlby was influentially engaged with the theoretical concerns raised within Object Relations, i.e. the importance of the development of first human relationships (for a comprehensive, comparative discussion of Object Relations, see Greenberg & Mitchell, 1983). Indeed, the field of psychoanalysis in the 1940s and ‘50s United Kingdom in which Bowlby toiled had begun to do what Freud had not been able to do, namely, study the development and treatment of the child.18 Bowlby was one of several key figures driving this shift (see fuller historical discussion in Chapter Five.)

On the one hand, Bowlby retained both his psychoanalytic and psychiatric professional adherences from his early theoretical publications (1958a) to the conclusion of his career (1991). This can also be easily established by notation of the presence of these two professional bodies as target audiences in later speeches and articles penned from 1979-1988 (see collection in Bowlby, 1988). On the other hand, to say that Bowlby’s ideas emerged from psychoanalysis and that he retained his allegiances does not imply that his proposed ideas need necessarily be considered reducible as psychoanalytic ones (for a detailed discussion see Eagle, 2013). Establishing whether Bowlby’s ideas count as psychoanalytic will depend in part on one’s definitions of terms for earlier and later versions of psychoanalysis. As highlighted in Table 2.2, Attachment Theory has been clearly accepted by

18 Freud’s ideas for infancy and development were “reconstructive”, based on speculations derived from adult clinical data (Eagle, 2013, p. 72). This shift to a more explicit prospective developmental focus was therefore a potentially significant departure from Freud’s original methods. Melanie Klein took up such work beginning in the 1920s in Berlin and would remain central in the field of Object Relations in the United Kingdom upon her move to London in 1926 to her death in 1960 (for an insider’s biography see work of noted Kleinian theorist Segal, 1979). She is commonly acknowledged as one of the key founders of the Object Relations field of psychoanalysis (Greenberg & Mitchell, 1983, pp. 119-150). However, Object Relations was not alone in this shift away from reliance on so-called ‘adult clinical developmental data’. Klein’s London rival from the field of Ego Psychology, Anna Freud was also involved in the study of and clinical work with children (Mitchell & Black, 1995, pp. 25-28).
numerous proponents across several schools. This serves as evidence of his remarkable success in partially reforming psychoanalysis. However, Bowlby’s ideas were also strongly rejected by many of his contemporaries (again, for a historical discussion see Chapter Four). Indeed, his proposals have also remained spurned by contemporaries. Mainstream, French psychoanalysis has accused Bowlby of seriously misrepresenting Freud’s ideas on infant sexuality (Zamanian, 2011). The question of the relationship between Attachment Theory and psychoanalysis is an important one. However, I will delay a response until Part Two after more substantial work has been concluded in Part One. But as Attachment Theory has been integrated within several fields of empirical study both inside and outside of contemporary psychoanalysis, some precision must be retained. Attachment Theory emerged from the world of ‘classical’ psychoanalysis and has been noticeably influential in the ‘contemporary’ world of psychoanalysis. It is now time to delve one step deeper into the notion of metapsychology, one of Bowlby’s specific areas of concern.

2.2 Metapsychology within ‘Classical’ Psychoanalysis

2.2.1 Freud’s View(s) on ‘Metapsychology’

Understanding what Bowlby sought to ‘radically overhaul’ requires some initial clarity of Freud’s metapsychology. Although the term has been occasionally employed outside of psychoanalysis in the later part of the last century (e.g. as a reference to the philosophy of the science of psychology Rakover & Kaplan, 1990), the concept remains for the most part a ‘psychoanalytic’ term. However, like many things Freudian, little consensus exists when it comes to defining this notion in psychoanalysis (Brenner, 1980; Gill, 1988). Nonetheless, it is possible both to acknowledge Freud’s witch-like ambivalence in his employment of the term and to establish some viable employment(s), especially as potentially considered within Bowlby’s approach. As with psychoanalysis, Freud’s ideas should also ideally be kept distinct from later elaborations of metapsychology (see 2.4.2 below.) A case can be made that Freud indicated at least four distinct but nonetheless related approaches to metapsychology: two focused on the specific contents of the mind’s workings, two on possible methodologies for studying that content. I will argue that both approaches more broadly considered might fit into a single characterisation of metapsychology.

19 The Rakover & Kaplan work is an interesting one devoted to issues in theory of psychology. Its choice of title—Metapsychology: Missing link in behavior, mind and science—is in ways quite odd. On a first read, it appears that the term metapsychology is never mentioned much less defined by the authors.

20 Finding one’s way through the dense and sometimes confusing discussions of Freud’s metapsychology can be slightly exasperating. Differences in perspectives inevitably become matters of interpretation of Freud’s own thinking: i.e. which time periods, which texts, which types of texts—published works, letters, an unpublished work—, which theories, which relationships between theories, which relationships to clinical theory and observations, and ultimately hermeneutic readings of Freud’s mind. Consequently, one finds different renditions within detailed discussions by insiders (Brenner, 1980; Eagle, 1984; Gill, 1988; Imbasciati, 2010; Nagera, 1970) as well as in the detailed (Kitcher & Wilkes, 1988) and the more cursory approaches within external commentators (Ellenberger, 1981; Glymour, 1993; Grünbaum, 1984; Sulloway, 1992). Many of the discussions by Freudians may also manifest more contemporary post-Freudian polemics that may have coloured their views of history.

21 Brenner has also noted the term ‘metapsychology’ existed prior to Freud: “it was and still is used to study the relation between mind and body” (1980, p. 190). Webster’s On-line Dictionary traces its origin to 1868 and denotes it as an example of International Scientific Vocabulary. However, dictionaries are not terribly helpful, typically rendering the term as a speculative non-empirical supplement to psychology and/or as some derivative of Freudian psychoanalysis (e.g. see R.R. Holt, 2002).
Freud’s first 1901 published employment of ‘metapsychology’ was as in terms of a depth psychology that distinguished a psychology of the unconscious from a psychology of the conscious. This latter perspective was the standard view of psychology in the late 1800s. Brenner also traced this employment to prior unpublished inchoate ideas where “Freud clearly equated ‘unconscious’ with ‘biological’ and meant by metapsychology the relation between what is conscious/psychological and what is unconscious /biological” (1980, p. 191). Indeed a subset of commentators have regularly contended this initial focus on content, i.e. the unconscious, remains the foundation for any later expansions of his metapsychology (see Brenner, 1980, p. 193).

Freud’s second published employment of the notion is also focused on theoretical content. Gill (1988, p. 36) describes this as the more common or usual case, and is dated to 1915. Freud is often quoted in discussions of this second use: “I propose that when we have succeeded in describing a psychical process in its dynamic, topographical and economical aspects, we should speak of it as a metapsychological presentation [italics in original]” (Freud (1971) cit by Gill, 1988, p. 36). The term metapsychology in this context has often come to be associated with two notions. First, metapsychology becomes a more prescribed narrower systematic activity of theoretical renovation of a very specified content—i.e. expounding the noted foundational triad of metapsychological models (Sulloway, 1992, pp. 61-65). It also becomes associated with the period in which that work was conducted, from roughly 1914 to 1923 (Ellenberger, 1981, pp. 510-518). However, debate continues as to the degree to which the traditional 1915 contents associated with narrower restrictive, approach might also have been extended—implicitly or explicitly—to incorporate genetic, adaptive and the later 1923 structural approaches (see discussions in Rapaport & Gill, 1959). Indeed, as discussed below, Bowlby would accept Rapaport and Gill’s proposals for a broader set of contents within this second employment of metapsychology.

A third slightly more methodological employment of metapsychology has also been recognized by Glymour (1993). He notes that in the 1899 Interpretation of Dreams, Freud refers to his unpublished 1986 Project for a Scientific Psychoanalysis as his ‘metapsychology’ (Glymour, 1993, pp. 477-478). In this vein, Freud’s ‘metapsychology’ was to be aligned with his Project, something that captured his

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22 The source is Freud’s The Psychopathology of everyday life. Gill (1988) has described the first employment, depth psychology, as the occasional employment within psychoanalysis but nonetheless one still in use. For example, see the recent approach taken by Imbasciati (2010).

23 Freud’s first discussions of metapsychology are typically traced to Freud’s letters with Fleiss between 1896-1899 (see Brenner, 1980, pp. 191-192).

24 The 1915 quoted text is variously cited as originating in The Unconscious (Brenner, 1980, p. 192; Gill, 1988, p. 36), or alternatively within a subtitled section ‘Justification of the Unconscious’, in the third essay The Unconscious, within the five that comprise Freud’s publication, Metapsychology (Imbasciati, 2010, p. 74). In Reiff’s collection of metapsychology papers, Freud’s proposed work is translated as Preliminary Material for a Metapsychological Theory (S. Freud, 1963, p. 147n141).

25 The topographical model is based on the “distinction of the unconscious, the preconscious and the conscious”; the dynamic includes “psychic forces in conflict with one another”; and the economic consists of the “regulation of mental forces through the pleasure-unpleasure principle” (Ellenberger, 1981, p. 511).

26 Indeed, this version of metapsychology has achieved a sort of normative status amongst many within more ‘classical’ renditions of psychoanalysis, reflecting Gill’s earlier descriptor usual. For example, see entry for ‘metapsychology’ in the International Dictionary of Psychoanalysis (Roussillon, 2005).

27 Rapaport and Gill (1959) replaced topographical with structural models, something controversial for many classical observers. In doing so, they acknowledged that although Freud’s structural model should supersede the topographical, he nonetheless did not replace the topographical in his metapsychological pronouncements. Rapaport and Gill do not reject topographical distinctions—conscious, preconscious and unconscious—, but nonetheless suggested that the topographical was already implied within the dynamic and economic (1959, p. 154). Similarly, they add what they see as two additional implied metapsychological assumptions in Freud, the genetic—developmental—and the adaptive—the role of environmental interaction. Again, this fifth distinction would be highly contestable as to how significant a role it may have played for Freud.
hopes for grounding psychoanalysis in the natural sciences as a 19th Century empirical investigation of the mind and its structures. As this work was never published, it may also have been considered less explicitly in metapsychology discussions (but see also R.R. Holt, 1982, p. 237). Indeed, Glymour’s analysis is directed at Freud’s eventual abandonment of any scientific aims for psychoanalysis, especially the aims of the Project.

A fourth employment of the term is indirectly attributable to Freud but may be seen as remaining faithful to his general metapsychological enterprise more broadly. Rapaport and Gill noted, “At some point in time the development of every science, the assumptions on which it is built must be clarified. Freud meant metapsychology to do just that for psycho-analysis” (1959, p. 153). Ellenberger has also suggested Freud’s initial metapsychological aim was to provide “a complete restructuring of the framework of psychoanalytic theory . . . sufficiently comprehensive to encompass all the facts and aspects of psychoanalysis” (1981, p. 511). Such an application has been inferred to provide the rationale for the inclusion of a broader range of theoretical concepts beyond the 1915 triad than ascribed the second’s more narrow application above. Rieff, who compiled a collection of Freud’s theoretical papers entitled General psychological theory: Papers on metapsychology, suggested in his introduction that “metapsychology’ and ‘general psychological theory’ may be understood as interchangeable terms” (S. Freud, 1963, p. ix).28 Greenberg and Mitchell have also described Freud’s metapsychology more generally as “the attempt to take apart the psychic machine, to figure out the forces and counterforces that operate within it” (1983, p. 22). Finally, anticipating post-Freudian response to issues of metapsychology, Eagle has noted that “one can think of Freud’s metapsychology as a sort of placeholder for comprehensive explanatory accounts that await further knowledge and further theorizing” (2011, p. 44).

To summarise, metapsychology captured Freud’s initial hopes for a connection between psychoanalysis and possible causal explanatory contributions from natural science. Which of his four considerations should be selected—content, method or some combination? As noted, the narrow employment of definition two—i.e., the 1915 triad only—has more often been identified as Freud’s more valid metapsychology (see discussion of contemporary employment below.) However, the term need not be restricted to either a specific historical interlude or extrinsically selected subset of texts/concepts, especially based on Freud’s indeterminate employment as noted in the epigraph. Gill has also suggested that definitions one—the depth psychological content consideration—and two—the expounding of content of a narrower theoretical triad—might best be considered together as the two conceptually related standard content options for the term (1988). I will suggest an alternative even more encompassing consideration for both content and methodology. I will retain a view of metapsychology as a sort of scientific lynchpin, retaining a potentially systematic explanatory effort—i.e. methodological—to account for the breadth of full mental life—i.e. content. As such it could encompass each of the four identified definitions, whilst rejecting the alternative—albeit commonly

28 Although many of the Freud papers that Reiff has selected fall within the so-called metapsychology period (i.e. 1914 up to 1923), he is clear that important papers written before—such as the pivotal 1900 The Interpretation of dreams—and after—especially the 1923 The Ego and the id—should equally be consulted as proper sources of Freud’s general theory (S. Freud, 1963).
recognised—employment of the second narrower option alone. If one can accept the fourth, broader approach, then the initial three can also be retained within. As will be shown next, this is exactly the sort of approach employed in Bowlby’s renovation objectives.

2.3 Bowlby’s ‘Radical Overhaul’ of Freud’s Metapsychology

Bowlby’s proposals for Attachment Theory embrace both the contents and method of metapsychology (for his major discussions see 1969/1982, 1988). His methodological proposals will be taken in Chapter Three. His content suggestions can be divided into at least three distinct efforts: a proposed change to the contents of Freud’s metapsychology, a suggested reform that encompasses each of the five central topics for psychoanalysis identified above, and a rejection of key underlying theory—Freud’s psychical energy / drive theory. Once again, what should appear obvious is both the radical scope of his programme and Bowlby’s willingness to jettison key aspects of the classical Freudian framework.

2.3.1 Bowlby’s ‘Radical’ Metapsychological Work

In principle, Bowlby remained faithful to the more empirical model of metapsychology described above—i.e. part content, part method. Indeed, he was also quite explicit as to which portions of Freud’s metapsychological content he sought to reform and which he sought to retain. Bowlby’s proposed reforms abandon any pretence of an unexamined commitment to continuity with or retention of Freudian models. The consequence would be the forsaking of a significant portion of Freud’s theoretical edifice.

For comparison of his approach with that of Freud’s, Bowlby accepted an expanded five-part content version of Freudian metapsychology described by Rapaport and Gill (1959): the dynamic, the economic, the structural (which superseded the topographical), the genetic and the adaptive.

Now there is no difficulty with the structural, the genetic, and the adaptive . . . The points of view not adopted are the dynamic and the economic. There are therefore no propositions concerning psychological energy or psychological forces; concepts such as conservation of energy, entropy, direction and magnitude of force are missing (Bowlby, 1969/1982, p. 14).

Bowlby’s exclusion of both the dynamic and economic models was indeed radical and explains why he would find such little initial support across psychoanalysis. Although Bowlby’s theoretical ideas first emerged in the 1950s, he nonetheless maintained fluency with the range of ongoing metapsychological discussions in psychoanalysis (1984). However, a review of ten post-1969 specific psychoanalytic metapsychological discussions suggests only one of ten had engaged Bowlby’s ideas.²⁰

²⁰ As noted above, this was a modified version of the second content approach to metapsychology. Bowlby also acknowledged the need for some sort of structural model that would encompass the unconscious. However, as an understudy to Klein in the ‘object relations’ field, he would have had less interest in the specifics of the formal structural model: id, ego and superego.

2.3.2 Bowlby’s Specific Psychoanalytic Reforms

Bowlby’s specific reforms can be gleaned from his perspective of psychoanalysis. “Psychoanalytic theory is an attempt to explain the functioning of personality, in both its healthy and its pathological aspects, in terms of ontogenesis. In creating this body of theory not only Freud but virtually all subsequent analysts have worked from an end-point backwards . . . In many respects what is attempted here is the opposite” (1969/1982, p. 4). Hence, Bowlby would become one of the first psychoanalysts to commit to the systematic study of development in a prospective fashion. His programme for reform can be divided into five tasks that match the full breadth of the definition of psychoanalysis above in Subsection 2.1:

1. (Conception of Mind) Revising Freud’s model of psychic function in terms of the notion of ‘behavioural systems’ found in ethology that also reflected common evolutionary descent, and thereby abandoning both Freud’s dynamic and economic models in favour of ideas current in biology.

2. (Assumption of Developmental Continuity) Offering a new lifespan developmental model, that also seeks to study development from infancy onwards in a prospective manner, preserving but also altering Freud’s genetic approach.

3. (Conception of Psychopathology) Updating notions of developmental or psychogenic disorder, noting especially environmental and interpersonal risks, therefore retaining the adaptive model.

4. (Conception of Object Relations) Identifying causal mechanisms underpinning interpersonal (attachment) experience that support the explanation of each of the initial three, and thereby reestablishing a link to the natural sciences but shifting his focus from ideas taken from 19th Century physics and chemistry to later 20th Century ones in biology, ethology and the cognitive sciences.

5. (Conception of Treatment) Calling for a reformulation of a newly informed psychotherapy based on the first four, whilst rejecting any claim that clinical data might provide confirmatory evidence for his developmental theories.

Bowlby would first integrate ethological notions of behavioural systems into an empirical—or more specifically, a naturalist or causal realist31—developmental metapsychology, i.e. Attachment Theory (1969/1982, 1973, 1980).32 He would second offer an implicit model of lifespan development from which, third, developmental disorders might be identified and explained (1988). In doing so, he would fourthly seek to break open the black box of the mind and study the causal and functional processes within (1969/1982, pp. 85-140), eventually calling upon relatively new explanatory notions from the cognitive sciences, especially the notion of working models or representational models or internal working models (IWMs) (1973, pp. 203-209, 317-319; 1980, pp. 229-244; 1988, p. 165). In the fifth proposed renewal, Bowlby would also argue that a more causally based approach to

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31 Bowlby would not employ these overt philosophical terms to describe his theoretical work. However, I will argue that his work reflects both its ethological roots and what today’s philosophy and evolutionary developmental biology (evo-devo) would describe as a naturalist, causal realist approach to development (Callebaut, Müller, & Newman, 2007, pp. 41-42), terms that would implicitly also apply to an attachment-informed psychotherapy.

psychotherapeutic intervention might be designed on the basis of a new systematic metapsychology, an empirically based developmental theory, an accompanying approach to developmental disorder, and an inherent focus on causal mechanisms (1988, pp. 58-76). The truly ‘radical’ element here lies with the scope of these proposals. Bowlby sought to significantly alter each topic/task of ‘classical’ psychoanalysis, as well as discarding two of its long held foundations, the dynamic and economic models. Central for this thesis and the core of Part One then is a critical assessment of how this project has fared.

2.3.3 The Key Theoretical Departure: a Rejection of the Psychical Energy Drive Model

One crucial point relative to Freud’s metapsychology is worth acknowledging here at the outset: Bowlby’s rejection of psychical energy or the economic model that included the so-called drive theory—also referred to as the instinctual psychical energy model. As Eagle suggests, “I believe Bowlby is correct in observing that Freud’s instinct or drive theory is at the heart of his metapsychology” (1984, p. 6). “Freud’s fundamental vision of the human condition is embodied . . . in the drive/structure model . . . Drive is a concept on the frontier between the psychic and the somatic, an endogenous source of stimulation.” (Greenberg & Mitchell, 1983, p. 21). One consequence of the rejection of a drive model in favour of an attachment approach is the requirement to reassess the function of close relationships for our understanding of the regulation of negative affect across the lifespan.

In the discussion of Table 2.1 above, I noted three different possible renderings of a pivotal theory in for Freud: repression, constancy principle and genetic developmental position. However, if one can accept that in fact many of Freud’s numerous theories were often interconnected, a precise designation of a single theory would appear potentially less crucial. I will therefore accept Bowlby’s proposals for abandonment of the drive theory as a reasonable starting point, especially in light of his proposals for replacements that might be verified via empirical investigation.

2.3.4 The Irony in Bowlby’s Approach to Freud

There may also be an interesting dose of irony in Attachment Theory’s overhaul of Freud’s theoretical edifice. First, in insisting on an empirical metapsychology, Bowlby also made reference to Freud’s

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33 As mentioned above, many discussions of nosology within philosophy of psychiatry have focussed on the importance of causal validity for identifying disorders. In addition to rendering disorders more intelligible, such advances would provide direction for design of intervention.

34 This model was first prosed in 1905 (S. Freud, 1905/1953). For a detailed discussion of drive, both its initial development within and later influence on Freud’s metapsychology see two chapters in Greenberg & Mitchell (1983, pp. 21-49, 50-78).

35 Note should be made of a dissenting opinion here, namely rejection within French psychoanalytic discussions dating from 1967 of a so-called Anglo-Saxon conflation (via mistranslation) of instinct (Instinkt in Freud’s German) by drive (Trieb) (Laplanche & Pontalis, 1973). Strachey is typically blamed but closer inspection suggests Freud was also a consenting party to the translation conflation (Ray, 2012). The critics have sought to distinguish Freud’s rarely employed instinct as a biological survival function from a more psychological drive that encompasses infant sexuality. Laplanche & Pontalis (1973) have also noted Freud’s conflicting terminological positions arising from his initial discussion of sexual drives and later discussions of life and death drives—notions that might be better captured by instinct. For a recent review and discussion, see Zamanian (2011). However, I will state that the positions argued for by the French writers—and some English writers as well (e.g. Mills, 2004)—require a theoretical commitment both to Freud and Laplanche’s Seduction Theory. This was something that Bowlby did not accept, namely the early primacy of sexuality in human motivation. Even if the terms diverge as the French commentators have argued, Bowlby would have nonetheless rejected both employments in place of his ethological option. Attachment will be shown to have been ‘natural’ through and through, both as phylogenetically shared with other non-human animals, and as something that will also extend into human adult life and adult relationships.
own biological aspirations, quoting from the unpublished 1895 Project for a Scientific Psychology: “The intention is to furnish a psychology that shall be a natural science” (as cited by Bowlby, 1988, p. 58). Attachment Theory has sought to recover a once empirical/biological, though fleeting, metapsychological ambition that had arguably long since been abandoned by Freud (Glymour, 1993).

Second, Bowlby retained respect for the deep aspiration of Freud’s metapsychological enterprise as captured in The Project. As late as 1980 in a summary of his inaugural address as the Freud Memorial Visiting Professor at University College London, Bowlby argued for the importance of this perspective: “So long as analysts are willing to define psychoanalysis by any [non-explanatory] theory, they must not complain that their discipline is cold-shouldered by academics. They are condemning it to frozen inertia” (1988, p. 58). For Bowlby, Freud’s questions of the mind’s workings remained valid and important; only the answers Freud provided via the methods of classical psychoanalysis were considered suspect.

2.4 Attachment Theory and ‘Contemporary’ Psychoanalysis

2.4.1 Differentiating ‘Contemporary’ from ‘Classical’ Psychoanalysis

Freud may have sustained an external appearance of unification within classical psychoanalysis (Borch-Jacobsen & Shamdasani, 2012), but divisions and controversies emerged post mortem leading to significant splintering over the past 70 plus years.\(^37\) As Shedler observed, “The development of psychoanalytic knowledge did not end with Freud any more than the development of physics ended with Newton or the development of the behavioral tradition in psychology ended with Watson” (2011, p. 152). How might we account practically for today’s ‘contemporary’ panorama? And how might we understand Bowlby and his reforms within this second landscape? Together these comprise a differentiation exercise covered in this subsection.\(^38\) The value of distinguishing more contemporary movements also serves to demonstrate the immense success Bowlby would eventually gain.

Let me turn initially to the first task, identifying the diverse landscape of ‘contemporary’ psychoanalysis. To capture this more complex present terrain, I have constructed below Table 2.2: A Historical Framework for Situating Attachment Theory within Psychoanalysis.\(^39\) This framework serves only as an introductory heuristic, and is not intended as a comprehensive perspective. Also, in this

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36 Eagle has described this non-explanatory approach present in many contemporary approaches as a “clinical-theory-only” option (1984, p. 149), a strategy that Erwin (1997) depicts as often deflecting Freud’s questions of the mind and its origins.

37 Division and disputation also emerged amongst colleagues and disciples Freud’s lifetime (Makari, 2008).

38 A third more complex question also arises, how might the relationship between these more contemporary practices and classical perspectives be conceptualized? I will hold off on this last question until Chapter Seven. The question will also be shown to play an important role the discussion of Part Three. Indeed this third enquiry requires greater familiarity with the development, breadth and history of Attachment Theory, all considered in Part Two.

39 This cannot be a comprehensive survey. As far as I can observe, no such detailed work exists. Clinical practice in psychoanalysis is also increasingly eclectic, integrated and plural. Consequently, any partition of psychoanalysis post-Freud will be inadequate. An omission Jung and inclusion of Sullivan might also be found simplistic. I acknowledge especially the geographic limits: mostly, USA, and UK. Australia has at least four psychoanalytic schools broadly defined: the Australian Psychoanalytical Society (APAS—emerged from Object Relations perspectives), Psychoanalytic Psychotherapy Association of Australasia (more classical in origin), the Australian Centre for Psychoanalysis (ACP—Lacanian) and Australian New Zealand Association for Psychotherapy (ANZAP—a mixture of Self-Psychology and relational approaches). I include Meares work from the latter school, as he would appear to be one of the more original Australian writers.
brief descriptive discussion, focus will not be given to formally differentiating the notions of psychoanalytic, psychodynamic and psychoanalytic psychology, though differences can nonetheless be significant.\footnote{The employment of these terms may vary; nonetheless each term might appropriately sit within broad discussions of contemporary psychoanalysis. However, I do not see these two terms, psychoanalysis and psychodynamic, as synonymous. Indeed, many therapists claiming some sort of psychodynamic identification may reject a psychoanalytic label. However, many of the distinctions between the two may be less relevant for this thesis. Chapter One noted that the thesis scope precludes looking directly at the implications of Attachment Theory for an ‘informed psychotherapy'. Nonetheless, a few words might be useful for less familiar readers. There is no generally accepted distinction demarcating psychoanalytic from psychodynamic psychotherapy. But the definition of the second would seem to typically be dependent on the contents of the definition of the first. Perhaps the simplest approach is to see psychoanalytic as early, i.e. Freud, his first followers and today’s relatively strict adherents, and psychodynamic as emerging with later reforms. Clearly, more traditional psychoanalytic adherents may have historically considered their practice superior to the latter, perhaps as “watered down” psychodynamic variants. Ghaemi employs the term dogmatism in a useful manner in his model for approaching The Conceptual Status Quo in Psychiatry; he described the more classical psychoanalysis as practiced within American psychiatry as an example of “yesterday’s orthodoxies” (2003, p. 11). However, such ‘puritanical’ comparisons are less common today, especially as the practice of ‘classical’ psychoanalysis declines (Bornstein, 2001). More likely, differences today may be considered descriptively, often within more pluralistic frameworks. McWilliams discussion (2004, pp. 13-26) has proposed psychoanalytic/psychodynamic distinctions may be considered as either differences in kind or in degree but would appear to side with the latter. She identifies a possible continuum “from psychoanalysis through the exploratory psychodynamic therapies” where differences in (1) therapeutic timeframe, especially length (long-term vs. short-term) and times-per-week (four typically associated with psychoanalysis and 2 or less for psychodynamic), and (2) role if any provided to client transference (where transference is considered the presence of maladaptive childhood interpersonal patterns manifest in current interactions with therapist)—again ranging from psychoanalysis’ focused on emergence within treatment of historical transferences to less intense therapies with minimal emphasis on transference (McWilliams, 2004, p. 13). In principle, psychodynamic work may be quite short-term, less focussed on transference and in many ways carry a focus on crisis or client problems that may appear quite similar with other modalities such as Interpersonal Psychotherapy (IPT) or Cognitive Behavioural Therapy (CBT). For an acknowledgement of less known similarities in practice between these supposed distinct modalities, see Wachtel (1997).}{\footnote{This modest survey has been relatively well received and has been identified as a good postgraduate introduction (Sophie Freud, 1997; Goldberg, 1997; Schrenzel, 1998). Although not without the predictable limitations of simple one volume survey, it would seem adequate for my purpose here.}} I generally follow the Mitchell and Black survey (1995)\footnote{This modest survey has been relatively well received and has been identified as a good postgraduate introduction (Sophie Freud, 1997; Goldberg, 1997; Schrenzel, 1998). Although not without the predictable limitations of simple one volume survey, it would seem adequate for my purpose here.} in constructing this framework, employing their seven identified schools. However, where appropriate, I have added a few supplementary theorists to their discussion. I have also included two additional ‘schools’: a French group and developmental research tradition. To address the second task of situating Bowlby within this terrain, I have also employed a highlighting colour scheme to differentiate (1) Bowlby’s target fields for reform [magenta], (2) Bowlby’s and other Attachment researchers place within the field [yellow] and (3) the wide range of contemporary adherents who have either adopted Attachment Theory or indicated friendly recognition or acceptance [green].
Researchers in the USA and UK have made significant contributions to psychoanalytical development, including the adoption of approaches such as relational and intersubjective theories. From the 1970s onwards, there has been a movement towards a more empirical focus, with researchers like E. Erikson, H. Sullivan, and C. M. Hesse, emphasizing the role of the environment and social interactions in psychological development.

<table>
<thead>
<tr>
<th>Psychoanalytic Schools</th>
<th>Brief Theoretical Description</th>
<th>Key Figures</th>
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</thead>
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<tr>
<td>Classical Psychoanalysis (beginning in late 19th Century and ongoing but also dispersed across schools below)</td>
<td>Freud’s evolving life work includes a diverse array of theoretical and clinical ideas, including central position for notion of psychic drives and repression.</td>
<td>Sigmund Freud and eventual but influential opponents: A. Adler, C.G. Jung, O. Rank, S. Ferenczi.</td>
</tr>
<tr>
<td>Neo-Freudians and Interpersonal Psychoanalysis* (emerged in 1920s)</td>
<td>Place greater focus on both external influences upon development (including notions of self-system and security) and reconfigured therapeutic method as more ‘participative’.</td>
<td>Historical: H.S. Sullivan, E. Fromm, K. Horney. Contemporary: D.B. Stern (1983)<strong>, P. Bromberg (2006)</strong>. Excellent work within both interpersonal and relational schools.</td>
</tr>
<tr>
<td>Ego Psychology (from the 1920s)</td>
<td>Focused on Freud’s Structural Model [id / ego / superego], ego defenses and also empirical study early developmental.</td>
<td>Anna Freud, R. Spitz, M. Mahler, H. Hartman, Sandit, Fonagy and Target (1997)**</td>
</tr>
<tr>
<td>Object Relations* initially including Attachment Theory (from the 1920s and in the United Kingdom from the 1940s)</td>
<td>Turned focus of psychoanalysis to relational development. The Kleinian and British schools* differed along several dimensions. But both schools were deeply committed to developmental study.</td>
<td>H. Gunthrup, W.R.D. Fairburn, A. Klein, W. Bion M. Ballint, D.W. Winnicott, J. Bowlby (1960/1962)</td>
</tr>
<tr>
<td>French Schools of Psychoanalysis (‘classical’ dating to Freud’s days and Lacanian from early 1960s)</td>
<td>French Psychoanalysis split between more classical adherents and the Lacanian followers in 1963. Lacan introduced radical changes to both theory (‘Nietzschean’ and ‘linguistic’ reading of Freud?) and method (variability in session length and number).</td>
<td>A. Green, J. Laplanche, J. Pontalis, E. Rudenesco, J. Lacan**</td>
</tr>
<tr>
<td>Identity- (from 1950s) and Self-Psychology* (from 1970s both initially from USA)</td>
<td>Emerged from a rejection of instinct view in Ego Psychology in favour of a socialisation view emphasising identity and self-hood. Janetian notions of traumatic roots of pathology have also been reapplied.</td>
<td>E. Erikson, H. Kohut*, R. Meares (2006)**</td>
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<tr>
<td>[Catchall category.] All sought to preserve yet modernize Freudian notions or as an integration with philosophy of language or adaptation of hermeneutics.</td>
<td>E. Erikson, H. Kohut*, R. Meares (2006)**</td>
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<tr>
<td>‘Psychoanalytical’ Developmental Researchers (from the mid 1970s in the USA and UK)</td>
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*Four (sub-) schools may overlap in clinical technique.

**Individuals may be recognized within multiple schools.

[References are not comprehensive for earlier figures.]

Table 2.2: A Historical Framework for Situating Attachment Theory within Psychoanalysis.

Key: (K) = Kleinian.

- Bolbwy’s Targets for Overhaul
- Attachment Researchers within Psychoanalytic Tradition
- Attachment ‘Psychoanalytical’ Theorists and Researchers
As the magenta shading indicates, Bowlby took first aim at the theoretical or metapsychological foundations within Freud’s ‘Classical Psychoanalysis’. He also included as targets, ideas developed in the next generation of psychoanalysis, both within the ‘Object Relations’ tradition (Bowlby, 1969/1982, pp. 16-18), including those of his supervisor Melanie Klein (1973), and amongst adherents of ‘Ego Psychology’, including the work of Anna Freud (1932/1992).

A couple of observations on Bowlby’s relation to Freud and these next generation ideas may be in order here. On the one hand, Bowlby clearly shared Klein and Anna Freud’s interests in the study and treatment of children. [See discussion of the historical interactions between Bowlby and these two child analysis pioneers in Chapter Five.] He also clearly acknowledged that his theory was “derived” from ‘Object Relations’—although as an evolutionary theory it clearly differed as well (Bowlby, 1969/1982, p. 17). On the other hand, he carefully differentiated his own ideas on attachment from a wide range of alternatives in his relatively systematic review of the literature on childhood separation anxiety (Bowlby, 1960b). Indeed, Bowlby considered Klein and Anna Freud’s perspectives on separation anxiety as inadequate and less informative than Freud’s. Bowlby saw their theoretical ideas, which actually emerged in the 1920s as outdated in comparison to Freud’s later 1930s views.  

Both Klein and A. Freud failed to take account of Freud’s final treatment of the subject in his 1926 Inhibitions, Symptoms and Anxiety (Bowlby, 1969/1982, pp. 377-390); here he proposed a more sympathetic attitude to the importance of maternal relations. Consequently, Bowlby’s interest remained the empirical renovation of the original Freudian edifice, including this more suggestive later work.

As can be seen with the green and yellow shading in the column on the right, the acceptance of Attachment Theory by researchers across contemporary psychoanalysis has been relatively widespread. Although Attachment theory has been embraced by a significant segment of contemporary psychoanalysis, the theory has also been taken up outside this realm in other recognized empirical fields including developmental psychology, developmental psychopathology and social psychology (see discussion in Chapters Three, Four and Five.) The theory has also played a central role in the emergence of the psychoanalytical developmental research (PDR) tradition, a field that overlaps with these first two additional fields (see Subsection 2.4.3.)

2.4.2 Contemporary Attitudes to Psychoanalytic Metapsychology

A closer look at the role played by metapsychology within contemporary psychoanalysis may also be illustrative. Contemporary discussions of metapsychology can be demonstrated to have moved in at least five slightly distinct directions. First, for the more classical proponents, the narrower version of the second employment of metapsychology mentioned above has often been granted a sort of

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42 Klein saw early anxiety as emerging form an aggressive or death instinct and placed less value in exploring early observations. Bowlby acknowledged that Klein’s ideas “not only developed . . . independently of Freud’s but [had] frequently underlined the differences between them.” She consequently paid less attention to actual observation. In contrast, Anna Freud, along with Dorothy Buringham, first studied separation anxiety in clinical context (Burlingham & Freud, 1944). Anna Freud nonetheless retained throughout her writings the early Freudian notion of anxiety within an early object relation as a consequence secondary drive.

43 Without going into endless detail, I have identified each of these green supporters through a reading of their individual works and an identification of their explicit statements concerning Attachment Theory.
‘received’ or ‘orthodox’ status during the post-Freud decades. For this perspective, Freud’s 1915 ‘metapsychology triad’—dynamic, economic and topographical—has often retained a central place (Holder, 1970). Roussillon’s (see 2005) entry for ‘metapsychology’ in the International Dictionary of Psychoanalysis provides a good example of such a retention. However, as Imbasciati (2010, p. 76) has noted, the consequence here is that metapsychological ideas originally proposed as hypotheses for understanding the mind have arguably become ‘reified’ and accepted as existing realities. This might be described as ‘metapsychology without science’. Not surprisingly, this more ‘orthodox’ approach has also raised serious critical questions as to its coherency and lack of any possible empirical status for any psychoanalysis defined by a metapsychology in this manner (see especially Grünbaum, 1984, 2004).

One consequence of both the external criticism of psychoanalysis and its relatively chronic lack of empirical support has been the move by other schools of psychoanalysis to abandon traditional metapsychology in favour of alternative approaches. These have focused almost exclusively on clinical phenomena. Three of these are worth noting, what may be identified as the second, third and fourth examples of contemporary approaches to metapsychology. A more extreme response has been the outright rejection of all science including natural science, and therefore any role for an empirical metapsychology; science has been deemed inappropriate and inapplicable for psychoanalysis. In scenarios of this sort, psychoanalysis may typically be considered a ‘subjective’ clinical phenomenon, one that by its very nature is therefore not amenable to objective measure (see discussion in Modell, 1981). A next related response has been to suggest an alternative ‘candidate science’ for approaching psychoanalysis, namely hermeneutics (G. S. Klein, 1976; Schafer, 1975). In this approach, natural science is depicted as the realm of explanation and distinguished from the realm of understanding, typically described as personal meaning. Psychoanalytic interpretations retain a central role for psychoanalysis but are increasingly understood in the context of an individual’s evolving narrative. However, little attention has been paid here to the empirical establishment of an underlying mental apparatus that might also support the creation of such a narrative, much less the status of truth within (see discussions in Eagle, 1977; Woolfolk, 1998).  

The second non-scientific and third hermeneutic option just noted might also sit within a more generic fourth option, what Eagle has described as ‘clinical theory only’ option. He has noted, “The advocates of clinical-theory-only approach reject any account or model which makes use of a causal, natural science mode of explanation, attempts to establish generalizations, and attempts to elucidate the mechanisms underlying observed behavioral phenomena” (1984, p. 147). Indeed, Eagle’s (2011, 2013, 2014) reviews of the field show how this fourth generic option represents the approach taken by numerous contemporary post-Freudian schools of psychoanalysis. I might add that occasionally psychoanalytic theorists concerned with therapeutic study may also occasionally mix non-explanatory

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44 Bowlby has proposed an interesting approach to the question of cause versus reasons/meaning raised within psychoanalysis whereby clinical concerns that accommodate hermeneutic meaning might be considered within his notion of developmental pathways, i.e. namely that a persons narrative history can be acknowledged as unique, whilst preserving the requirement for causal understanding (Bowlby, 1984). See discussions in Chapter Three and Chapter Six.

45 It may also be worth noting that an argument can be made that the vast majority of psychotherapies, including most applications of CBT, function equally without underlying causal frameworks of the sort argued for by Bowlby. This is indeed the case for the identification of disorders in psychiatry more broadly (see discussion in Murphy, 2006).
discussions alongside more scientific explanations, the result of which are less than sustainable arguments.\textsuperscript{46}

Finally, a fifth, less commonly recognised alternative has sought to retain the original empirical designs for metapsychology captured in Section 2.2. Unlike the four options just described, this approach has retained hopes for anchoring psychoanalysis to natural science in part by expanding the range of natural sciences. Indeed, Bowlby’s proposals that incorporated both systems theory and information theory in the cognitive sciences would fall under this last option (for details see discussion in Chapter Three). Indeed Bowlby (1984) identified several approaches that he be considered to parallel his own: Rubinstein (1967), Peterfreund (1971), Rosenblatt and Thickstun (1977) and Gedo (1979). Although Attachment Theory’s retention of an empirical causal metapsychology remains relatively unique, the theory has nonetheless been regularly taken up in ‘clinical-only theories’ without its metapsychological commitments.

\subsection*{2.4.3 What Exactly is ‘Psychoanalytic’ Developmental Research (PDR)?}

One final look at contemporary psychoanalysis demonstrates the influence of Attachment Theory on a broader field of infant research. Now perhaps one of the better kept secrets in the scientific world has been the emergence of an empirical subfield, what has been called ‘psychoanalytic’ developmental research (PDR), focussed particularly on the study of infants (see especially D. N. Stern, 1985). This field of study also reflects the strife present throughout the history of psychoanalysis: it has been embraced by some adherents yet equally rejected by others (see discussions in Davies, Sandler, & Sandler, 2000). My goal here is threefold: (1) to introduce briefly the nature and scope of this ‘psychoanalytic’ research work, (2) acknowledge the founding role played by Attachment Theory, (3) identify a few central psychoanalytic constructs taken up in broader more mainstream field of emotion research. However, the description here will only be introductory with most details to be taken up in later discussions of the thesis.

\subsubsection*{2.4.3.1 Nature and Scope of a Lesser-known PDR Field}

Emotional mental phenomena and their development in the context of important others have arguably been the focus of psychoanalytically informed theory and research for many decades now (for an excellent survey see Stein, 1991).\textsuperscript{47} However, for many the idea of a ‘psychoanalytic empirical’ study might sound like an \textit{oxymoron}: both for some psychoanalysts (see especially remarks by Green, \textsuperscript{46}This may indeed be the case for Cortina & Liotti (2010, 2014) whose interesting work on phylogenetic understandings of attachment and intersubjectivity are taken up in Part Three, Chapters Seven, Eight and Nine.

\textsuperscript{47}I will argue in more detail in Chapter Five that this research is actually an extension of earlier psychoanalytic work begun by adherents within both Ego-Psychology and Object Relations from the 1940s. As will be clear throughout the thesis, the 1960s and 1970s studies of infant attachment behaviour patterns conducted by Mary Ainsworth and colleagues (1963, 1964, 1969; 1978) served as one of the launch pads from which more creative infant studies would follow. Indeed, beginning with Stern’s initial studies of infant interaction (1977) and continuing up to today, for example in the work of his student Beebe (Beebe & Steele, 2013), the observation and experimentation of infants has expanded in a stunning fashion.
2000) and non-psychoanalysts alike (see the more recognisable critic Crews, 1997). Who, what and how do these ‘psychoanalytic’ researchers study? Some detail may help.

Who are these researchers? ‘Psychoanalytic’ research can mean work that is carried out by psychoanalysts. Indeed, each of the 13 researchers identified in the bottom right corner of Table 2.2 has at times accepted the psychoanalytic descriptor either for their empirical work or indicated important overlaps between their work and that of psychoanalysis. This identification is also not intended as a comprehensive list. However, whereas many in this field are practicing psychoanalysts, e.g. Stern, Tronick, Beebe, Fonagy and Target, others are not. But those who aren’t may nonetheless, typically or occasionally, publish their research in psychoanalytic journals, e.g. Main (1993), Trevarthen (2009), Crittenden (Szajnberg & Crittenden, 1997), Ainsworth (1997), and Sroufe (1982). But the breadth of this research has also been published in non-psychoanalytic journals (e.g., Steele, 2013; Trevarthen & Aitken, 2001). As regards the ‘who’ question, we have a mix of individuals with multiple allegiances, some trained psychoanalysts and others slightly more anonymous. What the group do have in common is that they are virtually all academics: developmental psychologists (PhDs) or psychiatric researchers (MDs).

So what do these researchers study? Much of the research has broadly focused on pre-linguistic infant interpersonal emotional development (for a good discussion see Beebe & Lachmann, 2002). More specifically this has included Stern’s work on infant-maternal interaction (D. N. Stern, 1977), Tronick’s face-to-face intersubjective study (1977), Trevarthen’s work on both early intersubjectivity (1980) and ‘musicality’ within communication (Malloch & Trevarthen, 2009), Beebe and colleague’s work on early micro-cross modal transactions and their predictive role for later attachment (Beebe & Steele, 2013). This work has also focussed on maladaptive development: Main’s work (1986), and later Lyons-Ruth (1987) on dysregulated affect in the context of attachment disorganisation; and Crittenden’s proposals (Crittenden, 1985; Crittenden & Liandini, 2011) for a more dynamically conceived impact of maladaptation. Mention can also be made of Fonagy and Target’s important work on metallization and the emergence of reflective functioning beyond infancy (Fonagy, Gergely, Jurist, & Target, 2002). A couple of uniting developmental themes then might be emotion and their emergence in the context of relational intersubjectivity.

Of particular interest may be the question of ‘how’ these individuals study. In common with the discipline of developmental psychopathology (for a recent discussion of this field see Cicchetti, 2013), PDR has employed both experimental and observational methods. Tronick’s ‘still face experiment’ and his video analysis of bi-directional regulation in mother infant face-to-face communication were truly innovative (see discussion of Tronick work in Lachmann & Beebe, 2015). Similar comments can be made relative to the work of Stern and Trevarthen.

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48 And as with any endeavour in the often, dysfunctional extended family of psychoanalysis, vehement protests also abound when it comes to the consideration of the mere possibility of psychoanalytic developmental research. In reviewing the proceedings of a conference called to discuss the merits of PDR, Davies et al (2000) have described the discussions as an example of “tribal warfare”.

49 Part of this innovation emerged from infant observation within some of the training curricula. The first steps emerged with the more of innovative Sander
2.4.3.2 The Founding Role of Attachment Theory within PDR

In describing the field of PDR, the Boston Change Study Process Group\textsuperscript{50} trace the beginnings of this research to the 1950s and indicate it had functioned as an important shift for psychoanalysis. They also note, “John Bowlby’s observational work emphasized the importance of the actuality of what happens with parents and children and places their actual relations in the forefront” (Boston-Change-Process-Study-Group, 2010, p. xv). Attachment Theory and PDR have maintained a fruitful relationship. Again as noted in the bottom right section of Table 2.2, each of the 13 researchers noted have embraced Attachment Theory. Indeed, Bowlby had also acknowledged commonality between his work and that of both Sander and Stern had also “searched for new theoretical models” beyond classical psychoanalytic approaches to development (1984, p. 8).

2.4.3.3 DPR and the Field of Emotion Research

An equally important second relationship exists between a subset of the core contents of PDR and the combined fields of emotion science and philosophy of emotion. Once again I will be brief. Two discussions merit mention here, the first a more direct one the second more indirect. First, Parkinson, Manstead and Fischer (2005) have provided a contrast to the more predominant study of emotions as individual or private matters in their Emotion in social relations: Cultural, group, and interpersonal processes. They propose that emotions can also be studied at three levels of sociality: the interpersonal, the group and cultural levels. Now at no place is the term psychoanalysis mentioned. However, in addition to making substantial use of Attachment Theory to discuss the socially derived emotion grief and the ontogenetic development of relational temperament (Parkinson et al., 2005, pp. loc. 3380-3891 & 4114-4163), they also engage with equal weight Trevarthen’s notion of intersubjectivity (Trevarthen & Aitken, 2001).\textsuperscript{51} Indeed, Parkinson and colleague’s work feature importantly in the next survey.

Second, Griffiths and Scarantino (2009) survey of emotion research within the field of situated cognition\textsuperscript{52} highlights four topics: social situatedness, non-conceptual emotional content, cultural scaffolding and dynamic coupling. Once again, no specific mention is made of DPR, or DP for that matter. However, without going into great detail, each of these topics finds relevant parallel discussions and empirical research at some point in time in developmental study broadly labeled DPR.\textsuperscript{53} The more obvious candidate would be the discussion of cultural scaffolding by Griffiths and

\textsuperscript{50} The Boston Change Process Study Project (2010) is an important collaborative endeavour between developmental researchers and adult psychotherapists. They are proposing a unifying model of change within psychoanalysis that connects the study of early affect development with proposals for the identification of non-interpretative mechanisms in adult psychotherapeutic change. This is clearly an encouraging response to previous lack of connection with the natural sciences in clinical only theories (Eagle, 1984). The group has identified five threads of observable shifts in current ideas of therapeutic change (2010, pp. xiii-xvi) that overlap significantly in places with situated trends in emotion research as highlighted by Griffiths and Scarantino (2009).

\textsuperscript{51} Brief mention is also made of the early studies of infant-mother dyadic interactions in works by Tronick and Stern.

\textsuperscript{52} Griffiths and Scarantino indicate that much of the work they review may not be conceived by their authors as necessarily falling within situated cognition, but that they nonetheless use this work to “demonstrate that a situated approach to emotion exists and is backed by a substantial experimental literature” (2009, p. 438).

\textsuperscript{53} In addition to this brief example of cultural scaffolding, I will suggest examples for Griffiths & Scarantino’s three other topics. In their treatment of social situatedness, they acknowledge that “some emotional behavior simply cries out for a transactional analysis” (2009, p. 440). Again as noted in the discussions above, ‘transactional’ work by Sameroff (A. Sameroff, 1975) and in collaboration with Emde (A. J. Sameroff, Emde, & Anders, 1989) has championed just such frameworks. Non-
Scarantino that cites Parkinson and colleague’s employment of *intersubjectivity*. As noted above, Trevarthen’s research has been one of the key areas of study within PDR. For now, a fairly solid case can be made that PDR has demonstrated legitimate empirical credentials particularly in non-developmental fields of emotion research. Equally, Attachment Theory has played a central role here. However, with the possible exception of Crittenden and colleagues, and Fonagy and colleagues’ work, less consideration of lifespan development beyond the first few years is considered.\textsuperscript{54} As will be noted in Part Two, Bowlby’s Goal-Corrected Partnership (GCP) beginning in the fourth year is virtually absent. (See definition and description in Section 4.2.)

To summarise, DPR is neither research conducted exclusively by psychoanalysts nor research about psychoanalytic practice. It is rather empirical research that has emerged from psychoanalytic interest in infant observation and developmental study. It both embraces Attachment Theory and traces a portion of its roots to Attachment Theory. Finally, I can conclude this subsection on Attachment Theory and contemporary psychoanalysis by re-emphasising the rather significant impact the former has had upon the latter.

**2.5 Chapter Conclusion**

Bowlby’s proposals have extended to the full breadth of classical psychoanalysis; as such, the sheer scale has been indeed truly ‘radical’. His fivefold reform encompassed new approaches to conceptions of the mind, development, psychopathology, object relations and clinical treatment. And above all, he retained a focus on causal explanation derived from the natural sciences. This causal focus remains relatively unique in the field of psychotherapy (and nosology). Ironically, his engagement with metapsychology for this overhaul was arguably resonant with Freud’s multiple employments of the term. And although his ideas would not find a welcome audience in the more classical schools of psychoanalysis, the influence of Attachment Theory on more contemporary approaches and research has been shown to be quite considerable. However, his insistence on the retention of an empirical metapsychological programme would also set Attachment Theory apart from many of the ‘clinical-only’ approaches prevalent in contemporary psychoanalysis (and psychotherapy more generally). Attachment Theory has also played an important role in the emergence of the influential field of Psychoanalytic Development Research, a sector that has provided innovative approaches to infant research. With an initial description of his proposals in place, we are now in a position to look at the second radical dimension of his programme, his commitment to a naturalist position, what I will describe as his ‘TLC strategy’.

\textsuperscript{54} I might also acknowledge the attachment longitudinal work of Main and colleagues (2005) that has importantly assessed attachment at 1, 6 and 19 years of age as perhaps an additional inclusion here as well. However, the vast majority of attachment longitudinal research would not fit the DPR label employed here. See survey of this subfield in Grossman & Grossman (2005).
CHAPTER TWO REFERENCES


CHAPTER THREE: Bowlby’s Approach: Tinbergen Meets Langer and Craik

[Bowlby’s] approach is essentially biological: he is concerned with adaptedness as well as causation, and while making full use of analogy, he does not confuse it with homology. But his biological turn does not lead him to minimise the difference between man and animals, and as a clinician he recognizes that subjective feelings have a proper place in his scheme (Hinde, 1969, p. 793).

3.0 Chapter Introduction

If Chapter Two identified Bowlby’s proposals for overhauling the content of psychoanalysis, this chapter will depict the second plank of his ‘radical’ endeavour: an empirical method integrating the mental within a biological and evolutionary perspective. I will label this as his implicit Tinbergen + Lange + Craik (TLC) strategy; it also represents a commitment to naturalistic philosophy. As will be demonstrated, conceptually speaking, Attachment Theory is principally a biological endeavour. In reviewing the 1969 publication of Bowlby’s initial volume Attachment, Hinde noted this central shift to biology away from both classical psychoanalysis—which as we have seen in Chapter 2, had in many quarters refused alignment with natural science—and behaviourism. Hinde (1969) also acknowledged Bowlby’s efforts to include mental phenomena—including “subjective feelings”—within his biological approach. However, despite decades of research and disputation, Bowlby’s theoretical writings have received neither the critical scrutiny nor appreciation they merit. Indeed, as indicated in Chapter One, any assessment and subsequent rearticulation of Attachment Theory might better include Bowlby’s original ideas and methods. What becomes clearer upon closer inspection is also the enduring value his insights may provide for current attachment discussions.

My aim in this chapter is to identify Bowlby’s approach to understanding attachment phenomena, including the implicit TLC strategy. The key components were articulated in Volume One and Volume Three of Attachment and Loss (Bowlby, 1969/1982, 1980). The TLC strategy also provides a framework within which to continue the assessment of Attachment Theory in Part One and tackle the question of the role of a Goal-Corrected Partnership in Part Two.

The chapter begins with brief summary of the Bowlby’s TLC conceptual strategy (Section 3.1). Section 3.2 discusses Bowlby’s application of each component for Tinbergen’s four questions in ethology. This is followed by a discussion in Section 3.3 of Bowlby’s views on emotion, beginning with

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54 I have selected the names of three influential individuals whose acronym reads TLC, potentially paralleling ‘tender loving care’. In doing so I do not want to ‘sentimentalise’ attachment interactions or their study. On the one hand, TLC probably parallels Bowlby’s early popular discussions of attachment as a form of ‘love’ (1953); on the other hand Bowlby also tended to employ the term less often in many of his later writings. I have been made aware that one of the first employments of ‘tender loving care’ in the English language may have originated with Shakespeare’s play King Henry the VI, hardly a text known for sentimentality. (See line 1971 from the play at http://www.opensourceshakespeare.org.)

55 Exactly which science best captures Attachment Theory may be an open question. It is probably more often described as a form of psychology, or developmental psychology. However, Holmes (2014) suggests it should be seen as a form of psychoanalysis, whereas van Rosmalen and colleagues (2014) have suggested it might better be considered as evolutionary study. I will retain a broad biological position that explicitly includes the psychological. Similar positions to my broader view are Sameroff’s (Ma, 2006; 2010) biopsychosocial, Murphy’s (2006) cognitive neurosciences and Tommasi’s cognitive biology (2009).

56 I want to clarify that in conducting a more thorough assessment of Bowlby’s thoughts here my aim is to gain conceptual lucidity and so assist my assessment of current empirical issues in Chapter Four and beyond. Bowlby’s ideas are no doubt important but not sacrosanct. They should be approached in a historical, empirical and critical manner. Indeed, several of Bowlby’s original ideas would shift with new evidence, something noted by Rutter (1995). So in turning to Bowlby, I am not endorsing the unhelpful practices common within some discussions of psychoanalysis that retain a sort of canonical place for Freud’s work and concepts with disregard for possible change from either conceptual evolution or experimental research.
his creative employment of the later work of Langer and followed by his unique proposal for a ‘felt appraisal’ within attachment phenomena. Next up (Section 3.4) is a consideration of his turn to the cognitive sciences and his innovative application of Craik’s notion of Internal Working Models (IWMs).

In Section 3.5 I will argue that Bowlby’s innovation may have been ahead of its time relative to both the field of ethology and the cognitive sciences. I argue for its under-recognition in virtually all fields of study. Section 3.6 suggests how Bowlby rejected both behaviourism and psychoanalysis, appealing instead to both an array of adjacent empirical disciplines as well as philosophy. His method—arguably, a type of philosophical naturalism—will also support both my assessment and rearticulation of Attachment Theory. Finally, I conclude the chapter with a few clarifying philosophical remarks around phenomena, theory and data in Section 3.7.

3.1 Identification of Bowlby’s Implicit Tinbergen + Langer + Craik (TLC) Strategy

Chapter One’s introduction alluded to the theoretical depth employed within Attachment Theory. Before introducing more specific details of the content of Attachment Theory in Chapter Four, I would like now to specify what I argue to be one of the key sources of profundity. Bowlby innovatively crafted a method for overhauling psychoanalysis: his implicit TLC strategy. More specifically, I want to make the case for an explicit reading of Bowlby’s more implicit employment (and combination) of three elements from three respective fields: Niko Tinbergen’s (1907-1988) ‘four questions in biological explanation’ (1963), Susanne Langer (1895-1985) ‘mind body questions in emotion study’ (1967)—something that entails a form of ‘non-reductive materialism’—, and Kenneth Craik’s (1914-1945) “working models” (1943/1967). I say implicit because Bowlby never explicitly employed such a formula in quite the way I am doing. Indeed, two of three components—(T) Tinbergen’s four questions and (C) Craik’s models—were not specified by Bowlby, nonetheless he sought to introduce ethology, emotion research57 and the cognitive sciences into developmental psychology and Attachment Theory (1969/1982, p. 37).

Briefly, the TLC strategy involves: (1) a commitment to four realms of behavioural explanation in biology (see details in Subsection 3.2 below) which includes the evolutionary continuity of behaviour between humans and other mammals (what will come to be described under phylogeny as homology);58 (2) a retention of mechanistic explanation that incorporates at least two key concepts from the cognitive sciences: control theory and IWMs; (3) an explanatory commitment to a more complex approach to biological phenomena, including positing of multi-level causal interactive explanations; (4) a rejection of both mind-body dualist and epiphenomenalist philosophical positions, that along with the prior point represents a proposal for a non-reductive materialist position or naturalism; and (5) an endorsement of a more integrated complex approach to emotions that suggests ‘felt appraisal’ processes.

57 ‘Emotion research’ in this thesis covers the study of emotion in both the field of philosophy of emotion and affective science.
58 In some ways, Bowlby’s approach might seem to contrast with Langer who is acknowledged for emphasising the distinction between humans and non-human animals.
In sum, he argued for greater integration of psychology within biological phenomena, not unlike Freud’s original Project. Indeed, Bowlby developed Attachment Theory and this TLC strategy as an interdisciplinary response for the study of development, gleaned in part from his extensive cross disciplinary experiences beginning in the 1950s—an “eclectic approach that would eventually become Bowlby’s trademark” (van der Horst, 2011, p. 159).60 Indeed, Hinde has termed Bowlby’s Attachment Theory “the example par excellence [italics original] of the fertility of an eclectic approach” (1991, p. 412).60

3.2 Bowlby’s Turn to Ethology and his Employment of Tinbergen’s “Four Problems”

From the 1950s Bowlby increasingly turned from psychoanalysis to the field of animal biological behaviour (ethology)61 (see recollections by Hinde, 2005). He would retain this ethological approach through the course of his career, as evidenced in the title of a last joint publication with Ainsworth: An ethological approach to personality development (1991). I want to suggest however that the specific role implied by the assignment of the ethology label within the theory is not adequately understood nor well acknowledged. This is particularly true for the influence of Tinbergen’s On the aims and method of Ethology (1963).62 The historian Van der Horst (2011) has sought to correct this state of affairs and has recently documented with an exceptional degree of historical detail the mutually productive intellectual encounter between Bowlby and the field of ethology. I will return to Bowlby’s work momentarily. But an initial identification of a more relevant insight from Tinbergen’s work is required.

