Evolutionary Explanation in Psychology and the Development of Joint Attentional Capacities

by

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Abstract

Many psychologists are interested in the development and evolution of psychological capacities. The neo-Darwinian evolutionary paradigm serves as a metatheory in psychology, structuring evolutionary and developmental claims. These claims are often adaptationist claims, meaning psychological capacities are seen as naturally-selected adaptations and their development in individuals is causally connected to these adaptations. After Lewontin and Gould’s (1979) well-known critique of adaptationism, the problematic “strong” adaptationist explanations in the biological sciences all but disappeared. In this dissertation, I argue that evolutionary explanations in psychology are still of this problematic “strong” variety. In the following I review the claims of psychologists who study the development of early social understanding and argue that a strong adaptationist stance misconstrues the nature of psychological capacities and their development. Few psychologists recognize that the Modern Synthesis lacks a model of development, hence, problems arise when a non-developmental evolutionary metatheory inappropriately informs developmental models in psychology. I argue developmental psychologists, in spite of the current non-developmental evolutionary metatheory, can improve upon their research by shedding adaptationist assumptions and adopting a pluralistic perspective on evolutionary explanation.

Joint attention is an important capacity that is often thought to be an adaptation important for the development of uniquely human capacities. I argue adaptationist-oriented researchers have insufficiently accounted for its development. I thus present two studies that examine the development of pointing and point following, two important joint attentional capacities, in human infants. In Study 1, I use parental diary data to examine the development of pointing in infants and present an account of its development which contrasts greatly with the adaptationist accounts of other joint attention researchers. In Study 2, I examine the development of point following in 9- to 12-month-old infants. I found that infants improved in point following at a steady rate and that mothers’ verbally directing infants’ attention at 9 months was predictive of infants’ point following ability between 9 and 12 months and infant language production at 12 months.
Keywords: evolutionary metatheory; neo-Darwinism; adaptationism; joint attention; infant pointing
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<tr>
<td>DST</td>
<td>Developmental Systems Theory</td>
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<td>EP</td>
<td>Evolutionary Psychology</td>
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<td>Evo-Devo</td>
<td>Evolutionary-Developmental Biology</td>
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<tr>
<td>ToM</td>
<td>Theory of mind</td>
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<td>SSSM</td>
<td>Standard Social Science Model</td>
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Introduction

When Tomasello and Carpenter (2007) aptly noted “Human cognition seems very different” (p. 121), their actual position echoed the common sentiment that human cognition seems uniquely different, and many psychologists are interested in determining what capacities make this so. Language and culture are obvious examples that come to mind when one tries to characterize what makes humans so different from other organisms (see Dobzhansky, 1963; Laland & Janik, 2006; Tomasello, 1999), although some argue humans only differ in the degree to which these aspects characterize human life (e.g., Savage-Rumbaugh, Shanker, & Taylor, 1998). Humans also collaborate with each other to a greater extent than other animals (Tomasello, 2014). A high degree of social understanding is thought to make such collaboration possible. Indeed, this “mental state understanding” is what grants human beings the title of being the world’s experts at mind reading (Tomasello, Carpenter, Call, Behne, & Moll, 2005, p. 675), making human social understanding the benchmark for comparative studies of social understanding in other species (i.e., Premack & Woodruff, 1978). This high level of mental state understanding is said to emerge between three and five years of age in humans, the age at which children can pass a “false belief task” (Wellman, Cross, & Watson, 2001). This task assesses whether or not a child understands that other people hold beliefs and act on them even when their beliefs, unbeknownst to the belief holder, do not accord with changes in reality.

Prior to false belief understanding, social understanding develops throughout infancy. Joint attention, the ability to coordinate attentional states with others, is often taken to be the earliest and clearest indications of an understanding of other minds (Racine, 2012; Tomasello, Carpenter, & Liszkowski, 2007). Although certain joint attention skills are shared with other primates (Call & Tomasello, 2008; Tomasello, Call, & Hare, 2003; Tomasello & Herrmann, 2010), collaborative forms of joint attention, such as pointing to share attention or provide information for another, have been argued to be exclusive to humans (Tomasello et al., 2005, 2007; Tomasello & Herrmann, 2010). Joint
attention also has garnered much attention for its theoretical and empirical links to other capacities. For example, joint attention is linked to language development (Baldwin, 1993; 1995; Bates, Camaioni, & Volterra, 1975; 1976; Bickerton, 2005; Butterworth & Morissette, 1996; Colonnesi, Stams, Koster, & Noom, 2010; Morales, Mundy, & Rojas, 1998; Mundy & Gomes, 1998; Tomasello et al., 2005 but see Slaughter & McConnell, 2003) and is a precursor to other social cognitive capacities including false belief understanding. Although other primates understand intentional action and perception, it is argued that humans have a species-unique adaptation that transforms understanding about attention and intention into human-specific forms of collaboration, cooperative communication, and joint attention (Tomasello et al., 2005, 2007).

Recently, many “core knowledge” researchers have been producing empirical evidence that has led many to question the standard picture of social cognitive development, in some ways, calling into question the importance of foundational capacities such as joint attention. These researchers have moved the timing of the emergence of high level psychological capacities to very early in infancy. Several studies using non-verbal tasks have found that infants in their second year of life exhibit false-belief understanding (Onishi & Baillargeon, 2005; Surian, Caldi, & Sperber, 2007; see also Buttelmann, Carpenter, & Tomasello, 2009) and it has been recently argued that infants exhibit false belief understanding as early as 10 months of age (Luo, 2011). This latest figure is well before the age that many infants even begin to engage in joint attention activities like pointing (Carpenter, Nagell, & Tomasello, 1998), and far before the age at which most children can pass verbal false belief tests at the average age of four.

In the context of understanding the development and evolution of social understanding, attributing high level psychological capacities to infants is not uncommon in psychology. Developmental psychology is rife with claims about “innate” capacities that infants are born with, or capacities that have early onsets, that contribute to the development of higher level social cognitive capacities. In this respect, core knowledge is a recent incarnation of these kinds of claims. Although it is important to recognize that epistemological metatheoretical issues are important to be aware of in psychology (see Overton, 2006), attempting to label these sorts of claims as “nativist” belies the complicated nature of these issues in psychology. Not only is the concept of “innate” not
clearly defined (see Bateson & Mameli, 2007; Mameli & Bateson, 2006), nativist claims about the “innateness” of certain psychological traits, for example, are commonly put forward by developmentalists who do not consider themselves to be nativists (see Wereha & Racine, 2012). What seems pertinent is to examine how these claims seem to be attaining their validation. I suggest that this validation is, in part, provided by modern evolutionary theory and popular interpretations of it. Claims about innateness amount to a particular way of explaining development; that is, as seeing developmental outcomes as predetermined by evolutionary adaptations. In this dissertation, I argue that a particular approach to understanding and studying evolution called adaptationism has come to inform psychology upon both evolutionary and developmental matters.

Adaptationism is a perspective based upon neo-Darwinist evolutionary theory (also known as the Modern [evolutionary] Synthesis) that prioritizes establishing which organismic traits are attributable to naturally selected adaptations. Evolutionary Psychology is a paradigmatic example of an adaptationist program. From this perspective, current psychological capacities (phenotypes) are adaptations that were naturally selected in a species’ evolution and reliably develop in the life course of the organism. Claims of innate capacities or adaptations continue to maintain their popularity because they seemingly explain the developmental and evolutionary origins of our psychological capacities. Furthermore, often in developmental psychology “innate” capacities are thought to be these adaptations.

A main purpose of this dissertation is to examine the nature of the evolutionary claims made in psychology in general and in developmental psychology in particular. I argue that adaptationism, and the neo-Darwinian modern evolutionary synthesis that it

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1 This is not to say that epistemological issues do not exist in this case, as one’s epistemological leanings can surely influence how one interprets and to what ends a person employs theory and methodology; however, the more specific problem of how evolutionary metatheory influences understanding of development and evolution in psychology is the focus of this dissertation.

2 Although there is considerable discussion regarding the issues surrounding adaptationist explanation, following largely the well-known critique of Gould and Lewontin (1979), developmental psychologists are arguably not well versed in these debates, and even less attention is paid to evolutionary metatheory in psychology. Moreover, their critique is still worth considering explicitly because even though it may have had an impact on evolutionary biology, it has not had the same impact on evolutionary thinking in psychology.
is based upon, provides a particular kind of evolutionary explanation that is insufficient for explaining psychological capacities. What is required is thought about the nature of our evolutionary explanations, what a focus on evolution can and cannot tell us, and how we can go about providing a part of these explanations in psychology. One of the main issues lies in the fact that the neo-Darwinian synthesis was formulated in population-genetics where biologists study allele distributions in populations, not individuals and how they develop.\(^3\) Although a population-genetic approach is sufficient for certain applications (i.e., studying evolution limited to tracing allele distributions), the problem is that this metatheory has come to inform disciplines such as psychology on both evolutionary and developmental issues. For psychology, this has led to perpetuating predeterministic notions of development and attendant innatist and adaptationist explanations that, as I attempt to demonstrate in this dissertation, are problematic.

In Part 1 of this dissertation I examine the notions of evolutionary explanation, review evolutionary approaches in psychology based on an adaptationist perspective, and critique the methatheoretical evolutionary structure in psychology. I will use Evolutionary Psychology as an illustrative example of an approach that explicitly adopts a “strong” adaptationist perspective in psychology. After explaining the problems with the term “innate,” I will then describe alternative evolutionary perspectives that may be more appropriate for use in developmental psychology in particular and psychology in general.

In Part 2, I conduct a broad review of social cognitive research. Researchers are interested in earlier (i.e., intersubjectivity, joint attention) and later forms of social cognition (i.e., false belief understanding) as aspects of an understanding of mind. Although joint attention is considered to be foundational to explicit false belief understanding, I will review the claims of core knowledge and early theory of mind theorists who contend that implicit false belief understanding can be seen in infants in the first year before joint attention develops. I will analyze these claims in the context of their adaptationist commitments. Because of the problematic nature of “implicit” false

\(^3\) Perhaps confusion has existed between conflating evidence for Darwinian evolution (which comprises very many different converging lines of evidence across numerous disciplines) with the population genetic neo-Darwinian metatheory per se. Furthermore, the apparent success of neo-Darwinian metatheory may also be a function of psychologists not being privy to the discussions and debates that exist in evolutionary biology about the paradigm itself (Wereha & Racine, 2012).
belief understanding, and the unambiguous importance of joint attention, in Part 3, I take a closer look at joint attention research. Although researchers have recently noted that it is surprising that little is actually known about the development of pointing, I will go beyond this observation and attempt to explain why this is the case. I then use entries from a diary study to problematize the standard picture of pointing development and argue that the origins of pointing can only be understood through developmental analysis. Part 4 consists of a study of the development of point following in 9-12 month old infants.
1. **Evolutionary Metatheory and Adaptationist Explanation in Psychology**

1.0. **Evolution and Psychology**

It is perhaps underappreciated how profoundly the neo-Darwinian Modern Synthesis of evolution has influenced, and continues to influence, psychology. In some ways however, evolution seems to be a neglected topic, so much so, that some have characterized psychology as a science that has ignored evolution to its peril.\(^4\) Psychology, it is claimed, is not alone in this regard. Such a sentiment is said to be shared by other allied disciplines, leading some to refer to this position as the Standard Social Science Model (SSSM) (Shackelford & Liddle, 2014; Tooby & Cosmides, 1992) that assumes the human mind is largely a product of cultural learning. Although the validity of the SSSM critique will be discussed to some extent in the following sections, it echoed at the time the charge was levelled, a frustration over what was taken to be psychology’s neglect of evolutionary issues in conceptualizing why humans think and behave as they do. Moreover, to a particular group of critics under the banner of

\[\text{\footnotesize\(^4\) In the Evolutionary Psychology manifesto The Adapted Mind, Tooby and Cosmides (1992) argue psychology is not “scientific” and that making it so entails accepting the paradigms that structure what in their eyes are more reputable disciplines. Of course, the goal of using such rhetoric was in the aim of convincing psychologists to accept their proposition that psychology should be structured by evolutionary theory. Their stance is worth quoting in full: “After more than a century, the social sciences are still adrift, with an enormous mass of half-digested observations, a not inconsiderable body of empirical generalizations, and a contradictory stew of ungrounded, middle-level theories expressed in a babel of incommensurate technical lexicons. This is accompanied by a growing malaise, so that the single largest trend is toward rejecting the scientific enterprise as it applies to humans. We suggest that this lack of progress, this “failure to thrive,” has been caused by the failure of the social sciences to explore or accept their logical connections to the rest of the body of science—that is, to causally locate their objects of study inside the larger network of scientific knowledge” (p. 23).}\]
Evolutionary Psychology, explicitly adopting evolutionary theory offers to change the nature of the discipline itself. It is considered by many to represent a metatheory that has the potential to unite all the disparate fields of study within psychology, presenting a means for psychology to come into its own like the biological sciences did after adopting neo-Darwinism (see Buss, 1995; Gintis, 2007).

Evolutionary theory in psychology may not yet be the unifying paradigm some want it to be, but at the very least, psychologists recognize that the tenability of any psychological theory rests on it being consistent with the dominant evolutionary paradigm. Understanding the human species, in part, involves understanding how we evolved. Evolution is relevant to the study of biological entities. Psychologists are interested in the similarities and differences between us and other species in order understand how our capacities are related to or different from those held by the rest of the animal kingdom. Psychologists and anthropologists study present day hunter-gatherer societies in hopes of catching a glimpse of a form of life that characterized most of human history. Similarities found in cross-cultural work play into notions of human nature and the universality of the traits that define our species, while differences point to developmental malleability and adaptability. Much work in psychology, though perhaps not always explicitly “evolutionary,” at the very least subscribes to basic assumptions of evolutionary theory that are often unacknowledged or taken for granted. Indeed, there are many approaches to the evolutionary study of human behaviour. Many of these approaches, however, converge upon a particular kind of evolutionary explanation.

The nature of evolutionary explanation in psychology is an important yet all but neglected issue. And, as in dealing with anything related to evolution, it is a complicated

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5 The use of capital letters refers a certain group of psychologists (the “Santa Barbara” school) who adopt the particular framework of the pioneers of the field, i.e., John Tooby and Leda Cosmides (see Barkow, Cosmides, & Tooby, 1992).

6 Dobzhansky's (1973) famous quote “nothing in biology makes sense except in the light of evolution” could apply to psychology just as well. Dobzhansky (1973) characterizes the metatheoretical aspect of evolutionary theory with a quote from Pierre Teil-hard de Chardin, “Is evolutionary a theory, a system, or a hypothesis? It is much more-it is a general postulate to which all theories, all hypotheses, all systems must henceforward bow and which they must satisfy in order to be thinkable and true. Evolution is a light which illuminates all facts, a trajectory which all lines of thought must follow—this is what evolution is” (p. 129).
one. I argue that it is the case that a particular kind of evolutionary explanation is put forth in psychology, namely, a strong adaptationist one. The debate about adaptationist approaches is ongoing. There is a continuing discussion with myriad facets and even more opinions from researchers that span the biological sciences and various areas of the social sciences and humanities (see Godfrey-Smith, 1999; Lewontin, 1983; Mayr, 1983; Orzack & Sober, 2001; Sterelny & Griffiths, 1999). The breadth of these issues is too vast to provide even a modest survey. However, understanding the main tenets of the adaptationist perspective, at least the pertinent ones for psychology, is important in gaining traction on the nature of explanation in psychology and the attendant assumptions that are grounded at the metatheoretical level. It is important to consider what adaptationist explanations offer psychology, what they cannot offer, and whether or not for psychologists they provide a sufficient account for understanding human behaviour. I argue that evolutionary explanation has unfortunately become synonymous with adaptationist explanation, and a broader conception of what an evolutionary explanation is has the potential to change the methods and goals of psychologists’ understanding of the evolution of human psychological traits.

In the following sections, I outline the general evolutionary approaches in psychology, and characterize adaptationism and the nature of the explanations it provides. I will focus on one particular approach, that of Evolutionary Psychology, to exemplify the nature of adaptationist explanation. This deconstruction will lead to an analysis of the Modern Synthesis and a brief review of some of the discussions surrounding the future direction of evolutionary theorizing and what it means for evolutionary explanation in psychology.

1.1. Evolutionary Approaches in Psychology

Before exploring the nature of evolutionary explanation, it is first useful to take stock of the main evolutionary approaches in psychology. Evolution is a complicated matter, if only for the fact that it so all-encompassing. As Dobzhansky (1973), a founder of modern evolutionary theory, reminds us in his famous quote footnoted above, all biological phenomena fall under the auspices of evolution. The particular interests, preferences, theoretical or epistemological commitments, etc., of each researcher will, in
part, influence the approach they take to understanding their area of study. Any evolutionary approach, however, is a particular application of the received basic evolutionary theory, currently, the Modern Synthesis. In this section I list and characterize four main approaches to the evolutionary study of human behaviour, and allude to their adaptationist affiliation, which is useful for considering how psychologists typically think about the evolution of psychological capacities. Adaptationism will be dealt with explicitly following this survey.

1.1.1. Sociobiology

Sociobiology is “the systematic study of the biological basis of all social behavior” (Wilson, 1975/2000, p. 4). The study of social behaviour draws from many fields, including zoology, ethology, anthropology, evolution, archeology amongst others, with Wilson (1975/2000) aiming to make sociobiology into a branch of evolutionary biology and in particular, modern population biology. Sociobiology argues that, in a similar way in how physical traits evolve, individual and social behaviours also evolve. Thus, social behaviours are at least partially inherited, and as such, are subject to natural selection. From an adaptationist perspective, animals will act in ways that have conferred fitness advantages over evolutionary time. Sociobiology’s positive impact was its being concerned not only with the evolution of social behaviour in non-human animals, but also with human social evolution, for example, the social behaviour of our human ancestors and adaptive features of organization of extant primitive human societies. Wilson saw how over the last 40 years all of the biological sciences were reformulated and integrated by adopting the Modern Synthesis, and he saw the social sciences as one of the last remaining biological sciences to be integrated. Wilson regarded sociobiology as a way of reformulating the frameworks of sociology and the other social sciences to bring them under the umbrella of the Modern Synthesis.

Although it is often largely credited for (re)introducing evolution to psychology, Wilson’s (1975/2000) particular approach was met with a great deal of criticism that still exists to this day. Wilson saw sociobiology as a rally against the social sciences’ neglect of evolution and its favouring explanations of cultural learning. Wilson famously (or
infamously) devoted the last chapter of his *Sociobiology: the New Synthesis* to an application of evolutionary biological reasoning to human behaviour. Although attempting to introduce an evolutionary level of explanation in understanding human behaviour in the social sciences is commendable, such ideas would likely have gotten more traction with a more nuanced approach. Wilson recognized that individuals were shaped by their physical, social and cultural environments, however, his nativist, gene-centred, reductionist approach led to intimations of biological determinism that were strongly opposed by psychologists and evolutionary biologists such as Richard Lewontin and Stephen Jay Gould. Thus, for Wilson, behavioural diversity was a product of genetic variation, due to differential survival and adaptations leading to behavioural differences between the sexes, yet he emphasized the existence of a universal human nature that can be identified and studied (i.e., Wilson, 1978).

The “New Synthesis” of behavioural biology that Wilson claimed it to be is a bit of a misnomer as the term sociobiology had been used in various ways since the 1940s, but the “newness” stems from a thorough incorporation of modern evolutionary theory. Wilson’s book certainly popularized the term and made it the general term for evolutionary approaches to mind and behaviour (Griffiths, 2008).

It is interesting that proponents for particular types of evolutionary explanation conflate criticism towards their particular brand of evolutionary explanation with resistance to evolutionary explanation in general. It seems the former is more often the case, but this response has become a useful piece of rhetoric that started with Wilson’s response to his critics, perfected by proponents of EP (the new version of sociobiology, at least according to Wilson [1975/2000], as will be explained below).

It should be noted that Wilson’s (1975/2000) approach is not necessarily a function of the discipline itself, but rather part of the application of the genocentric evolutionary metatheory that is criticized in this dissertation. Sociobiology consists of an interdisciplinary program of research, and as such, Wilson’s perspective is just one many perspectives. Other eminent sociobiologists such as Sarah Blaffer Hrdy (1999) and Frans de Waal (2005) have perspectives which are difficult to interpret as deterministic. Even though Wilson’s sociobiology was meant to take seriously social influences, critics found his approach was still too genocentric to adequately address culture and its role in evolution. Sociobiologist Richard Dawkins’ (1976/2006, 1982) notion of memetic evolution has been much more successful in this regard.

It should be noted that Wilson (e.g., Lumsden & Wilson, 1980, 1981) published some of the first works on gene-culture coevolution (described below), which exemplifies how important a role he considered culture to play in evolutionary theorizing. However, this work had limited impact on modern dual-inheritance theory as it too was met with much criticism for its reductionistic and deterministic qualities (Alper & Lang, 1981).
1.1.2. **Behavioural Ecology**

Behavioural ecology is the study of the evolution of behaviour and cultural diversity based upon ecological and social factors. It relies heavily upon mathematical models borrowed from evolutionary biology and ecology with the purpose of making predictions about traits of adaptive significance. Behavioural ecology emphasizes the role of the environment in eliciting the production of adaptive phenotypes. It tracks the outcomes of these adaptive challenges, taking any genetic, developmental, or organismic factors for granted, and thus does not endeavour to distinguish what mechanisms give rise to the match between phenotype and the adaptive challenges posed by environment. Because the tracking of these proximate mechanisms is not the focus, behavioural ecologists have been able to demonstrate the range of adaptive outcomes that are possible in species. This approach is focused on behavioural flexibility (phenotypic plasticity), thus it is more interested in *adaptability* rather than traits that are considered to be psychological or behavioural adaptations. Remaining mechanism neutral leads behavioural ecologists to focus on real-time adaptability rather than past adaptation.

Applying behavioural ecology to humans comes largely out of the work of anthropologists and cross-cultural psychologists, who study how various pre-industrial or subsistence societies have come to adapt to their local ecological demands. Human behavioural ecology takes an adaptationist approach, but unlike other approaches focuses on current adaptive fit to the environment, not an ancestral one. This approach is in line with the tradition of the social sciences to focus on behaviour as a product of the myriad of proximate factors that influence behaviour, in particular, the social and cultural demands that are often of interest to psychologists and that are more pertinent to explain behaviour than the demands of far removed ancestral environments. Indeed, if

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11 Griffiths (2008) argues that behavioural ecology is the product of adaptationism in English-speaking ethology, which came to dominate the study of animal behaviour. Armed now with the population genetic models of Hamilton (1964), this theoretical structure could be superimposed on Tinbergen’s experimental tradition. Animal behaviour research became concerned with testing the predictions of population genetic models of survival value of phenotypes in the laboratory and the natural environment (Griffiths, 2008).
adaptability is the key to human evolution, then the demands of ancestral environments are rendered even less relevant to explaining currently adaptive behaviour.

Human sociobiology was an application of behavioural ecology to the human species. However, sociobiology differed from behavioural ecology in that it was not mechanism-neutral, at least not in Wilson’s (1975/2000) account. His emphasis on genetics as the ultimate source of developmental information made that clear. Nevertheless, most sociobiological studies are now conducted under the umbrella of behavioural ecology. This connection seems to lend more credit to the view that behavioural ecology may not be mechanism neutral at all. In mapping adaptive phenotypes perhaps one can remain mum on the mechanisms that give rise to these traits; however, the adaptationist perspective is one that favours selection, often viewed as operating on genotype. In behavioural ecology what is inherited in evolution is the genotype. The next approach formalizes what can be considered to be another form of inheritance of evolutionary importance.

1.1.3. **Gene Culture Co-Evolution**

Gene-culture coevolution, or dual-inheritance theory, is the approach that takes most seriously the influence of social and cultural factors in evolution. The influence of social and cultural factors, is in fact, considered to be an independent system of inheritance separate from the genetic inheritance system. The cultural inheritance system is conceptualized in the same way as the genetic system, with cultural traits competing with each other as alleles or genotypes do. Because they are conceptualized in the same way, standard evolutionary models can be applied to cultural traits. Genotype and phenotype influence each other bidirectionally with the environment influencing both systems in evolution. Thus, the evolutionarily relevant novelty comes not from random mutations, but from behavioural variants of transmitted culture.

Such an evolutionary approach is the choice of cultural evolutionists who take seriously the role of social learning and how novel behaviours propagate among

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12 Once again, even though Wilson (Lumsden & Wilson, 1980, 1981) attempted to build a model of gene-culture coevolution, subsequent researchers separated these inheritance systems to a larger extent which stands as the defining feature of this approach.
members of a population in accordance with the dictates of the population’s local environment. This approach, unlike others, is most like the SSSM in its emphasis on social learning. However, it extends the conception of social learning in terms of its evolutionary importance in its effect on increasing fitness. That is, for example, novel behaviours can then lead to selection for the neural or genetic mechanisms that gave rise to the novel behaviours. The standard example of gene-culture coevolution is lactase persistence, where genes for continued lactase production into adulthood were selected in populations with a history of dairy farming (see Holden & Mace, 1997; Swallow, 2003). The next approach is arguably the dominant, or at least most popularized, evolutionary approach in psychology.

1.1.4. **Evolutionary Psychology**

Evolutionary Psychology (EP) differs from the other approaches in that it is presented as a broad theoretical framework for psychology based on neo-Darwinian evolutionary theory. Proponents of EP claim that it stands to unify research in the myriad areas in psychology by framing psychologists’ thinking about psychological phenomenon within the context of a biologically-based paradigm that has been successful in unifying the biological sciences (Shackelford & Liddle, 2014). Thus, not only does an EP framework stand to unite psychology, but its adoption stands to add an air of respectability to psychology, bringing it out of the “dark ages” into a scientific (i.e., evolutionary) renaissance. Evolutionary Psychology has its roots in sociobiology, but whereas Wilson (1975/2000) sees EP as an extension of sociobiology, Evolutionary Psychologists have tried to distance themselves from sociobiology. This distancing may be an attempt to be viewed as its own discipline with its own tenets, or perhaps to distance itself from the controversies associated with Wilson’s sociobiology. However, it has much in common with sociobiology with its focus on adaptations, natural selection, and genetic inheritance. The main distinction between EP and sociobiology is that EP assumes a mismatch between the current human environment and the environment of

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13 In this respect, EP is a merger of fields rather than a particular field (Barrett, 2008)

14 From an EP perspective, psychology is run amok with disparate theories and empirical findings, evidently impeding scientific progress (e.g., Confer, Easton, Fleischman, Goetz, Lewis, Perilloux, & Buss, 2010; Tooby & Cosmides, 1992).
evolutionary adaptedness (EEA) equated to the Pleistocene period of human evolution, whereas sociobiology is concerned with current adaptations (Griffiths, 2008). This means that Evolutionary Psychologists see much of current human behaviour as adapted to an ancestral environment rather than adapted to the current environment, which explains why current behaviour does not always seem to be adaptive. EP has been exceedingly well promoted. The provocative claims of EP are fodder for mass consumption through media coverage and the popular writings of its proponents are best sellers (e.g., Pinker, 1997, 2002). EP has its own journal, *Evolutionary Psychology*, and through its interesting and sometimes controversial claims has done much to rouse thinking about evolution in contemporary issues in psychology.

Much like sociobiology’s extension of the concept of adaptation from physiological and physical adaptation to behavioural adaptations, EP further extends this expanded notion to a motivational, emotional, and psychological context. EP suggests that the human mind is “massively modular” in its construction in that it consists of an enormous number of context-specific information processing units that have accumulated due to selection of these mechanisms as solutions to recurrent problems faced by our hunter-gather ancestors (Cosmides & Tooby, 1994, 2013; Sperber, 1994; Pinker, 1997). Evolutionary Psychologists argue that their position contrasts greatly from what they argue to be dominant position in the social sciences. They describe what they call the Standard Social Science Model (SSSM), which they argue asserts the primacy of learning processes in human development. Premised upon the notion that humans are “blank slates,” such a model, according to Evolutionary Psychologists, precludes any recognition of developmental adaptations or constraints (Tooby & Cosmides, 1992; Shackelford & Liddle, 2014). Contrary to the SSSM and its apparent assertion that the human mind consists of a general-purpose learning mechanism that is nearly completely influenced by culture, Evolutionary Psychologists promote an “Integrated Causal Model” (ICM) that differs from the SSSM in several ways (Cosmides & Tooby, 1992). That is, rather than a general purpose learning mechanism, humans are born with countless...
modules that process particular kinds of information. The claim of massive modularity (a.k.a. the “Swiss-Army Knife” model) postulates that each of these domain-specific mechanisms is an evolved adaptation to particular recurrent problems our hominid ancestors had to face in our evolutionary past.

Evolutionary Psychologists argue that we have psychological adaptations selected for the benefits that they confer upon the individuals. The period during which these adaptations were acquired is known as the Environment of Evolutionary Adaptedness (EEA), which is presumed to have ended with the dawning of the Holocene, and mostly consists of the period of Homo evolution. A few adaptations Evolutionary Psychologists study, however, refer to hominid and primate adaptations in general, such as colour vision, bipedalism, and preference for sweets and fats. Given that the modern era has only existed for 10,000 years or so, it is thought that the majority of our psychological adaptations were formed during the Pleistocene when our ancestors lived as hunters and gatherers. This is the presumption that current human cognition may not be adaptive in the present human environment but nevertheless exists as adaptations to past selection pressures. Ours are “stone-age minds in modern times.”

Whereas Evolutionary Psychologists claim that the SSSM denies the biological underpinnings of human thinking and behaviour, these researchers stress the importance of studying both biology and culture to understand the development of human psychological capacities. That is, Evolutionary Psychologists understand that both “nature” and “nurture” are equally important to study, rather than ignoring “nature” as psychologists are argued to do. Thus, they subscribe to the “interactivist consensus”

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16 Confer et al. (2010) define psychological adaptations as “information-processing circuits that take in delimited units of information and transform that information into functional output designed to solve a particular adaptive problem” (p. 111).

17 Because there are adaptations to these selective problems, it is also assumed that these were reliably reoccurring problems that the human species had to face for innumerable generations.

18 This phrase is a take on a founding principle of EP, that “Our modern skulls house a stone age mind” (see Evolutionary psychology: A primer by Cosmides and Tooby, http://www.cep.ucsb.edu/primer.html).
(see Sterelny & Griffiths, 1999). The interactivist consensus and EP’s position on development are examined in following sections.

1.1.5. **A Family of Evolutionary Approaches**

Although it is recognized that these are all equally valid evolutionary approaches in psychology, it is less often explicitly acknowledged that they form a family of adaptationist approaches. This is not typically taken to be a problem, as adaptationism is consistent with modern evolutionary theory, so these approaches are relatively uncontroversial in this regard. It is important to note that there are some arguably significant differences between these approaches as they differ, for example, in their focus of study, the environment in which they see behaviour as adaptive, and in how they conceptualize the speed at which behaviour is able to change to adapt to new circumstances. Because these approaches are adaptationist, some argue any differences make these approaches largely complementary (e.g., Nettle, 2009). Others, such as Brown, Dickins, Sear, and Laland (2011), in a review and evaluation of the way these approaches can be integrated, note that some differences are fairly substantial. A second inheritance system for culture, for example, may be difficult to integrate with other approaches. However, this difference may be a matter of perspective. Whereas Nettle (2009) acknowledges the adaptationist commitments of these approaches, Brown et al. (2011) do not. That is, Nettle sees the differences as minor in comparison to their shared tenets. Similar to Nettle, I argue that any differences are superficial compared to the similarities. Before evaluating these approaches in depth in terms of their adaptationist commitments, it is first necessary to characterize adaptationism in more detail.
1.3. Adaptationism

The adaptationist stance is based on an assumption that an organism’s present physical, behavioural, and cognitive features are products of past selection of features that increased fitness in the species in light of physical and social environmental demands. The adaptationist stance takes one evolutionary mechanism, natural selection, as the key causal mechanism for adaptive evolution. It is not that other mechanisms such as drift and recombination are not recognized as important to evolution, but rather it is argued that these other mechanisms do not lead to the development of adaptations.

