

RESEARCH ARTICLE

Attachment and the archive: barriers and facilitators to the use of historical sociology as complementary developmental science

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Argument

This article explores historical sociology as a complementary source of knowledge for scientific research, considering barriers and facilitators to this work through reflections on one project. This project began as a study of the emergence and reception of the infant disorganized attachment classification, introduced in the 1980s by Ainsworth's student Mary Main, working with Judith Solomon. Elsewhere I have reported on the findings of collaborative work with attachment researchers, without giving full details of how this came about. Here, I will offer personal reflections arising from the process, and my work in what Hasok Chang has called history as “complementary science.”

Keywords: complementary science; developmental science; interdisciplinarity; collaboration; history

Introduction

Chang (2004, 2017) has argued that there are three kinds of benefits that may be gained for science from work in history and historical sociology:

- i) increasing scientists' critical awareness of the contingency of their present ideas and practices;
- ii) recovery of relevant ideas that have been lost with changing paradigms;
- iii) use of awareness of contingency, and recovery of ideas, to develop new hypotheses for testing (what Chang terms “extension”)

Yet Sumner has asked what a Changian approach would look like in practice, since above all it is not clear how one could ever actually enter the position of complementary scientist from the basis of work as a historian (Summer 2005). Gordin has therefore exaggerated, in a friendly manner, that whilst Chang may do complementary science, he is “still the sole practitioner” (Gordin 2014, 413). However, Viney and colleagues report a growing trend of British historians “embracing the role of critical collaborator” (Viney et al. 2015, 2; see also Whitehead et al. 2016; Callard et al. 2016a, 2016b). Rees has identified one facilitating factor for collaborations of this nature, in noting that “historians and sociologists of science are actually the individuals with most expertise in moving between disciplines” (Rees 2016, 451). To date there has been no published account by a self-identified Changian of how their work came about and how entry was gained for a two-way conversation with scientists. This reflects a broader situation in which, though there are exceptions, “accounts of” and “methodological reflections on interdisciplinary research have not kept pace with this flourishing of cross-disciplinary projects and agendas” (Bernini & Woods 2014, 603). The present article sets out to offer personal reflections on the process of using history

as complementary science in the area of developmental psychology. Naturally, a limitation of personal narratives is that observations are anecdotal, rather than based on a rigorously assembled body of evidence. However, personal accounts have been used to good effect in the past specifically in the history and sociology of science, where questions of process are particularly salient. (e.g. Mesman 2007; Smith 2013; Kontos, & Grigorovich 2018; see also Turner et al. 2018). Indeed, quite recently this journal dedicated a special issue exclusively to personal narratives by historians of science (broadly construed) (*Science in Context*, December 2013, volume 26). The wider issues encountered in the course of this work will be drawn out, as well as their potential bearing on other interdisciplinary projects in general, and on uses of history for complementary science in particular.

I will here be foregrounding my relationship with psychologists and the complex and at times fragile process through which trust and collaborations were built. However, to give context, I should highlight that this is something that has emerged out of my work on the topic; it was not an aim at the start. Nor has it been the main focus of my research or publications, which have generally been historical and sociological (see e.g. Duschinsky 2015; Reijman et al. 2018). These works have been written according to conventional academic norms of genre, which mandate little opportunity to discuss the actual nuts-and-bolts work of interdisciplinary research (cf. Vanderstraeten & Vandermoere 2015). To my mind, my main audience has been other historians and sociologists of science. However, I have also published results from the critical collaborations described below in psychology journals (e.g. Solomon et al. 2017). Neither my historical and sociological work nor the results of the critical collaborations will be in focus here. Instead, I will attempt to describe the barriers and facilitators I have experienced to engaging in two-way conversation with attachment researchers as a historical sociologist.