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60 Van der Horst (2011) has documented Bowlby’s early collaborative projects from which I summarise here. Bowlby’s widely recognised eclectic theoretical approach emerged in the 1950s from his consultative work with leading global experts on behalf of the World Health Organization—Maternal Care and Mental Health (1951). Between 1953 and 1956 he would participate in the ‘WHO Study Group on the Psychobiological Development of the Child’. From 1959 to 1965 he convened and oversaw four bi-annual study groups—the CIBA Symposia—whose results were later published as Determinants of Infant Behaviour: Volumes I–IV (Foss, 1961, 1963, 1965, 1969). Finally, Bowlby extended the interdisciplinary method to the weekly Tavistock meetings he chaired in the 1950s and 1960s. Van der Horst suggests the WHO, CIBA and Tavistock channels served to expose Bowlby to what might be called a sort of ‘who’s who’ from within numerous fields. Van der Horst identifies three respective sets. From the WHO attendees he notes E. Erikson, J. Huxley, K Lorenz, M. Mead and J Piaget; from the CIBA symposia, M. Ainsworth, J. Gewirtz, H. Harlow and R. Schaffer; and from the Tavistock, R. Hinde, R. Laing, and J. Robertson (van der Horst, 2011, p. 158). As for the last set of study group meetings, Hinde has also cited the value of an extraordinary combination of “two varieties of psychoanalysts, two varieties of learning theorist, a Piagetian, psychiatric social workers, myself as an ethologist . . . nothing theoretically in common but all interested in the same problem—parent-child relationships” (1991, p. 411).

61 Bowlby’s actual formulations are somewhat more complex. He first acknowledges a theoretical breakthrough: the elucidation by analytic biology and control theory of the principles underlying the “adaptive, goal-directed behaviour”. These had in turn been exploited by ethology, experimental psychology and neurophysiology (1969/1982, p. 37). In Volume Three of his attachment trilogy, he would describe his paradigm as adopting principles derived from ethology and control theory, and replace abstract Freudian terminology to “forge links with cognitive science” (1980, p. 38). This counts as one of the rare employments of the term cognitive science. See footnote below for a more comprehensive description of Bowlby’s various formulations.

62 Bowlby also consulted closely with Harry Harlow in his experiments with Rhesus monkeys in the distinct field of comparative psychology (Harlow & Zimmermann, 1958). See discussion in van der Horst (2011, pp. 103-129)

63 Tinbergen’s influence on Bowlby can be understood as part of a post-Lorenzian 1960s ethology. This may be described as an ethology that had embraced Lehrman’s important developmental critique of Lorenz’s Theory of Instinctive Behaviour with its “problem of innateness” and the maturation of behaviour” (Lehrman, 1953, p. 341). The work of Hinde (1970) and his collaboration with Bateson (P. P. G. Bateson & Hinde, 1976) that sought to integrate ethology and comparative psychology should also be included here as part of Bowlby’s post-Lorenzian influence. Van der Horst (2011) provides a good historical narrative here. See also Burkhardt’s historical discussion of both this Tinbergen inspired shift for ethology and a mention of Bowlby’s interaction with Hinde and Bateson in this period (Burkhardt, 2005, pp. 408-446, 448). In terms of explaining ontogeny, Tinbergen supported an interactionist alternative to the inadequacies of pursuing rival, or even separate complementary, innate and learnt agendas (Tinbergen, 1963, pp. 423-427)—what philosophically might be termed strong versions of nativism and environmentalism. Ethology also brought a more complementary approach for research methods providing a place for natural observation alongside experimentation (Tinbergen, 1963, pp. 411-413). Tinbergen had also implicitly endorsed a more multi-level approach in acknowledging both neurophysiological and behavioural levels as equally valid complementary foci for research (Tinbergen, 1963, pp. 414-416).
3.2.1 Tinbergen’s Four Problems

Tinbergen (1963) introduced a degree of conceptual sophistication into the study of the biology of behaviour with his quadripartite approach to explanatory ‘problems’—what he identified as (1) causation, (2) survival value, (3) evolution and (4) ontogeny. Tinbergen’s ‘problems’ have been often recast as ‘questions’.

Questions of causation ask about the mechanism by which organisms do what they do, and questions of ontogeny ask how those mechanisms are built . . . Questions of survival value ask: whether any effect of the observed process contributes to survival if so how survival is promoted and whether it is promoted better by the observed process than by slightly different processes’ (citing Tinbergen.) [Finally,] the course of evolution is received by inferring phylogenies and homologies . . . evolutionary questions concern the historical trajectory that brought organisms to their present state . . . (2009, pp. 24-25)

The merit of Griffiths’ particular version—the inclusion of homology—will be considered below in Section 3.5 and further in Chapter Seven. For the moment, Griffith’s rendition may serve as an illustration of the interrogative rendering of Tinbergen’s approach.

3.2.2 Bowlby’s Under-Recognised Implicit Employment of Tinbergen Explanation

I want to suggest that the full explanatory power provided to developmental theory by Tinbergen’s quadripartite approach has not been deeply appreciated by attachment researchers (but see Bradshaw & Schore, 2007; Fraley, Brumbaugh, & Marks, 2005; Gillath, Karantzaz, & Fraley, 2016).

Indeed, Van der Horst (2011, pp. 98-100) appears to have been the first to have identified the presence of Tinbergen’s quadripartite explanatory tasks within Bowlby’s first theoretical volume (1969/1982). Bowlby engaged each of the four specific questions in serial fashion: evolution discussed in three chapters, followed by two chapters on causation/mechanism, one chapter each on function and ontogeny (Bowlby, 1969/1982, pp. 37-84, 85-123, 124-144, 145-172 respectively). He also demonstrated knowledge of their conceptual independence: for example, “Thus the immediate causes of a system are one thing; the function of that system is quite another” (1969/1982, p. 125). A final argument in support of this implicit case emerges from the recognition of the influence of Hinde’s

63 Tinbergen (1963) traces biology’s concern with multiple explanatory perspectives to Huxley’s position on ‘three major problems in biology’: causation, survival value and evolution. Huxley’s proposals emerged from his 1913 and 1923 studies of avian courtship (Keeley, 2000). Some 40 years later Tinbergen would introduce the fourth ‘problem’, ontogeny. Tinbergen does not mention Mayr’s (1981) influential ultimate-proximate dichotomy nor does he appear to endorse such a view. However, many writers who take up Tinbergen have (see discussion in Dewsbury, 1992). Tinbergen’s earlier work on instinct (1951) also mentioned each of the four realms but not as part of a systematic recommendation as in his later piece on biological aims and method. In this earlier work his concern was with causation of behaviour.

64 The four problems are also referred to as the “The Four Questions” or “The Four Whys” with slightly different emphases, sometimes resulting in confusion (See discussion in Dewsbury, 1992). Hogan and Bolhuis also acknowledge this conceptual muddle but suggest that the divergent employment of these four terms may be inevitable: “The best that can be hoped for is that authors and readers both realize that this is the case, and try to be as clear as possible” (2009, p. 33).

65 The two sources acknowledged above (Bradshaw & Schore, 2007; Fraley et al., 2005) each include a discussion of Bowlby, attachment and the current value of applying Tinbergen’s quadripartite framework. However, neither identifies Bowlby’s use of that framework. Main also acknowledged the four realms in a discussion of attachment avoidance, again without acknowledgement of Bowlby (1981).

66 H. Barrett (2006, pp. 39-41) has noted Tinbergen’s general influence on Bowlby in Tinbergen’s works published through 1953. She also recognises the four questions [without citation] but makes no specific mention of Bowlby’s employment of these concepts. Indeed Barrett would appear to be less familiar with the evolution of Tinbergen’s thoughts and makes no mention of his important 1963 essay on ethology. She points to criticisms of Tinbergen’s distinctive views on ‘fixed action patterns’ but fails to recognise his later shift regarding these very questions for development (Barrett, 2006, p. 41).

67 My breakdown of Bowlby’s (1969/1982) approach to the four questions differs slightly from that proposed originally by Van Der Horst (2011, pp. 98-100). He has included only the discussion on Environment of Evolutionary Adaptation (EEA) for the realm of evolution. I have included Bowlby’s chapters that precede and follow as well. These focus on the attachment behavioural system, something I will include as part of a wider look at Bowlby’s approach to phylogeny.
work upon Bowlby—something that emerged from their well-documented close collaboration. Hinde had also embraced Tinbergen’s quadripartite approach in his own work (1982). However, what can be made of the relative lack of current recognition of Bowlby’s employment of Tinbergen framework? First, as far as I can verify, Bowlby never explicitly stated that he was employing the quadripartite approach. For Bowlby it may not have merited explicit mention simply because it would have been assumed to be an obvious core feature of the ethology of the 1960s. Curiously, Zeifman—a recognised developmental/attachment researcher (Zeifman & Hazan, 2008)—has produced an impressive ethological discussion of research on human infant crying based on the Tinbergen quadripartite framework; she would appear to have been unaware of the similar conceptual approach by Bowlby that I am proposing (Zeifman, 2001). Second, the Tinbergen approach would also recede from view post-Bowlby with the decline of ethology and subsequent rise of sociobiology (Wilson, 1975) and evolutionary psychology (Buss, 1995), neither of which arguably paid full notice to the four realms (see discussion in Griffiths, 2008). Indeed one potential additional source of conceptual confusion may also be the general inclusion of Attachment Theory into the very field of evolutionary psychology (Simpson & Belsky, 2008) with little visibility of phylogenetic history. Bateson has also reflected the more general disappearance of the quadripartite framework when he argues for a belated retrieval of those four realms within ethology (2003). Indeed, it appears to be in the midst of a recent renaissance. Consequently, Bowlby’s implicit use may have not been recognised by later attachment commentators less familiar with the history of ethology (note again the exception being the historian van der Horst, 2011). Finally, a more general question may be in order as well. How familiar have commentators been with Bowlby’s actual writing? There may arguably exist some likelihood that people simply may be less familiar with Bowlby’s original work. Indeed, Bowlby’s dense three-volume theoretical work would

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68 Although Bowlby and Hinde never collaborated in publishing their thoughts, both have acknowledged their intellectual cooperation (for Bowlby’s acknowledgement see Bowlby, 1988; for Hinde’s acknowledgement see van der Horst, van der Veer, & van Ijzendoorn, 2007). See also van der Horst for a detailed review of the historical sources that reflect this lifelong partnership (2011). At the same time, Bowlby and Hinde’s ideas were not always identical. In fact Hinde’s ideas for the complex development of human relationships (Hinde, 1982, pp. 204-211) would appear more robust and current than Bowlby’s earliest ideas. See discussion of Bowlby’s ideas and attempts to account for the phenomena of complexity in human relationships within Attachment Theory below.

69 Hinde would employ the four realms in his major work on ethology, but with evolution and development grouped together as a single historical question (Hinde, 1970). His follow-up publication in the Fontana Masterguides Series gives a central position to each of the “Four Whys”—devoting a chapter to each—: he describes the relation between the four as “Core Ethology” (1982, pp. especially 19-21, 128-132).

70 Bowlby’s 1969 work is referenced here but without any recognition of his employment of Tinbergen. After the conclusion to her article, Zeifman added, “I am indebted to Steve Robertson for initially suggesting the organizing framework of this article.” It appears that this might be a reference to the employment of Tinbergen in her article. Bowlby’s use of Tinbergen framework would appear to not have been obvious, even to an attachment researcher who herself has made an explicit employment of that framework.

71 The question of an appropriate configuration of evolutionary psychology within Attachment Theory will be addressed in Part Two.

72 Although the influence of classical ethology may have waned, Manning has argued not so the Tinbergen explanatory framework (2005). He observes, “I have yet to meet anyone who does not accept that Tinbergen’s four questions for the study of behaviour—function, evolution, causation and development—are the right ones” (Manning, 2005, p. 289). Allusions to and discussions of the ongoing conceptual relevance of his four questions have appeared more recently within the fields of evolutionary psychology (Pellegrini, 2008), evolutionary developmental biology (Callebaut, Müller, & Newman, 2007), combined looks at development and evolution (P. Bateson & Gluckman, 2011), comparative psychology (MacLean et al., 2012), philosophy of biology (Griffiths, 2009) and philosophy of psychiatry (Geerts & Brüne, 2011). Indeed Geerts & Brüne have discussed evolution and ontogeny in light of Attachment Theory and psychopathology; however, they do not consider a use of four questions by Bowlby (2011, pp. 121-122). Badcock has employed the quadripartite framework to propose an ambitious, evolutionary systems theory for psychology (2012).
appear to be rarely quoted directly or even paraphrased indirectly, except perhaps by a few more engaged researchers (see for example Petters & Waters, 2010; Rutter, 1991). Researchers may make vague or more general reference to his work and may rely on access to his ideas indirectly through later commentators. I will hold off for the moment on specifics but will identify examples throughout the thesis.73

3.2.3 The Explanatory Task of Integration

I want to conclude this somewhat lengthy but necessary excursion into Tinbergen’s framework with a glance at the task of integration, something he considered equally deserving of attention in “a comprehensive, coherent science of Ethology” (Tinbergen, 1963, p. 411). As mentioned above, Tinbergen provided little guidance beyond his simple exhortation to integrate. Hinde has optimistically suggested the relations between the four questions comprised “core ethology”, and organisation of content under each realm was “not necessarily independent: they can be both interdependent and interfertile” (1982, p. 128). However, Laland and colleagues suggest the lack of integration has left the approach vulnerable to unhelpful deterministic ultimate-proximate readings and therefore a less useful tool (Laland, Odling-Smee, Hoppitt, & Uller, 2013, p. 728).74 For the moment we may have to simply acknowledge we lack a more formal path of integration, if indeed one might exist. As mentioned earlier, Zeifman has applied Tinbergen’s approach to infant crying the result of which is an impressive ethological discussion. She concludes, “Considering all four of Tinbergen’s questions at once highlights the interrelations and ‘fuzzy’ boundaries between them and also accentuates the manner in which each individual question informs the others” (Zeifman, 2001, p. 280). In this sense, Bowlby’s quadripartite endeavour may count as an integrated effort as he approached each question seriatim just as Zeifman has done.

Some caution is in order when approaching Attachment Theory from an ethological perspective. I will argue that a foundation in Tinbergen’s four questions provides a robust path to scientific explanation. However, two sorts of risks lurk. Unrealistic demands for concluded theoretical explanations (Kagan, 2011) would seem misguided. Such requests would appear to fail to consider the nature of unresolved questions in philosophy of biology and might easily lead to an unreasonably harsh assessment that the theory simply counts as an unproven hypothesis. Alternatively, any re-articulation

73 Hacking has made a similar observation for certain writers’ superficial engagement with Freud (1998).

74 The topic of a possible path for an integration of Tinbergen raises an interesting historical question about possible relevance of Aristotelian discussions of causation. Falcon (2012) has noted that Aristotle’s four causes—material, formal, efficient and final—can be similarly understood as why-questions that request a “certain type of explanation”. She suggests that in his theory of causality, Aristotle “explores the systematic interrelations among the four modes of causality and argues for the explanatory priority of the final cause” (Falcon, 2012, p. 13). As mentioned, specificity of integration of an Aristotelian sort is missing from Tinbergen. Aristotle’s programme proposed a necessary and sufficient model for causation. Tinbergen makes no such ambitious claims. Indeed, Dewsbury has suggested that discussions of Aristotle’s notions of causation are not immediately germane in his review for the various discussions of approaches to Tinbergen (1992, p. 91) but more relevant for discussions of ultimate and proximate distinctions (1999, pp. 189-190). But again see Laland and colleagues for a different perspective here (Laland et al., 2013, pp. 728-729). Hogan and Bolhuis suggest of the four Aristotelian causes, “only the efficient cause corresponds to Tinbergen’s usage” (2009, p. 28). Young (2011) has directly compared the two models in a chapter in his recent book focussed on developmental causality. He has produced a table that includes a comparison of Aristotle and Tinbergen as well as more contemporary developmental approaches. Young has suggested that Tinbergen “related his four questions to Aristotle’s four causes” but suggests he has found “Tinbergen’s parallels wanting” (2011, pp. 657-660). Unfortunately, Young’s discussion here disappoints. What is presented as a proposed Tinbergen approach to Aristotle is not supported by the cited reference (Tinbergen, 1963). Indeed, Tinbergen would appear to have made no such comparison. What Young does offer is a helpful collection of useful contemporary approaches to complex development. His success in producing a thoroughgoing synthesis for developmental causality may be less certain.
of the theory must take on board lessons from the theory's history and not inflate ambition beyond reasonable limits (see note on this ongoing risk in Hinde, 1991). The structure identified in the opening chapter for Part Two includes a chapter covering each of the four explanations as applied to the question of language and attachment. I will argue that to explain phenomena within Attachment Theory, one must possess a reasonable grasp of the explanatory content within each quadrant, a modicum of clarity around the theoretical boundaries that separate them, and where needed some sense of how the four may relate to one another.

I now turn to the second major component in Bowlby’s theoretical innovation.

3.3 Bowlby’s Curious Turn to Langer and Philosophy of Mind/Emotion

One of the more sophisticated and philosophically interesting aspects of Bowlby’s conceptual work is his chapter ‘Appraising and Selecting: Feeling and Emotion’ (1969/1982, pp. 104-123). Bowlby engages issues in philosophy of mind relevant for attachment phenomena including mind-body relationship, the nature of emotion and mental causation. He turns to the later work of Langer in seeking to establish how attachment phenomena might be understood as felt processes within an appraisal framework. His ideas also anticipated the more complex views of emotion found in today’s current research.

3.3.1 Langer’s Mind: An Essay in Human Feeling

Although Susanne Langer is best for her pioneering work in the field of philosophy of art (1953, 1957), late in her life she later turned her philosophical focus to an ambitious three volume project: Mind: An essay in human feeling (1967, 1972, 1982). This undertaking sought to ground human organismic ‘feeling’—a term that for her encompassed all conscious mental experience—within a fundamental biological worldview.75 Indeed, I would suggest her ideas might be understood as sharing some commonality to those who have employed the notion ‘cognitive biology’, especially Millikan (1984) and Tommasi and colleagues (2009). Bowlby quoted Langer when introducing relevant philosophical problems he raised in considering attachment phenomena as ‘felt appraisals’:

The vexing question in the philosophy of the biological sciences is how something called ‘feelings’ enters into the physical (essentially electro-chemical) events that compose an animal organism . . . The fact that we feel effects of changes in the world about us, and apparently in ourselves, too, and that all such changes are physically describable, but our feeling them is not, presents a genuine philosophical challenge (Langer (1967) as cited by Bowlby, 1969/1982, p. 106).

Innis has described Langer’s three-volume work, a “naturalist, but ‘non-reductive’, metaphysics of the mind” (2007, p. 5). I suspect it is this philosophical position that Bowlby found as an attractive alternative to predominant mentalism/dualism and epiphenomenalism that he rejected (1969/1982,

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75 A portion of Langer’s early work—e.g., Feeling and form: a theory of art developed from Philosophy in a new key (1953)—was devoted to the continuation of Ernest Cassier’s work on the emergence of symbol and myth in relation to human feeling. However, Langer’s final project, Mind, took a decidedly biological turn, insisting that both symbol and myth be considered evolutionary and biological phenomena. In some ways this turn might be seen to parallel expanded notions of behavioural and symbolic evolution discussed in philosophy of biology by writers such as Jablonka and Lamb (2005).
Innis has also noted “What she took from biology and psychology, her principal empirical sources, was this act-concept and the generalized notion of sentence as a phase of biological processes, not a new ‘ontological’ level” (2007, p. 11). Indeed, Bowlby introduced Langer’s notion of process when raising a practical clinical question: “How is [the clinician] to picture the relation of private to public, of subjective to objective, of feelings to physics, of body to mind?” (1969/1982, p. 108). He turned to Langer who had noted “the phenomena usually described as ‘a feeling’ is really that an organism feels something . . . Being felt is a phase of the process itself” (Langer 1967 as cited by Bowlby, 1969/1982). Bowlby candidly acknowledged that little was actually known as to how such a phase might occur—but see Subsection 3.3.3 below for a more current perspective—, but with a viable alternative to substance dualism and epiphenomenalism in hand, he could therefore turn his attention to identifying what sort of processes might attain the phase of being felt.

3.3.2 Bowlby’s Approach to Emotion as ‘Felt Appraisal’

Bowlby’s chapter ‘Appraising and Selecting: Feeling and Emotion’ (1969/1982, pp. 104-123) constitutes a relatively brief but dense discussion. As noted, I might also suggest that it may count as one of the more innovative but the less noticed chapters. He clearly demonstrated a comfort with discussions in philosophy of mind and philosophy of emotion. His approach to emotion in the context of attachment began with Langer’s notion of process. Part of his innovation emerged by combining the felt aspect from Langer’s process with Magda Arnold’s (1903-2002) early cognitivist work on emotion appraisal (Arnold, 1960a, 1960b). However, he did so with one proviso: unlike Arnold, he sought to capture attachment emotional phenomena in terms of control theory. Consequently, his approach would introduce a possible degree of dynamic complexity—something present in Langer’s work—not that found in Arnold’s discussion. Without going into great detail, Bowlby identified at least six aspects for consideration in the understanding emotion both generally and in the context of

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76 For a recent survey of her thought, see Innis (2009). Bowlby would both agree and disagree with Langer’s perspective. On the one hand, in addition to the points mentioned in the text, her focus on act as interaction between organism and environment, the organism as an open system and the need for dynamic approaches, and the central importance of language would all find favour with Bowlby. However, when it came to the tension in approaching the evolutionary relationship between human and non-human animals—i.e. the two poles of continuity and discontinuity—Bowlby would probably emphasise more the former where Langer would the latter.

77 Her work also inspired a similarly named work on clinical approach to psychotherapy by the British psychiatrist and psychoanalyst Robert F. Hobson, Forms of feeling: The heart of psychotherapy (1965).

78 Langer’s choice of the term ‘process’ might be misunderstood here. Although she studied under and was influenced by Whitehead (Dryden, 1997b), she was also quite clear in establishing that she was not a ‘process philosopher’ and rarely made reference.

79 Control theory introduced important conceptual leverage for Bowlby to address complex biological process. Although not cited by Bowlby, Langer had been prescient in suggesting that causal relations in biology might be more complex. Indeed, Dryden has noted that for Langer, “the analysis of biological activities in terms of linear sequences of cause-and-effect relationships, while not impossible, is often irrelevant to understanding their more complex causal architecture” (2007, p. 42m46). Bowlby also suggested that mechanisms for emotion within attachment phenomena might also one day be understood in very similar terms (1969/1982, p. 109n101).
attachment: process, feeling, multi-level physiology, appraisal, motivation for action, and expression. In short, Bowlby acknowledged a causal role for appraisal: “Appraising processes of which feeling may be a phase undoubtedly play a causal role. To what extent and in what way feeling itself plays such a role remains undemonstrated” (Bowlby, 1969/1982, p. 118). Bowlby remained quite cautious, not overstating conclusions in the absence of empirical data, something he would do throughout his work. He also noted the need for conceptual rigour when discussing the emotions. “When the language of feeling becomes an obstacle to recognising that feeling entails action of a particular sorts, it is best abandoned and replaced temporarily by a language of behaviour” (Bowlby, 1969/1982, p. 123).

3.3.3 Bowlby’s Ideas in Light of Current Evidence for Component and Dynamic Process Approaches to Emotion

Bowlby acknowledged that his approach to a felt process of appraisal was “no more than a sketch” and probably raised more questions than provided answers. This reflected the state of emotion research in 1969. However, two similarities with more empirically supported current research are noteworthy. On the one hand, Bowlby’s approach would appear to be partially encompassed within specific features in component process models (CPM) whose components include appraisal, physiology, feeling, tendency to action and expression (Scherer, 2009). On the other hand, his consideration of control theory in terms of the process of emotions aligns well with research suggesting more dynamic, unfolding patterns for emotion (Grandjean & Scherer, 2008).

Once again, in ascribing the ‘Langer’ label to Bowlby’s strategy, the emphasis is less one of her specifics—unlike my employment of Tinbergen and Craik. Langer’s approach to action and process clearly remains valuable for understanding appraisal processes. But it was Bowlby’s emphasis on the need to be conversant in philosophy of mind and emotion research—both affective science and philosophy of emotion—that embodied his turn to Langer. I now turn to the third component in his strategy, Craik’s contribution from cognitive sciences.

3.4 Bowlby’s Implicit Application of Craik’s IWMs

The last component of Bowlby’s innovative triad was the grafting of the cognitive sciences60 onto the Tinbergen framework, especially what I will call his implicit employment of Craik’s notion of internal working models (Craik, 1943/1967).61 Johnson-Laird has summarised Craik’s idea quite succinctly: “Thinking is a manipulation of internal representations of the world” (1983, p. x). I want to suggest

60 A word about the cognitive sciences may be useful here. I will adopt Boden’s dual approach for defining the field: “Cognitive science is the interdisciplinary study of mind, informed by theoretical concepts drawn from computer science and control theory” (2006, p. p.12). Hence she acknowledges two main types of computation drawn from each of the latter two fields. However, she also accepts that her broader view “carries a health warning” as it may not be shared by all in the field. Some “purist symbolists” may prefer to restrict computation and exclude cybernetics (Boden, 2006, p. 13). I have also chosen the plural terms cognitive sciences, something that is also not uncommon. My interest is to highlight the current disciplinary breadth within the field, something the choice of the plural reflects. The singular use has been associated with a conceptual emphasis on the more integrative aspect within cognitive science (Boden, 2006, p. xxxv and 13). See also discussion below.

61 Bowlby will rarely employ the coupled terms cognitive science(s) in his work, though he makes explicit use of conceptual and empirical work that may be rightly seen as emanating from within the cognitive sciences. As Bretherton has noted (1999, p. 343), Bowlby would also never cite Craik directly. Bowlby would adopt Craik’s IWMs via Young’s A model of the brain (1964, pp. 125-137)—a work inspired by the study of the octopus brain.
here that the nature of Bowlby’s conceptual turn to the field of cognitive sciences has also been neither well acknowledged nor adequately assessed. I will be relatively brief and delay fuller discussion of IWMs to Chapters For and Eight.

3.4.1 Bowlby’s interdisciplinary Combination of the Cognitive Sciences

Similar to his turn to ethology, aspects of the cognitive sciences are routinely recognised as part of Bowlby’s synthesis (Bowlby, 1979/2005a, pp. 151-152; Mikulincer & Shaver, 2007, p. 4). However, there are actually several conceptual moves into the cognitive sciences by Attachment Theory.

Control theory figured quite heavily in descriptions of behavioural systems—a phylogenetic concept taken from ethology (Bowlby, 1969/1982, pp. 40-50). Piaget’s ideas on ego development for the cognitive development of the child also informed questions of attachment ontogeny (Bowlby, 1969/1982, pp. 350-354). Information theory was called upon to provide a functional explanation for defensive exclusion, a potentially suboptimal if not maladaptive adaptation (Bowlby, 1980, pp. 40-74). Finally, the notion of IWMs was taken up to explain the underlying causal mechanisms supporting the operation of behavioural systems, especially in his later theoretical work (Bowlby, 1988, pp. 119-136, 158-179). This last employment will be of greater interest for the thesis’ exploration of Bowlby’s TLC strategy.

3.4.2 Bowlby’s turn to Craik’s Working Models

Bowlby’s liberal use of concepts from the cognitive sciences is also less systematic and evolved across his five major theoretical works and collections. When it comes to Craik’s specific notion of mental models, Bowlby variously employed the terms models, mental models, working models, representational models and internal working models (1988, p. 120 asterisk). Craik had noted, “Human thought has a definite function; it provides a convenient small-scale model of a process so that we can, for instance, design a bridge in our minds and know that it will bear a train passing over it instead of having to conduct a number of full-scale experiments; and the thinking of animals represents on a more restricted scale the ability to represent, say, danger before it comes and leads to avoidance instead of bitter experience” (1943/1967, p. 59). As will be shown, post-Bowly attachment approaches to the cognitive sciences may have paid less attention to Bowlby’s actual work. So when it comes to the cognitive sciences, both the breadth of Bowlby’s employment and their explanatory importance may be less appreciated (for possible exceptions see Bretherton, 1992; P.

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82 Bowlby’s formulations of the configuration of component disciplinary fields that support Attachment Theory may differ slightly. Here are three examples: (1) analytical biology, control theory, ethology, experimental psychology and neurophysiology (Bowlby, 1969/1982, p. 37), (2) ethology, control theory, cognitive psychology, neurophysiology and developmental biology (Bowlby, 1979/2005a, pp. 151-152), and (3) control theory, information processing, ethology and comparative psychology (Bowlby, 1988, p. 59). The research tradition that follows Bowlby often presents the theory’s foundation in summary fashion as a sort of theoretical stir-fry with variable mixes of disciplinary ingredients that have grown over time (see both introductions and various chapters in the two attachment handbooks: Cassidy & Shaver, 1999, 2008).

83 This appears in what Ainsworth and Bowlby would describe as one the more foundational chapters of Bowlby’s trilogy (Ainsworth & Bowlby, 1991, p. 399).

84 Here is a summary of the relevant terms employed within subject indices of the five key theoretical sources: (1) control theory, cognitive maps, working models, Piagetian theory (Bowlby, 1969/1982), (2) working models and Piagetian theory (Bowlby, 1973), (3) cognitive psychology, control theory, and models of world and self, (Bowlby, 1979/2005a) [this is a collection ranging from the 1950s-1970s], (4) cognitive psychology, control theory, information processing, Piagetian theory and representational models (Bowlby, 1980) and (5) cognitive psychology, control systems, information processing and representational models (Bowlby, 1988) [this is a collection of addresses/articles from 1979 to 1980].
Attachment Theory has also not typically been recognised as subject matter for the cognitive sciences. From an historical point of view, Bowlby’s most fruitful theoretical period—1969-1988—would have spanned both sides of the emergence of the cognitive sciences in the 1970s. In discussing the pivotal 1975 Chomsky-Piaget language and learning debate near Paris, Gardner has suggested that “the field of the cognitive sciences at Royamount seldom tackles issues of affect or motivation and is dubious of laboratory tinkering of academic psychology” (1980, p. xv). Affect and motivation were matters associated with the declining fields of psychoanalysis and behaviourism and hardly of interest at this point in time (Gardner, 1980, pp. 136-137). Indeed, the index to Boden’s two-volume work, Mind as machine: A history of cognitive science (2006), includes numerous entries for ethology, development, developmental psychology, and psychopathology in several contexts but makes no mention of either Bowlby or Attachment Theory.86

3.3.4 The Relative Prominence of Craik’s Ideas in the Cognitive Sciences

The value of Craik’s work can be seen in its expansion by Johnson-Laird (1983) and colleagues (Khemlani, Barbey, & Johnson-Laird, 2014). As an acknowledged pioneer of cognitive science, Craik also warrants considerable space within Boden’s history. She provides a useful look at Craik’s consideration of mental illness within his reference to “recurrent patterns in reality.” I will quote both her summary and her citation of Craik in full.

The star in one’s eye, whether literal or idiomatic, may of course be illusory. Craik grasped the nettle of non-veridical representation. He argued that perceptual illusions, errors in abstract thought, false beliefs,

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85 However, as Johnson-Laird has indicated, it is not just attachment researchers who neglected the IWM concepts: “This deceptively simple notion has rarely been taken sufficiently seriously by psychologists, particularly by those studying language and thought” (1983, p. x).

86 What is most crucial for attachment theory is the potential inclusion or exclusion of more affective subject matter within the cognitive sciences. Without going into explanatory detail, I should acknowledge that this dimension has been the subject of great debate within the cognitive sciences, philosophy of emotion and the field of psychotherapy. Bowlby would operate under an assumption of inclusion where both felt and appraised concepts are considered together (1969/1982, pp. 104-123), something I will raise again in Chapter Six.

87 In discussing resistance to the 1978 Sloan Hexagon definition that identified six key domains—one of which is psychology—and eleven subdomains, Boden noted “virulent disagreement” reminding readers, “there’s still no agreed definition of the field” (2006, p. 522).

88 I will return to Boden’s work later in the thesis to argue that indirectly most of the key developmental systems concepts and cognitive mechanisms at stake within Attachment Theory can indeed be located within cognitive sciences.
and much of psychopathology also depend on adaptive modelling of the external world. But in all cases the models are skewed, often rigid, and counter-productive. For example: '[Hysterical conduct] is a form of adaptation . . . achieved by narrowing and distorting the environment until one’s conduct appears adequate to it, rather than by altering one’s conduct and enlarging one’s knowledge til one can cope with the larger real environment. Dissociation and schizophrenia and repression are further mechanisms for attaining this splendid isolation and pseudo-adjustment and of excluding difficulties and awkward suspicions (1943, p. 90)’ (Boden, 2006, p. 213).

An argument can be made for the commensurability of Craik’s model based description of hysterical conduct to Bowlby’s approach to rigid behaviour within disordered personality, some 25 years later.

### 3.4.5 Pivotal Role for the Cognitive Sciences in Attachment Theory

The cognitive sciences provided a pivotal component—namely, causal mental mechanisms—that have underpinned Attachment Theory. Indeed, this is also more the case now that the study of emotions are increasingly being integrated within the cognitive sciences. That Bowlby has taken up the notion of cognitive mechanisms or internal working models from the cognitive sciences has indeed received a degree of attention by attachment researchers (Bretherton & Munholland, 2008). Petters & Waters have also suggested that Bowlby “may have referred to ideas in Control Systems Theory and AI as much to break the old Freudian paradigm than to specify in great detail a way forward to more involved modelling and simulation” (Petters & Waters, 2010, p. 55). However, this attention has been perhaps less proportionate to the seemingly critical role it might need to play in supporting the enduring lifespan ontogenetic dimension within Attachment Theory. “As a metaphor, [the internal working model] is extremely powerful, in fact too powerful, because it will explain anything. If it is to be useful in the long run, and it may well be, much more detailed work will be necessary” (Hinde, 1991, p. 415). Some of that work has begun (Marvin & Britner, 2008). However, as will become apparent, discussions of Attachment Theory and the cognitive sciences have lacked a degree of sophistication. Chapter Eight will raise the influence of more current discussions of mental models in cognitive sciences for understanding Goal-Corrected Partnership.

### 3.5 The Uniqueness of Bowlby’s TLC Formula

Bowlby’s combination of ethology’s biological study of behaviour with an explicit consideration of both emotion from an inchoate field of emotion research and mental representation as studied in the

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89 The field of emotion research has exploded in the last decade. An illustration of the extraordinary expansion of emotion study is the emergence of the journal Emotion Review in 2009. “The aim of the journal is to publish theoretical, conceptual and review papers (often with commentaries) to enhance scientific understanding of emotion theory and research. It accepts papers from a wide disciplinary spectrum — wherever emotion research is active.” [http://emr.sagepub.com/ accessed 23 April 2015.] Most issues include a closing feature: ‘Views From a Discipline: Current Research in [named topic]’. A quick glance at the table of contents in Volumes 5 and 6 (i.e. 2013–2014) reveals the following topics: ‘Behavioral Neuroscience’, ‘Philosophy’, ‘Developmental Psychology’, ‘Anthropology on the Field’, ‘Linguistic Anthropology’, ‘Health Behavior Science’ and ‘Cognitive Neuroscience’.

90 Both arguments can be gleaned from a statement made by Bowlby and Ainsworth in their last joint publication where they identify an overlooked chapter that highlights the foundational or “basic” position played by the cognitive sciences in the theory: “The third volume of the trilogy was concerned with loss (Bowlby, 1980). Near the beginning of it he included one of the most basic chapters of the trilogy—entitled ‘An Information Processing Approach to Defence’—that is as pertinent to the earlier two volumes as it is to the third” (1991, pp. 7-8). As Fonagy also points out, Bowlby’s later work would be “increasingly influenced by cognitive psychology” (2001, p. 15).

91 Engagements with cognitive sciences in the attachment discussions may tend to lack depth. For example, Fonagy and Target (2007) make a helpful appeal to the cognitive sciences—especially embodied cognition—as a potential empirical platform for a proposed integration of Attachment Theory and psychoanalysis. These researchers have introduced an important degree of empirical rigour to psychoanalysis. However, their employment here of both Bowlby and the cognitive sciences would appear to possess some disputable positions. Robbins and Zacks (2007) have provided an incisive review of the Fonagy and Target reading of cognitive science. However, I am not aware of any response by either Fonagy or Target to the response.
cognitive sciences was truly innovative for his time, both for the study of humans (Archer, 1992, pp. 8-9) and for their near and distant animal relatives (Hinde, 2005). That innovation can be understood in terms of its initial originality; neither field had made such claims at the time nor would have been open to such considerations. Second, the explanatory value of the conceptual move can be evidenced by a more mainstream take up of such a combination by later proponents from both fields.

3.5.1 An Initially Unwelcomed Integration

Bowlby’s strategy would have hardly been welcomed by either the more settled field of ethology, where Tinbergen served as lead conceptual spokesperson, or the emerging discipline of cognitive science. C. Allen and Bekoff have suggested, “Classical ethology arose in an era when the scientific pendulum had swung hard against theorizing about the mental states of nonhuman animals” (2005, p. 126). Indeed, Shettleworth (2012) suggests that when it came to studying specific matters of animal mental mechanisms Tinbergen was closer to the behaviourist leanings common to comparative psychology. She reminds her readers that the study of animal mental life per se was not particularly unusual; however, Tinbergen would have clearly rejected such a move for 1960s ethology. She quotes Tinbergen: “Because subjective phenomena cannot be observed objectively in animals, it is idle either to claim or to deny their existence. Moreover, to ascribe a causal function to something that is not observable often leads to false conclusions” (Tinbergen, 1951, p. 4). His position remained the same more than a decade later (Tinbergen, 1963).

Boden has also noted that combining animal study with cognitive research was equally unacceptable: “In the 1950s and early 1960s, … the ethologists weren’t invited to the cognitive tea parties” (2006, p. 257). Cognitive scientists following a more representational approach were hardly concerned with the possibilities for animal minds (but see a discussion of Arbib’s 1960s interest in frogs’s eye research by Boden, 2006, pp. 1169-1177). The originality of Bowlby’s combination can be supported by its initial unattractiveness to both ethology and cognitive science (including emotion) cited here.

3.5.2 A Strategy Ahead of its Time: Ethology Embraces the Mental and the Human

Tinbergenian evolutionary studies in animal behavior have been contrasted with those inspired by Darwin’s 19th Century approach that emphasized mental continuity across species (Shettleworth, 2012). Indeed later developments within ethology have been more open to mental considerations; such approaches may serve as further evidence for the original insightfulness of Bowlby’s formulation. Two discussions within ethology, each with slightly different states of resolution merit recognition: cognitive ethology and human ethology. Cognitive ethology extends the functional subject matter of ethology to the mental; human ethology expands the target species for study. In both cases Bowlby’s

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92 I know of only one discussion that has acknowledged Bowlby’s explicit combination: as an early case combining ethology and cognitive psychology, but not Tinbergen + Craik. Bradshaw and Schore cite the past work of both Tinbergen (1951) and Bowlby (1969) in suggesting “the integration of ethology and psychology is nothing novel” (2007, p. 425). They do not however, specify the cognitive sciences or more specifically working models.

93 Both Kruuk (2003) and Manning (2009) have concurred with Shettleworth’s reading of Tinbergen here.

94 Boden notes Arbib’s exceptional AI work: especially his *rana computatrix* (1972) that built on the important late 1950s neurophysiological observations of feature detectors in the frog’s retina (Lettvin, Maturana, McCulloch, & Pitts, 1959). Arbib’s work predated the more recognised emergence of Computational Neuro-Ethology (CNE) as discussed below.
work may be considered influential, albeit in differing ways.

Attachment Theory had already suggested what later cognitive ethological theorists would next propose.\(^{95}\) Whereas the theory could be considered a mainstream component in classical ethology (Hinde, 1982), the theory has been curiously missing in some of the cognitive ethology discussions.\(^{96}\) Neither attachment nor Bowlby appear in the index of C. Allen and Beckoff’s important critical approach to cognitive ethology, *Species of Mind* (1997). However, in Bekoff and Pierce’s later *Wild Justice*, social attachments feature prominently in discussions of animal empathy (Bekoff & Pierce, 2009). Equally, C. Allen’s more recent formulation of a general definition for cognitive ethology reads: “Perhaps the simplest characterization of cognitive ethology is that it is the marriage of ethology and cognitive science” (2004, p. 590). This would seem to parallel in near exact fashion what I claim for the implicit method underlying Attachment Theory.

In the late 1970s, about the same time that cognitive ethology was first emerging, the field of human ethology was also finding traction. Two sets of academic conversations of human ethology were published in 1979; both give extensive attention to Bowlby and Attachment Theory. Ebil-Ebelsfeldt’s target article and accompanying discussion in *Brain and Behavioral Science* (1979) introduced the relatively new discipline in that journal’s second year.\(^{97}\) Bowlby’s work also features prominently in the second publication which featured papers and responses from more than thirty participants at a 1975 symposium on human ethology (Von Cranach, Foppa, Lepenies, & Ploog, 1979). These later developments in both sub-fields testify to the lasting nature of Bowlby’s innovation for ethology.

### 3.5.3 A Strategy Ahead of its Time: Cognitive Sciences Embrace the Animal

Since the 1980s, to reuse Boden’s metaphor, ethologists may have been granted entrance to the cognitive tea party. Boden describes the eventual emergence of Computational Neuro-ethology (CNE) fashioned in part on the success of Arbib’s modeling of animal learning and prediction (2006, pp. 1169-1177 and 1286-1299). Boden suggests that “What Craik had termed models, [Arbib] termed schema”. Once again, we have an example of a blending of cognitive science with ethology not unlike that first proposed in Attachment Theory. Keeley looks more closely at the critical issues at stake in arguing that CNE might indeed count as a proper integration of the cognitive science with the wider ethological study of animal behavior as captured in the field of neuro-ethology. “In order to make my case for CogSci as the computational neuroethology of humans, I need to make a case for the role of evolutionary biology in the study of cognition.” (Keeley, 2000, p. S409). Indeed, a closer look at

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\(^{95}\) Explicit discussions of the possibility for a cognitive ethology emerged with Griffin’s (1976) controversial introduction of a proposed study of animal intentionality and consciousness. This postdates Bowlby’s first major theoretical articulation but received no mention in his second edition (1969/1982). Discussions for an appropriate candidate for some form of cognitive ethology have expanded in less controversial directions and will be taken up again in Chapter Six.

\(^{96}\) Cognitive Ethology has focussed primarily on questions regarding the mental capacities in non-human animals. Attachment Theory as applied in the context of other animals would appear to be potentially relevant for these more contemporary discussions. I will for the moment hold off on a discussion of Burghardt’s curious suggestion for adding a fifth aim to Tinbergen: namely, private experience (1997). This suggestion was made in a 1997 collection on animal mental life, *Anthropomorphism, Anecdotes and Animals* (Mitchell, Thompson, & Miles, 1997). Again, neither Bowlby nor Attachment Theory is mentioned in the index for the collection of 29 articles.

\(^{97}\) Ebil-EBelsfeldt suggested the field emerged in the mid 1960s and includes the attachment work of Bowlby and Ainsworth produced in that time. A less sympathetic commentator correctly pointed out that Bowlby’s first published work on ethology and attachment in 1958 the decade before may indeed have been one of the first (Schubert, 1979).
Keely’s discussion of the value of embracing ethology for cognitive science—multi-level approaches to explanation, homology, and continuity/discontinuity of species—gives an impression of deep similarities with Bowlby’s combinatorial approach.

In one sense, Bowlby chose wisely. Each part of the TLC component has retained an influential position in their respective fields of ethology (and biology), emotion research and the cognitive sciences. But it was not the mere inclusion of three promising conceptualisations but also their insightful integration that best evidences his conceptual innovation.

3.6 Bowlby’s Method and the Potential Value for a Re-articulation of Attachment Theory

In Section 2.1 above, I indicated four possible empirical/philosophical views emerging from Bowlby’s TLC strategy: evolutionary mental continuity, complex explanatory frameworks, rejection of the two predominant views on the mind-body problem and a more integrative approach to emotions as felt appraisals. I would now like to highlight the method of investigation he employed (Subsection 3.5.1). Bowlby might appear to address attachment phenomena by engaging in a sort of philosophical naturalism—an expression I will also briefly describe. The philosophical and empirical discussions from the 1950 and 1960s with which he engaged are obviously dated. However, as I will show, today’s more current discussions in some of those very same fields offer a range of new possibilities for Attachment Theory. And so in a last Subsection 3.5.2, I will suggest how a similar move to similar fields today might specify how one might reapply that method in seeking to rearticulate attachment theory.

3.6.1 Bowlby’s Eclectic Naturalistic Method

Inserted within Bowlby’s Tinbergen quadripartite exposition (Chapters One to Ten in Bowlby, 1969/1982) sits a philosophically informed discussion of emotions and mental causation, “Chapter Seven: Appraising and Selecting: Feeling and Emotion” (pp. 104-123). Whereas Bowlby engaged ethology and issues in philosophy of biology in his turn to Tinbergen as discussed above, he ventured explicitly into new realms of philosophy in this chapter. Again what is particularly striking here is Bowlby’s brief but nonetheless solid familiarity with issues in philosophy of mind and philosophy of emotion. That he may usefully be termed a philosophical naturalist requires some elaboration as this appellation lacks a consensus definition (Papineau, 2009).

I consider Bowlby a philosophical naturalist for at least two reasons. First, Bowlby acknowledges the value of an adequate understanding of causation and emotion both for his theory and for clinical practitioners whose work is deeply anchored in affective experience. Indeed, Bowlby had his contemporary clinicians in mind when he notes discussions of philosophical inadequacies of both mentalists (dualisms of Freud and Janet) and epiphenomenalists (extreme behaviourists) and asks, “How is he [clinician] to picture the relation of the private to the public, of subjective to objective, of feelings to physics, of body to mind?” (1969/1982, p. 107). But with Langer, he accepted that answers
were simply not yet available: “Though insoluble [the explanatory question of an emotional phase of being felt] at present, the problem may one day yield to study” (1969/1982, p. 109n101). This position would seem a clear naturalist commitment: one that might more specifically be depicted as a sort of ‘methodological naturalism’.99

My second reason for approaching Bowlby’s nascent naturalism is quite practical and related to the approach of this thesis. On the one hand, naturalism has underpinned the project of “naturalising the mind”, central to later cognitive ethology (see the in depth discussion in Allen & Bekoff, 1997). As such, it would appear an appropriate path for furthering our knowledge of Attachment Theory, provided adequate allowance is made for human normative dimension (i.e. the non-scientific components that contribute to a ‘good life’ or ‘well-being’) when discussing maladaptation and disorder. (This issue will be addressed in Chapter 8). On the other hand, a reapplication of Bowlby’s turn to the developmental sciences (i.e. biology and ethology) and philosophy of emotion would also seem a more defendable move for rearticulating of Attachment Theory. I now want to specify the two areas of study that may further inform a rearticulation of the theory.

3.6.2 Two Planks for Constructing a Rearticulation of Attachment: DST and the Philosophy of Emotions

I will nominate two fields of research for assistance in rearticulating Attachment Theory. For the moment I will make only minimal recognition of how such matters might apply. (A detailed look would require the greater background discussion that follows in the next chapter.) This selection adheres to the sort of methodological naturalism inherent in Bowlby’s work. The first is Developmental Systems Theory (DST) (for introduction see Oyama, 1985/2000). This field would appear to be a natural ally for Attachment Theory. It might also serve as a useful complement to the Tinbergen framework. On the one hand, DST clearly addresses Laland and colleague’s (Laland et al., 2013) criticism of the common reliance in the biological sciences on Mayr’s ultimate-proximate causal dichotomy, a criticism they also extend to some less integrated, fragmented applications of Tinbergen’s framework. DST also articulates a unified approach to biology, something in line with Tinbergen’s unspecified aims for integration of his four realms (1963). On the other hand, the dynamic approach to developmental causality within DST would also appear to compliment Tinbergen’s push for a more interactive approach for ontogeny (i.e. Nature x Nurture) whilst offering new ground for considering the development of attachments. These two suggested applications of DST for Attachment Theory, the first more broadly conceptual, the second more scientific methodological, may mirror comments on DST made by the philosopher Peter Godfrey-Smith. He has identified two intertwined intellectual

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99 These discussions might be seen as forerunners to today’s difficult if not intractable engagements with questions of phenomenal consciousness and qualia. They precede the publication of Nagel’s influential article, “What is it like to be a bat?” (1974). For a discussion of these sorts of questions in the context of cognitive ethology, see Allen and Bekoff’s chapter on the issues of consciousness (1997, pp. 139-159).

98 Papineau has proposed: “For the sake of argument, let us thus understand methodological naturalism as asserting that at bottom philosophy and science have just the same aims and methods, namely to establish synthetic knowledge about the natural world, in particular knowledge of laws and causal mechanisms, and to achieve this by comparing theories and empirical data” (2009, p. 26). Methodological naturalism can also be differentiated from ‘ontological naturalism’. The latter has engaged the more metaphysical questions of mind-body relationship, mental causation and various versions of physicalism. Again see Papineau for a survey of possible positions (2009).
projects within DST: one, a ‘philosophy of nature’—whose work “tries to give a careful philosophical redescription of the picture of the world that science seems to be delivering”—, and the other a ‘philosophy of science’ focussed on a program of scientific research (Godfrey-Smith, 2001, p. 285). I want to suggest that DST might serve as a more dynamic overlay to the Tinbergen framework. Both share similar sets of projects.100

A second field for inclusion in a rearticulation of Attachment Theory is philosophy of emotions (for a comprehensive introduction, see the edited Oxford Handbook, Goldie, 2009). When Bowlby turned to philosophy to address questions of emotion, he encountered significantly less resource that one finds today (for a review of recent work see Griffiths, 2013).101 Indeed at least three areas would appear to be significantly relevant for Attachment Theory. First are the more dynamic approaches to emotion found in the field of situated cognition (e.g., Colombetti, 2013). Bowlby’s employment of a control systems approach would seem to anticipate such perspectives (Bowlby, 1969/1982, pp. 235-262). The second, related in ways to the first, regards promising approaches to the integration of cognition and affect in contemporary neuroscience (e.g., Pessoa, 2013; and a BBS commented article by Pessoa, 2015). These will be shown to be relevant to Bowlby’s discussions of linguistic and cognitive dimensions operating within affectively conceived ‘Goal-Corrected’ attachments (Bowlby, 1969/1982, pp. 350-358, 368-370). The application of homology within emotion research constitutes the last area for selection. Indeed, Bowlby’s turn to ethology principally reflects an application of phylogenetic homology: attachment systems are shared across mammalian species and traceable to common ancestors. This particular approach has also emerged in both cross-species and human infant studies of the so-called basic emotions (for a review of an altered/expanded approach see Clark, 2010b; for the initial ‘standard’ presentation regarding see Griffiths, 1997). More recently, Jason Clarke has very interestingly articulated an application of serial or developmental homologies in suggesting continuity between the basic emotions and more complex forms in humans (Clark, 2010a, 2013).

An exploratory move into each of these areas follows Bowlby’s naturalistic inclinations. If Tinbergen’s four realms provide an explanatory framework for an assessment of Attachment Theory, then these additional areas offer key inputs for a rearticulation. I will therefore return to DST and these three topics in philosophy of emotions in Chapters Six through Ten.

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100 My proposal here is not intended to suggest that this is the first suggestion for an application of systems approaches to Attachment Theory. Nor does it suggest that this is the first connection of Tinbergen’s quadripartite approach to developmental systems perspectives.

101 For much of Bowlby’s career it was psychoanalysis—not philosophy and not the cognitive sciences—that sustained a focus on theories of affective experience (see survey by Stein, 1991). Indeed, much of the important infant research into affective development, beginning with Stern (1985), has retained the title of psychoanalytic developmental research (Beebe, Knoblauch, Rustin, & Sorter, 2005; Trevarthen, 1980) (Beebe et al., 2010). However, this early situation has changed significantly. The fields of both affective science and philosophy of emotion have caught up with and most probably surpassed many of the current psychoanalytic and psychodynamic theories, who their retain focus on affects. These advances are not well acknowledged by the latter. Indeed, it is not at all clear that too many of the participants from all schools of psychotherapy are up to date with the more sophisticated discussions in philosophy of emotion. See discussions of contemporary scientific views of emotion in Chapter Eight for additional clarification.
3.7 A few Words about Phenomena, Theory and Data

Attachment Theory has been erected upon important philosophical assumptions—inherent to a combined quadripartite ethology and the cognitive sciences—that may be described as evolutionary, realist, normative and causal in nature. The specifics for each will become apparent as the thesis evolves. Differences with these assumptions when applied to the Attachment Theory may result in unavoidable conflict. So a little up front clarity may assist in later diagnosis and resolution of issues. I want to offer a few additional clarifying remarks regarding scientific terminology. Let me provide both an example of potential confusion along with philosophical guidance by Bogen and Woodward (1988) on how to account scientifically for phenomena.

An important 2003 cross-disciplinary Dalhem Conference assembled researchers from a range of disciplines to critically consider the relationship between the concepts of attachment and bonding (Carter et al., 2005b). The unique gathering ambitiously sought to synthesize human and non-human studies in hopes of integrating studies of these two phenomena. For most mammalian species, attachment and bonding may co-occur as a more or less single phenomenon, whereas the two temporally diverge within the human species—attachment as a post-six months’ phenomenon, distinct from early post-birth bonding (see discussions below in Chapter Four and Eight). The conference sought to unpack conceptually divergent disciplinary approaches, particularly those practiced by the two chairpersons, Carter and Ahnert: behavioural biology and developmental psychology respectively. The discussions brought helpful clarity by highlighting important differences in these two distinct phenomena across species—this remains a common confusion as will be noted in Chapter Seven.

However, in producing a synthesis to support their conceptual goals their accomplishment may have been hampered by an inadequate diagnosis of deeper philosophical differences implicit across their disciplines. A statement in their introduction may be suggestive here.

It is important at the outset to recognize that both attachment and bonding are hypothetical constructs [italics mine]. No one has ever seen [italics mine] an attachment or a social bond or directly measured their qualities and strengths. As the authors of this volume write about these constructs, we are struggling to give words to processes that evolved long ago [italics mine] . . . [whose enabling] mechanisms [are] based on neural circuitry and endocrine processes (Carter et al., 2005a, p. 2).

Has a well-intentioned effort to synthesize divergent approaches risked confusing ‘operational constructs’ and potentially ‘unseen evolutionary mechanisms’? The conference statement may appear to count as an instance of what Murphy has described in psychiatry as an “unstable blend” of operationalist approaches—often neo-empiricist—with causal ambitions—typically realist endeavours in biology (2006, p. 201). When faced with such conceptual dilemmas, critical discussions in psychiatry have usefully turned to philosophy of science. (For a summary of these problems in psychiatry and proposed ‘causal path’ for solution see chapter in Murphy, 2006, pp. 201-253). I will...
also turn to philosophy here to support the chapter’s aim of providing a coherent picture of Bowlby’s approach.

Bogen and Woodward’s threefold division of *phenomena, theory and data* may be useful.

Our argument turns on an important distinction which we think has been ignored in most traditional analyses of science: the distinction between data and phenomena. Data, which play the role of evidence for the existence of phenomena, for the most part can be straightforwardly observed. However, data typically cannot be predicted or systematically explained by theory. By contrast, well-developed scientific theories do predict and explain facts about phenomena. Phenomena are detected through the use of data, but in most cases are not observable in any interesting sense of that term . . . Facts about phenomena may also serve as evidence, but typically such facts are evidence for the high-level general theories by which they are explained (1988, p. 314).

I contend that this distinction can also help to diagnose disputations within Attachment Theory discussion that might be more properly understood as those concerned more with *causal validity*—phenomena and facts about phenomena—and others that are more focussed on *construct validity*—reliability of measures and observable data. The scientific theories operating within Attachment Theory are not to be misunderstood as mere hypotheses about inaccessible black-boxed processes. For immediate purposes, I will approach constructs more broadly as a potential causal explanatory bridge connecting phenomena and theoretical framework.

### 3.8 Conclusion

I want to conclude this introduction of Bowlby’s TLC method by reiterating three points. First, he made good choices in selecting equally robust fields—ethology, philosophy of mind/emotion and cognitive science—as alternatives to psychoanalysis and behaviourism. Second, the combination of ethology and the cognitive sciences in the late 1960s would appear to have indeed been quite original. Indeed, cognitive sciences in those days had also excluded emotions and bodily issues raised by Bowlby. And third, that combination has stood the test of time: the unique approach has gained a not insignificant favour in both ethology and the cognitive sciences. In fact the role of emotion in both animal study and cognitive science are now standard practice. And as I have emphasized, Bowlby’s more innovative philosophical accomplishments have gone virtually unnoticed by philosophers of science and developmental researchers since the original publications of his attachment theoretical trilogy beginning in 1969. To be clear here, that he turned to ethology and added bits from other sciences including the cognitive sciences is well known and acknowledged. His philosophical interests are very rarely understood. But few have indicated how and why those moves turned out to have been so strategic. With Bowlby’s crucial strategy and methodological position in place, I can now turn to the specific contents of the theory in Part Two.

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103 Bogen and Woodward have concluded that “because our account treats the assessment of reliability as largely independent of the construction of systematic explanations, it can explain why inferences to the existence of phenomena, and the procedures used to analyze data and establish reliability, are often robust enough to endure fundamental changes in explanatory theory” (2006, p. 334).
REFERENCES


PART TWO: A Philosophical and Historical Assessment of Attachment Theory
Chapter Four: Identification of Constructs of Attachment Theory

What for convenience I am terming attachment theory is a way of conceptualizing the propensity of human beings to make strong affectional bonds to particular others and of explaining the many forms of emotional distress and personality disturbance, including anxiety, depression, and emotional detachment, to which unwilling separation and loss give rise (Bowlby, 1977, p. 151).

[An attachment is . . . ] an affectional tie that one person (or animal) forms to another specific individual [and . . . ] is thus discriminating and specific. The first is most likely to be formed with the mother, but may soon be supplemented by attachments to a handful of other specific persons . . . An attachment tends to endure” and “to bridge gaps in space and time . . . [parenthesis in original] (Ainsworth, 1969, p. 971).