Evaluating adaptationism is difficult. In a sense, the notion of adaptation is uncontroversial even to the most vociferous critic of adaptationism. Evaluating the adaptationist stance is problematized by many factors, including not only variations in how strongly researchers adhere to an adaptationist stance, but also different senses of what is ubiquitously taken to be a central concept in evolution: adaptation itself. The main criticism of adaptationism I will discuss has to do with the overemphasis on a particular notion of adaptation and the exclusive focus of the evolutionary mechanism of natural selection. This criticism applies most fully to a position that will be referred to as “strong” adaptationism (Atran, 2005). Strong adaptationism leads to the most problematic form of evolutionary explanation in psychology, which is the target of my critique.

1.3.1. Strong versus Weak Adaptationism

Strong adaptationism asserts that “any functional cognitive design that is too complex to result from pure chance must be either an adaptation or a by-product of an adaptation” (Atran, 2005, p. 40). Weak adaptationism asserts that “most higher-order human cognitions are by-products of earlier evolutionary by-products that were not

19 Psychologists typically focus on psychological adaptations (e.g., Barkow, Cosmides, & Tooby, 1992; Tomasello, 1995; 2008), though some focus upon physical adaptations (e.g., Shackelford & Goetz, 2007).

20 Fitness refers to the ability of an organism to survive and reproduce, leading to an increase in frequency of a genotype and/or a phenotype in future generations in comparison to others.
adaptive to fulfill a specific function relative to some particular ancestral environment” (p. 40). These positions differ largely in regard to how they conceive of adaptations, the ability to detect adaptation, and the ability to determine the putative cause of the adaptation.

In general, adaptationists are interested in determining what features of organisms or groups of organisms are adaptations or by-products of adaptations. Although both sides are interested in determining the selective pressures that led to the development of these adaptations, weak adaptationists see this as highly difficult if not impossible, while strong adaptationists, who I argue are Evolutionary Psychologists, have churned out prolific research programs doing just this. How is this possible? Strong adaptationists believe the cause of these adaptations, the selective force, can be inferred from the functional design of these adaptations (Atran, 2005). To strong adaptationists, adaptations are detectable and can be reverse-engineered to determine the selective problems that gave rise to them (Wereha & Racine, 2009). Reverse-engineering refers to the notion that cause can be inferred from design, and as such, strong adaptationists feel that it is a fairly simple task to identify what an adaptation is “for.” Weak adaptationists, who still believe adaptation is a real and important component of evolution, argue this is not possible, as the putative causes of any adaptations are buried “too far down” in evolutionary history. Indeed, no simple experiment or functional data can support or negate historical adaptive hypotheses (Nielsen, 2009). That is, prior adaptations could have been appropriated for different purposes (exaptations) or current features are completely unrelated to adaptation per se and exist as unintentional structural consequences of adaptations (spandrels) (Gould, 1991; Gould & Lewontin, 1979; Gould & Vrba, 1982; Rose & Lauder, 1996).

Reverse-engineering such features is even more untenable from this perspective because rather than natural selection, these spandrels are sensitive to cultural selection, negating natural selection’s assumption of functional design (Atran, 2005). Weak adaptationists argue that although paleontology and archeology can tell us a part of the story of the evolution of humans based on physical evidence, information on behavioural and social evolution is all but nonexistent (Gould, 1997). Strong adaptationists, in contrast, believe one can surmise the evolutionary history of a species by deconstructing
a species’ design features, no matter whether these are due to natural or cultural selection.

It is interesting to note the parallels between current forms of “strong” adaptationism and a form of adaptationism that is considered to be the “old” or “pre-Spandrels” (Gould & Lewontin, 1979) form of adaptationism. This “old adaptationism” was heavily critiqued for its unbridled invocations of adaptation and for overextensions or misapplications of selectionist reasoning. This view began to decline with George C. Williams’ (1966) book *Adaptation and natural selection: A critique of some current evolutionary thought*. In it Williams (1966) argues for a rather conservative approach, reminding researchers of the special nature of the term and that it should only be used when it is absolutely necessary or under the burden of overwhelming evidence.  

This form of adaptationism was put to an end by Gould and Lewontin’s (1979) famous paper *The spandrels of San Marco and the Panglossian paradigm: A critique of the adaptationist programme*, which is often simply referred to as the “Spandrels paper.” In it they critiqued what they labelled as “adaptationism” as a research program that assumes a priori all features of organisms can be thought of as optimal adaptations. They argued for a pragmatic pluralistic approach to the study of organismic design. A pluralistic approach offers more complete and sophisticated evolutionary explanations that consider more mechanisms of evolution than simply natural selection and consider a large role for the unpredictable contingencies of history that are needed to explain the patterns and regularities of evolutionary pathways (Gould, 1997). Following this critique, a “new” adaptationism, one that can be considered to more akin to “weak” adaptationism, became the standard adaptationist approach in biology.

What is astounding is that even though the old or strong version of adaptationism would not be accepted by anyone in biology after the Spandrels paper (for a while many researchers were even afraid to utter the word adaptation, see Rose and Lauder, 1996), this view is currently and popularly held in psychology. Adaptationism has been and continues to be extensively critiqued. Although many of the issues I raise pertain to

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21 One of his main contentions, however, was with what he thought of as an overextension of adaptationist thinking in the notion of group-selection. In his delineating the proper application of evolutionary concepts Williams laid down the basis of the popular genocentric view of evolution.
adaptationism in general, dismissing adaptationism in all its forms is beyond the scope of this dissertation. Adaptationism has proved itself in the past and there may still be problems for which it is useful.\textsuperscript{22} I argue, however, that a strong adaptationist approach offers woefully inadequate evolutionary explanations in psychology. To begin with, it provides a curious conception of adaptations.

1.3.2. \textit{Adaptation}

The notion of adaptation stems from a long held appreciation for how well organisms seem to “fit” their ecological niches. This insight is what inspired ideas as different as Darwin’s (1959) transmutation of species and Paley’s (1802/1828) watchmaker argument for a divine creator. However, it is difficult to characterize the nature of this fit. In a broad sense, organisms can be considered to be rife with adaptive features. If one views organisms as being a system, the complexity and functionality of these systems is adaptive overall (van Valen, 2009). The problem is it is difficult to distinguish which if any features are adaptations per se. If one considers the overall system to be adaptive, however, it may be difficult, if not non-sensible, to state that any part of the system is an adaptation. The impossibility of parsing an adaptive system in such a way may be one reason, among others, why many researchers find themselves to be in a paradoxical situation where adaptation is thought to be ubiquitous, yet difficult to detect.

Strong adaptationists do not seem to have a problem identifying adaptations at all, as they parse the adaptive features of organisms into separate identifiable adaptations. This is a particulate concept of adaptation in which organisms are considered to be collections of naturally selected features (see Wereha & Racine, 2009, 2013). Rose and Lauder (1996) note that this untenable notion was once held by the disciplines of comparative morphology and biomechanics under the influence of the “old” adaptationism, leading to a belief that every character was shaped by natural selection, in isolation from other characters, for its current role. Many strong adaptationists argue

\textsuperscript{22} Godfrey-Smith (1999) considers the fact that although adaptationism was important to develop Darwin’s insights and construct a comprehensive evolutionary theory, biology may have outgrown adaptationism.
they do not subscribe to a particulate conception of adaptation, largely because they understand that selection works not on individual features of an organism but on the organism itself (e.g., McCullough, Kurzban, & Tabak, 2013, p. 43). However, the massive modularity hypothesis of EP that conceives of the human mind as a collection of domain-specific naturally selected mental organs clearly meets the criteria for a particulate concept of adaptation and is a stance very much like the one formerly held in comparative morphology and biomechanics.

Even if we accept strong adaptationists’ stated innocence of their belief of particulate adaptations (they are not innocent, see Wereha & Racine, 2009; 2013), it is still fair to characterize their conception of adaptation as a static rather than a dynamic one. One way to think about this is to consider whether strong adaptationists think adaptation took place in the past, or it takes place in the present. This is a distinction between adaptation and adaptability, and leads to differences in what one considers to be an adaptation and the putative role of natural selection (Wereha & Racine, 2009, 2012). A static conception of adaptation is one that is taken to be a product of past selection for the features that endowed a fitness benefit to the individuals of the species. A static conception of adaptation makes the organism a passive participant in evolution. A species’ features, a product of natural selection, are all it has to face subsequent rounds of natural selection. A dynamic conception of adaptation considers that organisms are active participants in evolution that are not limited by prior species-specific adaptations. They are able to adapt, developmentally and behaviourally, effectively changing, eliminating, or creating the selection pressures they face (Bateson, 2013; Corning, 2014; Laland, 2014; Mesoudi et al., 2013). In psychology, strong adaptationism leads to conceiving adaptation as static, “concrete” psychological mechanisms that are the product of past selection in the species. Because they exist

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23 An inherent problem with a static conception of adaptation is its circularity in that all any organism of any given generation of a species is equipped with to face subsequent rounds of selection are the set of faithfully developing adaptations it was endowed with from previous generations. At first glance at such a conception, one would be hard pressed to see how anything can evolve. Of course, the mechanism of change that is invoked is mutation. Thus evolutionary change comes from selection of stochastic mutations that leads to more adaptive phenotypes. This “static” conception of evolution is, as will be shown below, severely limited in how it characterizes the relationship between genes, organisms, and selection.
and persist, they are considered to be adaptive. Because there can be real-time behavioural and developmental adaptation in an organism’s life course, however, these past adaptations may no longer be adaptive. In this respect, adaptations can promote or inhibit organismic adaptability (van Valen, 2009). That past adaptations are always beneficial is not a safe assumption if one considers adaptability a serious factor in evolution.24

Another issue is distinguishing between adaptation and adaptation “for.” Strong adaptationism often uses adaptation in the latter sense. Van Valen (2009) points out a proclivity among researchers to refer to adaptation as features that have consistently served their adaptive function since they were selected. For example, wings are taken to be examples of adaptations for flight. However, this notion confuses adaptation per se (i.e., being adapted) with adaptation for a specific function (p. 269). He illustrates this notion with the example of the connection between brains and reading. Our brains are important for reading, but reading did not play a part in the evolution of brains. Hence although reading is an adaptive trait, it is considered to be an adaptive byproduct. Adaptation “for” nomenclature raises the ire of Reid (2007) who argues that there is no teleological, evolutionary force working that presents a “problem of flight” and then leads to the “solution” of this problem as wings. He argues that selectionists assume natural selection is such a force, which has given creationists evidence that evolutionists have simply renamed God as natural selection. Adaptations in the strong adaptationist stance are often of the “for” variety. This is the standard sense of adaptation in Evolutionary Psychology (Richardson, 2007), and stems from not only a focus on reverse-engineering, but adaptive thinking (Rellihan, 2012). This project has to do with inferring solutions from adaptive problems (Sterelny & Griffiths, 1999), and thus, our capacities are adaptations “for” solving evolutionary relevant problems.25

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24 This may be so at a behavioural level but Evolutionary Psychologists would argue that at a developmental level different phenotypic outcomes were “designed” by natural selection. So in some ways, adaptation occurs in “real-time” at a developmental level, which is closer to the notion of adaptability. However, these developmental outcomes are products of past selection so they are still considered to be static adaptations.

25 It should also be noted that this view also is based on the assumption that environments pre-exist or at least are something separate from the organism, an assumption that is problematized by systems approaches which will be discussed to some extent below.
1.4. Adaptationism and Psychology

The standard evolutionary approach in psychology is an adaptationist one. It is amusing that despite their shared adaptationist perspective, proponents of EP (e.g., Barkow, Cosmides, & Tooby, 1992; Cosmides & Tooby, 2013; Confer et al., 2010), sociobiology (e.g., Wilson, 1975/2000), and gene-culture co-evolution (e.g., Gintis, 2007; Laland & Brown, 2002), all believe their respective disciplines uniquely have the potential to unite psychology under a single unifying framework. What is even more amusing is that they are all astounded over how this validation has not yet come to pass for any of them, and some mull over how they hardly receive any attention at all. For example, Evolutionary Psychologists believe they are breaking new ground by introducing psychology to evolutionary theory. Resistance is characterized as psychology being ill-prepared to handle the “truth” of EP. However, it is more likely that Evolutionary Psychologists are mistaking critiques of their strong adaptationist agenda as attack on evolutionary theory in general. By virtue of studying biological beings, all psychologists must consider evolution important to some extent (Gintis, 2007).

However, others have pointed out that psychology and allied disciplines have already been united: by adaptationism itself (Price, Brown, & Curry, 2007). In the following I reconsider the main approaches to the study of evolution in psychology in the context of the issues with adaptationism in general. If it is indeed a unifying approach, it is not without serious problems. I will briefly outline some problems with sociobiology, human behavioural ecology and gene-culture co-evolution before presenting a more detailed critique of EP, as it most clearly demonstrates the problems with strong adaptationism.

Dobzhansky (1963) stated “Since evolution consists essentially of responses of the species genotype to challenges of the environment, and since man’s environment is chiefly that shaped by his culture, it seems to follow that human biological evolution must

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26 Many psychologists take a Developmental Systems Theory (DST) perspective on developmental and evolutionary matters. However, this is still a “non-standard” approach though its ideas are becoming more popular (Bjorklund & Ellis, 2014; Gottlieb, 2007; Griffiths, 2007; Lickliter, 2008; 2014; Lickliter & Honeycutt, 2003, 2013, Machluf, Liddle, & Bjorklund, 2014; Spencer, Blumberg, McMurray, Robinson, Samuelson, & Tomblin, 2009; Stotz, 2014; Wereha & Racine, 2012). DST will be examined later in the paper.
be directed mainly by culture” (p. 314), and his view that several genetic responses can equally respond to environmental challenges led to his notion of “evolutionary indeterminism.” The notion of such indeterminacy and the view that human-kind is now at the evolutionary whims of culture was countered by Wilson’s sociobiology, which took evolutionary agency back from organisms and their environments and put it into genes, countering the SSSM notion of tabula rasa, and reviving the notion of developmental and adaptive programs (i.e., Lorenz, 1965). As stated earlier, it is difficult to assess sociobiology as a whole due to the varying approaches of its adherents. However, the view I characterize as being the most problematic is the “strong” Wilsonian sociobiological position, which, with some minor differences, is now the program of EP (critiqued below).

Human behavioural ecology focuses on current, not past adaptation. Such a conceptualization is useful for a more “real-time” evolutionary perspective where evolution can still be considered to be occurring in terms of current adaptive behaviour rather than past, not always currently adaptive, adaptations. One of the main issues is its claim regarding mechanism-neutrality. As previously stated, the adaptationist stance is partial to natural selection. Remaining mechanism neutral is potentially useful, as it leaves the door open for invoking multiple developmental and evolutionary mechanisms. However, it is doubtful how an adaptationist approach, that champions natural selection as the causal mechanism of evolution, can remain “mechanism neutral.” Moreover, as will be explained later, developmental mechanisms are ignored entirely. Indeed, if adaptations are the interest of behavioural ecologists, according to adaptationism natural selection is the only way to explain it.

Gene-culture co-evolution most explicitly considers other forms of evolutionary relevant inheritance. It takes cultural selection seriously by formalizing it as a separate inheritance system along with genetic inheritance. However, as an adaptationist approach it still focuses on selection as the main mechanism of evolution. Further, the cultural inheritance system is conceptualized as one that is similar to the genetic inheritance system, and aside from parity and convenience of this conceptualization for selectionist modelling, it is not clear if it is appropriate to consider cultural inheritance in the same way (see Fracchia & Lewontin, 1999). For example, cultural inheritance changes at a much faster rate than genetic inheritance, and it is not clear if the “gradual”
model of Darwinian accumulation of cultural capital holds when it is often characterized by rapid qualitative change. Although a step in the right direction of taking seriously the role of non-genetic factors in evolution, it is still a very limited notion of inheritance, as models of expanded inheritance track these factors at the genetic, epigenetic, behavioural, and cultural level (Jablonka & Lamb, 2005; Mesoudi et al., 2013; Stotz, 2014). And to some extent, expanded notions of inheritance and systems thinking where the levels of influence act on each other bidirectionally, problematizes the formalized separation between genetic and cultural inheritance in dual-inheritance theory. Moreover, in dual inheritance theory cultural inheritance is conceptualized as a vertical transfer of information in the same way genetic inheritance is conceived. However, the horizontal transfer of ideas, as well as the creation of new ideas, may be more properly considered to be occurring between people (i.e., horizontal transfer) (Gabora, 2011, but see Greenhill, Currie, & Gray, 2007). Ultimately, humans have had culture before they were even humans (Sterelny & Griffiths, 1999). It is difficult and perhaps completely inappropriate to conceptualize genes and culture as two independent “interacting” factors.

1.4.1. The Strong Adaptationist Stance of Evolutionary Psychology

For Evolutionary Psychologists, present day humans “bear the scars” of our evolutionary past: the selection pressures we faced in the EEA can be surmised by the adaptations we currently have. Thus, Evolutionary Psychologists can describe the EEA of our Homo ancestors, which outlines the selection pressures that led to the evolution of human psychological adaptations. Thus, they claim it is possible to reverse-engineer present adaptations to describe the evolutionary conditions for the selection of current psychological capacities, which in turns explains our psychological capacities.

Evolutionary Psychologists have been surprisingly adept at deflecting accusations of circular reasoning in their reverse-engineering account. Evolutionary Psychologists have become most sensitive and have tried to avoid the most obvious claims that would attract the now derogatory label of “just-so” stories (see Gould & Lewontin, 1979; Gould & Vrba, 1982). The term is derived from Rudyard Kipling’s “Just-So Stories” that are fantastical origin stories for children that explain things such as “how the camel got his hump” or “how the leopard got his spots.” Referring to an evolutionary
explanation from EP as a “just-so story” implies that the unverifiable explanations Evolutionary Psychologists give are essentially convenient fictions. As interesting and thought provoking selectionist explanations may be, the problem is that they typically lack any scientific or historical substantiation. Evolutionary Psychologists argue, however, that they do not provide unsubstantiated conjecture. Rather, they argue they provide testable hypotheses that are based on neo-Darwinian evolutionary theory and provide empirical evidence for the existence of the purported psychological adaptations. That the existence of these adaptations (psychological phenotypes) is also apparent evidence for the developmental robustness of these adaptations, again, is circular logic. The reason why this is circular is that the evidence Evolutionary Psychologists present does not represent an independent criterion for supporting the supposition of a hypothesized adaptation. That is, no evidence is provided, be it paleontological, genetic, developmental evidence, etc., to substantiate the existence of the adaptation beyond the mere exertion of its existence and its putative selective cause. Another aspect of circularity stems from their relying on evolutionary theory to do double duty: it is used as a means of deriving hypotheses and supporting those very same hypotheses (Rellihan, 2012).

Although many EP claims may be very reasonable presumptions about which traits could be adaptations, and the selective pressures that led to them, it may be the case that many Evolutionary Psychologists overstate the usefulness and understate the speculative nature of these statements, which leads many psychologists to believe that these are “just-so stories.” The less innocuous aspect of selectionist explanation lies in its implicit support of the status quo. Reverse-engineering a phenotype implies a form of determinism where the outcome was selected for in our evolutionary past and inherited by future generations. Again, considering the general consensus that adaptation is difficult to infer, it’s all the more astounding that Evolutionary Psychologists infer the existence of adaptations (in which adaptiveness is no longer a criterion of inference
because these are outdated adaptations that are no longer adaptive!) then go on to describe the selective context for these adaptations.27

The “concrete” nature of EP adaptations severely mischaracterizes the nature of human cognitive capacities, which typically have long and complex ontogenies that cannot be attributed to a “simple” reliably developing cognitive adaptation.28 The disruptive camouflage of leopards’ spots is an example of a trait that conferred a selective advantage to the species over, presumably, a non-spotted phenotype. Whereas a leopard cannot do too much to change his spots, there is much more variability in human psychological outcomes. This is a controversial statement indeed, as Evolutionary Psychologists argue that the variability in human cognitive capacities is small, or at least superficial relative to the grand cognitive architecture shared by all members of the species, and this is why they are interested in the similarities and the universal nature of this architecture.29 This politically sensitive stance, however, seriously overlooks the nature of human cognition and the variability associated with it. Leopard spotting is a physiological adaptation that is highly constrained developmentally. Many human cognitive capacities, particularly the ones that EP generally seems to be

27 Both strong and weak adaptationists claim they uphold the conservative standards of invoking adaptation set by Williams (1966). Weak adaptationists point out the difficulty of inferring adaptation, while strong adaptationists like Evolutionary Psychologists (see Shackelford and Liddle, 2014 for a recent example) argue that they take a conservative approach, and any “evidence” they present, along with the problems with circular logic, leads to what they see as overwhelming evidence for adaptation. However, they differ from Williams in their stance on natural selection. Williams stated “I regard it as unfortunate that the theory of natural selection was first developed as an explanation for evolutionary range. It is much more important as an explanation for the maintenance of adaptation” (Williams, 1966, p. 139). Certainly, EP invokes natural selection as both the creator of adaptation and filter of adaptations, which is a consequence of overextending one evolutionary mechanism to explain organismic form. Natural selection will be discussed later in this paper.

28 The “simplicity” stems from certain assumptions of developmental robustness stemming from a problematic view that links genotype and phenotype, an issue that will be taken up in Section 1.6.

29 Such a position has been commended as an advance from sociobiology (e.g., Gould, 1997) but it is also a wise stance to take in a field of research where critics are sensitized by sociobiology and wait to pounce at any hint of determinism that implies differences between human races or cultural groups. Just-so stories about differences between the sexes are, however, apparently acceptable.
interested in, however, have a long and complex ontogeny. Variability in this context refers to not just superficial differences, like the “decorative façade” on a universal cognitive structure, but can rather represent actual differences in the cognitive structure itself. This possibility is ruled out a priori by EP. By virtue of presupposing a universal architecture, the development of these cognitive capacities is rendered a moot point.

Another related argument stems from the difference between mosaic and connected traits (see Sterelny & Griffiths, 1999). A mosaic trait is one that can evolve in relative independence from the rest of the phenotype. Skin colour, for example, is thought to be a product of selection, as different tones have an adaptive function in different environments with relatively little change in the rest of the organism. Connected traits are inherently linked to other aspects of the phenotype and are developmentally entrenched. Sterelny and Griffiths use lung number as an example; that is, lung number is a function of a bilateral organismic plan and the developmental mechanisms involved in this symmetry. The human behavioural repertoire is not a collection of mosaic traits. Any “mental mechanism” plays a role in a multitude of behaviours, and “Hence, individual behaviours are unlikely to have histories of their own, or to have independent adaptive significance (Sterelny & Griffiths, 1999, p. 321). Thus, speaking of many adaptations, and especially any specific adaptations, humans have for child abuse and rape (see Thornhill & Palmer, 2000; Thornhill & Thornhill, 1987, 1992) are completely misconceived.

30 It would seem that any variance in “cheater-detection” would be better explained by developmental and sociological factors than selectionist theory. However, the “ultimate” reason these are even relevant factors are because these proximate mechanisms are also “important” only to bring about the adaptation.

31 One can even view this from a neo-Darwinian perspective: that behavioural and psychological adaptations need to be less developmentally constrained in order to increase adaptability to living conditions that can change drastically. Such flexibility can confer higher fitness to these individuals, spreading this capacity in the population. It is interesting that at this population level of analysis that Evolutionary Psychologists do not translate the clear implications to individuals: that psychological adaptations may be of a very different variety from physiological adaptations.

32 Evolutionary Psychologists, of course, think development is important in the “nature versus nurture” sort of way, an issue that will be taken up in the following sections.
1.4.2. **A Lacking Unification**

It is claimed that the biological and behavioural sciences are unified by adaptationism. If this is the case, then this unification is lacking. The strong adaptationist program of EP has little traction. Behavioural ecology and dual inheritance theory share some similar limitations. The scope of the adaptationist program is too limited. Moreover, the study of the evolution of human behaviour and cognition is wrought with difficulty. The lines of evidence other evolutionary scientists use are often not available or of no use for the purposes of psychologists. Adaptationism is a narrow approach to the study of evolution that deals mostly with adaptation by natural selection, and there are reasons to doubt how good of a job adaptationists do at what is supposed to be their bread and butter. What is needed is a healthy appreciation of the limits of not only adaptationist, but also evolutionary, explanation. In this way, we can be in a better position to conceptualize the problem and survey our options for an evolutionary approach to the study of psychological capacities.

1.5. **Evolutionary Explanation**

Evolutionary explanations in psychology are of a limited variety. Adaptationist explanations focus on adaptations as the result of a single evolutionary mechanism, natural selection. There has been much resistance to reconsidering adaptationist approaches, especially strong adaptationist approaches, since Gould and Lewontin’s (1979) Spandrels paper. Even recently, EP proponent Clark Barrett (2013) placed Gould and Lewontin into the “glass half empty” camp for their “equating evolutionary hypotheses with children’s fairy-tales that can be made up at a whim and bent to any purpose” (p. 247). Barrett also claims that the stance of the critics is that psychologists (Evolutionary Psychologists) should stay out of the evolutionary study of mind, brain and behaviour (p. 347), and that its study is futile because we either know everything already or will never know anything (p. 349). It is astonishing that such sentiments still exist, 36 years after the Spandrel’s paper. In light of my earlier discussion of the problems with strong adaptationism, Barrett’s arguments are hard to substantiate. Barrett is conflating critiques of a strong adaptationist approach to studying the mind and behaviour with critiques of the possibility of studying the evolution of mind and behaviour. These are not
the same thing. Nor do critics like Gould and Lewontin say that we know everything we can know about evolution. Rather, they state there are certain things that we cannot know, particularly the details of the patterns of life’s history: a problem that EP does not conceive as a problem.

Evolutionary theory needs to explain two general aspects of organismic form: consistency and change. Darwinian theory is able to deal with both aspects. In a broad sense consistency in form is explained by descent (and heredity of ancestral traits), change is explained through natural selection of variations in traits. Neo-Darwinism further explains these aspects in terms of genes, where consistency is due to inheritance of genetic factors and change is due to selection of genetic variants that give rise to survival enhancing traits (adaptations). Indeed, it seems much can be explained in evolution by focusing on heredity (i.e., descent). However, evolutionary explanations entail more than is implied by the strong adaptationist stance of EP.

1.5.1. Pluralistic Approach

A pluralistic approach, as argued by Gould (1997), involves using multiple lines of converging evidence to create an evolutionary account rather than relying on reverse-engineering adaptationist logic. The arguments for a broader approach to studying evolution are fairly simple, yet have been beyond consideration in the face of adaptationism being the default evolutionary approach in psychology. Adaptationism is an approach that equates evolution largely with adaptation. Adaptation, however, is only one explanation of organismic form. If understanding organismic form is the purpose of evolutionary investigation, then adaptation tells only a small part of the story. A pluralistic approach recognizes that other factors outside of adaptation influence evolution. Although “controversial,” it is difficult to see how an adaptationist stance, and especially a strong adaptationist stance, can be considered tenable if one seriously considers arguments about adaptations, byproducts, exaptations, and spandrels. Once again, these notions problematize the assumption that adaptations exist in the same form since their selection (van Valen, 2009). Natural selection and adaptation play a privileged role in adaptationist explanation, but it is more likely that psychological capacities are largely byproducts of past adaptations that have been put to different uses a long time ago. It would seem then, that the project of psychologists would be to explain our current
capacities not with reference to specific adaptations but to understand the interaction of adaptive structures which give rise to our adaptability. Our adaptability defines our species and it limits investigations and distorts the conception of our capacities to view them as a vast assortment of individually selected mechanisms that have persisted in the same form since the time they first arose.\(^{33}\)

As opposed to a particulate conception of adaptation, adaptability implies a more “dynamic” conception of evolution in which adaptations can be coopted for uses for which they were never selected. This situation can change the selection pressures a species can face, leading to different adaptations and exaptations and to the emergence of current adaptiveness that was never “selected for” in the sense meant by strong adaptationists. From this perspective, historical contingency is an important, yet nearly completely elusive component of evolutionary explanation. If one considers evolution as “changes in form,” then natural selection is only one mechanism that can explain change. The history of life involves not just biological factors but non-biological factors. Gould (1997) simply argues that any set of general principles (and especially so if one only invokes natural selection) are insufficient to explain the actual patterns of life’s history. That is, we simply lack details about what happened over the eons that comprise the histories of species. It is astounding that this notion is so controversial. In adaptationism, this historical story is ignored, and in any case, it is often unavailable. However, from a strong adaptationist perspective, this historical story is not lost; rather, it is contained in the design features of organisms. Hence, adaptations can be reverse-engineered to surmise the adaptive problem they evolved to solve.\(^{34}\) And here we are: the “old” or “strong” adaptationist stance, which Gould and Lewontin (1979) critiqued and put to rest 36 years ago in evolutionary biology, is alive and well in psychology.

\(^{33}\) Coyne and Berry (2000) in their critique of Thornhill and Palmer’s (2000) argument for rape as an adaptation, criticize the authors for stating two hypotheses: one that rape is an adaptation, and one that rape is a byproduct of other adaptations (in this case, male promiscuity and aggressiveness). However, this is still not the sort of explanation I advocate. It rests on the assumption that male promiscuity and aggressiveness are adaptations, and not themselves byproducts of other adaptations, for example. Adhering to Evolutionary Psychologists’ self-professed conservative approach to identifying adaptations, the onus is on the authors to prove this is the case.

\(^{34}\) Aspects of the historical can be gleaned from comparative studies of related species, but strong adaptationists even ignore this avenue of evidence in favour of reverse-engineering.
A few psychologists, like Evolutionary Psychologists, put evolution front and centre in their theories and empirical programs. And although most other psychologists may not do the same, or may want to avoid the topic altogether, all psychologists would acknowledge that “evolution is important” to some extent in understanding human minds, or at the very least, played an important role in how minds came to be. If psychologists are not taking an adaptationist position regarding evolution in psychology, they take the “interactivist” position. I detail this position below.

1.6. The “Interactivist” Position

Rather than saying that evolutionary matters are irrelevant, most psychologists take the “interactivist” position in explaining developmental outcomes. This is the position to take to avoid provoking any controversy in psychology. It is general enough in that it takes evolution as a given but does not make any strong commitments as to what this acknowledgment means. One of the few commitments it does make is fairly innocuous: that an organism is the product of an interaction between genetic and environmental factors. The genetic component is generally seen as what evolution (phylogeny) provides, and psychologists study how humans develop (ontogeny) while taking into account the environmental circumstances. Thus, any developmental outcome is a product of an interaction between genes and environment. This perspective has become the standard position in psychology, and few disagree with the general impetus of the statement, i.e., the ubiquity of these interactions means that neither source is “more important” than the other and supposedly puts an end to nonsensical dichotomous views such as “nature versus nurture” (see Tooby & Cosmides EP primer).

Although everyone invokes the importance of interaction, this platitude does little to explain development, and furthermore, it entails a problematic conception of

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35 Indeed, this is an “interactivist consensus” in psychology (Sterelny & Griffiths, 1999).

36 Granting both sources equal grounds sounds fair in theory but does not actually equate to paying attention to both sources in practice. Perhaps this is why Evolutionary Psychologists rallied for more explicit adoption of evolutionary theory, as the interactivist position essentially perpetuates the status quo of the SSSM, as evolution is simply “taken for granted” and social psychology becomes the study of the influence of variations of proximate environmental factors.
interaction. What I argue is the interactivist consensus, as ordinarily received, is still inherently a dichotomous view. On the one hand, seeing organisms as the products of genes and environments dissuades all but the most militant claims that a particular factor is “more important” than the other. On the other hand, this thinking does not get us very far. In fact it seems to simultaneously presuppose a problem and a solution in a particular way that may actually hinder our attempts to studying development within an evolutionary context. The issue is whether or not it is appropriate to conceive “genetic” and “environmental” factors as independent sources of variation. There are different perspectives on the nature of interaction. Developmental Systems Theory, for example, problematizes this separation (I will detail this perspective in a following section). However, as soon as this separation is made, it opens the door to misconceiving the nature of development.