Attachment research

The field of science in question is the subdiscipline of developmental science known as attachment theory, originating in the work of John Bowlby and Mary Ainsworth in the middle of the twentieth century. Attachment theory suggests that children are predisposed to seek a familiar caregiver when hurt, anxious, or ill. Ainsworth identified individual differences in attachment using an observational procedure called the “Strange Situation,” comprising two brief separations and reunions between the child and their caregiver (Ainsworth et al. 1978). Subsequent research has documented moderate associations between individual differences in the Strange Situation and child behavior and mental health, even decades later (Groh et al. 2017). Attachment theory forms a major paradigm in research on infant mental health and remains widely influential as a model of relationships and emotional development. Attachment theory not only forms an international research program but has attracted a widespread clinical, policy and public audience, and has achieved renewed salience with the neurological turn in policy and public discourse about childhood (Wastell & White 2017). However, it is not an uncontroversial field of research. Criticisms have been raised over appeals to attachment by the political right and by governmental organizations who use certain themes from attachment theory to highlight the importance of the family whilst sapping the welfare supports that might help families who are struggling (Duschinsky et al. 2015a; Keddell 2017). Additionally, high-profile academic criticism from the 1970s to the present has questioned the standing of attachment research (Rutter 1972; Lamb et al. 1985; NIMH 2016, 95).

The origins of attachment theory have received extensive discussion by historians. Researchers have placed these developments within the broader history of psychology and of twentieth-century social science (e.g. van Dijken 1998; van der Horst 2011; van Rosmalen et al. 2016), the history of the child guidance movement (e.g. Stewart 2009), and development in the welfare state in Britain (e.g. Thomson 2013). There has also been a rich tradition of discussion of the relationship between attachment theory and postwar gender and parenting cultures (e.g. Birns 1999).

The popularity of the topic of early work in attachment theory for historians of science resides at least in part in the fact that “the theory offers a looking glass into the social foundations and effects of science; the function and logic of scientific controversies and disciplinary hierarchies; and the interrelation of descriptive and prescriptive scientific theories, scientific and popular discourse, and science and ideology all at once” (Ruck 2014, 410). A major contributor to this literature has been Vicedo (2011, 2013, 2017). In particular, Vicedo gives close and welcome attention to Bowlby’s debt to ideas from animal behavior research, and she situates Bowlby’s early academic publications and writings for popular magazines very well within the broader field of the history of scientific images of childhood. However, Vicedo goes on to develop an overextended critique of attachment research. Vicedo has repeatedly claimed that she is tracing the flaws of attachment research from “the mid 1950s to the present” (Vicedo 2011, 424). She argues that the paradigm is impervious to criticism, and highlights the fit between some of Bowlby’s ideas and conservative ideologies of motherhood. She regards this fit as the ultimate reason for the popularity of the theory given that, in fact, “the scientific evidence in support of attachment theory has been insufficient and deeply flawed” (Vicedo 2013, 238).

Yet Vicedo’s assumption that attachment research suffers the same unscientific basis from the mid-1950s to the present has been criticized by both historians and psychologists. Midgley (2014, 266) describes Vicedo’s account of attachment theory as “polemical” since it claims to debunk attachment research, but displays “lack of attention to the work of Mary Main and others in the last forty years” (Midgley 2014, 266). Vicedo’s account of the activities of attachment researchers always stops at 1978. Other than one obituary for Ainsworth from 1999, Vicedo excludes all mention of subsequent developments in attachment theory by Ainsworth’s students, or their students, and makes no citation of the field’s journal *Attachment & Human Development* or the field’s compendium the *Handbook of Attachment*. Missing the developments that have occurred with the second generation of attachment researchers, Vicedo’s history is therefore described by Midgley as “less interesting” and “outdated” (ibid., 268; see also Duschinsky et al. 2019).