4.0 Chapter Introduction

What exactly does Attachment Theory claim? In this chapter I will provide a philosophical introduction to attachment that delves into theory contents from a more critical perspective. In addition I will identify Bowlby’s under-recognised attachment notion of a Goal-corrected Partnership. Part Three of the thesis will argue how this concept is indispensable for a coherent articulation of a lifespan theory, for it shines a light on the role for enhanced cognitive capacities within attachment phenomena. However, a more general answer to this chapter’s question of what an attachment might be is less obvious and more complex than might be expected. Certainly, Attachment Theory is now such a large multi-dimensional endeavor, that it may resist a chapter length exposition. Indeed, a more thorough historical depiction follows in Chapter Five and a more conceptual expansion in Chapter Six.

To address the explanatory challenge, I will re-engage Bowlby’s TLC strategy introduced in Chapter Three, particularly the Tinbergen (T) quadripartite explanatory framework for understanding biological behavior (Tinbergen, 1963). Brüne has also recently noted Bowlby’s strategic employment of Tinbergen whilst acknowledging, “psychiatry has been curiously unaware of the prospects and opportunities inherent to Tinbergen’s ethological methodology for improving the understanding and therapy of psychiatric conditions” (2014, p. 6). The quadripartite approach can once again bring focus to difficult subject matter.

The chapter’s exploration of attachment will proceed in eight steps providing a relatively original philosophical analysis of the theory. The approach will also require a degree of conceptual explanation and will therefore be slightly longer than the other chapters in this thesis. The first step (Section 4.1) asks more generally what is (an) attachment within Attachment Theory and what are the phenomena to which the theory has sought to direct our attention. Included here is a proposed

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104 This chapter seeks to provide a unique introductory foundation for the discussions that follow. For philosophers less familiar with the content, it offers a thorough summary. For attachment researchers and psychiatrists/practitioners less familiar with philosophical concerns, an equally required discussion of a few critical questions is provided.

105 The accumulated knowledge garnered by Attachment Theory has advanced dramatically since Bowlby launched his reform project dating back to the 1950s. But despite the theory’s prolific research record (Cassidy, Jones, & Shaver, 2013), including a range of accompanying handbooks (Cassidy & Shaver, 1999, 2008, 2016; P. Holmes & Farnfield, 2014a, 2014b, 2014c), and useful monographs (Crittenden & Clausen, 2000; Gillath, Karantzas, & Fraley, 2016; Goldberg, 2000; J. Holmes, 2014), no standard summary exists.

106 As argued above in Chapter Three, Bowlby’s TLC strategy was an integrated one. The application of the Tinbergen framework here in Chapter Four does not preclude an inclusion of the Langer (L)—i.e. consideration of emotion philosophy/science—nor the Craik (C) elements—i.e. cognitive science—within this exploration of attachment basics. However, a deeper application of the L and the C sub-strategies will be conducted in the chapters that follow especially Chapter Six and Chapter Eight respectively.
Tinbergen inspired four part working definition for Attachment Theory. The next section (Section 4.2) expands on this proposed definition identifying thirteen individual attachment constructs set against the backdrop of the Tinbergen framework. Sections 4.3-4.7 discuss the initial 12 constructs individually—covering phylogeny (1), ontogeny (4), function (5) and mechanism (2) respectively by section. Details clarify both the state of empirical work to date and a few key philosophical questions relevant for biology, developmental science and psychiatry. These four sections will therefore perform some important conceptual heavy lifting, assisting the reader to assess from a higher vantage point both what is known and what may not yet be known within Attachment Theory. Along the way key hypotheses will be acknowledged, assessed and gaps in knowledge identified. Four specific points particularly relevant for psychiatry can be signposted: (1) attachment causal antecedents, (2) attachment outcomes, (3) attachment risk and (4) attachment mental mechanisms (or IWMs). Section 4.8 depicts the last identified Construct 13, the organisational construct. This perspective does not fit neatly into a Tinbergen framework but rather appears to incorporate all three of his phenotypic questions. A brief conclusion (Section 4.9) highlights the more relevant points of the chapter and directs the reader to the last two chapters of Part Two.

A simple comprehensive answer to the chapter’s basic question—What’s attachment?—has proven surprisingly elusive.\(^{107}\) Thompson has recently presented an insightful developmental summary of Attachment Theory in the Oxford Handbook of Developmental Psychology (2013). He outlines how the theory has successfully responded to important questions about the impact of early development for longer-term health. “The answers yielded thus far are surprising [and have] contributed to a rich, provocative portrayal of early psychological growth.” However, he concludes somewhat pessimistically suggesting that this portrayal is “provisional, nonconsensual (some attachment theorist would disagree with this view) and uncertain [parenthetical remarks in original]” (2013, pp. 212-213).\(^{108}\) So, my undoubtedly ambitious goal for the present chapter will be to briefly set out an approximately adequate and current view of the basic constructs for not only early development but for the full lifespan of Attachment Theory.

The chapter begins with a pair of epigraphs: one by Bowlby and the other by his collaborator Ainsworth. Bowlby’s remarks were made in a presentation to his fellow professionals at the 50\(^{th}\) Maudsley Lecture to the Royal College of Psychiatrists in 1976. His emphasis was on the role of attachment experience for lifespan mental health and as a potential causal role in the emergence of disorder. However, unlike the enormous expansions across related fields such as developmental psychology and social psychology, attachment has not generally received less interest within the field of adult psychiatry\(^{109}\). I hope to partially correct that both here and in Chapter Six.

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\(^{107}\) Thanks go to a few philosophers who on multiple occasions pointed me in the direction of this obvious enquiry.

\(^{108}\) Thompson cites the generative aspect of Attachment Theory identifying a number of questions requiring further research. His concluding rhetorical question is a curious one: “In this atheoretical data-driven era of psychology what more could one ask for?” (2013, p. 213) I would argue that the TLC strategy and rearticulation offered in Part Three, might offer a useful alternative to an atheoretical psychology.

\(^{109}\) A few considerations may be helpful in understanding the status of attachment within psychiatry. On the one hand, as demonstrated in Chapter Two, attachment has and continues to provide an important platform for much of psychoanalysis—broadly defined (Hopkins, 2013). However, the place of the highly fragmented field of psychoanalysis within psychiatry remains
Ainsworth and colleagues actually had several questions in mind in the 1960-70s when they revolutionised the study of complex infant behaviour through the introduction of their unique experiment, the Strange Situation Protocol (SSP) (Ainsworth, Blehar, Waters, & Wall, 1978). First, that a human “or animal” may form these ties raises questions of evolutionary history (phylogeny). What aspects of attachment do we share with other species and what might be considered more uniquely human? Next, that attachments may be “formed . . . endure . . . [and] bridge gaps in space and time” suggests a possible assumption of an underlying lifespan developmental story (ontogeny). What exactly develops and how? Third, that these ties may be considered “affectional, . . . discriminating and specific” and can be categorized by number as “the first” and “supplemented by a handful” may raise an additional question: to what purpose are they directed (function), something clearly articulated in the Bowlby epigraph as well? And finally, what may be less explicit in this epigraph, one may ask what are the causes of the patterns of infant attachment behaviours that Ainsworth and colleagues observed and measured (mechanism) and Bowlby described in his address? Are they to be considered as narrowly conceived surface phenomenon—in an empiricist fashion—or might they be seen to operate via underlying mental capacities? These four illustrative sets of questions gleaned from the epigraph have been drafted to mirror the four explanatory tasks of Tinbergen’s conceptual approach. So, with this brief introduction of the explanatory challenge in place, the specifics of the chapter may now be considered.

Once again, the overall message from the chapter’s ambitious philosophical exercise will suggest that Attachment Theory is potentially more complex, and indeed more interestingly incomplete, than might be expected when considering the theory’s research success.

4.1 What is (an) Attachment in Attachment Theory?

4.1.1 A Need for Greater Clarity

What is ‘attachment’, or similarly, what are ‘attachments’ in Attachment Theory? As will be demonstrated, the term is regularly employed in both fashions. Informative scholarly summaries of the theory abound and may be differentiated in part by length and purpose.\(^{10}\) Shorter discussions may be expected when considering the theory’s research success.
typically serve as introductions to related investigations, especially attachment research findings. For example, see Unger & De Luca’s treatment of the impact of child physical abuse on adult attachment (2014, pp. 223-224). Lengthier treatments have often aimed to engage in more complex discussions. Again for a recent example see J. Holmes excellent survey of John Bowlby and Attachment Theory (2014). Nonetheless, as I have indicated, I want to suggest that the field still lacks what would appear to be an agreed robust comprehensive articulation that fits the minimal requirements of the TLC position.

4.1.2 Definitional Framework: Attachment Theory as Quadripartite explanantia

I suggest that a practical and potentially more coherent approach for rearticulating Attachment Theory might be to first re-embrace Bowlby’s original TLC strategy. So instead of the traditional singular explanandum and the singular explanans, I would like to offer an alternative: a plural explananda and a plural quadripartite explanantia. Such a move would raise the conceptual bar by requiring any summary definition for Attachment Theory to include minimally each of Tinbergen’s four independent explanatory realms, as well as incorporation of affective and cognitive mechanisms. The framework might also embrace Zweck’s recent observations about the inherent complex, contingent and
tradition of psychoanalytic research has emerged that may combine early infant study applied to later adult therapeutic work (for a good example see Beebe & Lachmann, 2002). This point was also well highlighted in Chapter Two. However, earlier traditional psychoanalytic practice often eschewed empirical research in part due to a heavy emphasis on the employment of case studies. Theories were often discussed in conceptual and clinical terms with little allowance for further robust empirical support (Eagle & Wolitzky, 2011). What is clear is that the dissemination of attachment discussion to a wide range of audiences may suffer from overly simplified if not poorly informed renditions of the theory. I hope to flush out these tendencies and offer an alternative here.

An exhaustive survey of attachment literature is well beyond the scope of this thesis. I will identify a relevant portion of that literature that seeks to provide reasonably comprehensive summaries. In addition to the Bowlby and Ainsworth corpus introduced in Chapter Three and discussed in its historical context in Chapter Five, an attachment conceptual bibliography includes a number of useful broad treatments of the theory. This includes the latest third edition of The Handbook of Attachment (Cassidy & Shaver, 2016), monograph by H. Barrett’s (2006) finely detailed analysis discussed in the footnote below, as well as Goldberg’s study (2000) and one edited anthology by Goldberg and colleagues (1996). More recently, a potentially complimentary three volume handbook (P. Holmes & Farnfield, 2014a, 2014b, 2014c) has also been published, focused in part in integrating the less recognized work of Crittenden’s Dynamic Maturational Model (Crittenden & Clausen, 2000; Crittenden & Liandini, 2011) within the broader attachment corpus. Social psychologists Mikulincer & Shaver’s second edition of Adult Attachment (2016) is perhaps one the first comprehensive overviews and as such is an important reference, as is, Simpson & Rhode’s (2015) very recent Attachment Theory and Research: New Directions and Emerging Themes. Two journals are also noteworthy: Attachment and Human Development and Development and Psychopathology. Each has produced noteworthy special issues dealing with relevant specific discussions. A more practice-oriented journal has also emerged: Attachment: New Directions in Psychotherapy and Relational Psychoanalysis. Of historical/theoretical value are the four attachment specific target articles with accompanying commentaries in the journal of Behavioral and Brain Sciences that provide an excellent introduction to historical evolution of critical questions (Del Giudice, 2009; Kraemer, 1992; Lamb, Thompson, Gardiner, Charnov, & Estes, 1984; Rajecki, Lamb, & Ommascher, 1976). There have been numerous special editions of journals and reviews looking at more specific attachment questions, for example: attachment lifespan concepts in Human Development (1976); adult attachment in Review of General Psychology (2000b); a target article with commentaries addressing measurement: categorical versus dimensional differences, in Developmental Psychology (2003); attachment and aging in Attachment and Human Development (Magai & Consedine, 2004); two issues in Monographs of the Society for Research in Child Development; “Growing points of attachment theory and research” (1985) and atypical patterns of development (1999); attachment and psychotherapy in Psychotherapy Research (Strauss, 2000) and in the Journal of Consulting and Clinical Psychology (Davila & Levy, 2006); one indirectly relevant on developmental pathways in Developmental Psychopathology (D. Cicchetti & Rogosch, 1996); and one looking at implications for legal context of custody disputes (McIntosh, 2011). The Attachment Theory and Research website managed by the Psychology Faculty at SUNY Stony Brook catalogues attachment research publications by individual author within five categories: developmental, clinical psychologists/psychiatrists, social/personality psychologists, models/mentors, and favourite articles, http://www.psychology.sunysb.edu/attachment/vitae/vitae_index_2010.html. Chapter Five’s historical depiction of the theory’s evolution also presents additional bibliography.
probabilistic nature of developmental study (2013). But before commencing, some initial delineation of attachment phenomena is in order.

4.1.3 The Theory’s *explananda*: Attachment Phenomena

Generally speaking, attachment theory is concerned with the explanation of multiple phenomena across the lifespan—the *explananda*—that may be properly denoted as *attachment phenomena*. As Bowlby states, his theoretical framework for attachment was “designed to accommodate all [italics mine] those phenomena to which Freud called attention—for example, love relations, separation anxiety, mourning, defence, anger, guilt, depression, trauma, emotional detachment, sensitive periods in early life—and so to offer an alternative to the traditional metapsychology of psychoanalysis” (1988, p. 25). The attachment phenomena to which he referred might best be captured as threefold: developmental, affective and interpersonal. Included here will be an individual’s lifetime range of close interpersonal relationships as well as the implicit affective regulatory capacities that emerge from one’s attachment experiences. In terms of potential developmental health risk, this capacity would appear to relate especially to the experience of the more negative affects. In terms of the three phenomena descriptors, Bowlby’s suggestion of *defence* might count as a possible exception here. This term might also logically apply to other less affective and non-interpersonal matters. However, Bowlby’s employment of the notion ‘defensive exclusion’ has been shown to play an important role in less optimal styles of affective regulation. Unlike Freud, Bowlby does not reduce personality development to these phenomena alone, nor does he posit a single psychosexual apparatus underpinning all behaviour (J. Holmes, 2010). These distinctions in approach to the domain of personality are clarified within Bowlby’s more complex approach to multiple behavioural systems (Bowlby, 1969/1982, pp. 65-84), something elaborated below in Subsection 4.4.

4.1.4 A Working Definition

With at least the minimal requirements for the identification of a conceptually adequate definition in place—the *quadripartite explanantia* and *attachment explananda*—, I can now offer a current summary working definition with markers for each of Tinbergen’s four realms. As will become evident in the thesis and echoing Thompson’s views above, this articulation is not fully embraced.

WORKING DEFINITION: Attachment Theory proposes a lifespan developmental approach for understanding emotionally close relationships and the regulation of negative affect, something that emerges from and remains connected with interpersonal experience. [Evolution/phylogeny] First, the theory sees human beings as possessing a species-specific attachment behavioural system that

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113 This quote is taken from an updated version of a talk given by Bowlby to the American Orthopsychiatric Association in 1981, a time when he was systematically seeking to rebuild bridges with the therapeutic world, including a psychoanalytic world that had begun to relinquish its more classical concepts, especially its devotion to Freud’s original metapsychology.

114 The construct of defence has emerged from psychoanalysis but is given a radically new interpretation by Bowlby. He will introduce his most extensive discussion of *defensive exclusion* in the context of findings from information processing (Bowlby, 1980, pp. 44-74). He will seek to explain defence in the context of both affective and interpersonal phenomena. As such he will see important overlaps with notions of cognitive dissonance. However, the latter is a broader construct that would encompass the former, not vice versa (See review of dissonance theories in Harmon-Jones & Mills, 1999).

115 Bowlby does not ignore sexuality but considers it to be a separate unique behavioural system, one whose development proceeds later in childhood than the Freudian infancy-based counterpart. For a discussion of the interaction of these two systems—the attachment and the sexual—see contributions in Diamond and colleagues edited volume (D. Diamond, Blatt, & Lichtenberg, 2007).
develops across the lifespan and serves a survival function. This behavioural system has emerged from within evolution and can be seen to have evolved homologously (i.e. in a manner that is of the same kind) in other species, especially non-human primates. Second, the theory posits a genetically selected, species typical disposition to engage important conspecifics—to form attachments—for survival. This disposition to engage others promotes adaptive survival through the formation of both infant-caregiver bonds and adult pair-bonding (and other peer bonds). More broadly, the attachment system can be seen to promote survival via its contribution to social competency, self-regulation and individual resilience. These functions are supported by an affect-regulatory capacity whose maladaptive functioning results in greater risk for poor mental and physical health. Third, the mechanisms that support the engagement of this affect-regulatory capacity—in both unique interpersonal relationships and social interaction more generally—span multiple levels and domains. In addition to the molecular (genetic and epigenetic), neurological, endocrine and autonomic levels, such mechanisms also operate at the psychological level as internal working models (IWMs). These models contain implicit 'felt representations' of both the self and the environment, especially the important 'other' of the interpersonal environment. As such they may both interpret and predict behaviour. Fourth, the underlying mechanisms emerge in the context of developmental interactions between the system's disposition and a range of intrapersonal, interpersonal and other ecological factors. In our species this development emerges in infancy and continues throughout life— Influencing peer, romantic and caregiving relationships. Although early experience is important, attachment development is not restricted solely to periods of earliest immaturity. Consequently both continuity and change are to be expected. Attachment Theory has proven valuable for psychotherapeutic work with children and adults introducing (poorly acknowledged) possible causal links for understanding change, growth and development.

4.1.5 Definitional Recap: Lifespan *explananda*, four *explanantia*

This summary definition meets the minimal requirements of the TLC strategy: four biological explanations of lifespan behavior are present. My emphasis in defining attachment as a capacity for the regulation of negative affect is not unsupported (Simpson & Beckes, 2009) but may be less recognizable than more standard approaches that may focus on attachment as a bond (Bowlby, 1969/1982) or more recently as emerging personality (Mikulincer & Shaver, 2012b). However, this emphasis on negative affect also leans in the direction of providing greater explanatory support to the fields of psychiatry and psychotherapy. Nonetheless, there are obviously a great number of terms and concepts included in the definition that require further discussion. With this initial high-level effort at a *quadripartite explanantia* in place I can now move to a next level of detail, the question of individual constructs. Here the definition may be unpacked in a more analytic fashion.

4.2 Identification of Attachment Constructs within a Tinbergen Framework

4.2.1 Constructs in Attachment Research

One avenue for both exploring and clarifying the content of Attachment Theory is via an identification of constructs employed across the research. Sroufe & Waters have noted: “Assumptions

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116 The summary definition approaches Tinbergen’s four realms in Phylogeny-Function-Ontogeny-Mechanism order. This sequence has been initially adopted in order to provide an intuitively simpler definition. However, in the rest of the thesis I will adopt a Phylogeny-Ontogeny-Function-Mechanism order. There is obviously no preferred or perfect sequence as each realm has been posited as unique. I seeking to address the greater complexity of the theory this second option seems preferable. See also footnote 119 below.

117 I have employed the term construct here in a relatively broad and neutral manner, applicable to the biological as well as the psychological dimensions of attachment phenomena. In doing so, I make no commitment to empiricist or behaviourist perspectives. In Chapter Three I have situated Attachment Theory firmly within the field of biology (i.e. Tinbergan’s ethology) but indicated that for Bowlby it also included mental mechanisms (i.e. Craik and cognitive science). As such a set of attachment constructs may also be understood as implying some version of the integrated notion, ‘biopsychosocial’. Murphy (2006, pp.
concerning the nature of constructs underlying developmental research are often only implicit, yet they guide data collection and interpretation of results” (1977, p. 1184). As will become apparent in this section, attachment theorists have employed quite a few constructs for depicting attachment phenomena and on occasion in differing manners. Indeed, I will focus on thirteen here. Not surprisingly the theory has resisted wishes of those researchers seeking a singular operationalised construct by which to measure attachment across the lifespan (e.g. Barrett, 2006, p. 342n344). However, these multiple applications of the attachment term have also given rise to a certain degree of conceptual as well as sematic confusion. Indeed, Rutter has noted the confusion that may arise when the same term—attachment—has been used “to refer to discrete patterns of behavior (such as proximity seeking), to a relationship, to a postulated inbuilt predisposition to develop specific attachments to individuals, and to the hypothesized internal controlling mechanisms for this predisposition” (1991, p. 357). Hopefully this exercise may partially clarify such queries, but the requirement here is not to explain away but rather to embrace the theory’s inherent complexity. For example, Thompson has noted that attachment researchers must account for heterotypic continuity—“assessing a psychological construct in different but developmentally appropriate ways at different ages” (2013, p. 196). Identifying the potential range of attachment constructs within current research can provide a useful next level of detail for grasping the complex terrain of Attachment Theory.

### 4.2.2 Thirteen Attachment Constructs within a Tinbergen Framework

Thirteen related attachment constructs\(^\text{118}\) will be proposed as capturing key connections between attachment phenomena and explanatory theory, each from slightly different perspectives. These thirteen constructs may also be more easily grasped when grouped within Tinbergen’s four realms of explanation with the possible exception of the thirteenth construct.\(^\text{119}\) The diagrammatic presentation provided below in Figure 4.1 continues a process that makes implicit assumptions within Attachment Theory more explicit.

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\(^{118}\) My intent here is both analytic and heuristic. The approach employed here is not exhaustive. The uses to which the term attachment has been employed are no doubt more than those offered here.

\(^{119}\) A note of appreciation goes to Marc de Rosnay for directing me to the logic of beginning an application of a Tinbergen framework with ‘phylogeny’.
Twelve of the thirteen numbered attachment constructs can be allocated directly across Tinbergen’s four realms: one in Phylogeny, four in Ontogeny, five in Function and two under Mechanism. A thirteenth organisational construct would appear to count as a more dynamic personality construct, one that does not slot into one of the three phenotypic realms but may potentially include each. I will now introduce each construct seriatim. The first twelve will be included across four relevant Tinbergen sections: 4.3-4.6. The thirteenth construct will be initially addressed in Section 4.7, while implications for embracing biological complexity across psychiatry will be taken up in a more detailed conceptual discussion in Chapter Six. Where required the length of the introductory discussions for each of the attachment constructs will vary in order to aid conceptual clarity. In many cases, reference both to other constructs and across other realms may be required, a conceptual demand regularly noted by ethologists working with Tinbergen’s framework (Bateson, 2012).

4.3 A Phylogenetic Construct (1)

The attachment behavioural system (Construct 1)—what I will consider here as a phylogenetic construct—may be conceptually the least recognizable of the overall construct group and potentially the least concise. One crucial philosophical point that merits mention here is the relationship between the phylogeny question and a proposed prioritized role for homology commonly proposed in philosophy of biology discussion and accepted in Part Three. Homologies—what Griffiths and other have termed “a relation of biological ‘sameness’” (2006, p. 5)—can be summarized as falling under two banners: phylogenetic (or historical)—“two characters are homologous if they have a common
The concept of a *behavioural system* adopted by Bowlby emerged from work within later ethology as a type of ‘middle way’ response to two sorts of inadequacies: first, the early Lorenzian (1950) nativist instinctual drive models (see important critique by Lehrman, 1953) and, second, the learning approaches dominant in both behaviorism (for a discussion of Skinner’s influence see O’Donohue & Ferguson, 2001; Skinner, 1938) and the more simplistic versions of comparative psychology (see discussion in Gottlieb, 1976). Indeed, the behavioural system notion remains current within contemporary ethology. An example of system units from avian species has been delineated in the anthology *Developmental Psychobiology*: dustbathing, hunger, aggression and sex (Hogan, 2001). Hogan defines the ‘behavior system’ more generally as “an organization of perceptual, central, and motor mechanisms that act as a unit in some situations” (2014, p. 2) but also suggests the unit of analysis determined by levels and degree of complexity for systems may also vary widely. Such complexity of possible systems was also acknowledged by Bowlby (1969/1982, pp. 65-74). This wide variety in object for study represents a conceptual boundary challenge in ethology; it is similarly the case in the study of behavioural homology (Ereshefsky, 2012) addressed in Part Three. Although Hogan and many ethologists have indeed embraced Tinbergen’s quadripartite framework, they do not engage the phylogeny question as fundamentally a question of homology: see various discussions in *Tinbergen’s Legacy* (Bolhuis & Verhulst, 2009). However, other ethologists working with behavioural homologies obviously have (Ereshefsky, 2007, p. 661).

The attachment system notion was developed in part by Bowlby from his encounter with ethology and control systems theory (1969/1982) to differentiate complex observable infant-caregiver goal directed patterns of behavior from simpler fixed-action behaviours. As Ereshefsky has noted, “Phylogeny is essential for identifying behavioral variants as the same behavior” (2007, p. 661). Although Bowlby did not employ the term *homology*, his approach may be considered to fall under the category of historical homology. As noted in the definition above, the attachment system is minimally

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120 Ereshefsky’s (2012) historical or phylogenetic group actually combines both the transformational—whose definition I have employed above—, and taxa homologies. I have elected the transformational definition as a proxy definition for the historical category.

121 ‘Behavioural systems’ may also be described as ‘behaviour systems’ (Hogan, 2001); they may also in places be used interchangeably as ‘motivational systems’ (Lichtenberg, Lachmann, & Fosshage, 2011).

122 For a recent discussion of ‘Lehrman’s dictum’, see Griffiths (2013).

123 The attractiveness of such a middle way for Bowlby would appear obvious: he also had sought a ‘middle way’ between drive based psychoanalysis and behaviourism.

124 As previously mentioned, Bowlby may have confused his message by his employment of the more traditional Lorenzian terminology whilst proposing a unique developmental story. Although Bowlby contributed to general discussion of behavioural systems, it was his specific focus on attachment that was more groundbreaking. Hinde wrote “it is in part through [Bowlby’s] influence that our understanding of the early development of social relationships has been put on a firm footing during the last 20 years” (1974, p. 189).

125 Bowlby’s early collaboration with Harlow regarding the latter’s experiments with rhesus monkeys (see discussion in van der Horst, 2011) and his own detailed phylogenetic considerations of attachment in related species within his major theoretical exposé (1969/1982) clearly argued that we share this attachment system via common descent with other species. From an evolutionary perspective, early infant attachment behaviour may be considered homologous having been studied in other
envisioned as a species-specific evolutionary generated behavioural propensity (or trait) to form close relationships. Within attachment study, the near universal observation of this manifestation of this predisposition in human development—bar extreme deprivation in the first six months of life (see discussion of attachment disorder below)—has also underpinned what has been termed the ‘universality hypothesis’ (van Ijzendoorn & Sagi-Schwartz, 2008, p. 881).

However, the attachment system has not been viewed as a fully ‘pre-wired trait’ but rather has been described as more akin to a ‘toolkit’ of possible fitness strategies that may develop along multiple paths (Simpson & Belsky, 2008) or what Ereshevsky (2007) has noted as a ‘module’. (See the more detailed discussion of the attachment systems and modularity in Chapter Eight). The system emerges at birth for most species that possess the system, though its appearance is delayed several months for developing human infants (see discussion of ontogeny below.) Bowlby (1969/1982, pp. 65-84) initially proposed that the basic attachment system would be triggered in response to perceived threat of separation from a caregiver. However, he would also come to expand his view on triggering stimuli to apply to more general states of distress, including negative affect.126 The attachment system was also originally proposed to operate in terms of homeostasis, settling back to a steady state after bringing an individual back from distress to a more secure place. However, the system’s operation might better be depicted as an example of Waddington’s notion of homeorhesis where the adjusted system returns not to a pre-set static position but rather back within an ongoing developing trajectory (1962).

Similar to his fellow ethologists, Bowlby also identified the attachment system as one amongst several behavioural systems, e.g. sexual system, caregiving system, exploration system, and power system. Once again, a key departure here from the Freudian tradition has been the recognition of a later (ontogenetic) emergence of sexual behavioural system than proposals within psychoanalysis that envision a primary psychosexual drive present within infancy (Freud, 1905/1953). I will return to the question of attachment and others systems in Chapter Eight.

4.4 Ontogenetic Constructs (2-6)

The next four constructs are things that develop, processes that contribute to development or perhaps both. Bowlby depicted attachment influence as extending “from the cradle to the grave” (1973, p. 139) whereby current experience interacts with earlier experience. Attachment ontogenetic constructs must therefore be considered as moving targets, each potentially demonstrating heterotypic continuity: all four may manifest themselves differently from infancy through to later adulthood in a cumulative fashion. However, indicating exactly how such phenomena might both evolve and continuously influence later experience across the life-span remains a particularly challenging conceptual task

mammalian species (Insel, 2000; Landers & Sullivan, 2012), especially other higher primates (for a survey of research on Rhesus monkeys, see Suomi, 2008).

126 Indeed, it was Mary Main’s research in the late 1970s and 1980s with both early avoidant (1979) and more seriously disturbed disorganized attachment formation (Main & Solomon, 1986) (see function discussion for clarification of terms) that first considered ‘separation distress’ as a possible subcategory within a larger triggering ‘distress’ category as operating within the attachment system.
(Thompson, 2008b). Attachment antecedents (Construct 2) are envisioned as causal contributors to the formation and maintenance of both early and later individual attachment ties (Construct 4) across the lifespan, and by inference, also encompass both attachment patterns (Construct 3) and the later emerging attachment based regulation capacities (Construct 5).

4.4.1 Attachment Antecedents

Attachment antecedents (Construct 2) provide the first episode of an attachment causal story. Indeed, as indicated below, the influence of early caregivers—studied as sensitive interactions between caregiver and infant—has been shown to be causally relevant for attachment. However, this is only part of an ongoing partially completed story as other influences clearly have a causal role to play. Hence, the study of attachment predecessors has expanded from the initial study of caregiver behavior influences in a number of directions to include additional proximal and distal factors. Indeed, research on this range of possible diverse antecedents has also generated important new unanswered questions, enquiries that point to greater conceptual complexity.

4.4.1.1 Antecedents as Interactions/Transactions

First, causal antecedents are best seen in terms of interactions—or to use the more conceptually current term first proposed by Sameroff (1975), transactions—that generate attachment phenomenon. These transactions may typically occur across numerous domains and levels. In addition to the noted interpersonal transactions, genetic and more distal ecological transactions such as Socio Economic Stress (SES) have also been studied (Fearon & Belsky, 2016). Using slightly different terminology, Gillath (2015) has conceived these transactions as encompassing both micro-level interactions with caregivers and other close conspecifics and macro-level context ecological interactions that indirectly influence these micro-processes.

4.4.1.2 ‘Causal’ Sensitive Interactions

Causal antecedents were originally hypothesized to be early sensitive interpersonal interactions with a first primary caregiver(s) (Ainsworth, 1964), a position historically captured in the construct ‘maternal sensitivity,’ and pivotal to what has been re-termed the ‘Sensitivity Hypothesis’ (van Ijzendoorn & Sagi-Schwartz, 2008, p. 881). A more narrow reading of this hypothesis has suggested

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127 Attachment Theory has been appropriately criticized for a disproportionate emphasis on the role of biological mothers in attachments (see discussion in Karen, 1998, pp. 313-344) and the theorised requirement for a single primary early attachment or monotropy. Indeed, Rutter (1995, p. 551) has noted Bowlby’s abandonment of his early depiction of monotropy, something now replaced by Hrdy’s (1999) more anthropologically accurate notion of alloparenting that also includes shared parenting with non-biological conspecifics. I would also suggest the theory’s ideas about maternal care have also been inaccurately critiqued at times by those who have not been able to acknowledge the corrective shifts made by the theory over time (for recent example see Vicedo, 2013) but see also comment by van der Horst, 2014). See also Chapter Five for a survey of historical conceptual shifts and developments of the theory.

128 Ainsworth (1975) has differentiated the use of the term ‘interaction’ from ‘transaction’ within developmental discussion as follows: interaction is best associated with higher level and more traditional discussions describing the interactive relations between nature and nurture, whereas transaction addresses the multiple influential relations that make up an organism in the world, from the level of quarks up to individual organisms and their environments. The latter would include other organisms.

130 Fearon and Belsky (2016) have identified six empirically demonstrated components of sensitivity: prompt responsiveness to distress; moderate appropriate stimulation; warmth, involvement and responsiveness support; interactional synchrony; autonomy support; and mutually responsive interactions.
attachment antecedents might be exclusively identified with these early caregiver interactions without need for alternative contributing variables. On the one hand, Fearon and Belsky have summarised, “the preponderance of evidence is more rather than less consistent with the sensitivity—or at least the quality-of-care-hypothesis” (2016, loc. 11211). This somewhat nuanced conclusion merits further elaboration. Meta-analytic summaries of sensitivity studies have demonstrated consistent but nonetheless only moderate correlational evidence, i.e. 0.23 size effect (De Wolff & van IJzendoorn, 1997). Nonetheless, this correlational evidence has been supplemented by important causal experimental evidence, demonstrating how attachments have been altered in clinical settings via enhancements to previously less sensitive interactions between caregiver and infant (see meta-analysis by Bakermans-Kranenburg, van IJzendoorn, & Juffer, 2003). This would also appear to represent a potentially interesting developmental example of what Woodward (2008) has termed causal interventionism within his discussion of philosophy of psychiatry.

4.4.1.2 Additional Antecedent Influences?

However, lower size effects for sensitivity point to a possible gap in antecedent knowledge for a sensitivity hypothesis. Indeed, van IJzendoorn (1995) has helpfully coined the term ‘transmission gap’ when comparing these relatively low—but nonetheless causal—size effects for sensitivity on attachment security and parental attachment states of mind regarding their own security that may often better predict future infant attachment. How might such gaps be explained? Fearon & Belsky (2016, loc. 11231-11255) have provided a thorough discussion of four possible gap explanatory candidates: (1) ‘technological’—perhaps there is a ‘sensitivity’ measurement issue—, (2) ‘moderator’—a distal influencer, such as lower SES might impact sensitive interactions—, (3) ‘domain’—yet to be identified ‘non-sensitivity’ parental influences may exert impact— and (4) ‘third variable’ explanations—mediating factors independent of parental behaviour such as infant temperament. They also identify three suggestive ‘domain’ explanations that point to similar parental psychological capacities, functioning independently of measured sensitivity: ‘mind-mindedness’ (Meins, Fernyhough, Fradley, & Tuckey, 2001), ‘reflective functioning’ (Slade, Grienenberger, Bernbach, Levy, & Locker, 2005) and ‘empathic understanding’ (Oppenheim, Koren-Karie, & Sagi, 2001) have all demonstrated correlations with security.132 These more cognitively enabled capacities will be relevant in the thesis discussion of Goal-Corrected Partnership in Part Three.

Perhaps missing from these discussions of antecedents are the important findings generated by Main and colleagues (Main, Hesse, & Kaplan, 2005; Main, Kaplan, & Cassidy, 1985) on the predictive role of care-giver ‘attachment states of mind’ as measured by the Adult Attachment Interview (AAI) (Main, Goldwyn, & Hesse, 2003; Main, Hesse, & Goldwyn, 2008). The relevant consideration here is the

131 The question of labels for Cohen statistical size effects may also be an inexact one. Whereas De Wolf and van IJzendoorn (1997) describe the range of reported meta-analytic evidence as moderate, an earlier study by considers the size effect of much of the same data to be weak (Goldsmith & Alansky, 1987). For a more detailed discussion see Belsky (1997).

132 Meins and colleagues have defined ‘Mind-mindedness’ as a primary care-giver’s ‘proclivity to treat her infant as an individual with a mind, rather than merely as a creature with needs that must be satisfied” (2001, p. 638). ‘Reflective functioning’ was first defined by Fonagy and colleagues as “the ability to take account of one’s own and others’ mental states and, thus, to understand why people behave in specific ways.” (1991, p. 203). Finally, ‘empathic understanding’ encompasses interpersonal “emotional and cognitive maternal processes that take place when mothers are engaged in caregiving, and can be seen as the mental correlate of sensitive caregiving behavior” (Oppenheim et al., 2001, p. 17).
predictive intergenerational correlation between infant attachment and the measurement of adult attachment. Care-giver states of mind—"ways of recounting life history with respect to attachment" (Main et al., 2008, pp. 31-32)—better predict infant attachment than the actual sensitive interactions currently measured. Main and colleagues (Main et al., 2008) have also importantly equated their elaboration of ‘states of mind’ with Bowlby’s notion of IWM. For the moment I will forgo a more detailed discussion of the AAI, attachment differences and the conceptual notion adult ‘attachment states of mind’ as these will be taken up in discussions of function and mechanisms below, as well the historical discussion in Chapter Five. However, it should be noted that the influence of adult’s ‘state of mind’ here might also be considered as dynamic component of the infant’s proximal/distal ecology. Once, again for a relatively full discussion of the range of possible proximal and distal antecedents, including so-called maternal sensitivity—encompassing a particularly valuable ecological contribution, though lacking Main’s insights—, see Belsky and Fearon (2008) and Fearon and Belsky (2016).

This causal antecedent question has proven to be more complex than a single-variable predictive linear analysis might typically allow, something Belsky (1984) has long considered but unfortunately may not be consistently acknowledged by his fellow attachment researchers (Belsky, 2005).133 As will be discussed below, if attachments change and indeed develop anew in post-infancy periods, then a more dynamic notion of antecedents may also be required. As Fearon and Belsky conclude, “Crucially, sophisticated models explaining precisely how parental behavior influences the organization of attachment are largely lacking and have not moved much in recent years beyond the rather general internal working models concept. These are critical challenges for the coming decades of attachment research” (2016, p. loc. 1040). The specifics of influential interactions are taken up within the discussion of the next construct, patterns of attachment.

4.4.2 Attachment Patterns

Attachment patterns (Construct 3) capture the potential complexity of responses that arise when the attachment system is triggered. These patterns encompass a range of observable individual and dyadic differential responses aimed at meeting security needs (Bowlby, 1969/1982, pp. 301-303).134 As discussed below in Chapter Six, I will identify a general fourfold attachment pattern: (1) an attachment resting state of relative security, (2) an interruptive moment marked by separation anxiety or other sorts of experienced distress, (3) a possible follow-on experience of loss or sadness (or continued distress), and finally (4) a series of restorative efforts that render reconnection to caregiver and a return to the initial homeostatic security.135

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133 More generally, developmentalists are also plagued by this hangover of non-dynamic approaches to evolving nature-nurture interactions (see discussion of the organisational construct and Developmental Systems Theory below in Chapter Six.)
134 Bowlby had initially described these as attachment behaviours but shifted to the more flexible notion of ‘patterns of behaviours’ to capture the complexity of a broader range of behavioural interactions. Behaviours, such as vocal signaling or infant locomotion, drafted in on behalf of the attachment system operation, might also function in non-attachment experience such as play (Bowlby, 1969/1982).
135 An alternative, less optimal pattern evoked by longer separation of 2-3 year olds has also been noted from the early days of attachment. Bowlby (1969/1982, pp. 26-27) acknowledged and supported his colleague Robertson’s observational work with children in temporary institutional care, where patterns evoked by separation were followed by protest, despair and detachment. Such patterns were hypothesized to lead to pathological risk for disorder.
The concept of ‘patterns of behaviour’ emerged from the field of ethology.136 Bowlby selected these in part to provide a more robust alternative to the approach taken by traditional behaviourists who sought to identify singular differentiated countable attachment behaviours. That approach failed to capture the ongoing complexity inherent to the transactional dynamics of attachment phenomena.137 Ainsworth expanded the concept of patterns of behaviour (1964) and with colleagues subsequently measured these in the context of patterns of infant-caregiver attachment behaviours in the SSP experimental setting (1978). This sequence is in principle applicable to all attachment relationships at any point in the lifespan. However, age-specific differences are to be expected: namely, with infants the expression is manifested predominantly via observable patterns of behavior, whereas with increasing age mixtures of behaviours, cognitions and emotions, all influenced by linguistic capacities that may comprise attachment patterns. In doing so, attachment patterns can also be seen as reflecting a process for the regulation of negative affect, i.e. Construct 5. Included here is a more internal attachment pattern of self-soothing whereby affects may be regulated in a secure manner. Attachment patterns function both as the template capturing the emergent antecedent sensitivity identified in Construct 2, and once settled within a formative relationship, provide a next model for the ongoing maintenance of those ties, as Construct 3.

### 4.4.3 Attachment Ties

*Attachment ties*138 (Construct 4) emerge in response to the attachment system predisposition where repeated interactive antecedent attachment patterns become stable as learned phenomena (Ainsworth, 1973; Bowlby, 1969/1982). Not surprisingly, attachment as depicted by the construct *tie* may also be considered the more common employment of attachment terms or constructs: the notion encompasses the more obvious semantic meaning of the term.139 Thompson has offered a definition of a tie or relationship: “A relationship can be characterized as an integrated network of enduring emotional ties, mental representations, and behaviors that psychologically connect one person to another over time and across space” (Thompson, 2015, p. 202). One might add that such integrated phenomena not only “connect . . . across space and time” but may also evolve: they emerge, enlarge, shift, alter and even dissolve with experience as well. The defining feature of an *attachment tie*—as opposed to other non-attachment relationships—is its accompanying goal of security maintenance (Ainsworth & Wittig, 1969). Equally, early ties are not restricted to single maternal caregivers but have been increasingly studied in context of father relationships (Bretherton, 2010; Lamb & Lewis, 2010a), attachment ties with extended family (Poehlmann, 2003) and in an anthropological manner with what Hrdy (1999) has described as *alloparenting* that may include early attachment ties with non-familial

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136 This relatively standard phrase has been captured in Burkhardt’s title of his informative historical survey of that field entitled *Patterns of Behavior* (2005).
137 For a survey and assessment of early theoretical alternatives for explaining attachment behaviours, see Rajecki et al. (1978).
138 For theoretical reasons, I have not employed an equally common term *bond*, a term semantically similar to *tie*. The employment of the term bond has been associated with studies of the health benefits of mother-infant postnatal physical contact and consequently has led to possible confusion. As will be shown below in Figure 4.2 and elsewhere, bonding begins at birth and therefore precedes attachment formation that commences in the mid first year of infancy (Carter et al., 2005).
139 First, *ties* are the obvious semantic equivalent of attachment. Second, the attachment term used in this context has also served as the title for both Bowlby’s three volume work, *Attachment and Loss* (1969/1982, 1973, 1980), and the first volume in that series, *Attachment* (1969/1982).
members (Ahnert, 2005). Such early ties are also defined both as asymmetrical and involuntary (or less voluntary) within child-parent configurations. Across developmental time they appear more symmetrical/reciprocal and voluntary in the context of peer, adult-to-adult and romantic configurations (Zeifman & Hazan, 2008).  

I will reserve most of the detailed exploration of overlapping questions regarding antecedents, sequelae, sensitive periods, continuity/discontinuity and developmental until Chapters 6, 7 and 8. However, two slightly related points may merit mention up front as they are important for subsequent discussion. First, what Holmes (2001) has coined the historically important ‘continuity hypothesis’, may be considered one of the ongoing and more controversial positions taken up by some attachment researchers (see cautionary critique by Lamb & Lewis, 2010b). Seemingly still championed by notable researchers such as Main and colleagues (2005), it suggests that earliest attachments generally remain stable or continuous across the lifespan potentially reflecting an early critical or sensitive period in early development. In such discussions, development post early formation rarely seems a consideration. Rather, focus would appear on later outcomes. However, this version of a continuity position has also been demonstrated to be a too simplistic rendering of attachment development (Sroufe, Coffino, & Carlson, 2010), something not in line with Bowlby’s more mature ideas (1969/1982, pp. 371-378; 1988) and not supported by important prospective longitudinal data (Pinquart, Feußner, & Ahnert, 2012; Raby, Steele, Carlson, & Sroufe, 2015). As discussed in Section 4.7, a more complex approach to attachment relationship development is required.

A second slightly related observation considers the partial neglect or lack of full acceptance of Bowlby’s proposed full normative ontogeny for attachment ties. As depicted in Figure 4.2, Bowlby contended that the development of attachment ties passed through two phases: a Phase III: Proximity Maintenance and a Phase IV: Goal-Corrected Partnership.

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140 There is a growing consensus that certain configurations of therapeutic relationships may also count as attachment relationships, though this recognition does not consider all therapeutic relationships as attachment ones. Attachment Theory has proven extremely valuable for both clinical-theoretical discussions and empirical investigations expanding our understanding of the efficacious contributions of the psychotherapeutic relationship (Mallinckrodt, 2010). The first adaptations of attachment models within psychotherapy depicted the therapeutic relationship as a potentially asymmetrical attachment relationship, modeled in part on the early secure base phenomena of the child-parent relationship (see description of the ‘secure base’ in the functional construct discussion below) (see also discussion in Bowlby, 1988). See also discussion of empirical therapeutic research focused on adult attachment change under IWMs discussion below.
He describes the emergence of a ‘goal-corrected partnership’, requiring enhanced cognitive capacities, including language (Bowlby, 1969/1982, pp. 265-268, 368-370).

His initial considerations in the 1969 edition are best understood against the background of an emerging developmental cognitive science still in its infancy (Boden, 2006). Bowlby updated his 1969 discussions of a GCP suggesting a clearer timeline of development into the 4th and 5th year.

The development of what … I term a goal-corrected partnership turns on a child’s capacity both to conceive of his mother as having her own goals and interests separate from his own and to take them into account. … At the third birthday it is only a very small minority of children who are capable of this feat. By the fifth birthday, however, the great majority can probably do it. The period during which the transformation is most likely to occur is the fourth and fifth years (Bowlby, 1969/1982, p. 368).

Part Three will take up the question of the place of a GCP in Attachment Theory and offer a detailed rearticulation. For the moment it will suffice to note that a portion of both psychoanalytically and cognitively focused researchers may not fully acknowledge Phase Four.

### 4.4.4 Attachment Regulatory Capacity

An attachment regulatory capacity (Construct 5) serves as a last possible ontogenetic construct in my analysis. As a capacity the construct may also be more related to evolving attachment processes than specific evolving attachment entities. The consideration of biological processes alongside biological entities has gained considerable traction in the philosophy of biology (Nicholson & Dupre, in press). However, there may be less consensus as to how to best approach the status of a possible capacity, or indeed if it belongs as a construct at all. Considerations have typically approached the notion of an attachment related capacity in at least one of two manners reflecting: either (1) from a more developmental perspective, attachment as a contributor to a more general developing regulatory capacity, influenced by but perhaps distinct from early attachment ties and experience (Thompson,
2015; S. F. Waters et al., 2010)—hence it might better fit within the attachment outcome construct in the function section below but not as attachment capacity—or (2) alternatively, as an attachment specific capacity more often associated with adolescence and adulthood lifespan periods that describes how attachment strategies and scripts might operate, especially in the context negative affective experience (Mikulincer & Shaver, 2016; Mikulincer et al., 2016). My preference here is the second option but in a slightly expanded fashion as described next. I do so because of the role such a capacity might play in understanding attachment style/differences—constructs taken up in the functional section. Such differences have also provided important empirical insight into risk for disorder. (See fuller discussion below.) But a few more conceptual considerations are required.

If one does accept the attachment capacity construct as I suggest, further questions still loom. Which portions of the lifespan might be included: Early, late or all? Indeed, in addition to lifespan variables, a few other related dimensions require acknowledgement. What might be the object of regulation: Stress, emotion/affect, self/other, or some combination of the three? Is the operational context intrapersonal, dyadic/interpersonal, or both? Positions vary here, in part because of lifespan complications relating to possible timing for emergence of notions such as autonomy, social competency and individual agency (McAdams, 2015). In addition to opting for a specific attachment capacity, I will argue for a broader more inclusive response to these questions on two conceptual grounds. First, I would prefer to strategically embrace heterotypic continuity across attachment that supports a more dynamic complex developmental story. Second, such capacities provide greater insight into the how and why of attachment phenomena than simple entity considerations, i.e. that attachment ties or even attachment style may not permit alone. This last point ties in well with the need for a mechanistic story that encompasses attachment operations or processes in Section 4.6.

Let me summarise the capacity construct. Within Attachment Theory this capacity emerges within both early and ongoing attachment experience. It may also shift across the lifespan. Early on the capacity is more likely to be observed as a property of relevant individual relationships. As mentioned, sometime in adolescence a more global capacity associated with the individual emerges. However, the capacity may continue to function in both manners: as a property of a relationship and as a global property of the individual. As indicated, the object of a capacity process has been discussed in regards to at least three related conceptual notions: regulation of stress (Simpson & Rholes, 2012), regulation of affects (or the emotions)\(^1\) (Shaver & Mikulincer, 2013) and regulation of the self (Thompson, 2015). These three conceptual regulatory areas—stress, affect and self—may overlap and each has been explored in terms of a broader set of phenomena than the narrower attachment capacity discussed here.\(^2\) In short, stress regulation might be conceived as emerging first within dyadic a subset or contributor to emotion regulation, whereas emotion regulation might typically be

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\(^1\) A detailed review of these two sets of research literature is beyond the scope of this exercise. For more detailed discussions, one is directed to discussions in handbooks for both emotion regulation (Gross, 2013) and self-regulation (Vohs & Baumeister, 2011). I will also briefly address the slightly divergent paths followed within both self-regulation and emotion regulation in Chapter Nine.
seen both as a subset of self-regulation and as also emerging sooner in early childhood via regulation within dyadic caregiving contexts. As Thompson notes, “Whether considered with respect to the growth of cognitive self-control, moral conduct, emotion regulation [italics mine], social competence, or other domains, the development of self-regulation [italics mine] integrates multiple developmental processes from within and outside the child” (2015). Calkins and Leerkes (2011) highlight one of those processes describing the emergence of a more combined self-regulation of emotions from early attachment dyadic experience. Although emerging from within specific relationships, the regulatory capacity eventually takes on a more global applicability for new social, interpersonal and intrapersonal experience. Finally, the capacity may also capture processes encompassed within the attachment ‘competence hypothesis’—i.e. “attachment security leads to differences in children’s competence to regulate their negative emotions, to establish satisfactory relationships with peers and teachers and to develop cognitive abilities” (van IJzendoorn & Sagi-Schwartz, 2008, pp. 881-882). This latter hypothesis will be revisited in the attachment outcomes section below.

The articulation of this regulatory capacity construct also provides important input for the field of psychiatry where affect or mood disorders typically represent a high proportion of diagnosis and treatment (Russo & Nestler, 2013). In particular, early dysregulation of stress (L. M. Diamond, 2015), as well as emotion—especially negative affect (Dante Cicchetti, Ackerman, & Izard, 1995)—, have also been associated with higher risk for psychopathology This concept of affect regulation has been discussed extensively by a range of attachment researchers, particularly in the field of social psychology beginning in the 1980s (see discussion in Crowell, Fraley, & Shaver, 2008). One the one hand, this regulation notion has been employed particularly as an individual global construct—see attachment style discussion below—in the study of adults (Mikulincer & Shaver, 2008, 2016), and to a lesser degree with adolescents (Allen & Miga, 2010). However, the dyadic component associated with early regulation by a caregiver remains manifest within later attachment relationships (Overall & Simpson, 2015). Individuals are seen as possessing strategic options for both self-soothing and engaging important others for relief of distress. On the other hand, researchers within Psychoanalytic Developmental Research (PDR) have also devoted important focus to interrelated notions of affect and self-regulation, but as predominantly early infant interpersonal phenomena (Schore, 1994, 2003a, 2003b; Stern, 1985). The regulatory construct is particularly relevant for understanding both the functional and mechanistic constructs that now follow.

4.5 Functional Constructs (6-10)

The next five constructs are treated within the explanatory realm of Function. Implicit here is a certain degree of theoretical complexity: constructs relate to other constructs—both within and without the functional realm—and may also include their own further sub-constructs. Indeed, Rutter and

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143 A measure of adult attachment focused on use of language—i.e. coherence and collaboration—has also been taken up by developmental researchers to measure an adult’s “state of mind relative to attachment” via the Adult Attachment Interview (AAI) (Main et al., 2008). This instrument has also been conducted with adolescents where researchers have suggested a coherent narrative might also be considered as a measure of an individual’s capacity for effect regulation when engaged with self-narrative (Allen & Miga, 2010). I will return to the AAI below in Chapter Five as it represents a noteworthy evolution in the history of attachment.
co-workers have warned that as conceptual dimensions may overlap, functional constructs may occasionally be subject to a degree of less helpful conflation (2009). And as with the previous realm of explanation, constructs must be considered to evolve across developmental periods and within differing contextual settings thereby accounting for a degree of ongoing phenotypic contingency. Once again, to the degree that the measured functional constructs extend the attachment health dimension raised by the presence of an attachment regulatory capacity, they lend important value for psychiatry and related health fields. One philosophical advantage for employing the Tinbergen framework will also emerge in this section and the mechanism that follows. Attachment Theory would seem to provide a conceptual example of how to retain both dominant philosophical versions of function: the adaptive and mechanistic (Godfrey-Smith, 1993).

4.5.1 Attachment Aims/Goals (Construct 6)

Attachment aims/goals reflect the inherent fitness or survival value of the attachment system. This counts an example of the adaptive employment of function in philosophy. In seeking to anchor attachment within an evolutionary framework, Bowlby suggested that the attachment system was ‘goal-directed’ or ‘goal-corrected’—this last notion capturing the more dynamic role of ongoing feedback or monitoring within a homeostatic system (1969/1982, pp. 67-71). Attachment fitness goals, when met by the operation of the system, are primarily captured by the more general construct of attachment security. Security permits an individual to explore life from a more stable, safe and reliable self-state. However, this security is manifested differently across the lifespan (i.e. as a heterotypic construct).

Bowlby described the security goal in early infancy as quite similar to our closest mammalian relatives, as the maintenance of physical proximity (Bowlby, 1969/1982, pp. 235-260), especially in the triggering context of separation anxiety (Bowlby, 1973, pp. 178-183). Ainsworth added two additional dimensions to proximity as security: the notion of both a secure base that supports infant exploration and a safe haven as a place where the infant returns when distressed or hurt (Ainsworth, 1978). Over time the security goal within specific human attachment relationships transforms from simple proximity to availability, permitting greater autonomy for the child. Availability emerges first as a predominantly physical or spatial phenomenon. As a child learns to communicate more competently and trust the responsiveness of the relationship, maintaining closer proximity is less required. More recent research has added support of exploration as a further dimension to secure base and safe haven, an activity where fathers’ roles appear to be more apparent (Bretherton, 2010). And as the child’s cognitive capacities expand, children internalise the ongoing presence of important others as representations; as attachment figures can now be kept in mind, a sense of mental availability replaces physical presence. Stroufe & Waters (1977) coined the generic concept felt security, a notion applicable to the full lifespan dynamics, something that encompasses both proximity, physical and mental availability. Their notion also reflects Bowlby’s ideas that mechanisms of attachment might

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144 Early observational research on patterns of behavior conducted by Ainsworth (1964) also acknowledged that ‘proximity’ might be managed with variable degrees of distance.

145 A Risky Situation (RS) protocol has also been devised to assess infant-father attachments (Paquette & Bigras, 2010).
include ‘felt appraisals’ (1969/1982, pp. 104-123). Lifespan attachment goals are realized first via experience in relationship-specific attachment ties (Construct 4), but are theorised to coalesce into the more global regulatory capacity (Construct 5) that would appear to emerge in later childhood. When measured across the lifespan within general populations, security has also been shown to be present amongst the majority (typically over 55%) of infants (van Ijzendoorn & Kroonenberg, 1988) as well as adolescents and adults (Bakermans-Kranenburg & van Ijzendoorn, 2009; van Ijzendoorn & Bakermans-Kranenburg, 1996)—something also referred to as the ‘normative hypothesis’ (van Ijzendoorn & Sagi-Schwartz, 2008, pp. 881-882).

### 4.5.2 Attachment Outcomes (Construct 7)

Attachment outcomes depict the developmental consequences of attachment experience across the lifespan. These health impacts have been heavily studied producing a fairly broad range of correlated outcomes across social, emotional and cognitive domains for childhood (Solomon & George, 2008; Thompson, 2008b, 2016), adolescence—especially in terms of peer relationships (Allen, 2008; Allen & Tan, 2016)—and adulthood (Mikulincer & Shaver, 2007; Mikulincer et al., 2016). For example, Thompson has surveyed the evidence for early childhood outcomes identifying empirical associations across “parent-child relationships, close relationships with peers and other partners, personality, emotion regulation, self-concept, emotion understanding, social cognition, memory and conscience” (2008b, p. 349). These phenomena have also been described within discussion of the ‘competence hypothesis’ (van Ijzendoorn & Sagi-Schwartz, 2008, p. 882): early and ongoing attachment experiences are seen as leading to later behavioural outcomes.

Two related challenges confront the outcome research. First, the need remains to move beyond the typical correlative basis of a portion of studies to a more complex explanatory understanding of how early experience might impact later behavior. That it does impact is thankfully no longer in dispute. Although many of the longitudinal studies have been designed to track the impact of early attachment on later development, there would also appear to be a lingering conceptual risk that somehow attachment development is somehow completed in early life and we need only wait to measure the later outcomes. Second, and perhaps not surprising, inadequately hypothesized simplistic early attachment causal depictions with later outcomes have been shown to fail predictive validity (Lamb & Lewis, 2010b). At the same time, prediction for complex developmental systems is typically seen as a more probabilistic matter. (See also discussions of Construct 13.) The relationship with ongoing developmental outcomes remains valid but also in the context of other contributing factors (Sroufe et al., 2010). As Belsky has also reminded, longitudinal study can also avoid ecological invalidity: both early and ongoing security—or lack thereof—have both been shown to have differential impact on

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145 His newly updated version retains these domains in a slightly different order (Thompson, 2016).

146 A related serious concern taken up in later discussion is the status of the vast array of attachment measures. Comparability of research data related to health outcomes remains problematic for several reasons, including adequacy of psychometric properties of instruments and differing sets of notions of attachment difference applied.

147 In all fairness and as noted elsewhere, the early lack of age appropriate measures across the lifespan may have hindered longitudinal studies.
outcomes (2005, p. 85). Outcomes literature has also been given greater granularity when considered in tandem with the following constructs.

### 4.5.3 Attachment Styles/Differences (Construct 8)

Attachment styles/differences, along with further elaborations in Constructs 9 and 10, seek to acknowledge the functional consequences of divergent attachment phenomena for individual phenotypic development. Research into attachment differences has identified and measured attachment variations in attachment goals or aims, namely some aspect of security. Perhaps one of the more striking contributions arising from the theory, has been its contribution to our understanding of human individual difference, within the field(s) of personality and social psychology, developmental psychology, and developmental psychopathology (see further discussion below in Chapter 6).

Attachment differences will be distinguished along two slightly distinct normative dimensions: one more personality-distributional (Construct 8) the other more health-specific (Constructs 9 and 10). However, neither Bowlby nor Ainsworth—who along with her colleagues, first measured security differences in attachments—articulated what in principle should have allowed both for a personality, non-pathological reading of attachment differences as well as for a more pathological construal. Arguing from a more evolutionary perspective, Bowlby has long indicated how less secure differences might also be considered more adaptive as supporting alternative reproductive strategies (2013). Hence the need for these two normative dimensions of difference: one distributional, the other health related.