It is difficult to avoid developmental dichotomies when development is conceived of as a product of two separate, independent factors. One factor is liable to be privileged above the other. This is especially so when the factors are conceived of as playing different roles in the interaction. Let us again examine the view of proponents of Evolutionary Psychology as they conveniently state explicitly their view of interaction, which is only implicit in the interactivist consensus. Far from any naïve belief that an “evolutionary” psychologist would favour the “nature” (genetic) side of explaining development, Tooby and Cosmides, in their EP primer, scoff at the very notion of “nature versus nurture.” They reiterate the consensus view that “Every aspect of an organism’s phenotype is the joint product of its genes and its environment” (their emphasis). Stating whether or not genes or environment play a more important role in an outcome is tantamount to asking whether the width or the length of a rectangle is more important to

37 I conjecture that if you goad them long and hard enough to explain “how it all (development) works”, the most ardent interactivist will revert to explaining it in terms of the supposedly outdated “nature versus nurture” perspective.

38 Another reason these authors argue that EP is a unifying theory for psychology is its apparent ability to put to bed forever such outlandish notions as “nature versus nurture”. As I demonstrate below, it does not do so at all.
determining is area. It is nonsensical. They go on to position the role of the genes, in a way they think is in line with the consensus, stating “Genes allow the environment to influence the development of phenotypes.” Further, “Indeed, the developmental mechanisms of many organisms were designed by natural selection to produce different phenotypes in different environments” (their emphasis in both statements).

Although organismic traits can indeed be conceived of as the product of this kind of interaction, such that parsing the “importance” of either factor is a futile project, it is not the case that these factors, in the received view of interaction, are on par in terms of what they contribute to organismic form. This is clear in what Tooby and Cosmides state about the interactivist consensus. The fact that genes have the potential to “allow” the environment to influence the development of phenotypes grants genes causal priority. In a sense genes are thought to provide organismic structure but environment is permitted to influence how this structure unfolds. Further, they state that developmental mechanisms were designed by natural selection to produce different phenotypes in different environments. There are two ways to read this. First, it speaks to phenotypic plasticity: developmental mechanisms are sufficiently plastic to permit organismic adaptation to different environments. I do not think this is what they mean in this case, however. The second way is to read this as saying that natural selection has designed adaptive phenotypes through the selection of the developmental mechanisms that give rise to them. That is, the design of adaptive phenotypes pre-exist in genes and/or the developmental mechanisms to which they give rise and that are brought about by the appropriate environmental conditions for the adaptive phenotype. Consensus may be reached on viewing the products of interaction, but the nature of this interaction is a different, neglected issue.

It is difficult to see how the interactivist consensus conceives of interaction any differently from what are considered to be “out-dated” views of interaction. In the interactivist view, environment is often considered of secondary causal importance. It is

39 Or so Tooby and Cosmides argue. Perhaps this is a poor analogy because length and width can be measured independently. However, as Lewontin (1983) points out, environment exists only in relation to a particular organism. That is, environment cannot be measured independently of the organism, and more importantly from a developmental standpoint, cannot be predicted or measured prior to the organisms’ development or it action in the world.
viewed as a “trigger” for a developmental program that is assumed to reside in genes. This is the view that genes contain “information” or “code” for phenotypic traits. This view is, arguably, still widely held in much of psychology. Individuals are considered to be the products of unfolding “developmental programs”, or “blueprints” (Lehrman, 1953, 1970; Griffiths & Stotz, 2000; Oyama, 1985, 2000; Oyama, Griffiths, & Gray, 2001; Planer, 2014; Stotz, 2014). Nativistic explanations about innate capacities, even if they emphasize the “biological” aspect of development, are still seen as a product of interaction. Even if there is little room for “experience” per se, experience or environmental factors are still at least seen as “triggers,” and is thus, under my characterization of the interactivist consensus, still an interactivist position. This problematic view of interaction goes beyond EP and nativistic developmental claims. It is widely held in psychology, but can be hard to notice because it is built in to basic concepts and empirical tools of psychology.

This assumption that one can divide and quantify the sources of developmental information into two independent sources is deeply rooted in psychology and is supported by the common practices and methods used by psychologists such as the analysis of variance (ANOVA) (Griffiths & Tabery, 2008; Meaney, 2010). Psychologists adopted methods derived from agricultural studies in the 1920s in an effort to increase scientific rigour. These studies examined the productivity of different strains of plant species under varying environmental conditions. ANOVA was developed to analyze the relative contributions between genetic and environmental factors. Measuring the genetic component of behaviour was referred to as heritability, and was introduced to psychology through the study of IQ in twins in the late 1950s (Richardson, 2013). The science of behaviour genetics, the study of the inheritance of behavioural traits, attempted to quantify the relative contributions of these two components, giving apparent
empirical validation to the conception that persists to this day in psychology, despite the interactivist consensus, a distinction between “nature” and “nurture.”  

Developmental models were built upon the assumptions of behaviour genetics, such as the commonly used reaction range model that describes the phenotypic range of a trait given a particular genotype and environment (Gottesman, 1963). This model formalizes the distinction between these two sources of variation: although the potential of any genotype was determined by environmental factors, the limits of phenotypic development were set by the genome. The variability in this interaction became known as the “range of reaction” in this model. The reaction range model suggests that if one could accurately measure both components one could predict developmental outcomes. Gottlieb (1992/2002; 2007) argues that the reaction range model represents the predetermined epigenesis stance of development, an explanation of organismic form that has existed for ages that he argues is commonly held in psychology where developmental information is thought to pre-exist in the organism itself. In modern times, in psychology, this information is often thought to exist in genes.

For a long time, the promises and projects of behaviour genetics to explain phenotypic variation seemed to only be impeded by insufficient tools. The hope of many in behaviour genetics was that future methods and technologies could substantiate the implication of their statistical models that suggested that there are “genes for” certain traits like personality (e.g. Bouchard Jr. & Loehlin, 2001). With the mapping of the human genome, for example, many anticipated that the project would provide the empirical evidence to realize what these statistical models predicted. However, to the surprise of many, these studies had the opposite effect. Rather than substantiate “genes for” certain traits, such projects have rather shown that the term is

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40 The phrase “nature versus nurture” was put forward by Sir Francis Galton, Charles Darwin’s cousin, in 1874. Galton is considered to be the father of behaviour genetics. Although the terms were introduced by Richard Mulcaster in 1582, and both were thought to be equally important, Galton put these two factors in opposition to one another, leading to the conceptualization that has dominated developmental thought since (West & King, 1987; see also Spencer et al., 2009).

41 One should use caution in accepting the promissory notes that scientists issue to future technological breakthroughs i.e., always being “10 years away!” from solving all the problems they currently face.
merely a metaphor for a much more complicated process. Single-nucleotide polymorphisms (SNPs), nucleotide differences that usually only have two alleles, being relatively rare, have become perhaps an unrepresentative, oversimplified model for the relationship between phenotype and genotype, leading to the assumption that genetic changes can have discrete phenotypic outcomes. However, the 1:1 relationship is not representative of most phenotypic outcomes because they are influenced by multiple genes. The realization that the genome is comprised of so little “coding” DNA and so much “non-coding” or regulatory DNA challenges further challenges the “genes for” nomenclature and has changed the focus of research to the factors that mitigate gene expression. No longer able to stave off criticism by writing a promissory note to future research, it is necessary to seriously consider the criticisms of the assumptions of behaviour genetics (see Gottlieb, 2003; Richardson & Norgate, 2005), heritability studies (Richardson, 2013; Taylor 2010), and the purported sources of developmental information (Oyama, 1985). These assumptions give persistence to the idea of “innate” traits and dichotomous developmental views (e.g., nature vs. nurture) that are argued to no longer exist under the interactivist consensus.

Development is a central issue here: what is the connection between genotype and phenotype? Can one actually measure the relative contributions of “genetic” and “environmental” components and how does evolutionary theory inform these assumptions? I argue that modern evolutionary theory has influenced models of development, as genes are taken to be units of inheritance that are the “evolutionary” components that contribute to the development of individuals. The problem is, however, that modern evolutionary theory does not speak to development at all. Much confusion and misconception continues to persist because a non-developmental theory of evolution is obviously a poor candidate to inform models of development. In the following sections I provide a brief history and review of the tenets of the neo-Darwinian Modern Synthesis. I will demonstrate how and why it is a non-developmental theory and characterize the view of development that it seemingly, yet improperly, informs. I will also discuss again the limits of adaptationism in the context of utilizing a non-developmental evolutionary theory. I will then discuss efforts to formally address development in evolutionary theory, and discuss what this means for evolutionary explanation in psychology.
1.7. Neo-Darwinism

The strong adaptationist stance, critics argue, is based upon an interpretation and application of modern evolutionary theory that has been dubbed ultra-Darwinism (Gould, 1997; Reid, 2007). They define ultra-Darwinism as the reductionistic, gene-centric view of evolution that is borne out of Darwinian and neo-Darwinian evolutionary theory and its focus on natural selection. Natural selection is an important concept in adaptationist explanation. It is likely that Evolutionary Psychologists as strong adaptationists believe their stance gets clout from the particular version of natural selection that stems from ultra-Darwinism. Natural selection refers to how traits become dominant in populations due to the differential reproductive advantage those traits have over others in a population. Ultra-Darwinism takes Darwin’s metaphorical use of natural selection (itself a metaphor based on the selective breeding in domesticated animals) and treats it as an actual force that molds and shapes organismic form over time (Eldredge, 1995). Thus, organisms’ fitness is determined by their inherited adaptations (i.e., the ones that were “triggered” to develop within the context of their physical and social environments during their life course) and by natural selection (which re-selects the forms that prove to maximize fitness). However, natural selection is not an external force that acts upon organisms and chooses which traits perpetuate and which disappear, as it is commonly thought to do in its colloquial sense. What we call natural selection is an outcome of variation in traits amongst organisms living, reproducing, and interacting with each other and their environments. Ultra-Darwinists often think of natural selection as an active agent, yet if it is considered only to be the filter of variation, it cannot also be thought of as the source of variation. Such an untenable position on natural selection is part of why critics refer to modern evolutionary theory as “ultra”-Darwinism: its hegemonic position leaves it blind to its own inconsistencies or problems.

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42 It may be more appropriate to perhaps refer to this as “ultra-neo-Darwinism”, particularly the genocentric aspect; however, critics argue that this stance is based on Darwin’s contradictory reference to natural selection as both a passive and active agent in evolution (Reid, 2007, p. 27).

43 Evolutionary Psychologists claim that natural selection is the only component of the evolutionary process that can introduce complex functional organization into a species’ phenotype (and Tooby & Cosmides cite Williams and Dawkins in their EP primer).

44 Selection “pressure” is another unfortunate metaphor run amok as a function of natural selection’s promotion to an active ontological/causal status.
with its basic assumptions. If we view natural selection as an outcome of organisms developing and interacting with each other and their environments, this changes our focus in evolutionary studies as:

The history of interactions between those organisms and their environment, along with the experimental nature of reproduction, are causally sufficient to explain evolution. Improved usefulness or adaptiveness arise as qualities of some of those experimental evolutionary novelties. Thus, evolutionists should concentrate on the generation of change and regard differential survival and differential reproduction as epiphenomenal to evolution. (Reid, 2007, p. 58)

Reid’s (2007) view of what the focus of evolutionary studies should be is quite different from the view put forth by neo-Darwinism. It is necessary to examine how such vastly different views on evolution can exist when the Modern Synthesis is thought to be the backbone of the biological sciences. In the following, I outline the tenets of neo-Darwinism by providing a brief history of the development of the Modern Synthesis and the influence it has had on psychology, namely, the role the Modern Synthesis has had in improperly informing models of development in psychology.

1.7.1. **A Brief History of Neo-Darwinism**

The modern evolutionary synthesis, based upon the integration of Darwinian selectionism and Mendelian inheritance, was formed between 1936 and 1947 (Lewontin, 1983). Huxley provided the name of the theory in his 1942 book *Evolution: The Modern Synthesis*. Darwin’s (1959) account of species evolution presented a strong model of phenotypic change but it lacked an adequate model of heredity. Darwin provided a provisional model called pangeneses (a “blending” model of inheritance) that was not without problems that even Darwin noted, including not having a material stratum for which these hereditary particles, or gemmules, could have their effects. It was the (re)discovery of the particulate inheritance model derived from Mendel’s now famous pea plant experiments that provided the missing component of Darwin’s theory of evolution.

See Mayr and Provine (1980) for a detailed account of the creation of the Modern Synthesis.
The science of population genetics, which developed between 1918 and 1932, played an important role in the development of the Modern Synthesis. Most importantly, it provided the evidence that Mendel’s particulate model of inheritance was consistent with Darwin’s notions of natural selection and gradual evolution. Because of this privileged position of providing the missing component to Darwin’s theory, it was able to greatly influence the structure of the synthesis itself. Population geneticists promoted their field by stating that the science of genetics was the way to advance evolutionary theory. They did this by offering a means of tracking evolutionary change: demographic analysis of changes in allele frequencies in populations. In promoting itself as the wave of the future in evolutionary theorizing, population genetics distanced itself from developmental science. Previous to this time, the study of development (at the time called embryology) and the study of heredity were a unified science until the rise of the study of genetics in the 1920s. In this separation, genetics became the study of the transmission of hereditary traits while embryology became the study of the expression of those traits (Gilbert, 2012, p. 21). Thomas Hunt Morgan’s championing of genetics profoundly influenced the Modern Synthesis, formally separating the fields of genetics and embryology. Genetics represented a “new school” that according to Morgan, put the study of evolution on the proper track (Morgan, 1932a, b), in essence, bringing evolutionary biology out of the domain of natural history into the domain of science (Gilbert, Opitz, & Raff, 1996). Morgan’s student, Theodosius Dobzhansky, went a step further and redefined evolution as “changes in gene frequency” (Gilbert et al., 1996).

With the Modern Synthesis, the conception of evolution changed. Understanding evolution as the “transmutation of species” thus fell away as the Modern Synthesis adopted a population-genetic framework, along with a new definition of evolution. Thus, because they were not explicitly addressed, developmental issues were automatically considered to be of secondary importance. Population geneticists were not concerned with the complicated problem of how genes are realized in ontogeny as phenotypes. Dealing with the “messy” details of development would only hinder progress in the study of evolution (Griffiths & Neumann-Held, 1999; Sansom & Brandon, 2007; Sterelny & Kitcher, 1988). The classical geneticist’s view of heredity only focused on the transmission of genes, and by implication, reliable developmental processes were simply presupposed (Robert, 2002; Sapp, 1987). Development was essentially “black boxed”
(Hamburger, 1980), and by using phenotypic outcome as a proxy for allelic variation, the
argument was that great strides in evolutionary biology could be made in studying allelic
distributions in populations.

It should be noted that another reason development was left out of the Modern
Synthesis was that, ultimately, embryology was not able to advocate as strongly as
population genetics for having development included in the Modern Synthesis. It is
interesting to note that before and during Darwin’s time up until the Modern Synthesis,
changes in development were thought to provide the mechanism for evolutionary
change (see Gould, 1977). However, following the rejection of Haeckel’s Biogenetic
Law as a potential mechanism, embryology had already sufficiently refocused on
empirical research and separated itself further from evolutionary biology. As a result,
embryology did not offer a developmental mechanism that could be adopted for use in
the Modern Synthesis. Population genetics could offer a mechanism, and without
competition, along with the influence of its proponents, genetic change became the de
fatto mechanism to explain evolution.

For several reasons, development was left out of the Modern Synthesis. It is not
that the founders of neo-Darwinism did not recognize that developmental issues like cell
differentiation were not important, but, rather, they acknowledged that such issues were
complicated and not yet fully understood. What they had at a time was a population-
genetic framework that could lead to a “mostly” complete evolutionary synthesis where
reliable development merely had to be assumed. By defining evolution as changes in
allele frequencies in populations, the individuals and their development was glossed
over, leaving over a genetical theory of evolution (D. Moore, 2008). Population
geneticists’ assumption of reliable development became an assumption of the Modern
Synthesis. Thus, the Modern Synthesis greatly simplified the problem of evolution,
reducing it to a matter of understanding heritable variation by means of mutation and

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46 Haeckel’s recapitulation theory is that “ontogeny recapitulates phylogeny”, meaning that over
the life course of an animal it goes through developmental stages that resemble the ancestral
forms representing the organism’s evolutionary history. This idea could be viewed as a
reasonable summation based on some aspects of embryological development, but his ideas
were most likely so vociferously attacked due to his desire to synthesize Lamarkian and
Darwinian concepts.
genetic recombination and segregation, and natural selection. Instead of being a phenotypic science that analyzes change in morphological or developmental structures, evolution became epiphenomenal to the genetics of populations (Gilbert et al., 1996; Sansom & Brandon, 2007).

Although the Modern Synthesis was a framework of great explanatory power, the fact that it was a nearly comprehensive theory has had greater implications than its founders may have expected, in that what it did leave out begs the question of just how comprehensive the theory actually is. Far from being concerned with development as something divorced from genetics, Conrad Waddington was a developmental researcher known for his integration of genetics and development; his work laid the foundations for systems views of biology and epigenetics. Waddington was a critic of the neo-Darwinian framework’s neglect of development and its overly simplistic models of gene selection and trait evolution. Waddington (1941) argued that:

a theory of evolution requires, as a fundamental part of it, some theory of development. Evolution is concerned with changes in animals, and it is impossible profitably to discuss changes in a system unless one has some picture of what the system is like. Since every aspect of an animal is a product of development, or rather is a temporary phase of a continuous process of development, a model of the nature of animal organization can only be given in developmental terms. (p. 108)

The Modern Synthesis emphasizes population genetics over development. However, such a separation is largely artificial in terms of understanding the mechanisms of evolution, and is a product of trying to create a state-of-the-art model of evolution based on the state-of-the art in biology (i.e., population genetics) and ignoring aspects that were less understood at the time (i.e., development).

It would seem that modern evolutionary theory is lacking something crucial. The aspects of phenotypic evolution that cannot be explained by the Modern Synthesis are those aspects that are developmental in nature. Rather than deal with allele distributions, psychologists are primarily concerned with individuals. Individual organisms are outcomes of the very developmental processes that the classical geneticists ignored
in order to simplify statistical modelling (Alberch, 1991; Gottlieb, 1992/2002). This evolutionary paradigm does not include development. Rather it only assumes that it reliably occurs. Assuming reliable development in a model that focuses on genes inherently leads to a view in which genes are considered to be: the de facto creators of developmental mechanisms, the instigators of developmental processes, and the storage units of organismic information. Such a view is prevalent in psychology, where development is seen as being largely directed by genetic activity (Gottlieb, 1992/2002). However, this is simply a caricature of development: the relationship between genotype and phenotype is misconstrued. This confusion manifests in developmental psychology in the concept of "innate" traits.

1.8. Innate Traits and Psychology

The notion of “innate” traits is frequently invoked in developmental psychology. Despite its frequency of use by psychologists, it is unclear what exactly this term means. Difficulty with defining innateness is a function of many factors, including different uses of the term and its use to describe various phenomena. Most importantly, however, this concept seems to be based upon very old assumptions about the nature of development and the misuse of a non-developmental evolutionary metatheory that gives these assumptions apparent support.

These old assumptions in psychology are preformationist assumptions about development; that the outcomes of development are predetermined and dictated, most often in modern thought in psychology, by genes (Gottlieb, 1992/2002). Psychologists have interpreted the Modern Synthesis as supporting these intuitions about development: the population-genetic framework tracks alleles as proxies of phenotypic traits. Psychologists have mistaken these proxies as causal entities; however, the connection between genotype and phenotype is not explained by the Modern Synthesis and its population-genetic framework. These two levels of organismic form are linked by development. Given that the Modern Synthesis is a non-developmental evolutionary theory, the actual connection is not addressed. The assumption is that development reliably occurs: whatever the complicated processes that comprise development are, they are simply taken for granted. Psychologists seem to have taken “reliable
development" to mean that the causal connection between genotype and phenotype is one where genotypes must contain information about organismic traits, hence there are “genes for” traits and “organismic blueprints.” The founders of the Modern Synthesis did not posit such a connection, but such a notion fits well with the preformationist assumptions held by psychologists. Thus, many aspects of phenotype were considered to be “innate” traits, meaning that they were causally connected to genetic factors; a popular notion that exists to this day in psychology. Beyond the term’s dubious use as an explanation of the link between genotype and phenotype, other factors contribute to the problematic nature and use of the term.

Innateness is a rather complex concept and is too convoluted to have much use in psychology, and this is especially so because it is employed in different ways. One of the issues with it is that it is a commonly used word that when used colloquially it is typically non-problematic and is part of “folk-biological” explanations of human and animal natures. The issue is whether the term has any utility within scientific discourse and for several reasons I would argue it does not. One problem is that innateness is often invoked as both a description as well as an explanation of species-typical capacities so the two uses of the term are often conflated. Saying something is innate can be used as a general description of the presence of a trait (e.g., that it reliably develops) or can refer to the underlying causal mechanisms of the trait (e.g., that it is an adaptation or it is a “biological” or “genetic” trait). However, for developmental psychologists, circularity ensues, as innateness is used as both a description and an explanation of a trait. If used as a description of its reliable development, it is certainly not an explanation of its reliable development: the job of a developmental researcher is to describe how traits develop. Labelling a trait as innate often leads to a tacit assumption of reliable development, apparently negating a developmental analysis which would seem to be called for in an explication of the mechanisms and processes that underlie the emergence of any trait.

Moreover, labelling a trait as innate can refer to a number of related aspects that fall under the concept. Some of the popular usages include traits that reliably develop, those that are unlearned, those that appear quickly, and those that appear early in development (e.g., joint attention) or appear later in development (e.g., secondary sexual traits), etc. Immediately there are red flags here in terms of what sense of “innate”
is being employed. Innate traits may emerge early, but other traits thought to be innate, like secondary sexual characteristics, occur during the transition into adulthood. One of the common threads connecting these usages is that so-called innate traits are typically considered to be traits that are set apart from “acquired” traits, i.e., traits that vary largely due to learning and experiential factors. Once again, however, such dichotomous views of development are doomed from the very start. The distinction between “innate” and “acquired” traits does not make sense even under the charitable reading of the interactivist stance, as it is not clear at any point in development what portion of a trait is constituted by the “innate” and the “acquired” (that is, this is not a useful means of describing the development of psychological traits). Yet, to psychologists, there still seems to be something right about the concept of innateness. Indeed there may be, but it is likely that innateness does not refer to a natural kind, but to a set of potentially related natural kinds (Mameli & Bateson, 2011). It is not clear how the same term that can be used to refer to so many different things (that are sometimes related, sometimes not, depending on how the term is employed) has unproblematic usages in science. Mameli and Bateson (2006) provide a thorough examination of 26 possible meanings of the term innate and find no single definition that is useful as a scientific concept.

The concept of innateness is insufficient for research in developmental psychology. It is a folk-biological term that at best is too convoluted to have any utility in research. What is worse is that it is an outdated notion that receives apparent support from a misconceived connection between genotype and phenotype that stems from an evolutionary metatheory that completely left out the difficult problem of relating these two levels of organismic form. Current reference to the innate in psychology is simply passing the buck on to a lower level of analysis, assuming that genetics will somehow provide the whole story for explaining development. But passing the buck, even if it is in a legitimate case, which it is not, clearly cannot be the purview of developmental research in psychology. Whether or not we should salvage the term in developmental psychology is not the main issue. The main issue is that innate claims in psychology are often based upon problematic conceptions of development. If our notions of development are informed by the evolutionary metatheory, as I argue they are, then we need an evolutionary metatheory that explicitly addresses development.
1.9. Why is Development Important?

Failing to explicitly incorporate development into evolutionary theory has led many to refer to the Modern Synthesis as the “unfinished synthesis” (Eldredge, 1985; Reid, 1985), and an increasing number of evolutionary biologists have been calling for an “expanded evolutionary synthesis” (Carroll, 2008; Gilbert, 2003; Kutschera & Niklas, 2004), an “extended synthesis” (Martinez & Esposito, 2014; Mesoudi et al., 2013; Müller, 2014; Pigliucci, 2007; 2009; Pigliucci & Finkelman, 2014; Pigliucci & Müller, 2010) or a “developmental synthesis” (Amundson, 1994) that disintegrates development and evolution. An evolutionary framework that does not include development leaves only a gene-centred biology that presents explanations that border on long rejected notions of preformationism with the idea that genes contain developmental information (Callebaut et al., 2007; Planer, 2014; Stotz, 2014).

There has been increasing appreciation that development is a missing component to evolutionary theory. The fact that evolutionarily relevant organismic changes occur in individuals during development has led to a shift in focus on genes in evolution per se to a broader conception in which they are seen as playing a role in modifying developmental processes. Thus, rather than seeing evolution as changes in gene frequencies, evolution is seen as the reorganization of developmental systems (Shishkin, 1992). Oyama (2000) states that when “systems of developmental processes change, in constitution or in prevalence, evolution occurs” (p. 342). Jablonka and Lamb’s (2007) definition includes genes but extends inheritance beyond genetic factors in that evolution is defined as “a set of processes that lead to changes in the nature and frequency of heritable types in a population” (p. 470). The (re)integration of development and evolution is the basis of evolutionary-developmental biology (evo-devo) (Carroll, 2005, 2008; Gilbert, 2001, 2003; Gilbert et al., 1996; Gilbert & Epel, 2009; Hall, 2000;

There have been many critiques of genes as containing developmental information (Godfrey-Smith, 2000a; Griffiths, 2001; Jablonka, 2002; Maynard Smith, 2000; Planer, 2014; Sarkar, 1996; Sterelny, 2000). In the most useful application, genes code for amino acid chains that fold to make proteins, otherwise, stating that genes contain information about biological form is merely metaphorical. If taken literally, is the case that there simply are not enough genes to determine the phenotype in this way. For example, Ehrlich (2000) points out that there are 100-1000 trillion synaptic connections in the human brain, far too much “information” to be contained in any genetic blueprint.
Ioannidis, 2008; Laland et al., 2008; Love & Raff, 2003; Müller, 2007; Pigliucci, 2009; Pigliucci & Müller, 2010; Robert, 2002, 2003, 2004; Sansom & Brandon 2007; Wagner, 2000; West-Eberhard, 2003). In this field it is recognized that “most evolutionary changes are introduced during ontogeny, in the sense that ontogenetic modifications, and modifications in developmental processes, produce evolutionary changes; moreover, developmental mechanisms themselves evolve” (Robert, 2002, p. 599). Thus evolution is not limited to changes in allele frequencies, but refers to changes in developmental processes of individual organisms. Evo-devo studies the evolution of developmental mechanisms and how these mechanisms lead to the evolution of phenotypes based on both genetic and non-genetic variation. Some see evo-devo as the incorporation of molecular biology and evolutionary theory. Rather than seeing evolution as the creation of “new” genes, evo-devo notes how gene networks are conserved in evolution and how large-scale morphological changes can occur through changes in gene regulation.

Heterochrony\(^{48}\) refers to changes in timing of developmental processes. De Beer (1958) noted that such changes could lead to novel traits that could actually drive evolution. Heterochrony may have played an important role in human evolution. The slowing of development leading to the perpetuation of juvenile traits is referred to as neoteny. Neoteny has been thought to characterize not only human physical traits, such as retaining a fetal skull shape (Gottlieb, 1992/2002; Penin, Berge, & Baylac, 2002), and our relative hairlessness (Rantala, 2007), but the prolonged juvenile state has been thought to play an important role in the development of our psychological capacities (Portmann, 1944/1990). For example, Portmann argues that increasing skull size in humans put a selective pressure on earlier birthing to ensure passage through the birth canal. Humans born in this more altricial state than other apes allows humans to benefit from an “extrauterine year” of social and cognitive development. The relative helplessness in human infants has been suggested to be one of the preconditions for joint attention and communicative development. Analogously, chimpanzees in captivity come to point without training because this “referential problem space” mirrors the situation of helpless human infants (Leavens, Hopkins, & Bard, 2008). Shanker and

\(^{48}\) A concept originally conceived of by the maligned Haeckel and developed by de Beer (1958).
Greenspan (2004) suggest that this extended attachment period to primary caregivers sets the conditions for emotional communication, a crucial component of social communicative development.

The novel traits that arise due to changes in timing in development, leading to many neotenous traits in humans, may be a key, and largely overlooked, aspect that explains human psychological capacities. Acknowledging the existence and significance of these traits is a result of employing a perspective on evolution that considers evolutionary novelty to be something that is created through changes in development. Failing to take seriously heterochrony is not unexpected when the dominant evolutionary metatheory demands researchers couch explanations in terms of gene action in a model that equates phenotypic changes simply to mutational genotypic changes, rather than how genes can change the timing of developmental processes. These changes can indeed come about by changes in gene regulation, but such changes need not be due to point mutations. In a gene-centric view, a prolonged adolescence does not seem prudent in terms of fitness, and in fact, seems counter intuitive: the main goal of development in this perspective is to reach sexual maturity to procreate successfully, the sooner the better. Considering the prolonged juvenile state in humans, and the persistence of these characters, heterochrony is likely an important key to understanding the evolution of our psychological capacities.

1.10. Developmental Systems Theory

The problems with neo-Darwinian theory have been known in some areas of psychology. A series of empirical and conceptual insights led to the creation of a view of development that contrasted with the conception that the Modern Synthesis seemed to dictate. These problems became known in the field of comparative psychology in the 1950s. At the time, Lorenz’s view of instinct was once the dominant model of development in animal behaviour research. Lorenz’s view could be characterized as an “interactivist” perspective, in that he assumed a separation between “genes” and “environment,” and though he says organisms are products of interactions between these factors, the factors did not have equal causal effect. Lorenz considered certain traits to be largely under “genetic” rather than “environmental” influence. For instance, in
the attachment mechanism of "imprinting," the mechanism itself was thought to develop independently of the experience of the organism; however, experience was necessary to determine with what the organism would form an attachment. Under this model, subjects could be raised in social isolation to minimize the influence of experience on the development of the mechanism, and the organism would be exposed to stimuli in the laboratory during testing to examine how the mechanism functions. Daniel Lehrman's (1953, 1970) seminal insight, in a critique of Lorenz's work, was that raising organisms in social isolation did not eliminate experience; it merely led to an impoverished developmental experience. Experience is not something that can be eliminated; it can only be changed.

Lehrman's insight and those of others (e.g., Beach, 1955; Kuo, 1921; Schneirla, 1956), was integral in changing the way psychologists thought about development. Developmental Systems Theory (DST) was formalized with Susan Oyama's (1985) critique of the received view that genes contain "developmental information." She argued, rather, that developmental information is created anew in each generation through the dynamics of development, that is, through the bidirectional interactions of multiple levels of the organism. DST emphasizes equal contributions from all factors influencing development, including genetic, epigenetic, and environmental factors.49 From this perspective, organisms do not just inherit DNA, but they inherit numerous epigenetic resources at a cellular level (Gray, 1992; Johnston & Gottlieb, 1990; Oyama, 1985; Oyama et al., 2001; Moss, 1992; West-Eberhard, 2003, 2005). For example, these resources include basal bodies, DNA methylation patterns, membranes, and organelles (Jablonka & Lamb, 2005; Jablonka & Raz, 2009; Moss, 1992). Inheritance is extended beyond cellular structures to include any factors that reliably reoccur in the development of a species that influence development. For example, inheritance is also extended to ecological factors such as symbiotic microorganisms, social traditions (i.e., culture), and aspects of habitat (i.e., certain environmental affordances or niche factors) (Gray, 1992).50 As stated earlier, even though gene-culture coevolution makes culture

49 Not prioritizing any factor over another encourages focusing on how these factors influence one another, leading to a focus on processes rather than privileging genetic factors.