Likewise other commentators have observed that Vicedo conflates Bowlby with attachment research, appearing unwilling to recognize that, as contemporary researchers have done, “one can jettison the idea of mother love as instinctual, as well as the fixation on the mother as the crucial attachment figure, without discarding all of the theory’s insights” (Plant 2015, 459; see Kraemer & Roberts 1996; Orbach 1999). Curiously, much the same overextended claims about the contemporary attachment research in Vicedo’s historical work can be seen quite widely in the social sciences too (e.g. Garrett 2017). These claims seem indebted to rather standardized assumptions of “critical” social science and theory in which psychological knowledge is regarded as homogenous in kind and function (Sedgwick 2003). Among the factors contributing to these assumptions, one of significance here is likely to have been the conditions of academic production, which separate critics in a variety of ways from the practices they are describing, obscuring internal differences in the object of study (Bourdieu 2000; Latour 2013).

In 2012, I began a project in the history of science looking at the infant disorganized attachment classification introduced by Ainsworth’s former student Mary Main in the 1980s in work with Judith Solomon at the University of California, Berkeley. In brief, disorganized attachment is coded on the basis of confused, disoriented, or apprehensive infant behaviors observed in the Ainsworth “Strange Situation.” These behaviors are heterogeneous, but in various ways are understood to represent disruptions of the coherent use of the caregiver as a safe haven when anxious or alarmed (Main & Solomon 1990). In community samples with relatively few adversities, around 15 percent of infants show a high degree of confused, disoriented, or apprehensive behaviors towards their caregiver in the Strange Situation. However, this increases to a majority of infants from families drawn from samples facing multiple compounding adversities, or where maltreatment of the child has been documented (Granqvist et al. 2017).

Disorganized attachment was situated by Main and Solomon as standing in contrast to the three “organized” attachment strategies: secure, avoidant, and resistant, in which attention and

behavior are coordinated in a coherent way in maintaining the caregiver's accessibility. The secure strategy entails direct interaction with the caregiver on reunion and the capacity to be readily comforted. By contrast, the avoidant strategy entails the coherent direction of infant attention and behavior away from the caregiver whilst retaining a degree of proximity; the resistant strategy entails displays of distress and frustration by the infant, keeping the attention of the caregiver and not returning to play. Individual parents are generally not informed of the classification the infant-caregiver relationship receives when the Strange Situation is used either in research or in clinical or forensic contexts, so there are fewer looping effects than in many other areas of psychology. In the influential hypothesis developed by Main and Hesse, the different disorganized behaviors express, to varying degrees, an infant's experience of conflict about approaching the caregiver for comfort, in the context of previous experiences of alarming behavior by that caregiver (Main and Hesse 1990; Hesse & Main 1999).

There is only a small correlation between an infant's disorganized attachment classification with one caregiver and her classification with another caregiver, suggesting that the classification largely – if not entirely – taps a relationship-specific experience. Researchers have identified a variety of alarming experiences that can increase the likelihood of a disorganized attachment classification. Subsequent research has confirmed that frightening, dissociative, or frightened behavior by a caregiver – which may be presumed to alarm the child – is associated with infant disorganized behaviors seen in the Strange Situation (Madigan et al. 2006). An important finding by Cyr and colleagues (2010) was that for infants from families experiencing five or more socioeconomic risk factors, rates of disorganized attachment are similar in prevalence to samples of infants known to be maltreated. Cyr and colleagues argue that the accumulation of socioeconomic risks leads to a disorganized attachment behavior in the Strange Situation by creating a chaotic, frightening, and distressing situation for a caregiver or caregivers. This predicament may evoke adult behaviors that, though certainly not abusive, nonetheless alarm the infant, such as frightened retreat, frustrated and erratic behaviors, or exhaustion bordering on dissociation. The exact processes involved are still matters of inquiry. However, rates of disorganized attachment have been found to decrease where interventions are offered that provide family support (Bernard et al. 2012; Granqvist et al. 2017).