Bowlby (1973, pp. 363-371) had proposed a more dynamic approach to personality development than prevailing psychoanalytic models allowed, calling upon ‘multiple pathways’ notions first articulated by Waddington (1957)—especially epigenesis and homeorhesis. From a more general attachment perspective, personality outcomes may be expected to be multiple, each following distinct developmental pathways. Unlike traditional developmental approaches, one normative size does not fit all here. Indeed, a closer look at the personality style/difference construct also reveals that it can be distinguished along several dimensions. I will identify five here. Difference dimensions can be

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149 Attachment Orientation is also a term used interchangeably with style and difference (Feeney, Collins, Van Vleet, & Tomlinson, 2013).

150 Bowlby also turned to Waddington’s theory of epigenesis as an innovative alternative to psychoanalytic and traditional developmental approaches. He gained three conceptual advantages in doing so: (1) greater flexibility for posting a wider range of developmental outcomes, (2) a more robust approach to differentiating developmental deficits—i.e. the unhealthy from the healthy, and (3) a more sophisticated notion for depicting how early experience influences later experience—i.e. the notion of homeorhesis. He employed a railway metaphor to contrast the two approaches. On the one hand, development in psychoanalysis might be depicted by a simple single-track connection between start and destination where deviations were considered as non-normative delays. Psychoanalysis typically measured individual differences “in terms of the degrees of progression, fixation, and regression” (1973, p. 368). On the other hand, Waddington’s biological approach had identified multiple potential pathways between start and destination, like a more complex railway network: “At conception the total array of pathways potentially open to an individual is determined by the make-up of the genome. As development proceeds and structures progressively differentiate, the number of pathways that remain open diminishes” (Bowlby, 1973, p. 364). In depicting environmental variation—coining the notions of ‘environmentally stable’ and ‘environmentally labile’ developmental process, Bowlby adapted the notion of homeorhesis—in contrast to homeostasis—to human personality development. He described a self-regulative developmental trajectory whereby the accumulation of past experience interactively shapes future experience but not in a critical period manner. “The developmental process is conceived as able to vary its course, more or less adaptively, during the early years, according to the environment in which development is occurring; and subsequently, with reduction of environmental sensitivity, as becoming increasingly constrained to the particular pathway already chosen” (Bowlby, 1973, p. 367).
distinguished (1) as objects of description—tie or a more general capacity (Shaver & Mikulincer, 2004), (2) as differentiators of an attachment security goal—secure and less secure (i.e. anxious avoidant and anxious ambivalent) (Ainsworth et al., 1978), (3) as regulatory strategies—activated, hyperactivated and deactivated (Mikulincer & Shaver, 2007), (4) as reflections of two continuous underlying dimensions of variability—avoidance and anxiety—(Bartholomew & Horowitz, 1991), and (5) as normative population distributions (Agishtein & Brumbaugh, 2013).

In terms of this last dimension, attachment differences/styles may be understood to represent a normative distribution of differences in degree of security, without necessarily specifying any accompanying potential impact on health. Indeed the ‘Normativity Hypothesis’ (van Ijzendoorn & Sagi-Schwartz, 2008) has argued that attachment security should be considered the norm as opposed to the less secure styles. Indeed, from this purely normative-distributive perspective, cross sectional reviews of attachment difference distributions for infants through to adults have demonstrated a fairly similar pattern where the secure styles have been demonstrated to typically cover a majority of those surveyed—58% secure, 24% avoidant insecure and 18% anxious insecure (van Ijzendoorn & Bakermans-Kranenburg, 1996). For discussion of these less secure patterns, see discussion of the following constructs.

Mikulincer and Shaver have described Attachment style as “the systematic pattern of relational expectations, emotions, and behaviors that result from a particular history of interactions with attachment figures” (2015, p. 18). As such it is typically associated with a more adult global personality-like attribution and can respond to the more general question: what is one’s attachment? Attachment difference might apply more broadly to both individual ties as well as the just mentioned more global designator. However, it is also worth acknowledging that the attachment difference/style construct is more complex than a first glance might suggest. Indeed, Fraley and colleagues have recently emphasised in their research how specific individual attachments may differ from one another as well as again from global attachment capacities or styles (2011). The next two complementary constructs offer functional depictions of the difference construct but focused more explicitly on health implications.

4.5.4 Attachment as Resilience Buffer (Construct 9)

Attachment as resilience buffer captures the functional phenomena whereby more optimal attachments and attachment styles—i.e. the more secure ones—have become associated with both better mental (Mikulincer & Shaver, 2012a) but also better physical health outcomes throughout the lifespan (e.g. Pietromonaco & Powers, 2015). Once again the value of such findings for prevention, intervention and psychiatric treatment is obvious. However, a few opening words of caution may be in order to avoid risk of theoretical overreach and conceptual confusion. First, secure attachments may indeed contribute to individual and interpersonal resilience but these influences must not be seen in

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151 A handful of attachment researchers with multiple disciplinary credentials—e.g., Rutter and colleagues (Rutter et al., 2009) in psychiatry and developmental psychopathology and Roisman (2009) in developmental psychopathology and social psychology—have notably paid close attention to these complexities.
isolation but rather as part of a wider historical series of interacting contributing processes and mechanisms (Stroufe, Egeland, Carlson, & Collins, 2005). Indeed, as Belsky and Pluess (2011) have persuasively argued, when it comes to heterogeneity of health outcomes, attachment phenomena are far from the only contributors to individual phenotypic difference.

Second, what has recently been termed the ‘attachment buffer hypothesis’ (Karremans, Heslenfeld, van Dillen, & Van Lange, 2011) directs enquiries as to how the more optimal functional outcomes identified in Construct 7 (attachment outcomes) might emerge—i.e. what might be the contributing attachment mechanisms and processes. However, indicating exactly what counts as such a buffer requires some conceptual clarification as perspectives on resilience have fragmented since the early 1970s when Gamerzzy first introduced the notion in the context of prevention work with children of schizophrenic parents in (1971). Indeed, Rutter has sought to differentiate four valuable fields of health study whilst nonetheless arguing for the greater comprehensiveness of resilience study: “The fields of competence, positive psychology, risk and protection and resilience all have importance, but it is a mistake to want to group them together” (2012a, p. 343). Although a buffer effect might fit well within the more summative risk and protection approach, I will argue it fits better with views of resilience that predominate in the field developmental psychopathology. Here risk and protective factors are incorporated within a more interactive framework which highlights both individual and contextual difference (Dante Cicchetti & Garmezy, 1993). (See Rutter (2006) for a detailed discussion of the conceptual differences between these approaches.)

Despite decades of advances within the field of developmental psychopathology that have extended more dynamic notions of resilience (Rutter, 2012a), we nevertheless lack a current conceptual consensus as to the make-up and contributors for resilience—e.g. see recent comments on target article by Kalisch and colleagues on resilience in BBS (2015b). Supkopff and colleagues (2012) have nonetheless suggested we are currently moving beyond (1) intra-individual (or trait-like) approaches and (2) context only transactional approaches, to (3) a more complete developmental lifespan approach that integrates the first two in a more cumulative historical manner (see discussion of the Organisational Construct 13 below.) In a similar vein, Rutter has broadly suggested that “It is generally accepted that resilience is defined as relative resistance to environmental risk experiences, the overcoming of a stress or adversity, or a relatively good outcome despite risk experiences” (2012a, p. loc. 1198). Once again, implicit here are two related transactional dimensions for resilience, one broadly ecological (for a discussion see Michael Ungar, 2011), the other intrapersonal, i.e. cognitive/affective regulatory (for a positive appraisal model see Kalisch et al., 2015b). I propose...
that a coherently articulated Attachment Theory position envisions both of these two dimensions within the attachment Constructs 4, 5 and 8 (ties, regulatory capacities and style). On the one hand, the possibility of gaining an appropriate level of interpersonally facilitated relief from stress in the face of adversity—be it implicit as mere assured felt presence or explicit as conversational reassurance—counts as an example of ecological facilitated resilience in line with Ungar’s model. On the other hand, individual differences in exercising positive reappraisal of the sort noted by Kalisch and colleagues that leads to relief of stress, would also count as a possible form of secure self-soothing within a framework of attachment as emotion regulation. I now turn to a depiction of what might be considered a less adaptive level of resilience, what I will term attachment risk where more specific attention is given to less secure difference.

4.5.5 Attachment Risk (Construct 10)

Attachment risk identifies potential suboptimal and more detrimental developmental pathways and their impacts on health. In some ways risk as lack of adaptability in the face of adversity may sit at the opposite end of an attachment health outcomes spectrum, i.e. as a contrasting pole to attachment as resilience buffer to adversity. Risk here is associated with a potential lack of dynamic resilience and not merely the summative ‘risk and protective factor’ approach, an important differentiation made by Rutter (2006). As with the study of resilience, developmental history, context and individual difference also play pivotal interactive roles. Indeed, one of the more important contributions of Attachment Theory has been the identification of individual attachment difference in the context of both resilience and risk. I will now briefly turn to introduce four risk constructs that manifest attachment difference. To keep the discussion manageable, I will focus for the moment on their relevance for infant and early childhood research. Adult differences will be taken up below.

Rutter (2009) has noted a degree of heterogeneity in discussions of attachment relationship risk in childhood noting at least four distinct sub-constructs within Attachment Theory. The first two are also typically associated with attachment differences or styles, namely, security and organisation, while the latter two have been associated with two specific DSM Attachment Disorders, Reactive Attachment Disorder and Disinhibited Social Engagement Disorder (previously termed Disinhibited Attachment Disorder in prior versions of the DSM III and IV.)

4.5.5.1 Insecurity and Risk

First, Insecure classifications derived from a security/insecurity categorical split have been applied to infants and younger children. They were first measured by Ainsworth and colleague’s SSP (1978) and depicted as anxious-ambivalent and anxious-avoidant, capturing interactive differences between individual infant and caregiver dyads.\textsuperscript{154} As noted in discussion of attachment patterns, secure

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\textsuperscript{154} In a preface to the 2015 republication of Ainsworth and colleagues Patterns of Attachment in the Psychology Press Classics Series, Waters (one of the original authors), Bretherton and Vaughn (2015) have drawn attention to the ABC adaptation under adversity” (2015, p. e124) missing out a range of ecologically relevant considerations. I would also reject Kalisch and colleagues endorsement of an underlying but overly simplistic hedonic model of emotions (2015b, p. 5). On the other hand, Ungar, who emerges from the more complex developmental psychopathology tradition highlighted by Garmezy and Rutter (1983), argues for multiple factors in an ecological approach to resilience.
attachments (labeled Group B) are typically seen as the consequence of consistent, sensitive caregiving interactions. In contrast, anxious ambivalent attachments (labeled Group C) might follow from inconsistent and therefore on occasion insensitive interactions, whilst anxious avoidant ones (labeled Group A) may be consistent but insensitive rejections of the infants needs (see Chapter 9 Ainsworth et al., 1978). Whether these specific risk constructs ought to be considered as categorical or dimensional has been taken up with greater detail and captured in Fraley and Spieker’s enquiries and response (2003; 2003) and invited accompanying comments (Cassidy, 2003; Cummings, 2003; Sroufe, 2003; E. Waters & Beauchaine, 2003). More current discussions in a recent volume of Monographs of the Society for Research in Child Development (Booth-LaForce & Roisman, 2014), might suggest that the argument has moved in the direction of dimensional approaches. Indeed, I will concur with Fraley and colleagues (2015) below in favour of a dimensional approach on the grounds for its support of greater complex developmental causality.

4.5.5.2 Disorganisation and Risk

A second related but nonetheless unique dimension, organisation/disorganisation, identified and differentiated early maladaptive attachment—i.e. disorganised attachment—form behaviours that did not fit well within the original Ainsworth tripartite security/two forms of insecurity division. This second development has been associated with the work of Main and colleagues (1986)—researchers who have incidentally not always distinguished the security construct from the organization construct. Alternative formulations for assessing more complex maladaptation beyond the insecurity construct have also been developed, notably in Crittendon’s Dynamic Maturational Model (2000) that emphasises interaction with ongoing affective/cognitive development into later childhood. Whereas the first set of insecurity risks is better described as suboptimal—individuals so identified may be also be seen as highly functional in many areas of life (Ein-Dor, Mikulincer, Doron, & Shaver, 2010)—, disorganisation is generally seen as maladaptive if not pathological.

4.5.5.3 DSM and Attachment Risk

The final two attachment risk constructs have been associated with the inclusion of attachment disorders within the DSM tradition beginning with the DSM-III in 1980 and carrying forward to the most recent 2013 DSM-5 (Zeanah & Gleason, 2015). These disorders emerged well before the introduction in the mid to late 1980s of the developments regarding the maladaptive or disorganized attachments just discussed. In the DSM-5 these third and fourth attachment risk constructs are labeled respectively Reactive Attachment Disorder (RAD) and Disinhibited Social Engagement Disorder (DSED), and categorized as Trauma- and Stress-related Disorders (American-Psychiatric-Association, 2013). Both of these disorders are associated with early extreme social neglect

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Classification System in the SSP where ‘maternal sensitivity’ is described as consisting of four constituent facets: “sensitivity to infant signals, cooperation with ongoing behavior, acceptance of the infant’s needs, and physical and psychological availability” (p. xxviii). Efforts at aligning the two insecurity differences with evolutionary considerations have helped to de-pathologise readings of insecurity in terms of differences in adaptive strategies (Simpson & Belsky, 2008, pp. 137-139), especially in the context of r- and K- reproductive strategies in response to unstable (the two forms of insecurities) or stable environments (security) (Draper & Belsky, 1990). Similar efforts to do the same with the more pathological maladaptive disorganisation and Disinhibited Social Engagement Disorder (DSED) would appear less convincing.
particularly in the context of institutional and foster care and therefore originally associated as a single disorder.

Zeanah and Gleason have depicted RAD as “an emotionally withdrawn/inhibited phenotype” (2015, p. 1), a condition that might be described as an inability to make or engage attachments, due in part to a lack of early adult availability. They describe DSED as “an indiscriminately social/disinhibited phenotype” (2015, p. 1), focused on inappropriate social interactions marked by an absence of stranger anxiety. As mentioned, although previously considered as subtypes of a single attachment disorder, they are now seen as distinct with differing causes and symptoms. Indeed, the change of name to DSED in DSM-5 from Disinhibited Attachment Disorder in DSM-IV has been interpreted as a removal of DSED from attachment phenomena. However, Lyons-Ruth has also recently discussed the ‘unsettled case’ for the exclusion of DSED as attachment phenomena, “If we are to move away from an attachment framework as the most comprehensive and empirically grounded framework for understanding and preventing DSED, an equally powerful alternative framework needs to be advanced” (2015, p. 226). The two disorders have in ways been traditional outliers to the main theoretical research tradition: one focused on a lack of attachment, the other with behavior to non-attachment figures. My identification of these two alongside insecurity and disorganisation constructs is primarily for sake of coherence and clarity. As Rutter and colleagues have reminded, “It is seriously misleading to view all of these patterns through the lens of security/insecurity” (2009, p. 529). The final set of constructs provides a closer look into the ‘how’ or the mechanisms for attachment phenomena: causal stories ideally should enlighten our understanding of current function and eventually inform intervention.

4.6 Mechanistic Constructs (11-12)

Two sets of related attachment mechanisms have been identified for this exercise, the distinction reflecting in part a choice for both a manageable and informative discussion for levels of analysis: one psychological—internal working models (IWMs), Construct 11—, the other a decidedly catchall multi-level, biological one, Construct 12. Petters, a cognitive scientist whose work in attachment will be described below, has described such a split as an example of high and low level implementations of

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156 “Core features of RAD in young children include the absence of focused attachment behaviors directed toward a preferred caregiver, failure to seek and respond to comforting when distressed, reduced social and emotional reciprocity, and disturbances of emotion regulation, including reduced positive affect and unexplained fearfulness or irritability.” (Zeanah & Gleason, 2015, pp. 1-2).

157 “Core behavioral features of DSED include inappropriate approach to unfamiliar adults and lack of wariness of strangers, and a willingness to wander off with strangers” (Zeanah & Gleason, 2015, p. 2).

158 The disorders have been considered as examples of ‘atypical attachment’ in Chapter 29: Challenges faced by Foster and Adopted Children in The Handbook of Attachment (Dozier & Rutter, 2008) and have also been explored in comparative studies between disorganisation and the attachment disorders (van Ijzendoorn, 2003).

159 One avoidable consequence for my choice would be a superficial reading of the selection of a pair of distinct “mind and body” constructs. However, this paired mechanism selection is in no way a commitment to either dualism or reductive materialism. In describing Bowlby’s TLC strategy in Chapter Three, Attachment Theory was been proposed as a fundamentally a bio-psycho-social endeavour: a non-reductive materialist philosophical position. (See further discussion of causal influence and causal direction in Chapter Six.)
attachment,\textsuperscript{160} where he notes the high level IWMs are to be associated with the functioning of the attachment behavioural system, Construct 1 (2006, p. 38).

To avoid any confusion, it may be worth noting that although utilizing similar terminology, Petters' employment of lower and higher `implementation levels' does not reflect Marr's influential levels analysis in cognitive science nor the approach proposed by Wimsatt in biological discussions.\textsuperscript{161} Marr had distinguished three levels in his work on vision: the functional, the psychological/computational and the biological level of 'implementation' (1982). Nor does the distinction employed here intended to reflect the use of level made by Wimsatt (1994) whereby biological explanation is seen to be restricted to within a level activity, not between levels. Indeed, Murphy has suggested that neither what he designates as Marr's epistemic employment of level nor Wimsatt's natural organizational approach are adequate for more inherent inter-level complexity potentially encountered in psychiatry and nosology (Murphy, 2006, p. 103). I will argue below in Section 6.6 that Murphy's observations apply equally to the complexity of attachment phenomena. Indeed, the introduction of more complex approaches to levels of analysis can be seen throughout Bowlby's mature theoretical work (beginning with 1969/1982) as well as within previous reviews of attachment research (e.g. Pietromonaco & Barrett, 2000a).

One implicit challenge in approaching the nature of attachment mechanisms is accounting for potential complexity, something recognized quite regularly by researchers (Coan, 2008; Fox & Hane, 2008). On the one hand, from a Tinbergen approach, attachment mechanisms would be concerned with current operations of attachment processes: what he identified from Lorenz as "initiation, coordination and cessation" of complex patterns of behaviour (1963, p. 413). On the other hand, Tinbergen also noted in the same discussion that such activities were truly interactive possessing both internal and external causal influencers (1963, pp. 413-416). Equally the expected lifespan heterocontinuity and plasticity of attachment phenomena—i.e. attachment systems evolve with developmental experience over time in the context of differing attachment relationships—and the ongoing role of changing ecological/interpersonal causal influences, also preclude singular mechanistic models. Indeed, although the Tinbergen focus for mechanistic investigation is well defined, in practice attachment researchers studying underlying causal process may not have followed this path quite so clearly. Attachment mechanistic research has evolved in what might be depicted as a more pragmatic if not fragmented direction, from the study of 'individual differences' to research on possible 'developmental antecedents' for those differences and, perhaps more recently, to the examination of the actual 'operation' of the attachment system IWMs. A comprehensive review of mechanism research is beyond the range of this thesis. However, I would like to fill in a few of the key details of the mechanism constructs next and return to the story in Part Three.

\textbf{4.6.1 Attachment Internal Working Models

\textsuperscript{160} Gillath (2015) has also employed similar terminology describing macro-level and micro-level attachment process research.

\textsuperscript{161} Petters' dissertation (2006) makes no mention of ideas by either Marr or Wimsatt.
Attachment Internal Working Models (IWMs) (Construct 11) (or mental mechanisms) have been depicted as psychological mechanisms that emerge in development. The employment of the plural ‘models’ has provided conceptual space for the inclusion of multiple components—especially ‘self’ and ‘important others’ in the environment—whilst the ‘working’ label indicates expectations of change or plasticity over time. Indeed, Bowlby described the development of an attachment system where such “control systems become increasingly sophisticated, in part by their coming to incorporate representational models of the environment and important people in it and also of the self as a living active person” (1988, p. 62). Gillath and colleagues have recently re-emphasised how Bowlby reframed “psychoanalytic concepts such as internalization and object representations . . . using theories in cognitive psychology” (2016, p. loc. 2164). As noted in Chapter Three, the employment of a Craik-like IWM concept counted as one of Bowlby’s key engagements with the cognitive sciences and information processing.  

Historically, the study of IWMs within Attachment Theory was initially slow to start in comparison to the study of other attachment constructs, not doubt influenced by change in the cognitive landscape from Bowlby’s days (see review of IWM study in Bretherton & Munholland, 2008, 2016; Gillath et al., 2016). Consequently, conceptual gaps and overreach regarding IWMs have regularly been seen as sources of confusion or theoretical hindrance (e.g. Belsky & Cassidy, 1994). However, much progress has also been made since those early days. IWMs may count as one of those places in Attachment Theory where compiling a brief picture is difficult: there is an expanding range of literature with which to contend. Research into IWMs has also typically evolved around (1) functional/outcome questions associated with security, (2) conceptual discussions of mental mechanisms and their contents, (3) employment of specific measures, and (4) application of IWM research across the various lifespan periods. For sake of brevity, only a partial selection of the four follows.

Thompson (2015, p. 227) has noted that attachment theorists often cite three functions associated with the operation of IWMs: prediction, interpretation and self-regulation. These are processes in service of the functional goal of security. They underpin interactions with specific attachment figures along with the more general attachment regulatory capacities and attachment styles mentioned above. Thompson has also noted a range of corresponding outcomes including, “emotion understanding, social problem-solving skills, self-concept, conscience, emotion regulation, attributional biases, and

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162 For a detailed look at Bowlby’s ideas for IWMs see Bretherton & Munholland (2016) description of his 15 major postulations.

163 Bretherton & Munholland have identified four areas that may engender possible misunderstanding or disputation as regards IWMs:

1. Whether attachment working models are to be considered as relationship-specific representations or general strategies;
2. how to understand the stability and change of working models;
3. to what degree and under what circumstances internal working models are consciously accessible and subject to defensive processes; and
4. how to conceptualize the processes in the intergenerational transmission of attachment working models (2008, p. 104).

Based on this chapter’s ontogenetic constructs discussion, the answer to the first question would appear to be both. The second and third questions remain major open questions for the theory. Finally, the fourth question of intergenerational transmission is probably best considered in terms of an expanded mediational model, where additional transmission influences beyond caregiver sensitivity interact dynamically (Verhage et al., 2016).

165 Indeed, the field probably lacks a good monograph devoted solely to the subject matter.
memory” (2008a, p. 348). Others have noted similar but slightly differing lists (Gillath et al., 2016). I will return to these IWM related functions/outcomes in discussions of cognition and GCPs in Chapters Seven and Eight.

A summary of relevant contents of IWMs includes more than the representations of ‘self’ and ‘other’ noted above. Gillath and colleagues (2016, loc. 2194) have also identified four attachment-related building blocks for IWMs: (1) memories, (2) beliefs, attitudes expectations, (3) goals and needs, and (4) plans and strategies. One might also add that these building blocks have typically been seen as present at implicit or procedural levels—i.e. they may emerge initially in infancy and remain out of awareness—but may also nonetheless be partially accessible at more explicit levels, especially in terms of an individual’s personal narrative—something typically addressed in an attachment focused psychotherapy (Taylor, Rietzschel, Danquah, & Berry, 2015). A few of these details here will be further engaged in Part Three’s discussion of a GCP. However, one additional dimension for exploring IWMs can be noted: how might IWMs be structured so as to incorporate both specific models and more personality like global models? The answers have proved more challenging, but Gillath and colleagues have pointed us in the direction of the need for greater conceptual dynamism here. As depicted in Figure 4.3 below, they have added feedback loops between specific and global models into Collins and colleagues (Collins & Allard, 2001; Collins & Read, 1994) more traditional hierarchical approach. I will provide something similar in discussing proposals for an attachment Dynamic Lifespan Personality Construct in Chapter Six.

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165 In discussing possible relationships between maternal and paternal IWMs, Bretherton (2010) has summarized research on the question of influence of multiple relationship specific models here: (1) monotropy—the now disregarded idea that only one caregiver is represented within—, (2) a hierarchical approach, (3) an independence one—where figures may perform unique roles and functions—, and (4) an integrated model, How models function would also appear to differ by lifespan period.
Research into IWMs was initially conducted in the context of the early development of attachment-caregiver representations (Bretherton, 2005; Thompson, 2008a). This has been extended into later childhood, especially via employment of the ‘secure script’ measurement modeled on the SSP (see discussion in H. S. Waters & Waters, 2006) and now includes adolescence (Furman, Simon, Shaffer, & Bouchez, 2002). At least three strands of adult research that seek to measure IWM influence can be identified: (1) Main & colleague’s (1985) influential ‘move to the level of representation’ in the field of developmental psychology based in part on their founding work with the Adult Attachment Interview (AAI)\(^ {166}\) (see discussion of Main’s ‘move to the level of representation’ and AAI in Chapter Five), (2) work in the field of social psychology, especially in regards romantic attachments (e.g. Pietromonaco & Barrett, 1997), and (3) adult clinical research indicating both impact of attachment upon therapeutic process/outcome and change in global attachment style as a consequence of therapy (Mikulincer, Shaver, & Berant, 2013). This last consideration of change in a less functional attachment style (or regulatory capacity) would count as a philosophical example whereby function retains both adaptive and mechanistic dimensions. One of the potentially less noticed advantages of Tinbergen approach is its allowance for what the philosopher of science Godfrey-Smith has recommended: “We should accept both senses of function, and keep them strictly distinct . . . The difference is in the type of explanation” (1993, p. 202). For a more detailed review of these developments in the context of the historical developments of Attachment Theory, see Chapter Five.

One final notable development also merits mention here before closing this introduction to the IWMs construct. First, Petters and Waters (2014)\(^ {167}\) have recently provided several conceptual reviews of IWMs from an information processing perspective, including interesting suggestions for a revised ‘embodied’ approach for IWMs, i.e. an implicit endorsement of a multi-level IWM construct. These proposals build on Petters’ earlier dissertation, Designing agents to understand infants (2006), where Attachment Theory has been interestingly integrated into a Cognitive Science / Artificial Intelligence (i.e. robotics) problem solving platform. His computational focus was on infant attachment behaviours.

“The explanation of how and why these behaviours are produced constitutes the problem that this thesis is attempting to solve . . . The problems are explaining what kind of information processing architectures may give rise to a set of infant behaviours linked to the phenomenon of attachment . . . The solutions to this problem are in the form of designs and implementations of autonomous agent simulations” (2006, pp. 49-50). This initial success would appear to represent a first integration of attachment theory, evolutionary psychology and artificial intelligence.\(^ {168}\)

\(^{166}\)See review in Bretherton and Munholland (2008, pp. 114-122).

\(^{167}\)Waters has a long history as a proficient attachment researcher including being part of the original SSP team with Ainsworth (Ainsworth et al., 1978) and introducing Attachment Theory to G. Alan Sroufe and the University of Minnesota where their collaboration on conceptual work has been highly influential (Sroufe & Waters, 1977; E. Waters, Wippman, & Sroufe, 1979).

\(^{168}\)As Petters highlights, “One of the most significant contributions of this work is that it shows how [the identified] varying facets of Attachment Theory can be integrated within simulations . . . Prior to this work theories that attempted to explain attachment phenomena were expressed in words and diagrams only. This work is the first realisation of Attachment Theory that is precise and detailed enough to be implemented in a computer program” (2006, p. 169 & 171).
4.6.2 Attachment Biological Mechanisms

Bowlby suggested that the attachment system might be activated by multiple classes of causal factors . . . “all acting together . . . each class of factor interacts with all the others, the texture of causal conditions is as intricate as a Persian carpet” (1969/1982, p. 87).169 The more inclusive attachment biological mechanisms (Construct 12) may capture the breath of both Bowlby’s original expectations and current attachment multi-leveled research within a single designated construct. However, without adequate conceptual clarity, serious risk exists for confusion when discussing any potentially entitled ‘biology of attachment’.

At least four concerns must be clarified here: three dimensional, one methodological. A first question asks which levels have been studied and can be straightforwardly addressed. A second set of conceptual enquiries parallel those noted above for IWMs. What is the specific object of study: Is biological research focused on attachment differences, antecedents or current regulatory processes operating within global and specific relational contexts? Equally, a third related question asks at what point in the lifespan might research be considered: early infancy, childhood, adolescence or adulthood? A final fourth query raises the methodological question of the appropriate value of non-human animal research for human attachment, which remains controversial and unresolved (for a detailed review see Steel, 2008).

The first question raised concerns for the specific levels of research. Fox and Hane (2008) have offered a simple two-level breakdown for the studying the biology of human attachment: psychophysiological and neuroscientific. However, sublevels for these two are also easily identified based on the array of biological phenomena measured. Indeed, the same study may also investigate multiple levels. The neurobiological and psychobiological170 Research on human attachment has included minimally studies at each of the following levels: genetic/molecular (Fraley, Roisman, Booth-LaForce, Owen, & Holland, 2013; Raby, Cicchetti, Carlson, Egeland, & Andrew Collins, 2013), epigenetic including GxE interactions (Jones-Mason, 2011), neuroanatomical—including, for example, the roles of cortical asymmetry (Schore, 2005), amygdala (Lemche et al., 2006) and prefrontal cortex (PFC) (Gillath, Bunge, Shaver, Wendelken, & Mikulincer, 2005) —, neurochemical—especially the role of oxytocin171 and neural oxytocin receptors in social bonding (Buchheim et al., 2009)—, HPA/endocrine (Hostinar, Sullivan, & Gunnar, 2014) and autonomic systems (Porges, 2011) where

169 Bowlby’s starting point for discussions of complex causality here were ideas common to both ethological and neurophysiological discussions. His first causal discussion was taken up under the heading of “Activation and termination of behavioural systems” (Bowlby, 1969/1982, pp. 85-97) where he specifies complex interactions of a comparable kind between hormone level, environmental stimulation, and organisation of behavioural system within the CNS are now known to play causal roles also in the behavior of lower animals” (1969/1982, p. 91). He adds a following chapter entitled “Appraising and selecting: feeling and emotion” (Bowlby, 1969/1982, pp. 104-123) that seeks to expound on human specific attachment systems that explores the role of what I have noted in Chapter Three a relatively innovative notion of ‘felt appraisal’.

170 The employment of more traditional term psychobiological has been associated with stress and HPA research (Levine, 1972) and marked an early effort to both avoid biological reductionism at the expense of the psychological and embrace the relevance of animal study. Michel has suggested that, “Psychobiology” meant psychological phenomena were the primary focus, not reducible to physiological processes, but considered an aspect of biological science” (2013, p. 211). The notion of neurobiology—a subset of both neuroscience and biology—has expanded the psychobiological non-reductive study of stress by integrating the Central Nervous System as studied within neuroscience. For a discussion of this later synthesis see McEwen (2007).

171 Oxytocin is technically not a neurochemical but more correctly a hormone, or neuropeptide hormone synthesized in the hypothalamus (Nave, Camerer, & McCullough, 2015).
physical measures have included levels and changes in heart rate, cortisol and EEG asymmetry (Fox & Hane, 2008). The relative state of confidence in the comprehensiveness of these explanations emerging from research at these multiple level varies; equally, no single level account should be expected to be singularly sufficient (Freedman, 1992).

The second question concerns the specificity of attachment objects—or constructs—studied in biological research. The granularity of the particular studies may confound any claims to simple claims such as ‘the biology of attachment’ or ‘neuroscience of attachment’. As noted above, neurobiological studies have variably focused on attachment difference (Benetti et al., 2010), attachment developmental antecedents (Gervai, 2009) (Landers & Sullivan, 2012) and attachment operations (Myron A. Hofer & Sullivan, 2008). However, these studies may also overlap and fail to clearly fit within singular attachment research categories. As noted above, based on Tinbergen’s approach, the primary focus of mechanistic study is the current operations of attachment processes. However, the consequence of the study of multiple attachment objects means for example that genetic or neuroanatomical studies of attachment may be less comparable than on first glance. Care must be taken to ensure that the specifics of attachment are carefully clarified before drawing broad conclusions.

The third dimensional question concerns the specific lifespan period studied. This may easily lead to a confounding challenge if writers either fail to adequately differentiate lifespan differences (Cozolino, 2014) or give ontogenetic functional priority to a single, typically earliest period (Schore & McIntosh, 2011). Nonetheless, more differentiated neurobiological studies of attachment have been conducted for infancy (Moriceau & Sullivan, 2005), later childhood (Borelli, West, Weekes, & Crowley, 2013), adolescence (Gallo & Matthews, 2006) and adulthood (Vrtička & Vuilleumier, 2012). The majority of neurobiological lifespan study has focused on infancy and adulthood. This is probably to be expected as the former uniquely captures each of the three attachment objects studied—antecedents, difference and operation—whilst the latter represents the largest segment of species population.

Finally, a fourth methodological question raises the employment of non-human animal studies within Attachment Theory. Although depicted in my thesis as a human developmental theory, attachment studies extend well beyond humans, a fact discussed in Chapter Three and Section 4.2.2 above. Indeed the initial suggestions for biological approaches to attachment emerged from non-human animal studies, especially Harlow’s initial studies in the 1950s. Important informative attachment neurobiological studies have been conducted with rhesus monkeys (Suomi, 2008) (Kraemer, 1992) prairie and montane voles (Insel, 1997), laboratory rats (Myron A. Hofer, 1987) and mice (Masson et al., 2006). Unfortunately some neurobiological summaries would appear to fail to adequately differentiate between these informative studies and unhelpfully prematurely generalize these findings to human populations (e.g. Cozolino, 2014).172 Feldman and colleagues work discussed in Chapter

172 Cozolino’s (2014) ‘popularized’ but speculative work is often quoted in clinical discussions. Although Cozolino accepts the limitations of animal studies, he goes on to discuss as if they nonetheless generalise to human populations. My point here is not to dismiss popular works of science as they obviously play an important role in the field of science. Indeed, Cozolino’s work is importantly accessible to clinicians but may occasionally miss the relatively uncertain state of neurological research. For broader discussions of such issues in the ‘popular realm’ see the journal, Public Understanding of Science.
Six offers a good example of a cautious and informed approach to the employment of animal research. The array of possible biological levels and multiple objects of study also highlights the more fundamental issue of accounting for developmental complexity within attachment study, something I now address.

4.7 An Organisational Construct: Combining Ontogeny, Function and Mechanism

4.7.1 The Attachment Organisational Construct

Finally, a last more dynamic lifespan construct, the attachment organisational construct (Construct 13) can also be identified. As this construct will be taken up in greater detail in Chapter Six I will be relatively brief here. As depicted in Figure 4.1, this construct does not fit within any of the four individual Tinbergen realms. It may rather partially encompass each of the four, or at least minimally the three phenotypic discussions. The presence of these overlaps in a single construct may also point to possible conceptual limits of a Tinbergen framework for unraveling biological complexity.

The notion of organisation also encompasses at least two valuable perspectives: attachment as a multifaceted lifespan entity and attachment as unfolding biological process. Sroufe, Waters and Matas introduced the attachment organisational approach in the 1970s (Sroufe, 1979; Sroufe & Waters, 1977; Sroufe, Waters, & Matas, 1974).

... Attachment refers to an affective tie between infant and caregiver and to a behavioral system, flexibly operating in terms of set goals, mediated by feeling, and in interaction with other behavioral systems. In this view, behavior is predictably influenced by context rather than constant across situations (Sroufe & Waters, 1977, p. 1185).

The definition of attachment here may be productively contrasted with the two offered by Bowlby and Ainsworth in the epigraph at the start of the chapter. Relevant for attachment has been the inclusion within the organizational construct of matters such as the role of context; coherence of organisation over time; the interplay of emotion, cognition and social behaviour; and a focus on individual difference. The introduction of a revised organisational construct has also been accompanied by a prospective longitudinal method capturing the study of attachment and its interaction with multiple facets (Sroufe, 2005). This shift has arguably come to represent a pivotal conceptual development for Attachment Theory, one importantly accepted by Bowlby in his own updated theoretical considerations (1969/1982, pp. 371-375).

4.7.2 Flagging Extensions and Implications of the Organisational Perspective

Chapter Six will return to a discussion of the thirteenth construct. In particular, three important logical extensions of the organisational construct will be considered. I will suggest how this construct might also be considered a dynamic lifespan personality construct (DLPC), something that captures how the evolving history of difference in attachment capacities might be understood as similar but nonetheless a unique approach to personality differences. Next, the construct will grounded in broader developmental science, whereby attachment can be conceived as a developmental system as envisioned within developmental systems theory (DST). Last, I will also turn to the implications these
extensions of the organisational may hold for psychiatry and the philosophy of psychiatry. One consequence of this perspective is the need to re-emphasise the inadequacy of traditional ‘continuity only’ position.

4.8 Chapter Conclusion

According to Tinbergen, we can distinguish four biological explanations. Following this conceptual guidance, Chapter Four has gleaned twelve relevant constructs across those four domains, plus a thirteenth organisational construct. The twelve have permitted a more clearly differentiated discussion from which the theory might be approached. Indeed a few of the constructs have required considerable discussion to adequately explain their role in the theory. The chapter has also indicated initially where and how attachment may offer interesting possibilities for the field of psychiatry, notions expanded in Chapter Six. Most notably here are the identification of attachment causal antecedents, attachment outcomes, attachment resilience/risk and the underlying attachment mental mechanisms. The organizational construct would appear to not fit the Tinbergen framework. However, it has shown promising success in charting longitudinal pathways for individual development. Consequently, attachment can now be demonstrated to interactively combine earlier historical experience with later ongoing contextual development across the lifespan.

This concludes my assessment of the current constructs for Attachment Theory. Two tasks remain in Part Two. Chapter Five provides an historical survey of Attachment Theory with a focus on theory evolution. This will further support the identification of past controversies, resolutions and unresolved questions. Chapter Six will conclude Part Two’s critical assessment of attachment. As just noted, this chapter will critically expand the organisational perspective within Attachment Theory, whilst identifying important Implications for psychiatry.


Ein-Dor, T., Mikulincer, M., Doron, G., & Shaver, P. R. (2010). The attachment paradox: How can so many of us (the insecure ones) have no adaptive advantages? *Perspectives on Psychological Science, 5*(2), 123-141. doi:10.1177/1745691610362349


CHAPTER FIVE: Understanding the conceptual evolution of Attachment Theory

Those ignorant of history are not condemned to repeat it; they are merely destined to be confused. (Gould, 1977, p. 4).

5.0 Chapter Introduction

Attachment Theory research has undergone a considerable degree of complex unwinding over the past 65 years, perhaps more so than most commentators might recognise (but see Barrett, 2006, p. 11). Simply put, the historical story of Attachment Theory I present here is one of a movement from less adequate, even simplistic beginnings, to increased complexity, empirical sophistication and broader acceptance. But this narrative is also an incomplete one: Attachment Theory is hardly a closed book with few unresolved questions. Indeed, Gould’s adage above may well be applied to the many typical discussions of Attachment Theory where triumphant narratives reside (e.g. Sears & Sears, 2001) alongside seemingly conclusive indictments (Harris, 2009; Vicedo, 2013): confusion begets further confusion. Now, a lack of historical perspective is clearly not the only source of misconceptions; this thesis demonstrates that the seeds of confusion are indeed multiple. However, with Gould, I want to also argue that a lack of adequate grounding in the historical evolution of the theory may be crippling, if not fatal, for grasping adequately its current significance.

In short, the chapter will demonstrate how Attachment Theory has evolved from a more traditional linear developmental theory to a more contemporary complex dynamic one. A case will also be made for the near consensus acceptance of the theory by the scientific community. From a conceptual perspective, despite incredible expansion along several important dimensions, a less noted gap in consideration and take-up of Bowlby’s Phase Four Partnership is highlighted. Lastly, a more sociological observation identifies the loss of conceptual gatekeeping for the field with the passing of both Bowlby and Ainsworth followed by an appeal for a return to Bowlby’s original TLC strategy for settling conceptual disputes.

The chapter will proceed in six steps. I will first (Section 5.1) set out the aims of this historical survey, namely a focus on theory evolution. In doing so four periods of development will be presented: Section 5.2 A Pre-theoretical Period: A First Recognition of Childhood Environmental Risk (1927-1951); Section 5.3 ‘Maternal Deprivation’ and Subsequent Ethological Theory Development (ca. 1951–1969); Section 5.4 Attachment Theory Maturation Period (1969-1990); Section 5.5 Post-Bowlby Exponential Multidimensional Expansion (1991-Present). After a brief description of the theoretical advances within each period, I will conclude with a brief summary Section 5.6.

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173 There would appear to be an unavoidable but curious semantic challenge in producing prose that seeks to depict the historical unfolding of Attachment Theory. The very terms one might employ to describe change—i.e. that a theory may be depicted as ‘growing’, ‘developing’, ‘evolving’ or ‘maturing’—are themselves also crucial conceptual terms with bearing on the theory itself: i.e. ‘growth’, ‘development’, ‘evolution’ and ‘maturation’.

174 Gould’s quote points to the value in taking a historical perspective in deciphering the relationship between evolution and development. His insights would seem particularly appropriate here for the historical evolution of a developmental theory.
5.1 Aims for a History of Attachment Theory

5.1.1 Historical Continuity and Change in Theory Development

The aim in this chapter is to condense the history of a research tradition that extends from the first half of the last century to the present, one that has become truly prolific—accepted by critics (for a recent example see Vicedo, 2013) and supporters alike (e.g. Rutter, Kreppner, & Sonuga-Barke, 2009). However, there appears to be no up to date comprehensive history of the theory.\footnote{Historiographical approaches have included depictions in terms of Bowlby’s life periods (Barrett, 2006); in simple ethological terms (M. D. Ainsworth & Bowlby, 1991; van der Horst, 2011); via periods of theory building and consolidation (Bretherton, 1992); from a cultural-historical perspective (K.E. Grossmann, 1995); relative to divergence and convergence with psychoanalysis (Eagle, 2013; Fonagy, 2001; J. Holmes, 1993, 2010; Mitchell, 2000); within a broader historical eclecticism (Karen, 1994, 2008; van Dijken, 1998); in terms of dominant scientific models—ethology, psychobiology, meaning-making/inner-subjectivity (Kraemer et al., 2005); as stages in unity/differentiation cycle of theory and research (Crittenden, 2000); and even more philosophically, as a Lakatosian research program (van Ijzendoorn & Tavecchio, 1987).} A detailed narrative would no doubt be a valuable thought exercise but it could encompass multiple volumes. So the challenge here is to be particularly focussed in order to quickly establish in a critical fashion my important argument: Attachment Theory has not been a static endeavour but rather has evolved and continues to evolve over time as an unfinished but nonetheless successful research enterprise. Fortunately, a growing corpus of historical studies now exists that make this task slightly more manageable.

5.1.2 Recognition of Previous Historical Study

Karen’s relatively thorough narrative (1998)\footnote{Throughout this thesis, I will quote from the 1998 reprint with the slightly altered title, Becoming attached: First relationships and how they shape our capacity to love (Karen, 1998). Karen’s narrative is primarily historical and directed to a non-philosophical audience. Conceptual disputes are addressed throughout but as one would expect, little systematic treatment of underlying philosophical assumptions and conceptual progress are provided. He has also provided a follow-up reflective piece (Karen, 2008) but again without the sort of analytic treatment required for this thesis.} and Holmes biography (1993)\footnote{Holmes has published an updated second edition “with a particular emphasis on the contribution of attachment ideas to the practice of psychotherapy” (2014, p. xii).} provided the first general historical surveys of Attachment Theory. Both are clearly supportive\footnote{Karen provides a helpful depiction, one with which I am sympathetic. Holmes takes a decidedly psychoanalytic perspective, one that requires closer scrutiny, something I will take up in Chapters Six, Seven and Eight.} of the theory; Karen’s work is now obviously limited in terms of later developments. These have been supplemented by several scholarly treatments—many relatively recent—that have brought to bear insights gleaned from a fresh look at newly available primary sources, especially the archives of relevant figures, correspondence and interviews with colleagues and family. Such works have focussed on Bowlby’s early life (van Dijken, 1998), his early intellectual influences (van Dijken, van der Veer, van Ijzendoorn, & Kuipers, 1998), the social context of and his impact on pre-World War II (Newcombe & Lerner, 1982) and post-war World War II UK politics (Mayhew, 2006)—where he is depicted as part of a more democratic socialist leaning endeavour—, and an unpublished dissertation on his partnership with Ainsworth as well as the work she carried out with her numerous students (Isaacs, 2007). More recently, Bowlby’s valuable engagement with the field of ethology (van der Horst, 2011) and a reinvestigation of the theory’s early shortcomings and the reception of attachment ideas in the America by more ‘traditionally minded’ proponents up to 1970 (Vicedo, 2013).} Helen Barrett (2006)
has also compiled an important detailed critical examination of the theory's tenets but the work does not include a systematic historical perspective.\(^{39}\) Finally, a brief updated history of the interaction of Attachment Theory and the field of psychoanalysis has also recently emerged (Eagle, 2013).

### 5.1.3 Proposing Four Periods of Attachment Theory Development

I propose a relatively simple historical sketch: four periods divided by three significant pivotal milestones. The four periods include a ‘Pre-theoretical Period: A First Recognition of Childhood Environmental Risk’ (1927-1951), a ‘Period of Maternal Deprivation and Initial Ethological Theory Development’ (1951-1969), a ‘Period of Attachment Theory Maturation’ (1969-1990), and lastly, a slightly tumultuous "Period of Attachment Theoretical Expansion" (1990-Present). The three historical milestones dividing these periods are: (1) Bowlby’s 1951 WHO publication of the *Maternal care and mental health* upon which he entered the world stage of childhood mental health,\(^{40}\) (2) Bowlby’s 1969 publication of *Attachment*, the first volume of his theoretical trilogy, and (3) 1990, the year of Bowlby’s death and the end of the Bowlby-Ainsworth era.

### 5.2: Pre-theoretical Period: A First Recognition of Childhood Environmental Risk (1927-1951)

Three points can be highlighted from this pre-theoretical time. Bowlby’s ongoing exposure to a broad range of intellectual and empirical influences (5.2.1), his early willingness to dispute core principles within the field of Psychoanalysis (5.2.2) and, in particular, his emphasis gleaned from clinical experience on environmental risk in childhood (5.2.3).

#### 5.2.1 Bowlby’s Early and Ongoing Exposure to Multiple Intellectual Influences

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\(^{39}\) I remain sympathetic to H. Barrett’s exceptional effort: *Attachment and the perils of parenting: A commentary and a critique*. She has insightfully observed “Surprisingly few writers have seriously addressed the task of teasing out the truth from the fiction that surrounds Bowlby and his theory of attachment . . . There is a tendency for those [books on attachment] directed at less specialist audiences to over-simplify or, at times, to misrepresent the ideas they present and to present rather one-side views which, though engaging, may be of dubious evidential status” (2006, p. 11). Equally, as an avowed attachment supporter—as both a researcher and policy advocate—her efforts here at bringing a critical perspective to the subject matter on behalf of practitioners are exemplary. At time of publication, she served as “the Chair of the International Attachment Network, an educational charity promoting wider understanding of Attachment Theory” (Barrett, 2006, p. 2).

\(^{40}\) I am appreciative of Marc de Rosnay’s suggestion to include a focus on the WHO Report.
Bowlby is noted for his capacity to reach into numerous fields of enquiry when he sought to study developmental phenomena (van der Horst, 2011). This inclination emerged early (for a detailed survey of this period, see van Dijken, 1998). His chosen undergraduate honours study (Tripos) at Cambridge from 1925-1928 provides a first example. This two-part curriculum would prepare him for future entry into medical school and thereafter psychiatry. The Natural Sciences portion of the Tripos included evolutionary biology whilst the Moral Philosophy section provided exposure to early cognitive psychology and the specific work on memory pioneered by Frederic Bartlett (van Dijken, 1998, pp. 38-43). In 1928, before commencing medical school, Bowlby would also teach for a year in a ‘progressive school’ where he would gain exposure to ‘new education’—a form of schooling that applied ideas from ‘new psychology’ (van Dijken, 1998). From 1929-1937, Bowlby completed studies in medicine and psychiatry, gained entry into psychoanalysis and commenced PhD studies in psychology. He began child clinical work in parallel with these various fields of studies—something that would lead to conflict with his psychoanalytic supervisors and colleagues (see discussion below). Van Dijken (1998, pp. 103-128) provides a detailed look at Bowlby’s service as a Psychiatrist during World War II, where Bowlby saw first hand the impact of separation on children as well as war’s impact on soldiers. But it may have been his experience at designing and implementing follow up research on officer selection that gave him exposure to statistical methods that he would apply in his first major work (see discussion on 44 Thieves below). After serving an assignment at the London Child Guidance Clinic during the war, Bowlby landed a position heading the child treatment section at the Tavistock Institute (Rustin, 2007). It was here that he would forge his first notable collaboration, with the social worker John Robertson (Alsop-Shields & Mohay, 2001). This partnership would eventuate in the 1952 controversial documentary A Two Year Old Goes to Hospital (for a discussion see Karen, 1998, pp. 67-86; Robertson, 1952). As this very brief summary demonstrates, from the outset of his career Bowlby benefitted from an exceptionally broad intellectual, empirical and clinical exposure.

5.2.2 Bowlby’s Early Relationship with Psychoanalysis

Bowlby appears to have had a challenging relationship with his colleagues in the British Psycho-Analytic Society from the beginning of his training. Van Dijken (1998, p. 76) acknowledges an unpublished Bowlby note where he described himself in this period as ‘a young man not given to

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182 Bowlby’s engagement with notions of memory, schemas, and internal models bears remarkable similarity to the early work of Bartlett (see detailed discussion in Chapter Ten). Although Bowlby makes no explicit mention of Bartlett’s influence from undergraduate days in his works, I would agree with van Dijken (1998, p. 43) that Bowlby’s employment of these ideas from early cognitive science could hardly have been purely coincidental and not influenced by his early exposure. However, this was also some 30 years prior to his first major theoretical paper (Bowlby, 1958a) and nearly 45 years from his first full discussion of cognitive models or IWMs. Bowlby would have been aware of similar terminology applied by Piaget and takes up his key ideas in an indirect fashion from Craik. Indeed, as mentioned previously, Bowlby had cited a passage from philosophical text by Langer. On the page adjacent Langer also made reference to Bartlett’s work on memory formation and schemas (1967, pp. 4,5). Bowlby must have been aware of Bartlett but simply did not go as deeply into the cognitive sciences as ethology.

183 Van Dijken describes Bowlby’s time at Priory Gate School in Norfolk as an example of the ‘new education’ prominent in the early 20th Century European naturalist education movement noted for employing terms such as ‘freedom’, ‘self-expression’, and ‘activity’, all “united in their opposition to the old way of education” (1998, p. 48).

184 Van Dijken describes this ‘new psychology’ movement as including behaviourism, gestalt and psychoanalysis (1998, pp. 54-56).

185 Van Dijken suggests less is known about Bowlby’s experiences as a PhD student under the supervision of Cyril Burt, but the studies were discontinued at some point (1998, p. 73).

186 Bowlby’s relationship with Robertson was productive but also encompassed a degree of conceptual dispute about the nature of observational evidence collected by Robertson in his work at the Tavistock (Vicedo, 2013, pp. loc. 3577, 4142-4156).
undo [sic] respect for his seniors." Post-Freud (i.e. after Sigmund’s death in 1939, not ‘post-Freudian’) psychoanalysis in the United Kingdom from the mid 20th Century is typically noted for at least two trends: (1) its important turn to the study of childhood (see Zaretsky’s chapter on the Post-World War II study of mother-infant relationship2004), especially under the influence of Melanie Klein and Anna Freud (for a brief historical sketch of their respective contributions and schools, see Mitchell & Black, 1995, pp. 25-59 and 85-111), and (2) the rather ferocious internecine theoretical warfare that erupted, especially between the followers of Melanie Klein and Anna Freud in the 1940s and beyond (for a detailed survey the primary sources, see King & Steiner, 1991).187 Bowlby’s role in the first trend is the subject of this thesis. Although still a relative newcomer having only gained full membership in 1939, he played a more conciliatory role in the second. In her introductory presentation of the biographies of ‘main contributors’ to the Freud-Klein Controversies 1941-1946, King includes Bowlby identifying him as someone who gradually formed a more independent position and quoting his personal communication that indicated he had become “alienated by what he saw as intolerance of the Kleinians” (King & Steiner, 1991, p. ix). Prior to this in his formal training period, Bowlby appears to have fallen foul of his training supervisor. Bowlby had conducted child clinical work in his medical/psychiatric training in a fashion—not five times per week and on occasion with explicit engagement of children’s parents—not accepted by the society for his parallel psychoanalytic training. Indeed, his first application for membership was initially knocked back (see discussion of his early training in van Dijken, 1998, pp. 94-102). But it was probably Bowlby’s insistence on environmental risk in infant development that would from the outset bring him into contention with his psychoanalytic colleagues.

5.2.3 Bowlby’s Interest in Childhood Environmental Risk

Bowlby’s differences resided primarily with his closer colleagues, i.e. Klein and her supporters, for whom intrapsychic fantasy served as the primary realm for psychoanalytic study—though with a diminished or lessened role for the infant’s real experience (Mitchell & Black, 1995, p. 94). In fact Bowlby’s ideas about environmental risk were in ways more aligned with those expressed by Anna Freud and Dorothy Burlingham whose work during the war is widely quoted (Burlingham & Freud, 1942, 1944). Bowlby’s broad clinical experience and emerging thoughts about risk associated with separation would seem to have directed him away from the narrower Kleinian position (Bowlby, Figlio, & Young, 1986, pp. 37-38).

Bowlby had regularly written up his early clinical experience along with his emerging thoughts on childhood risk and separation. He had composed several pieces in this period (see discussion of the early work in van Dijken, 1998, pp. 90-101) but his most notable was the detailed empirical study 44 Juvenile Thieves (Bowlby, 1944). This early paper derived from work in the late 1930s contained the first details of his theoretical leanings and demonstrated a commitment to the empirical study of aetiological risk. Bowlby would later comment that by the time of publication, “I’d already committed myself to studying the effects of separation” (Bowlby et al., 1986, p. 39). Rutter has noted the

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187 Zaretsky provides a good historical discussion of this period’s conflicts in which she cites Young-Bruehl’s (2008) comparison of Anna Freud’s followers to a “hierarchical convent” and Klein’s to a “charismatic cult” (2004, p. 262).
importance of the work on its 50\textsuperscript{th} Anniversary (Rutter, 1995, p. 549). The paper has also been more recently re-engaged (Dixon, 2003; Follan & Minnis, 2010). Bowlby’s ideas were radical in the sense that they would ultimately lead him to reject a psychoanalysis based on Freudian theories in favour of a psychoanalysis based on “the phenomena to which Freud called attention” (Bowlby et al., 1986, p. 45). We can now turn to the second period for a look at Bowlby’s formal theoretical work and his arrival on the global developmental stage.

5.3 ‘Maternal Deprivation’ and Subsequent Ethological Theory Development (ca. 1951–1969)

This second period of theory development, roughly 1951-1969, might be best described as the phase of conceptual emergence. I begin with Bowlby’s entrance into the field of development via his proposals for a possible casual role for ‘maternal deprivation’ in psychopathology in the early half of the 1950s (4.3.1). I then acknowledge his continued interdisciplinary collaboration—especially with the Canadian developmental psychologist Ainsworth (4.3.2). In a last step I chart the early, protracted emergence of his more ethologically informed Attachment Theory beginning in the late 1950s and culminating in 1969 with the publication of his comprehensive theoretical articulation Attachment, the first volume of his trilogy Attachment and Loss (4.3.3).

5.3.1 Bowlby’s 1951 WHO Report on Maternal Deprivation

Bowlby entered the world’s developmental stage with the publication of his Maternal care and mental health (1951),\textsuperscript{188} a work sponsored by the World Health Organisation (WHO) that surveyed research and interventions with children at risk across six post-war Western countries.\textsuperscript{189} In this work Bowlby would reconceptualise his own early emerging views (see above) in terms of the psychoanalytic developmental notion, maternal deprivation. This notion was first introduced by Spitz and colleague in a discussion of possible causal precursors of childhood psychiatric risk (Spitz & Wolf, 1946). In short, maternal deprivation was seen by Bowlby as a causal contributor to poor childhood mental health:

What is believed to be essential for mental health is that an infant should experience a warm, intimate, and continuous relationship with his mother (or permanent mother-substitute—one person who steadily ‘mothers’ him) in which both find satisfaction and enjoyment. A state of affairs in which a child does not have this relationship is termed ‘maternal deprivation’” (Bowlby & Ainsworth, 1965, pp. 13-14).

Rutter (1972, p. 120) has credited Bowlby with importantly compiling the first ever systematic review of maternal deprivation. However, despite this value, the study proved controversial and received quite mixed responses.

\textsuperscript{188} These ideas were also published in two popularised editions as Child care and the growth of love (Bowlby, 1953a; Bowlby & Ainsworth, 1965).

\textsuperscript{189} The countries are France, the Netherlands, Sweden, Switzerland, the United Kingdom, and the United States of America (Bowlby, 1952, p. 6). The work would be republished two times over the following 12 months followed by a slightly corrected second edition in 1952. I will quote from this second edition (Bowlby, 1952). The work provides a survey of both research and discussions with child care workers from six Western countries. The report’s “Part II: Prevention of Maternal Deprivation” would prove most influential
Virtually all parties to attachment discussions have agreed that the maternal deprivation approach was highly controversial—attachment researchers (e.g., Bowlby, 1969/1982, p. xxii), historians (e.g., Karen, 1998) and critics (e.g., Rutter, 1972)—albeit for slightly differing and at times overlapping reasons. The public controversy raised by questioning possible risks to infants through separation from caregivers would rage for decades to come—often generated via misstatements by supporters and foes alike (Karen, 1998, p. 65). More specifically the possible implications for working mothers and day-care has been a point of serious dispute (Karen, 1998, pp. 313-344; for a recent restatement of these concerns see Vicedo, 2013). Early research on the impact on attachment of time spent away from maternal caregiver was actually mixed, but as some early studies suggested higher levels of risk, the controversy would intensify in the 1970s and 1980s. However, more insightful consideration of evidence has also elevated the discussion of day-care, suggesting more clearly when and how it might be a higher risk and where it may be quite suitable (Howes & Spieker, 2008). Careful attention is also needed to distinguish between the more general proposed institutional reforms and, alternatively, the more nuanced academic disputations regarding causal roots of developmental psychopathology. Bowlby’s strong indictment of various forms of institutional child care would eventuate in wide ranging changes to public child welfare policies (Karen, 1998, p. 65; Rutter, 1972, p. 120). Rutter has also observed from a vantage point three decades later, that with mounting human and animal evidence, Bowlby’s general position taken via maternal deprivation “would be amply demonstrated [namely] that early experience may have serious and lasting effects on development” (1972, p. 123).