50 The particular cultural niche humans inhabit has been increasingly considered to be an integral aspect of human evolution (e.g., Boyd, Richerson, & Henrich, 2011)
evolutionary important, from a DST approach this is taken as a given, and makes the distinction between biological and cultural largely artificial.

1.10.1. The Individual in Evolution

Re-evaluating the role of genes as part of a larger developmental system has led to a change in focus on organisms and how evolutionary change takes place first in the individual. In an extreme gene-centred view of evolution, organisms are “vehicles” for genes, and selection favours those genes that build the best adapted vehicles (Dawkins, 1982). However, rather than relegating evolutionary change to the random action of genetic mutation and the selection of the favourable traits those mutations supposedly give rise to, it is being increasingly recognized that evolutionarily relevant change is initiated by the behaviour of the organism itself. The “constructionist” or “dialectical” view of development recognizes that organisms are not passive participants in evolution, and that organisms’ actions can have direct evolutionary implications (Lewontin, 1983). This view recognizes the neglected aspect of organisms’ behavioural adaptability (Bateson & Martin, 2000; Reid, 2007). Organisms can actively select and modify their own environments in a process called “niche construction” (Laland, 2014; Laland, Odling-Smee, & Gilbert, 2008; Odling-Smee, Laland, & Feldman, 1996, 2003). Active control of the environment, and the capacity to choose environments and sexual partners, can lead to rapid evolutionary change (Bateson, 2013; Bateson & Martin, 2000; Corning, 2014; Johnston & Gottlieb, 1990). Contrary to the neo-Darwinian paradigm, evolutionarily significant changes can emerge not through genetic changes, but through behavioural changes:

Change in trait frequency involves genetic accommodation of the threshold or liability for expression of a novel trait, a process that follows rather than directs phenotypic change. Contrary to common belief, environmentally initiated

51 “Supposedly” because mutations do not literally give rise to phenotypic changes as something that can be conceived of as existing outside the influence of all the factors involved in development.

52 Even though Dawkins’ (1982) extended phenotype pushes the notion of phenotype beyond what is typically associated with neo-Darwinism, his is still a genocentric view where genes are selected based on the adaptive advantages those extended phenotypes provide to the genes (Griffiths & Gray, 1994).
novelties may have greater evolutionary potential than mutationally-induced ones. Thus, genes are probably more often followers than leaders in evolutionary change. Species differences can originate before reproductive isolation and contribute to the process of speciation itself. (West-Eberhard, 2005, p. 6543)

Gottlieb (2002) describes the neophenogenetic pathway, i.e., the way evolutionary change takes place. The first stage in evolutionary change is an alteration in development that leads to behavioural changes that animals experience living under different conditions from their forebears. The second stage involves morphological change, where new environments bring out latent morphological and physiological developmental possibilities without changes to underlying genetic structure. The third stage of evolutionary change involves the long term geographic or behavioural isolation that is normally associated with evolutionary change in the Modern Synthesis (i.e., genetic change). However, by the time this final stage is reached, phenotypic evolution has already taken place. Genes may rightly be considered to be part of the developmental system that adds to the consistency of species-typical development; however, it may not be considered to be the original “source” of consistency as it is thought to in the standard preformationist interpretation of the origins of biological form.

1.11. Challenges of Integrating Evolution and Development

There seems to increasing agreement that organismic evolution occurs through changes in developmental processes. How this integration will take place in terms of evolutionary theory, however, is still not clear. Integrating development and evolution is not a straightforward task. One of the issues is that development and evolution take place at different levels of analysis. Development occurs at the individual level and evolution occurs at the population level (D. Moore, 2008). Even though evolution is necessarily dependent upon changes in development at the individual level, evolution can only be observed at a species level across multiple generations. In this respect, even though many critics are calling for a developmental revolution in evolution theory, it seems the difference in levels of analysis presents a challenge to a fully unified evolutionary theory. It is not clear how far evo-devo will take us in providing a new evolutionary theory because it still seems to be a gene-centred discipline and in many
ways still grounded in a neo-Darwinian paradigm. Further, considering that neo-Darwinism is the foundation of the biological sciences, there is much infrastructure in place and any challenge to the paradigm would need to draw out neo-Darwinisms’ core inadequacies, instead of being consumed by the already established framework and becoming simply a particular approach or area of study within neo-Darwinism (which evo-devo may well be). Psychologists absolutely need to be keeping up with developments in evolutionary biology (see Finlay, 2007). However, DST is a better general perspective for developmental psychologists. Psychologists typically do not deal with genes, so assuming parity of developmental influences can encourage psychologists to focus on developmental processes and discourage passing off the heavy lifting of developmental explanation to future studies involving “lower” more “biological” levels of analysis. A potentially important issue though is that DST does not speak much about evolution per se and is more useful for clarifying thinking about development. When DST advocates speak of evolution it is in terms of changes in the entire developmental manifold. Such a perspective would need to be formalized in a new evolutionary theory, but in this respect, it is similar to evo-devo in expanding conceptions of evolutionary change.

Another issue has to do with the insistence that development and evolution are intrinsically linked. Indeed they are, but how this is conceptualized is not clear, because, if this is true, it could be the case that the separation between the two is another false dichotomy. If development and evolution are not something that can be thought of as separate processes, then the notion of “integrating” them, which is in vogue right now in evolutionary biology, may not make much sense. This situation mirrors the dichotomous view inherent in the interactivist consensus: both “development” and “evolution” are important in explaining organismic characters (the consensus) but how they interact is the question (and in few cases would they both be seen to be “on par” according to the interactivist consensus). The very notion may be suspect, and if this is the case,

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53 The term “evo-devo” is used to refer to the currently dominant approach to the task of re-integrating evolution and development. Debate exists about the nature and impetus of the integration of developmental and population-genetic models, (i.e., evo-devo, devo-evo, devgen-popgen, and eco-evo-devo) (Gilbert, 2003; Müller, 2007). The evo-devo synthesis may be in vogue right now just because it is still fairly consistent with neo-Darwinian theory.
psychologists are misinterpreting development and evolution and their intrinsic connection.

1.12. Revisiting Evolutionary Explanation

As previously described, using an adaptationist perspective derived from a non-developmental evolutionary metatheory has major limitations for the study of psychological capacities. At a metatheoretical level, a new evolutionary theory that fully incorporates development is needed to complete the "unfinished synthesis." This change is likely to occur in other disciplines like evolutionary developmental biology. Psychology, however, cannot wait for this to happen and psychologists may indeed be able to encourage such changes by challenging the standard theory in their own discipline rather than tacitly accepting it. Psychologists need to adopt a perspective on evolution that focuses on changes in organismic form or changes in developmental systems in lineages rather than the current limited view regarding changes in allele distributions or the selection of beneficial point mutations. In this respect, psychology can make important contributions to evolutionary studies (see Lickliter, 2014; Wereha & Racine, 2012), rather than evolution being solely the domain of population geneticists and evolutionary biologists.

What is also needed is a pluralistic approach in psychology. Part of a pluralistic approach is recognizing that we are putting together a puzzle with many pieces, not a problem that can be explained by post-hoc functional analysis. From a pluralistic perspective an evolutionary explanation is necessarily an incomplete explanation (i.e., the details of historical contingencies), but there is still much we can learn from the convergence of evidence from many different disciplines, none of which has the whole-story. The cohesiveness of our evolutionary explanations must be judged against this evidence, not against the evolutionary metatheory itself. In the current adaptationist climate in psychology, the most salient change that would take place would involve properly placing the notion of adaptation within a broader evolutionary context rather than framing everything pertaining to evolution around the notion of adaptation. Further, the study of psychological capacities would involve how past adaptations have been (re)used or redeployed to support our adaptiveness, and how changes in the timing of
development (heterochrony) promote adaptiveness. Such a view of evolution has implications for how we view our evolved nature: we are not bounded collections of countless individual specifically-selected adaptations. Rather, it is our relatively unbounded psychological and behavioural flexibility that is the characteristic that baffles psychologists when they ponder in what way “human cognition seems very different” (Tomasello & Carpenter, 2007, p. 121).

It is my contention that problematic “old” adaptationist claims are not limited to the claims of Evolutionary Psychologists, but that similar ones are made in research into early social understanding in human infants. This research area can be seen as a nexus for the issues discussed up to this point. It is an area of psychology where a non-developmental evolutionary metatheory and developmental phenomena inevitably meet. Rather than leading to an outcry by psychologists in the face of the incommensurability of these issues, they have instead mistakenly put their faith in the hegemonic biological metatheory, leading them to derive consistency where none exists. As I will demonstrate in the review that follows, inconsistent claims are not uncommon in this area, and an inappropriate evolutionary metatheory is a large part of the cause.
2. Review of the Developmental and Evolutionary claims of Social Cognitive Developmental Research

In this section I review the developmental and evolutionary claims made by psychologists conducting early social cognitive developmental research. This area has broadly been referred to as “theory of mind” research, which refers to research that investigates how infants come to understand and navigate their social world.\(^5\) This research garners so much attention because it is thought that the early forming capacities in infancy, such as the capacity for joint attention (the ability to coordinate attention with another) are foundational for human forms of interaction and cognition. Due to the significance of these fundamental capacities, there has been much interest in their evolutionary origins. Many researchers make explicit claims about their origins. Others do not make evolutionary claims per se, but make developmental claims that are consistent with a deterministic view of development that stems from the non-developmental evolutionary metatheory in psychology. Further, many researchers rely on comparative research to argue which capacities humans share with other primates and which ones are unique to our species, and often make similarly problematic claims.

At this point, I will reiterate that the issues surrounding evolutionary explanations are complex, and this dissertation is meant to be a step in elucidating some of the problems as they relate to developmental psychology. The issues that pertain to psychology need to be worked out primarily by psychologists. There have been too many promissory notes written to scientists outside of psychology and too much

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\(^5\) The term “theory of mind” refers to a particular research program that has, unfortunately, to some extent, become synonymous with social understanding research in general. However, because not all researchers ascribe to the “theory of mind” approach or its assumptions, it is more appropriate to simply use the terms “social understanding” or “social cognition” research (Carpendale & Lewis, 2010). “Theory of mind” will be used in this review when it is explicitly stated by particular researchers.
uncritical incorporation of notions from biological disciplines that have resulted in an oversimplification or distortion of the explananda of our studies.

Perhaps some researchers understand the difficulties in creating evolutionary explanations and therefore tend to avoid making such claims and choose to let the empirical evidence “speak for itself.” This seems to the approach taken by a group of psychologists known as core knowledge theorists. Core knowledge theory posits that humans, and some animals, have basic sensitivities to different kinds of information about the physical and social world (see Spelke, 1995; 2000; Spelke, Breinlinger, Macomber, & Jacobson, 1992; Spelke & Kinzler, 2007). This includes core knowledge regarding domains as diverse as mathematics and moral behaviour.

The developmental and evolutionary rationale of core knowledge theory is that adaptations “pre-equip” infants with an infrastructure that leads to the development of these domains in ontogeny; a starting point that accounts for humans’ rapid or high-level development in different conceptual areas. As Spelke and Kinzler (2007) state:

humans are endowed neither with a single, general-purpose learning system nor with myriad special-purpose systems and predispositions. Instead, we believe that humans are endowed with a small number of separable systems of core knowledge. New, flexible skills and belief systems build on these core foundations. (p. 89)

This “insight” or middle-ground position on a potentially problematic continuum is what core knowledge researchers see as a radical alternative to the evolutionary study of psychological capacities. While EP seems to address the inadequacy of a “general-purpose learning mechanism” posited by the SSSM (which itself is a strawman position posited by Evolutionary Psychologists), core knowledge theorists do not see adult cognitive capacities as particulate adaptations, but as products of developmental processes that are based upon a small number of foundational adaptations. As should be clear by now in this dissertation, the difference between these two positions is superficial. The reasoning is essentially the same. The difference comes from the fact that core knowledge theorists are not positing specific adaptations as explanations for adult capacities, and so they avoid the most egregious violations of positing “just-so”
stories. However, it is difficult to see how the assumption that core capacities/adaptations exist does not lead to "just-so" explanations when core capacities for math and racial differentiation are posited. In this respect core capacities are simply "just-so" redescriptions of human capacities that, again, masquerade as developmental and evolutionary explanation. From even a "modest" developmental perspective, it is a long course in development to mathematical abilities and racial preferences. In this respect, it is not at all clear what we gain with the conceptualization of core capacities.

What core knowledge theorists aim to characterize is that "something" that allows us to develop our species-typical capacities. Core knowledge researchers, and Evolutionary Psychologists, posit essentially innate knowledge structures that pre-exist as adaptations. However, as I suggested earlier, the key to understanding our cognitive abilities lies in understanding our adaptability. A conception of initially encapsulated particulate core capacities that come “online” and interact seems incommensurate with the notion of adaptability. Whether or not core theorists can adequately address adaptability is an open question. However, its usefulness as it stands now is limited. Core knowledge theory is more of the same of Evolutionary Psychology. Despite its claims of being more “developmental,” it shares the same inadequate conceptions of development as EP. To further elaborate on how many of these claims seem to be “old” adaptationist claims, consider the following statement by core knowledge researchers:

The presence of social evaluation so early in infancy suggests that assessing individuals by the nature of their interactions with others is central to processing the social world, both evolutionarily and developmentally. The capacity for such evaluations can be seen as a biological adaptation: cooperative behaviour such as group hunting, food sharing and warfare can be beneficial to individual members of a group, but can only successfully evolve if individuals can distinguish free riders from cooperators or ‘reciprocators’, those willing to do their fair share. (Hamlin, Wynn, & Bloom, 2007, p. 558)

Such statements are indicative of a strong adaptationist stance, and had I not indicated this statement came from core knowledge theorists, one would be forgiven for misattributing it to Evolutionary Psychologists. Not only is the reasoning identical in that social evaluative skills are conceived of as a specific adaptation unchanged since it was
acquired by the species, but cheater detection and the “problem” of altruism are a central topic in Evolutionary Psychology (e.g., Cosmides & Tooby, 1989, 1992, 2005, 2013). Although the claims of core knowledge theory are interesting, because of these problems I leave core knowledge theory to move on to examine what I believe are more promising developmental approaches in cognitive and social developmental research.

Other researchers embrace the complexity of evolutionary explanation (see Tomasello, 2008) and go about it by avoiding some, but not all, of the pitfalls. As developmental psychologists, many researchers are aware of the erroneous notion of the deterministic view of development (e.g., Tomasello & Call, 2008). However, it is very difficult for psychologists to avoid the problems with a non-developmental evolutionary metatheory if it still exists as the evolutionary metatheory. What is needed is for psychologists to seriously evaluate the evolutionary ideas they hold because even if they avoid making what is typically taken to be evolutionary claims, their evolutionary assumptions influence their developmental claims. As noted in the previous section, although their relationship is complex, evolution and development are still intrinsically linked; they are not two separate processes as intimated by the structure of the Modern Synthesis.

In this brief and selective review of the developmental research into infant social understanding, I will demonstrate that the claims that are made in this area are adaptationist in nature and are of the classical Modern Synthetic “old” or “strong” variety. In this respect, many features drawn from the discussion of Evolutionary Psychology will be used as points of comparison for evaluating the nature of these developmental claims. These comparison points include whether or not the claims 1) are strong adaptationist claims (i.e., are not unlike the explanations of EP, for example), 2) entail dichotomous views of development (i.e., posit “two kinds” of development), and relatedly, 3) invoke evolutionary factors as causal factors in development (i.e., invoke innate or genetic mechanisms), and finally, 4) whether or not researchers reverse engineer developmental outcomes (i.e., speculate on the nature of development in lieu of adequate, real-time developmental analysis). I begin with a review of theory of mind research.
2.1. “Theory of Mind”

“Theory of mind” is a term that, for better or for worse, has become a “catch-all” term for denoting research into social understanding. Because the scope of social cognitive developmental research is so broad, I restrict my review here to research concerning the development of the earliest understanding of minds and communicative development, which includes research into primary intersubjectivity and early “theory of mind.”

Social understanding develops very early in infancy. Some of the claims regarding the earliest emergence of understanding of minds come from research regarding primary intersubjectivity. Much of this research invokes the concept of innateness as an explanation of these capacities. Trevarthen argues for a “theory of innate intersubjectivity—that the infant is born with awareness specifically receptive to subjective states in other persons” (e.g., Trevarthen & Aitken, 2001, p. 4). Leslie (1991) argues that the development of a theory of mind depends on a “rich and specific innate endowment” (p. 63). Baron-Cohen (1995) states that “mindreading has an innate, biological, modular basis” (p. 12) and Carruthers (1996) states, “at least the core of this folk-psychological theory is given innately, rather than acquired through a process of theorising, or learning of any sort” (pp. 22–23). Baron-Cohen and Swettenham (1996) state, “genetic mechanisms normally enable neural structures or processes for mind-reading” (p. 159) and that “development is partly, if not completely, driven by individual, biological factors within the child” (p. 161). Meltzoff and Gopnik (1993) present a sort of “starting-state nativism” (Gopnik, 1996) as they contend that “children are innately equipped with certain kinds of information about the nature of persons” and “they innately apprehend other human beings as ‘like me’ in fundamental ways” (p. 359). Further, “these mechanisms provide an important beginning point for constructing notions of minds and persons” (p. 360). Baillargeon, Scott, and He (2010) state “like several other researchers, we assume that infants are born with a psychological reasoning system that provides them with a skeletal causal framework for interpreting the actions of others” (p. 111).

which creates a “domain-specific processing stream adapted for understanding the behaviour of agents” (Leslie, 1994, p. 213). In effect, the development of a theory of mind is based on a genetically-based cognitive mechanism that becomes functional early in life. Modular accounts have been given for the development of autism. Autism may result from the “lack of (or damage to) the innate initial basis of normal development” (Carruthers & Smith, 1996, p. 6). Leslie (1991) considers autism to be the result of “biological damage to and subsequent cognitive dysfunction of the ToM module” (p. 64). Not only are these modular factors innate, but human bodies are possibly specifically adapted to share attention. Butterworth (1991) states “the specialized function of the index finger in relation to shared attention may be innate” (Butterworth, 1991, pp. 229–230).

The capacity for imitation in newborns stands as one of the most persuasive demonstrations of an innate foundation to intersubjectivity. Trevarthen (1998) states that neonatal imitation provides evidence of “effective interpersonal intelligence” (p. 15) in newborns, and provides the basis of his theory of innate intersubjectivity. Infants are born with “motives to find and use the motives of other persons in ‘conversational’ negotiation of purposes, emotions, experiences and meaning” (Trevarthen, 1998, p. 16). To explain how the capacity for imitation develops so early, even as soon as 42 minutes after birth (Meltzoff & Moore, 1998), Meltzoff (1985, 1990) proposes a mechanism called cross-modal matching that allows an infant to experience a sense of similarity between his own actions and that of a model. This sense of similarity is deemed to be the foundation on which the child develops a “theory of mind.” Further, infants are said to be capable of early forms of representation and that “early imitation is an intentional activity” (Meltzoff & Moore, 1998, p. 57).

Premack and Woodruff (1978) were the first to pose the question as to whether or not theory of mind exists in other animal species (i.e., whether an individual “imputes mental states to himself and others” p. 515). This question has led to much research and debate as to whether or not, and to what extent, other animals attribute mental states to others. Many researchers argue that some non-human animals understand some mental states (i.e., Premack, 2007; Seed & Tomasello, 2010; Tomasello & Herrmann, 2010) while others argue otherwise (e.g., Penn & Povinelli, 2007; Penn, Holyoak, & Povinelli, 2008). The existence of theory of mind is consistent with neo-Darwinian theory as its
presence in other animals (primates in particular) would provide evidence for the gradual selection of changes that have given rise to the complexity of human cognition (van der Vaart, 2013). However, many researchers argue that humans are unique in their social understanding capacities (i.e., Penn et al., 2008), and, as such, theory of mind is thought to be a solely human adaptation for inferring mental states from observable behaviour (Barrett, Henzi, & Rendall, 2007).

2.1.1. Theory of Mind and the Adaptationist Stance

It is difficult to characterize the perspectives of all “theory of mind” researchers as a whole, but it is clear from the above review that many programs of research that involve the investigation of the earliest indications of social understanding often invoke “innate” factors that provide the foundation for social understanding. Many of these researchers focus on the development of social cognitive skills in infancy and make fewer explicit claims about adaptations per se, but their “starting-state” developmental conceptions are adaptationist in nature. That is, theory of mind researchers study development as a process that occurs after certain necessary structures emerge. These structures are “innately” specified and set up the conditions for the potential for subsequent development. The development of these prerequisite conditions, however, is not conceived of as emerging in a similar way. There is little recognition that the structures they take for granted are themselves products of developmental processes, or if this is recognized, they are thought to be the products of developmental processes dictated by “genetic” factors. Similar to the adaptationist EP approach, this way of viewing these issues sets up the situation for “two kinds” of development, one that precedes “external” influence and a subsequent one that includes it. So although many of these researchers avoid speculation about the evolution of these mechanisms, the existence of these mechanisms is taken for granted in an adaptationist manner. Further,

55 Making a distinction between “observable” and “mental” behaviour is not unproblematic. These issues will be alluded to later in this dissertation to the extent that the distinction is often invoked in the “rich/lean” debate in social cognitive research, but for further discussion on this issue see (Racine & Carpendale, 2007; Racine, Leavens, Susswein, & Wereha, 2008; Susswein & Racine, 2008).

56 Any “environmental” influence in the former is relegated to a “trigger” that turns on a developmental program rather than it being viewed as an ever-present co-factor in development.
the view that development can only occur from a pre-existing starting point works well for adaptationists, because these structures are thought to be adaptations themselves. How else would development get off the ground? Their view is only “developmental” after this “starting state” is established; there does not seem to be an appreciation that these conditions are themselves products of developmental processes. It is troubling that so-called developmentalists are taking this “starting state” for granted. This “starting state” is a set of reliably developing cognitive mechanisms, i.e., adaptations, which are not empirically validated, but whose existence, rather, is posited and characterized by evolutionary metatheoretical assumptions.

In evaluating the claims of theory of mind researchers with respect to the four criteria laid out above, it seems that many researchers adhere to many tenets of 1) a “strong” adaptationist approach. Many 2) hold dichotomous views of development that only become “developmental” after 3) the evolutionarily derived “innate” or “genetic” factors, possessed exclusively by humans, come online. There are also many instances of 4) reverse-engineering developmental outcomes, though these are more characteristic of modularity claims. Developmental analysis seems to be applied to a limited extent, i.e., after an innately given structure has been established. Moreover, these researchers are undoubtedly ardent interactivists, but as previously shown, the typical conception of interaction does nothing to problematize the list of assumptions, and, in fact, informs and supports them.

Of course not all social understanding researchers routinely make evolutionary claims often because it is simply beyond the scope of the research project at hand. However, it is clear in many researchers’ conceptions of development, especially those who couch explanations in terms of innate factors, a preformationist conception of development plays out. By virtue of focusing on developmental factors, there are more examples of developmental analysis, but its application is only limited to after the innately given structures emerge. Further, the nature of later developing abilities may be

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57 The use of “innate” can perhaps be reasonable if used in the sense that no one researcher is expected to explicate the entirety of the developmental history of all the structures they are studying at a single time point in an organism’s development. However, assuming the development of these structures occurred in a way that “innate” is usually used is not unproblematic and leads to the persistence of inappropriate dichotomous views of development.
misconstrued due to the supposed relation of these capacities to the innately given structures. For example, what is the advantage of conceptualizing autism as a breakdown of a “theory of mind module,” that is itself reliant upon other functioning modules? Such an explanation is overly simplistic and misconceived. These issues, as indicated in the last section, could be addressed with the adoption of a developmental metatheory that problematizes conceptions of “innate,” dichotomous views of development, and necessitates developmental analysis of what are thought to be “innately-specified” cognitive structures.

2.2. Joint Attention

Infant joint attention is another intersubjective capacity that emerges early in infancy and is considered to be foundational to the development and evolution of psychological capacities unique to humans. Joint attention has been subject to a considerable amount of interest and research (e.g., Adamson & Bakeman, 1984; Bakeman & Adamson, 1984; Behne, Liszkowski, Carpenter, & Tomasello, 2012; Carpenter et al., 1998; Leung & Rheingold, 1981; Liszkowski, 2011a, b, 2012; Liszkowski, Carpenter, Henning, Striano, & Tomasello, 2004; Liszkowski & Tomasello, 2011; Matthews, Behne, Lieven, & Tomasello, 2012; Moore & Corkum, 1994; Moore & Dunham, 1995; Mundy, Block, Vaughan Van Hecke, Delgado, Parlade, & Pomares, 2007; Mundy & Gomes, 1998; Racine, 2012; Racine & Carpendale, 2007; Tomasello, 2008, 2009; Tomasello et al., 2005, 2007). This interest has been, in part, fueled by the link between joint attention and language development (Baldwin, 1993, 1995; Bates et al., 1975; Bickerton, 2005; Butterworth & Morissette, 1996; Colonnesi et al., 2010; Morales et al., 1998; Mundy & Gomes, 1998; Tomasello et al., 2005 but see Slaughter & McConnell, 2003), its link to other social cognitive capacities (Bates et al., 1975; Bruner, 1983; Carpenter et al., 1998; Colonnesi, Rieffe, Koops, & Perucchini, 2008; Trevarthen, 1979), and it being an early indicator of a mentalistic understanding of others (Racine, 2012; Tomasello et al., 2007). Certain joint attention skills have been shown to be shared with other primates (Call & Tomasello, 2008; Tomasello et al., 2003), while others such as declarative pointing to share attention or provide information for another, are argued to be human-specific adaptation that marks a dividing line between humans and other great apes (Tomasello et al., 2005, 2007; Tomasello & Herrmann, 2010). Joint
attention skills are also studied for their communicative functions. Infants can communicate with gestures long before they can communicate linguistically, and in fact, these skills provide the performative basis on which language is based (Bates et al., 1975). Thus, the ability to produce pointing gestures and the ability to comprehend pointing gestures have garnered considerable attention.

Joint attention receives so much attention because of its importance in both the phylogeny and ontogeny of human cognitive capacities. It is considered to be a capacity that is foundational to particular forms of human cognition. It is thought to represent a level of social understanding, often termed a “mentalistic” understanding, of other agents that is unique to our species. Joint attention skills have been grouped into categories: 1) responding to joint attention (RJA) includes following the gaze and points of another, 2) initiating joint attention (IJA) includes pointing, showing and gaze alternation, 3) initiating behavioural requests (IBR) refers to the use of eye contact, reaching, giving and pointing to obtain an object via another person and 4) responding to a behavioural request (RBR) refers to the infant’s ability to respond to the requests of an adult (Mundy et al., 2003).

These skills are popularly thought to represent two different levels of social understanding. IBR and RBR are associated with a less complex “behavioural” understanding, whereas IJA and RJA are associated with a more complex “mentalistic” understanding. These interpretations of joint attention, that these capacities indicate a “mentalistic” or a “behavioural” understanding, are the two sides taken in the “rich/lean” debate. This debate describes the impasse between early social understanding researchers over what these capacities signify about the nature of the infant’s understanding of her social world. “Lean” interpretations consider a particular instance of joint attention as presenting a less-complex “behavioural” understanding, while “rich” interpretations attribute adult-like (mental-state) social understanding to the very same instances. The debate is a bit of a misnomer because both sides obviously recognize that typically developing infants eventually develop complex mental state understanding characteristic of adult humans. However, the debate regards how to characterize the nature of the earliest appearances of joint attention in infancy. How these instances are interpreted has potentially important implications for how joint attention is
conceptualized, how it is considered to be related to other capacities, how it develops, what is considered to be its role in evolution, and how we explain its evolutionary history.

To address these issues, developmental psychologists have focused on certain joint attention skills, for example, infant point production and point comprehension, to examine what these skills signify about infants’ understanding of other minds. Social cognitive development researchers agree upon the importance of joint attention skills. However, there is a great deal of debate surrounding some of the basic questions regarding their nature and development. There are questions regarding the nature of the processes underlying joint attention skills. For example, Tomasello and colleagues (Carpenter et al., 1998; Tomasello et al., 2005, 2007) argue that the various joint attention skills are a function of a common factor, a complex psychological understanding of others as intentional agents. Other researchers argue that different types of joint attention skills are functions of partially separate processes (Bibok, 2011; Mundy & Gomes, 1998). There is also debate about their emergence and ontogenetic relationship between joint attention skills. Some researchers have found that pointing comprehension precedes production (e.g., Bruner, 1983; Carpenter et al., 1998; Leung & Rheingold, 1981), while others report that point production precedes comprehension (Desrochers, Morissette, & Ricard, 1995; Murphy & Messer, 1977).

Although indeed there still seem to be many empirical questions to investigate regarding joint attention, the general stance in the field is arguably the one taken by Michael Tomasello and his colleagues. Their work is certainly the most influential in joint attention research, and Tomasello is among the very few joint attention researchers who explicitly addresses the evolution of these capacities. Tomasello (1999) claims that in order to understand human cognition we must search for “some small difference that made a big difference—some adaptation, or small set of adaptations, that changed the process of primate cognitive evolution in fundamental ways” (p. 510). Tomasello now refers to this small difference as shared intentionality, characterized as being a motivation that grounds human intersubjectivity; something that is absent in other apes (Tomasello 2008; 2009; 2014; Tomasello & Carpenter, 2007; Tomasello et al., 2005, 2007). This is a motivation to share attention with others, and although Tomasello and colleagues may grant that under certain circumstances apes in captivity come to point to
direct attention, they claim that apes do not point under ordinary conditions in the wild (but, see Leavens & Racine, 2009).

Tomasello and colleagues argue that humans and other great apes share many capacities for understanding others. However, substantial differences exist. They argue that other apes do not have a “theory of mind” as humans do. Other apes are thought to understand others in terms of a perception-object psychology rather than a human-like belief-desire psychology (Call & Tomasello, 2008). This difference roughly translates to the two forms of social understanding noted above; that is, non-human primates demonstrate a lower level “behavioural” understanding of others as opposed to a high level “mental” understanding characteristic of humans. The difference is that humans have a species-specific adaptation called shared intentionality. This adaptation (to be more precise, this “suite of adaptations” Tomasello & Carpenter, 2007 p.121) is what converts the basic behavioural understanding shared by all great apes into the mentalistic understanding that only humans exhibit. Such a stance puts the possibility of distributed forms of cognition out of reach of other primates (see Call & Tomasello, 1997, but see Barrett et al., 2007 for an alternative view).

2.2.1. **Joint Attention and the Adaptationist Stance**

Because Tomasello’s is the most influential program in joint attention research, and because he presents a developmental and an evolutionary model of joint attention, I single out his theories and research program for evaluation in the following. Although Tomasello and colleagues are critical of nativist explanations and the inadequacy of simply citing the role of an adaptation without explaining the ontogenetic process (e.g., Tomasello et al., 2005, p. 688), this group simultaneously makes claims consistent with a nativist approach. Their notion of shared intentionality is a strong adaptationist conception. This is readily apparent in one of their most interesting yet largely unnoticed statements. Tomasello and colleagues claim that the nature of shared intentionality, this uniquely human motivation (i.e., adaptation) to share attention, could be revealed through the employment of isolation experiments (Tomasello et al., 2005, p. 690, see Wereha & Racine, 2012 for further analysis of this claim). Isolation experiments have long fallen out of favour in developmental research because they are premised on the erroneous notion that one can separate and individually study the “genetic” and
“environmental” factors in development. As previously stated, this notion is founded on the fallacious developmental dichotomy between “nature” and “nurture” and is based on a mischaracterization of the role of genes in development (Lehrman, 1956, 1970). Further, this species-specific adaptation/motivation proposed by Tomasello and colleagues is thought to be something that comes prior to or is somehow encapsulated from developmental processes because they state “We assume that these motives are not created by socialization processes” (Matthews et al., 2012, p. 818). Such statements clearly represent a strong adaptationist position and a deterministic conception of development. However, in their theoretical work, Tomasello and colleagues tend to discount criticism that their approach does not seriously consider life history and rearing conditions, and protest the many claims that their approach is inherently deterministic (Boesch, 2007, 2008; de Waal, Boesch, Horner, & Whiten, 2008; Leavens & Racine, 2009; Racine et al., 2008; Wereha & Racine, 2012).

Tomasello’s stance is characterized as deterministic because of a strong adaptationist perspective that leads to his paying insufficient attention to life history factors, which is especially problematic in his claims about the differences between humans and other great apes. Tomasello and colleagues consider their perspective to be developmental because they agree that chimpanzees understand psychological states but think the question is then “which ones and to what extent” (Tomasello et al., 2003, p. 153). To them, this is a developmental perspective because they do not deny that chimpanzees develop mental state understanding. Tomasello and colleagues simply argue that chimpanzees’ mental state understanding is not as complex as that of humans. However this line of questioning and their developmental perspective does not go far enough because these questions are not contextualized in terms of how mental state understanding develops. For Tomasello, at an evolutionary level, shared intentionality is an encapsulated mechanism that is not influenced by developmental factors. As such, his stance risks reifying or potentially exaggerating perceived differences between primate species and minimizing the need for developmental analysis. These issues lead to a form of determinism in Tomasello’s view, leading him to study, for instance, pointing in infants at 12 months of age and speculate upon the development of these capacities without actually looking at how these capacities develop. Longitudinal research, rather than cross-sectional research occurring long after
the developmental milestone in question has been reached, is more appropriate for questions about the origins of these capacities. Thus posing the question as “which ones, to what extent, and how this understanding comes to be” represents a more complete approach that can allow us to properly conceptualize and compare the cognitive abilities of humans and our great ape cousins (Wereha & Racine, 2012).

For Tomasello and colleagues, the difference between humans and other great apes comes down to humans having a critical adaptation that other apes do not have. From such a perspective, it is not unexpected that the details of the developmental history of individuals become a secondary concern. This reasoning is the same as I have shown with the EP, core knowledge, and early theory of mind perspectives:

In terms of ontogeny, Tomasello et al. (2005a) hypothesized that the basic skills and motivations for shared intentionality typically emerge at around the first birthday from the interaction of two developmental trajectories, each representing an evolutionary adaptation from some different point in time. The first trajectory is a general primate (or perhaps great ape) line of development for understanding intentional action and perception, which evolved in the context of primates’ crucially important competitive interactions with one another over food, mates, and other resources (Machiavellian intelligence; Byrne & Whiten, 1988). The second trajectory is a uniquely human line of development for sharing psychological states with others, which seems to be present in nascent form from very early in human ontogeny as infants share emotional states with others in turn-taking sequences (Trevarthen, 1979). (Carpenter & Tomasello, 2007, p. 124)

Here development is explained with reference to a critical adaptation. Further the authors cite Trevarthen, whose work I discussed in the previous section. It is taken for granted by Trevarthen and Tomasello that humans are endowed with uniquely human capacities that appear early in development. Moreover, for Tomasello, the interesting part of infant social cognitive development occurs when the shared ape and uniquely human lines of development interact at 12 months of age, further downplaying the importance of experiential factors prior to this age. Trevarthen and Tomasello are not wrong to state that human capacities appear very early in ontogeny, but a “starting-state” developmental account that is bolstered by suspect assumptions about the relation
between evolution and development is problematic, especially when it leads to ignoring the important developmental processes that lead to our human-specific capacities. Instead, these human-specific capacities and their development are simply taken for granted. It is surprising that developmental psychologists pay insufficient attention to the development of the very things they are supposed to be studying.

It is clear that Tomasello and colleagues’ conception of shared intentionality is largely 1) a strong adaptationist conception in that it adheres to 2) dichotomous views of development in which they invoke the “sound” logic of isolation experiments and view the development of motivations as “something separate from socialization.” Hence, the 3) evolutionary story explains much in their account. Shared intentionality is an adaptation unique to humans, thus it is innate in so far as it is thought to be a “biological” mechanism not affected by “social” influences, and thus transforms capacities shared by primates into uniquely human capacities. These researchers, then, 4) apply developmental analysis to some extent as they compare psychological capacities across species but fail to sufficiently examine the development of these capacities within species. And, as I will demonstrate shortly, they pay little empirical attention to how joint attention skills such as pointing develop before the age of 12 months.

2.3. Summary

Table 1 summarizes the key developmental and evolutionary assumptions of influential and dominant positions in theory of mind and joint attention research. Included in the table is Evolutionary Psychology as a point of comparison representing an extreme adaptationist account. It is useful to keep in mind the distinction between preformationist and probabilistic assumptions (Gottlieb, 1992/2002). Although misinterpreting neo-Darwinian metatheory often seems to support preformationist developmental assumptions, there is variability in the claims made depending on the subject matter, particularly in areas that focus on the earliest emerging psychological capacities. As infants grow, their already established foundational capacities are transformed or set the stage for the development of more complex capacities. Such subject matter lends itself to more “developmental” approaches and explanations.
Table 1

*Comparison of Evolutionary and Developmental Claims in Developmental Psychology*

<table>
<thead>
<tr>
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<th>EP</th>
<th>Early ToM</th>
<th>JA (Toma.)</th>
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<tbody>
<tr>
<td>Strong adaptationist</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Dichotomous views of development</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Causal evolutionary factors</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Developmental analysis</td>
<td>No</td>
<td>Some</td>
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</table>

*Note.* EP = Evolutionary Psychology; Early ToM = Early theory of mind research; JA (Toma.) = Joint attention, as per the approach of Tomasello and colleagues.

Therefore there seems to be a mix of preformationist and probabilistic assumptions in early theory of mind and Tomasello's joint attention work, where the preformationist assumptions are provided by the evolutionary metatheory, while the probabilistic assumptions are dependent upon the particular capacity under consideration.\(^{58}\) In this respect the evolutionary metatheory is inherently limiting, minimizing the ability of researchers to fully appreciate the implications of their knowledge of developmental processes and how these insights could be used to construct research programs that are more appropriate for studying the development and evolution of psychological capacities. The evolutionary explanations provided are overly simplistic and naïve compared to what is deemed to be required in a pluralistic evolutionary explanation, as described in section 1.5.

Rather than waiting for an explicitly developmental evolutionary metatheory to arrive that will replace the Modern Synthesis, developmental psychologists can begin by taking a more “developmental” approach. In this regard I agree with Boesch (2007, 58 It should be noted that Tomasello offers a simulation view where joint attention skills stem from an understanding of others as mental beings like the self. However, his non-developmental metatheoretical assumptions likely plays a role in the lack of empirical attention he pays to development of joint attention prior to 12 months of age, making it doubtful how “developmental” his simulation account really is.
2008) when he describes two theoretical approaches that characterize comparative research investigating the development and evolution of psychological capacities. Boesch argues that there exists a developmental approach that stresses the importance of environmental and social factors and a deterministic approach that minimizes the importance of these factors. These two perspectives can be viewed as representing developmental and non-developmental evolutionary metatheories, respectively (Wereha & Racine, 2012).

What I refer to as a developmental approach is a perspective that emphasizes developmental processes; an approach already taken by numerous developmental researchers, such as the proponents of developmental systems perspectives (i.e., Griffiths & Gray, 1994; Gottlieb, 2007; Lickliter & Honeycutt, 2003, 2013; Oyama et al., 2001). Researchers must focus on the organism in its natural environment and emphasize description to properly contextualize the organism’s capacities. One cannot properly conceptualize psychological capacities without grounding them in the context of the developmental life history of the organism itself. This approach is needed, in part, to provide the evolutionary, developmental, and ecological context (the “developmental” context for short) that is typically lacking in developmental and comparative studies of psychological capacities.

So far in this dissertation I have attempted to show that issues at the metatheoretical level have implications for developmental psychologists at the theoretical and empirical level. If developmental psychologists find it difficult to believe such “far removed” metatheoretical issues impact the way they do research, it is likely much more of a stretch for them to believe that the metatheoretical assumptions involving evolution bear so heavily on their conduct in the lab and the way they explain the developmental phenomena they study. However, I have argued that despite a tacit acceptance of evolution being important for understanding psychological phenomena, the ways in

59 Not only does ignoring context lead to problems, but so does invoking conceptions that may inappropriately colour our understanding of what we are trying to study. Provine (2005) argues that intention, as conceptualized by Tomasello et al. 2005, may be unnecessary in explaining social understanding and its inclusion may be mischaracterizing the nature of human cognition. Provine argues for a more conservative interpretation of explaining human and animal actions.
which it is important are seldom elucidated. There are indeed psychologists who take evolution seriously and attempt to explain explicitly the evolution of psychological capacities. However, as I have shown, most take a strong adaptationist approach, which is untenable and has been all but rejected in other biological sciences. Many developmental psychologists maintain simplistic and inaccurate views of development that can be traced back to mistakes in interpreting the Modern Synthesis as modeling development when it does not actually do so. These mistakes are now built into the infrastructure of psychological research and are “informally formalized” most clearly in the assumptions of the “interactivist consensus,” the persistence of dichotomous views of development, and innateness misused as an explanatory concept. Although these problems are all certainly related, it is the misuse of “innateness” that epitomizes what I find to be the most curious mistakes made in developmental research.

When innateness is invoked as explanation, it leads to “passing the buck”: an assumption that the phenomena at the level of study that developmental psychologists are concerned with can only be explained at a “lower” level of study beyond the purview of these psychologists themselves. These “innate structures” are taken for granted and left for some “other” biological researchers to explain. The problem is there is no “lower” level of biological study in the way these psychologists assume. This belief is based upon erroneous assumptions these researchers hold about the nature of development that stem from misunderstandings derived from the metatheoretical level about the relation between evolution and development, and in particular, the relation between genotype and phenotype.

2.4. An Illustrative Example: Matthews, Behne, Lieven, & Tomasello (2012)

In the following I focus on a single article in order to pull together the issues I have raised above. The reader who is not yet convinced that metatheoretical issues are relevant for research may be persuaded when I show how a single article can be rife with the problems I have discussed thus far. In the following I briefly outline the strong adaptationist assumptions these researchers make and some of the attendant conceptual, theoretical and empirical problems that stem from them.
Matthews et al. (2012) present a training study in which infants are explicitly exposed to pointing (so-called “training”) in the hopes of determining whether the origins of pointing stem from “spontaneous outset” in which pointing emerges largely out of individual factors or a “socialization view” where pointing emerges largely out of social factors. In this study, 9, 10, 11 and 12 month old infants were each exposed for one month to an extra 15 min a day of pointing gestures by their mothers (training), and were tested for their declarative pointing and gaze following skills three times during their month of training. Infants’ pointing ability was found to be related to their ability to follow gaze, but not to the training they received. Infants’ pointing frequency was also related to their ability to follow gaze and to maternal pointing in free play, not in training. However, the researchers found that infants’ ability to gaze follow and frequency of gaze following was influenced by training and maternal pointing in free play. The authors interpret these findings as meaning that the onset of joint attention skills is largely determined by prior social cognitive advances. This is in apparent contrast to a “socialization” view where pointing is thought to be largely due to the influence of learning from and imitationing adults. The authors argue, however, that socialization, of course, does play a role by later influencing gaze following and point frequency, but does not account for the origins of joint attention capacities.

Matthews et al. (2012) lament that although we “know a good bit” (p. 817) about joint attention in infants of 12 months of age, we know little of the origins of the pointing gesture. They maintain that joint attention rests on separate capacities that are necessary for joint attention to obtain. These capacities include:

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60 Alarm bells should be ringing when “developmentalists” are describing outcomes of processes as the interaction between two independent developmental factors. Such an approach may be useful for carefully proscribed situations with defined parameters, but not as a general approach to understanding developmental phenomena. And here we see again the “nature/nurture” distinction alive and well despite claims that the issue is a dead one. It will not be overcome until, I argue, the evolutionary metatheoretical issues are addressed. Such partitioning is exactly the same kind that Tomasello et al. (2005) espoused when they argued that isolation experiments are the ideal way to understand how something develops. Although they argued isolation experiments are impossible to do due to ethical reasons concerning isolating subjects, I argue its most egregious ethical violation is its rampant logical incomprehensibility.
(i) motoric prerequisites for arm extension and index finger extension toward external objects; (ii) motivational prerequisites for communicating with others in various ways (e.g., requesting things imperatively, or indicating them declaratively); and (iii) social-cognitive prerequisites for following, directing, and sharing attention with others. (p. 817)

These prerequisites on the surface seem reasonable, but they are not as unproblematic as this research group may wish them to be. These prerequisites are the premises upon which Matthews et al. base their theoretical explanation of joint attention. And as such, their theoretical explanation is only as valid as the premises on which it is based. Motor capacities are a logical requirement of the ability to point. The other two prerequisites, however, are trickier. For instance, in (ii) we once again see the notion of motivation being invoked. Given that it is taken to be a prerequisite, it is thought to exist prior to, and is necessary for, the development of joint attentional abilities. And as shown earlier, this motivation is thought to be an evolved adaptation. It is in this evolutionary context that "motivation" becomes explanatory: its presence is explained as a naturally selected (particulate) adaptation, it plays an important role in development, and its adaptive advantage is never explicitly stated but taken as a given (i.e., in the sense that the superiority of human cognition is self-evident, and from here, we are not far from problematic "evolved for" nomenclature). It is only in this evolutionary context that invoking "motivation" makes any sense because it is the neo-Darwinian adaptation that is doing the heavy lifting. The causal sense of motivation is one that is often invoked in psychology (Danziger, 1997) and it is in this sense that Tomasello and colleagues use the term. However, by doing so, Tomasello and colleagues are only thinly veiling a strong-adaptationist stance. Further, by taking these "motivations" for granted, explicating their nature is pushed down a level of analysis outside the purview of these

Matthews et al., unsurprisingly, take a dichotomous view of development here in the way they conceive of the role of these motivations/adaptations: "We assume that these motives are not created by socialization processes, but it is possible that they are integrated with communicative pointing by one or another process of social learning. That is, infants may learn that their goal to share attention can be realized through pointing." Once again, these motivations pre-exist as developmentally encapsulated adaptations that after a certain point are influenced by other factors. This notion is completely untenable in light of what is known about development yet is held by strong-adaptationist "developmentalists." Such problems should not be unexpected since these authors explicitly adopt the conception of motivations and the attendant developmental assumptions espoused in Tomasello et al. (2007).
researchers, with a promissory note written that some imaginary biologists working at a fictitious level of analysis will validate their conceptions of structures that do not exist. If one fails to adopt this stance, as I do, then “motivation” simply reverts to being a mere (re)description of behaviour. To say that an organism is “motivated” to do x, mainly means, at minimum that the organism is doing x, and at most means that the organism is putting a fair amount of effort into doing x, but what is clear is that redescribing behaviour is not an explanation of behaviour. Perhaps one could argue that an organism may be motivated for certain reasons, and thus one could invoke these reasons as an explanation for the behaviour. Perhaps yes, but this is not a causal relationship as Tomasello and colleagues mean when they state that motivation is an adaptation. In this case, the adaptation is causally connected to the behaviour in a way reasons are arguably not.

A related conceptual problem has to do with some grammatical quirks. Consider the difference between the statements “an organism is motivated” to “an organism has a motivation.” From a description of behaviour, the concept of motivation takes on a more tangible quality in that it is something that exists and is “possessed” by the organism that “has a motivation.” In most cases, however, this is merely a play of the structure of grammar (see Bibok, 2011). These issues are not unrelated to Matthews et al.’s (2012, p. 817) third premise either. This premise states that social-cognitive prerequisites are needed for following, directing, and sharing attention. This is the issue I will discuss at some length later in this dissertation; that developmental psychologists have been more interested in the “underlying capacities” of joint attention than how joint attention develops in and of itself. This again is largely a problem of redescription masquerading as explanation. To say that a human infant “has the cognitive capacities” and the “pro-social motivations” to point is just reiterating that an infant can point. The details of how this comes about are glossed over because pointing is thought to be a function of social-cognitive understanding, thus to understand pointing, one must “get in the head” of the infant to see what she understands of the mental world. However, rather than limiting our investigations of the “social-cognitive prerequisites for following, directing, and sharing attention with others” (Matthews et al., 2012, p. 817) (whatever those might possibly be), we can make more progress in keeping our focus on the actual development of the skills of following, directing and sharing attention. Because, if giving explanations in terms of
“motivations” and “social-cognitive prerequisites” are merely redescriptions of behaviour, then we can actually see right before our eyes what infants understand about the mental world by the development of their non-verbal communicative skills (see Carpendale et al., 2013). To repeat, what infants understand of the mental world is laid bare in how they interact with others. To describe these interactions is in part to speak of their mental lives. Infants’ communicative capacities are in and of themselves the very ground on which we attribute social understanding; thus, the capacities themselves constitute social understanding (see Racine, 2012). Of course, we should expect inconsistencies, incompletely formed capacities and “transitional” phases (Lock, 2001). There is no need to look any deeper to see, as it were, “what the infant understands of other minds.” Such a question takes away from understanding the actual development of social understanding.

These theoretical and conceptual issues have implications for Matthews et al.’s empirical program. These researchers differentiate between “Spontaneous Outset” and “Socialization” accounts, noting that the key difference between them is “the extent to which parent modeling of and responding to the pointing gesture is proposed to be necessary for infants to start pointing” (p. 819). If socialization is important in the origins of pointing, then increased pointing on the part of the parents should lead to earlier onset of pointing in the infant. If it is only important in that it elaborates upon a pointing gesture that emerged after spontaneous outset, then parental pointing could influence the frequency or quality of pointing rather than age of onset. However, it is not clear that these two positions are tenable. Even the authors state earlier that the issue is much more complex than this: that there is likely an interplay of “learned” and “spontaneous” (i.e., biological) components (nature and nurture62) (pp. 817-818). If this is the case, such recognition problematizes these two positions because they are overly simplistic caricatures. If the interdependence of these interacting factors is widely acknowledged, why do the authors revert to two positions that are in dire contrast to the systems notions discussed earlier? The authors, however, feel that they are justified in perpetuating the myth of these two positions because “there is no experimental work to pick apart this

62 Invoking these terms, again, should be sufficiently indicative of these researchers’ view of development.
interplay and establish precisely where and to what extent socialization affects this fundamental development in human communication" (p. 818). Unfortunately, due to the caricature of the "socialization" position, the authors equate socialization to a 15 minute training session a day in which infants are exposed to pointing by their parents. Equating socialization to 15 minutes extra a day of exposure to pointing is certainly not how socialization was conceptualized by Bates (Bates, 1979; Bates et al., 1975, 1976), whose work established psychology’s interest in joint attention and whose work I more fully describe in the following section. That is, 15 minutes of relatively uncontextualized exposure to pointing differs greatly from the routinized interactional structures upon which communicative gestures are grounded. Such rhetoric effectively makes the Socialization position a caricature. Because this "socialization" was ineffective, by default the Spontaneous Outset view wins. The strong adaptationist assumptions provide apparent grounds for the two separate positions, and ultimately, due to its circularity in how these assumptions set up the problem and presuppose a solution, apparent support for the Spontaneous Outset position is not unexpected.

2.5. Moving Forward in Joint Attention Research

I have critiqued the evolutionary and developmental claims of early social cognition researchers largely on the grounds of the inadequacy of the underlying metatheoretical framework neo-Darwinism provides. Although many psychologists claim to heed Williams’ (1966) caution regarding the difficulty of employing the concept adaptation, human-specific adaptations are often invoked with a minimum amount of rigour, as Leavens, Hopkins and Bard (2008) note:

In general, any strong claim that a species-specific cognitive or behavioural capacity (of which speech is the most salient example) evolved de novo in humans must therefore also claim (a) that the selective contexts in which these traits appeared are strictly limited to recent times (from slightly before the Miocene/Pliocene boundary to the present), (b) that the selective contexts predating the Miocene/Pliocene boundary are irrelevant to understanding both the evolution and the development of those traits, and therefore (c) the study of our nearest living relatives, the Asian and African great apes, will not produce data
relevant to understanding the development in humans of the traits in question. (pp.194-195)

Strong adaptationist assumptions lead researchers to see species differences as stemming from intrinsic factors, leading to a neglect of understanding the development (or lack of development) of cognitive capacities within other species, resulting in erroneous conclusions about the development and evolution of our psychological capacities. By holding problematic evolutionary assumptions and not sufficiently meeting the modest criteria noted above, most evolutionary explanations are “just-so” explanations despite the objections of EP researchers and researchers who claim their theories are “more” than just another just-so story (e.g., Tomasello, 2008). Because these explanations share the same evolutionary metatheoretical assumptions, and because strong adaptationist claims are seldom evaluated in respect to the criteria cited above, the deus ex machina nature of these claims continue to raise the ire of critics.

Thus far, my review of developmental research has been mostly critical. Now I will briefly highlight the efforts of development researchers who are aware of the problems with the developmental and evolutionary claims made in developmental psychology and go about doing research with a perspective that is informed by a developmental evolutionary metatheory. Other researchers I note are simply those who argue for good developmental approaches and methods which stem from an awareness of the nature of developmental phenomenon that is not coloured by non-developmental evolutionary assumptions. Their work, I argue, is fully compatible with a developmental evolutionary perspective. In the following I briefly survey the works of some of these researchers, citing their perspectives in their own words to demonstrate how psychologists, in lieu of a formal adoption of a developmental evolutionary metatheory in psychology, can still do good research that is consistent with one.

An approach that is consistent with the evolutionary developmental perspective I argue for in this dissertation is that of Bard and Leavens (2008). Bard and Leavens take the perspective of both comparative and developmental psychologists. As comparative psychologists, they gain an evolutionary perspective in their comparisons across species and across cultures. As developmental psychologists, they focus on the emergence of capacities within species during their lives, but especially during infancy. For them,
“Asking both comparative questions and developmental questions at the same time, allows us to get closer to answering questions about commonalities and distinctions in developmental processes across primate species” (p. 90). This perspective yields a much more conservative approach in invoking species-specific adaptations. Whereas others have cited that human-specific adaptations underlie the development of pointing, Leavens and colleagues’ work has done much to show that humans and apes share the same biological capacity for joint attention and that species differences in pointing abilities are due to species-typical experiential factors in ontogeny (Bard & Leavens, 2008, Leavens et al., 2008; Leavens & Racine, 2009, Racine et al., 2008, 2012). Bard and Leavens are careful to focus on the development of psychological capacities in both species before making any comparisons between them. This perspective is not unlike that of Boesch (2007, 2008), who characterizes the approach of Tomasello and colleagues in their comparative work as “deterministic” for failing to adequately examine the development or lack thereof of capacities in comparative species before making cross-species comparisons. Boesch (2007) argues that:

Any comparison between humans and chimpanzees requires an assessment of the breadth of variation in the emergence and presence of cognitive abilities within each species. Different environmental conditions or upbringing experiences affect the emergence and development of different cognitive abilities. (p. 235)

With inadequate evolutionary assumptions and a neglect of what is simply good scientific methodology, invoking species-specific adaptations when they do not exist, or insufficiently demonstrating that they do exist, are problems that stand in the way of an accurate depiction of the evolution and development of our psychological capacities.

It should also be noted that Leavens and colleagues’ developmental perspective is part of a systems approach that contrasts starkly with the adaptationist assumptions of researchers like Tomasello and colleagues. Leavens et al. (2008, p. 189) state:

In short, living systems are open systems at all levels of analysis. There are substantive implications for psychology, generally, and comparative psychology, in particular, of the multiscalar dependence of the systems we study, of which we
wish to briefly note, here, the widespread dualistic assumption that individual brains constitute the loci for computations, the products of which then cause overt behaviour. We note that the communicative phenomena we discuss, here (pointing and its accompaniments), are interactive, distributed phenomena that are usually not manifest except in particular social and physical contexts.

Such a view is incompatible with those of strong adaptationists who espouse the existence of particulate adaptations that exist, presumably, as cortical networks. The perspective of Leavens and colleagues problematizes the positivistic nature of psychological research that often inappropriately reduces complex social phenomena, inappropriately, to capacities or adaptations at an individual level. These are phenomena that should be studied from a broader context, not just at the level of the individual but also including the people around the individual, as well as the history of interaction between them. Why people do what they do can more often be explained by life history factors than what is assumed by those guided by cognitivist or adaptationist assumptions and should be a taken for granted tenet of developmental research.

Developmental phenomena are complex. Many researchers recognize this fact. A major problem with adaptationist explanation is that it does not adequately capture the complex nature of development and offers models that are oversimplified to the point of being untenable. Researchers who use dynamic systems approaches recognize this complexity and try to provide a more sufficient account of development:

In this approach, expressive and communicative actions are organized as a complex, cooperative system with other elements of the infant's physiology, behavior, and social environment. Development of behavior does not occur uniformly across these domains as the singular result of an increasingly elaborated set of structures or prescriptions, but by the emergent order resulting from asynchronous changes between the component actions. We assume developmental processes are multicausal, nonlinear, and complex. Although this complexity and nonlinearity confounds experimental analysis and makes simple

63 “The fact that the brain processes information is not an accidental side effect of some metabolic process: The brain was designed by natural selection to be a computer” (Cosmides & Tooby, 2013, p. 203).
interactive models untenable, in real systems, it is the source of both stability and change. (Fogel & Thelen, 1987, p. 747)

Such insights are slowly making their way into joint attention research. Chris Moore (2008) in a review of research into gaze following, states that for decades, research was driven by a desire to understand what gaze following represents about social understanding. This question, he argues, was misconceived. Gaze following does not follow from some “insight” into the minds of others, but is more likely, over its protracted developmental trajectory, reflective of various aspects of social cognitive development. Thus, C. Moore (2008, p. 69) suggests:

If these conclusions are correct, then we need to think of gaze following as a dynamic system: Different factors will influence infants’ tendency to follow gaze in different contexts and at different points in development. A complete understanding of its development will require a systematic manipulation in combination of the variety of variables we know to be important—including age, social cues and interactive context, target salience, and spatial layout—and in combination with other aspects of cognitive development. We need to examine both how adults encourage infants’ gaze following and how infants become able to monitor gaze in both explicitly interactive and more subtle noninteractive contexts. We also need more research on the extent to which gaze following may contribute to various aspects of social learning.

Systematic manipulation of these factors is not a standard approach in strong adaptationist developmental accounts, but from a systems perspective this is a necessary part of a full understanding of the development of joint attention. Some current research programs, so-called skill-based approaches (Bibok, 2011), are a step in this direction. The work of Peter Mundy and colleagues represents a skill-based research program in which different social-cognitive skills are seen as constituting joint attentional systems (see Mundy & Newell, 2007). Whereas strong adaptationist modular approaches explain autism, for example, as a lack of a “theory of mind” module, Mundy and colleagues’ attentional-systems approach explains autism in a much more conceptually sophisticated and biologically plausible way in that:
One advantage of this parallel and distributed-processing model of joint attention is that it directly connects theory on social pathology to a range of phenomena in autism associated with neural connectivity, constructivist and connectionist models of cognitive development, early intervention, activity-dependent gene expression and atypical ocular motor control. (Mundy, Sullivan, & Mastergeorge, 2009, p. 2)

What is needed in joint attention research is more developmental analysis and longitudinal research programs. Many joint attention researchers employ cross-sectional experimental methods that involve assessing the joint attention skills of infants who are already established pointers. These methods, obviously, have important and necessary uses in joint attention research. For instance, they are useful in establishing the existence of certain capacities and the parameters at which they function. However, these methods often lack the detailed longitudinal analysis required to shed light on the development of communicative gestures. Moreover, how infants use communicative gestures is a function of the particular interactional history of the infant and is simply not accessible through exclusive use of cross-sectional experimental methods.

Strong adaptationist assumptions have greatly influenced developmental research, and, in part, are the reason why the perspectives of the researchers noted above are not as widely shared as they should be. Development is complex, but adaptationist explanation offers simpler, intuitive answers. This is especially so when the ideas presented are seemingly sound in their consistency with their evolutionary assumptions. The confirmatory nature of this circular argument is in part why it is so difficult for researchers to see the problems. Moreover, the complicated and potentially tedious, detail-oriented nature of systems methodologies and the accompanying analyses do not attract many converts either. However, there is little choice as complex phenomena entail complex methods for their study. As Chris Moore (2008) notes, joint attention researchers need to ask more questions about development itself. In the next two sections of this dissertation I attempt to answer questions about the development of joint attention capacities.
3. The Development of Pointing

In this section, I present a more detailed examination of joint attention research to examine why, paradoxically, the development of pointing is ignored in the study of joint attention. In the following, I argue that the empirical question of how pointing actually develops has taken a backseat to theoretical debates regarding what pointing indicates about infants’ understanding of minds. I argue that this neglect is due, in part, to issues that stem from the metatheoretical level commitments that I have previously described. However, a broader diagnostic approach is needed to contextualize the relevant issues. In this respect, I will contrast the popular social cognitive approach, as best exemplified and expounded by Tomasello and colleagues, with the original approach of the founders of the study of joint attention to examine the differences between these approaches and how they relate to the debate about the origins of pointing. This analysis will set up the context for the studies presented here and in the next section of this dissertation that examine the production and comprehension of pointing, the two components of the ability to point (Behne et al., 2012; Colonnesei et al., 2010). I begin this analysis with describing the relevance of pointing for debates about infant social understanding.

3.1. Joint Attention and the “Rich/Lean” Debate

Pointing has received much attention in infant cognitive development research. It is thought to be one of the earliest indicators of mental state understanding. Although the importance of pointing is generally agreed upon (Carpendale & Lewis, 2004, 2006), it is the nature of infant pointing and its relation to infants’ social understanding that is hotly debated. It is this question that is considered to be the question in joint attention research as “The current theoretical debates about infant pointing and prelinguistic communication center, as so many other topics in infant cognitive development, on whether the most accurate interpretation is a cognitively rich or a cognitively lean one” (Tomasello et al., 2007, p. 705). This debate is often framed, as mentioned earlier, as
being a question of whether or not an infant has a "mentalistic" or a "behavioural" understanding of the person for whom a point is intended. A "mentalistic" understanding is characterized as being an "adult-like" social understanding of others' attentional states whereas knowledge of mental states is not required for a "behavioural" understanding. Tomasello and colleagues’ “rich” interpretation of pointing, arguably the dominant position in the debate, "posits that when young infants point for an adult they are in some sense trying to influence her intentional/mental states" (Tomasello et al., 2007, p. 706). Pointing, then, is taken to be reliant upon, and indicative of, a mentalistic understanding of others. This position is also known as the “common-sense view” (Tomasello, Kruger, & Ratner, 1993) as this mentalistic understanding is considered to be similar to what adult humans are taken to have, hence the assertion that infant pointing demonstrates “adult-like” understanding of others’ mental states.

With the focus on what pointing means in terms of an infant’s understanding of other people, researchers have noted that apparently little is actually known about pointing itself (Liszkowski & Tomasello, 2011; Lock, Young, Service, & Chandler, 1990; Matthews et al., 2012; Tomasello, 2008). Nevertheless, researchers have speculated upon the origin of pointing and are currently at odds over whether it is initially a non-social gesture that later becomes a social gesture or whether it is social from the very beginning. Bates et al. (1975), whose work is in large part responsible for spawning the interest in infant pointing, argued that pointing emerges in non-social uses prior to social uses. Similarly, Carpendale and Carpendale (2010) have recently argued for a neglected view in which pointing originates as a manifestation of an infants’ own attention that, over time, becomes a social gesture. Tomasello and colleagues (Matthews et al., 2012; Tomasello et al., 2007; Tomasello & Liszkowski, 2011) argue that pointing is social from its very first use.65

64 “Adult-like”, yet rudimentary. That is, infant thinking is similarly structured to that of adults, but is an earlier version where adult-like knowledge and social understanding is not expected.