However, the maternal deprivation approach also suffered from serious methodological shortcomings highlighted in early academic discussions (e.g. O’Connor, 1956). Bowlby’s approach would prove too simplistic, suffering from an overly generalised depiction of the concept of deprivation—depicted in terms of disruptions in family life, separation from primary caregiver and especially institutional care that may be wholly lacking in any personalised care—and relatively exaggerated causal claims to a range of pathological syndromes (for a detailed discussion see Rutter, 1972). Indeed his earliest ideas relied upon fairly simplistic assumptions about drives and critical periods shared by both ethology and psychoanalysis in the early 1950s (Bowlby, 1953b). Serious conceptual work was clearly required, something Bowlby also readily acknowledged: “In the field of mental health and its relation to parental care, investigators have so far done no more than perceive gross relationships. It is for workers in the coming half-century to refine perceptions, to elucidate complexities, and to give the power to prevent mental illness” (1952, p. 63). Despite these theoretical shortcomings, Bowlby had engaged a collaborative process with researchers from around the world that would prove valuable.

5.3.2 The Emergence of an Innovative Interdisciplinary Process

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190 Indeed, Rutter begins his important critique of the field suggesting, “No area of controversy in psychology has given rise to such widely differing assertions as the topic of ‘maternal deprivation”’ (1972, p. 13).

191 A note about terminology may be helpful here. In her article, “A taxonomy of interdisciplinarity” in the Oxford Handbook of Interdisciplinarity, J.T. Klein acknowledges the presence of a core vocabulary for an interdisciplinarity typology consisting of multidisciplinarity, interdisciplinarity and transdisciplinarity, adopted from a 1972 OECD taxonomy (Klein, 2010). In an earlier
From this rather raucous theoretical beginning would also emerge a distinctly collaborative interdisciplinary process that resulted in a reconceptualisation of Bowlby’s ideas on early development. His most notable partnership was with the Canadian developmental psychologist Mary Ainsworth (Isaacson, 2007), but a few others deserve a first brief mention.

First, Bowlby had maintained his involvement with the WHO throughout the 1950s, participating in four gatherings of the WHO’s Study Group on the Psychobiological Development of the Child (Tanner & Inhelder, 1971) conducted between 1953-1956. Van der Horst indicates that through these sessions Bowlby would gain exposure to some leading researchers in fields far beyond psychoanalysis—including Erik Erikson, Julian Huxley, Konrad Lorenz, Margaret Mead and Jean Piaget. He would continue this collaborative approach in the weekly interdisciplinary discussions at the Tavistock, where notable figures including Robert Hinde and Ronald Lange would regularly attend. And finally similar collaborations would underpin the important biennial CIBA Symposia on infant-mother interactions (van der Horst, 2011, p. 158). These last events resulted in the publication of four edited volumes entitled Determinants of Infant Behaviour: I-IV (Foss, 1961, 1963, 1965, 1969).

Second, it was in the field of animal behaviour that Bowlby would strike up the most important collaborations. Of most impact would have been his engagement with leading figures in the fields of ethology in Europe—Lorenz, Tinbergen and Hinde—and comparative psychology in America—Harlow—, the field from which would emerged his ideas on the applicability their respective methodologies to the study of child development (Bowlby, 1957; for a comprehensive historical discussion of Bowlby’s engagement with the field of ethology see van der Horst, 2011). See Figure 5.1 below for van der Horst’s sociogram’s depiction of Bowlby’s contacts and communications.

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132 Controversy would follow Bowlby for most of his career. His challenge of psychoanalytic dogma would eventuate in hostile interactions with colleagues within psychoanalysis—especially those aligned with Anna Freud and Melanie Klein—and would remain acrimonious for decades resulting in his withdrawing from active participation in the UK psychoanalytic societies (Karen, 1998, pp. 101-118).

133 As has been well documented by van der Horst (2011). Bowlby would take up ideas from leading ethologists, garnered both from reading their works and via ongoing personal conversations: first Tinbergen (1951) in 1951; shortly after Lorenz (1937) in 1953; Hinde (1956) in 1954 and lastly the bi-directional influence with Harlow (1958) in 1958. Van der Horst has produced a “sociogram” (2011, p. 127) depicting network of multiple interactions—both direct and indirect—between Bowlby and leading ethologists by first date of contact. He also includes Julian Huxley in 1951 and Stephen Suomi in 1971, as well indirect connections via others in this list with William Thorpe and David Lack.
As mentioned of particular notice is the long-term collaboration between Bowlby and Ainsworth. In 1950 Bowlby had already joined forces with Ainsworth—a Toronto psychologist with a decade of research interests guided in part by her PhD work with William Blatz's (1895-1964) security theory (Blatz, 1967). Together Bowlby and Ainsworth would lay the foundations for Attachment Theory via their numerous publications over the next two decades (for a relatively comprehensive survey of the Bowlby-Ainsworth partnership see dissertation by Isaacson, 2007).

5.3.4 From Maternal Deprivation to an Ethological Theory of Attachment

Bowlby (1969/1982, pp. xi-xvi) described his part in the eventual conceptual articulation of Attachment Theory as proceeding in two stages after the WHO report. First from his collaborative work between 1952 and 1954 at the Tavistock with the social worker Robertson came the methodological decision to focus research on the impact of separation as the major contributor to maternal deprivation. They described the proposed pathological process in what would become the familiar phrase, 'protest–despair–detachment' (Robertson & Bowlby, 1952). Thereafter in a second stage there emerged a series of critical papers by Bowlby looking at three theoretical issues—affective bonds (Bowlby, 1958a), separation anxiety (Bowlby, 1960b, 1961c), and mourning (Bowlby, 1960a, 1961b, 1963).

194 Bowlby and Ainsworth did not work alone. First, after the Second World War, Bowlby took up a position practicing child and family psychiatry at London’s Tavistock Clinic in London. Upon receiving funding for research into the effects of childhood separation in 1948, he would engage the services of James Robertson, a social worker. Their early collaboration was also noteworthy (Alsop-Shields & Mohay, 2001), as were the Robertson’s documentaries—produced in part with his wife Joyce (Barrett, 2006, pp. 26-27). Second, although Ainsworth began her initial work on infant security working alone, others in the field of developmental psychology such as H. Rudolf Schafer and Peggy Emerson (1964) were also engaged in complementary study. Ainsworth most recognised empirical contribution, the innovative Strange Situation Protocol, was with the result of collaboration with student researchers (M. D. S. Ainsworth, Blehar, Waters, & Wall, 1978). Ainsworth would eventually mentor a great number of attachment scholars (Isaacson, 2007).
These would eventually underpin his three major conceptual volumes on Attachment (1969), Separation (1973) and Loss (1980). With this work, Bowlby had provided a remarkable “translation of the diffuse concept of ‘love’ into a more precise formulation of attachment”, an accomplishment Rutter and colleagues would describe as the first of five landmarks in attachment research (Rutter et al., 2009, p. 529).

Ainsworth, the other half of the Bowlby-Ainsworth partnership, had left London in late 1953 for Africa and would eventually settle in Baltimore in 1956 where she found employment at Johns Hopkins. She would work there until 1975 when she moved to the University of Virginia (see Isaacson, 2007 for biographical details). Her contributions to the Attachment Theory partnership during this period can be seen as at least threefold. First, she supplemented Bowlby’s conceptual work providing valuable clarifications of extant literature for the key notions of separation (1962) and attachment (e.g., M. D. S. Ainsworth, 1969) whilst also introducing insights from her own previous work on the normative concept of security (M. D. S. Ainsworth & Ainsworth, 1958). Second, she produced evidence from two detailed naturalistic observations for identifying and understanding behaviours underpinning attachments. The first research was conducted in Africa with the Ganda (M. D. S. Ainsworth, 1967); a later paper combined the Africa work with later work conducted in Baltimore (M. D. S. Ainsworth, 1964). Third, her work with these Baltimore children was expanded to include the development and trial of an important innovative behavioural experiment, the Strange Situation Protocol (SSP) that sought to measure behavioural differences in security and thereby confirm and complement the naturalistic home observations (M. D. S. Ainsworth & Bell, 1970; M. D. S. Ainsworth & Wittig, 1969). The experiment was first trialled in 1964 and included follow up longitudinal studies; the full impact of this work would not be felt until the 1970s especially after the full publication of the initial SSP studies (M. D. S. Ainsworth et al., 1978). Rutter and colleagues have identified the development of this experimental innovation as second of the five landmark achievements of Attachment Theory (Rutter, Kim-Cohen, & Maughan, 2006).

My transition point for the end of this first period and the commencement of the following period is the publication of Attachment and Loss (Bowlby, 1969) which contained the culmination of two decades of conceptual research with an ethological approach for understanding attachment phenomena. With Ainsworth’s experimental work gaining steam and Bowlby’s more comprehensive theoretical position now in place, Attachment Theory was ready to move into a more mainstream academic position.

5.4 Attachment Theory Maturation Period (1969-1990)

The next two decades of attachment study would see the emergence of a flourishing field of infant attachment study. Three specific features of this period merit mention. First is the ongoing development of the theory and supportive research especially as made by Bowlby and Ainsworth (5.4.1). Second sees the emergence of serious peer reviewed discussions within the larger scientific community (5.4.2). And third, the discovery of a potential maladaptive form of early disorganised attachment by Main and colleagues (5.4.3). Each of these three points towards the maturation of the infant development component of Attachment Theory.
5.4.1 The Emergence of a Mature Theory

During this period as indicated above, Bowlby would supplement his initial 1969 volume on attachment. He would publish his second (1973) and third (1980) volumes on attachment as well as republish a second edition of his initial volume (1969/1982). He would also continue both to lecture widely and to importantly update his theoretical positions (1988). Bowlby’s pre-1969 conceptual approach would give way to more nuanced positions in the trilogy. But as I will argue, aspects of Bowlby’s more mature theoretical point of view may have been stuck in two worlds, issues eventually resolved in his last theoretical publications. As we have seen, the early formulation of maternal deprivation was crude, lacking precision. The study (1951) and its two popular editions (1953a; 1965) were couched in familiar terms—notions that were at the same time endorsed by both Lorenz and classical Freudians—that would prove particularly unhelpful as the terms are often under-clarified and easily over applied. Included are the notions of critical periods, imprinting, instincts and hydraulic metaphors for innate drives. Bowlby’s more developed ethological notions as presented in his Attachment trilogy were truly ground-breaking, including important conceptual advances not fully appreciated to this day (see previous discussion in Chapter Three on his TLC strategy.) However, he clearly missed the mark with his efforts to retain and reform familiar but problematic terminology most likely to win over his past colleagues—for example, rejecting instinct but retaining instinctive behaviour—rejecting critical periods but retaining less clear sensitive ones. Not only did he fail to gain over his psychoanalytic associates for whom this terminology had remained valuable, he planted seeds for confusion that have lasted to this day. Although the details had changed significantly from his earlier work, the terminology was retained and this new slant could go less noticed and therefore be easily misunderstood.195 Indeed, Licklitter miscasts Bowlby’s theory in this very typical fashion as one based on a Lorenzian approach to imprinting (Licklitter, 2008, pp. 398-400). However, others interested in Bowlby’s ideas had noted the shifts in his more mature thinking from earlier ideas (Oyama, 1979, p. 98). Fortunately, Bowlby had the good sense to eventually relent. He gave up his ill-conceived terminological reform in his last works—no mention of imprinting, no reliance on instinctive behaviour as somehow conceptually distinct form instincts, little reliance on critical or sensitive periods (Bowlby, 1988).196 And by embracing Sroufe and Waters ‘organisational construct’ approach (1977) from the emerging field of developmental psychopathology he also left us with a much more dynamic theory in the end (Bowlby, 1988).

Although predominantly theoretical in focus, Bowlby would also continue a habit begun already in the 1950s: the careful integration of Ainsworth’s empirical work within his own (see especially his additions to the second edition of his Volume One 1969/1982, pp. 361-377; and later discussions in 1988). From the 1970s the Ainsworth team’s experimental findings on infant-caregiver security (and insecurity) as measured by the SSP would gain notable prominence. As mentioned above, the

195 Bowlby would appear to be seeking to accommodate both sides of an argument about instincts in 1950-1960s ethology—i.e. the early positions of Lorenz versus the later (post-Lehman) Hinde. Vicedo (2013) documents well Bowlby’s efforts at sustaining his support from Lorenz. Hinde has acknowledged providing input, tutoring and criticism of Bowlby’s work. He also noted Bowlby’s belaboured efforts to retain a discussion linked to Freudian terminology (van der Horst, van der Veer, & van Ijzendoorn, 2007).

196 It is worth noting, that imprinting, critical periods and hydraulic notions of drive are empirical notions valuable in many contexts, but not in the attachment discussion.
breadth of this work would appear as single volume (M. D. S. Ainsworth et al., 1978). With the all the cards now on the table—i.e. a relatively mature theory and emerging empirical data for support—the stage had been set for a more thorough academic review.

5.4.2 The Contribution of Peer Reviews to Theory Maturity

Attachment Theory evolved in part due to important scientific peer reviews in the period up to 1990. These would come in the form of both conceptual critique and challenges to data findings. Three specific historical episodes and the important responses by attachment researchers draw a picture of theory progress driven by give-and-take of critical disputation. The first two events are peer-reviewed articles devoted to Attachment Theory published in the then new but distinguished journal *Behavioral and Brain Science (BBS)* in 1978 and in 1984. The third is Rutter’s important decade (1972-1982) of criticisms of the treatment of maternal deprivation and his eventual acceptance (Rutter, 1981) of an empirically driven conceptual expansion of Attachment Theory as an *organisational construct*, first proposed by Sroufe & Waters (1977).

In the first *BBS* discussion, Rajecki, Lamb & Obmascher (1978) weighed up five197 competing cross species198 theoretical accounts of infant attachment phenomena in addition to the general social attraction—namely, maltreatment effects, secure base effects and separation effects. The first two authors also provided summary responses (1978) to a fairly distinguished cast of 27 reviewers,199 one of whom was Ainsworth (1978). In the following year, ten more comments would be reviewed including one by Bowlby (1979) and another by Main (1979). None of the five considered theories were deemed adequate to explain all of the phenomena but Attachment Theory was deemed superior to learning accounts. But the theory was thought to struggle in explaining anomalous attachments to non-adult objects and the phenomena of maltreatment. Nonetheless, Attachment Theory had passed an early test as a reasonable empirical theoretical option in a peer reviewed programme.

Whereas the first *BBS* article looked at Attachment theory amongst alternatives, six years later, a second article (Lamb, Thompson, Gardiner, Charnov, & Estes, 1984) focused more specifically on the employment by attachment researchers of the foundational experimental Strange Situation Protocol (SSP) devised by Ainsworth and colleagues (M. D. S. Ainsworth et al., 1978). Lamb and colleagues conclusions were much more hard hitting this time around:200

^Thus these studies provide little evidence concerning the specific dimensions of *maternal*

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197 The authors identify eleven possible accounts of attachment and four less relevant historical accounts (including Freud’s notion of cathexis). After discussing an initial six ‘salient’ accounts that did not garner significant current use (including Anna Freud’s account of ‘anaclitic’ approach), they focused on five: Bowlby-Ainsworth approach, Kaufman’s classical conditioning, Gewirtz instrumental/operant learning theory, Carins’ contiguity analysis, and Hoffman-Solomon’s opponent process model (1978, pp. 417-422).

198 Available data dictated four species for cross-comparison: “precocial birds, dogs, monkeys and humans” (1978, p. 423).

199 Of note are the developmental psychologist Robert Cairns, the developmental biologist Gilbert Gottlieb, the stress researcher Megan Gunnar, the ethologist Klaus Immelmann, the psychiatrist Michael Rutter and the primatologist Steven Suomi. See *BBS* 1(3) 436-461 for the full cast of peer commentators.

200 My interests here are again conceptual and empirical. The context of this second article differs greatly from that of the first. Lamb was a former student of Ainsworth. Many in the attachment field viewed the critical approach taken by Lamb and colleagues as harsh and would therefore not cooperate. However, not all attachment researchers chose to not engage, e.g. Cicchetti as well Grossmann & Grossmann. For a discussion of the reaction by attachment researchers, see Karen (1998, pp. 264-266).
behaviour that are of formative importance . . . Strong claims regarding the antecedents, interpretation, temporal stability, consistency, and predictive validity of Strange Situation behavior are only partly supported by the empirical and theoretical literature" (1984, pp. 127, 145).

Although the proposals for a radical overhaul of the SSP would never materialise, responses to ‘strong claims’ would eventually come in two forms: greater empirical rigour around measurement (see overview in Solomon & George, 2008) (see also discussion below on a new independent experiment) and reconceptualization of key tenets—continuity and predictive validity—that engaged greater theoretical complexity along the organisational and developmental pathways lines previously argued (L. Alan Sroufe & Waters, 1977). 201 Attachment Theory was now in a better position to benefit from critical peer input.

As acknowledged previously, Rutter’s first edition of Maternal Deprivation (1972) was a serious broadside to the more simplistic articulations of Attachment Theory within the context of Bowlby’s WHO report. Rutter has been an early champion of the need to embrace greater complexity in addressing developmental risk and the clarity of his analysis of attachment was no different. He noted the need to differentiate between the disruption of a bond and failure to develop a bond; possible distinction between disruption and non-harmful separation; issues with the employment of the term monotropy; and the inadequacies of the use of such a general term as maternal deprivation (1972, pp. 124-125). By the time of his second edition (1981) much had indeed changed. The capacity of the theory to change in response to both conceptual and empirical critique is probably what permitted Rutter to embrace the tradition. Indeed, he noted four areas of change from the earlier articulations: abandonment of analogies with animal imprinting, softening an all or nothing approach to early sensitive periods—parenting impact falls only in first two years—, relinquishment of the notion of monotropy—i.e. disposition to attach to a single caregiver—, and a recognition of the impact both early and later (1995). Rutter’s views were eventually accepting of attachment but he has retained his capacity to seek clarity where issues have been confused or remained unresolved (see for example Rutter et al., 2009).

A last word about Rutter may focus on his collaboration with the attachment researcher L. Alan Sroufe in the field of developmental psychopathology (Rutter & Sroufe, 2000; L. Alan Sroufe & Rutter, 1984) noted in Chapter Four. This is a field that has embraced Attachment Theory in terms of both empirical study—especially contributions complex longitudinal study (L. Alan Sroufe, Coffino, & Carlson, 2010) and conceptual development (L. Alan Sroufe & Waters, 1977). Sroufe and Waters rearticulation of Attachment Theory as an organisational construct—i.e. both transactional and cumulative phenomenon laid the ground work for redressing the more simplistic articulations of Attachment Theory. Indeed, not only Rutter, but Bowlby would come to endorse this approach (1988).

5.4.3 Discovery of a more Pathological Form of Attachment and the Revision of SSP

201 Indeed, in response to ongoing research, Lamb’s conclusions would also appeared to have softened where he would appear to now embrace less ‘strong’ claims for the SSP and attachment parent-infant interactions (Lamb & Lewis, 2010).
In the 1980s Main and colleagues noticed what others had also seen: some infant behaviours in the SSP seemed difficult to code and difficult to rate categorically (Crittenden, 1985; Main & Solomon, 1986) (Lyons-Ruth, Connell, Zoll, & Stahl, 1987). The crucial observation here was the disruptive role of frightening behaviour in the context of care-giver interactions. Main and colleagues would eventually reformat the SSP to include a fourth category of attachment: ‘disorganised’ attachment (Main & Solomon, 1990). Indeed, some have observed (Rutter et al., 2009) that unlike previous attachment approaches that focussed on maternal sensitivity as key for understanding the emergence of attachment, disorganisation was a state of dysregulation and might be better considered as orthogonal to traditional notions of security-insecurity. This discovery provided what Bowlby and Ainsworth had not been able to locate: a true developmental risk if not a disorder in itself (Lyons-Ruth & Jacobvitz, 2008). I will return to the implications in Chapter Eight.

What should be clear is that by the time of Bowlby’s death in 1990, Attachment Theory possessed a fairly robust empirical platform from which to study infant attachment. But the more ambitious agenda of multi-level lifespan approach was only beginning.

5.5 Post-Bowlby Exponential Multidimensional Expansion (1991-Present)

My final historical period highlights the relatively staggering fertility of research associated with Attachment Theory. I will sketch five salient dimensions of growth: lifespan stages, levels of analysis, domains of study, scientific disciplines, and clinical and policy application. Mention should also be made that one of the more important developments: Main’s innovative ‘move to the level of representation’ (Main, Kaplan, & Cassidy, 1985), which slightly predates the 1991 time frame, counts as both a lifespan and level of analysis contribution.

5.5.1 Research across the Lifespan

Bowlby had always positioned attachments as phenomena that spanned the lifespan (Bowlby, 1969/1982, pp. 141-144), but he said very little beyond his major area of focus, early childhood. From the mid to late 1980s research had begun to engage the study of attachment phenomena at other stages of the lifespan, especially in work with adults. In fact, research has expanded to include a focus on all specific life-stages. See summaries for current positions beyond infancy for toddlerhood/pre-school (Marvin & Britner, 2008), middle childhood (see Kerns & Richardson, 2005), adolescence (Allen, 2008), adulthood (Mikulincer & Shaver, 2007) and late adulthood (Cicirelli, 2010). Indeed, parallel ventures emerged for the study of adult attachment: (1) Main and colleagues (Main et al., 1985) focus on attachment states of mind in potential adult care-givers in the developmental tradition, and (2) a range of contributions in the field of social psychology looking at specific romantic relationships (e.g. Hazan & Shaver, 1987). Several important prospective longitudinal studies, some beginning as early as the 1970s, have also emerged, permitting a closer look at questions of continuity and change (see survey in Klaus E. Grossmann, Grossmann, & Waters, 2005). However, as Thompson and Raikes have astutely acknowledged, “the challenge faced by attachment researchers is like the challenges of establishing the heterotypic continuity of personality constructs”
(2003, p. 704). “There is ... uncertainty regarding what is involved in the concept of security when moving to older age groups beyond infancy” (Rutter et al., 2009, p. 534). Issues of adequate measurement across different stages in the lifespan for the various age specific measures remain an evolving dilemma (Crowell, Fraley, & Shaver, 2008; Solomon & George, 2008). More important for this thesis will be the treatment of Bowlby’s Phase Four Goal-Corrected Partnership, i.e. initially proposed as post-infancy development from two to five years of age (Bowlby, 1969/1982). In the first two editions of their chapter in the Attachment Handbook Marvin & Brittner suggest that the “goal correct partnership needs much research. Very little is known except for the general construct, its early development, some implications regarding its relations with self-concept and academic performance during middle childhood and its importance for the relationship between adolescents and parents” (Marvin & Britner, 1999, pp. 62-63; 2008, pp. 288-289). I will hold off on further discussion in Chapter Seven

5.5.2 Research across Multiple Levels of Analysis

Attachment research began with the study of behaviour but always proposed that internal mechanisms were also at play. Today at least five levels of empirical analysis can be identified within the expanding attachment research throughout this last period: behavioural, psychological, endocrine, neurological, and genetic/epigenetic. Bowlby had made the Internal Working Models (IWMs)—i.e. mechanisms functioning at the psychological level—a central platform of his theoretical approach (Bretherton, 1999; Bretherton & Munholland, 2008). When Main and colleagues (Main et al., 1985) sought to understand adult states of mind relative to attachment they articulated a ‘move to the level of representation’. This move also counted as the first effort to both conceptualise and assess IWMs with adults. In doing so they moved attachment research beyond its familiar historical starting point at the level of behaviour. Their development of a complementary analogue measure—the AAI—to roughly parallel the SSP has provided an invaluable tool for both theory/research (Hesse, 2016) and more recently for clinical use (Steele, Steele, Sroufe, & Jacobvitz, 2008). Unlike the self-report measures that assess current relationship content, the AAI assesses ‘states of mind’ relative to attachment by weighing both coherence of an attachment narrative and accompanying collaboration with interviewer. The measure was derived from an innovative application of Grice’s (1975) philosophical observations on properties of conversation. Indeed, in Chapter Four’s discussion of an attachment capacity construct, I have alluded to the value of the AAI conceptualisation of affect

202 Crittenden makes similar observations, “It is ironic that, although the great majority of attachment research has been carried out by developmental psychologists, the theory as practiced is substantially less developmental than most other developmental theories. That is attachment researchers have paid more attention to the validation and temporal extensions of the infant patterns of individual differences than to the interactive effects of maturation and experience on organisation of attachment beyond infancy” (Crittenden, 2000, p. 6).

203 Note should also be made that although early studies might have focussed on one level for investigation, more recent employment of the multi-level term neurobiology has also become common (Hofer & Sullivan, 2008). Often implicit in this approach is an effort to combine neurological, psychobiological (HPA/stress), genetic and epigenetic levels in the study of a particular phenomena. For example see Hostinar and colleagues study (Hostinar, Sullivan, & Gunnar, 2014) of the mechanisms underpinning social buffering and HPA activity.

204 For a discussion and comparison of the AAI and self-report traditions, see Roisman (2009), an experienced AAI and attachment developmental researcher. For a recent and conceptually relevant discussion of the AAI, its psychometric properties, and its role in addressing key questions in attachment study—including continuity/change and antecedents—, see collected articles in the 2014 Issue 3 of the Monographs of the Society for Research in Child Development (see Introduction by Booth-LaForce & Roisman, 2014).
regulation manifest in narrative articulation. Hence, the AAI can be seen as a first and foundational effort to connect the behaviour level with both mental mechanisms and its manifestation in language, whose value has endured.

Bowlby also postulated attendant biological mechanisms (Bowlby, 1969/1982, p. 87). Studies focussed on stress regulation and attachment have been the first to take this more biological turn, driven in part by animal studies (Hofer, 1987). Work in this field has extended to both Hypothalamus-Pituitary-Axis (HPA) activity and autonomic nervous system functioning (see survey in Diamond & Fagundes, 2010). Ambitious studies of proposed neurological activity underpinning attachment are multiplying rapidly generating relatively high hopes for progress (Swain, Lorberbaum, Kose, & Strathearn, 2007). Investigations range from the study of early neurological development and risk (Tottenham, 2012) to adult neurological functioning (Coan, 2010). Genetic and epigenetic studies are also growing in number. Attachment studies focussed on difference in attachment styles are regularly conducted at the level of allelic variations across a range of candidate genes (for example the role of oxytocin in an attachment longitudinal study, Raby, Cicchetti, Carlson, Egeland, & Andrew Collins, 2013), albeit with sometimes mixed results (Roisman, Booth-Laforce, Belsky, Burt, & Groh, 2013). The study of more dynamically considered genetically based susceptibility to environmental influence has also shown promise for understanding attachment differences (Belsky & Pluess, 2011). Meanwhile epigenetic studies of the impact of maternal care have also emerged lending possible evidential support for issues regarding intergenerational and transgenerational transmission of attachment styles (Champagne, 2008; Champagne & Curley, 2010). Not surprisingly, calls for multilevel complex approaches to the study of attachment phenomena are emerging (Meyer, Wood, & Stanley, 2013).

5.5.3 Research across multiple domains and multiple disciplines

Attachment Study began as an approach to the study early emotional development (L.A. Sroufe, 1997), but over time has expanded into domains of cognition, social cognition and ecology. This expansion has been driven by at least two factors, one internal to the theory and the other more external. The first consideration has been the requirement to account for differential functioning of attachment across the lifespan beyond early implicit pre-linguistic affective phenomena. The second is the increasing convergence of previously independent developmental disciplines into more complex unified approaches to human development.

In terms of the first factor, Attachment Theory has branched out beyond developmental psychology and is now studied in a range of allied disciplines including but by no means limited to affective science (Panksepp, 1998), cognitive development (S.C. Johnson et al., 2010), cognitive developmental neuroscience (M. H. Johnson & de Haan, 2011), behavioural biology (Suomi, 2008), developmental psychobiology (Hofer, 1987), neurobiology and social neuroscience (Cacioppo, Bianchi-Demicheli, Hatfield, & Rapson, 2012), evolutionary developmental psychology (Hernández Blasi & Bjorklund, 2003), social psychology (Shaver & Mikulincer, 2010), infant social development (P. Rochat, 2009), social ecology (Belsky, 2005) and lastly developmental psychopathology (Rutter &
Convergence across the developmental sciences has emerged as a more recent phenomenon, driven in part by the conceptual contributions of Gilbert Gottlieb (Hood, Halpern, Greenberg, & Lerner, 2010). Gottlieb’s notions of probabilistic epigenesis and bidirectional or coactive causality have been particularly potent drivers of this ongoing shift (2010). (See also discussion of the attachment organisational perspective and developmental systems theory in Chapter Six.) Models and practices emerging from the field of developmental psychopathology—a field for which attachment has played an instrumental role (Carlson & Sroufe, 1995)—have also embraced complexity with a degree of sophistication (Cicchetti, 2013). Oyama (1979, p. 100) had also noted the value Sameroff’s (1975) ideas on social transactions as candidate mechanisms for embracing biological complexity for developmental systems. Indeed more recently he (2010) has also provided a high level unified model of development depicted in Figures 5.2 and 5.3 below that have incorporated attachment within a lifespan approach.

![Figure 5.2 Sameroff’s (2010) Biopsychosocial Ecological System Model: Part One](image)
Such lifetime experiences also provide a model from which to extract translational considerations, including intervention in the case of risk, my next point.

5.5.4 Translational and Clinical Expansion

Attachment Theory has also provided significant input to the identification (Rutter et al., 2009) and treatment of psychopathological risk and disorder (P. Holmes & Farnfield, 2014a), particularly childhood trauma and maltreatment (Toth, Gravener-Davis, Guild, & Cicchetti, 2013). Numerous efforts have been made to translate theoretical insights into practical interventions. These now range across the lifecycle and include: the initial infant-parent dyadic work and other interventions with young children (Berlin, Zeanah, & Lieberman, 2008; Oppenheim & Goldsmith, 2007), work with school age children (Bergin & Bergin, 2009; Golding, 2012), work with adolescents (Scharf & Mayseless, 2007), work with adults (Slade, 2008) (Berant & Obegi, 2009), work with older adults and work with couples and families (S. Johnson, 2008). Public sector work has also incorporated attachment insights: at the policy level (Rutter, 2008), in terms of social work (Howell & Sanchez, 2011), within forensic and family law (McIntosh, 2011), in the context of institutional care (Bakermans-Kranenburg et al., 2011), and for adoption and foster care services (Kaniuk, 2014; McLaughlin, Zeanah, Fox, & Nelson, 2012). The extremely wide range of work engaged would also appear to be vary in quality, find differing levels of evidential support and most likely reflect a range of emphases taken from Attachment Theory. However, what we do not have yet in the midst of all of this growth is any sort of acknowledged criteria for inclusion as an attachment informed interventions.
5.5.5 Empirical Advances Drive Wider Acceptance

The cross-lifespan multi-level attachment research endeavour has been remarkable in scope and size. Accompanying this growth has been a much more general reception of the theory by a broader range of scientists. First, a few notable critics would appear to have softened their criticisms: e.g. Lamb (Lamb & Lewis, 2010)(also noted above) and Michael Lewis (Lewis, 1997) whose undergraduate textbook Introduction to Infant Development includes a solid treatment of Attachment Theory (Slater & Lewis, 2007). Two notable critics have not found grounds to endorse Attachment Theory: Jerome Kagan and Judith Harris. Kagan’s original issue with Attachment Theory was the seeming wholesale dismissal of possible genetic personality influences (1995). Although issues of temperament have been embraced within attachment studies (see a review of these complex issues in Vaughn & Shin, 2011), Kagan’s views do not seem to have been significantly altered, even when introducing van der Horst’s (2011) important historical work on Bowlby (Kagan, 2011). Harris’ views (1998) on the minimal impact of early experience are based on what Rutter labels “misleading extrapolation”, and would seem to continue the unsatisfying dichotomous Nature versus Nurture discussions. Rochat and Gopnik are two infant researchers—social and cognitive—whose latest commentaries (Gopnik, 2009; P. Rochat, 2009) make space for attachment discussion where previous works made no mention (Gopnik, Meltzoff, & Kuhl, 1999; Philippe Rochat, 2001).

One event that may have potentially influenced reluctant sceptics might be the apparent independent validation of the basic SSP categories (see Gopnik, 2009, p. 184). In 2008, Solomon and George (2008, p. 392) noted the construct validity of the SSP attachment security construct remained “technically incomplete” as no cross-validation of the two standard insecure categories—avoidance and ambivalence. That may now have changed. S.C. Johnson and colleagues may have filled this gap by developing an innovative cognitive habituation experiment that measured early infant attachment, both security-insecurity and security-avoidance-ambivalence (S.C. Johnson et al., 2010; S. C. Johnson, Dweck, & Chen, 2007). These experiments would appear to have filled a lingering psychometric validation gap. This may serve as the last piece of an acceptance puzzle, permitting a take-up of the theory by those more anchored in the experimental traditions.

5.6 Chapter Conclusions

I bring this rather brief theoretical saga to conclusion re-emphasising two important points and adding one observation not yet made.

First, Attachment Theory has from the outset been an evolving scientific enterprise, one beset with expected inadequacies but also equally responsive to the need for ongoing improvement. Bowlby’s proposals for Attachment Theory evolved quite significantly over his career and enhancements to his thinking remain an ongoing reality today. Although this would hardly seem a radical position, it is not well recognised. Failure to countenance these historical developments contributes much to today’s conceptual confusion.
A second conclusion concerns the relative solid scientific standing of Attachment Theory. The experimental tradition initiated by Ainsworth and colleagues’ innovative approach in the SSP remains viable today. Entry into the academic world from the 1960s would deliver tough reviews, insightful criticisms and provided the contents for further research. That included further validation and expansion of the SSP. From the 1990s the field has moved in numerous directions encompassing the full lifespan and engaging multiple levels of analysis. As of 2015, the theory has amassed extremely wide if not consensus scientific support. However, in very few ways is any specific aspect of the theory complete. Indeed, as argued in Chapter Four, the unresolved questions may be more fascinating than the resolved. Which leads to a last point.

A third point considers the curious lacunae noted in attachment studies: a relatively lack of serious engagement with the role of language in attachment development beyond infancy—i.e. Bowlby’s Phase Four Goal-Corrected Partnership. As already highlighted in Chapter Two’s acknowledgment of the PDR tradition, studies of infancy have uncovered the presence of important sophisticated transactions between pre-linguistic infants and their caregivers. Indeed attachment research has played a foundational role here, identifying a role for implicit affective phenomena within attachment phenomena. However, I would suggest that one of the more interesting but hardly recognised disagreements in the literature evolves around the question of language and explicit interactions—equally affective—in attachment phenomena. Part Three of the thesis will focus on this question in significant detail.

My final remark is an observation. It also serves to reintroduce the four Tinbergen realms in the chapters that follow. Attachment Theory’s current success may also contain the seeds of a different sort of problem. Bowlby and Ainsworth provided nearly 40 years of careful guidance and gatekeeping for the theory. With their deaths, the field has faced the daunting task of assessing and regulating theoretical development of a now sprawling worldwide research endeavour. Attachment research centres are proliferating.205 Editorial practices for key handbooks (Cassidy & Shaver, 1999, 2008) and journals—e.g. Attachment and Development and Attachment—no doubt reflect attachment points of view grounded in the attachment tradition. But as indicated in Chapter Four, important theoretical questions remain unresolved (see discussion in Waters & McIntosh, 2011). Any editorial approach will inevitably be limited where consensus is lacking or questions are not kept in front of mind. However, the inadequacies emerging from a lack of theoretical consensus have also been noted—e.g. by a gathering of over 20 notable developmentalists (Carter et al., 2005). As the numbers of centres and publications grow, questions of theoretical consensus would appear more pressing. The recent 2014 publication of a three volume Routledge Handbook of Attachment may be a case in point. Designed for practitioners, the work’s breadth looks encouraging. I would also argue its approach to the

205 The following are examples of attachment research centres associated with identifiable attachment researchers: E. Watters at the Stony Brook Attachment Lab, SUNY, New York (http://www.psychology.sunysb.edu/attachment/sunylab) [may not be currently active?]; P. Crittenden at the Family Relationships Institute, Miami, FL (http://familyrelationsinstitute.org); M. and H. Steele at The Center for Attachment Research, New School of Social Science Research, NYC (http://www.attachmentresearch.com/); P. Shaver at the Adult Attachment Lab, UC Davis, CA, (http://psychology.ucdavis.edu/research/research-labs/adult-attachment-lab); and R.C. Fraley, combines academic research with on-line behavioural investigation at Department of Psychology, University of Illinois at Urbana-Champaign (https://internal.psychology.illinois.edu/~rcfraley/research.htm). There are also numerous training and treatment centres such as the Bowlby Centre in London (http://thebowlbycentre.org.uk/).
conceptual renderings in its *Theory* volume (P. Holmes & Farnfield, 2014b) is refreshing as it includes two chapters (Farnfield & Stokowy, 2014; Shah & Strathearn, 2014) devoted to the less recognised but insightful lifespan approach of Crittenden and colleagues (Crittenden & Clausen, 2000; Crittenden & Liandini, 2011). But it also presents a version of Attachment Theory quite distinct in ways from those in the second latest *Handbook of Attachment* (Cassidy & Shaver, 2008). (See discussion below from Chapter Six onwards.) Since the days Kuhn initiated the sociological study of science (1962), social questions have been commonplace in the study of science. Indeed, a practical, sociological question may be pondered here, “Who owns Attachment Theory?” I don’t know the answer but I will suggest that regardless of whichever hypotheses one might offer, the requirement for the ongoing increasing conceptual coherence and empirical validation of Attachment Theory remains as critical today as did when Bowlby first hypothesised the causal environmental precursors to juvenile delinquency. Hopefully the chapters that follow might add to that requirement.

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206 I have adapted this phrase from the title of Casement’s anthology, *Who owns psychoanalysis*? (2004). I am more hopeful that Attachment Theory might find adequate responses here than have its psychoanalytic neighbours.
CHAPTER FIVE REFERENCES


Bowlby, J., & Ainsworth, M. D. S. (1965). *Child care and the growth of love. Based ... on the report "Maternal Care and Mental Health"* (M. Fry Ed. 2 ed.).

Bowlby, J., Figlio, K., & Young, R., M. (1986). An interview with John Bowlby on the origins and reception of his work. *Free Associations, 1*(6), 36-64.


CHAPTER SIX: The Attachment Organisation Perspective: DLPC, DST and Implications for Psychiatry

It may not be as widely known that Bowlby also proposed a particular viewpoint on development. This was a non-linear, transactional model, akin to various systems perspectives and to Gottlieb’s concept of probabilistic epigenesis (Sroufe, 2005, p. 350).

6.0 Chapter Introduction

Attachment Theory has found an important home in the broad fields of developmental and social psychology as well as the discipline of developmental psychopathology. It also has much to offer both psychiatry and its critical study—the philosophy of psychiatry. Fundamental here may be the capacity of attachment research to embrace developmental complexity beyond mere abstract discussion. It’s organisational construct and approach might best serve as binding platform across all these fields. With this in mind, Chapter Six will set out another ambitious task: to further detail and expand the organisational perspective identified in Chapter Four. To do so five primary tasks will be conducted:

1. to recast the organisational construct as a dynamic lifespan personality construct (DLPC),
2. to position the discussion of attachment organisational development and the DLPC more formally within contemporary Developmental Systems Theory (DST),
3. to draw out the conceptual consequences for engaging DST,
4. to present an interesting encounter between attachment and the RDoC project in psychiatry,
5. to draw out major implications for attachment, psychiatry and philosophy of psychiatry.

The adoption of the organisational perspective within Attachment Theory described in Chapter Four opened an important door for the study of attachment as a more complex lifespan developmental phenomena. Chapter Five provided a helpful historical perspective from which to identify important but occasionally unrecognised shifts in the theory. This included the introduction of the organisational perspective in the 1970s during the theory’s Maturation Period. Both Chapters functioned as analytic exercises to assist readers to grasp the rapidly expanding world of Attachment Theory. This concluding chapter of Part Two will shift gears providing a more conceptual expansion of what has come before.

Briefly, Section 6.1 will revisit the organisational perspective set out in Section 4.4. Of importance here will be a final review of the psychoanalytically inspired ‘continuity hypothesis’ in light of the organisational perspective. I will argue on conceptual grounds that despite Attachment Theory having been well received within most quarters of current psychoanalysis, Attachment Theory should not be considered a psychoanalytic theory. Section 6.2 provides both a general description of my proposed DLPC and a brief suggestion as to how it might be integrated within personality psychology. Next, Section 6.3 suggests how the organisational perspective might gain greater conceptual traction when considered in terms of...
Developmental Systems Theory (DST). Indeed, the DLPC as I have proposed might also be appropriately described as a developmental system. The following section (6.4) looks at a few relevant considerations from adopting a DST position that might be made more explicit within Attachment Theory. Section 6.5 provides a case study demonstrating the challenges for integrating the complexity inherent in an organisational attachment perspective within psychiatry. This critical look at the inclusion of attachment within the NIMH Research Domain Criteria (RDoC) Project suggests that the theory indeed belongs. However, an ideal fit that adequately allows for attachment complexity may yet to have been found within the RDoC matrix. Finally, Section 6.6 brings focus back to conceptual issues identifying a few critical implications of these organisational discussions for Attachment Theory for both psychiatry and philosophy of psychiatry. Of particular interest here will be the relative fit between attachment’s causal mechanistic approach and Murphy’s call for a shift to causal validity in psychiatry.

6.1 The Organisational Perspective Revisited

6.1.1 Definitional and Semantic Clarification

Section 4.4 identified the organisation construct as one that would appear to combine ontogeny, function and mechanism. Before expanding this discussion, a clarification is in order. Note should be made that the term organisation has been employed in multiple fashions within attachment study. From a functional perspective, note has been made of Main & Solomon’s (1986) insightful proposals for an organisation/disorganisation construct. Rutter and colleagues (2009) had also differentiated this organisation construct from the earlier security construct. The disorganisation measure identified chaotic maladaptive attachment associated with severe attachment risk. Indeed, the disorganised construct has dominated studies of risk for poor attachment related mental health outcomes (Lyons-Ruth & Jacobvitz, 2008) and the notion of ‘reflective’ or ‘cognitive collapse’ will be discussed in Chapter Eight when looking more closely at the GCP. However, despite its important contribution to understanding psychiatric developmental risk, the functional construct discussed in Chapter Four should not be confused with the thirteenth construct identified in Section 4.4 and discussed below.

Equally, the term self-organisation is also employed as a property of an agent/self whether in discussing resilience striving (Cicchetti & Rogosch, 1997) or the development of attachment self-representation within a regulatory reflective function (Fonagy & Target, 1997). Indeed, this last employment would appear to be a further manifestation of the functional term organisation/disorganisation. I am not aware of an attachment use for organisation as employed in self-organisation and emergence in complex systems. For a discussion of these two employments of the term self-organisation see Lewis & Granic (1999) As noted, the organisational perspective is broader and encompasses multiple realms.
6.1.2 Further Elaborations of the Organisational Construct from the Minnesota Longitudinal Study

The conceptual complexity inherent within the organizational approach has also been honed from insightful prospective studies of development. For example, *The Minnesota Study of Risk and Adaptation from Birth to Adulthood* (Sroufe, Egeland, Carlson, & Collins, 2005) has provided a first template for the study of multiplicity in early and ongoing experience of risk within the context of complex child development. Sroufe & colleagues noted:

> Our study has been as much a study of change as a study of continuity. Even when there are clear relationships between early experience and some later outcome, this relationship is probabilistic, not deterministic . . . Understanding developmental processes underlying continuity and change is more important than simply understanding that early experience often predicts later behavior (2010, p. 44).

Indeed, a follow-up discussion (Sroufe et al., 2010) on findings for the influence of early attachments in their study is far from simple.

Four illustrative points gleaned from Sroufe and colleagues (2010) can depict the organisational divergence of early influence across development. (1) In some cases, such as attachment disorganisation, early attachment experience by itself has been shown to strongly influence later behaviour. Indeed, infant attachment as a single factor has proven a better predictor of outcomes than other factors. (2) However, “It must also be noted that linkages between infant attachment and theoretically relevant outcomes, while virtually always significant, in some cases were small. Moreover, and of great importance, these relationships were quite often enhanced when infant attachment assessments were combined with other measures of both early and later experience” (p. 39). (3) Indeed, the processes of developmental change have also proven to be dynamic across time. “Later experiences, in turn, may alter representations, and these altered representations provide a new framework for subsequent experience, in an ongoing cyclical way” (p. 39). Elsewhere, they have also noted in places the presence of a type of ‘double mediation’ model, “wherein secure attachment in infancy predicted early elementary school social competence, which predicted adolescent friendship security, which, finally, predicted daily positive emotional experiences and less negative affect in conflict negotiation in young adult romantic relationships” (p. 46). (4) Lastly, a lack of direct significant link between early attachment experience and adolescent outcomes continues to confound this period of current lifespan thinking, especially the lack of support for a simple continuity approach.\footnote{Sroufe and colleagues (2010) identification of empirical issues across adolescence has been picked up by adolescent attachment researchers (Allen & Tan, 2016; Scharf & Mayseless, 2011). They have also applied the organizational perspective emphasising how early, later and ongoing experience must all be considered when exploring adolescent attachment. Allen and Tan conclude, “The growing complexity of the adolescent social, emotional and cognitive world necessitates a growing complexity of our understanding of attachment system as an organic factor within it” (2016, pp. 399, loc. 15564 )}

In total, what this evidence suggests is simplistic deterministic explanations of lifespan development would appear to be no longer
useful, which brings the discussion to a next point.

6.1.3 The Organisational Approach and the ‘Continuity Hypothesis’

The ‘psychoanalytically inspired continuity hypothesis’ noted in Subsection 4.4.3, remains associated with attachment, especially in discussions of predictive power for stability of earliest attachments across the lifespan. This would seem to be the case for Main (2005) and her followers (Howard Steele & Steele, 2014) as well as amongst engaged critics (M. Lewis, Feiring, & Rosenthal, 2000; M. Lewis & Rudolph, 2014, pp. 3-4) and interested commentators (Lickliter, 2008). Once again, as stated above, the ‘continuity hypothesis’ contends that earliest attachments generally remain stable across the lifespan potentially reflecting an early critical or sensitive period in early development (J. Holmes, 2001). The organisational perspective offers a clear alternative here.

The organisational perspective recognises a more dynamic relationship between developmental continuity and discontinuity, and affirms Bowlby’s insistence on multiple pathways. Thompson (2001) has also argued persuasively why attachment should not be considered a sensitive period phenomenon. Equally the emergence of more robust models for consideration of how early experience impacts lifespan development—such as in adolescence—has further supported key tenets of the organizational approach. In addition, the most recent and largest longitudinal study of continuity or stability of early attachment rejects the simplistic ‘continuity hypothesis’ but nonetheless importantly demonstrates “evidence of weak but statistically significant stability in attachment from infancy through late adolescence, and lawful sources of continuity and change over time—maternal sensitivity, father absence, paternal depression, and negative life events” (Booth-LaForce & Roisman, 2014, p. vii). Indeed, as the organisational perspective has argued, context and ongoing experience do matter.

This rejection of the simplistic ‘continuity hypothesis’ in favour of an organisational perspective may assist in addressing a lingering question: Should Attachment Theory rightfully be considered a psychoanalytic theory, even if the latter seemingly rejects one of the former’s basic tenets? A commitment to empirical method is central for Bowlby. Nonetheless, many have responded affirmatively (Hinde, 2005; Jeremy Holmes, 2010; Main, 1993) whilst others describe a more nuanced overlap (Fonagy, Luyten, Allison, & Campbell, 2016). I will follow Bowlby (1988), Eagle (2014) and Rutter (2006) who all conclude in the negative. As long as psychoanalysis fails to fully ground all of its developmental accounts within a committed empirical framework, it would appear to better to retain a clear conceptual

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209 The study has been published with invited comments in the Monographs of the Society for Research in Child Development. A pair of invited commentators, van IJzendoorn and Bakermans-Kranenber—two of the earliest and perhaps more influential statistical (meta-analytic) pioneers in Attachment Theory—, have noted “It is wonderful to see a fourth generation of attachment researchers emerging with major contributions to the ever-evolving theoretical frame-work of attachment theory. The large longitudinal NICHD SECCYD dataset, the eclectic use of measures at multiple levels of human functioning, and the application of advanced statistical tools make this monograph a truly outstanding achievement” (2014).
distinction between Attachment Theory and its nonetheless valuable applications within psychoanalysis. The concepts of attachment theory may play a psychoanalytic role even if the theory is not itself psychoanalytic in orientation.

6.2. The Organisational Construct as a Dynamic Lifespan Personality Construct (DLPC)

6.2.1 Proposal for a DLPC

I propose that an attachment organisational construct might also be depicted as a dynamic lifespan personality construct (DLPC). Figure 6.1 depicts how attachment ontogenetic constructs—processes and entities—interact and change across the lifespan. The dynamic nature can be understood via several dimensions. First, the proposed construct reflects the transactional approach developed by Sameroff (1975; 2009). Such transactions also involve interpersonal dyadic phenomena. Attachment interactions, patterns, ties and capacities engage one another in what might be called a ‘multiple or reciprocal mediation’ model, to borrow terms employed by Sroufe and colleagues (See 6.1.2). A global regulatory capacity also exists alongside multiple distinct relationships, patterns and transactions, each potentially reflecting similar and different unique styles. Equally, transactions include engagement with cumulative past history—early impacts later—alongside present context. Therefore, additional considerations of non-attachment specific influences, ‘Other distal and proximal factors’, have also been shown to impact attachment reflecting a more ecological perspective (Belsky, 2005). The lifespan dimension also suggests how the dynamic aspect of the construct might be expected to reflect both continuity and change throughout an individual’s life where different periods—especially adolescence—may also bear unique influences. Indeed, Crittenden and colleagues (Crittenden & Clausen, 2000; Crittenden & Liandini, 2011) perhaps more than most, have charted how a ‘lifespan’ perspective in attachment might be elaborated. More recently, Simpson & Rholes (2015) have extended this organisational developmental approach to an exploration of attachment processes in the formation and maintenance of adult romantic relationships.

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210 As indicated in Chapter Two, attachment may indeed fit well within various psychoanalytic models. Indeed, not all psychoanalytic models make commitments to a ‘continuity hypothesis’. However, where psychoanalytic approaches do not fully embrace empirical commitments, but mix empirical with more clinical approaches—approaches that posit developmental assumptions without accompanying commitments to ground these in empirical study—then they would fall short of the goals set within Bowlby’s overhaul. Sroufe has recently re-suggested that Attachment Theory has “saved psychoanalytic or psychodynamic perspective in the clinical fields and preserved these perspectives for the good of psychology” (2016, pp. 997, loc. 37416). His observations are indeed useful ones, acknowledging important shifts. However, I might argue that this judgment may nonetheless be premature, as the field continues in places (Eagle, 2013, 2014) to retain its ambivalent relationship with Bowlby’s empirical recommendations.
The personality component is perhaps the more interesting aspect of the three in a DLPC proposal. Indeed, Sroufe has promoted a type of ‘personality hypothesis’ that connects individual development to unique outcomes: “the quality of primary attachment relationships strongly influences early personality organization for attachment” (1986, p. 845). Waters (1981) had also previously suggested that attachment represented three complementary models, a behavioural system, a relationship and environmental responsiveness of individual differences (a more dynamic version of traditional personality trait models.)

6.2.2 Attachment DLPC and Personality Trait Difference

Indeed, attachment researchers have more recently argued for the inclusion of attachment within personality theory and research. Fraley and Shaver have noted, “Most of the classic issues, debates, and conundrums in personality psychology have played themselves out within the field of attachment research, with largely productive results” (2008, p. 536). Attachment is also regularly studied employing concepts such as ‘self’ (Cicchetti & Beeghly, 1990) and ‘person’ (Sroufe, 1990). In fact, social psychologists have also noted the role attachment pays in emerging ‘personality’ (Mikulincer & Shaver, 2012). See also the discussion of self-organisation above.

However, it is the trait approach within traditional personality study that may be more relevant here. The DLPC construct contrasts with and complements the more traditional dispositional approaches to difference found in infant temperament study (Rothbart, 2007) and adult personality trait studies (Ashton, 2013). For a detailed look at the similarities and differences between the approaches to personality traits and attachment global capacities see Fraley and

[211] Although philosophical discussions of these two terms are vast (Gallagher & Shear, 1999; Tauber, 2016), delving into the details would be beyond the current scope.
Shaver (2008). Early attachment disputations focussed in part on whether observed *attachment differences* might better be considered in terms of temperament in infancy (Vaughn, Bost, & Ijzendoorn, 2008) and to a lesser extent perhaps as adult personality traits in later adulthood. However, today, such disputes have receded as evidence has demonstrated interesting but nonetheless orthogonal relations between attachment difference and differences in both temperament (Vaughn & Shin, 2011) and adult traits (Noflife & Shaver, 2006).\textsuperscript{212} Whereas the focus construct for each area of study counts as what Deonna & Terroni (2012) describe philosophically as *affective dispositions*, attachment may differ dramatically in its potentially more dynamic lifespan nature. Attachment differences appear to develop, change and manifest in in more variable contexts—e.g. specific relationships—than temperament and traits might appear.\textsuperscript{213} Attachment research has also become a robust partner in the personality difference and health literature with output resembling if not exceeding research for the Five Factor (Costa & McCrae, 2003), the HEXACO (Ashton, Lee, & de Vries, 2014) and Sensitivity Reinforcement Theory (Corr, 2008)—formerly Gray’s (1987) BIS/BAS approach. Each approach to difference has also provided support for the identification of possible individual risk factors and has valuably contributed to the field of psychiatry. However, as argued next, the study of the influence of both personality and attachment on well-being has also not been restricted to trait-difference study alone.

6.2.3 The DLPC Integrative Value for Personality Research

In arguing for a more integrated approach to personality studies, McAdams has also noted “the idea of the trait is not robust and multifaceted enough to account for the full gamut of personality” (2015, p. 253). He has directed attention to two additional strands of *personality* study in social psychology. He proposes a notion of human selfhood that “envisions three separate but interacting lines of personality development, running from infancy or early childhood through adulthood. Over time, a person becomes a social actor, a motivated agent, and an autobiographical author” (McAdams, 2015, p. 253). Figure 6.2 depicts McAdams position and indicates at a very high level where and how attachment might fit within a current view of an integrated personality research. I propose that attachment might support McAdams position, integrating well within his three threads of study. Fraley & Shaver have also suggested that “Attachment theory and research provide a model of integration across what

\textsuperscript{212} For example, RST research on attachment style suggests modest correlations between attachment avoidance and BAS; modest correlations between attachment anxiety and FFF system; and marked correlations for both with BIS (Jiang & Tiliopoulos, 2014). Five Factor and attachment style research has also shown a range of correlations. For example, notable correlations have been found between both anxiety and avoidance and Neuroticism (Noflife & Shaver, 2006).

\textsuperscript{213} Trait discussion is hardly unanimous on these points. Fraley & Shaver (2008) have summarised the controversial discussions of trait development and measurement within personality research. Indeed, they suggest psychometric challenges in demonstrating test-retest stability for both global attachment and personality traits may share more in common than might be expected. They also importantly note more dynamic trait proposals such as Mischel & Shoda (2008) Cognitive-Affective Processing System (CAPS) developed within personality research. Fraley & Shaver note the latter has been also been applied to adult attachment development (Zayas, Shoda, & Ayduk, 2002). My next remarks on McAdams proposals for integration and attachment might also reflect this integration.
were once separate and ferociously defended fiefdoms within personality psychology” (2008, p. 538).

I will suggest here that at a relatively high conceptual level attachment as a DLPC might integrate well with each of the three personality threads. McAdams positions the study of dispositional traits within the earliest development of the social actor. Attachment shares similar interests here. Indeed, more dynamic approaches to possible integrated personality study—cognition, affect and context—(Walter Mischel & Shoda, 1995; Walter Mischel & Shoda, 2008) have also previously been taken up an applied to attachment (Zayas et al., 2002). (See discussion in Chapter Eight.) As noted, attachment may exert moderating influences on early temperament. A possible role for later adolescent traits in attachment development has also been hypothesised (Pietromonaco & Barrett, 2000)—hence the question mark in the diagram. (Possible bidirectional influences of attachment upon traditional personality constructs, and v.v., are noted by the use of green arrows.) Whereas attachment has been shown to be orthogonal to these traditional dispositions, questions of interaction between the two over the lifespan are now the norm in attachment study. Attachment also appears to be potentially well correlated with the agent and narrator strands of personality development, these latter of which have served as mainstays in social psychology. On the one hand, these final two are often considered to focus on the uniqueness of individuality personality as opposed to difference. However, on the other hand, attachment may provide a basis for both identifying underlying supportive capacities and thereby introducing a notion of difference within these valuable realms of studies. Discussions in Chapter Four have also indicated now the notion of agent/agency has been included within the attachment ‘competency hypothesis’ discussions and provides a possible demonstration of how certain attachment outcomes come to pass. Equally, the concept of a narrator with a narrative history
has also played conceptually important roles within Holmes (2001) proposals for an attachment ‘narrative competence’ hypothesis. This has also served as the basis for the innovative AAI adult measure (H. Steele, Steele, Sroufe, & Jacobvitz, 2008). Finally, Fraley & Shaver (2008) have also identified how these latter more humanist traditions in personality research match up well with attachment studies.

6.2.4 Attachment DLPC as Support for Goal-Corrected Partnership

The previous subsections represent an obviously general proposal and space does not permit a more detailed investigation. But it suggests how the DLPC complements the early trait thread of personality study, and might introduce a possible difference marker within the two latter ‘uniqueness’ constructs. The DLPC also supports the acceptance of a Phase Four GCP. Chapter Seven and Chapter Nine will suggest how a GCP might be better understood as lifespan developmental phenomena. The DLPC suggests how a GCP operates across the breadth of personality development, something that must therefore entail the cognitive capacities originally envisioned by Bowlby. Although the notion of a dynamic personality construct may prove valuable for expanding our conceptions of attachment relative to the related field of personality development, there is a second field of study from which attachment has found strategic conceptual support, Developmental Systems Theory (DST).

6.3 Attachment Organisational Construct within a Developmental Systems Theory (DST) perspective

6.3.1 Attachment and DST

Attachment as organisation (and DLPC) and DST make for a natural fit. (This discussion was signposted in Subsection 3.6.2 of a previous chapter where DST was introduced as a methodological complement to Tinbergen.) At least two points can be highlighted here. Although not fully acknowledged in either direction, both the organisational perspective for Attachment Theory and the broader field of DST have been recognized as conceptually compatible—if not virtually similar—methods for approaching development. Second, a simple point here is that an individual’s attachment system might also be reasonably considered to be part of a developmental system. The latter concept provides a conceptual platform for integrating attachment as both process and entity.