65 Although they argue they have empirical evidence for this claim, I argue that this is more of a theoretical claim. That is, pointing is a function of a complex human-specific social-cognitive pro-social motivational structure (shared intentionality) and therefore this research group interprets the first instances of pointing as inherently social. In the following I study I show evidence to the contrary.
The “rich/lean” debate entails interpreting what an infant knows about other minds when they point or respond to a point. Although this problem of interpretation is rife with conceptual issues (Racine, 2012), there is an obvious empirical gap in the theoretical interpretations of joint attention in infants. Even if we grant that infants may use pointing in “adult-like” ways to direct an adult’s attention, how is it that infants know that pointing is an effective means to do this? How do infants come to understand the meaning of a pointing gesture, whether it is their own or others? It is the attention-direction function of the pointing gesture that is taken for granted in joint attention research. Aside from the possible influence of directional movement or proximity of the gesture to an object, there is nothing inherent in the pointing gesture that directs attention. Moreover, a pointing gesture can be used to do many things, and directing attention is just one of the many uses it may come to have. How an infant learns to use and respond to pointing gestures is a developmental question that is seldom addressed. How pointing develops has simply been overlooked in the “rich/lean” debate which focuses on interpreting what infant pointing and point following indicates about what they understand about the minds of others.

3.2. Bates et al.’s (1975) Approach to the Study of Joint Attention

To examine the issue of the origins of the pointing ability and its relative neglect in the current joint attention literature, it is useful to examine the seminal project of Bates et al. (1975). This study did not just foster a great interest in joint attention, it also provided the groundwork for its study. Bates et al. (1975) charted the development of communicative gestures, namely pointing, from non-intentional communicative gestures to language use (i.e., they examined the development of intentional communication from its non-intentional beginnings before the production of speech through to the first instances of speech). They based their framework of analysis on an area of study in philosophy of language and linguistic semantics called “speech act theory” or “performative analysis.” Bates et al. (1975) adopted for their framework Austin’s (1962) proposal that utterances should be considered acts in and of themselves rather than descriptions of events (in contrast to traditional propositional analysis). Any utterance is comprised of several such acts. This conception helps bring attention to the fact that
meaningful communication occurs prior to speech in human communicative development and may provide the basis for the development of language later in infancy.

Austin (1962) outlined three speech acts: locutions, illocutions, and perlocutions. Locutionary acts refer to the act of speaking itself. Illocutionary acts refer to the functions a speech act may have through recognized social conventions (i.e., asking, commanding, scolding, praising etc.). Perlocutionary acts refer to the intentional and unintentional effects a speech act has on a listener. Applying these categories of speech acts to infants means that locutions refer to verbal utterances, illocutions (the intentional use of conventional signals) refer to gestures like pointing, and perlocutions refer to anything an infant does that has an effect on a listener (i.e., an infant's cry may have meaning for an adult which is not shared by the infant).

Basing their approach on speech act theory and empirical research which showed that certain performative structures develop well before the onset of speech, Bates et al.'s approach grounds the development of communicative gestures in activity and the social interactional history of the particular infant (also see Racine & Carpendale, 2007). Communicative gestures were thought to be based upon and emerge from pre-existing interactional structures. Bates et al. proposed a developmental trajectory in infancy of perlocutions, to illocutions, to locutions. Intentional communication was inferred from careful observation of its "first manifestations in gesture, eye-contact, and prelinguistic vocalizations" (p. 207). They were especially interested in the cognitive prerequisites needed to move through this trajectory. They framed cognitive development in a largely Piagetian perspective, contending that, "both performatives are first constructed 'on the plane of action,' employing objects rather than propositions. The use of propositions, or locutions, will then emerge gradually within the prepared performative schemes" (p. 209).

In the context of Bates and colleagues' framework, pointing is recognized as having an important role. The intentional use of gestures such as pointing represents the existence of more complex interactional structures (one which language is based upon, from this perspective) and represents intentional communication before infants begin speaking. Because of the importance of such gestures in communicative development,
Bates et al. created a system of classifying gestures by communicative intent that has become standard usage in joint attention research, limiting their study to "only the two most general performatives, the imperative and declarative" (p. 208). As imperatives in adult language are ways of controlling another's behaviour, they used the term "proto-imperative" to note the intentional use of another person as a "tool" in achieving some goal. Bates et al. explain a more "controversial" definition of declaratives provided by Parisi and Antinucci (1973) (cited in Bates et al., 1975) who describe declaratives as a particular kind of imperative. Thus, "proto-declaratives" are intentional attempts that direct adults' attention to some event or object. Such a purpose could be achieved through various means beyond pointing, such as showing, giving, teasing, etc.

The perspective and approach of Bates and colleagues necessitates a level of developmental analysis to understand the origins of communicative uses of the pointing gesture. From their perspective, intentional communication is based upon performative structures that develop prior to spoken language, and thus, attention must be paid to the development of these structures if one is to understand the development of communicative gestures and speech. Investigating the origins of these capacities therefore requires a longitudinal research program that involves detailed observations of infants well before the intentional use of communicative gestures right through to the onset of speech. By employing this analytical perspective and methodological approach, Bates et al. found evidence that the pointing gesture is used non-socially before it is adopted as a means of intentionally directing the attention of another (Bates et al., 1975).

Bates et al.'s (1975) seminal study established an interest in the development of pointing and set the groundwork for studies in the development of joint attention. However, not all joint attention researchers who have followed their lead agree that communicative pointing emerges from non-communicative uses of the gesture. Although a non-social origin is expected from Bates et al.'s (1975) framework, and is supported empirically (See Bates, 1976, 1979; Carpendale, Atwood, & Kettner, 2013; Carpendale

66 Because declaratives typically involve propositions and a speaker's commitment to the truth of these propositions, such a definition may not apply to the utterances of an infant, but can be extended to the communicative functions of their gestures (i.e. pointing).
& Carpendale, 2010; Lempert & Kinsbourne, 1985; Leung & Rheingold, 1981; Lock, 1978, 1992, 2001; Müller & Carpendale, 2004; Vygotsky, 1978), the arguably dominant position is that pointing emerges de novo as a communicative gesture (e.g., Liszkowski & Tomasello, 2011; Tomasello et al., 2005; 2007). In the following section I look at the differences between Bates’ position and that of Tomasello and colleagues.

3.3. Differences Between Bates and Tomasello

Nearly four decades of joint attention research have passed since Bates et al.’s foundational study. And inevitably, different theoretical perspectives and empirical approaches have emerged, each contributing in part to the debate about the origins of pointing. In order to make sense of the different claims in the field, I ground my analysis in the original perspective of Bates and colleagues. There are several aspects to note about the perspective and approach of these researchers that serve to illuminate, or provide contrast for, certain current issues and debates in the study of joint attention. The disagreements between joint attention researchers stem, in part, from the fact that neither Bates et al.’s perspective nor their approach was adopted in its entirety. Certain aspects of their work have been emphasized to the neglect of others. I argue that differences amongst joint attention researchers can be attributed to this neglect, as well as the misinterpretation and misapplication of their methods.

3.3.1. The Rich/Lean Debate

The question of “how does pointing develop?” has been succeeded by the question of “what do infants understand about minds when they point?”. This question is at the crux of the “rich/lean” debate. Not only is the development of pointing taken for granted in current joint attention research, the significance of pointing itself is conceptualized in a very different way. For Bates, pointing and other communicative gestures are based upon interactional structures that exist between infant and adult. Communicative gestures gain their meaning in this already meaningful interactional context and sets the stage for the development of more complex interaction and communicative skills, culminating in speech. Bates and colleagues have been characterized as taking a “lean” perspective, as they invoke the role of learning
mechanisms (see Racine, 2012 for review). However, referring to Bates et al. as “lean” researchers would insinuate that their interests and approach are the same as current "lean" joint attention researchers. This is an inappropriate label because they were not engaged in the “rich/lean” debate; they were interested in the actual development of pointing which is seemingly not the interest of the researchers involved in the current “rich/lean” debate. Bates et al. (1975) were indeed interested in intentional communication, but for them, intention was clear in the infants’ coordinated interactions with others based on situational context and interactional histories of the infants. There was no mystery about “what’s going on in the head” of the infant. There was no need to move an explanation of social understanding to a “lower” level; they were interested in the actual real-time coordinated joint activity that itself constitutes and demonstrates social understanding. As such, it is difficult to see what position Bates and colleagues would take in the “rich/lean” debate. Indeed, it does not seem like they would have a position at all. However, by virtue of studying the actual development of joint attention, and by describing increasingly complicated forms of interaction as constituting understanding itself, it is clear the issue of “interpretation” that is the basis of the “rich/lean” debate is not a critical issue from Bates and colleagues’ perspective.

3.3.2. The Origin of Pointing

Tomasello and colleagues argue that pointing is social from the beginning. They base their interpretation of the nature of the pointing gesture largely upon studies of 12-month-old infants. They argue that this evidence shows that infants consider whether an adult can attend to their points before pointing for the adult (Liszkowski, Albrecht, 67 It should be noted that Tomasello (2003) does acknowledge that certain conventional gestures can have social origins, namely through a process called “ontogenetic ritualization” which is defined as “a process of mutual anticipation in which particular social behaviors come to function as intentional communicative signals” (Halina, Rossano, & Tomasello, 2013, p. 653). He argues this is the means by which the “arms-up” gesture young children use when they want to be picked up by an adult attains its communicative function. He also acknowledges that pointing can have such origins but invokes this process in the development of pointing in captive apes rather than in humans, and in this case, it is limited to the development of imperative pointing, or “pointing” as he puts it. Declarative pointing, for Tomasello, stems from mental state understanding and an evolved motivation to share attention that is specific to humans, which he argues is why neither wild nor captive apes point to share attention (see Tomasello, 2008).
Carpenter, & Tomasello, 2008), and infants persist in pointing for an adult when the adult fails to share attention and interest in what the infant wants the adult to attend to (Liszkowski et al., 2004). Liszkowski and Tomasello (2011) recently found that although there are morphological differences in pointing by 12 months that require longitudinal study to examine earlier behavioural forms of the gesture, the pointing act itself is a fully communicative act of reference. Liszkowski (2011) stated that the above findings “establish experimentally that 12-month-olds point with the intention to communicate” (p. 35). Although they make this out to be a more substantial discovery than it is, I agree with Liszkowski and Tomasello on this point. Save for some extremely “lean” interpretations of pointing (see Moore & Corkum, 1994), there is not much debate over the fact that 12-month-old human infants intentionally communicate. Although the paradigm Tomasello and colleagues use satisfies the criteria for intentional communication, it does nothing to reveal the developmental and evolutionary nature of pointing. The rich/lean debate surrounds whether or not infants’ social understanding is “mentalistic” in nature; it has little if anything to do with how pointing develops, and the links to evolutionary factors are taken for granted in a non-developmental evolutionary metatheory (i.e., in the case of Tomasello, his theory of shared intentionality).

Tomasello and colleagues use cross-sectional methods to determine how infants at one year of age use pointing gestures. Unfortunately, they inappropriately conflate developmental outcomes with the processes of development that led to these outcomes. For example, they assume the way infants use pointing gestures at 12 months is the way they used them from the very beginning. As noted earlier, cross-sectional methods are utilized in developmental research to determine what psychological capacities infants have and the parameters at which they operate.68 These are important empirical questions that should be addressed, but they do not deal with the processes of development that lead to the capacities in question at all. Instead, Tomasello and colleagues allow their theory of shared intentionality to inappropriately fill in the empirical gaps in their research. To them, pointing is a priori social in nature because it rests on

68 There are many reasons (theoretical, empirical, practical, etc.) to employ cross sectional methods, and I will not evaluate each here. My main purpose is to demonstrate how cross-sectional methods, as employed by those arguing for a side in the “rich/lean” debate, does little to inform us on the development of pointing.
pre-existing, uniquely human adaptations to share attention and cooperate with others. Theory, however, is not evidence, and sometimes theory can subtly take researchers off course, changing the nature of what is under study, and leading researchers away from empirical questions. In the case of infant pointing, the question of how it develops has remained unasked and therefore unanswered in Tomasello’s research.

### 3.3.3. Conceptualizing Pointing

Tomasello and colleagues conceptualize pointing in a very different way than Bates et al. (1975), and their adaptationist theory of shared intentionality is compatible with their cognitivist assumptions. The purpose of this part of this dissertation is largely to point out the irrelevance of the empirical data that Tomasello and like-minded researchers use to argue for the social origins view of pointing. Although the purpose is not to explicate all the cognitivist assumptions in Tomasello’s theory, these assumptions do bear on their interpretation of pointing and on their position in the “rich/lean” debate. In this regard, it is illustrative to note that Tomasello’s view seems to imply that an infants’ ability to use pointing is an indicator of their stage of social cognitive development. That is, pointing is considered to be 1) a proxy for their psychological development and/or 2) a product of their psychological development. In this model, pointing is of secondary importance to the cognitive features which purportedly underlie pointing. Utilizing cross-sectional paradigms with established pointers makes sense if pointing is viewed as a proxy for other processes or are viewed as a product of them, as researchers are interested in which underlying processes are supposedly operating and to what extent. Thus, from a “rich” perspective, infants use pointing in a way that is consistent with their mentalistic understanding of others. Using these methods sidesteps the question regarding how pointing develops, and if there is an interest in it, it is assumed that it develops as a product of the underlying social-cognitive processes of a pointing infant. The research program has apparent support due to the self-confirming circularity of the argument (Bibok, 2011). That is, pointing is conceptualized in a particular way and experimental methods are chosen based upon these conceptions.

69 In a similar way that these capacities are indicators of, and products of, adaptations.

70 That is, this development is hidden and happening on a cognitive level rather than happening in the real world in social interaction or activity.
Unsurprisingly, the empirical evidence provides apparent support for the suppositions. Pointing thus maintains its role as a proxy of social cognitive development: a reliable indicator of the processes that seem to be the main interest of “rich/lean” researchers.

The issues facing the study of joint attention are complex. There is conflation of conceptual, theoretical, and empirical issues (Racine, 2012), as well as issues at the metatheoretical level (Wereha & Racine, 2012). The question of how pointing develops has, paradoxically, become a theoretical issue instead of an empirical one. How infants come to use pointing to communicate has been neglected in favour of what pointing is presumed to represent about the complexity of an underlying social understanding of the infant, as if this is somehow separate from the use of pointing itself. As I pondered above, if an infant is using a pointing gesture to direct attention, how is it that the infant comes to know that a point can direct an adult’s attention? Even if we accept a “rich” interpretation and agree that infants point in adult-like ways, how did they learn this use of the gesture? This problem also applies to infants comprehending pointing gestures of adults because presumably infants would have to understand the mental state of the pointer to comprehend the point. The attention-directing nature of pointing is taken for granted in the “rich/lean” debate, and its origin is speculated upon rather than actually studied. In this respect, legitimate longitudinal programs studying the development of communicative gestures like pointing could help end some of the theoretical conjecture by joint attention researchers led astray by the “rich/lean” debate. From my perspective, the rich/lean debate is misguided: it does not speak to the development of pointing gestures, and merely consists of confusing interpretations of what pointing gestures “mean” in terms of infant social understanding. I argue, like others before, that the use of pointing gestures constitutes social understanding. From this starting point, a more developmental perspective ensues, as does a research program that requires longitudinal methods to examine how infants come to use and understand pointing gestures over the transition between dyadic and triadic forms of interaction.

71 Of course how infants use these gestures may be a legitimate question and is not mutually exclusive with the former question; however, the demarcation between these questions, if they can even be viewed as separate questions, can be made and has to do with conceptual assumptions more fully explored elsewhere (Racine, 2012; Racine et al., 2008; Susswein & Racine, 2008).
In the following sections I examine how well the particular claims and assumptions of social-origin of pointing advocates, namely those of Tomasello and colleagues, hold up to longitudinal data regarding the communicative development of infants. I will show that several of the claims these researchers make are untenable in the light of these data.

3.4. The Development of Pointing

Whereas others have noted that little is known about the development of pointing, I have attempted to explain why it has, paradoxically, been ignored in joint attention research. Even recent studies that have attempted to remedy this situation, such as that of Matthews and colleagues (2012), are still woefully inadequate. This should not be surprising given that such studies are still informed by the same paradigms, assumptions, and confusions that led these researchers to ignore the development of pointing in the first place. I credit them with finally taking some steps in studying the development of pointing at an earlier time in infant development before it has become well-established communicative gesture at 12 months. However, it is unlikely that an adherence to the strong adaptationist theory of shared intentionality will ever lead to an interpretation of the data that is not consistent with this theory. This is especially so when this theory is congruous with evolutionary and developmental assumptions that are widely held and thus are seldom examined. To understand the development of pointing, one cannot reverse engineer the developmental process: one has to study development as it occurs.

One of the ways one can obtain useful longitudinal data is through the use of parental records. In the following, I present data obtained from a diary study in which mothers were asked to record observations of the day-to-day interactions with their own children. Diaries have been used sparingly in recent developmental studies (e.g., Adolph, Robinson, Young, & Gill-Alvarez, 2008; Canfield, 2007; Carpendale & Carpendale, 2010; Carpendale et al., 2013) but are a means of obtaining detailed data on the normal, often quickly changing, day-to-day activity of an infant that is inaccessible to experiment-based or even semi-naturalistic observational methodologies.
The following data will be used to address several issues that arise from the strong adaptationist theory of shared intentionality. According to this theory the way humans come to share attention is underwritten by species-typical pro-social motivations. The experimental findings that infants are proficient pointers at 12 months of age, along with the theorized socio-motivational adaptations underlying this outcome, lead Tomasello and colleagues to conclude that pointing emerges as a fully social gesture. However, this claim is made without adequate longitudinal data to back it. Besides the obvious lack of developmental data on the origins of pointing, Tomasello and colleagues’ argument also implies consistency in development in that it invokes the existence of a universal mechanism that leads to a species-typical “cognitive revolution” in infants at one year of age.

I begin this analysis by demonstrating that the social origin theory of pointing is problematic because the non-social origin position has much empirical support (Bates, 1976, 1979; Carpendale et al., 2013; Carpendale & Carpendale, 2010; Lempert & Kinsbourne, 1985; Leung & Rheingold, 1981; Lock, 1978, 1992, 2001; Mead, 1934; Müller & Carpendale, 2004; Vygotsky, 1978). This will set the stage for a closer examination of the assumptions of shared intentionality. Unless one is explaining pointing by the “insight” model or “pro-social motivation” model of pointing that posits a single origin of pointing, there is no particular reason to believe that pointing has a single origin. Consistency in developmental outcomes can be expected by the interaction of reliably reoccurring factors in development (Carpendale et al., 2013; Carpendale & Wereha, 2013). However, considering the variability of an infant’s history of social experiences that involve sharing attention and variability in individual capacities, one should expect to see pointing emerge through various means (Carpendale & Carpendale, 2010). Even though infants may come to use pointing in similar ways, this does not necessarily mean that pointing emerges in the same manner for all infants. Therefore, the development of pointing will be examined in the context of specific experiences of infants.

Adding to the standard picture of consistency in pointing development that Tomasello and colleagues routinely consider the first instance of a gesture (e.g., a declarative point) to be an indication for a capacity for that gesture (e.g., Carpenter et al., 1998). I will demonstrate, using these data, that the developmental picture is not so clear
because the first indication for a capacity is not often a reliable indicator of the onset of that capacity. Further, Tomasello and colleagues use aggregate data to draw their claims; however, I argue that individual variation should not just be dismissed. This variation may actually represent different developmental pathways infants take in their communicative development, which is contrary to the tacit assumption of a single trajectory in the development of pointing gestures. Taking individual development seriously is also more consistent both with the perspective and approach set forth by Bates and colleagues and a developmental evolutionary metatheory. I will present data that infants can be very inconsistent in their use of gestures and it is not until around the one-year-old mark that they begin to use these gestures consistently and even then many children are not pointing consistently at this age.

3.5. Diary Methodology

These data comes from a longitudinal study investigating the development of communicative gestures in infants (see Carpendale & Carpendale, 2010). Participants were recruited through word-of-mouth of a research assistant in the Greater Vancouver Area. The infants joined the study at various ages, from three weeks to 15 months of age. The participants were 16 infants and their mothers. Eight participants were not able to provide sufficiently detailed accounts or were not able to provide detailed accounts for a sufficient time. In the analysis that follows, only the records of six infants (under the following pseudonyms) are analyzed: Allen, Bonnie, Celine, Kernie, Markus, and Mitchell. The two other diaries did not contain sufficient information for analysis for this particular study.

The mothers of the young infants were asked to maintain a diary record of significant communicative interactions with their children. Rather than making observations at regular intervals, the mothers were asked to make notes regarding these interactions, such as the appearance of new gestures or new communicative behaviours in general, and changes in forms of interaction, as they occurred. The primary investigator (J. Carpendale) and a research assistant met with each mother-infant dyad to describe the study, the interactions of interest, and how to maintain the diaries. The research assistant made weekly telephone calls to each mother to answer any questions
they may have had, to talk about their experiences, and to remind or encourage participants to maintain their respective diaries. The research assistant would also meet in person with the mothers once a month for the same purpose. Diaries were sent to the research assistant regularly via email.

There is a tendency to downplay the usefulness of diaries in a field where research is biased toward experiment studies (see McCall, 1977). Strong adaptationist assumptions and a questionable privileging of experimental procedures leads to research that is inappropriate for understanding the development of capacities such as joint attention. That is, experimental procedures are often employed after development has already occurred. A critique that comes from this strong adaptationist/experimental camp is that the observations made in diary studies are “anecdotal” accounts. Such “anecdotal” accounts, however, play a much more important role in scientific inquiry than these researchers seem to appreciate. For example, in ethological research, a method known as ad libitum sampling, where notes are made on all facets of observable behaviour as it occurs, reflects a legitimate and arguably the most common form of observational technique in field studies of behaviour (Altmann, 1974). Similarly, diary descriptions reflect a legitimate means of recording important and quickly occurring changes in infant development.

Critics of “anecdotal” accounts often state such accounts are inherently biased. Bias may enter in two ways. One way is that only the most salient behaviours will garner the attention of the observer, rendering the description somewhat incomplete or potentially misleading. The second way is the potential bias of the observer, who may unintentionally pay more attention to certain behaviours than others, and/or may “read in” to the behaviours inappropriately. Also, because mothers are interacting with their infants, mothers may potentially influence their infants actions and do not offer a more distant “outsider” perspective. Indeed it is questionable to what extent mothers are “objective observers” of their own children. Although this bias is important to keep in mind, these issues may actually be strengths. Mothers know their infants well and are sensitive to the changes in their capacities. Another potential advantage of any parental bias is that it provides us a window into the nature of the relationships between parents
and infants. It is possible that any “richness” in interpreting infant behaviour is an important part of infant development itself. That is, parents interact with infants as though they exhibit intentionality beyond that which they may actually be capable. It is within this interactional context, where parents react to the actions and communicative bids of infants as meaningful, that creates the structure in which infants can come to understand that their actions and communicative gestures have meaning for others, and is an important factor in the development of intentional communication and social understanding (Carpendale & Carpendale, 2010). Only within a strong adaptationist perspective, which focuses on the individual infant itself rather than the entire developmental system involving the mother-infant dyad, does such “bias” seem more problematic than advantageous.

This study design has the advantage of describing development as it happens, rather than inappropriately attempting to reverse-engineer outcomes post hoc. Moreover, important developmental events often occur in short temporal windows that may not be accessible to even frequent experimental or observational methods. The advantage of frequent observations to more accurately portray the developmental process may have a trade-off in validity with the researchers’ intrusion and the artificiality of experimental, semi-experimental, or observational methods used. Parental diaries can be a solution to this issue, with naturalistic observation of important developmental milestones as they occur.

I now turn to evaluating the diary data with respect to the claims of researchers who argue that pointing emerges as a fully-formed social gesture. I also use these data as a means of illustrating the implicit and explicit assumptions contained in this account.

### 3.6. Reflexive to Social Pointing

It is generally accepted that at around one year of age, infants become prolific pointers. This “pointing explosion” precedes the so-called “naming explosion,” which

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72 It is interesting that critics of the “richness” of the observations of parents are themselves the advocates of a “rich” view of social cognitive development.
occurs around 18 months of age. Declarative pointing is thought to be a key factor in language acquisition (see Colonnese et al., 2010). Perhaps the “naming explosion” is the better known of the two, but because joint attention is considered to be foundational for language capacities, the “pointing explosion” is no less important. Tomasello et al. (2007) argue that at 12 months of age, infants are using pointing in sophisticated “adult-like” ways to interact with their social world. Though this may be the case, as previously noted, most of Tomasello’s research is unconvincing in terms of shedding light on the origins of the pointing gesture because this work is conducted largely after pointing has already developed (Carpendale & Carpendale, 2010).

The diary data, however, is more suitable for examining the origins of pointing. More specifically, it helps demonstrate what is occurring previous to the “pointing explosion” that occurs at 12 months. It appears that the transition into this stage of proficient pointing is largely a transition from non-social to social forms of pointing. The data from several diaries in this study describe this transition from less “reflexive” pointing, i.e., reacting to objects or events in the environment that gains the infants' attention, to more intentional uses of pointing that suggest deliberately employed social uses. For the infants in this study, this seemed to occur mostly between 12 and 13.5 months. The following two examples describe this transition between “reflexive” and more deliberate, purposeful pointing:

Allen (12 months): it was pointing as usual for a while but lately I’ve noticed some new behaviours emerging. One is that he doesn’t seem to point like a reflex every time I look at him. It seems more controlled and willful when he wants to show me or ask for something. When he wants to be breastfed he points to my necklace and says something that sounds like "brust."

Kernie (13.5 months): In recent days he seems to want to get my attention when he points. Before he seemed to be pointing just because. Now he will look at whatever it is and point, then look at me and then back to look at whatever it is and say "da" and point. He will repeat this until I respond. It is the repeating until I respond that is different now. Before he didn’t expect me to respond. I try to figure out what he’s pointing at. If I get it right he will smile. I will label whatever it is - a picture, a toy - and he will smile.
Both examples describe a change from "reflexive" and relatively random "pointing just because" to "more controlled and willful" with repeated pointing until the mother responds correctly when "before he didn't expect me (his mother) to respond." Because the basis of joint attention is coordinated action, it is unclear how intentional this pointing activity can be if mothers are describing their infants' pointing as reflexive and uncoordinated. Moreover, in Allen's case, this reflexive pointing is most often directed at his mother. Tomasello's research makes no reports of such pointing, and from his social-origin perspective, it is not clear what social function such pointing would fulfill. A similar transition is also demonstrated in the pointing records of Bonnie. The following two examples demonstrate how this change in pointing behaviour can change substantially within a month:

Bonnie (12 months): Bonnie points a lot - mostly to interesting things in her environment, without much looking back to check that we are also paying attention. Sometimes, she will point at something and then look at me, vocalize "dah," continue pointing and looking at me again. I usually try to see what she is pointing at and talk about whatever that thing is. She doesn't seem frustrated if I don't pick up on it or don't respond. She will just move on to something else.

Here we see the reflexive nature of some of Bonnie's pointing. Her pointing is largely directed to things that attract her attention in her environment. This is a non-social use of pointing where pointing seems to be a function of the infant's own attention or is a means of maintaining visual tracking of objects. This pointing for the self has been more fully described by Carpendale and Carpendale (2010). The non-social aspect in this example is demonstrated by the fact that Bonnie does not seem to coordinate her attention with others, exemplified specifically here by her seeming disinterest in whether her mother responds or not. However, it appears that around 12 months of age Bonnie is beginning to discover the social-functions of pointing when she begins to engage in visual checking with her mother. This instance may represent a transitional phase where she is coming to understand that pointing can be used to share attention. A month later, Bonnie is using pointing for explicitly social purposes:

Bonnie (13 months): This morning at breakfast, Bonnie didn't want to eat much. The back door was open and apparently, she had other plans in mind. Soon after
sitting to eat, she signed "finished," and then pointed to the open door. I took this to mean, that she wanted to go outside. Since we were not finished breakfast, we cleaned Bonnie up, let her out of her high chair, but closed the door to prevent her from going outside. She fussed and cried. This confirmed to me that her point meant that she wanted out to play.

At this point Bonnie now appears to use pointing in a way in which she expects a specific response and is dissatisfied when her expectations are not met. If Bonnie was pointing as a function of her attention or to maintain a visual focus of an object it is unlikely that she would protest so strongly when her parents did not oblige what was essentially a request to play outside.

The final example of this transition, similar to Bonnie’s, demonstrates a change in behaviour where fulfilling expectations is key. At 13 months of age Markus seems to engage in non-social pointing and only occasionally engages in communicative (social) pointing:

Markus (13 months). I have noticed Markus pointing a few times in a way that I don’t think he is trying to communicate anything to me. Once we were at the open gym at our rec center and he stopped playing, looked at the ceiling and pointed up. He may have been pointing to the fan but he didn’t look at me and his pointing only lasted a few seconds and then he resumed playing. Other instances of this type of pointing are similar, he will casually point to something without looking at me or make any noise to try to get my attention like he does when he wants something he is pointing at.

A mere two weeks later, however, Markus has discovered the social uses of pointing, and correspondingly, there is a substantial increase in his frequency of pointing:

Markus (13.5 months). Markus seems to be very excited about his ability to point, he seems to love pointing out different things to me and this seems to be the most common type of pointing that he engages in. Animals and other kids seem to be the most common things that he will point out. When he is pointing to animals and kids he will always vocalize as well. He will, at times, point to things that he wants (e.g., food) but this is also accompanied by a reach towards what
he wants and a sort of whiny noise. Because he is trying to reach what he wants he doesn’t always point, but instead makes a grabbing gesture to indicate that he wants to have it.

When Markus discovers the social nature of the pointing gesture he points in a declarative context, that is, to share attention. It is noted that he points imperatively as well, but the imperative function seems to be incompletely developed. It seems that Markus is reaching for what he wants and thus his actions are directed towards the item (the grabbing gesture, i.e., opening and closing his hand). He does not employ a canonical pointing gesture in these instances, but his desire for the object is clear and is fulfilled by his parents. Such situations could be important transition phases where infants have already learned the meanings of some gestures that in turn provide the basis for the further development of their gestures, in this case, of imperative pointing.

These diary excerpts demonstrate several things. First of all, in line with the standard picture of pointing development, pointing is used socially at about one year of age. However, diary excerpts show a picture of how the pointing gesture develops that differs considerably from the standard view, which states that pointing begins as a social gesture. The “reflexive” and uncoordinated use of pointing gets put to deliberate social uses near one year of age. Indeed, for several of these infants, it is social pointing that constitutes the “pointing explosion.” It is this substantial increase in pointing frequency and consistency that Tomasello and other researchers argue is a function of a fundamental cognitive shift that infants undergo. According to Tomasello’s “rich” view, pointing is a function of infants coming to understand other’s mental states. However, pointing is explained in terms of this mentalistic insight rather than grounding an explanation in the interactions and contexts in which infants come to point. Perhaps the perceived low frequency or inconsistency of pointing prior to the pointing explosion can lead these researchers to ignore these earlier instances of pointing. However, it is more likely due to their lack of developmental analysis of pointing and to the fact that the strong adaptationist theory of shared intentionality is incommensurate with non-social
forms of pointing.\textsuperscript{73} at least prior to the development of social forms of pointing. Moreover, the mentalistic insight still does not address how infants come to understand the communicative functions of pointing. Even if infants form such an insight, it is not likely that it comes with an attendant insight into pointing gestures being “the” way to influence other’s mental states. How infants come to understand that points direct the attention of others is the next question that will be explored.

\section*{3.7. Different Developmental Pathways}

An unacknowledged assumption of the standard social-origins account of pointing development is that of a single developmental pathway. Social-origin advocates do not explicitly state this assumption, and most would not deny that there are individual differences in the development of pointing. However, an assumption of consistent development is apparent in two major respects. In Tomasello’s account, for example, a general developmental pathway is assumed in the theory of shared intentionality; that is, the uniquely human adaptation for pro-social behaviour interacts with adaptations shared by the great apes for joint attention. The consistency of joint attention development by one year of age adds to the canalized developmental notions of ultra-Darwinian adaptations. This assumption of consistency leads to empirical methods that reify this assumption. Cross-sectional experimental designs collapse scores across participants. Individual differences are treated as noise. In developmental research, however, this “noise” is very important and, in fact, tracing and explaining this variation in terms of a multitude of factors is the purpose of developmental analysis. Contrary to a strong adaptationist view, a systems view sees consistency in development as a product of myriad of factors and processes that reliably take place rather than the action of particulate adaptations (Carpendale & Wereha, 2013; Wereha & Racine, 2012).

\textsuperscript{73} Indeed, Tomasello recognizes that pointing develops before the age of 12 months as ontogenetic ritualization. However, if he paid adequate attention to the development of pointing, he’d likely discover the conceptual distinction he’s drawn between these “two forms” of pointing, one demonstrating a “behavioural” and one a “mentalistic” understanding, is flawed, and that the actual use of pointing is demonstrative of social understanding itself.
In the following, I review diary excerpts to evaluate the importance of individual differences and argue that they lead to an understanding of the developmental process underlying how infants come to understand pointing. Table 2 shows the developmental trajectories in how six infants from the study vary in terms of the transition from non-social to social uses of the pointing gesture. The first category denotes the first use of a pointing gesture to explore the environment. Prior to this the infants would only touch things with their whole hand. The second category denotes the first instance of self-directed pointing that does not seem to have a social purpose. Because it is not a safe assumption to assume the first instance of a gesture indicates a new capacity, the third category is included to mark the point when parents note a substantial increase in the frequency and contexts the capacity, in this case, pointing for self. Similarly, the fourth category includes the consistent use of pointing for social purposes, which are the figures described in the above section on the transition from non-social to social pointing.

There are a few things of note in this table. One of the observations undermines the assumption that the first use of a pointing gesture can be taken as a fully-formed capacity for the gesture. It seems that although some infants begin to use self-directed pointing consistently after it is first used, other infants do not. Although many researchers assume that an instance of a behaviour equates to a capacity, these observations show a two-week lag for some infants between the first use of self-directed pointing and its consistent use, which I would argue is a more appropriate criterion for inferring a capacity. That is, it is not clear in what sense one can state that an infant has a capacity if it is not manifest in their behaviour. We can often safely assume adult humans have capacities that may not manifest in any given situation; however, this stance is difficult to take with infants, especially when we are interested in the development of the very capacities we can only later take for granted with adults.

It is also important to note that non-social uses of pointing (pointing to explore objects and pointing as a manifestation of the infant’s own attention) precede consistent social uses of pointing. There is some variability between the infants in the length of time between consistent self-pointing and consistent social pointing. Markus and Kernie, for instance, did not point much at all until 13 months, and then very shortly after began pointing socially. Other infants like Bonnie, who was using her index finger to explore the
Table 2

*Developmental Trajectories of Six Infants From Non-Social to Social Uses of Pointing*

<table>
<thead>
<tr>
<th>Infant</th>
<th>1st Exploratory Point</th>
<th>1st Point for Self</th>
<th>Consistent Self-Point</th>
<th>Consistent Social-Point</th>
</tr>
</thead>
<tbody>
<tr>
<td>Allen</td>
<td>10.5</td>
<td>10.0</td>
<td>11.5</td>
<td>14.0</td>
</tr>
<tr>
<td>Bonnie</td>
<td>7.5</td>
<td>9.5</td>
<td>10.0</td>
<td>13.0</td>
</tr>
<tr>
<td>Celine</td>
<td>10.0</td>
<td>11.75</td>
<td>11.75</td>
<td>13.5</td>
</tr>
<tr>
<td>Kernie</td>
<td>9.5</td>
<td>13.0</td>
<td>13.0</td>
<td>13.5</td>
</tr>
<tr>
<td>Markus</td>
<td>10.75</td>
<td>10.75</td>
<td>13.0</td>
<td>13.5</td>
</tr>
<tr>
<td>Mitchell</td>
<td>10.0</td>
<td>10.0</td>
<td>10.5</td>
<td>11.75</td>
</tr>
</tbody>
</table>

world much sooner than the other infants and was the first to demonstrate consistent self-directed pointing, still did not use pointing for social purposes until the time that most of the infants did, even the infants who did not use pointing much at all before one year of age. The lengths of these trajectories differ among the infants as well. Mitchell first used a pointing gesture at 10 months and was pointing socially before one year of age. Kernie first used his index finger at 9.5 months and demonstrated social pointing at 13.25 months. Although this outcome can be redescribed by invoking an adaptation, it is more useful to examine the consistencies in development that lead to this outcome. As such, similarities in social experience would be the most likely factor influencing the outcome, and differences would likely influence the individual trajectories. Although summary tables like this can be useful, they are also limiting as there is no context for explaining these differences, and the differences could be idiosyncratic. For example, mothers may miss important instances of pointing. This lack of context is not unlike the problem that occurs in cross-sectional research that collapses across individual
differences, giving an impression of a single trajectory. I now turn to a detailed look at
the trajectories of these infants.

The table above is concerned only with canonical pointing gestures. However,
some infants were pointing in other ways prior to these times. For example, although
Celine did not start using her index finger to explore the world until 10 months of age, by
9 months of age, she was pointing with her whole hand to the pet cat and dog. These
were non-social points that were a function of her own attention. Prior to whole hand
pointing, she demonstrated a different kind of “pointing” which leads into another issue.
As noted above, parental records may contain a level of “richness” that should not be
unexpected. Such an instance is found in the following, where shortly after 7 months of
age, her mother describes how Celine “points with her eyes”:

Celine (31 weeks). We do the "where's the kitty?" thing daily, and she gets it right
every time. She looks around and finds him, then looks back to me, as though for
approval. She doesn't make any hand gestures, but definitely "points" with her
eyes.

Whether or not Celine is actually “pointing” in a strict sense here is debatable (as it is not
clear that Celine is “pointing” in this way for her mother), but this issue is less important
than the fact that her mother is interpreting her behaviour as a “point.” What it signifies to
her mother is that Celine understands the game. The “where’s the kitty?” game is more
than a game: it is a routine they practice every day and provides the context for Celine to
learn about social interaction, and also, gain a practical knowledge about attention and
meaningful interaction. It is these kinds of routines that Bates and colleagues argue to
be the basis of language, and many other researchers have followed in explicating the
importance of these routines. Any sufficient developmental account of the ontogeny of
pointing should be grounded in these routines and how they lead to increasingly
complex language and gesture structures, and which, in turn, influence the complexity of
the routines the dyads engage in. Once again, cross-sectional research done after
pointing develops completely overlooks this important information and hence
misconceives the developmental nature of pointing, taking it for granted with reference to
adaptations.
Kernie’s is an interesting trajectory. Kernie’s first use of the pointing gesture occurred between 8 and 9 months and the only time his parents observed it was during reading time. At 13 months, however, he began pointing a great deal:

Kernie (13 months). This week Kernie is pointing at lots of things (he never used to point at anything) and he says "Da." I always tell him the name of whatever he’s pointing at and he looks at me like he’s listening. When he points he looks at me to get my attention, but his purpose in getting my attention seems to be just that. He doesn’t point at something requesting that I give it to him for example.

Kernie (13.5 months). In past entries I’ve said that Kernie rarely points, then he became a pointing sensation overnight when he started spending lots of time with another boy at daycare. The pointing was something new for him and he didn’t understand that I would respond, he’d just constantly point at everything and say "da" and not expect me to respond. But then, last week he used pointing with purpose.

It is interesting to note that Kernie seemed surprised that his mother was responding to his gesture. This is unlike other infants who did not seem to take particular notice of their parents’ responses when they were engaged in self-directed pointing. However, because Kernie started pointing at 13 months of age, he is likely already sensitive to the attentional states of his parents. However, it is curious that he was not pointing until this age. Kernie seems to have picked up the gesture after spending time with another boy in daycare. The surprise he shows at his parents’ response to his pointing could be that he was largely just mimicking the gesture before and did not understand the meaning behind it, or perhaps he was using pointing only with his friend and did not realize that it would be meaningful to his parents as well. It is odd that if pointing emerges as a function of mental state understanding, as Tomasello and colleagues contend, that he would act surprised at his parents’ responses. It does not take long for Kernie to discover the social functions of pointing such that he begins using it deliberately and consistently within a couple weeks. Kernie’s sensitivity to his parents’ actions is evident by ten months of age in his tendency to mimic what they do:
Kernie (10 months). Kernie is mimicking lots of our actions during play. A few days ago I was sitting on the floor with him and showing him how some of his blocks stick together and come apart like Lego. Then yesterday he was playing with James and he had two of his soft blocks in his hands. He kept lifting them up to the height of his forehead and mashing them together. James thought this was him trying to stick them together. I will post a video of this shortly. Last week I was breastfeeding Kernie on the couch. James came and sat beside me on the side opposite to Kernie. James had a really sore shoulder and asked me to give him a bit of a massage around his shoulder. I did this while I nursed Kernie. Kernie was very interested in this. After a few minutes he stopped nursing, sat up, and started rubbing James’ back, sliding his open hand back and forth across James’ back.

What is clear here is that mimicking, for infants, is not an uncontextualized copying of adult behaviour in a “monkey see, monkey do” type of manner. Mimicking here consists of actual involvement of the infants in routines with parents. Infants here are learning about behaviours that have meaning in certain contexts. Because the contexts involving pointing gestures are more varied, it does not seem to be the case that mimicking can explain the appropriate use of pointing in all the possible communicative contexts in which it can be employed. That is, an infant can point to mimic an adult, but the infant still needs to learn how to use it meaningfully in interaction with adults (Carpendale & Carpendale, 2010). Mimicking pointing in the strict sense, may not lead to an infant learning how to point; however, involvement in the types of routines between infants and parents that involve sharing attention provide the basis for learning the communicative uses of pointing.

From these examples it should be clear that individual differences in development should not be written off as “noise.” In developmental research, this “noise” should be considered to be the stuff of development itself. Relating this variation to social interactional factors in the infant’s environments can lead to understanding how social understanding, and in this case, the communicative functions of pointing, develop. Although many developmental researchers may agree, it is surprising that claims about the development of pointing can be made by researchers who do not actually study the development of pointing.
Thus far I have used longitudinal data to demonstrate the non-social origins of pointing and the importance of examining individual variation in an attempt to draw attention to the problems with the current standard approach to conceptualizing joint attentional abilities and studying of how they develop. The final issue I use these data to examine relates to how infant pointing gestures are categorized.

3.8. Categorical Issues and the “Adult-like” Nature of Infant Pointing

The final issue I raise concerns how pointing and joint attention are classified in the research literature. Although categorization is an undoubtedly useful exercise in research, it is equally true that maintaining a manageable number of categories can lead to oversimplification of a phenomenon, and, in turn, to divisions that perhaps are largely artificial. It is sometimes difficult to draw a partition between “non-social” and “social” behaviours in an inherently social species such as humans who are embedded in a social and cultural milieu, and this is especially true for infants whose development hinges to a large part upon the extent to which they engage with their social world. However, in other ways the distinction between non-social and social pointing is unproblematic. Researchers who argue for a non-social origin of pointing (i.e., infants point for their own purposes then come to understand through social interaction that points can have meaning), like Carpendale and Carpendale (2010), argue that pointing is related to the attention regulation of the infant itself. That is, pointing seems to originate as a function of the infant’s own attention or visual tracking. In this sense, calling the origin of pointing “non-social” is not a problem. What I refer to as social pointing, in the nomenclature of joint attention research, are instances of declarative and imperative pointing. For those who argue for a non-social origin of pointing, the big question is how non-social pointing develops into declarative and imperative forms of pointing. For those who argue for a social origin of pointing, the focus is on the difference between these two social forms of pointing, because as stated earlier, they are thought to represent different levels of social understanding. In general, however, the distinction between declarative and imperative forms of pointing is still considered to be important for both perspectives.
Bates and colleagues are credited for the creation of these two categories of pointing, and this contribution has been invaluable to joint attention research. However, it may be the case that categorizing points in terms of these two super-categories can lead to a somewhat restrictive situation where points fall under one of the two categories (or three in the case of “informative pointing”). Further, the focus on these two super structures of pointing, ironically, has taken attention away from the specific ways infants are using pointing. That is, the actual development of social understanding that different uses of pointing (in increasingly complex and coordinated ways) represents is potentially minimized or misconstrued when they are categorized in such a way. This is especially so because the distinction between these two forms of pointing has been formalized by strong adaptationist assumptions that these forms of pointing are underwritten by different psychological mechanisms. However, Bates and colleagues made a distinction between communicative functions, not psychological mechanisms.

Put another way, the standard account states that infants point in “adult-like” ways when they begin pointing socially (imperatively and declaratively), meaning they are pointing to influence the mental states of others. However, is it the case that no important developments in pointing take place after this point? And is declarative pointing really the critical adaptation current researchers think it is? A few researchers have examined the development of pointing between 1 and 2 years of age (e.g., Moore & D’Entremont, 2001), however, not much attention is paid to this time period. This is likely due to the fact that, according to the standard account, any changes that take place in imperative and declarative pointing are only important in a quantitative sense because the important qualitative shift (mental state understanding and attendant social pointing) has already taken place. However, it may be the case that the categories are limiting how we think about social understanding.

If one forgoes the adaptationist or cognitivist redescription of the standard account, it is clear that the ways in which infants use pointing gestures constitute social understanding. If this is the case, then what happens before and what happens beyond the first instances of declarative and imperative pointing is important, and in and of itself, constitutes social understanding. In the following, I briefly expand on these issues using examples from the diaries, with the goal of suggesting the direction in which joint attention research should move.
The diary entries summarized below illustrate the development of gaze and point following. As will be seen, the development of gaze and point following has a long developmental trajectory. Yet, according to shared intentionality theory, being able to follow a pointing gesture follows from understanding that the other person wants (the infant here) to understand the intention behind the gesture (Tomasello et al., 2007).

Once again, here I expand upon an example already noted above, from Celine:

Celine (24 weeks). For the past several weeks, whenever the cat was around, I'd point him out to Celine and say "There's the kitty Celine" and make the sign for kitty. I do it with the dog as well. Today we were on the bed folding laundry (a common occurrence) and Udo (the cat) jumped onto the bed. I said "Celine, where's the kitty?" while doing the sign, but without pointing at him. She immediately directed her eyes toward the cat. I was shocked, and skeptical, so I did it a few more times over the next 15 minutes or so. I'd say 16/20 times, she looked right at the cat. Amazing!

Celine (29 weeks). We do the "where's the kitty?" thing daily, and she gets it right every time. She looks around and finds him, then looks back to me, as though for approval. She doesn't make any hand gestures, but definitely "points" with her eyes. She doesn't do this with the dog though. She doesn't seem to understand what the "doggy" is yet. I've also asked her where "Da Da" is, then I point at John and he waves, but no response yet with that one either. But, this does seem to indicate that she knows what "where's the..?" means.

In the next study I take a closer look at the relationship between point following and language directing speech, but from the example above we can see how language and gestures become meaningful within routines, in this case, within the "where is?" game. Such a scenario makes it difficult to interpret her following her mother's point as a function of mental state understanding. Rather, these are the very activities that give rise to understanding mental states that we attribute to linguistic humans. This may be the origin of mental state understanding but there is still a long way to go. It is not clear how proficient Celine is at following points outside of this "where's the...?" routine either.
Although it is argued that such routines would give meaning to this pointing gesture, Celine’s diary entries do not speak to this (i.e., they mostly concern infant pointing itself).

What happens past the age of one year also cannot be minimized by the declarative/imperative distinction. While social pointing is taken as the mark that infant understanding is qualitatively equivalent to adult understanding, the diaries suggest that pointing, and social understanding, keeps developing in important ways.

Allen (14 months): still points to things of interest but also now points to direct people to do things, i.e., points to his sister then to a toy he wants on table, also points when answering “where you want to go?”

Allen (17 months): points from me to where he wants me to look, he looks at me and points behind him when the person he is referring to went in that direction, he points to label objects in the book, he points to label objects around him and to label people too, he points to body parts, he points with his index up and places it close to his mouth (as if to say now pay attention this is serious!) when he is recounting an event of great significance such as “mama Eden down and bobo” and he will point down where Eden fell and point to his head to indicate where she got hurt all the while looking at her and me at the appropriate time.

At 14 months, Allen is more emphatic in his pointing, in that he points directly at other people. This is a kind of pointing where the communicative intention is unclear. It is not a proto-imperative point in which he’d be “requesting the adult” nor is it a proto-declarative point in which he is “wanting to share attention with the adult.” This point simply doesn’t have a clear communicative intention in this manner. It is more likely that pointing at other people is a function of his own attention on the person with whom he is involved, At 17 months, Allen uses pointing in many different situations. For example, he uses it in a narrative context in describing the way Eden got hurt.

Mitchell 12.75 months. Matthew has been working with Mitchell over the past week to wash his hands at the sink. Three days ago, I was holding Mitchell close to the kitchen sink and he started rubbing his hands back and forth. He would grab his left hand in his right hand and move his right hand along the left hand and then switch and do the same thing with his left hand. It looked like he was
washing his hands. I didn't understand at first what he was doing. Later, he did it again when Matthew was in the room and he said, "Oh, he is washing his hands." I then brought Mitchell close to the sink and he pointed to the faucet and made the sign again. I turned on the water and Mitchell put his hands under the stream.

Rather than pointing to demand an object, Mitchell here is pointing to demand an activity. He did not point to the faucet because he wanted the faucet, but pointed because he wanted to wash his hands. Although this can be classified as an imperative point, pointing for an action rather than an object seems to denote a more complex understanding about activity and interaction. The diaries have many entries that demonstrate new uses of pointing, such as pointing in response to questions like “what do you want to eat?” and “where do you want to go?” It is not that other social uses of pointing have gone unnoticed. Liszkowski, Carpenter, Striano, and Tomasello (2006) describe “informative pointing” as a use of the pointing gesture to direct the attention of others who are, for example, looking for something they lost. This formal recognition of a different communicative function of pointing is a step forward in appreciating how infants come to point in different ways. Focusing less on the imperative/declarative distinction can go some way into studying the different kinds of ways infants actually use pointing in social situations to get a better picture of the development of their social understanding.

3.9. Discussion

By looking at even a few diary entries and the different trajectories they show, Tomasello's so-called “social origin” theory to pointing development seems hard to substantiate, as is the theory of shared intentionality on these particular grounds. Despite any criticism as to the anecdotal nature of these accounts, these reports are still more useful than studies that are interpreted as supporting shared intentionality, as the studies that support it utilize little developmental data during the period prior to when infants become proficient pointers.

As previously noted, systematic reporting should be incorporated with ad libitum reports to minimize any biases in reporting. To this extent, checklists, regular visits from researchers collecting data, or regular video recordings that can be coded at a later time
can be useful. More systematic parental reporting can go a long way in minimizing any unwanted bias. For example, it has been shown that parents can provide reliable reports of attainment of infant motoric milestones using checklists (Bodnarchuk & Eaton, 2004). Of course, one has to balance attaining sufficient data with recognition of the burden of involvement in such projects to minimize attrition and maximize involvement.

Joint attention is the capacity for coordinated action. As such, it requires some understanding of attention, which infants gain when they discover that their pointing gestures are responded to by others and they learn that pointing has meaningful communicative uses. Not only do infants need to understand that their own gestures have meaning, they need to understand that others’ points are meant to be followed. This first study demonstrates the requirement of longitudinal observational data to properly conceptualize joint attention capacities and their development. In the following study, I examine the development of infant point following in 9-12-month-old infants.
4. The Development of Point Following

The previous section concerned largely the development of point following in infants. Not only has little attention been paid to how infants come to use pointing gestures, little attention has been paid to how infants come to understand the pointing gestures of others. The purpose of this study is to investigate how infants learn about the communicative function of adult pointing. In the following I provide a brief review of the extant literature on 1) the logically prior ability of gaze following, 2) point following, and 3) the link between attention following and language development. I then describe a study that investigates 1) how infant attention to adult communicative cues change between 9-12 months of age, 2) the role parental interaction may have in the development of point following, and 3) the relationship between parental interaction, point following, and language development.

4.1. Gaze-following

Gaze-following is a triadic social-interactive ability that develops early in infancy (see C. Moore, 2008 for review). It is a fundamental interactive skill that develops in the first year and is essential for later language development (Brooks & Meltzoff, 2005; Butterworth & Morissette, 1996) and social cognition (Camaioni, Perucchini, Bellagamba, & Colonnesi, 2004; Carpenter et al., 1998; Mundy & Newell, 2007). Gaze following is argued to be one of the earliest signs of mental state understanding (Bretherton, 1991), but exactly what it indicates about social understanding is less clear given that it is a capacity that is shared with other animals (e.g., Kaminski, Call, & Tomasello, 2006). Indeed, it is in the context of establishing whether or not gaze following, as in other form of joint attention, represents a form of social understanding involving mental state attribution that most gaze-following research is conducted (C. Moore, 2008).
Gaze following appears and develops between six and 12 months (Adamson & Bakeman, 1991; Butterworth & Itakura, 2000; Butterworth & Jarrett, 1991; Corkum & Moore, 1998; D'Entremont, 2000). Scaife and Bruner (1975) conducted the first systematic study of gaze following ability in infants. They found that 30% of 2- to 4-month-olds would follow adult gaze at least once during a two trial test in which an experimenter made eye contact with the infant, turned to one side and repeated the procedure on the other side. Gaze-following increased to 39% of 5- to 7- month-olds, 67% of 8- and 10- month-olds, and 100% of 11-14 month-olds. Collis (1977) clarified Scaife and Bruner's (1975) findings by analyzing the probability that infants were turning in the correct direction by chance and concluded that only infants 8 months and older were responding at levels greater than chance. Ten months of age accords with Corkum and Moore's (1998) findings that although infants can be trained to follow gaze at 8 months of age, they do not do so reliably until 10 months.

Gaze-following is not to be conflated with eye-following per se, as there is evidence that infants make use of multiple cues prior to eye direction cues and come to use these in conjunction with eye direction cues. It is not until the middle of the 2nd year that infants come to follow eye direction alone (Brooks & Meltzoff, 2002; Butterworth & Jarrett, 1991; Moore & Corkum, 1998). Prior to following eye-direction proper, infants rely on head direction as they interpret adult looking behaviour as a main directional cue (Brooks & Meltzoff, 2002; Corkum & Moore, 1995). Brooks and Meltzoff (2005) studied gaze following at 9, 10, and 11 months and found that 9-month-olds pay attention to body orientation rather than whether or not eyes are open or closed, whereas the older infants were more sensitive to such eye cues. Infants 12 months and older follow attentional direction when eyes and head are oriented in the same direction, but have difficulty when these cues diverge, even up to the latter half of their second year (Corkum & Moore, 1995; Lempers, 1979; Lempers, Flavell, & Flavell, 1977). Head movement appears to be an important directional cue as well. Moore, Angelopoulos, and Bennett (1997) found that 8- and 9- month-old children can be trained to follow gaze if they can see the motion of a head turn whereas they are not able to follow gaze with a

74 However, other lines of research using habituation paradigms have found that infants are sensitive to eye cues much earlier in infancy (e.g., Hood, Willen, & Driver, 1998).
static head orientation. Senju, Csibra, and Johnson (2008) found that 9-month-olds are sensitive to gaze direction and object location and are biased to attend to scenes with object directed gaze.

Infants become better at following gaze to specific targets between 12 and 15 months (Carpenter et al., 1998). By 12 months infants are able to follow an adult’s gaze past the first target in their line of sight instead of fixating on the first object they come upon (Butterworth & Jarrett, 1991; Morissette, Ricard, & Décarie, 1995), and will move around an object that is blocking their view of what an adult is looking at (Tomasello & Moll, 2004). By varying the angles and distances that targets were placed with respect to the infants, Morissette et al. (1995) found that infants improved in locating specific objects between 15 and 18 months of age.

4.2. Point Following

Infants begin to follow pointing gestures between 10 and 13 months (Carpenter et al., 1998; Desrochers et al., 1995; Leung & Rheingold, 1981; Tomasello et al., 2007). By 12 months, infants become highly adept at comprehending the pointing gestures of adults. For instance, they reliably fixate upon the targets of pointing gestures instead of the pointing gesture itself (Butterworth & Grover, 1988, 1990). Not only do 12-month-old infants understand the referential nature of pointing, they also understand the different communicative functions of pointing such as declarative pointing (Carpenter et al., 1998) and informative pointing (Behne et al., 2012; Liszkowski et al., 2006). They are able to follow points to both nearby and distal objects (Lempers, 1979).

Although point following in 12-month-olds has been extensively studied, there have been far fewer studies examining the development of this capacity. Churcher and Scaife (1982) studied point following in 3- to 8-month-olds using the procedure used to examine gaze following by Scaife and Bruner (1975). They found that infants looked at the experimenter’s eyes or pointing hand, indicating that infants did not understand the referential nature of the gesture. Similarly, Butterworth and Grover (1988, 1990) found no difference between the amount of time 6- and 9-month-olds look at their mother’s pointing hand and the target to which she was pointing. Infants in this age group have
not come to understand that a pointing hand represents a means of directing attention, and is not itself the target of attention.

By 9 months, however, infants have been found to have some point following capacity. Lempers (1979), for example, found that 9-month-olds could fixate on objects that were located between them and a pointing adult, but could not locate more distal objects. Murphy and Messer (1977) found that 9-month-olds can only reliably follow certain forms of pointing gestures made by their mothers. In a testing situation where they are seated beside their mothers, infants could follow their mothers’ points to targets on the same side of the room as her pointing hand but could not follow points directed forward and across their own bodies. By 14 months, infants are able to follow different forms of pointing (i.e., forward, away from, or across the infant) (Murphy & Messer, 1977).

Rohlfing, Longo, and Bertenthal (2012), using a habituation paradigm similar to Hood et al. (1998), investigated whether infants, much younger than the age at which they start following pointing gestures, are sensitive to pointing gestures. They found that 4.5- to 6.5-month-olds’ attention is shifted by a moving pointing hand, not a static one, and that they shifted their attention to the direction of the pointing finger only if the hand was moving in the same direction. The authors suggest that infants are prepared to orient to targets which likely contributes to their learning of the communicative function of points. Rohlfing et al.’s (2012) findings, much like the findings in gaze-following research, may point to the importance of object directed motion as an important cue in directing attention. Along a similar line of argument, Butterworth and Ikatura (2000) argue that infants may be able to more accurately follow points rather than head and eye movements because of the greater magnitude of lateral movement (in comparison to facial features) is more effective at redirecting attention. Butterworth and Ikatura (2000) state that although infants use head and gaze cues before they can follow pointing, they fixate upon the first object in their field of view, whereas pointing is more effective at directing attention to peripheral targets.

There are some differences in the literature between studies that had targets and those that did not. Scaife and Bruner (1975), whose testing paradigm has been used extensively in the field, did not contain targets. Targets can be rewarding stimuli for
infants, so to avoid conflating a rewarded behaviour with a response to a communicative bid, avoiding the use of targets is preferable. However, Morissette et al. (1995) argue that salient or distinctive targets are needed for the infant to shift their attention from the adult to the adult’s gaze direction. There is some evidence that procedures using targets elicit gaze following to a greater degree. For example, 12- to 14-month-olds are more likely to follow gaze when a target is present than when there are no targets (Butterworth & Jarrett, 1991; Caron, Kiel, Dayton, & Butler, 2002; Moore & Povinelli, 2007). Moore and Povinelli (2007) found that 24-month-olds were more likely than 12-month-olds to follow gaze when targets were removed but the adult continued to look where they had been.

Although there is much evidence that gaze following develops before point following, several studies have found the opposite relation (e.g., Butterworth & Grover, 1988, 1990; Lempers, 1979; Lempers et al., 1977; Morissette et al., 1995). Carpenter et al. (1998) found that point following emerged prior to gaze following for 13 out of 24 infants, and in the same month as gaze following for 9 out of 24 infants. It is somewhat difficult, however, to evaluate the relationship between these two joint attention skills based on the extant literature. Carpenter et al. (1998) note that some differences in the testing procedures (such as different positions and distances of targets between gaze-following and point-following tasks) make comparing results difficult. Differences in results in this research in general are likely due, in part, to different testing procedures, the types of gestures used by the adults, scoring procedures, location of targets relative to the infant, and presence or absence of distractor objects (Flom, Deák, Phill, & Pick, 2004). Carpenter et al. (1998) state that gaze-following and point-following to specific targets develops around the same time, between 12-15 months. Therefore the developmental relationship between these two joint attention skills is not completely clear.

4.3. Relation with Language

Joint attention has seldom been studied in and of itself, but rather in the context of its potential relation to later developing capacities, especially, language (Baldwin, 1995; Bates et al., 1975, 1989; Brooks & Meltzoff, 2005; Bruner, 1983; Butterworth &
Morissette, 1996; Camaioni, 2001; Desrochers et al., 1995; Iverson & Goldin-Meadow, 2005; Liszkowski et al., 2007). This focus is understandable considering Bates et al.’s (1975) seminal work explicitly examined the relation between joint attention and language, arguing that these early communicative gestures already represent the structure for verbal language use. Bruner’s (1975, 1983) emphasis on viewing language acquisition in terms of processes of communication rather than formal linguistics has also done much to influence developmentalists. This perspective emphasizes the continuity from prelinguistic to linguistic communication and also the importance of routines; interactive sequences that provide the structure for infants to come to understand intentional behaviour and communicative gestures. For example, infants eventually learn the linguistic equivalent of the waving gestures they acquired in interactions largely structured by their parents in meeting or departing contexts.

Bruner’s conception has led many researchers to focus on the mother-infant dyad and the nature of their joint engagement. For instance, joint engagement and infant vocabulary size is highly correlated (Tomasello & Todd, 1983) and joint engagement influences language acquisition rather than the other way around (Smith, Adamson, & Bakeman, 1988). It is during episodes of joint engagement in well-practiced routines that early symbolic gestures are acquired (Acredolo & Goodwyn, 1988). Within the context of joint engagement in routines, researchers have examined the relation between adults directing their infants’ attention and following their infants’ attention, and the relation to language acquisition. Bakeman and Adamson (1984) found that mothers whose language followed the attentional focus of their children in episodes of joint-engagement at 15 months of age had children with larger vocabularies at 21 months of age, whereas mothers who used attention directing language had children with smaller vocabularies at 21 months. Akhtar, Dunham and Dunham (1991) further distinguished between two kinds of directives in child-directed adult speech: lead prescriptives are statements that attempt to direct the attention of the child to a new object, and follow prescriptives which direct the attention of the child to a new aspect of the object the child is already attending to. They found that lead prescriptives at 13 months were correlated with having smaller productive vocabularies at 22 months, whereas follow prescriptives were
correlated with larger productive vocabularies at this age.\textsuperscript{75} Whereas past research had examined children’s language at 17 or 18 months and older,\textsuperscript{76} Carpenter et al. (1998) investigated individual differences in mother’s leading and following language and the relation to individual differences in children’s language development at an earlier age. They found that maternal following language at 12 and 15 months was positively correlated with word comprehension at 12 and 15 months, whereas maternal leading language was not related to word comprehension at any age. However, maternal following language at 9 months was related to infant vocabulary production at 13, 14 and 15 months.

4.4. The Current Study

Gaze following has certainly received more attention than the development of point following. There are several important insights, however, that can be drawn from the gaze following research that may be useful for studying the development of point following. Although “gaze” direction is principally associated with eye direction, the literature shows that gaze encompasses many directional cues, and, in fact, it is not until the middle to end of the second year of life that infants can follow eye direction alone. Cues such as head and body orientation, facial cues, as well as the overall natural dynamic movement of bodies provide the necessary structure for infants to become more adept at following the more developmentally complex, though perhaps more efficient cue, of eye direction. It is likely that such cues provide the context for the development of point following as well.