From an attachment side, Laible and Thompson (2000) have recast the organisational approach to attachment theory, first articulated by Sroufe and colleagues, in light of the dynamic notion of self-organisation. In doing so they have sought to explain ongoing attachment development in light of dynamic principles governing ongoing experience.²¹⁴ From

²¹⁴ Technically speaking Laible and Thompson have adopted a ‘Dynamic systems and self-organization’ approach (2000, pp. 299-303) that may differ in places with DST. Descriptions of self-organization as set out in the introduction to the same edited volume by Marc Lewis and Granic pointed initially to the influence of dynamic systems (DyST). However, Lewis and Granic also acknowledged parallel work carried out by systemic thinkers (i.e. DST), including attachment researchers mentioned in this discussion of organization such as Sroufe and Sameroff
the opposite direction, Susan Oyama (1985/2000) one of the first standard-bearers for DST, noted early the systematic perspective adopted by Attachment Theory. When acknowledging conceptual shifts in employment of sensitive periods in developmental study she also noted Bowlby’s employment of developmental pathways as an early example of interactionism (Susan Oyama, 1979, p. 96). In similar fashion, Taylor (2001) has selected attachment as one of his examples for a discussion of DST. He notes Bowlby's narrative reframing (1988) of an earlier longitudinal social ecological study of depression risk in middle aged women (Brown & Harris, 1978). Taylor notes how childhood loss of a parent was formally considered amongst other variables—what he terms the Brown-Harris-Bowlby account—to illustrate distributed social agency within DST perspective. Taylor’s article was published in an important DST philosophical publication, Cycles of Contingency (S. Oyama, Griffiths, & Gray, 2001). In addition, the co-inclusion of both Attachment Theory (Sroufe & Rutter, 1984) and DST (Cicchetti, 2010; A. J. Sameroff, 2000) as fundamental pillars for research within the discipline of developmental psychopathology would also argue for DST’s formal extension into Attachment Theory.

**6.3.2. Attachment as a Developmental System**

What may also be intuitively appealing for Attachment Theory is a recent proposal from within DST that focuses on process and the definitional boundaries for a developmental system. Griffiths and Stotz have suggested “what makes DST a process theory is that it seeks to explain developmental outcomes as the result of a dynamic process in which some of the interacting factors are products of earlier stages of the process, rather than as the result of the arrangement of pre-existing factors into a static mechanism” (in press, p. 5) The developmental system as an evolutionary unit is identified as the lifespan. This unit includes a traditional developing biological entity plus the history of the series of relationships with its broader environment. My proposal for a DLPC depicted above in Figure 6.1 might fit well within such an approach. This would also match the ideas within an attachment approach to IWMs where the self is partially depicted as consisting within series of representations of key attachment relationships over time as well as ongoing connections to broader environment.

Having extended the organisational perspective to the proposed conceptualisation of the DLPC and anchoring both within the DST, a few important consequences may be valuable for consideration.

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(2000, pp. 4-5). It is well beyond the current space limitation to engage the detailed differences between DST and Dynamic Systems (DyST). While the first engages a search for complex mechanisms, the latter has sufficed to include the more descriptive ‘attractor states’. For a brief review of these differences see Griffiths & Tarbery (2013). Although choosing the DyST terminology, Laibile and Thompson do not appear to abandon crucial attachment mechanisms, i.e. IWMs, in considering the possibility of the reorganization of attachment representations over time.

215 This study has interestingly been included in Kendler and colleague’s (Kendler, Kessler, Neale, Heath, & Eaves, 1993) formulation for an integrated multi-level model for major depression in women.
6.4 A few Conceptual Considerations Arising from the Organizational Perspective, DST and DLPC

What might be the specific consequences for extending an organisational perspective as a DLPC and endorsing DST? The chapter’s epigraph suggests that Bowlby was open to key fundamentals of DST. These also represent the practicalities underpinning the organisation perspective, DLPC and the employment of a developmental system label. When setting out a first framework for DST, Ford and Lerner (1992) identified two core tenets which I argue become crucial for this expansion of Attachment Theory as well: (1) ‘developmental contextualism’, and (2) ‘dynamic interactionism’. The first will be taken next whilst the second notion will be discussed in Subsection 6.4.2. Additionally, three concepts logically emerging of from a developmental pathways approach—INUS conditions, equifinality and multifinality—will be addressed in Subsection 6.4.3.

6.4.1 Developmental Contextualism

Griffiths & Stotz have recently observed, “Developmental contextualism is a modern version of the epigenetic, as opposed to predeterminist, view of development” (in press, p. 8). Indeed, Ford & Lerner have indicated their first tenet of DST as having been notably derived in part from the work of the developmental embryologist, Gilbert Gottlieb (1929-2006) (for a general overview of his theoretical work, see Gilbert Gottlieb, 1997). Indeed, Gottlieb has been acknowledged as having contributed to something of a revolution in developmental thinking (see discussion of his legacy in Hood, Halpern, Greenberg, & Lerner, 2010). Gottlieb has described a 'central dogma', predetermined epigenesis, that is currently being supplanted by the more complex probabilistic epigenesis (2010). He defines this notion in contrast to the 'central dogma':

... In line with the evidence now available at all levels of analysis, probabilistic epigenesis holds that there are bidirectional influences within and between levels of analysis so that the appropriate formula for developmental analysis becomes genetic activity → structure → function. In this view, neural (and other) structures begin to function before they are fully mature and this activity, whether intrinsically derived ('spontaneous') or extrinsically stimulated (evoked), plays a significant role in the developmental process (G. Gottlieb, 2007, pp. 1-2).

Gottlieb approach to multi-level considerations has been captured in a much-reproduced diagram, Figure 6.3.

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216 The ‘central dogma’ is no doubt a reference to Crick’s seminal discussion of transfer of information from DNA to proteins highlighted in the named article “Central dogma of molecular biology” (1970). For a recent discussion in terms of causal influence and DST, see Stotz & Griffiths (In Press).
The implications of ‘developmental contextualism’ for Attachment Theory would suggest that answers to developmental questions, including those related to attachment antecedents, continuity, operational processes and outcomes as introduced in Chapter Four, must be considered in terms of multiple levels. At the same time, attachment must therefore avoid granting ontological priority to any level, even the IWMs so pivotal for Bowlby’s proposed mechanisms. What is required is an integrated complex approach to the study of mechanisms manifest across multiple levels. (See also discussion of Gillath’s proposals in Section 6.5 below.) Indeed, Sameroff’s (2010) suggestions for a unified model of development has incorporated both Gottlieb’s multi-level approach and an attachment framework.

6.4.2 Dynamic Interactionism

If Ford and Lerner’s first principle established the depth of levels required for developmental study, their second tenet, ‘dynamic interactionism’, expands DST providing an alternative to the ‘static interactionism’. Two aspects of this second tenet are particularly valuable for raising the conceptual bar in attachment study: (1) notions of causation, and (2) explication of the role past historical development on a present system.

6.4.2.1 Notions of Causation

First, according to dynamic interactionism, causal influences may be multiple, bidirectional, and reciprocal. Gottlieb and Halpern have noted, “In a developmental systems view, the cause of development—what makes development happen—is the relationship or ‘coaction’ between two or more components themselves . . . Many coactional relationships are bidirectional, so they are subtle and complex and therefore not easily recognized” (Gilbert 217).

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217 To avoid confusion, I should indicate that the identified notion of dynamic interactionism was a term first introduced by Lerner (1978). He contrasted this term with static interactionism. The dynamic-static distinction has been incorporated into Ford & Lerner’s developmental contextualism (1992). Griffiths & Tabery (2013) have noted the authors’ acknowledgement of a similarity with Gottlieb’s probabilistic epigenesis. I would suggest therefore that the dynamic-static distinction might be seen to appropriately parallel the probabilistic-predetermined distinction.
Coactional or reciprocal causation posits a role for feedback within causal relationships (Laland, Odling-Smee, Hoppitt, & Uller, 2013). Indeed, Zelazo has recently updated Gottlieb’s model to indicate how and where these more dynamic paired interactions may be present between developmental influences. See Figure 6.4 below. However, the exact nature of these diagrammatic arrows may be less clear.

![Figure 6.4: Reciprocal and Bidirectional Influences (Zelazo 2013)](image)

On the one hand, what is clearly valuable here for attachment is the explanatory platform for expanding past efforts at providing one directional, single level notions of causality. Attachments as entities and processes depicted above for the DLPC can easily be anchored in these models as interpersonally, multilevel and contextually dynamic. On the other hand, care must be taken here to differentiate the same-level and between level-influences, whilst preserving a role for interventionist approaches. How exactly are we to understand these arrows? One risk for attachment research is the employment of vague notions of upward and downward causation. Stots & Griffiths (In Press) have recently raised questions of causal influence and indicated a path for integrating fine-grain interventionist and DST multi-level information theoretic positions. They also turn to useful distinctions made by Craver and Bechtel:

Both top-down and bottom-up causation, ‘... describe mechanistically mediated effects. Mechanistically mediated effects are hybrids of constitutive and causal relations in a mechanism, where the constitutive relations are interlevel, and the causal relations are exclusively intralevel’ (Craver & Bechtel, 2007, as cited by Stotz & Griffiths, in press, p. 21)

There are actually several similar conceptual renderings for this sort of developmental causation. It is also worth noting that Gottlieb’s ideas have been embraced by the discipline of developmental psychopathology (Rutter & Sroufe, 2000), a field that has embraced Attachment Theory in a more robust fashion and includes several active attachment researchers (Cicchetti & Toth, 2011; Rutter et al., 2009; Sroufe et al., 2010).

Causal questions are far from resolved in science, biology and philosophy. Stotz & Griffiths discussion of ‘interventionism’, ‘biological specificity’, ‘non-arbitrariness’ and ‘mechanistically mediated effects’ in biological systems is well beyond the scope for present purposes. But these discussions should offer greater clarity in approaching future complex causal questions for attachment.
For the moment it should suffice to acknowledge alongside one strand of current non-reductive thinking that the interlevel constitutive relations—arrows—should not be confused with intralevel causal relations—that drive development across time. The recognition in Section 4.3 of interventionist causation identified in the shift of attachment security via enhanced caregiver-infant interactions is interpersonal (behavioural) and ‘intralevel’ and might be considered a causal relation. Any further hypotheses about the role for or impact upon neurological or psychological mechanisms would therefore engage constitutive relationships across feedback phenomena.

6.4.2.2 Role of past history in present behaviour

Second, the role of the organism or system, including its cumulative history, in directing and influencing its own development must also be acknowledged. Griffiths & Stotz have recently noted how such dynamics must be studied as temporally extended, “The idea of developmental dynamics embodies one of the basic ideas of process biology, namely that the developmental system is defined, and in part physically produced, by the process of development” (in press, p. 12). The value for attachment is the provision by DST of a more dynamic model for explaining how early experience might impact later experience. This temporal-historical perspective has been captured in notions such as self-organisation (M.D. Lewis & Granic, 1999) and identified within Sroufe’s (1986) attachment ‘personality hypothesis’, something first discussed as coherence over time (Sroufe, 1979). The DST approach captures Bowlby’s developmental pathways approach he adopted from Waddington, the latter noted in several places in previous chapters.

6.4.3 Multiple Pathways: INUS Conditions, Multifinality and Equifinality

Lastly, these alternative notions for causal change and role of the system in influencing its own development point to the expectation for contingent multiple pathways for development. Bowlby had indeed anticipated just such a requirement in a chapter in his second volume on loss: *Pathways for Growth in Personality* (1980)\(^2\), which is manifest in research on discussions of *attachment difference* noted above. The evolution of these contingent pathways may be illustrated by three few useful notions in contemporary philosophical and developmental discussions: INUS causal conditions, equifinality and multifinality.

The INUS notion was first introduced by Mackie in a classic article (1965) that noted the inadequacies of applying a simple ‘necessary and sufficient conditions’ approach to the identification of causal conditions. He noted how many discussions of causality might better be depicted as *INUS causal conditions*, defined as “an *insufficient* but *necessary* part of a condition which is itself an *unnecessary* but *sufficient* for the result [italics in original]”

\(^2\) The origin of the notion of developmental pathways can be traced to Waddington’s work from the 1950s. He first coined the term epigenetic landscape as descriptor for internal interactive development (Waddington, 1957). Indeed, Bowlby made explicit epigraphic mention of Waddington in the chapter on pathways for personality development (Bowlby, 1980, p. 363).
Indeed, Schaffner has also identified the value of Mackie’s position for non-singular etiological discussions within nosology in philosophy of psychiatry (2008, pp. 75-76). Attachment development, when considered in light of the proposed notion of a DLPC, might also be better conceived along a line similar to that suggested by Mackie. Consequently, any attachment developmental case seeking simple, singular, linear necessary and sufficient conditions will inevitably be shown to be a weak one. Two related notions employed in the field of developmental psychopathology would seem to follow on from Mackie’s INUS conditions.

Cicchetti, has described how two relevant principles, *equifinality* and *multifinality* function in research (Cicchetti & Rogosch, 1996). These concepts seek to address “the myriad pathways to any manifestation of adaptive or maladaptive outcomes in any individual”: Equifinality can be understood to apply where “in an open system . . . the same end state may be reached from a variety of different initial conditions and through different processes”; multifinality acknowledges that “individuals may begin on the same major pathway and as a subsequent ‘choices’ exhibit very different patterns of adaptation or maladaptation” (Cicchetti & Rogosch, 1996, pp. 597-598). The two notions can also be seen to be logical outcomes of the INUS position. Together with the INUS notion, they indicate quite practically, how knowledge in development is unlikely to be accounted for in a simple linear ‘one-size-fits-all’ manner. They are particularly relevant for tempering attachment discussions where ‘prediction’ may be less the benchmark than previously expected. This would especially be the case for constructs as antecedents, outcomes, styles and the related but inadequate notions of ‘continuity’ without plasticity.

6.5 Attachment within the RDoC: An Imperfect Fit but Hopeful Exercise

6.5.1 The RDoC Project Criteria and Attachment

I shift here from the theoretical to a possible nosological application of Attachment Theory. i.e. the inclusion of attachment in the National Institute of Mental Health (NIMH) Research Domain Criteria (RDoC) project. I argue here in favour of the acceptance of attachment within the RDoC framework. Nonetheless, a degree of conceptual caution will also be counselled in light of growing recognition of the challenge presented by biological complexity. The developmental researcher Sroufe has recently suggested that “The developmental work on attachment suggests a different path forward from the continued search for correlates of attachment suggests a different path forward from the continued search for correlates of

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222 Multifinality should be distinguished from the similar sounding notion, *multiple realizability*, a concept discussed in philosophy of mind (Bickle, 2013). Each notion addresses a different conceptual question in a different Tinbergen realm. First proposed by Fodor (1974), Antony suggests “the theory of multiple realizability was meant to show how one could resist reductionism without embracing dualism” (2008, p. loc.2377). Although potentially sharing similar philosophical commitments, the focus of *multiple realizability* is to provide a possible explanation for the instantiation of mental states. As such it would seem to fall within Tinbergen’s realm of ‘mechanism’. Multifinality (and equifinality) is concerned with the multiplicity of causal developmental pathways. Those discussions fall within Tinbergen’s realm of ‘ontogeny’.  

223 An earlier draft of this section, including a summary of Subsections 1-4—“Attachment within the RDoC: Promising ‘biomarkers’ carrying deceptively complex conceptual baggage”— was given as a peer-reviewed presentation to the Association for Advancement of Philosophy and Psychiatry (AAPP) annual conference on 14 May 2016 in Atlanta.
DSM-based categories . . . [indeed,] understanding developmental course is more important than cataloguing manifest symptoms alone” (2013, pp. 1221, 1226). Indeed, attachment research would appear to clearly meet the RDoC program’s two inclusion criteria: (1) “strong evidence for the validity of the suggested construct”, and (2) “strong evidence that the suggested construct maps onto a specific biological system, such as a brain circuit” (Cuthbert & Insel, 2013, p. 6). After a brief display of the vast number of identified units of analysis for attachment, I offer a summary of two relevant studies from 2015. My final point, a theoretical comment, focuses on the relative fit between what Sroufe has described as an attachment informed “path forward”—captured above in ‘organizational construct’—and the less dynamic, more context neutral, endophenotype approach in the current RDoC project (but see also discussion of possible next directions for RDoC in Cuthbert, 2014).

6.5.2 The Specifics of Attachment within RDoC

The RDoC has divided its matrix into five major Domains: (1) Negative Valence Systems, (2) Positive Valence Systems, (3) Cognitive Systems, (4) Social Processes, and (5) Arousal and Regulatory Systems. The Attachment and Affiliation construct is one of four Constructs within the fourth Social Process Domain. Within each construct, research is directed to seven units into which individual elements are identified.

On the one hand the Attachment and Affiliation subcommittee group has assembled an impressive list of possible elements for potential research taken from research studies to date (See Table 6.1).

<table>
<thead>
<tr>
<th>Units</th>
<th>INDIVIDUAL ELEMENTS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Genes</td>
<td>3CRF, 3OXT, AVPR1A, CRFR2, DRD1, DRD2, MOR, OXTR, Tyrosine Hydroxylase</td>
</tr>
<tr>
<td>Molecules</td>
<td>3CRF, CRFR2, D1, Dopamine, KOR, Mu opioid receptor, Oxytocin, oxytocin receptor,</td>
</tr>
<tr>
<td></td>
<td>Vasopressin, vasopressin 1a receptor</td>
</tr>
<tr>
<td>Cells</td>
<td>Magnocellular OT</td>
</tr>
<tr>
<td>Circuits</td>
<td>Amygdala, BNST, FF gyrus, NAcc, OFC, PVN, VMPFC, VTA- NAcc- VP-amygdala</td>
</tr>
<tr>
<td>Physiology</td>
<td>Activation of sympathetic activity, HPA axis activation, HPA down-regulation, Immune</td>
</tr>
<tr>
<td></td>
<td>markers, Immune responses (“sickness”), Sex steroid changes, Vagal tone, Vagal</td>
</tr>
<tr>
<td></td>
<td>withdrawal</td>
</tr>
<tr>
<td>Behavior</td>
<td>Attachment Formation: Maintaining proximity, Preference for individual; Attachment</td>
</tr>
<tr>
<td></td>
<td>Maintenance: Distress upon separation</td>
</tr>
<tr>
<td>Self-Report</td>
<td>Adult Attachment Interview, Attachment Questionnaire for Children Scale, Attachment</td>
</tr>
<tr>
<td></td>
<td>Style interview, Bartholomew and Shaver Bereavement scales, Experience in Close</td>
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<tr>
<td></td>
<td>Relationships Scale, Inventory of Parent and Peer Attachment Scale, Parental Bonding</td>
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<tr>
<td></td>
<td>Instrument, QSORT, Parent Attachment interview, Social Anhedonia scale, Social</td>
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<td></td>
<td>subscales of depression</td>
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<tr>
<td>Paradigms</td>
<td>Separation, Social Buffering of Stress, Strange Situation</td>
</tr>
</tbody>
</table>
Many of those studies relied upon in assembling the elements may have been less than conclusive but remain viable platforms for further research. For example, various genetic studies have typically been difficult to replicate and inadequate alone to account for range of causal interactions that might account for attachment phenomena (Bakermans-Kranenburg & van Ijzendoorn, 2016) However, the elements are obviously not intended to form a comprehensive mechanistic picture, as the underlying mechanisms are far from known. Nonetheless, work focused on the influence of the neuropeptide oxytocin on early attachment has been quite encouraging and I will offer a quick review of Feldman’s team’s (Feldman, 2015) encouraging work with infants (See also the discussion of developmental antecedents, functional differences and mechanisms above.)

On the other hand, the depiction of attachment in the RDoC would appear to lack the sort of robustness and complexity as captured in the Tinbergen construct analysis provided earlier in the chapter. Notably the RDoC does not clearly differentiate lifespan phases. Equally, it does not fully identify the range of attachment phenomena for study. As demonstrated above, at least three distinct phenomena have been submitted to multi-level study: antecedents to attachment formation (ontogeny), attachment differences (function) and attachment real-time processes (mechanisms), including pivotal psychological mechanisms or IWMs. The Gillath survey (2015) considered below provides an example of neuroscientific research on just such psychological mechanisms that do not fit well within the current RDoC matrix. What follows are two example summaries containing simple acknowledgment of attachment ‘biological markers’, which demonstrate both the up side and potential down side to the current RDoC approach.

### 6.5.3 Attachment Infant Longitudinal Research: Oxytocin and Biobehavioural Synchrony

First for consideration is Feldman’s (2015) summary of her team’s complex developmental research on the interactions of oxytocin and biobehavioural synchrony—the last concept has been considered as a parallel formulation for attachment processes. The article is noteworthy in providing 15 pages of preliminary detailed conceptual preparation for the seven pages of actual longitudinal study that follow. It is also worth pointing out again that multilevel research has historically pursued at least three distinct Tinbergen explanatory objects: real-time attachment processes (mechanisms), developmental antecedents (ontogeny) and security differences (function). Feldman’s work combines each of the three. Feldman has delivered an

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224 Here is the combined definition employed by the RDoC: “Affiliation is engagement in positive social interactions with other individuals. Attachment is selective affiliation as a consequence of the development of a social bond. Affiliation and Attachment are moderated by social information processing (processing of social cues) and social motivation. Affiliation is a behavioral consequence of social motivation and can manifest itself in social approach behaviors. Affiliation and Attachment require detection of and attention to social cues, as well as social learning and memory associated with the formation of relationships. Affiliation and Attachment include the positive physiological consequences of social interactions and the behavioral and physiological consequences of disruptions to social relationships. Clinical manifestations of disruptions in Affiliation and Attachment include social withdrawal, social indifference and anhedonia, and over-attachment.” https://www.nimh.nih.gov/research-priorities/rdoc/constructs/affiliation-and-attachment.shtml Accessed 02/08/16.
exemplary approach for translating valuable but suggestive animal studies—in this case the formative role of oxytocin systems and biobehavioural synchrony in the context of early sensitive/critical periods for neurological development—into more applicable human longitudinal developmental research.

Feldman provides three conceptual clarifications: (1) “Biobehavioral synchrony is highlighted as an overarching mechanism by which the early environment exerts its effect through the coordination of biological and social processes during social contact” (Feldman, 2015, p. 372). Feldman and colleagues have importantly researched the unique influences of both infant-mother synchrony—typified as rhythmic-calm synchrony manifested in visio-affective phenomena measured via heart rate, stress attenuation and brain activation—and infant-father synchrony—externally, physically focussed interactions with the external world (Feldman, Bamberger, & Kanat-Maymon, 2013). (2) “Critical periods (CP) are defined as strict time windows during which experience provides information that is essential for normal development and permanently alters performance. . . [and (3)] SP are limited times in development during which the effect of experience on brain function is particularly strong” (Feldman, 2015, p. 370). The employment of these last two traditionally controversial notions (Bornstein, 1987) reflects their current more nuanced application to multi-leveled and interrelated developmental phenomena. One clarifying point may also be reinforced here: contrary to popular misconception, at the higher conceptual level of both relationships and capacities, Attachment Theory does not espouse simplistic critical or sensitive period notions (Thompson, 2001). Rather, CP and SP activities are identified as more fine grained, multi-dimensional and cascading phenomena. Consequently, the underlying neurobiological structures that partially support emerging attachment capacities may indeed be impacted by early adversity within such described periods—especially as a consequence of toxic stress.

225 The oxytocin work was interestingly pioneered by the former head of the RDoC initiative, Thomas Insel, in work (1997) with prairie voles (Microtus ochrogaster) and montane voles (Microtus montanus).
The depiction of the interdependent influence of these three levels attachment caregiving ties has also been shown to be applicable to later romantic and close friendship ties as well.

Feldman depicts her four longitudinal studies as ethically alternative ‘natural experiments’ with at risk individuals. The four cohorts include (a) premature infants in Kangaroo Care protocols (i.e. regular mother-infant skin-to-skin contact), (b) social developmental comparisons of triplets with twins and singletons, (c) children in the context of maternal postpartum depression, (d) children exposed to continuous war trauma. The diagrammatic presentation of the results in Figure 6.6 demonstrates differentiated results across several domains including cognitive, social, emotion-regulatory, and mental health. One fascinating result was the finding of developmental ‘catch up’ in the 2-5 year period for triplets who typically experience suboptimal synchrony in earlier childhood, no doubt a consequence of the enormous requirements on parents. This developmental correction would appear to provide an example of the “peers as therapist” findings identified by Harlow and Suomi amongst young Rhesus monkeys (Feldman, 2015, p. 387). Feldman concludes that “the results from these repeatedly assessed cohorts also demonstrate the complexity and multifinality of the human condition” (Feldman, 2015, p. 384).

Feldman’s study pursues four aims:
(a) chart a program for rigorous research on SP in humans by offering a human-specific conceptualization, (b) describe the critical role of the oxytocin (OT) system in supporting neural plasticity at multiple levels from single neuron to intergroup, (c) highlight synchrony as a central mechanism in this process, and (d) utilize four high-risk birth cohorts, each followed across the first decade of life, as distinct windows to formulate mechanism-based hypotheses on SP in human social development (2015, p. 370).
6.5.4 Attachment and Neuroscientific Evidence for Adult Internal Working Models

I move now to a second example, again a summary review. Gillath (2015) covers work—including research by his team—that has focused on the neuroscientific study of attachment psychological mechanisms in adulthood. Gillath acknowledges the challenge here: “It is still unclear what mechanisms allow the formation of IWMs and their updating over time, how IWMs differ from other cognitive structures and processes such as schemas, and how their top-down cognitive process differs from general top-down processes” (2015, p. 39) Four points will be highlighted here. First, adult processes for both the formation and identification of attachment figures have been shown to be particularly associated with activation of the amygdala and hippocampus. As such they represent bottom-up, conditioned tagging and implicit processing of emotional material respectively. Second, they found that top-down regulation of attachment-related thoughts are associated with the prefrontal cortex (PFC) in a manner similar to regulation on non-attachment-related thoughts suggesting that IWMs and their regulatory mechanisms may be manifestations of more general regulatory processes. A third point highlights the previously mentioned identified causal relations between improved health and IWM security activation via security priming techniques. When these experiments have been observed via neurological imaging, an interesting but complex picture emerges. Attachment security would appear to be a simultaneous manifestation of cognitive, emotional and behaviour/motivational brain regions. Last, attachment differences associated with capacities for emotion regulation—secure, anxious and avoidant—have also demonstrated correlations with different neurological regions. In particular, the intense experience of emotion associated with anxious style has been shown to correlate with lower activation in OFC, a region central to regulation.

227 These techniques include presentation of pictures (either explicit/supraliminal or implicit/subliminal) suggesting attachment-figure availability (e.g. a Picasso drawing of a mother cradling an infant in her arms); presentation of the names of actual people designated by participants as security-enhancing attachment figures; guided imagery concerning the supportiveness of an attachment figure; visualization of the faces of security-enhancing attachment figures; and viewing the photograph of an attachment figure.
In Figure 6.7 Gillath has highlighted an issue for attachment research that would appear to equally apply to the RDoC endeavour. Attachment may lack a fully integrated model that supports what he describes as macro- and micro-attachment phenomena. These would appear to partially parallel discussions of exophenotypes and endophenotypes, the former being deemphasised and the latter emphasised in the RDoC (Lilienfeld, 2014). I would add three additional challenges to Gillath’s depiction. An acceptable framework must also account for reciprocal dyadic interactions, Gene X Environment epigenetic transactions as well as account for the unique phenotypic history of an individual. However, Attachment Theory has in principle acknowledged these requirements in my proposals for an organisational approach as captured within terms of developmental systems.

6.5.5 Complexity in Attachment Research and RDoC Attachment Employment: Only a Partial Fit?

Cuthbert has encouragingly noted, “RDoC is intended as an integrative effort in which the goal is to relate measurements taken across multiple units of analysis rather than trying to work toward an eliminative reductionism . . . the inclusion of biological measures in an experimental classification system does not mean an exclusion of etiologies derived from social, cultural, or other environmental sources” (2014, pp. 140-141). This response is an important one. However, as Kendler has cautioned, identifying an integrative pluralism as a philosophical framework for psychiatry may also require us “to accept ‘patchy reductionism’ with the goal of piecemeal integration” along the way (2005, p. 433). My argument here is that Attachment Theory’s approach to complexity might provide important piece of that integration. Lacking any formal commitment to non-reductionism, or any systematic philosophical on questions of levels and causation, the RDoC intuitively appears underprepared to benefit from evolving gains made by Attachment Theory in embracing complexity.
To recap, RDoC research must ideally not exclude interpersonal and psychological attachment mechanisms. Indeed, as complex biological phenomena, attachments in the RDoC require consideration as both complex endophenotypes and exophenotypes. This point has been made more generally for the RDoC by Lilienfeld (2014). An organizational or developmental systems perspective has also proven useful in capturing both the multi-level and developmental dynamics—i.e. bidirectional and reciprocal systemic influences—observable across attachment phenomena. Hopefully, attachment may therefore prove a valuable developmental exemplar for the RDoC, offering an avenue for avoiding simple reductionism and preserving possible systemic roles for psychological mechanisms.

6.6 Specific Implications of Attachment as Organisation for Psychiatry

Outside the field of child psychiatry (Goodman & Scott, 2012; Thapar, 2015) and the DSM recognition of the Disinhibited Attachment Disorder (Zeanah & Gleason, 2015), Attachment Theory has played a much smaller role within adult psychiatry (Ma, 2006). Nonetheless, here are three reasons why both psychiatry and philosophy of psychiatry may benefit from greater interaction with Attachment Theory. First, the study of attachment as a DST suggested above, might provide an example of Kendler’s (2008) call for greater philosophical pluralism in psychiatry, that consciously seeks to embrace complexity (Subsection 6.6.1). Equally, attachment conceived as both a regulatory capacity of affect and self and a DLPC, might also serve as an interesting candidate for consideration within Zachar and Krueger’s (2013) call for pluralism in approaching personality disorders (Subsection 6.6.2). Finally, consideration will be offered as to how attachment’s indicated causal success might align with Murphy’s (2006) call for a move to causal validation (Subsection 6.5.3).

6.6.1 Attachment and Kendler’s Proposals for Greater Philosophical Pluralism in Psychiatry

As a more complex organizational approach encompassing DST, Attachment Theory matches well with hopes for embracing complexity in psychiatry. This has been one of the major endeavours for the field of philosophy of psychiatry, especially nosology (Kendler & Parnas, 2012, 2008). Attachment’s focus on mechanism and causality, and when teamed with DST credentials, offers an interesting non-reductive empirical developmental approach. As such it fits well with Kendler’s proposals to “sketch a coherent conceptual and philosophical frame-work for psychiatry [that] confronts two major questions: how do mind and brain interrelate, and how can we integrate the multiple explanatory perspectives of psychiatric illness?” (2005, p. 433). This framework includes eight recommendations:

1. Psychiatry is irrevocably grounded in mental, first-person experiences.
2. Cartesian substance dualism is false.

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228 Attachments and Attachment Theory play a relatively minimal role in the most recent Handbook of Philosophy and Psychiatry (Fulford et al., 2013), this despite an entire section devoted to Establishing Relationships. Where it does appear is in discussions of psychoanalysis (Hopkins, 2013; Lacewing, 2013).
3. Epiphenomenalism is false.
4. Both brain—mind and mind—brain causality are real.
5. Psychiatric disorders are etiologically complex, and we can expect no more “spirochete-like” discoveries that will explain their origins in simple terms.
6. Explanatory pluralism is preferable to monistic explanatory approaches, especially biological reductionism.
7. Psychiatry needs to move from a prescientific “battle of paradigms” toward a more mature approach that embraces complexity along with empirically rigorous and pluralistic explanatory models.
8. Finally, we need to accept “patchy reductionism” with the goal of piecemeal integration in trying to explain the complex etiological pathways to psychiatric illness a little bit at a time. (2005, p. 433)

Although it would be well beyond the scope of my current endeavor to address these specifics in detail, Attachment Theory may arguably serve as a good candidate for “a more mature approach that embraces complexity”. Indeed, as discussed below, attachment’s nomination with an RDoC matrix may serve as a good test case for making this point.

### 6.6.2 Attachment Constructs and Philosophical Discussions of Personality Disorders

One potentially interesting observation is the promise that three attachment constructs might offer for expanding our understandings of personality disorders. Whereas our knowledge of such disorders remains controversial and at best partial (Zachar & Krueger, 2013), attachment might provide possible insights into etiology, change and treatment and that might expand currently divided study (Lorenzini & Fonagy, 2013). For an interesting example, see Christian and colleagues (2016) discussion of attachment difference and psychopath.

An emphasis on attachment as risk (Construct 10), as dynamic lifespan personality construct (Construct 13) as well as a regulatory capacity in Construct 5, would all fit well within Zachar & Krueger’s (2013) pluralistic recommendations in consideration of the five possible models underpinning approaches to personality disorders. I summarise their models as follows:

1. **Pathoplasticity model**: affect course of other disorders (co-morbidity recognition)
2. **Spectrum model**: milder representations of deeper genetic predisposed disorders (e.g. schizoid PD, schizotypal PD schizophrenia)
3. **Decline in function model**: loss of function due to aberrant causal history like brain or emotional trauma (but emotional trauma is not always causally sufficient, and may require other factors/risks)
4. **Impairment distress model**: extremity of traits lead to rigidity/dyscontrol (e.g. five factors model)
5. **Capacity failure model**: similar to ‘decline’ but is a failure to develop.

Attachment constructs might reasonably align with four of the models. Attachment risk might contribute to (1) pathoplasticity models; the attachment ontogeny regulatory capacity construct might fit within both (3) a decline in function model, and (5) capacity failure model; finally, the DLPC construct might add a robust complement to the impairment distress model whereby a greater degree of plasticity might introduced into trait approaches. Attachment would seem to offer an inherently plural option for expansion of personality disorders (see also recent discussion in Levy, Johnson, Clouthier, Scala, & Temes, 2015).
6.6.3 A ‘Partial’ Shift from Construct Validity to Causal Validity?

As a biological endeavor, attachment research should be concerned with the discovery of causal physical relations—i.e. causal validity—, even as regards representational mental content. This causal interest also includes a commitment to the discovery of mechanisms that might underpin developmental change, especially IWMs. As mentioned in Chapter Two, this commitment to discovering valid causal explanations emerges from Attachment Theory’s (and Bowlby’s) TLC strategy: more specifically a commitment to non-reductive physicalism. However, for reasons discussed above, the theory's causal ambitions have only been partially realized. I want to suggest here that Murphy’s conceptual arguments for a shift from construct validity to causal validity in his proposals for a reformed psychiatric nosology (2006, pp. 201-253)—one that also includes a commitment to the discovery of relevant mechanisms—may apply in part to an argument for a refocus on causal relations in Attachment Theory research. Indeed, as a part of psychiatry, a focus on causal validity within attachment might also count as a small victory for psychiatry.

Murphy situates his discussions within the historical development of DSM research where a lack of an adequate commitment to causation in psychiatric nosology has remained a troubled affair for some time now (see the relevant discussions in the two edited volumes: Kendler & Parnas, 2012; Kendler & Parnas, 2008).229 A similar contextualizing of research for Attachment Theory may also be illuminating.

Attachment research emerged particularly through the empirical work of the developmental psychologists Ainsworth and colleagues in the late 1960s (see Chapter Five for a full history.) A significant portion of attachment research has been conducted within the fields of developmental psychology, psychiatry and social psychology. These three fields roughly match the tradition’s focus on childhood attachment, the diagnosis of attachment disorders and adult attachment—though some important lifespan work has also emerged from Main and colleague’s influential developmental study (1985). For slightly different reasons, each of these three fields has traditionally been anchored in neo-empiricist practices: namely reliance on operationalism with an emphasis upon the role of construct validity (for biological psychiatry see Murphy, 2006, p. 201) Operationalism and construct validity have consequently played an important ongoing role in attachment study, both childhood (Solomon & George, 2008, 2016) and adulthood (Crowell, Fraley, & Shaver, 2008, 2016).230 However, a few notable causal gains for attachment not typically acknowledged in the attachment measurement discussions have also been made along the way, although obviously at this stage many fewer in number. As mentioned above research in pursuit of the ‘sensitivity hypothesis’ has delivered a clear, albeit modest case for interventionist causal support for

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229 What might represent contextual change is the emergence of the RDoC project just mentioned in Section 6.5.
230 A note should perhaps be made here of the slightly paradoxical state of the psychiatric diagnosis of ‘Attachment Disorder’—a set of maladaptive behaviours resulting form non-attachment, where longitudinal and more conceptually causal study of institutionalized infants has conflicted with DSM derived perspectives (Rutter et al., 2009, pp. 435-436).
sensitive interactions (De Wolff & van Ijzendoorn, 1997). Success in attachment prospective longitudinal studies has greatly informed our understanding of a more complex developmental course of attachment (Grossmann, Grossmann, & Waters, 2005). More recent adult experimental work engaging ‘security priming’ in social psychology also looks promising for expansion of possible causal investigations (Carnelley & Rowe, 2010). Although attachment study has made encouraging inroads into the realm of causation, these potentially pivotal discussions are historically, proportionately small in comparison to the predominantly correlative studies (See Chapter Five’s historical review). But as neurobiological studies multiply, this proportion may also be shifting.

Murphy has argued forcefully for the fostering of causal validity within psychiatric nosology and the suppression of construct validity (2006, p. 202). His primary interest is the support of a more robust medical model for psychiatry that might one day categorise disorder in a causal fashion but also inform psychiatric prevention and intervention work (Murphy, 2013). As mentioned, Attachment Theory has from the outset shared a similar causal ambition. However, attachment study is not a branch of medicine/psychiatry, but rather a subset of lifespan biological behavioural development. Reliance on attachment constructs for behavioural study—especially in adult social psychology—remains a requirement. However, such a position does not endorse a neo-empiricist operationalist programme but can retain a parallel requirement for enhanced causal investigation. Therefore, one should expect construct validity to continue to play a valuable role in the attachment field but one in tandem with an increasing focus on causation. Having also argued for greater integration with personality research above, it obviously makes sense to remain open to the requirements for construct validity in pursuit of the broad study individual; difference across both personality social psychology. Indeed, the burgeoning enquiry into attachment biological research requires just such constructs delivered by the various measurement systems to frame its work. As Murphy has asserted the case for greater causal interest for psychiatry, I will also argue in a somewhat similar fashion for much greater emphasis on causal validity, though with a slightly more nuanced attitude to construct validity that reflects a differing research context.

### 6.7 Chapter Conclusion

The overall goal of this chapter has been to provide a conceptual extension of the attachment organisational construct and thereby assess possible value for psychiatry and philosophy of psychiatry. A few notable findings may be re-emphasised. Attachment Theory is best understood as distinct from psychoanalysis. This may especially be the case where a simple

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231 Halsam’s discussion (2013) in the Oxford Handbook of Philosophy and Psychiatry on the pragmatics for sound psychological assessment for psychiatry might be useful for my purposes here. Although he dodges the potentially important questions of causal validity raised in the preceding section by Murphy on a robust medical model for psychiatry (Murphy, 2013), his concise summary of reliability and validity are instructive. As a ‘biopsychosocial’ endeavour, attachment research will inevitably require a degree of psychological measurement and assessment for which both reliability—internal consistency, inter-rater reliability and re-test reliability—and validity—content, criterion-related and construct—will remain important (see descriptions of each in Halsam, 2013).
‘continuity’ hypothesis has been pursued by the latter. The organisational perspective provides a superior conceptual position for understanding lifespan dynamics. Section 6.2 took up the specifics of the thirteenth organizational construct positioning it as a *dynamic lifespan personality construct (DLPC)*. At the same time this construct was discussed as an example of a developmental system within discussion in DST. A detailed look at attachment and the RDoC project suggests how the former may be in a more advanced state as regards the recognition of developmental/biological complexity. Finally, Murphy’s case for a more central place for causal validity in psychiatry would also appear to apply well to multi-level research of attachment phenomena. The conceptual robustness implicit within an attachment organisational perspective offers much for psychiatry.

I’d like to conclude this chapter with a final comment made recently by Zweck in her discussion of interactive social development. She identifies three characteristics of the “true nature of gene x environment interaction”: such phenomena are “complex, contingent and probabilistic” (2013, p. 170). Attachment developmental phenomena, which if considered in light of the above chapter’s discussions, might also be equally considered as complex, contingent and probabilistic. I now move to Part Three where the Goal-corrected Partnership will be explored and the role of cognition in attachment lifespan phenomena might be further investigated.


Vaughn, B. E., Bost, K. K., & Ijzendoorn, M. H. v. (2008). Attachment and temperament: Additive and interactive influences on behavior, affect, and cognition during infancy


PART THREE - A Rearticulation ofAttachment Theory: The Goal-Corrected Partnership Revisited
CHAPTER SEVEN: The Goal-Corrected Partnership across the Lifespan

Man’s capacity to use language and symbols, his capacities to plan and build models, his capacities for long-lasting collaborations with others and for interminable strife, these make man what he is. All of these processes have their origin during the first three years of life, and all, moreover, are from the earliest days enlisted in the organisation of attachment behaviour. Is there, then, no more to say than this about developments in the organisation of attachment behaviour that occur during the second and third years of life? (Bowlby, 1969/1982, p. 358)

7.0 Chapter Introduction

In the 1969 1st Edition of Attachment (first edition remark is preserved in second edition, Bowlby, 1969/1982, p. 358), Bowlby hypothesised a formative role for cognitive contributions in the developmental organisation of attachment behaviours. As noted in Section 4.4 he described the ontogeny of Phase IV attachment as a Goal-Corrected Partnership (GCP), where explicit cognitive skills—including language—become integrated within attachment phenomena (Bowlby, 1969/1982, pp. 267-268, 356-358, 368-370). However, his slightly pessimistic answer to his question above—"Is there, then, no more to say..." as well as a recognition of "the continent of ignorance to be conquered" (1969/1982, p. 358)—remains surprisingly applicable today more than 45 years later. As noted previously in Chapter Three, Bowlby’s interests in cognitive development were consistent throughout his career and taken up in each of his major works (1969/1982, 1973, 1980). In the second edition of Attachment (1969/1982, p. 358), he readdressed the slightly rhetorical question of language and attachment. His original pessimistic response that brought attention to the “least studied phase of human development”—what he had termed “a continent of ignorance”—had given way to a recognition of “numerous and gifted explorers” whose work he takes up in a newly added Part V. As I note, nonetheless, focus on the specific notion of a GCP in early development continues to suffer despite his original call for greater attention.

Chapter Seven specifies three positions: it presents the GCP as the received position along with conceptual detail, identifies gaps between researchers in the employment of a GCP, and rearticulates s a possible lifespan expansion of the GCP. As an introduction, Section 7.1 describes what I defend as the revived position for the acceptance of a GCP, demonstrated in part by its presence across all periods of lifespan studies within the attachment field. Section 7.2 provides a formal depiction of the GCP (and by inference the accompanying cognitive/linguistic capacities). The discussion details the early conceptual evolution of the GCP both in Bowlby and Ainsworth’s student Marvin. However, as will be demonstrated in Section 7.3, the study of the GCP—especially the first GCPs emerging in Phase IV—has not
matched the enormous research effort devoted to the emergence of earlier infant attachment in Phase III. Possible reasons for this slight paradox will also be explored. Indeed, the subsequent section (Section 7.4) will highlight examples of discussions of attachment where the GCP has not been consistently taken up: although few formally reject the GCP, several significant researchers appear to fail to mention it. These omissions may nonetheless be identified in places as distinctly different versions, or as Thompson (2016) has suggested, mini-theories of Attachment Theory. One emerging finding here is the possible difference in an appropriately conceived relationship between attachment and intersubjectivity. The inconsistency in the take up and application of the GCP in some areas of Attachment Theory would appear to represent a potentially serious conceptual problem for the field. Equally, as the GCP may also offer conceptual support in assessing the impact of therapeutic relationship on therapeutic efficacy (Mikulincer, Shaver, & Berant, 2013), additional clarity would seem particularly valuable. Hence, Part Three’s ‘attachment rearticulation’ emerges in Section 7.4 where a more definitive updated dynamic alternative is provided, one that may also provide a possible bridge unifying some of the seemingly opposing positions depicted in Sections 7.2 and 7.4. In short, post-infancy attachment phenomena are depicted as capturing both the more implicit non-linguistic phenomena identified with Bowlby’s earlier Phase III attachment alongside the more explicit cognitively enhanced Phase IV phenomena. Finally, Section 7.6 concludes Chapter Seven pointing toward a reapplication of Bowlby’s TLC strategy in support of a new GCP articulation in Chapter Eight.

7.1 A received position status for the GCP

The acceptance of Bowlby’s proposals for a GCP can be arguably termed the received position in Attachment Theory. Indeed, the GCP has now been taken up in study across every lifespan period in attachment research, especially in the consideration of the emergence of new close peer/adult relationships (Zeifman & Hazan, 2016). I will briefly note discussion of the GCP in four developmental periods—early childhood/preschool, middle childhood, adolescence and adulthood—and in one lifespan survey of prosocial behaviour where GCP has also played an important conceptual role. A comprehensive review is neither possible nor required here for current purposes. However, developing a list of prominent GCP adherents is a relatively easy task and would also support this position here.

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233 A GCP would appear to be reasonably restricted to intra-species relationships. Nonetheless the attachment process and relationship differences, including in places underlying IWMs, have been extended in several interesting directions. Here are our non-personal relationships that have been studied in terms of both possible contributions to risks and benefits for mental health. These relationships—and accompanying measured security differences—include those with (1) pets (Walsh, 2009), (2) one’s own online avatars (Banks & Bowman, 2013), (3) homeland/nation (Ferenczi & Marshall, 2013), and (4) preferred sports teams (Dwyer, Mudrick, Greenhalgh, LeCrom, & Drayer, 2015). A terribly heterogeneous group that raises a range of philosophical issues as well! Keeley’s (2004) previous discussions on differentiating appropriate and inappropriate types of anthropomorphisms might be relevant here. Nonetheless, anything that might assist us in countering human loneliness, risk for intolerant nationalism and healthy capacities for distraction would be valuable.

234 The researchers who acknowledge and/or endorse Bowlby’s GCP in various fashions are undoubtedly the majority. The following partial list of key researchers might serve as a useful example: Ainsworth (1991), Crittenden (1992), Greenberg, Cicchetti, & Cummings (1990), George & Solomon (2008), Kobak & Duemmler (1994), Shaver &
7.1.1 GCP attachment in childhood

Others have also taken up consideration of first GCPs. Allen and Tan (2016) have likewise described this first GCP as *coordinated partnership*. The GCP notion—also described as *dyadic goal-corrected partnerships*—has also been included as a focus for treatment in Child-Parent Psychotherapy (CAP), a programme that treats children under-three years of age and their parents (see discussion in Berlin, Zeanah, & Lieberman, 2016). Mudrick’s (2016) recent dissertation trialling a new coding system that compares five and seven year olds parental representations of GCPs has potentially reinvigorated GCP research. She offers some innovative ideas seeking to identify differences in early GCPs. This work also straddles both pre-school and middle childhood periods.

Moving on from preschool, Kerns and colleagues (2016) highlight a developmental shift in child-parent attachments in middle childhood where the GCP shifts towards a type of *coregulation* of the secure base resulting in more *collaborative alliance* or what Waters and colleagues (1991) have also termed a *supervisory partnership*. The GCP has also been employed for the adolescence period. Allen and colleagues (2016) have noted how the GCP manifested in adolescent parental attachments shifts towards more of a *negotiated* sort of partnership, especially as new attachments—both peer and romantic versions of the GCP—emerge for the first time along with growth of greater personal autonomy needs.

7.1.2 GCP attachment in adulthood

The four phased development of attachment ontogeny has also been transposed as an analogous or expanded process applicable with adult romantic relationships (Zeifman & Hazan, 2016). Here is a reconstructed summary of their four-phased approach that redefines Bowlby’s childhood version:

- **Phase I**—‘preattachment’—is pictured as adult “flirtatious signals” and “playful sexually charged exchanges”.
- **Phase II**—attachment in the making—is described as the “throes of infatuation”.
- **Phase III**—‘clear-cut attachment’—sees the emergence of specific secure based attachment behaviours.
- **Phase IV**—‘goal corrected partnership’—may signal a decline in overt attachment behaviours to be replaced by exploration of more individual goals from an already secured base (Zeifman & Hazan, 2016).

Perhaps of interest here is the accumulating empirical study of the different phases of the adult model in terms of neural activity, sex hormones and neurotransmitters (see especially Fisher, Aron, Mashek, Li, & Brown, 2002). However, such a description would hardly work for peer and non-romantic attachment relationships. More conceptual work is clearly required.

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7.1.3 GCP and Lifespan Pro-social Behaviour

Finally, note should also be made of the employment of the GCP within recent studies of the role of attachment across the full lifespan, in this case the influence of attachment experience on prosocial behaviour (Shaver, Mikulincer, Gross, Stern, & Cassidy, 2016). These researchers identify the emergence of a goal-corrected partnership along with advances in emotion regulation (Construct 6 in Section 4.3) in early childhood, acknowledging that such regulatory capacities grow as mediators in care for others.

7.1.4 GCP Lifespan Terminology

Although not always consistent or with equal emphasis, the GCP appears across the study of the full lifespan of attachment. To conclude this brief review of a possible received view, researchers cited above studying post-Phase III Attachment have differentiated several age specific asymmetric GCPs: ‘early emergent’, ‘initial goal corrected’, ‘coordinated’, ‘supervisory’, ‘collaborative’ and ‘negotiated’ forms of the GCP as children grow into adolescence. The GCP as discussed within more symmetrical attachment relationships have replaced the asymmetric qualifiers with peer and romantic as seen in adulthood. The evidence cited suggests a received position for the GCP in Attachment Theory. With this general picture in place, the details of a GCP can be taken up.

7.2 Attachment as a Goal-Corrected Partnership (GCP): Bowlby’s Less Noticed Ideas

7.2.1 Attachment and Cognition: Preliminary Remarks

At least two overlapping relatively open sub-questions may be profitably identified from Bowlby’s query above about post-infancy attachment phenomena: (1) What might be the influence of attachment on developing cognition, and (2) What might be the role of developing cognition for the ongoing development of post-infancy attachment behaviour? This second ontogenetic question covering the GCP in Phase IV attachment will serve as the focus for Part Three of the thesis.

The first sub-query concerning the influence of attachment on cognition is a relatively straightforward empirical matter, taken up in longitudinal studies of the previously identified functional Construct 8 attachment outcomes (for a review of the major attachment longitudinal studies see Grossmann, Grossmann, & Waters, 2005). However, the possible influence of

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235 All attachment relationships include the exercise of caregiving by one or both partners depending on developmental period in the lifespan. However, in childhood attachments with adults the caregiving direction is one-directional and therefore asymmetrical. At the same time with age and maturity comes a declining degree of need for care, though attachment security/autonomy always includes some degree caretaking at all stages in life. Interestingly, recent studies of aging unwell adults and their adult children have focused on attachment relationships and their contribution to health. As the older parent becomes more dependent upon adult children, we may also see the emergence of new reversed asymmetrical attachment relationships (Merz, Consedine, Schulze, & Schuengel, 2009).

236 Hinde—Bowlby’s informal ethological tutor discussed in Chapter Three—obviously has endorsed attachment approach. He has also made extensive contributions to the field of human relationship development (Hinde, 1997). However, although his ideas on the complex development of a range of human relationships would marry well with discussions in Part Three of the thesis, I am unaware of his engagement with the GCP discussion.
attachment on cognitive development remains less resolved. On the one hand, attachment research has from the outset been more closely associated with demonstration of social and emotional developmental sequelae (Thompson, 2008, 2016). Not surprisingly, cognitive development has been considered to be less relevant as an outcome for attachment (Sroufe, 1988, 2016). Indeed, such views have been supported by studies that have typically shown little correlation between early IQ and attachment security (Van IJzendoorn, Dijkstra, & Bus, 1995). On the other hand, smaller but expanding research has more recently discovered important connections between early and ongoing attachment experience and subsequent cognitive development, especially in terms of school performance (Kerns, 2016). Indeed, more recent studies have also surprisingly uncovered correlative relationships between attachment security and IQ (West, Mathews, & Kerns, 2013). In a more speculative manner, Fonagy & Luyten (2009) have also suggested that cognitive capacities associated with Theory of Mind (TOM) emerge from early attachment, a perspective they describe as an inversion of the traditional theory. In terms of this first sub-query, the breadth of cognitively focussed attachment outcome studies fall outside the present chapter’s conceptual focus captured in the second question.

If the first sub-query considers the influence of attachment on cognitive development, the second sub-question reverses the developmental sequel. Here conceptual interest turns to the role of cognition on and within attachment phenomena: what is the relationship between cognition and attachment? Is it merely an influence or is better considered a constitutive property? I will argue, as have others, for a necessary role for cognition in attachment phenomena, particularly as captured within the GCP concept. Chapter Seven will elaborate this initial position. I suggest that the GCP in Attachment Theory might better be considered a lynchpin for understanding a lifespan theory, ongoing resilience and underlying mechanisms or IWMs. Consequently, theorists who either reject the GCP or downplay cognitive influences through omission of a GCP risk ignoring both important conceptual argumentation and empirical evidence that reflect both cognitive and affective aspects of attachment phenomena across the lifespan. Chapter Eight will provide the detailed argument for this position via an elaboration of attachment depicted as both phylogenetic and developmental homologies respectively, notions raised previously in Section 4.3. How might cognition be conceived within the GCP?

7.2.2 Bowlby’s First Definitions of a Goal Corrected Partnership (GCP)

As noted in Section 4.3 (see especially Figure 4.1), Bowlby posited four phases of attachment development, the latter two being formally constitutive for attachment development (Bowlby, 1969/1982, pp. 265-268). On the one hand, most of his theoretical discussion in his three volumes—Attachment (1969/1982), Separation (1973) and Loss (1980)—focused predominantly on the details surrounding the emergence, maintenance and risk related to infant ties, i.e., his Phase III discussion goal-corrected behaviours that sought to sustain
security. Nonetheless, his Phase IV GCP has been consistently acknowledged throughout his work. He noted:

By observing her behaviour and what influences it, a child comes to infer something of his mother’s set-goals and something of the plans she is adopting to achieve them . . . a child is acquiring insight into his mother’s feelings and motives. Once that is so the groundwork is laid for the pair to develop a much more complex relationship with each other, one that I term a partnership (Bowlby, 1969/1982, pp. 267-268).

In addition, the child is seen to be able to increasingly tolerate greater distance and eventually periods of separation from the caregiver, retaining an expanded more psychological sense of what Sroufe had termed felt security (again see Section 4.3). Mudrick has recently noted, “During early childhood, children begin to assert their autonomy in new ways and parents respond with varying levels of limit setting and sensitivity. As dyads learn to balance children’s self-assertions with parental containment, goal-corrected partnerships (GCP) are theorized to emerge” (2016, p. 1) To recap, what is manifested initially as physical proximity in Phase III, can now be captured by a notion of psychological availability within a GCP.

Bowlby’s suggestion of phased attachment development provided greater granularity for considering pre-attachment and attachment development; hence, both regularities and differences in the nature of interactions, selection of figures and timing could be explored over time. In discussing phases, he also noted “it must be recognised that there are no sharp boundaries between them” (Bowlby, 1969/1982, p. 266). Indeed the timing for these proposed phases remain less certain (van Rosmalen et al., 2014), for both the onset—possibly in the fourth year—and conclusion— from the fifth year or beyond (for a recent discussion of the research see Mudrick, 2016, pp. 1-5). Indeed, I argue in Section 7.4 that GCP development should be amended to include an extended temporal dimension: as an ongoing lifespan development. Early infant relational interactions, or transactions, in Phase I and II are vibrant and grow increasingly more complex with time. However, these initial phases give way in Phase III (around six months of age) to what may be termed attachment proper where the attainment of set goals—i.e. physical proximity with a preferred caregiver(s) for security—are accomplished via goal corrected proximity behaviours. As noted, beginning sometime in the third, or more likely in the fourth year, Phase IV commences, resulting in the increasingly more collaborative goal corrected partnership.

7.2.3 Supporting Developmental Observations

To refresh from Section 4.3, underpinning these expanding attachment ties (Construct 4), are antecedent causal transactions (Construct 2) and the more varied attachment patterns (Construct 3). Bowlby’s approach to the emergence of a GCP also relied upon at least five developmental observations: one intersubjective (again see Chapter Two for background to

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237 See both the description of psychoanalytic intersubjectivity research in Chapter Two and the discussion of intersubjectivity and the GCP below.

238 ‘Goal-corrected’ captured Bowlby’s position whereby learned patterns of behaviours and interactions might become ‘organised’, permitting multiple and more complex ways to achieve a ‘set goal’.

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the study of this phenomena), the remaining four more explicitly cognitive in nature. These observations point to the concurrent interpersonal, social, affective and cognitive influences within attachment phenomena.

7.2.3.1 Forms of Intersubjectivity

Attachment considerations have, from the outset, been intertwined with developmental studies of intersubjectivity. With few exceptions attachment and intersubjectivity have generally been envisioned as mutually embracing phenomena (Diamond & Marrone, 2003), although Lyons-Ruth (2007) has indicated how greater integration of the latter into the former is needed. When considering the pair, at least two challenges must be acknowledged: one definitional the other relational-conceptual. First, as with many developmental concepts, intersubjectivity has been interpreted diversely. The psychoanalytically inclined researchers Beebe & colleagues have noted “the term intersubjectivity has no single, coherent meaning either in psychoanalysis or in infant research. As a solution to this problem we recommend adoption of the concept of forms of intersubjectivity” (2003, p. 746). Rochat and colleague’s (2009) three-fold approach to intersubjectivity may count as an example of differentiated forms of intersubjectivity: primary, secondary and tertiary. The differences will be articulated below. This tripartite division may also hold value for consideration of a GCP as it encompasses all four phases of Bowlby’s (1969/1982) initial proposals for attachment ontogeny.

A second issue concerns the possible conceptual relationship between attachment and various forms of intersubjectivity. Despite the acknowledged embrace between the two, there is less consensus or clarity on how to conceive this relationship. In short, attachment research might be best seen as a subset within broader intersubjectivity research (Beebe et al., 2003) whereby intersubjective capacities can be identified as constituent properties of attachment relationships and broader attachment regulatory phenomena (Lyons-Ruth, 2007).