The gaze-following literature demonstrates that infants, gradually and over a long period of time, come to follow the eye direction of adults. C. Moore (2008) concludes in his review of the gaze following literature that the protracted nature of the development of gaze-following speaks against it being a simple insight (i.e., the insight model of

\textsuperscript{75} Carpenter et al. (1998) note that this relation may only exist early in language learning as older children become more adept at following adult attention in the second year of life and more able to attend to novel referents in an increasing variety of situations.

\textsuperscript{76} Eighteen months is within the range of when infants start to create sentences and undergo a vocabulary explosion (Colonnesei et al., 2010).
Tomasello, 1995) that others are capable of seeing things, and that it does not provide an obvious means of assessing social understanding in the sense of the original motivation for these sorts of studies. Rather, C. Moore (2008) argues that gaze following more likely reflects various aspects of social and cognitive development, and suggests that what is needed is a systematic program of manipulating the known extant variables to learn how infants come to follow gaze in various interactive and non-interactive contexts. It is likely that point comprehension follows a similarly protracted developmental course. This possibility is counter to the insight view that pointing is a manifestation of infants’ understanding of other’s mental states. Also, his call for a systematic program of study of gaze following could be implemented for point following as well.

Behne et al. (2012) argue that understanding the directing function of pointing is not the same as understanding communicative intent. Thus, they draw on Butterworth and Grover’s (1990) observation that point following often represents a geometric relation between the pointing gesture and an object. Behne and colleagues argue that this is a “simple” form of gaze and point following that needs to be distinguished from more complex intentional understanding. Once again, Tomasello and colleagues’ way of describing infant joint attention capacities is problematic and its empirical implications are contrary to the extant research. First of all, it is not clear if a separation between the attention directing function of pointing and communicative understanding exists or that any difference has been somewhat overstated. It is true that understanding communicative intent is more than being able to follow a pointing gesture, but point following develops in the context of communicative intention. One does not learn function of pointing and then learn that it is a communicative act. However, trying to make such a distinction more salient than it is may simply be convenient for their shared intentionality theory. This leads to a dichotomous view of development where a baseline capacity of understanding the attention of another or another’s attention directing gesture is transformed by shared intentionality to become a purportedly new capacity for understanding communicative intent. The dichotomous view of development is contrary to the extant research that shows the long and gradual development of gaze and point following capacities. Further, Tomasello’s research group concerns itself with infants’
point following abilities at 12 months of age, effectively ignoring the developmental history of the infants’ social understanding for its entire first year of life.

Based on the extant literature in gaze and point following, and contrary to shared intentionality theory, it is likely that infants develop a gradual understanding of others’ attentional states and communicative gestures. And due to the fact that the understanding of more developmentally complex communicative cues (e.g., eye direction, point following) seem to follow and be reliant upon earlier cues (e.g., head direction, object directed motion), it is difficult to see how “two forms” of understanding can emerge in the way implied by shared intentionality. Indeed, Thoermer and Sodian (2001) argue that an infant’s ability to follow the communicative gestures of others is not due to an “intentional” understanding separate from communicative context. It is this communicative situation that provides the pragmatic scaffolding in which communicative “intention” becomes clear. In this study I examine infant point following between 9 and 12 months of age. I also examine infant gaze to the pointing hand over this same time period to understand how infants use the manual cue in relation to their improving ability to follow pointing.

Following Bates et al. (1975), it is likely that infants are learning about attention through social engagement. It is expected that infants whose interactions with their mothers are characterized by more attention directing behaviours on the part of mothers will be more adept at following pointing at a later age. In this respect, it is hypothesized that maternal verbal and manual attention directing cues at infant age 9 months will be predictive of infant point following ability between 9 and 12 months. Because these interactional structures are the basis for language (Bates et al., 1975), it is expected that these same cues will be predictive of infant language development. Carpenter et al. (1998) did not find such a relation between maternal attention directing and language. Instead they found that maternal following language (comments upon what their infants were already attending to) at 9 months was predictive of infant language production at 13, 14 and 15 months. Because Carpenter et al. only found a relationship between maternal following language and infant language comprehension after the age range I am concerned with, I focus on infant language production, and expect, in contrast to Carpenter and colleagues, that maternal leading language (and maternal pointing) at 9 months will be predictive of infant language production at 12 months.
4.5. Methods

Participants: These data come from a longitudinal study that investigated the development of joint attention in infants (Racine, 2005). The subjects were 28 infant-mother dyads (14 boys, 14 girls) recruited by newspaper advertisements in the Greater Vancouver Area. Twenty-three infants were Caucasian and five were Asian. All infants were full-term and 18 were firstborns. The infants were all from two parent middle-class families. The infants were approximately 9 months of age in the first session and were assessed monthly at 10, 11, and 12 months of age. All assessments were videotaped. Parents were paid a small honorarium at the end of the study. Two infants (P17 and P27) did not complete all required trials and thus analyses are based on \( n = 26 \).

4.5.1. Early Social Communicative Scale (ESCS):

The infants were assessed using The Early Social Communication Scale (ESCS, Mundy et al., 2003). The ESCS is a videotaped structured assessment used to examine infants’ social understanding. The ESCS consists of a number of tasks used to assess such abilities as a) Initiating Joint Attention (IJA); b) Responding to Joint Attention (RJA); c) Initiating Behavioural Requests (IBR); d) Responding to Behavioural Requests (RBR); e) Initiating Social Interaction (ISI); and f) Responding to Social Interaction (RSI).

The ESCS was conducted monthly by the same two female experimenters in the home of each infant at 9, 10, 11 and 12 months of age. One experimenter conducted the assessment while the other completed a paper copy of the ESCS scoring sheet in real-time while observing the trials and interacting as little as possible with the infant. Two mini-DV camcorders recorded the assessments. One camera focused directly upon the infant while the other recorded both the experimenter and the infant approximately 45 degrees to the other camera. The infants sat on their mothers’ lap opposite to the experimenter. Mothers were instructed to interact as little as possible with their infants. The assessment took approximately 20 minutes (Mundy et al., 2003).

The current study involved the Gaze Following Task of the ESCS. Gaze following is an instance of Responding to Joint Attention (RJA). This task was administered twice per session. The trial begins with the tester bringing the infant’s attention to her face by
calling the infant’s name, tapping the table or touching the child, and then touching her own nose. The tester then begins a sequence of looking and pointing to different locations while emphatically stating the infant’s name.\textsuperscript{77} The tester then turns her entire torso to the location, uses a short-arm point (elbow touching her side) and states the infants’ name three times with increasing emphasis (i.e., Ava, Ava!, AVA!) with a second long pause between each iteration. The Gaze Following Task was presented once near the midpoint and once near the end of the assessment.

The videotapes were digitized and coded using the ELAN multimedia annotator program (http://www.mpi.nl/tools/elan.html). This program permits video tracking to the temporal resolution of one video frame (1/30\textsuperscript{th} second) and graphical waveform audio tracking to one millisecond. The footage from both cameras for each assessment was synchronized and displayed side by side in the annotator. The videos were coded to the frame that corresponds to the onset and offsets of the behavioural codes, reducing measurement error that could occur from real-time playback coding. The behavioural codes that were assessed in this study are listed below. Each code refers to a behaviour defined as physically and objectively as possible and labelled with a common sense description (i.e., Infant Gaze to Hand).

\textbf{4.5.2. Infant Codes:}

\textit{Infant Gaze to Experimenter (n = 26):} Instances in which the infant looks directly at the eyes or of the experimenter. Onset is defined as the first video frame that the infant looks at the experimenter’s eyes and offset is defined as the last video frame in which the infant is looking at the experimenter’s eyes (i.e., when the infant looks away from the experimenter’s face).

\textit{Infant Gaze to Hand (n = 26):} Instances of gazes to the experimenter’s hand exhibited by each infant. Onset is defined as the first video frame in which the infant looks at the experimenter’s hand and offset is defined as the last video frame in which

\textsuperscript{77} These locations are to the left, right, behind left and behind right of the child. These locations were to be indicated by posters in a lab setting but this was not possible in infants’ homes, nor are targets necessarily desirable (i.e., Scaife & Bruner, 1975).
the infant looks at the experimenter’s hand (i.e., when the infant looks away from the experimenter’s hand).

**Infant Point Following (n = 26):** Instances of point following exhibited by each infant. Onset is defined as the first video frame in which the infant shifts her gaze from the experimenter’s hand and offset is defined as the last video frame in which the infant looks in the direction of the experimenter’s point.

### 4.5.3. Free-play

Each dyad participated in a 15 minute free-play session designed to encourage gestural communication on the part of both the mother and infant. The dyads were provided with a set of 6 toys: colourful stacking cups, a doll, a puzzle, a picture book, a toy telephone, a rattle with moveable parts (Bakeman & Adamson, 1984; Carpenter et al., 1998). Three sets of similar but non-identical toys were used and were rotated each session such that on each dyad was not presented with the same set of toys on any two consecutive visits. The sessions were videotaped by two cameras at two different angles. Mothers were asked to encourage their infant to stay within the cameras’ field of view. Only the data from the 9 month free-play session was coded for this study. The behavioural codes that were assessed in the free-play sessions are listed below.

**Maternal Pointing (n = 28):** Instances in which the mother uses pointing gestures to influence the attention of the infant. These included proximate pointing gestures (touching the object or pointing to a nearby object) to distal pointing gestures (pointing to a far away object).

**Maternal Leading Language (n = 28):** Instances in which the mothers’ language attempted to redirect the infant’s attention.

**Maternal Following Language (n = 28):** Instances in which mothers’ language followed into the infants’ already-established focus of attention.
4.5.4. **Inter-rater Reliability**

The Gaze Following Task of the ESCS assessments was coded by the primary investigator. Because of the straightforward nature of this coding, i.e. counting instances of gazing to a pointing hand and instances of looking in the point direction and because ELAN permits analysis of video frames to milliseconds, inter-rater reliability was not established for this task and was instead focused on behavioural codes that were more prone to errors in coding, i.e., maternal leading and following language in the free play videos.

To assess inter-rater reliability in the free play videos, seven dyads (3, 4, 14, 19, 20, 23, 25) were randomly selected from the sample and were coded by an independent rater. Inter-rater reliability was assessed on a) Maternal Leading Language and b) Maternal Following Language. For Maternal Leading Language, the intraclass correlation coefficient was .985, 95% CI [.910, .997] and for Maternal Following Language it was .918, 95% CI [.523, .986]. The analyses show that there was a very high rate of agreement in coding maternal utterances. There was more variability in coding maternal following utterances, which was likely due to there being more following utterances overall and to the raters counting utterances in quick succession as single or multiple utterances, however, the utterances were still mostly categorized in the same way.

4.5.5. **Language Measures**

Infant communication was assessed using the MacArthur CDI Infant Form. The data were maternal reports of infants' comprehension (CDIUND12) and production of language (CDISAY12) at 12 months of age. Data for one infant (P12) was incomplete, hence data utilized for analysis is based on \( n = 27 \).

4.5.6. **Analysis**

**Point following and Gaze to Hand**

The first analysis examined infant gaze in the direction of a pointing gesture (point follow) and infant gaze at the pointing gesture itself (gaze to hand). Two
participants were excluded from the analysis (P17, P27) because they did not complete two trials of the point following task on one of the monthly testing sessions. The data from the remaining 26 infants was used for this analysis. Figure 1 depicts the mean number of times the infants looked at the pointing hand of the experimenter and the mean number of times they followed the pointing gesture of the experimenter during each monthly trial.

A one-way analysis with repeated measures on one factor (months) was conducted on the point following data. The analysis revealed a main effect for months ($F(3, 75) = 17.91, p < .001, \eta_p^2 = .417$). Six pairwise comparisons between the four levels were conducted ($p = 0.008$) to evaluate the source of the main effect for months. The main effect was due to a statistically significant difference between 9 and 11 months, $F(1, 25) = 18.72, p < .001, \eta_p^2 = .428$, and 9 and 12 months $F(1, 25) = 55.87, p < 0.001, \eta_p^2 = .691$, and 10 and 12 months ($F(1, 25) = 16.64, p < .001, \eta_p^2 = .400$). There were no statistically significant differences between 9 and 10 months ($F(1, 25) = 6.01, p = .022, \eta_p^2 = .194$), 10 and 11 months ($F(1, 25) = 8.44, p = .008, \eta_p^2 = .252$), and 11 and 12 months ($F(1, 25) = 4.82, p = .038, \eta_p^2 = .162$). Although there is a clear trend towards a significantly greater amount of point following at 11 months compared to 10, and at 12 months compared to 11, these comparisons do not survive a Bonferroni correction. A post-hoc power analysis further suggests that, given the sample size ($n = 26$), there is insufficient power to detect small effects.

A one-way analysis of variance with repeated measures on one factor (months) was conducted on the gaze to hand data. Mauchly’s test of sphericity revealed a statistically significant violation of the assumption of sphericity on the factor of months (Mauchly’s $W(1, 20) = 0.594, p = 0.031$). Because the analysis significantly violated the assumption of sphericity, the Geisser-Greenhouse correction was used. There was no main effect for months ($F(2.389, 59.716) = 1.091, p = 0.351, \eta_p^2 = 0.042$).

The analysis revealed that infants came to follow points at a steady rate from 9 to 12 months of age. Figure 1 clearly depicts the gradual development of this skill as point following increased geometrically during this period. There was no statistically significant change in gaze to the pointing gesture itself over this time, remaining at a relatively stable level through the four month period.
Regression for Point Following

Table 3 depicts the correlations between maternal language and pointing measures and infant language and point following measures. A multiple regression analysis was conducted to evaluate how well mothers’ bids to direct the attention of their infants (through leading language and pointing) at infant age of 9 months predicted infant point following from 9 to 12 months of age. The linear combination of the predictors was significantly related to infant point following $F(2, 25) = 4.285, p = .026$.

$^78$ Examination of outliers pertaining to the regression model revealed that the leverage value of Dyad 9 exceeded the acceptable cut-off [3(number of predictors +1/n)] (Stevens, 1992) suggesting this case should be further investigated in terms of having a potential influence on the model. However, the Cook’s distance value (.009) was well below 1 (Stevens, 1992) suggesting it does not have a significant influence on the regression model and can therefore be retained for the final analysis.
Table 3.
*Correlations between maternal language and pointing measures and infant language and point following measures*

<table>
<thead>
<tr>
<th></th>
<th>CDIUND12</th>
<th>CDISAY12</th>
<th>M_Point_9</th>
<th>M_Lead_9</th>
<th>M_Follow_9</th>
<th>Gaze_Hand</th>
<th>Point_Follow</th>
</tr>
</thead>
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<td>-</td>
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<td>-</td>
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<tr>
<td>CDISAY12</td>
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<td>-</td>
</tr>
<tr>
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<td>.315^a</td>
<td>-</td>
<td>-</td>
<td>-</td>
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<td>-</td>
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<tr>
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<td>.160^a</td>
<td>-</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>M_Follow_9</td>
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<td>.211^a</td>
<td>.061^a</td>
<td>.063</td>
<td>-</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td>Gaze_Hand</td>
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<td>-.063^a</td>
<td>-.016^a</td>
<td>.061</td>
<td>-.055</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
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<td>.211^a</td>
<td>.148^a</td>
<td>.328^79</td>
<td>-.136^a</td>
<td>.124^a</td>
<td>-</td>
</tr>
</tbody>
</table>

^p < 0.05, ^^p < .005.

^a Spearman's rho

Although this is a sizable correlation it is not statistically significant. The distribution violated normality; however, visual inspection of histogram approximates normality (nicely distributed with slight bimodality). To be conservative the non parametric measure is reported in the table, though it should be noted that with a parametric test, r = .520, p < 0.01
The multiple correlation coefficient was $R^2 = .271$, meaning 27.1% of the variance in infant point following can be accounted for by its relation to the linear combination of the predictors. Only maternal leading language was significantly predictive of infant point following ($\beta = .512, t_{24} = 2.787, p = .010$). Maternal pointing was not statistically predictive of infant point following ($\beta = .033, t_{24} = .178, p = .860$). Mother’s efforts to direct the attention of their infants, defined here as their use of pointing to direct their infants’ direction as well as directing attention through the use of language at nine months of age, accounted for a over a quarter of the variance in infant point following between 9 and 12 months of age. Moreover, this was largely due to the influence of maternal leading language, as maternal pointing did not have a statistically significant effect. With such a strong effect for maternal language, infants seem to be mostly responding to their mother’s comments rather than her pointing gestures.

Regression for Language Production

Language comprehension and production at 12 months of age was highly correlated ($r = .537, p = .003$). However, neither maternal language measure (maternal leading language or maternal following language at 9 months) was correlated to a statistically significant extent to infant language comprehension at 12 months ($p > .05$), similar to what Carpenter et al. (1998) found.

However, maternal leading language was significantly correlated with language production ($r = .432, p = .024$).80 A multiple regression analysis was conducted to evaluate how well mothers’ leading language and mothers’ pointing at infant age of 9 months predicted infant language production at 12 months. The linear combination of the predictors was significantly related to infant language production $F(2, 25) = 5.16, p = .014$). The multiple correlation coefficient was $R^2 = .31$, meaning 31% of the variance of language production was accounted for by its relation to the predictors. The two predictors had opposite effects. Maternal pointing was negatively related to a statistically significant degree ($\beta = -.510, t_{24} = -2.754, p = .011$), whereas maternal leading language was positively related to infant language production ($\beta = .467, t_{24} = 2.523, p = .019$).

80 Carpenter et al. (1998) found that maternal following language at 12 months was positively correlated with infant word comprehension 11-15 months.
This finding suggests that using language to direct infant attention, and using less pointing to direct infant attention leads to higher levels of language production at 12 months of age. More maternal leading language at 9 months leads to both more language production and point following at 12 months of age. Maternal pointing at 9 months does not influence infant point following at 12 months, and seems to have a negative impact on language production at this time.

4.6. Discussion

This study attempted to examine the development of point following, an important component of the ability to point, but one that has been relatively neglected in favour of infant pointing itself. First of all, this study examined how the ability to point changes between 9 and 12 months of age. As Figure 1 and the analyses shows, infants gradually improved in their ability to follow points at a nearly perfectly linear rate over the 4 month span. This finding is consistent with C. Moore’s (2008) gradualist, skill-based model of gaze following development, and contrasts with an “insight” explanation wherein joint attention skills stem from a mentalistic understanding of other people and their communicative bids. This study also examined infant gaze to the pointer’s hand. Previous joint attention research has largely taken for granted the attention directing nature of pointing gestures, hence little attention has been paid to how infants use manual cues in point following. In contrast to point following, infant gaze to the experimenter’s hand did not change over the same period. This finding demonstrates that infants look at pointing gestures at a relatively constant rate in this period in infancy. In one respect, one would not expect infants to look at the gesture any less over time, as in communicative interaction participants need to notice each others’ gestures in order to appropriately respond. Although rate of gaze to the pointing gesture did not change, there could still be changes in timing of behaviours that could constitute developmental change (Bibok, 2011). This study is a first step in the direction of such a microgenetic study. The possibility of such a form of development in point following is left open to verification in future research.

Based upon Bates et al.’s (1975) original conception of joint attention, it was expected that the more infants were exposed to having their attention directed by their
mothers (verbally and manually), the more adept they would be at point following. It was found that maternal leading language at infant age of 9 months accounted for over a quarter of the variance (at the aggregate level) in infant point following between 9 and 12 months. It is somewhat surprising that maternal pointing did not have the same relation. However, this result may point to the variability in maternal pointing. In this study maternal pointing was taken as a global measure. However, pointing was often employed in many different ways during the play sessions. Most importantly, pointing was often used in conjunction with both maternal lead and maternal follow utterances. Hence, it could be that due to the variability in the uses of pointing gestures by mothers, there is no clear connection to attention directing, or at least not as clear as maternal lead language seems to be in this analysis. Such a result is important because it again emphasizes the importance of examining infant interactional histories and draws attention to the perhaps non-obvious pathway joint attention development takes. In this case, in particular, how mothers verbally direct the attention of their infants influences infant point following, rather than influencing infant point production.

This study also examined the relation between maternal interactional factors and language development. Bates and most other joint attention researchers are in agreement regarding the connection between joint attention and language development. However, as I argue below, some researchers may conceive of the nature of language in different ways. In this study, it was found that maternal leading language at 9 months was predictive of infant language production at 12 months. It was also found that maternal pointing at 9 months bore the opposite relationship to language production at this time.

These findings are somewhat opposite to that found by Carpenter et al. (1998), who found that maternal following language at 9 months was predictive of infant language production at 13-15 months, though the relation they found occurred later than the one found in this study. Maternal following language is thought to influence infant language production due to the fact that mothers’ language pertains to what their infants are attending. In this perspective it is assumed such language consists of descriptive comments that presumably facilitate word learning. However, without qualitative analysis of the content of such language, what remains is a potentially contentious model of vocabulary acquisition where infants learn words through mapping them onto objects.
From a perspective that is based on that of Bates and Bruner, language is intrinsically linked with interactional structures, i.e., it is an aspect of interaction, and infants acquire language in conjunction with their social understanding of others in interaction. Although words can be learned through mapping words onto objects, this constitutes a limited sample of language (i.e., objects) and, moreover, the ability to label objects presupposes some early but already relatively complex social understanding. In this respect, it is not surprising that maternal following language is related to language ability at older ages. For example, Bakeman and Adamson (1984) found that maternal following language was related to larger infant vocabularies at 21 months (with the opposite relation found for maternal leading language). However, such a conception of word learning does not speak to how language is attained from its very beginnings. It could very well be, then, that maternal leading language plays an important part in language development, as the current finding seems to indicate. This study also attends to an earlier time period than most studies examining joint attention and language. However, if infant pointing reflects an already linguistic structure as Bates et al. (1975) claim, then examination of earlier time periods, such as the one studied here, is warranted.

Finally, maternal pointing at 9 months was negatively related to infant language production at 12 months. Although it was assumed that infants would learn more about attention through engagement with a mother who pointed more, the results do not support such a supposition. Why this is the case is not clear. One likely reason may be that mothers who point a lot are also generally sensitive to needs of their infants, such that they may be so attuned to their infants’ gestures that it decreases the need for infants to communicate verbally.

This study examined point following and language capacities for a short period relatively early in infancy. Ideally this study could be extended over a longer time frame, which may clarify some of the relationships found (or not found) in this study. For example, it is likely that maternal following language is important in language development in older infants, but at this age range, such a relationship does not seem to exist. The relationship between maternal pointing with infant point following and language development could be clarified by a detailed classification of maternal pointing gestures. Given that maternal pointing is often employed in different gestural manners (e.g., proximate versus distal points of various duration, touching objects in conjunction

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with leading and following language, and even other attention directing gestures such as sweeps of hand and head nods or “points”) and for different communicative functions, there may not be enough variability in each manner of employment to make such links. The categories of maternal pointing would likely have to be kept to a reasonable number and stands as an interesting question to examine in future research. The inverse relationship between maternal leading language and infant language production needs to be examined as well, perhaps by examining more closely the nature of the mother-infant relationship in terms of how sensitive mothers are to the communicative gestures of their infants, or by how much they encourage their infants to make linguistic communicative bids. Mothers who are more sensitive and less demanding may have children who score lower in language production as these infants do not need to use language to fulfill their needs or direct the attention of their mothers.
5. Conclusion

It has been 37 years since McCall (1977) surveyed the state of developmental research, which, over the 20 years prior to his review, had undergone substantial growth as a discipline. In it he bemoaned the state of the science, particularly the use of inappropriate experimental methodologies that do not provide the adequate means of studying development. Despite the apparent success of developmental psychology, McCall (1977, p. 333) suggests:

that, at present, we essentially lack a science of natural developmental processes because few studies are concerned with development as it transpires in naturalistic environments and because we rarely actually collect or analyze truly developmental data. This problem is believed to derive from the veneration of manipulative experimental methods, which have come to dictate rather than serve research questions.

It is dismaying to know that almost four decades have passed since McCall’s review and little has changed. I have come to a similar conclusion in this dissertation. As I have shown, experimental methods work more often with adaptationist assumptions, not developmental ones. And surveying the recent claims of leading joint attention researchers, it does not seem like things will change any time soon. For example, Seed and Tomasello (2010) provide their predictions for the next 30 years in primate research and suggest that this research will entail a cognitivist framework. As I have shown, cognitivist and adaptationist assumptions often go hand in hand, and if this research is conducted under the auspices of these assumptions, then the next 30 years looks very bleak for primate research as a developmental science.
As I have noted, many researchers make claims that are consistent with the tenets of both a development and non-developmental evolutionary metatheory. Even Tomasello, whose perspective I criticize to a large extent in this dissertation, notes the importance of understanding adaptability:

To deal with everything from the Arctic to the tropics, humans as a species have evolved a highly flexible suite of cognitive skills. But these are not individual cognitive skills that enable individuals to survive alone in the tundra or rain forest, but rather they are social-cognitive skills that enable them to develop, in concert with others in their cultural groups, creative ways of coping with whatever challenges may arise. Humans have not only skills of individual intentional action and cognition but also skills and motivations for sharing intentions and cognition with others. (Tomasello & Herrmann, 2010, p. 7)

Although they have identified the concept that is crucial for understanding human evolution, I do not believe this explanation is adequate on evolutionary and developmental grounds. An adaptationist view that reverse-engineers psychological capacities and attributes their development to specific adaptations is what we need to move beyond in psychology. In this dissertation I have tried to explicate the problems with this perspective, and the direction developmental psychology needs to go to make progress as a developmental science.

Psychologists need to take a broader view of evolution; to understand that looking at the products of evolution (i.e. our capacities) cannot in and of themselves tell us about their evolutionary origin. In general, psychologists would do better by taking a pluralistic approach in their evolutionary explanations. This entails understanding the difficulty with the term “adaptation,” recognition that human capacities are better characterized by exaptations, appreciating the limited nature of evolutionary explanation (i.e., the missing details of species’ histories), and recognizing the need to rely on multiple lines of converging evidence from various fields (e.g., anthropology, paleontology, genetic research, primate research, evolutionary developmental biology, and psychology, among others). Psychological research needs to be grounded in the evolutionary, ecological, and developmental context of the species in question rather than in adaptationist assumptions, and in this respect, more naturalistic and longitudinal
methods, along with cross-sectional experimental methods, should be employed when appropriate.

I have suggested that a major problem with the evolutionary metatheory of the biological sciences, the Modern Synthesis, is that it does not address development. Although this may not pose a problem for all areas of biology (e.g., population genetics), it poses a significant problem for psychology, a discipline where understanding development is paramount. Evolutionary metatheory informs psychology’s conception of development. This is a problem because the Modern Synthesis does not speak to the connection between evolution and development, and hence we see the persistence of “old” adaptationism in psychology: a problematic view where development is causally connected to naturally selected adaptations. I have suggested that a developmental evolutionary metatheory is needed in psychology to provide sufficient models of development and to clarify the complicated connection between development and evolution. In lieu of a formal metatheoretical shift, psychologists need to recognize the limits of neo-Darwinism and rid themselves of strong adaptationist thinking as the biological sciences did post-Spandrels. Even without the formal metatheoretical shift these disciplines were able to make significant progress within the boundaries of neo-Darwinism. In psychology there is little recognition of these boundaries, and, as I have argued, very little appreciation of the metatheoretical role neo-Darwinism plays at all.

The inherently developmental nature of psychological phenomena is recognized by some psychologists and this has led them to see beyond the blinders of a non-developmental evolutionary metatheory. I have mentioned some of these researchers. They take a truly “developmental” (i.e., recognizing the evolutionary, developmental and ecological factors) approach to the study of psychological phenomena. Even researchers who simply conduct good developmental research, I argue, by virtue of avoiding adaptationist assumptions, are producing work that is compatible with a future developmental evolutionary metatheory. What is clear is that “developmental” research conducted under the auspices of strong adaptationist assumptions provides at best pseudo-developmental accounts that are only consistent with adaptationist and cognitivist frameworks, not the developmental framework I have argued for in this dissertation.
Despite increasing attention to the inadequacy of adaptationist assumptions in psychology, strong adaptationist assumptions lie deep in the discipline, informing methods and theories, and change will be hard to come by. This is especially so when strong adaptationists are so invested in the metatheory that, despite claims to the contrary, they simply do not understand the cautions of Williams, Gould, and Lewontin, among others. Other disciplines in the life sciences heeded these warnings: psychology did not. Evolutionary thinking will change in psychology. It will have to. It will move beyond invoking particulate and additive “just-so” adaptations. A formal developmental evolutionary theory would likely shift research toward more fruitful areas of understanding our capacities (i.e., our adaptability) wherein heterochrony, a key developmental mechanism, can lead to a better understanding of the development and evolution of our adaptability. This, combined with an overall pluralistic approach to evolutionary explanation, is the future of our evolutionary and developmental studies. The only way that psychology will ever fully unite with other biological disciplines is under a developmental evolutionary metatheory.

I focused my critique largely upon developmental work in early social understanding, namely research into joint attention. In the studies I presented, I attempted to take a developmental approach to studying the development of pointing and point following in infants. The aim of Study 1 was to extend my conceptual critique of adaptationist “developmental” research programs to an empirical level. Whereas these latter accounts utilize experimental methods after development has taken place, I employed a different method to examine development as it occurs. Although these records are sometimes criticized for their potentially biased and “anecdotal” nature, studying pointing infants at one year of age, as these critics do, tells us absolutely nothing about the development of joint attention. These data demonstrated that infants use pointing gestures for non-social uses prior to social uses which is contradictory to an adaptationist explanation where pointing stems from pro-social motivations and is, a priori, social in nature and origin. These data also problematize the adaptationist assumption of consistent development, where it is clear that although infants may be pointing for social purposes by one year of age, infants have different trajectories in reaching this outcome. Further, I have shown that the first instance of a behaviour is not appropriate for the attribution of a capacity as there is a lag between the first time an
infant demonstrates a capacity and when they begin to employ it consistently. This demonstrates that skills develop within activity, and contrasts with popular views such as Tomasello’s where cognitive capacities are thought to underlie and organize activity, thus assuming consistency in action (e.g., Tomasello & Carpenter, 2013). And, finally, I showed that categorizing pointing gestures as declarative and imperative works for strong adaptationists as these are seen as different adaptations. However, such categorization downplays the significance of the social understanding infants demonstrate when they employ either category of points in increasingly complex ways. It is clear that a diary methodology in studying the communicative development is highly valuable. Supplementing these methods with check-lists, video-footage, and quasi-experimental methods conducted by parents and/or an experimenter are ways of extending this research to gather more details for a closer analysis of the means by which infants come to use pointing gestures come to be a meaningful component of activity.

In Study 2, I examined point following, an aspect of joint attention that has received less empirical attention than infant pointing. I found that infants, from nine to 12 months of age, gradually improved in their point following, in contrast to an adaptationist “insight” view of the development of social understanding. Much is taken for granted in this view, including the attention-directing nature of the pointing gesture itself. However, infants have to come to learn that one of the functions of this gesture is to direct attention. I found that infants looked at the pointing gesture consistently throughout this period, and suggested that it may be the timing of gazes that changes rather than rates of gazes. I also found that mother’s verbally directing attention was related to infant point following ability and linguistic production in infants. Maternal pointing was not related to point following ability and was inversely related to linguistic production. Exposure to pointing does lead to more adept point followers, but it was suggested that the variability in mothers’ pointing could be the reason no relationship was found. It was also suggested that infants with mothers who point more may have more sensitive mothers who attend to their needs, creating a situation where they are less likely to use words by 12 months of age.

This dissertation is the product of many years of confusion on the part of its author associated with trying to make sense of the claims made in developmental
psychology. It has been a difficult task, trying to interpret what I view as a myriad of conceptually and empirically contentious claims. I presented my argument that I am, much to the delight and surprise of myself (and hopefully others), not the one who is confused. The only way I have been able to make sense of the confusion was to trace it back to the point from which it much of it stems, and from doing this came the distance I needed to see the entire playing board. This dissertation is a small contribution towards helping other developmental psychologists see the board too, and hopefully, echoing McCall (1977), it could help developmental psychology come into its own as a true science of development.
References