On the one hand Bowlby (Bowlby, 1969/1982) recognised and endorsed the work of psychoanalytic developmental intersubjective researchers discussed in Chapter Two (for example, Sander, 1962; Stern, 1977) as contributing to relevant descriptions of both pre-attachment Phase I / II and Phase III attachment. On the other hand, interesting expansions of intersubjective conceptions quite relevant for attachment have also emerged post-Bowlby. For example, early implicit non-verbal affective reciprocal interactions—what are referred to as examples of primary intersubjectivity (Trevarthen, 1979)—are clearly recognisable in Phase I and especially Phase II in attachment ontogeny (1969/1982, pp. 265-298). Indeed, these types of transactions have been shown to be predictive of early attachment (Beebe et al., 2010). By Phase III these evolving interactions expand to include joint attention—secondary intersubjectivity (Trevarthen & Hubley, 1978)—and reflect the emergence of the infant’s set goal of security—proximity maintenance (Bowlby, 1969/1982, pp. 299-330). Phase IV phenomena may also be captured by post-symbolic intersubjectivity captured by the
notion of tertiary (Rochat et al., 2009) or reflective intersubjectivity (Martin, Sokol, & Elfers, 2008). Rochat & colleagues describe this third form as “an ontogenetically new process of value negotiation and mutual recognition that are the cardinal trademarks of human sociality” (2009, p. 173). Such a description mirrors well Bowlby’s GCP notion as described in the lifespan research below. I will therefore propose throughout Part Three of the thesis that dyadic transactions emerging first in pre-attachment intersubjectivity are taken up into attachment specific interactions throughout Phase III and Phase IV and beyond. As such, these interactions should be seen as constitutive parts of attachment ontogenetic phenomena. Attachment therefore would be seen as specific subset of intersubjectivity.

7.2.3.2 Four cognitive phenomena adopted from Piaget

Next, at least four cognitive phenomena, grounded particularly in studies of by Piaget (1954, 1963, 2001), also underpin the proposal for a GCP. Briefly, these are: (1) conceptual perspective taking (Bowlby, 1969/1982, pp. 368-370), (2) object permanence (Bowlby, 1973, pp. 51-55, 425-433), (3) requirement for assimilation and expansion of developing mental working models (Bowlby, 1973, pp. 203-210), and (4) employment of advanced cognitive skills including language acquisition that enable more complex communication, internal modelling and planning. These last ideas are captured in the opening epigraph above (for his acknowledgement of Piaget here, see Bowlby, 1969/1982, pp. 350-351; 1980, pp. 229-232).

7.2.4 Marvin’s Research on GCP Attachment in Early Childhood

The task of exploring Phase IV fell to Ainsworth’s student Marvin and his colleagues. The first studies of the GCP took place in the context of the immediate post-Phase III development—as opposed to other periods in the lifespan. His first explicit research on Phase IV Attachment recognised the important gains in behavioural research and ethology in illuminating earliest attachment, but suggested, “In view of these advances, it is curious that we know very little about this interpersonal relationship beyond the first year or 18 months of life” (Marvin, 1977, p. 26). As shown below, this view would persist into the first decade of the 2000s.

Marvin’s initial work helped to demonstrate how separation distress modifies and lessens with the onset of cognitive skills. This allows for the emergence of a partnership where appropriate behaviour inhibition can be accompanied by internal perspective taking. However, writing at a time before Bowlby’s work (1980) on IWMs had been fully formulated, Marvin was less clear about the attachment constructs at Phase III and Phase IV:

The question as to whether or not the partnership should be considered an attachment is more difficult. Certainly it is part of the close, loving relationship between mother and child, and therefore considered an attachment. However, the term attachment has come to refer to a relationship that is based in physical proximity and contact, and since the partnership refers to a relationship that is based on a balance of internal perspectives, perhaps the two should be considered as being in some sense independent (1977, p. 56).
In follow-up study (Marvin & Greenberg, 1982) and in his later work on the ontogeny of attachment, Marvin had taken on board Bowlby’s IWMs approach. He is also clearer in accepting a possible dynamic transformation from Phase III to Phase IV (Marvin & Britner, 1999). However, his recognition of the need to “understand more”, first acknowledged in 1977, would seem to have stubbornly remained for the next three decades.²³⁹ Nonetheless, Marvin and colleagues subsequent discussions of attachment ontogeny in the three editions of the Attachment Handbook (1999, 2008; 2016) have provided helpful detail and periodic updates on relevant research for the GCP in the post-toddler, preschool years. Indeed, they have probably been the most active contributors to discussion and research on the emergence of a GCP. Their inputs remain somewhat rare, as fewer discussions of GCP in Phase IV development would appear to have been engaged. They have notably differentiated between an initial emergent partnership for younger pre-schoolers and the more sophisticated form of a full goal corrected partnership for older pre-schoolers (Marvin et al., 2016). This differentiation acknowledges expanding cognitive skills possessed by pre-schoolers.

Previously and relatively early on in the development of the theory, Marvin (1977) had described five component cognitive skills related to the operation of supportive IWMs present for most children by age four:

1. the ability to recognise that the attachment figure possesses internal events including thoughts, goals, plans, feelings and so forth;
2. the ability to distinguish between the caregiver’s point of view and one’s own, especially when they differ;
3. the ability to infer from logic and/or experience, what factors control the caregivers goals and plans;
4. the ability to assess the degree of coordination, or match, between their respective points of view; and
5. the ability to influence the caregiver’s goals and plans in a goal-corrected manner (as cited in Marvin & Britner, 1999, p. 61).

These five line up with the cognitive observations noted for Bowlby above. However, what had not yet emerged in Bowlby’s day—or nor in 1977 when Marvin identified his list of cognitive skills at work in early attachment—were formal discussions of TOM in cognitive science.²⁴⁰ Bowlby (1969/1982, p. 370) had indeed anticipated a possible connection between Premack & Woodruff’s (1978) early seminal work with cognitive ethology and human cognitive development. Attachment has in places been considered in light of TOM capacities (Hughes & Leekam, 2004) perhaps as an overlapping construct (Fonagy, 1997) or more interestingly...

²³⁹ “Stubborn” is applied here in terms of ongoing noticeable gaps in theory. Where virtually every chapter of Second Edition of The Handbook of Attachment had been changed in some fashion, “to alter the chapter content and structure vigorously where needed to convey what was changed over the past decade” (Cassidy & Shaver, 2008, p. xii), the near word for word reproduction of Marvin and Britner’s work and the identification of few new resources aimed at Phase IV discussion may raise the question of why so little interest. Neither Fonagy nor Crittenden were mentioned in their second edition chapter. Indeed, perhaps it is less a question of interest than adequate identification of other contributions. But perhaps in the spirit of fairness, it should be also noted that Attachment Theory has from the outset been designed with clinical application in mind. Much of Marvin and colleagues work has focussed successfully on the development of clinical interventions (Hoffman, Marvin, Cooper, & Powell, 2006) as well as provision of forensic support (Marvin, Schutz, & Benjamin, 2009).

²⁴⁰ In the introduction to their edited book Theories of Theory of Mind, Caruthers and Smith (1996, pp. 1-4), date the full emergence of theory of mind into philosophy of mind at 1992, two years after Bowlby’s death.
as a possible explanation of difference in development of TOM (Laranjo, Bernier, Meins, & Carlson, 2010). Unfortunately, the notion of a GCP is typically not taken up in these discussions and attachment seems to be conceptualised as a past Phase III only activity. For an exception see Marchetti & Massaro (2009). As Beebe & colleagues have reinforced, “there is extensive evidence and agreement that not until a period distinctly later than infancy do children develop the capacities necessary for grasping that minds are perspectival and subject to error” (2003). I will suggest that attachment as a post infancy GCP must require a certain subset of TOM capacities. Godfrey-Smith’s differentiation of TOM notions may also be helpful here: he delineates the more technical employment of TOM in cognitive psychology—within the theory-theory or theory-simulation debate focussed on an ability to pass a false belief test—and an alternative folk psychology as TOM, “as the basis—whatever it is—of our ability to describe, interpret, and predict each other by attributing beliefs, desires, hopes, feelings, and other familiar mental states” (2005). Attachment post-GCP would both require the more technical skill, but as discussions of mechanisms in Chapter Eight will indicate, mental models would appear to be dynamically upgraded with experience throughout life.

In the first two editions of the Handbook of Attachment Marvin & Britner suggest that the “goal corrected partnership needs much research. Very little is known except for the general construct, its early development, some implications regarding its relations with self-concept and academic performance during middle childhood and its importance for the relationship between adolescents and parents” (Marvin & Britner, 1999, pp. 62-63; 2008, p. 288). This remark and much of their previous recognition of minimal research beyond this period has been withdrawn in the third edition (2016), in part superseded by advances across the lifespan. Although space unfortunately does not permit a thorough inclusion of her work, Crittenden’s (Crittenden & Liandini, 2011) lifespan attachment model, which also takes up the valuable gains in Main’s AAI work, develops an ongoing perspective based on an optimal integration of both cognition and emotion, sexuality and safety (for a summary see Farnfield & Stokowy, 2014). I turn now to an acknowledgment of the paradoxical character of the GCP.

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241 Crittenden made similar observations, “It is ironic that, although the great majority of attachment research has been carried out by developmental psychologists, the theory as practiced is substantially less developmental than most other developmental theories. That is attachment researchers have paid more attention to the validation and temporal extensions of the infant patterns of individual differences than to the interactive effects of maturation and experience on organisation of attachment beyond infancy” (Crittenden, 2000, p. 6).

242 Crittenden’s interest in expanding capacities for both emotion and cognition fit nicely with my suggestions for the DLPC in Chapter Six and the expanded GCP here. Her proposed lifespan measures and accompanying findings have not yet received the degree of peer exposure of others, possibly because of the lifespan notion. As noted previously, her work has been taken up in the new Routledge Handbook series (P. Holmes & Farnfield, 2014a, 2014b, 2014c).
7.3 The Paradoxical Neglect of the GCP within Developmental Discussions

7.3.1 The GCP Paradox

After having just established a case for theoretical reception of the GCP in Attachment Theory, its paradoxical omission must also be acknowledged. In the epilogue to an important 1990 edited volume, Attachment in the Preschool Years, Ainsworth reflected notable gaps in the take up of a few of Bowlby’s foundational ideas across the volume.243 She concluded with the observation that “few contributors . . . emphasize the development of goal corrected partnership [something] essential to Bowlby’s view” (1990, p. 464). Such a gap would seem even more notable when considering the edited volume is devoted to the very post-infancy Phase IV period that headlines the studies (M. T. Greenberg et al., 1990). A similar lacunae can also be found the 1991 edited volume Attachment across the Lifespan (Parkes, Hinde, & Marris), where Ainsworth’s contribution is the only one to mention the GCP.244

More than 25 years on, the situation with the study of early GCP has shifted, but perhaps only slightly. As Murdick recently notes: “Empirical evidence on the emergence and development of GCP is limited” (2016, p. Abstract). Indeed a quick glance at the subject index in the Third Edition of The Handbook of Attachment (Cassidy & Shaver, 2016) suggest a relatively sparse engagement with the concept.245 Marvin and colleagues (Marvin & Britner, 1999, 2008; Marvin et al., 2016) three useful versions of a primary chapter on attachment ontogeny in the Handbook series might also suggest the situation has remained chronically unchanged. Despite their summary of relevant work on attachment ontogeny, they have also consistently acknowledged a perplexing move away from early attachment development in post-infancy developmental research (or post Phase III) resulting in an imbalance between Phase III and Phase IV study.246

7.3.2 Possible Reasons for Evolution of this Paradox

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243 Ainsworth was reflecting on the edited volumes chapters in light of Cicchetti and colleagues (1990) introductory article to the volume. They had identified six core issues—one of which was the GCP—requiring consideration in developing a framework for studying attachment beyond infancy. Ainsworth notes the relative neglect of GCP in the volume.

244 Indeed, the subject index to the volume mislabels Ainsworth’s references as a Goal-Connected Partnership.

245 The subject index in the Handbook (Cassidy & Shaver, 2016, p. 1040 loc. 45593) notes only 11 places of discussion of the GCP across 14 pages in a text that numbers well in excess of 1000 pages.

246 A closer look at their work is also illuminating of the severity of the gap in Phase IV research. The latest study they cite as specifically highlighting the importance of the GCP dates only from 1992 (Marvin & Britner, 1999, 2008; Marvin et al., 2016). Equally, the much revised most recent chapter (Marvin et al., 2016) incorporates highlights from insightful studies on cognitive capacities at play throughout ontogeny. However, in terms of GCPs, this latest version makes reference to only three additional (but nonetheless helpful) attachment studies, relevant for early childhood. For example, a Ziv and colleagues (2004) study looked at infant attachment and social information processing in later middle childhood but without mention of a GCP. Hughes & Leekam (2004) enquiry looked at attachment development and theory of mind and included a notion of “joint goal-directed activity”, but made no mention of the sort of interaction of distinctive individual goals within a GCP. However, a 2013 study on the delayed gratification and attachment security does make specific reference to “component skills that allow the child and caregiver both to take responsibility, when their goals or plans conflict, in negotiating in a goal-corrected way toward a shared set of plans” (Mittal, Russell, Britner, & Peake, 2013, p. 481). What the paucity of newly cited material would seem to indicate is the ongoing relative lack of focus on the study of emergence of an attachment GCP.
Several reasons can be identified that might help explain this slightly odd imbalanced situation. I will note eight dividing them into two categories: (1) organic research focus and (2) misunderstanding or omission.

7.3.2.1 Paradox as a Consequence of Organic Development of the Field

Four possible explanations for the GCP paradox may be described as consequences of the organic historical unfolding of the general field of Attachment Theory. First, Chapter Five suggested that a clinical focus on prevention and intervention in Phase III might explain some of the lower interest in GCP. Indeed, Waters and Cummings (2000) have equally suggested a second influence: part of the issue may revolve around Bowlby’s possible theoretical overemphasis on earliest attachment. However, as noted above, Bowlby also readily acknowledged the meagre state of Phase IV research. Perhaps had both he and Ainsworth been younger when they introduced Attachment Theory we might have had a stronger emphasis on the GCP today. Nonetheless, a variant of this second notion might also yield a third reason. The emergence of a comprehensive set of measures for a GCP across the lifespan remains only partial. As noted in Chapter Five, from a simple historical point of view, the development of attachment measures emerged in an organic fashion reflecting the growth of interest in specific lifespan period. Focus went from Phase III to adulthood with the various other periods emerging thereafter. Finally, Marvin and colleagues (2016)—probably the leading Phase IV GCP researchers—offer two additional considerations. One applies in this subsection. They have suggested how preschool developmental study has also tended to shift focus from the study of developmental process (which would include the GCP) to research on the accomplishment of particular early developmental tasks—e.g. autonomy, self-control, independence and socialisation. These reasons suggest how today’s more mature field may be in a better position to embrace a lifespan notion of a GCP.

7.3.2.2 Paradox as a Consequence of Misunderstanding or Omission

A final set of approaches to the paradox identifies possible misunderstandings and omissions. First, Marvin and colleagues (2016) also note how the investigative neglect might also be due in part to a possible inaccurate devaluation of attachment in the pre-school period as somehow representing dependency—something that would be excluded in many approaches to early autonomy. Second, Chapter Three also hypothesised that many researchers may have been less familiar with Bowlby’s deeper theoretical work. This will be suggested to be the case for Cortina & Liotti (2010a). Such examples also point to a third research consideration. The GCP is virtually absent from a few influential researchers, especially Main’s representational work. Researchers may often rely on key accounts of attachment by other investigators. What may be of central importance here may also be the degree to which these researchers might also rely on a version of the ‘continuity hypothesis’ as discussed previously in Section 4.3 and 6.1. As I will also acknowledge below, this particular situation remains the source of confusion if not incoherence for Attachment Theory.
One last consideration emerges when Bowlby’s TLC strategy is brought back into view. Gilath, Karantzazas and Fraley have recently described Attachment Theory as a ‘grand theory’ also noting its connection to the Tinbergen tradition: “Attachment theory is one of only a few contemporary theories in social and personality psychology to answer ‘why’ questions from multiple timescales and perspectives” (2016, p. loc. 965). Equally, few researchers would seem to be able to match Bowlby’s fluency and breadth of current knowledge in fields such as philosophy and both the cognitive and affective sciences. Indeed, this last point was one of the key findings in Chapter Three. Practically speaking, thorough research must probably focus on only a few matters at a time, and more likely on only one or two of those ‘Why?’ questions. Consequently, the GCP may have also eluded many discussions in part due to the sheer breadth of TLC perspective and research requirements to choose a focus. Bowlby’s TLC strategy, a modification of Tinbergen’s four questions, requires regular attention to each of the four realms. This also suggests important overlaps between the GCP and with other realms might also need to be acknowledged.

### 7.3.3 Inevitable Conceptual Overlaps with other Attachment Constructs

One additional consideration might also help. Researchers may be interested less in questions of ontogeny than other realms of explanation. Perhaps they may also presume that the ontogeny question has been resolved and therefore direct their energies elsewhere. Indeed, an explanation of the GCP, an ontogenetic sub-construct of attachment ties, is also better explained via an engagement with at least three additional constructs identified in Chapter Four from different Tinbergen realms of explanation: the phylogenetic attachment behavioural system (Construct 1), functional risk (Construct 10), and the cognitive/affective mechanisms or IWMs (Construct 11). First, consideration of the concept of a GCP would appear to raise the question of what counts as the unique evolution of the human attachment system, especially as regards intersubjective and linguistic considerations. How might attachment be considered relative to these independent phenomena? This will be the focus of the rest of Chapter Seven. Equally, both Phase IV development and the ongoing lifespan expansion of operations of the GCP require some consideration of how experience might be internalised and therefore impact the shape of future attachment phenomena. As attachment has also been identified as a functional resilience buffer construct, the loss or underdevelopment of the cognitive capacities associated with GCP would conceivably be associated with attachment risk. Finally, the role of developing internal working models and considerations of accompanying cognitive/affective capacities subsumed within their operations will also require a degree of clarification. Therefore, each of Tinbergen’s realms can be seen to be at play here in an explanation of a GCP: phylogeny ontogeny, function and

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247 For the purpose of this thesis, I will assume a relatively unique position for human attachment informed by the more integrated cognitive-affective capacities at play in attachment phenomena. This position follows those in emotion research (Colombetti, 2013; Pessoa, 2013) that have demonstrated how the traditional sharp cognitive-emotion divide is empirically less sustainable. Crittenden’s (Crittenden & Clausen, 2000) proposals for attachment highlight the developmental integration of affect and cognition but do not take the more constitutive position found amongst emotion researchers. As indicated in Chapter Two, I also do not reject cognitive ethology and will raise its place in Chapter Seven’s phylogenetic enquiry into the GCP.
mechanism. As noted in Chapter Two, Bateson & Laland (2013) have also reinforced Tinbergen’s (1963) original insight that it is the very overlaps between the four realms that may prove the most informative for biological research. I now turn to researchers who do not recognise a GCP.

7.4 The Emergence of Conceptual Divergence: Alternative Status of an Attachment GCP and its place in a few key Versions of Attachment Theory

7.4.1 Attachment without a Phase IV Corrected Partnership: Initial definition of Key Researchers

The work of five highly productive and well regarded attachment researchers would appear to have made no mention in their work of a GCP, nor a Phase IV developmental period post infancy. These five are Rutter, Sroufe, Main, Fonagy and Liotti. The five have all played valuable roles in Attachment Theory, though the more clinically focussed Liotti may be potentially less recognisable. Their work and possible implications for a GCP will be the focus of this subsection. The analysis will also help to shape the arguments for a re-articulation in Section 7.5. The primary questions addressed here ask, (1) What can be made of the lack of take up amongst the five, especially as I have argued that the GCP might serve as the lynchpin for the lifespan dimension in Attachment Theory? More specifically, (2) What might this possible theoretical gap mean in the unique context of each writer’s attachment perspective and possible stated interest in cognitive capacities for attachment? Equally, the endorsement of the GCP so far might also risk wrongly suggesting that somehow, (1) Bowlby’s and Ainsworth’s ideas must be accepted as some sort of founding orthodoxy for attachment, or (2) that other viable alternative conceptual models do not require consideration. These possible alternatives, or mini-theories, that lack a GCP, nonetheless merit consideration.

I will argue here that the general theoretical concerns with the first two writers—Rutter and Sroufe—might best be understood as representing an implicit endorsement/acceptance of a GCP. Next, the work of Main and Fonagy, which share significant interest in both early cognitive development (representation, IWMs and TOM) and the more cognitively structured

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248 I am unaware of any direct explicit rejection of a GCP other than the previously mentioned citation of Kenny (2013, loc. 3198) who mistakenly attributes the GCP to classification of Bowlby’s ideas by Marvin & Britner. As it reads, her treatment of the GCP is also confusing and incomplete; it appears to be considered as part of Phase III description. There is no identification of the Phase IV, i.e. what is might be, no timing and no description. This may be due to a possible deletion of a portion of her original text.

249 The corpus of work for these five individuals is quite immense. A complete and comprehensive reading is beyond the brief of this section in an overall thesis. However, I have surveyed significant portion of these writers attachment work along with on-line searches of GCP and Phase IV against their names. The results here comprise a directionally accurate reading. As such, if offers valuable conceptual input for expansion of the GCP. Mudrick’s focused look in her dissertation on early GCPs at the relatively sparse attachment GCP research (Mudrick, 2016, pp. 6-8) does not include any of the five scholars named here. However, she does note a role of both Main’s AAI narrative insights and Fonagy’s metallization in her development of a GCP measure for young children.

250 Unlike Bowlby, none of these five researchers have worked alone. So the work of their numerous collaborators should not be minimized in my selection of these figures. This may especially be the case for Liotti whose work with Cortina is perhaps better seen as shared perspective.

251 Indeed, Rutter and Sroufe might be expected to show considerable overlap and agreement. They have collaborated on conceptual discussions of both attachment within the discipline of developmental psychopathology (Rutter & Sroufe, 2000; Sroufe & Rutter, 1984).
Adult Attachment Interview (AAI), is more difficult to assess. I will suggest that Main and colleague’s representational work is more perplexingly unclassifiable as regards the GCP. (Because her work has been both formative and influential for Attachment Theory, I will devote additional space for discussion of my position as regard her contributions.) As for Fonagy and colleagues, I propose that their discussions on the development of reflective capacities and metallisation in youngsters might be seen as reflecting the GCP attachment phenomena. Hence, the two concepts might be mutually subsumable one into the other. These metacognitive capacities have also been taken up in the study of intersubjectivity, a conception upon which Liotti and his collaborator Cortina have approached attachment. They differentiate the traditional Phase III activities from what they see as distinct intersubjective phenomena. So in contrast to the other writers, Liotti and Cortina may be considered an implicit exclusion/rejection of Bowlby’s GCP. I will summarise discussion and findings with a comparison in Table 7.1.

As noted above, Bowlby’s 1982 2nd edition of Attachment updated his views on GCP. However, his last published collection (Bowlby, 1988) of talks and chapters mostly from the early 1980s—between 1979 and 1985 bar one late 1988 exception—and published in 1988 make no mention of the GCP. Clearly, mention of a GCP does not serve as some sort of litmus test for theoretical purity. Equally, possible differences in position from Bowlby did not prevent his appreciation of their unique contributions. First, Rutter’s critical influence on attachment along with Bowlby’s open reception has been highlighted in Chapter Five. Indeed, Bowlby (1988) also clearly endorsed the valuable contributions to Attachment Theory by both Sroufe and Main in his writings and also made positive reference to work by Liotti. Finally, Fonagy’s attachment work arrived predominantly after Bowlby’s death in 1990 and Ainsworth’s retirement.

7.4.2 Clarification of Five Alternative Positions and Possible Relationships to GCP

Rutter: Rutter (2006) has long argued for more complex approaches to discerning attachments and their attendant outcomes. Equally, his research with early pre-attachment institutional deprivation (Rutter & English Romanian Adoptees Study Team, 1998)—and that of a host of others (see Nelson, Fox, & Zeanah, 2014)—has focused on pathological impact, especially the disinhibited attachment disorder acknowledged in Chapter Four. His work has also raised questions about a possible early point in development beyond which the formation

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252 Waters and colleagues have also suggested: “Problems inherent in the goal-corrected partnership concept may, in part, account for the fact that it receives less attention in the second and third volumes of Bowlby’s attachment series and has not been the starting point for recent advances in attachment theory and assessment” (1991, p. 8).

253 Finally, mention of the tone of the discussion here may be useful. On the one hand, note has already been made in Section 4.2 and Chapter Five, that support for either a continuity or related sensitive period positions is not empirically merited for Attachment Theory. These two points have also been the subject of disputes—sometimes bitter, often controversial—both within and without Attachment Theory. Indeed, three of these writers probably embrace these positions. My interest here is not to re-engage historical disputes but suggest a possible way beyond. So delving a bit deeper is necessary but can also be done without the baggage.
of attachments may be increasingly difficult. Bearing in mind the non-exhaustive study of the scope of my survey, one may still enquire, why might such an influential researcher avoid the GCP? A closer look at his work suggests a possible omission of the GCP concept might nonetheless be less problematic. Rutter is quite clear, in suggesting that an array of cognitive phenomena are most likely active in later romantic relationships—though clarification and ability to measure remain somewhat primitive. He has asked, “What are the attachment elements in adult love relationships (query confiding, self-reflection, and commitment)” (2006, p. 976)? Such ‘elements’ would seem to argue that the cognitively enabled GCP might fit well within his interests for Attachment Theory. I suggest that his work counts as an implicit acceptance of the GCP concept.

Sroufe: It came as somewhat of a surprise to find no mention of either a GCP or Phase IV attachment in Sroufe’s work. As noted in Section 4.3 and Chapter Six, his and Waters’ (1977) influential organisational construct offered a conceptual path away from inadequate employments of sensitive periods and simplistic continuity assumptions for early Phase III attachments. Indeed, Sroufe & colleagues’ (2005a, 2005b) longitudinal work has counted as one amongst many valuable prospective studies (Grossmann et al., 2005) that have demonstrated that both earliest attachment experience and later ongoing experiences impact both attachments and personality development. Sroufe & colleagues (2005a) discuss development in terms adaptation, within which attachment and other relevant experiences are elucidated. 254 Their post infancy toddler period focuses on Guided self-regulation whilst the following preschool period Emergence of coherent personality (Sroufe et al., 2005a, pp. 106-120, 121-147). The contents of both chapters include the sorts of collaborative and cognitive tasks elucidated above for GCP activities. However, with a strong focus on emotional development (Sroufe, 1997a), Sroufe has typically seen cognition as less influential for attachment, something noted at the beginning of this chapter. Lastly, it is also worth noting that Waters (Waters & Cummings, 2000), Sroufe’s co-partner in developing the organisational construct, has acknowledged Bowlby’s GCP as a component in their conceptual discussion. However, they (Waters et al., 1991) have also expressed concern that the GCP may have been inadequate for lifespan phenomena. (See also their criticism and my suggested expansions below in Section 7.4.) Sroufe may have shared these concerns. However, as with Rutter, I suggest here that the notion of an attachment GCP nonetheless would fit within Sroufe’s work on personality development. However, this consistent or “fit in with” does not count as an implicit endorsement/acceptance of some specific form of a GCP.

254 Sroufe (1997b) has elsewhere devoted considerable efforts to the exploration emotional development, something clearly present within this longitudinal study. Sroufe’s longitudinal study was particularly interested in the impact of early attachment experience in a low SES cohort. Their study also preceded many of the later gains in developing GCP measures for the full lifespan.
Main: Main would also appear not to have directly employed the GCP notion in her work. However, identifying an indirect position beyond this omission may be less obvious. I will argue that her position is best described as unclassifiable.

On the one hand as indicated above, Waters and colleague noted that Main and colleagues “developed their theory of adult working models from the secure base concept rather than the goal-corrected partnership (Waters et al., 1991, p. 223) implying she may have also found the concept lacking, preferring to retain a ‘secure base’ model for developing adult measures.”

Indeed, Main & colleagues (1985) turned to cognitive science in proposing their valuable ‘move to the level of representation’ as noted in Chapter Five. This explicit interest in cognitive study for attachment may differentiate her from the two previous writers above, but unites her with the two that follow. However, as noted above, in commenting on the contributions to the edited volume on attachment in the pre-school period (M. T. Greenberg et al., 1990), Ainsworth (1990) identified the notable lack of GCP consideration throughout most of the volume: this observation would have included both Main and colleagues articles on disorganisation (Main & Solomon, 1990) and on the unresolved adult attachment in the AAI (Main & Hesse, 1990) respectively. As noted in Chapter Five, her team’s application of Grice’s linguistic/narrative insights in developing the AAI measurement model invigorated the study of IWMs, our grasp of adult attachment, parenting, possible intergenerational patterns of attachment and affect regulation. However, Main’s initial interests in cognition appear to have emerged with considerations of possible IWM mechanisms in adulthood predictive for early infancy attachment rather than ontogeny of attachment beyond early infancy.

On the other hand, Main’s work also would appear to be in part quite relevant for conceptualising GCP. As noted her team has explored general adult attachment styles (or differences or ‘states of mind’) as an insightful integration of language, narrative, memory and affect. She and colleagues (Main, Hesse, & Kaplan, 2005) have also introduced valuable childhood representational methodologies within their Berkeley Longitudinal Study, also acknowledging Bowlby’s influence. This longitudinal work extended cognitive interest to older children including work with a specific reunion measure of attachment designed for children of six years of age (Main & Cassidy, 1988). They note that differences in six year old attachment behaviours “must be guided via differences in attention, thinking, language, and memory” (Main et al., 2005, p. 258). Indeed, Solomon and George (2016) indicate some continuity and influence between this early measure (Main & Cassidy, 1988) and Cassidy and Marvin’s first efforts at developing a measure for 2.5-4.5 year olds, Bowlby’s original Phase IV period. Marvin’s GCP work has been highlighted above. However, this interest is less focussed on attachment development per se but rather its stable expression into adulthood: the concept of

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255 For Waters and Cummings (2000), the ‘secure base concept’—the behavioural systemic capacity to engage others in the service of security—has served as the original defining feature of attachment relationships, differentiating these relationships from non-attachment relationships. They have also suggested how the initial GCP might have failed to account for this foundational security.

256 Again, for a thorough review of the AAI and related substantive issues, see introduction, seven chapters and supplements in Monographs of the Society for Research in Child Development (Booth-LaForce & Roisman, 2014).
a GCP is never raised. The initial turn to adults was primarily for gaining an adult measure for explaining identifying predictability of infant attachment. However, their longitudinal research seems to be seeking to establish both the ‘lawful continuity’ of earliest attachment across the lifespan and its near exclusive influence for later relational and caregiving outcomes as the norm (Main et al., 2005). (See discussion of peer criticism and conceptual resolution by Bowlby in Subsection 5.4.2) Post infancy attachment, even adult attachment, might therefore appear to be as much an outcome than lifespan attachment development. Cognitive skills may be usefully employed but seem to not be constitutive of an attachment relationship for older children; the attachment development appears to have possibly been concluded at Phase III. Attachment here looks more like the internalisation phenomena proposed by object relations, i.e. an unconscious personality construct. However, despite her interest in the role of cognitive capacities beyond early infancy, her position is unclear and also difficult to classify without more specific engagement with GCP.

Finally, I would also like to acknowledge what may be perplexing here: namely the presence within her work of potentially valuable insight into a GCP—especially the role of representation—that may nonetheless be less explicitly tied to early collaborative attachment experience without a conceptual framework like the GCP. In a sense the dots may have not been joined together, if there might indeed be a role for a GCP. Mudrick’s (2016) recent development of a GCP measurement for pre-schoolers might best illustrate this perplexing situation. In designing a new GCP measure for 6-8 year olds, she turns particularly to insights gained in the AAI to develop a method for capturing “Goal-Corrected Partnerships: Young Children’s Representations of Mother-Child Negotiation”—her thesis title. Indeed, in seeking to address the negotiated aspect of a GCP, she turns to the required capacity for narrative competence: “narrative coherence in the current study can provide a glimpse into successfully being able to navigate these conflict situations through representations of aspects of GCP” (Mudrick, 2016, p. 10). The outstanding question here is the degree to which the GCP is considered to be the concluding phase in a proper developmental account of attachment. Such a conceptual approach would seem to conflict with traditional but unsupported proposals for the sort of sensitive period / continuity positions claimed by Main and colleagues.257

Settling a clear relationship between Main’s position and Bowlby’s GCP would appear difficult to make. Her position may be inconsistent, or perhaps neutral. So I will describe her position as unclassifiable, i.e. either not classifiable,258 or possibly inconsistent.259

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257 I will return to the sensitive period and continuity questions below. However, it is worth noting the complex conceptual terrain that make up discussions of critical and sensitive periods (Bailey, Bruer, Symons, & Lichtman, 2001), whilst also recognizing that the application of such terminology to attachment would appear unsupported (Thompson, 2001). Studies have also regularly failed to demonstrate simple continuity in attachment (see most recent discussion in Solomon & George, 2016).

258 The psychometric issue of ‘non-classification’ of attachment remains somewhat unresolved. My choice of this term is not intended as a dismissive double entendre. Indeed Main & Solomon’s (1986) work on disorganisation has been considered truly ground breaking in clarifying past classification difficulties. Interestingly, Solomon—Main’s colleague here—has clearly acknowledged Bowlby’s GCP (Solomon & George, 2016).
Fonagy: Fonagy and colleagues have provided at least two relevant conceptual advances for Attachment Theory.260 First, they have extended Main’s important representational work with the AAI developing a complementary reflective-function (RF) coding system for the same interviews (Fonagy, Target, Steele, & al., 1998). Just as the AAI predicted correspondences between adults and their infants’ attachment, so too did differences in parental reflective functioning show similar correspondences (Fonagy, Steele, Steele, Moran, & Higgitt, 1991). Like Main, they do not take up the GCP. However, unlike Main they have also sought to specify a more expanded understanding of early intersubjective cognitive/affective functioning in the context of early attachment, though with a slightly modified approach to IWMs. They have contended: “Classically, in attachment theory this phase change from behavior to representation has been regarded as a modification of the attachment system propelled by cognitive development. Our contention here is the reverse: attachment actually propels cognitive development [italics in original]” (Fonagy, Target, Gergely, Allen, & Bateman, 2003, p. 416). Indeed they locate the source of their interest in RF or mentalisation in psychoanalysis (Fonagy et al., 2016).261 Nonetheless, they have studied GCP relevant cognitive notions such as theory of mind development in the context of emerging reflective functioning and early attachment. They identify a “synergistic relationship between attachment processes and the growth of a child’s capacity to understand interpersonal behaviour in terms of the mental states” (Fonagy et al., 2016, p. 780 loc 29555). Whereas attachment IWMs within a GCP are seen only to predict, newly emerging differentiated cognitive mechanisms interpret. However, this theoretical split of prediction (IWMs) from interpretation (RF and TOM) is less compatible with a cognitively constitutive lifespan GCP proposed herein.

Briefly, their second conceptual advance has been their study of reflective function in the context of pathological self-development. Serious personality disorders—especially Borderline (BPD)—appear during emotional distress to often be accompanied by a cognitive collapse of an already underdeveloped reflective functioning (Fonagy & Luyten, 2009). Such a failure might equally be considered as a breakdown of poorly developed Phase IV GCP capacities. Indeed, one may wonder if their view of the development of reflective function and the self in the context of early attachment from 2-5 years of age (Fonagy & Target, 1997)

260 Finally, both Sroufe and Main have worked within a developmental setting and have been focussed on measuring the impact of early attachment experience (and other variables) on later outcomes. Their longitudinal studies predated the development of many of the later GCP measures acknowledged in Section 7.1 Both have relied upon the infant SSP and adult AAI identifying them as ‘gold standard’ measures (Main, Hesse, & Hesse, 2011; Sroufe, 2003). They may therefore sit on the first side of a developmental psychology vs social psychology divide in Attachment Theory. Consequently neither appears to have approached attachment specific dynamics across the full lifespan. For a collaborative discussion of an emerging rapprochement between the two schools see Roisman & colleagues (2007).

261 As noted in earlier chapters, Fonagy and colleagues have provided a valuable service to Attachment Theory seeking regularly to clarify how attachment relates to the psychoanalytic tradition. Of particular interest is Fonagy’s discussion of the near disappearance of sexuality from relational psychotherapy. For a recent view, see their third contribution to the recent Attachment Handbook (Fonagy, Luyten, Allison, & Campbell, 2016).

262 Mentalization is defined as “the mental process by which an individual implicitly and explicitly interprets the actions of himself and others as meaningful on the basis of intentional mental states such as personal desires, needs, feelings, beliefs and reasons” (Bateman & Fonagy, 2004, p. 21); it also conceptually overlaps with notions such as self-reflection, metacognition and metaemotion discussed in the cognitive and affective sciences, as well as the developmental notions of intersubjectivity discussed in Chapter Two.
might not add ‘a bit more meat on the bones’ of Bowlby’s lean depiction of the GCP. My general suggestion here is that the strong conceptual resemblance between Fonagy & colleagues’ reflective function and underlying cognitive processes accompanying a proposed GCP might legitimately count for an *implicit equivalence* of the GCP, less the implicit endorsement suggested for the initial two. The RF for Fonagy, derived in part from AAI interviews, accomplishes theoretically part of what a GCP will do. Mudrick (2016, pp. 11-12) has also included Fonagy and colleagues’ mentalisation as a core feature in her approach to early GCPs. Indeed, J. Holmes (2001b) has made a similar observation describing a ‘metallisation hypothesis’ or ‘domain’ for attachment that specifically embraces Fonagy here. However, whether Fonagy might accept my position is a potentially interesting question. Along with colleagues,²⁶² he (Fonagy et al., 1991) has traced the RF not to attachment per se, but rather has identified it as a conceptual product of several research avenues including earlier alternative psychoanalytic discussion: namely Anna Freud’s approach to metapsychology. I will return to the clinical question of cognitive collapse in Section 8.4.

**Liotti:** Finally, the last researcher is perhaps one of the few to *implicitly* reject a GCP. He shares Fonagy’s interest in research on attachment and treatment of BPD (Liotti, 2011). Cortina and Liotti (2010a) have argued that attachment should be limited to a narrowly defined implicit system for protection in times of danger alone. They strongly differentiate this security only system from the affect regulatory activities often associated with attachment (see discussion of Construct 5 in Section 4.3). These more regulatory capacities are rather seen as properties of intersubjectivity. Indeed, they identify what they call “the classic formulation of the function of attachment as protection in moments of danger” (Cortina & Liotti, 2010a, p. 411), attributing this to major contributions from Bowlby (1969/1982, 1973, 1980) and Ainsworth and colleagues (1978). Although attachment and intersubjectivity may be ‘intertwined’ for Cortina and Liotti, they are described as phylogenetically distinct. Indeed, they suggest in places that Fonagy and colleagues have mistakenly conflated attachment and these interpersonal regulatory and reflective capacities. (In these instances, Cortina and Liotti might agree with my conclusion above that Fonagy be considered as an unknowing GCP endorser.) They propose attachment to be an evolutionary generated Fodor-like behavioural system—or module—and see it as a near identical homologue with other species. In their perspective, what may distinguish early human social emotional development from our mammalian relatives are the more uniquely human forms of intersubjectivity (Cortina & Liotti, 2010b). The value of their approach may rest in their engagement with phylogenetic considerations. Indeed, although I will disagree in part with their conclusions, it will motivate discussion in Chapter Eight.

Table 7.1 below summarises the positions of the five attachment researchers. Needless to say, my discussion is conducted at a high level and cannot do justice the breadth of each

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²⁶² Fonagy and Main’s positions are probably less distinct than I may be positioning here. Two of the leading AAI researchers—Miriam and Howard Steele (Steele & Steele, 2014)—are also contributors to the Fonagy work cited here. As noted, the RF is measured through a complementary coding of the AAI, so such overlap might be expected.
writer’s corpus of work. More than anything, the five positions may represent the identification of a lost opportunity. With the possible exception of Liotti, a direct engagement with Bowlby’s GCP might potentially make their remaining respective positions stronger. This may be especially true for Main and Fonagy. I have also not yet acknowledged how questions of personality development, attachment continuity and sensitive periods indicted in the table might have also influenced an omission of the GCP.
Table 7.1: Analytic Summary - Five researchers with little or no place for GCP

<table>
<thead>
<tr>
<th>Attachment Researcher</th>
<th>Rutter</th>
<th>Sroufe et al</th>
<th>Main et al</th>
<th>Fonagy et al (esp Target)</th>
<th>Liotti (and Cortina)</th>
</tr>
</thead>
<tbody>
<tr>
<td>General Perspective</td>
<td>Child Psychiatry</td>
<td>Organisational Perspective</td>
<td>Representational Perspective</td>
<td>Mentalisation Perspective</td>
<td>Fodorian like Modularity and Intersubjectivity</td>
</tr>
<tr>
<td>Relationship with GCP</td>
<td>Implicit acceptance</td>
<td>Not inconsistent “Fits in with”?</td>
<td>Unclassifiable</td>
<td>Implicit equivalence: Conceptual similarity with definitional overlap</td>
<td>Implicit rejection</td>
</tr>
<tr>
<td>Stated cognitive interest [or alternative]</td>
<td>YES</td>
<td>NO [Emotional Development]</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Continuity / Sensitive Periods</td>
<td>NO</td>
<td>NO</td>
<td>YES ?</td>
<td>YES</td>
<td>YES ?</td>
</tr>
<tr>
<td>Professional Role</td>
<td>Developmental Researcher</td>
<td>Developmental Researcher &amp; Clinical Psychologist</td>
<td>Developmental Researcher</td>
<td>Developmental Researcher, Clinical Psychologist &amp; Psychoanalyst</td>
<td>Therapeutic Researcher, Cognitive Psychotherapist &amp; Psychiatrist</td>
</tr>
</tbody>
</table>

7.4.3 A Last Word about Psychoanalysis, Attachment and a GCP

The elephant in the room for a discussion of a GCP within the final three writers may be the degree of reliance on psychoanalytic concepts—namely, sensitive periods and the continuity hypothesis—in forming their perspectives. This raises again the more general question about the general compatibility of Attachment Theory and the broader heterogeneous field of psychoanalysis. Previously in Chapter Six, I proposed that attachment should not be formally considered a psychoanalytic field and where required conceptual boundaries should be retained between the two. A further elaboration can be added on the basis of a possible position for a GCP. First, to reiterate, although attachment has been embraced by a sizable proportion of psychoanalytic theorists (Fonagy et al., 2016), the degree of conceptual and methodological match remains problematic (Eagle, 2013). Second, as noted previously in each of the chapters in Part One, early attachment considerations of timing, sensitive periods, expected stability for attachment, process for internalisations, and emotion regulation clearly overlap with psychoanalytic positions in object relations (J. R. Greenberg & Mitchell, 1983) and self psychology (Banai, Mikulincer, & Shaver, 2005). Indeed, as noted previously, it was the psychoanalytic writer J. Holmes (2001b) who seems to have identified a ‘continuity hypothesis’ in his synthesis of attachment and psychoanalysis. J. Holmes noted, “Attachment patterns in childhood have far-reaching affects on relations in later life and their mental

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263 Liotti is not a psychoanalyst but his collaborative work with Cortina, who is, would nonetheless seem to reflect the psychoanalytic views discussed in this subsection.
representations in adult life”; he also commented, this “confirms one of the basic assumptions of psychoanalysis” (J. Holmes, 2001a, pp. 7, 28).

On the one hand, the identification of the impact or outcomes of early attachment experience is clearly supported by carefully considered longitudinal study (Thompson, 2015, 2016). Indeed, the variable ways in which this might occur were elaborated in Chapter Six discussion of the organisational perspective. However, there is a subtle but nonetheless profound difference between the consideration of ‘attachment as an early only influence’—something akin to a traditional psychoanalytic ‘continuity’ position—and ‘attachment as both an early and ongoing influence. Indeed, Bowlby (1969/1982) and Ainsworth (1969) remained vigilant in selecting only those common psychoanalytical developmental positions that were also empirically supportable. As noted in Chapter Five, Bowlby (1969/1982, 1988) responded to criticisms of his own early overly simplistic employment of sensitive periods and normative stability (1958a) both accepting Rutter’s corrections (1981) and embracing Sroufe & Waters (1977) organisational approach.

I suspect it may indeed be difficult to reconcile fully my proposed version of a GCP introduced below with the more traditional object relations and self-psychology positions. Although Main, Fonagy and Liotti approach attachment quite differently—I would suggest Fonagy’s positions as closest to what I propose here—, they all appear to reflect some version of development assumptions contained in the psychoanalytic ‘continuity hypothesis’. When it comes to consideration of possible long-term influence, attachment would appear to be an infancy only phenomena for three writers, with the only notable exception of later trauma/loss. The sort of dynamic lifespan development I am arguing for below would not seem to fit within their models. Indeed, Main and colleague’s (1985) strategy to “surprise the unconscious” in the AAI—an innovative approach to observe implicit/procedural responses—would seem to assume attachment representations to be implicit and present from earliest childhood only, bar only exceptional later trauma or loss. Fonagy and colleagues’ interesting dynamic depiction of the emergence of anxiety disorder in children also restricts developmental attachment in personality to early childhood (Nolte, Guiney, Fonagy, Mayes, & Luyten, 2011). Cortina & Liotti’s (2010a) rejection of both an attachment emotional regulatory activity in favour of self-object functions from self psychology and attachment reflective capacity as more likely to be part of an evolutionary distinct modular intersubjectivity cannot be reconciled with a GCP conceived as part of a dynamic lifespan personality construct for attachment.

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264 See the contrasting discussions of possible sensitive periods for psychoanalysis (Beit-Hallahmi, 1987) and attachment (Thompson, 2001).

265 Many of the ongoing disputes concerning attachment are grounded in questions revolving around positions that seem to be based on some (inaccurate) version of attachment as a continuity and/or early only perspective. Main and colleague’s (2005) longitudinal work seems to reflect a stability and early experience only view. Critics of attachment such as would seem to also oppose just such a position (Harris, 2009; Kagan, 1995; Lewis, Feiring, & Rosenthal, 2000). Indeed, researchers more friendly with attachment also appear to study its impact on later developmental tasks as if it might be an ‘early experience only’ phenomenon (de Rosnay & Harris, 2002). Emotional discussions between parent and child are somehow not part of ongoing attachment phenomena.
7.5 A ‘Rearticulation’ of the GCP within Attachment Theory

7.5.1 Revisiting Phase III and Phase IV

One of the consequences of the paradoxical neglect of Phase IV development has been an imbalance in the available empirical data and conceptual considerations. This applies to both what a Phase IV GCP might be and how it might work, especially across the lifespan. The investigative interaction between Phase III attachment research and primary and secondary intersubjectivity carried out by Beebe and colleagues (2010) is a case in point. In a summary of her team’s work Beebe and Steele have proposed:

Microanalysis operates like a ‘social microscope,’ identifying ‘subterranean’ rapid communications, which are often not quite perceptible in real time. This approach offers the possibility of identifying aspects of maternal sensitivity and the origins of attachment with a more detailed lens (2013, p. 583).

Their work has demonstrated how early non-verbal interactions between four months olds and their caregivers—within Bowlby’s Phase II period—predicts infant-caregiver attachment at 12 months of age (Beebe et al., 2010). No mention is made of post-infancy influences.

Not surprising, we lack anything close to this degree of sophistication in studies of possible relationships between Phase IV development and either later intersubjectivity or expanding cognitive capacities. Again as suggested above, Waters & Cummings have noted,

Unfortunately, Bowlby’s emphasis on the early phase of attachment development has been a source of misunderstandings and missed opportunities. Misunderstandings because it suggests that secure base behavior emerges rather quickly, implying to some that learning and socialization play little part in his model. Missed opportunities because it doesn’t direct attention to the maintaining and shaping influence of caregiver behavior or developmental changes in secure base use beyond infancy, much less in the course of adult-adult relationships (2000, p. 166).

Waters & colleagues have also argued previously for the inadequacy of Phase IV GCP for understanding more complex lifespan attachment phenomena: “the goal-corrected partnership concept doesn’t capture later development of attachment very well” (1991, p. 7). The background and need for a balancing of research interest has now been laid bare. A ‘rearticulation’ of the GCP as something more conceptually sophisticated than the mere current recognition of ‘that which follows Phases I to III’ is in order. Indeed, the original GCP may also require enlargement. I suggest at least two expansions: one dynamic, the other lifespan.

7.5.2 The GCP as Inclusive Dynamic between Phase III and Phase IV

First, the development of a more implicit Phase III tie should not be considered as distinctly concluded or without influence on the newly emerging Phase IV. See Figure 7.1 below. Phase IV does not replace Phase III. On the contrary, the two should be seen as representing
an ongoing interacting dynamism where new affective/cognitive capacities become available for inclusion within prior Phase III attachment ties. These newly emerging capacities also contribute to an inchoate attachment regulatory capacity. Therefore, as evidence suggests (Van Ryzin, Carlson, & Sroufe, 2011), attachment ties may expand but may also deteriorate during the initial Phase IV development. This would also equally reflect the empirical longitudinal evidence for both continuity and change between early and later attachments (Pinquart, Feußner, & Ahnert, 2012). This more dynamically conceived GCP should also be considered the general model for attachment relationships across the lifespan. This is the second point for consideration.

Figure 7.1 Phase IV Goal Corrected Partnership: Initial Dynamic Reconsideration

The concept of Phase IV development should not conclude with a first attachment in preschool years but should also be extended beyond early childhood. The second plank takes up this requirement. Figure 7.2 captures the idea of ongoing developmental lifespan change. (i.e., the green arrow running through all development periods post).
7.5.3 Phase IV and the Accompanying Range of GCPs Do Not Stop Developing

The proposal to reconceive Phase IV from which GCP develops as a series of lifelong processes is supported by a number of observations. First, Phase III and Phase IV development are applicable across the full lifespan. Therefore, both GCPs and regulatory capacities can be understood to undergo ongoing development. Similar sorts of lifespan change for both emotions and emotion regulation (Consedine & Magai, 2006) as well as cognition (Howe & Brainerd, 1988) are now regularly acknowledged and studied. Attachment ties can be seen to both grow into and most probably regress in late adulthood or in the context of illness (Cicirelli, 2010). Both of these proposals have been depicted in Figure 7.2 above. The proposed differences can be noted via a quick comparison with the Ontogeny of Attachment as depicted in Figure 4.2 and discussed in Section 4.3. Bowlby’s attachment ontogeny discussion was focused on the initial emergence of capacities for first attachments as depicted in Phase III and Phase IV. This ‘development’ is followed by ‘growth’ beyond preschool Phase IV. However, lifespan studies have increasingly challenged the development-growth dichotomy. My suggestions in Chapter 6 that attachment be seen as both a DST and DLPC would therefore suggest attachment ontogeny be recast in a similar vein. (Again see employment of the two terms, ‘development’ and ‘growth’ in Figure 4.2). Indeed, both Waters & Cummings (2000) and Crittenden (Crittenden, 2008; Crittenden & Liandini, 2011) have embarked upon similar extensions. The innovative work of the former Ainsworth student, Crittenden, will be highlighted in Chapter Eight. The need for consideration of development into later life was also noted above and in Footnote 6. This lifespan
developmental period would also provide stronger conceptual support to both stability and change observed in both individual attachment relationships as well as a person's more general regulatory capacity or attachment style. It would also capture the phenomena of attachment organisation and developmental pathways discussed in Chapter Four.

7.5.4 The Dynamic State of a Combined Phase III and Phase IV in most GCPs

The second proposed plank of this rearticulation considers the more dynamic ‘here and now’ state of the operations within a GCP. First, Phase III’s more implicit primary and secondary intersubjectivities are retained within relational attachment phenomena throughout the lifespan, together with accompanying basic emotions and non-verbal regulatory patterns. Post infancy, Phase III exists alongside the cognitively more sophisticated skills at work within a GCP. However, the two may typically also work in a more dynamic fashion with one triggering the other and vice versa. Together they might account for various multi-level mechanistic operations, especially operations underpinning possible changes across the lifespan. To some degree, this position may also shine some light on the success of work by Main, Fonagy and Liotti with maladaptive attachment. They have identified discernable remnants of earliest Phase III attachment properties in the context of either a collapsed or poorly developed reflective capacities. These last two mechanistic/change and functional consequences highlighted here will be revisited in my defence of a revised GCP in Chapter Eight.

7.5.5. Attachment GCP and the Dynamic Lifespan Personality Construct

What both of these planks provide is a more thorough explanation of the proposed DLPC in Section 6.2. Personality, at least the attachment specific influences, is characterised by a series of emotionally close relationships and a capacity for self and affect regulation. As a type of developmental system, these form a unique individual network that might be pictured as a web of intricate relationships and capacities evolving with the passage of time. The DLPC is therefore a complex system both robust and fragile at the same time. A re-engineered notion of a GCP, its lifespan developmental period and accompanying internal, interpersonal and ecological dynamisms might also better describe an autonomous yet attached self.

7.6 Chapter Conclusion

Bowlby's notion of a GCP has been generally accepted and might well be termed the received position in Attachment Theory. The notion has found coinage in studies that cover the breadth of the lifespan. The value of a GCP notion is the attendant introduction into attachment phenomena of constitutive cognitive skills, especially tertiary intersubjectivity, language and TOM. However, the concept has not been universally taken up. This is probably best explained by multiple factors. In some cases, the omission has carried little meaning and the notion might be seen as implicitly accepted—Rutter, and possibly Sroufe. In
other cases attachment may have been understood as pre-linguistic only phenomena, possibly reflecting a psychoanalytic notion of sensitive periods captured in the 'continuity hypothesis'—possibly Main, Fonagy and Liotti. Lastly, acknowledgment of a possible inadequate conceptual grounding for the inchoate GCP might also suggest the notion may have also been found conceptually lacking—not adequately developed. In ways researchers have carried on without it—possibly Sroufe and Main. Consequently, the chapter offers a proposal for a more dynamic lifespan approach for understanding the GCP. Previous extensions of the organisational perspective from Chapter Six have been called upon to ground my rearticulation. Justification for a potentially ambitious conceptual rearticulation requires significant support. Chapter Eight will provide a further reapplication of the TLC strategy that defends this fresh approach to the GCP.
CHAPTER SEVEN REFERENCES


Chapter Eight: Grounds for Updating Bowlby’s Cognitively Enhanced lifespan GCP

...An increasing number of emotion researchers are converging on the conclusion that higher-cognitive emotions are evolutionarily rooted in simpler emotional responses found in primates (Clark, 2013a, p. 437).

8.0 Chapter Introduction

One way of supporting an expanded view for a GCP would be to argue its case in terms of a relatively simple thesis statement. I propose the following: Attachment Theory fails to account for attachment lifespan phenomena without a Phase IV dynamic lifespan GCP. More specifically, what is at stake here is the constitutive position proposed for cognition by Bowlby alongside behaviour and emotion: something I contend is fundamental to attachment lifespan phenomena.

The concluding chapter to Part Three argues for the acceptance of an expanded GCP as introduced in Chapter Seven and the inclusion of the entailed cognitive capacities as constitutive parts of attachment. Indeed, such capacities facilitate the reciprocal regulation of emotion in close adult relationships. Such a view extends the cognitive capacities to all post infancy attachment relationships, to the attachment derived regulatory capacities and across the theory more generally. This chapter is constructed as a refutation of the phylogetic attachment claims made by Cortina and Liotti (2010a, 2010b). Their position—what I will call a ‘phylogenetic GCP rejection’—appears to both limit attachment development to Phase III activities—homologously with other species—and to isolate attachment phenomena more broadly from concurrent intersubjective phenomena. From a philosophical perspective, their approach constrains properties of attachment relationships to the provision of safety in times of danger. I will argue that a Bowlby inspired expanded GCP would entail the contrary position: attachment extends into and beyond Phase IV and requires intersubjective capacities throughout, including responses in service of safety but also broader affect regulatory interactions.

My position is hardly radical. It is a reorientation of Bowlby’s original but perhaps less noticed interest in cognition, especially his previously noted cognitivist hopes for ‘felt appraisals’. The chapter’s argumentation pursues a reapplication of Bowlby’s TLC strategy, where behaviour, emotion and cognition have been studied as more integrated phenomena. Each of the four Tinbergen realms of behavioural explanation will be engaged across four central sections. The chapter will also provide a relatively counterintuitive approach for defending a

257 From a philosophical perspective, the employment of the term constitutive suggests that cognitive capacities are properties of attachment.

258 As noted in Chapter Three, Bowlby’s philosophical interest in emotion was grounded in both the positions of the early cognitivist Magda Arnold and aesthetic philosopher Susanne Langer. This position differentiates him form the more dominant influence of feeling/perception theory of William James prevalent in many psychoanalytic and humanist positions. As also previously noted, Scarantino (2005) has suggested that despite the dominant cognitivist and feeling positions from the 1960s onward, both have been shown to lack empirical support for their theoretical claims and are better considered as positions on emotions within a popular folk psychology.
constitutive position for cognitive skills in a GCP. The principle positions defending cognition will be initially derived from concepts derived from philosophy of biology but employed in the field of emotion research, namely the employment of phylogenetic and developmental homologies.259

The chapter begins in Section 8.1 with the development of a TLC inspired framework for the support of an expanded GCP. Findings in emotion research will be called upon in developing a counter homological strategy to defend the chapter’s GCP position against the Cortina-Liotti position. Section 8.2 makes a first application of homology in the context of attachment phylogeny. The earliest GCP as first described by Bowlby emerges in evolutionary history as an instance of the grafting of more sophisticated behaviours, affects and cognitive skills on to an earlier cross-species shared attachment. Phase III implicit and Phase IV explicit capacities are retained but not as successive phases. They are more dynamic dimensions of attachment phenomena. Next, Section 8.3 continues the homological application suggesting how arguments supporting adult pair-bonding might be equally considered as both phylogenetic and developmental homologies.

The chapter shifts focus in the following two sections. A functional—the third realm—consideration of the question of adult GCP capacities is raised next. Section 8.4 discusses the acknowledged collapse in ‘metacognitive or reflective capacities’ in the face of distress—a form of affect dysregulation—that seems to typify Borderline Personality Disorder (BPD). This deficit will be considered in terms of regularly noted correlations between childhood disorganised attachment and adolescent and adult BPD (Lyons-Ruth & Jacobvitz, 2016). A comparison and contrast of three ‘attachment’ approaches—Cortina and Liotti, Fonagy and colleagues, and Ruth-Lyons—will be conducted arguing that the identified dysregulation in BPD might arguably seen as capturing underdeveloped or damaged attachment regulatory capacities that emerge in the context of disorganised developing GCPs. Finally, Section 8.5 takes up the fourth Tinbergen mechanistic question. The consideration focusses on adult attachment multi-level mechanisms in the context of a romantic GCP. The section looks in two directions. First, Zayas and colleagues (2015) Personality-in-Context (PiC) Approach will be offered as a model for the development of relationship specific attachment IWMs in the context of co-regulation of emotion in adult romantic relationships. Second, experimental research on security priming with adults will be reintroduced; this evidence suggests how attachment IWMs influence intrapsychic, interpersonal and social experience. Taken together, a strong case can be made not only for the inclusion of cognitive capacities in GCP but their deep constitutive integration with behaviour and emotion as evidenced in the chapter. The chapter concludes arguing that Attachment Theory cannot account for lifespan GCP phenomena without the proposed cognitive components.

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259 In a sense, the emotion component in Bowlby’s strategy—the L or Langer dimension—will be called upon to provide the conceptual insights for establishing the cognitive contents—the C or Craik dimension.
8.1 Outlining a TLC based Framework and a Turn to Emotion Research

8.1.1 A New GCP Position

A discussion of the role of cognition for the GCP would also address once again what may be the lingering ‘early, continuity only’ issue that was arguably put to bed by Bowlby and Ainsworth many years ago in their endorsement of the GCP. The joining in Chapter Six of the organisational perspective, DST and a proposed DLPC provides clear conceptual support for this expanded position. Raising this question would also require non-acknowledgers of the GCP to clarify their ontological assumptions as to what an attachment might be. It asks once again the question at the core of Chapter Four's analysis: ‘what is an attachment in Attachment Theory’? If attachment exerts influence beyond early childhood—something that is now a clear statement of empirical fact—then how does it do so? More specifically, what might be the role of cognition in attachment phenomena beyond infancy —both in close relationships and a regulatory capacity—and more generally relationship between affect, cognition and behaviour in attachment phenomena?

8.1.2 Cortina and Liotti’s ‘Phylogenetic GCP Rejection’ Argument

The foil for my position will be what I call Cortina and Liotti’s (2010a, 2010b) ‘GCP phylogenetic rejection’ argument, something discernible from within the employment their ‘hierarchical evolutionary model’ of motivational systems (Cortina & Liotti, 2014). Here is their general evolutionary perspective: “Motivational systems – reptilian, mammalian and neo-mammalian – emerged in phases during the course of millions of years. These different phases did not replace each other, but became re-organized in the brain at different hierarchical levels, as can be seen and inferred by its architecture. This evolutionary model of the human brain was put forward by Hughlings Jackson in early 20th century and by Paul MacLean in the 1980’s. We explore some of the implications of this hierarchical, multi-motivational, cooperative-communication model of the mind for psychotherapy and psychoanalysis” (Cortina & Liotti, 2014, p. 865). Their notion of motivational system is similar to Bowlby’s behavioural system. However, I will take specific exception with their conceptions for a ‘hierarchical’ perspective, in favour of Bowlby’s provision of a more central role provided by am emerging personality informed attachment system. They define attachment

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260 This will count as the fourth time in the thesis that the continuity position has been named and challenged. The emphasis is intended to counter what may be the most crucial internal challenge to coherence within Attachment Theory.

261 A few points of clarification may be useful here. First, as noted below in Subsection 8.1.3.2 the discussion of the emerging neurological evidence supports greater networked emotion-cognition integration. On consequence is that simpler neurological models are giving way to more complex ones. In particular, the MacLean tripartite approach, which has been a helpful heuristic for grasping brain development and commonly employed—both phylogenetically and ontogenetically—, is now seriously challenged as too simplistic (see discussion in Labouvie-Vief, 2015). Second, although Jackson remains quite relevant (Franz & Gillett, 2011), some caution should be taken when reading his ideas on complexity in nature and against the possible influence of Haeckel’s disproven ‘ontogeny recapitulates phylogeny’ (Gould, 1977). Both Freud (Gould, 1987) and Janet (Salliot, 2004) also held historical-dated evolutionary perspectives that demonstrate outdated hierarchical influences. Although increased complexity is common throughout evolutionary development, it has been shown to no longer be appropriately considered a hierarchical rule. Complexity does not equal ‘fitness’. Also the discovery of complex capacities of in very simple life forms such as retro-viruses serves an example where complexity may exist at so-called lower levels. For a detailed discussion, see Gould (1977).
narrowly as a care-receiving motivational system whose “main function . . . is to seek protection” in times of danger, and distinguish it sharply from intersubjectivity whose “main function . . . is to communicate, at intuitive and automatic levels, with members of the same species and is to facilitate social understanding” (Cortina & Liotti, 2010a, p. 410). The two are “closely inter-twined during typical development, but become disjointed during atypical development” (Cortina & Liotti, 2010a, p. 416). Implicit here—i.e. their restriction of intersubjectivity to “intuitive and automatic levels”—would also appear to be a consideration of attachment as only the early Phase III period of infant attachment development, something more or less homologous with other species. Their position would also appear to rule out on evolutionary grounds any possibilities for an attachment GCP facilitated by constitutive cognitive capacities. Equally, the fact that intersubjectivity precedes the emergence of first attachments in human ontogeny is seen as further evidence for the distinctiveness of the two.

I will call this a ‘phylogenetic GCP rejection’ position. Indeed, in responding to suggestions by Fonagy (2001) for more cognitive roles in attachment, they have declared that any position that makes “attachment theory into a theory of mental representations of attachment-based relations” is a “revision of attachment theory”262 (Cortina & Liotti, 2010a, pp. 411-412). Although accepting such representations in humans, they also contend, “This does not mean that intensive parental care and the formation of attachment bonds emerged during the course of evolution to have coherent representations of attachment relations” (Cortina & Liotti, 2010a, p. 412), arguing that such capacities are not present in other species possessing attachment motivational systems. Their attachment evolutionary position seems to restrict evolutionary development to a literal unchanging homology with other species.263 Any additions seem to be required to come from another source, namely their proposed unique human motivational system for intersubjectivity. It is very hard to see how this position could entail the cognitively enabled GCP proposed in Chapter Seven, hence the label ‘phylogenetic rejection’.

On the one hand, Cortina and Liotti importantly seek to reconcile psychoanalysis with contemporary evolutionary study, especially in their consideration of the homo sapiens species as an ‘ultracooperative’ one (Tomasello, 2014). Indeed, they see their position as a natural follow-on to Bowlby, “who first put attachment theory within a modern evolutionary framework” (Cortina & Liotti, 2014, p. 866). With the possible exception of Fraley and colleagues (2005) valuable phylogenetic discussion of adult pair bonding, little examination of the attachment behavioural system and its phylogenetic position have been conducted in Attachment Theory.264 On the other hand, I will also disagree with Cortina and Liotti’s rendering of attachment and its proposed differentiated relationship with intersubjectivity. My

262 The “revision” suggestion here is confusing. As noted previously, the GCP received position with attendant cognitive expansions is grounded in Bowlby and Marvin’s foundational work and supported by Ainsworth. It is difficult to locate a pre-revised position here.

263 I must admit I find their position difficult to follow in places. They seem to suggest that the primary security function of attachment—as they define it—is somehow grounded in some original Bowlby, ‘pre-revised’ view of Attachment Theory. I suggest that no such view exists in a close reading of Bowlby.

264 What is presumed here, is the original foundational employment of homology by Bowlby in his initial identification of the phylogenetically received behavioural attachment system.
criticisms will also follow on similar considerations of both evolution and development, especially the lifespan dimension of attachment phenomena. Whereas Cortina and Liotti conceive the function of the attachment motivational system as one of ‘only protection’, I will turn to the GCP, something noticeably missing in their discussion of attachment. With Fraley and colleagues I will sustain the lifespan functional manifestations of attachments. Once again, I argue that attachment function is hardly limited to ‘early infancy only’ phenomena but must account for lifespan influences, especially adult relationships.

Although I will disagree with their perspective, their having raised possible phylogenetic explanations should not be undervalued. They are clearly on the right track in acknowledging the need for phylogenetic considerations. So my focus here on their position also represents an acknowledgment of the value Cortina and Liotti have brought to attachment discussions. I return now to Bowlby’s TLC strategy that suggested how behaviour, emotion and cognition within might ultimately be considered to be integrated phenomena.

8.1.3. Turning to Emotion Research to build a GCP Homological Strategy

Sections 8.2-8.5 provide a Tinbergen quadripartite approach for evaluating an expanded GCP in Attachment Theory: each section corresponds to one the four explanatory realms. But before taking up the four tasks, some preliminary conceptual work is required to help address specific questions concerning cognition and affect within a GCP. Emotion and attachment are indisputably closely linked; attachment is not possible without affects. Attachment interactions, patterns and capacities all include constitutive engagements with specific affective elements: particularly discrete emotions such as fear, sadness and anger. As indicated, philosophy of emotion and affective science may arguably provide a useful platform from which to further support a revised GCP position. For the purposes here, I will focus on how research with discrete emotions might also provide an approach that may apply analogously for understanding an attachment GCP. Three developments in research on discrete emotions are useful here: biological homology, emotion-cognition integration and proposals for a more unified and dynamic approach for the exploration of discrete emotion episodes. Space does not permit more than a general acknowledgement of the three threads. After introducing each relevant emotion finding, I will draw out possible connections for my suggestion of an expanded GCP articulation.

8.1.3.1 Biological Homologies and Discrete Emotions

First, homologies—both phylogenetic and developmental (for a discussion see Ereshefsky, 2012)—have proven useful in the study of discrete emotions. Griffiths’ (1997) employment of phylogenetic homology provided an explanatory position for BEs linking animal and infant emotions, as well as the similar automatic BEs that are also experienced by more mature humans. The identification of BEs has been challenged on failing to meet essentialist
criteria—a position not supported by Griffiths—; however, some sort of early patterned emotions would appear indisputable (for a discussion see Colombetti, 2009). The developmental timing for BEs—emerging in early infancy (Sroufe, 1997a)—also coincides with Phase II pre- and Phase III attachment development (Bowlby, 1969/1982; Marvin, Britner, & Russell, 2016). Indeed, one can hardly argue that the engagement with these earliest discrete emotions is not a constituent process within the early attachment system.

However, the explanatory status of more complex HCEs has proven more challenging for emotion researchers, especially any proposed relationship with BEs (again see discussions in Colombetti, 2013). Clark’s dissertation (2010a) and subsequent publications (Clark, 2013b) have provided a degree of help here. On the one hand he has introduced the notion of developmental homology to suggest how more complex emotions might emerge from less complex BEs (Clark, 2010b). Indeed, similarities in developmental timing and expanding cognitive complexity between HCEs (Sroufe, 1997b) and the development of Phase IV attachment can be readily observed. These differences would also seem to provide one of the major dimensions upon which to differentiate human and non-human attachment.

On the other hand Clark (2010b) has also deepened this homological consideration of HCEs. Based on a comparative study of shame, Clark goes further. He suggests how the traditional division between the so-called basic and higher emotions, where higher have been understood exclusively as human phenomena, may also be inadequate. Hence higher cognitive emotions may also be homologously represented in non-human species. Consequently at least two possible directions of homologies may exist for both basic and higher emotions: one phylogenetic, the other developmental. For a figurative depiction of a homological strategy for attachment experience see Figure 8.1 below. While Phase III attachment is clearly identifiable as a case of phylogenetic homology, Phase IV evolution might be potentially explained as both developmental and phylogenetic homologies. But the details must wait for further clarification below.

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265 However, the status and existence BEs remain disputed. Notably, key constructivist researchers such as J. A. Russell (2003) and L. F. Barrett (L. F. Barrett, Gendron, & Huang, 2009) have rejected both the existence of discrete emotions and BEs derived on the basis of homological phylogeny. They argue how more culturally, context variable positions better match the evidence for variation amongst emotional phenomena.

266 Indeed, Scarantino and Griffiths (2011) have employed Boyd’s (1999) anti-essentialist notion of the homeostatic property cluster theory to argue that such BEs are best understood as natural kinds.
8.1.3.2 The Emerging Integrated Affect-Cognition Relationship in Emotion Research

One aspect of emotion research particularly important for attachment is the growing recognition of a more significant overlap between cognition and emotion. Chapter Two identified Bowlby’s suggestions for ‘felt appraisal’ beginning in infant experience, an inherent endorsement for a more complex understanding of the integration of thought and emotion. The current integration has been driven from at least two perspectives. First, the more traditional ‘disembodied’ approach to emotion appraisals has made little space for any role of bodily phenomena. In contrast, Colombetti has argued how “appraisal is not neatly distinct from the bodily aspects of emotion but is in fact embodied” (2013, p. 83). At an experiential level, her alternative enactive approach incorporates behaviour and follows Frijda (1986) in suggesting how a more contextual based approach may link action and individual meaning. Scarantino (2005) has made similar appeals for greater integrative perspective, in his plea for an ‘empirical’ abandonment of cognitivist and feeling approaches to emotions. Second, Pessoa (2013, 2015) has issued a challenge to the current dominant hierarchical view in neurological research, which differentiates affect and cognition by brain region—upper neocortex and lower limbic respectively (Damasio, 1994; LeDoux, 1998; Panksepp, 1998). His review of the evidence suggests substantial neural interaction between the two regions and beyond. The popular hierarchical brain perspectives also typically segment cognition as ‘higher and left’ and emotion as ‘lower and right’ (e.g. Schore, 2005). Pessoa’s research offers a more hub based dynamic networked interaction approach that calls the simpler dominant perspective into question.\textsuperscript{267} Emotions, cognitions and behaviour are better situated as overlapping integrated phenomena.

\textsuperscript{267} Perhaps a few words are in order here. Pessoa’s observations are mildly revolutionary, potentially overturning many commonly recognized standard hierarchical dichotomies in favour of alternatively conceived continua. They also challenge many of the dual-processing cognition approaches popular today. The comments to
8.1.3.3 Discrete Emotion Episodes in a more Unified Dynamic Approach

After appealing to a basic-higher/complex distinction for discrete emotions to better understand a possible relationship of differences between Phase III and IV, acknowledgment of the limits for such a distinction is also in order. In place of a categorical distinction between the two, some researchers in the affective sciences have appealed to a more dynamic alternative unity—or continuum—based on the notion of open systems. Colombetti has sought to describe emotional episodes in terms of dynamical systems theory (DyST), which seek “to characterise the organism as complex, self-organizing, open and plastic, realising emotional episodes that are soft assembled, context dependent, and highly variable, yet patterned and recurrent” (2013, p. 58). In addition to appealing to coordinative muscular structures and emergent neural self-organisation, she also identifies a role for interpersonal or reciprocal dynamics. I would argue that just as such explanatory approaches shed light on discrete emotions, they are also applicable to a lifespan dynamic GCP. In fact, she appeals to Clark’s homological work in arguing for a lesser distinction between the BEs and HCEs. Instead, she seeks to provide a non-essentialist position that retains discrete emotions as more dynamic phenomena. Instead of genetically hard-wired affective programs, discrete emotions are soft assembled predispositions, and seen to be more complex phenomena that may also incorporate context. She explores how more dynamically considered discrete emotions might show both patterns of similarity, whilst making allowance for contextual, cultural and developmental differences. I would argue that such a position might also reflect an evolving lifespan attachment perspective.

8.2 Applying a Homological Strategy I: Phylogenetic Considerations

8.2.1 Human Attachment and Evolutionary Heterochrony

The value of Clark’s work with emotion homology has been to redirect our attention to primate capacities for experiencing more complex emotions. Perhaps both humans and our fellow primates have also incorporated intersubjectivity within attachment processes. One point worth exploring here is the delayed timing of human attachment. Considerable attention has been devoted to the study of the developmental timing of our species, especially in comparison with other primates (Gould, 1977, pp. 352-404). Indeed, Gould (1977, pp. 400-404) summarised these studies suggesting human development is most typified by his target article Behavioral and Brain Sciences were nearly unanimous in support of an abandonment of what has been termed a folk psychological dichotomy.

Colombetti argues for dynamical discrete emotional patterns that function as self-organizing phenomena. On the one hand such an approach accommodates past history well. However, she rejects the traditional role for representational processes, something potentially less valuable for attachment’s IWMs representational approach.

Colombetti also rejects the ideas of Fodor-like evolutionary delivered fully pre-wired modules for discrete basic emotions, an idea Cortina and Liotti (2010a, 2010b) have analogously and “partially” supported in their arguments for attachment note above. For a very interesting look at the proposed complexity and the potential continua of multiple multi-dimensional types of evolutionary mental modules see H. Clarke Barrett (2015).

Critics reject the existence of basic emotions as realist essentialist positions that cannot establish the patterns of similarity they claim. The opponents include Russell (2003) and Barrett (2006a, 2006b)—psychological constructivists who dispute categories of discrete emotions in favour of more context flexible core affects—and Scherer (2009)—who denies the existence of basic emotions in favour of a more dynamic component process model. For a good discussion of the issues at stake here see the edited volume by Zachar and Ellis (2012).
delayed juvenile development (neoteny) in comparison to virtually all other placental mammalian species, highlighted by both newborn dependency and greater parental engagement in upbringing. This delay is also typical for primate species. Such changes in developmental timing are described as forms of heterochrony: “changes in the relative time and appearance and rate of development for characters already present in ancestors” (Gould, 1977, p. 2) Human attachment may also suggest another unique component in human neoteny. Human infants appear to be one of the few, if not, only mammalian species to be born unable to immediately attach to a caregiver. Whereas most species are able to move to varying degrees, and sustain physical contact and early protection, human neonates must wait nearly 6 months to commence this process. In this case, the delay in human attachment counts as a further example of heterochrony. Indeed, this delay was one of the central themes of the 2003 Dalhem Workshop in Berlin on human Bonding and Attachment (Carter et al., 2005). What this delay might mean in terms of a possible relationship with intersubjectivity is a next question.

8.2.2 Attachment and intersubjectivity: Phylogenetic considerations

It is perhaps this unique attachment developmental timing that raises questions about the relationship between attachment and intersubjectivity. In humans, the former is delayed whereas the latter begins at birth (Trevarthen, 1979). How might one interpret this differentiation? Although delayed timing might suggest distinctiveness between the two, I want to suggest that an early attachment relationship requires intersubjectivity. As noted, Coritna and Liotti (2010a) have seen this timing differential as part of an argument that posits a higher phylogenetically independent intersubjectivity motivational system, unique from an older and more primitive attachment system. However, such phylogenetic claims must also turn to questions of whether intersubjectivity exists in other species, and if so how the relationship between the two phenomena might be best conceived.

Do non-humans evidence the type of primary or secondary affective intersubjectivity that precedes human attachment? And if so what might be its relationship with attachment? Unfortunately, animal intersubjectivity remains an unresolved if not controversial question. Indeed, Chapter Three previously pointed to issues related to the emergence of cognitive ethology. However, one might hypothesise accordingly that just as forms of animal intersubjectivity might also engage higher forms of emotions, some form of intersubjectivity might also be present in primate attachment. A detailed discussion of animal intersubjectivity is not possible. Nevertheless, I will identify a few current proposals that lend help to the

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271 This point is regularly confused. As noted in Section 4.4, early post-natal infant bonding is a distinct phenomenon from later attachment. See especially discussions in Carter (2005).

272 Thanks go to Paul Griffiths for directing me to Gould in approaching the attachment delay in human attachment.

273 For a recent review of current thinking see the 2016 inaugural edition of the journal Animal Sentience: An Interdisciplinary Journal on Animal Feeling, first commented article (Harnad, 2016) “Animal sentience: The other-minds problem”. The article elicited numerous commentaries relevant for this question including ones that also endorsed the possibility for a non-linguistic from of animal intersubjectivity (Racine, 2016).
view endorsing the presence of simpler forms of intersubjectivity in non-human animals. I will offer five suggestions in support. First, researchers on early human intersubjectivity such as Trevarthen and Aitken (2001) have indicated ‘yes’, there is some animal variety of intersubjectivity to be observed. Second, the emerging agreement on the presence of animal emotions—most likely basic in form and phylogenetically shared with infants—would also support such a consideration (Bekoff, 2007, pp. 1-28; Griffiths, 1997). Emotional interaction is often seen as a building block for early intersubjectivity. Third, canine play with conspecifics that avoids aggression and harm might also qualify as a form of animal play as intersubjectivity (Bekoff, 2007, pp. 85-110). Four, certain cognitively simpler forms of cooperation amongst non-humans have also been considered as a relatively basic forms of intersubjectivity (Gärdenfors, 2008). And finally earlier discussion in Chapter Six of animal biosynchrony (Feldman, 2015) in the context of attachment, might also point to animal intersubjectivity. Evidence suggests that other animals, especially primates, may also experience the development of primitive intersubjectivity. Most likely, non-human primate attachment would also require such capacities.

8.2.3 Attachment and Intersubjectivity: Synergistic Coevolution

I now turn to the follow on question: what might be the relationship between the two? On the one hand it is fairly straightforward position to argue that attachments are phylogenetically older than intersubjectivity, as the former would claim older common evolutionary ancestors. Tomasello (2014) has also suggested an independence between the two: that human cooperation emerges as a consequence group foraging and the formation of social organisation. He would appear to make little mention of either early attachment care-giving relations or its adult extension in human pair-bonds. However, in contrast to what Lyons-Ruth (2007) has described—but not supported—as a discontinuity position in evolutionary process between attachment and intersubjectivity, I return to Gould’s suggestion noted above that highlights how humans both share a common evolutionary neoteny with fellow primates and also possess a unique trajectory.

Human evolution has emphasised one feature of this common primitive heritage—delayed development, particularly as expressed in late maturation and extended childhood. This retardation has extended synergistically with other hallmarks of hominization—with intelligence (by enlarging the brain through prolongation of fetal growth tendencies and by providing a longer period of childhood learning) and with socialization (by cementing family units through increased parental care of slowly developing offspring) (Gould, 1977, p. 400).

On this reading it would seem better to argue that early human intersubjectivity might prepare for a more sophisticated and delayed form of human attachment that extends throughout the lifespan.

8.2.4 Attachment and intersubjectivity: Unique Human Evolution
Human infant and chimpanzee cooperation and resource competition have been experimentally shown to differ. Whereas chimpanzees rely on dominance systems to settle group completion for food, Tomasello suggests that “young children have a sense of distributive justice that is closely tied to collaborative activities” (Tomasello, 2014, p. 189), something potentially attributable to adult socializing. However, an alternative reading might point in the direction of extended attachments and greater family cooperation. Homological study assists in identifying similarities and differences. This might appear to be the case with attachment. Indeed Lyons-Ruth has argued for greater continuity between attachment and intersubjectivity in human species: “the organization of human attachment is radically different from the organization of attachment in all other species. This is because of the unique capacities of the human infant for intersubjective exchange” (2007, p. 598). On this reading, the delay in human attachment is less a differentiator from intersubjectivity but rather a preparatory enabler of the inclusion of intersubjectivity into subsequent attachment. Indeed the psychoanalytic developmental researchers Beebe and colleagues (2010) longitudinal studies of infant interactions from 4 months to 12 months, have demonstrated how intersubjective interactions at 4 months predict attachment security at 12 months. Having established a case for inclusion of intersubjectivity—both primary and secondary—in earliest attachment, a look at its place in the fuller lifespan follows.

8.3 Applying a Homological Strategy II: Ontogenetic Considerations

8.3.1 Phase III and Phase IV Lifespan Development

I have argued that Phase III attachment might be better considered as an evolutionary convergence of attachment and intersubjectivity. It is both similar and different from attachment in our nearest relatives. I now turn to the question of Phase IV and the lifespan dimension of attachment. Fraley and Shaver (2000) have previously suggested that infant-caregiver and adult romantic relationships represent attachment behavioural homologies, one has developed from the other. I think this is a correct statement. This also captures the later directions taken by Clark in emotion research described above. Turning once again to this field, I want to take up Colombetti’s suggestions for considering relationships between more basic and more complex discrete emotions. Following Clark she posits a homological relationship suggesting a more dynamic continuum. Phase III and Phase IV development might also be considered in similar terms to discrete emotions. First, Phase IV GCPs emerge from Phase III as a more complex, cognitively enabled relationship. Nonetheless both processes remain dynamically present in attachment experience, perhaps similar to experiences of grief that might include mixtures of basic—automatic felt experiences—and higher forms of sadness—whereby current feelings are actively regulated.

274 Once again, this position would be in agreement with intersubjective researchers (Beebe et al., 2010; Trevarthen & Aitken, 2001) who also see attachment as a particular manifestation of intersubjectivity.

275 Such mixtures are readily observable in responses to loss of love ones. Our grief rituals typically include the solace gained from Phase III type physical presence and Phase IV cognitively enhanced shared memory and reflective searches for meaning.
addition to overlapping, Phase III and Phase IV attachment experiences also exist within a sort of continuum whereby both might be seen to evolve more dynamically across the lifespan reflecting context, culture, specificities of unique attachment relationships, major life-events—traumatic and edifying—and global capacities emerging from early experience. Such an approach marries well with suggestions for a lifespan GCP and the more global DLPC in Chapter Six.

8.3.2 Attachment and Adult Pair-bonds: Which sort of Homology?

Perhaps the more compelling phylogenetic argument supporting a GCP is the emergence of pair-bonds in human evolution. However, the demonstration of this history is less straightforward than has been with infant attachment. Indeed, there is hardly a homological story to tell, i.e. our closest relative the chimpanzee—Pan troglodytes—, do not practice pair bonding. However, Fraley and Shaver (2005) have taken a closer look at a phylogenetic and comparative analysis of attachment pair-bonding. They have conducted a study of pair-bonded species to better understand adult romantic (human) partnerships, a subset of GCPs: “One of the greatest challenges for adult attachment theorists is to explain why the attachment system is active in human relationships, what functions it serves, and why it was co-opted by natural selection over the course of evolution” (Fraley et al., 2005, p. 731). As with previous researchers studying the evolution of relationships, they have explored two additional variables alongside pair-bonding: paternal care and neoteny. They have employed previously identified phylogenetic trees and added data for relevant behaviour. Their first study considered at pair bonding, paternal care and neoteny across a selection of 44 mammalian species; the second study focused on 63 species of primates. They have noted, “pair-bonded species were more likely to have fathers who played a direct role in child rearing than were nonpairing species”—the ‘paternal care hypothesis’—and “species that were more developmentally immature were more likely to exhibit pair bonding”—‘neoteny hypothesis’ (Fraley et al., 2005, p. 742). However, they have concluded that human pair-bonding is most likely to represent an evolutionary response to increased paternal care within the species, and indicated the neoteny position is less tenable. Although this represents only

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276 This study would appear to remain the lone evolutionary analysis of its kind in Attachment Theory. The approach is both creative and informative, and therefore exceptional. However, their employment of the terms ‘function’, ‘comparative’ and ‘homology’ within evolutionary analysis are potentially confusing and clash with those employed in current approaches in philosophy of biology that see descent from a common ancestor as the more fundamental analysis of evolution (Griffiths, 2006). The following statement would seem to provide an example: “Species may be similar to one another in their morphology and behavior because they evolved from a common ancestor, not necessarily because similar selection pressures led to the independent evolution of those traits [italics mine]” The contrast between evolved from a common ancestor and selection pressures for independent evolution is confusing, as if there are two kinds of evolution, the latter more informative. The Tinbergen approach advocated in this thesis acknowledges phylogenetic homology and speciation as the initial building blocks of evolutionary history. However, although their approach reflects a behavioural ecology perspective—probably still dominant determinist ‘Dawkins world view’ on approaching evolutionary function—this view has increasingly come under attack as a consequence of discoveries iuch as those n the field of epigenetics. For an accessible discussion see especially Jablonska and Lamb (2005), Evolution in Four Dimensions. For a more detailed philosophical look at the evolutionary function questions, see discussions in the edited volume Sober and Orzack (2001). Fraley and Shaver also make a less helpful reference to a possible Haeckel-based explanation for the evolution of attachment differences: i.e. ‘ontogeny recapitulates phylogeny.’ For a definitive analysis of the inadequacies of the continuously popular approach of Haeckel, see Gould (1977). Nonetheless, despite these theoretical concerns the work is unparalleled.

277 Neoteny is defined as above, as the evolutionary delayed juvenile developmental period.
a partial look, the emergence of adult romantic relationships in human evolution would appear to be less an example of phylogenetic homology and more an example of behavioral homology constrained by unique human evolution.

8.3.3 Adult Pair Bonds and Cognitive Capacities

Regardless of evolutionary specifics, the presence of adult pair bonds demonstrated to be influenced by early experience (see especially contributions in Zayas & Hazan, 2015) may be the strongest argument for cognitively enhanced lifespan GCP. Indeed, in describing the uniqueness of human intersubjectivity, Rochat and colleagues have noted, “What makes the difference is reciprocity, which infants develop very early in life and that is the fundamental aspect of intersubjectivity all life long” (2009, p. 43). Such reciprocity is one of the hallmarks of the GCP and is captured in expanded notion of tertiary intersubjectivity. As would be expected, tertiary intersubjectivity emerges in period similar to Phase IV GCP. Rochat and colleagues note that from middle of the second year:

The child now begins to engage in active negotiation regarding the values of things co-experienced with others. They manifest tertiary intersubjectivity, a sense of shared experience that rests on complex on-going exchanges unfolding over time: things that happened in the past, are manifest in the present and are projected by the child into the future. (2009, p. 184).

This acknowledgment of the emergence of tertiary intersubjectivity—including first and second person linguistic reflective engagement—demonstrates how a first evolving GCP might also incorporate enlarged cognitive capacities. These skills are both applied and developed in lifelong engagement with GCPs.

Two final related considerations of attachment in adulthood in support of a GCP remain: a look at the possible impact of maladaptive development on attachment reflective capacities within GCP relationships and a closer look at mechanisms research on relationship specific IWMs in adulthood.

8.4 Cognitive collapse in BPD: The functional argument for GCP related capacities?

8.4.1 Attachment, BPD and Symptoms Relevant for a GCP

This functional discussion focuses on commonly recognised symptoms associated with Borderline Personality Disorder (BPD) (American-Psychiatric-Association, 2013) and similar non-DSM diagnoses—including in the more heterogeneous developmental diagnostic umbrella of complex trauma (e.g. Zilberstein, 2013). The DSM 5 defines BPD as “A pervasive pattern of instability of interpersonal relationships, self-image, and affects, and marked impulsivity, beginning by early adulthood and present in a variety of contexts” (American-Psychiatric-Association, 2013, Diagnostic Criteria 301.83 (F60.3) ). Especially relevant here is what has been described in the psychodynamic literature as the collapse of mentalisation or reflective functioning in times of distress (Fonagy & Luyten, 2009; Fonagy, Luyten, & Strathearn, 2011). Chapter Seven has argued that such functions might best be understood
developmentally in terms of the emerging Phase IV GCP. What may be of additional interest here are possible correlations between BPD and disorganised attachment (Levy, Beeney, & Temes, 2011). Equally, the presence of severe emotional dysregulation and interpersonal relationship issues such as enmeshment and fear of abandonment would also seem relevant for a discussion of a lifespan GCP. The section suggests that maladaptive attachment development including the GCP may well contribute to the loss of cognitive regulatory capacities often seen in BPD. I also suggest that a more comprehensive GCP is a more parsimonious and preferable explanatory option than approaches that strongly differentiate intersubjectivity from attachment.

Before continuing the section’s argument, a couple of qualifiers may be required. This discussion will remain at a relatively high level with a focus on the possible impact of early GCP development on later life. As such it is more directional than comprehensive. Next, BPD is not exactly the most stable of possible example disorders for consideration. Indeed, our current state of knowledge of personality disorders in general (Zachar & Krueger, 2013) and BPD in particular is hardly precise or satisfying: unexplained comorbidity, lack of developmental considerations, questions of validity are all examples of the poor state of our understanding of what may be a considerable portion of the adult population (Fonagy et al., 2011). So no endorsement of either general approaches to personality disorders or BPD is being made. Nonetheless, the possible role of attachment in personality disorder remains promising (Levy, Johnson, Clouthier, Scala, & Temes, 2015).

8.4.2 Reflective Collapse: Three Descriptions, One Phenomenon?

Attachment informed psychoanalytic approaches to understanding and treating BPD have made great strides. In reviewing the empirical literature, Levy concludes: “Research also suggests that having integrated and coherent representations, or developing such representations, buffers against many of the symptoms of BPD. Particularly important and interesting are the findings suggesting that a core deficit in BPD is the capacity to engage in reflective thinking and the reliance on reflexive thinking, particularly in situations that call for reflection” (2011, p. 56). Clearly, the ongoing development of capacities first emerging in within a proposed GCP would appear to be pivotal. Nonetheless, some of the very researchers noted in Chapter Seven offer divergent theoretical positions on attachment and its relationship to this collapsed symptom. What can be made of these? Regardless of the proposed explanation and role given to intersubjectivity, mentalisation and attachment, they would all appear to be acknowledging similar a phenomenon here.

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278 Four symptoms can be noted: two are relationship specific whilst another two reflect affect dysregulation: “Frantic efforts to avoid real or imagined abandonment”, “A pattern of unstable and intense interpersonal relationships characterized by alternating between extremes of idealization and devaluation”, “Affective instability due to a marked reactivity of mood (e.g., intense episodic dysphoria, irritability, or anxiety usually lasting a few hours and only rarely more than a few days)”, and “Inappropriate, intense anger or difficulty controlling anger (e.g., frequent displays of temper, constant anger, recurrent physical fights)” (American-Psychiatric-Association, 2013, Diagnostic Criteria 301.83 (F60.3)).
As noted, Cortina and Liotti (Cortina & Liotti, 2010a; Liotti, Cortina, & Farina, 2008) do not recognize a GCP: they have differentiated attachment from intersubjectivity but nonetheless see early maladaptation emerging across both ‘systems’ as precursors to BPD. Equally, Fonagy and colleagues (Fonagy & Luyten, 2009; Fonagy, Luyten, Allison, & Campbell, 2016; Fonagy et al., 2011) distinguish attachment and mentalisation—reflective functioning—but may nonetheless join the two in early maladaptation in the context of understanding later BPD. In both cases early attachment disturbance would appear to interfere with the development of later intersubjectivity and mentalisation respectively. Finally, Lyons-Ruth (2007) who endorses the GCP suggests similar relationships between attachment and BPD as well as a contribution to cognitive collapse. But in her case the theoretical approach calls for only one integrated system that would also approximate my proposed GCP. I have not yet introduced her general position. She appeals to the same evolutionary writers as her theoretical rivals but arrives at a different perspective: “As Tomasello’s and Hobson’s arguments make clear, the more flexible adaptive mechanism of sharing mental states with others reframes our older, more highly channeled biological evolutionary heritage” (Lyons-Ruth, 2007, p. 603). This is similar to the argument made above in Section 8.2

The question of a GCP and developmental injury may rest in part upon how one understands an attachment relationship. For both Liotti and colleagues, and Fonagy and colleagues, the attachment relationship hardly seems a real, ongoing evolving parental relation that potentially spans a lifespan. Perhaps a return to the early language of Bowlby and Ainsworth might be useful, here. Child Care and the Growth of Love (Bowlby & Ainsworth, 1965) was the title of the popular version of the original 1950s WHO Report Attachment. Attachment was discussed as a relationship motivated by parental love. However, the two dissenting psychoanalytic positions would seem to accept the emergence of early attachment representations, not as part of ongoing evolving caregiving relationships, but rather as functional requirements for safety and foundations for future intersubjective capacities and mentalisation.

All three positions accept the symptoms and an etiological connection with attachment disorganisation. And in the end, all three probably go about treatment in relatively similar ways. So there may be less difference than the theoretical approaches might claim when it comes to cognitive collapse. Indeed, perhaps in the world of phenomena the theoretical differences may be less import. But it would appear that only Lyons-Ruth accepts the early relationship as a vital relationship that might indeed grow over time as the original GCP was.

279 On the one hand, I am sympathetic to Lyons-Ruth endorsement of an integrated approach to a GCP. On the other hand, I may choose to partially defer on her general rejection of the homeostatic/homeorhetic nature of an attachment system within a larger set of motivational systems. Indeed, Bowlby’s ideas for a GCP and the broader organizing role of a developed attachment system in the operation of other systems might allow one to not have to choose. Intersubjectivity as a sort of background steady state might fit well with attachment security as a sort of background affective mood identified as the starting position or state in attachment patterns in Chapter Four.

280 Indeed, Gopnik (American-Psychiatric-Association, 2013, Diagnostic Criteria 301.83 (F60.3) ), the cognitive developmental researcher, has suggested attachment might be considered a ‘theory of love’, possibly as a subset of broader conceptions of Theory of Mind (TOM).
intended to convey. Such a position would also seem to better support a lifespan approach that sees both regulatory capacities and close relationships as past, current and ongoing attachment experience. Lyons-Ruth response to the differences seems to be plea for the case that researchers not miss the obvious about the attachment tie construct. "In summary, I would argue that current attachment research needs to be expanded in three critical directions: first, from an emphasis on mechanisms of physical proximity and protection to an emphasis on mechanisms of intersubjective exchange; second, from an emphasis on processes of comfort and soothing to an equal emphasis on processes involved in maintaining the baby’s positive engagement with others; and, finally, from an emphasis on separation and loss events to an equal emphasis on the more continuous relational processes that regulate both positive engagement and the baby’s fearful responses to challenging events" (Lyons-Ruth, 2007, p. 601). I now turn to her first request, mechanisms of intersubjective exchange in adults.

8.5 Mechanisms Underpinning GCPs: Adult Romantic Relationships

A last argument in favour of a more dynamic lifespan approach to the development of GCPs turns to the envisioned mechanistic role of IWMs, especially in specific adult romantic relationships. Initial note can also be made of two additional attachment hypotheses relevant for IWMs and later post-language development of attachment for lifespan development. Operating from a more theoretical-clinical setting, J. Holmes (2001) he has sought to connect early later attachment phenomena in a potentially more astute fashion with later attachment experience. In some ways, he moves the positions of both Main and Fonagy in the direction of the GCP position I am taking. J. Holmes (2001) has added both a ‘narrative competence hypothesis’ and ‘mentalisation hypothesis’ to attachment theoretical propositions. These two hypotheses overlap. The first reflects the role of autobiographical narrative coherence captured in Main’s AAI representational approach to measuring adult attachment noted above (Main, Hesse, & Goldwyn, 2008). Mentalisation arises in the views just discussed in Chapter Seven and Section 8.4, i.e. Fonagy and colleagues’ (2011) proposals and research with ‘reflective function’, whose differences paralleled those seen for attachment in the AAI (Fonagy & Target, 1997). Both the narrative and mentalisation suggestions are relevant for understanding adult IMWs.

Two types of arguments will be constructed in this last section: one more conceptual, the second based on experimental research. The first introduces Zayas and colleagues’ recent Personality-in-Context (PiC) Approach: “a social-cognitive metatheory that provides a

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281 One obvious question here concerns the development of new attachment relationships by young children, especially in cases of adoption and foster care where early experience was missing or maladaptive. Dozier and Rutter (2009) have surveyed the longitudinal study that documents the potentially poor effects of such experiences on young children. Nonetheless, “Catch-up is seen across domains, showing dramatic physical, cognitive and social growth upon placement in families . . . remediation is often possible where children have loving, supporting caregivers”. Normative approaches to development—attachment included—must account for such delays and catch-ups. A more dynamic lifespan GCP could probably account for such. However, it would also seem hard to imagine how such later developments might fit into the sequelae envisioned by the two positions I seek to modify.
framework for understanding how two individuals go from being strangers—a stage in which their lives and minds are independent and asynchronous—to a full-fledged pair bond—a stage in which the lives and minds of both individuals are intricately intertwined" (2015, p. 159). Thereafter, causal evidence emerging from security priming experiments will be called upon to argue for a cognitively constitutive component in adult GCPs. Conscious interaction with ‘security scripts’ have been experimentally shown to cause changes in real time experience. Together the two offer a compelling case for retention and expansion of the GCP. But first, I will offer a brief picture of evidence for the more general lifespan IWM.

8.5.1 IWMs: Two Traditions in Tension

Thompson (2008) has noted how Bowlby’s integration of mental mechanisms remained incomplete. Two renderings of IWMs traditions existed and have remained in tension with one another: one psychoanalytic (including object relations), the other cognitive psychological. From a psychoanalytic perspective, “IWMs emerge early in the processes by which close interaction with a caregiver is interpreted through the perceptual-affective schemas of infancy, creating prelinguistic models that can have enduring influence but are largely inaccessible to conscious reflection” (Thompson, 2008, p. 349). The cognitive story suggests “IWMs are more sophisticated, consciously accessible, and explicit: they are tested against the evidence of experience, evaluated for internal consistency and the accuracy of their predictions, and used to evaluate, represent, and retrieve further information” (Thompson, 2008, p. 349). An adequate rendering of adult IWMs must retain both. Such integrated perspectives should ideally also acknowledge the more current findings on emotion-cognition integration discussed above.282

8.5.2 Personality-in-Context (PiC) Approach

Hazan, Zeifman and colleagues (2015; 1997, 2016) have importantly transferred Bowlby’s four normative phases of infant attachment development into adulthood as a preattachment phase, attachment-in-the-making phase, clear-cut attachment phase and the goal-corrected partnership phase. The adult sequelae have provided one the key arguments for a more dynamic lifespan notion of a GCP. Zayas and colleagues (2015) have also noted how little work has been done on understanding the development of relationship specific internal working models. In addressing this gap, they have engaged the Personality-in-Context (PiC) Approach. “PiC is a social-cognitive metatheory that provides a framework for understanding how two individuals go from being strangers—a stage in which their lives and minds are independent and asynchronous—to a full-fledged pair bond—a stage in which the lives and minds of both individuals are intricately inter-twined” (Zayas et al., 2015, p. 159). They address interesting questions of how an unknown figure becomes a person coupled in one’s

282 Note might be made here of psychosomatic work that reflects where such a tension within body and mind are approached as one complex integrated phenomenon. Tennant and McLean’s (2016) work linking somatic and psychological risk—i.e. the impact of emotions on heart risk—might apply here. McLean and colleagues (2001) have also extended this work to include attachment-based therapeutic considerations in psychiatric settings.
mind. Although their more innovative contribution may be the identification of a range of relationship turning points that can be tracked across relationship development—“times during which significant learning about the relationship partner, the self, and the self-in-relation-to-partner occurs” (Zayas et al., 2015, p. 165)—, I will highlight a few different but relevant findings for this discussion of a GCP.

Their approach represents an application of what has been termed a Cognitive-Affective Processing System or CAPS network. Within such an approach, IWMs are depicted as Cognitive Affective Units (CAUs) and encompass similar elements to those presented by others in Subsection 4.6.1. “CAUs represent affective reactions (basic evaluations of goodness or badness to full-blown feelings and emotions), encodings (schemas and categories of self, others, events, and objects), expectations and beliefs (about the world and likely outcomes in particular situations), abstract goals (desired and undesired outcomes, goals and life projects), and competencies and self-regulatory plans (behavioral scripts that organize action)” (Zayas et al., 2015, p. 160). One of the values of this approach is in its ability to account for both interindividual global attachment difference and intraindividual specific attachment relationship differences. Indeed, this approach would seem to provide a sophisticated theoretical model that might encompass my proposals for both a DLPC in Chapter Six and the GCP in Chapters Seven and Eight.

The growth in specific romantic attachments ultimately results in a dyadic connection of two individual’s unique CAPS systems. “Once the mental representation of each partner is formed, an individual may be more likely to engage in top-down, schema-driven processing (rather than bottom-up, stimulus-driven processing) and consequently, may interpret the behaviors of her partner as consistent with the schema. Over time, we expect that two individuals will form a dynamic dyadic system that generates stable and predictable patterns of behavior” (Zayas et al., 2015, p. 164).

The PIC approach seems to particularly hold well the implicit - explicit tension in IWMs noted by Thompson above. Indeed, Zayas and colleagues suggest that in adult GCP development Phase III and Phase IV—i.e. clear-cut and goal corrected phases—are difficult to separate and may occur simultaneously. They argue that unlike developing infants who initially experience the more bottom up driven attachment in Phase III, adults will possess both sets of bottom up and top down capacities for coping with possible separation (Zayas et al., 2015, p. 174). This would appear to confirm the dynamic lifespan interaction between phases proposed in Chapter Seven. I now turn to experimental evidence emerging from social psychology that would appear to also confirm my proposals for a GCP.

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283 They note CAPS has emerged from the innovative personality work of Mischel and Shoda (2013) that sought to introduce a cognitive-affective model that embraced interactions between personality invariance and situational context contextual and was updated in a consideration of close relationships by Zayas and colleagues (1995). “We illustrate this approach to personality-in-context with a hypothetical scenario and use this framework to organize research on attachment styles, rejection sensitivity, self-fulfilling prophecy, the self in relation to others, and interdependence theory.”
Security priming experimentation has emerged in social psychology over the past two decades following the initial development of self-report measurements for adult security beginning in the mid 1980s. Mikulincer and Shaver (2015, p. 18) have recently observed how this research has importantly established “the evidence for the psychological reality of the secure-base script in adulthood”, one of the conceptual depictions of underlying IWMs. Indeed, they have also noted the health benefits associated with these investigations. Improvements—for the moment temporary in nature—have been identified in terms of general well-being, interpersonal and prosocial interactions (Mikulincer & Shaver, 2007). However, for present purposes, it is the specific interpersonal interventions within adult romantic relationships that provide support for my proposals for an expanded GCP.

What exactly is security priming? Mikulincer and Shaver (2007, p. 143) have described these interventions as “temporarily activating mental representations of attachment figures” for some investigative purpose. Indeed, at least four types of well-validated priming or activation have been employed:

1. Subliminal presentation of pictures suggesting attachment-figure availability (e.g., a Picasso drawing of a mother cradling an infant in her arms; a couple holding hands and gazing into each other’s eyes);
2. Subliminal presentation of the names of people who were designated by participants as security-enhancing attachment figures;
3. Guided imagery concerning the availability and supportiveness of an attachment figure; and
4. Visualization of the faces of security-enhancing attachment figures.

Two are implicit and two are more explicit. Figure 8.2 describes how primes may impact both representations of general attachment capacities and specific relationships.
Mikulincer and Shaver (2015, p. 19) suggest, “secure people hold a reservoir of positive cognitions that help them remain relatively stable and calm when coping with stressful events.” They have also summarised expanding research on health outcomes from security priming with adults. These include: ‘Improvement in mood’, ‘improved tolerance for physical pain’, mitigation of “a well-known cognitive manifestation of post-traumatic responses”, “to affect the decision to help or not help a person in distress”, “led to greater compassion and willingness to help even when there was no egoistic reason for helping” “greater supportiveness toward a dating partner who was sharing a personal problem or exploring personal goals” and “effective caregiving” (Mikulincer & Shaver, 2015).

Of particular relevance for the GCP position are the cognitive-affective explicit studies, which lend support to constitutive role for cognition in attachment. Mikulincer and Shaver (2015) have identified three recent studies conducted between 2009 and 2011—(Eisenberger et al., 2011; Master et al., 2009; Younger, Aron, Parke, Chatterjee, & Mackey, 2010)— that demonstrated the viewing the photograph of romantic partner picture reduced experienced pain—introduced via heat stimuli—, measured both neurally and subjectively. Equally, exposure to security priming words results in changes in romantic partner expectancies—i.e. increased reliance on partner (Pierce & Lydon, 1998). This experimental evidence provides support for the notion of a cognitively enabled adult attachment GCP, something alterable via explicit routes. As such it serves as the final supporting point for the proposed GCP.

### 8.6 Chapter Conclusion

As noted in Chapter Six, attachment is increasingly seen as in integrated phenomena: cognition, affect and behaviour. The expanded dynamic lifespan GCP presented in Chapter Seven and argued for in Chapter Eight has been shown to display just such integration. The present chapter has assembled evidence from emotion research, evolutionary study, forms of intersubjectivity, clinical considerations in BPD, developmental study of adult romantic attachments and recent investigations into IWMs to argue that cognition indeed plays a constitutive role alongside emotion in attachment phenomena. The chapter has engaged Bowlby’s TLC method to update perspectives on the lifespan dimension of GCP. Liotti and colleague’s proposals for a ‘Phase III only’ attachment captured in what I have described as a ‘phylogeny GCP rejection’ would appear to not fit well with this more developed articulation of Bowlby’s GCP. Although many—including this writer—may agree with their identification of intersubjective and cognitive collapse in BPD, as well as a developmental connection to disorganised attachment, it is their less integrative, phylogenetic position that may lose support. Although I have been emphasising developmental complexity throughout the thesis, I will conclude suggesting that the GCP might be better conceived in a more parsimonious manner than suggested by Cortina and Liotti. The GCP is better seen as supported by one integrated process that constitutively includes emotion, cognition and behaviour.
CHAPTER EIGHT REFERENCES


Bowlby, J., & Ainsworth, M. D. S. (1965). *Child care and the growth of love. Based ... on the report "Maternal Care and Mental Health"* (M. Fry Ed. 2 ed.).


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Zayas, V., Günaydin, G. I., & Shoda, Y. (2015). From an Unknown Other to an Attachment Figure: How Do Mental Representations Change as Attachments Form? In V. Zayas & C. Hazan (Eds.), Bases of adult attachment: Linking brain, mind and behavior (pp. 157-185). New York: Springer.


Chapter Nine: Conclusion

The clinical implication emerging from all these diverse areas of research is that fostering more collaborative forms of communication may lie at the heart of evolutionary change, developmental change, and changes resulting from psychodynamic psychotherapy (Lyons-Ruth, 2007, p. 600).

9.1 Massive Expansion Requires Greater Conceptual Precision

This study began acknowledging the incredible growth in research across all domains and lifespan periods in the field of attachment. I might add that the past 24 months leading up to submission of this thesis—roughly November 2014-November 2016—has been particularly productive. Particular note can be made of Zayas’ 2015 volume, Bases of Adult Attachment: Linking Brain, Mind and Behavior. Many of the conceptual questions that initially presented themselves over the seven years of this study can be identified in the introduction’s focus on development of attachment in adulthood: an organisational perspective, goal-corrected partnerships, complex multi-level approaches to mechanisms, longitudinal study, IWMs and so forth. Not only does the volume represent a more integrated approach between the developmental and personality/social psychological traditions in adult study, the work demonstrates a greater degree of granularity, clarity of insight and the emergence of new questions to join some ongoing ones. Nonetheless, adult change in attachment as a consequence of psychotherapy is noticeably absent, no doubt a partial reflection of the researchers’ home disciplines. But this is possibly due to the relatively less sophisticated state of therapeutic study in general and of attachment’s role in therapy in particular. However, the enhanced precision present in the studies is truly exciting as that is what is increasingly required as the field expands.

9.2 Contribution of this Thesis

In some ways this work might be described as a Philosophy of Attachment: a critical engagement with the full research programme, including its guiding conceptual/theoretical positions. At the same time this engagement has also benefited from the inclusion of an historical perspective. Finally, efforts at both rearticulation and reorientation have also contributed to a renewed focus Bowlby’s increasingly influential lifespan or ‘cradle to grave’ perspective.

The three-part input represented by this thesis should hopefully contribute to a continual requirement for renewed coherence in Attachment Theory. Part One has delivered two insights about Bowlby’s goals and methods. His radical overhaul of psychoanalysis has been partially successful: his ideas have been embraced in most places and have also helped to spawn important psychoanalytic developmental research of infancy. However, gaps may remain between his commitment to empirical investigation and those places in clinical theory whose
developmental assumptions have yet to be grounded in appropriate developmental study. Bowlby’s parallel turn to both animal biology (ethology) and a commitment to philosophical naturalism resulted in a method well ahead of its time. The TLC strategy argued for the integration of behaviour, affect and cognition in a lifespan developmental model, ideas increasingly accepted across developmental science.

Part Two brought a degree of clarity to an at times confusing field of attachment. The 13 attachment constructs within a Tinbergen framework offered an original—or renewed—look at the theory more generally. This framework has not been employed for Attachment Theory in quite this way. One additional source of confusion was clarified through an historical survey of the theory’s development. Bowlby’s ideas changed over time but unfortunately the subtlety in those shifts are not always recognised by proponents and critics alike. A systematic engagement with an attachment organisational perspective suggested how attachment theory might be aligned with more systematic approaches to complexity: Developmental Systems Theory (DST). One creative output from this exercise was the re-identification of an attachment global capacity as Dynamic Lifespan Personality Construct (DLPC), suggesting how such a construct might fit within the field of personality study.

Part Three focussed in on Bowlby’s ideas for a cognitively enhanced Goal-Corrected Partnership (GCP) sub-construct of the ontogenetic construct attachment relationship or tie. Instead of viewing this solely as a Phase IV that succeeds a prior Phase III and spans 2-5 year age, the GCP was rearticulated as reflecting more dynamic lifespan phenomena. Several contributions from emotion research, evolutionary study, clinical considerations of cognitive collapse in adult BPD, and the development and mechanisms associated with adult romantic relationships were provided to make a case for the lifespan GCP. In the end, attachment phenomena are best considered as integrated behaviour, emotion and cognition.

9.3 Implications for Psychiatry and Psychotherapy

Throughout the thesis, there are examples of where and how attachment study might be of relevance for philosophy of psychiatry, psychiatry and psychotherapy. These included notions of risk and resilience in mental health and more specific questions for nosology, especially possible implications for the study of personality disorders. First, and most notably here is the potential value of the Tinbergen framework and the four questions it poses. Indeed, the four questions might valuably used to clarify current and ongoing issues. Additionally, three more specific implications may be identified: focus on causation, employment of lifespan development and an enhanced role for emotion research. each of these areas are arguably sorely missing in in both adult psychiatry and psychotherapy. Attachment brings a proven track record for engaging questions of mechanism and causation; this would apply to matters such as developmental
antecedents, difference as risk, current operations of IMWs or a possible role for oxytocin in attachment processes. These potential causal contributors also bring with them a more complex developmental approach as well. However, current nosology as evidenced in the last three DSMs would appear to have a dual aversion to both causation and development. Attachment Theory offers great promise here. Finally, Chapter Eight sought to update understandings of emotions for Attachment Theory in the context of a GCP. A last reflection here might sound to be a harsh one. But a case can be made that psychiatry and the multiple modalities practiced in the fields of psychotherapy may have become emotionally illiterate. There would appear to be little evidence that current insights from emotion research have been actively engaged in either nosology or the sketchy models of the mind and emotions underpinning treatment approaches. As a consequence we are left with duelling folk theories of emotion and mind. Which brings us to Bowlby’s last goal for overhauling psychoanalysis, one not specifically addressed in this thesis: attachment as a platform for conducting psychotherapy.

9.4 What’s an Attachment-informed Psychotherapy? Unavoidable Requirement for Greater Coherence

In Chapter Five I suggested attachment theory development had entered a period of maturity with the publication of Bowlby’s great theoretical trilogy (1969/1982, 1973, 1980) and the introduction of Ainsworth (Ainsworth, Blehar, Waters, & Wall, 1978) inspired SSP experimentation. Perhaps that maturity might in hindsight be better described as the period of younger adulthood of the theory. And perhaps the Zayas collection might represent not just an expansion, but rather a possible type of theory early middle age. Or perhaps a better metaphor is required? Possibly the notion of an asymptotic horizon?

Whichever the metaphor, Bowlby’s last position in his overhaul outlined in Chapter Two concerned the practice of psychotherapy. My original but delayed point of interest. Like most of things to do with Attachment Theory, this too is rapidly expanding. First, there are indeed a great many adaptations of Attachment Theory for support in therapeutic interventions: for infants and caregivers, children, adolescents, couples and families. No doubt attachment has also been taken up within numerous modalities. For example Taylor and colleagues (2015) have recently described empirical evidence for changes in attachment within adult therapy. Indeed, Part V of the latest edition of the Handbook (Cassidy & Shaver, 2016) contains 7 chapters devoted to “Psychopathology and Clinical Application” covering the full lifespan and a range of modalities. Additionally, two volumes in The Routledge Attachment Handbook are devoted to Assessment and Implications and Interventions, respectively (Holmes & Farnfield, 2014a, 2014b). I am also aware of local adaptations of attachment for practitioners including those within psychiatry

Scarantino (2005) has argued convincingly for the folk psychological status of most approaches to emotion. My observation here is a logical extension of his conclusion.

284 Scarantino (2005) has argued convincingly for the folk psychological status of most approaches to emotion. My observation here is a logical extension of his conclusion.
(Kozlowska & McLean, 2005, 2006). However, limitations in the clinical application of the theory have also been identified, suggesting a degree of caution as to the adequate translation of theory into practice (Allen, 2011a, 2011b; Zilberstein, 2013). Grounding a therapy within as vast and complex theory takes on the same risks identified by Sroufe at the outset of the thesis: overreach and trying to explain everything by one theory.

Let me offer one case. As noted in my Introduction on popular conceptions of attachment, one may regularly come across discussions of ‘attachment trauma’ without a great deal of clarity as to the contents of either word. I have demonstrated in this thesis the inherent challenges when employing the first term. Unfortunately, although youngsters and adults clearly suffer traumatic experience, I would argue that we desperately lack an adequate articulation of the term. Their suffering deserves our best response. Nonetheless, approaches to therapy that seek to address ‘attachment trauma’ are relatively common. A range of valuable well informed approaches to attachment in therapy that seek to wrestle with trauma can easily be identified. However, theoretical rigour may not be widespread. As a colleague has mentioned with a degree of mild frustration about book writing in psychotherapy, just put the words ‘attachment’ and ‘neuroscience’ in the title and it is guaranteed to sell! As greater funding goes into addressing childhood development, including attachment experience, there would appear to be pressing need to clarify what might count as an attachment informed therapy. As noted several useful attempts have already been engaged but a more systematic approach that might embrace the range of constructs identified here in light of distinct lifespan periods would appear helpful. Equally, the inclusion of more current knowledge of emotion research as suggested for the GCP would appear crucial as well. Finally, I have been made aware (personal member communication, September 2016), that the Society for Psychotherapy Research (SPR) has convened an ongoing Interest Section on Complexity Science. Attachment as articulated in this thesis would certainly provide support for such explorations.

9.5 A Last Word about ‘Words’ and Coherence

When seeking to bring greater coherence to unhelpful dichotomies in the discussions of complex human development, Oyama (2001) titled an article suggesting counsel to philosophers: “Terms in tension: What to do when all the good words are taken”. Although Attachment Theory struggles with many of the same issues in describing human development, it does not lack for ‘good words’.

285 Fortunately, the days of employing the potentially harmful Holding Therapy as a prescribed attachment intervention have mostly passed due to vigilant educational efforts (Steele, 2003).
286 I am a student member of this professional society. I was pleased to receive this notice and am convinced it is an appropriate step for psychotherapy research. In 2013 at an SPR conference, I previously proposed in a presentation, how Attachment Theory might serve a possible ‘conceptual/causal’ bridge between the two competing empirical psychotherapy research platforms in the APA (American Psychology Association): Evidence Supported Therapies (EST) and Common Factors Research. See discussion of these two positions in a Special Issue of Psychotherapy, beginning with commented target article by Laska and colleagues (2014).
Bowlby selected an excellent metaphor for describing attachment phenomena! However, applications of the now popular term ‘attachment’ may also suffer the same clinical colloquial challenges that Dell (2009) has identified for the term dissociation: it’s a noun, and adjective an intransitive and transitive verb. Indeed, the same employment is the case with Attachment Theory. Two of the most commonly employed attachment uses in clinical parlance are probably as nouns: a capacity (or style) and a relationship. However, attachment may fortunately be in an arguably more coherent place. Attachment Theory is probably more robust than discussions of dissociative phenomena—something that may lack an adequate supportive theory. From the emerging perspective of contemporary biology, attachment is both an entity and a process. Hopefully, this re-articulation of a DLPC and a GCP may have better connected these constructs with more process envisioned, dynamic lifespan attachment phenomena, adding additional coherence to the theory along the way.
CHAPTER NINE REFERENCES