Barriers and facilitators

History of science in the complementary mode is necessarily interdisciplinary. However, interdisciplinarity certainly need not contribute either to good history or good science, or what is accepted as such. Complex issues of power, epistemology, and culture – as well as potentially differences in the facets of reality itself that disciplines treat – have long been discussed as potential factors that make interdisciplinarity a complex endeavor (e.g. Mittelstrass 1993; Garforth & Kerr 2011). In their landmark book Callard and Fitzgerald (2016) offer an especially sustained and powerful look at the barriers to critical collaboration within interdisciplinary work, and also ways that interdisciplinary can prove an unhelpful or counterproductive aspiration. One important issue drawn out by Callard and Fitzgerald is the variety of forms of inequality of status between the disciplines. This hierarchy of status supports and is supported by disparities in available resources. Callard and Fitzgerald also point to serious differences in the epistemology and goals of historians, social scientists, and scientists, which make barriers for the coordination of common projects and joint attention to problems, and can result in projects that do not serve the interests of one or the other group. In turn, these factors undermine the conditions for mutually beneficial relationships, in which patience and trust can be built over time. Callard and Fitzgerald also express suspicion about the way that ideas of “mutual benefit” get constructed in interdisciplinary work, which can still be quite unequal or contrived. Moreover, even when these barriers are lowered, respective interests genuinely met, and “interdisciplinary” does actually prove a relevant and useful

aspiration, there remains an inefficiency and deep gamble to critical collaboration. Not least that the lack of clear tramlines mean that time and resources can get misdirected or diffused in ways that prove a hinderance or problem for some or all of the collaborators, sometimes producing subsequent feelings of regret. It therefore entails professional risk in the scramble of academic life, particularly for junior researchers (Klein 2010). This gamble has an important affective dimension: interdisciplinary is often exposing, like an unguarded emotion, as disciplinary tramlines provide established ways of asserting and hiding ourselves, and conventions for expressing our wish that things were otherwise in the world. It is like having someone watch us draw, for most untrained adults an uncomfortable experience.

In retrospect, I would identify four factors that, in the case of my work, lowered the barriers that normally block historians and sociologists from forms of interdisciplinarity felt to be mutually beneficial, and therefore capable of contributing to complementary science. These factors are: 1) Perceived problems in the reception and standing of the disorganized category; 2) My potential to offer “technical referred contributory expertise”; 3) Compatible philosophies of time between historical inquiry and attachment research; and 4) The distributed skills of colleagues. I will discuss these factors essentially in chronological order.

1. Perceived problems in the reception and standing of the disorganized category

Since the late 2000s, it had increasingly become clear to researchers (e.g. Main, Hesse, & Hesse 2011) that many clinicians and social workers had come to assume that: i) disorganized attachment reliably indicates child maltreatment; ii) that it is a strong predictor of later pathology; iii) that it represents a fixed or static “trait” of the child; and iv) that it is impervious to development or help. These assumptions by practitioners were regarded as consequential misunderstandings of the available research. On the other hand, questions had also arisen among researchers regarding the precise psychometric status of the disorganized classification, and specifically whether a category was the best way to approach the data (e.g. Waters & Crowell 1999; Fraley & Spieker 2003; Spieker & Crittenden 2010; Lyons-Ruth & Jacobvitz 2016; Solomon & George 2016; Groh et al. 2017). The stakes were higher than they seemed: attachment researchers have generally regarded the infant classifications as representing “natural kinds,” “carving nature at the joints” in differentiating the basic repertoires of human emotion regulation or dysregulation (Waters & Beauchaine 2003, 417). As such, debates about the psychometric status of one form of assessment were simultaneously a struggle over the fundamental nature of the human being and the goals of attachment research as a scientific endeavor in general.

There were a number of reasons for the emergence of these psychometric questions. One was the tide of a much wider concern across psychological science since the early 2000s to replace categories with dimensions in the interests of psychometric precision and statistical power (Fraley & Spieker 2003; Groh et al. 2017; cf. Waller & Meehl 1998; Kendler et al. 2011). Another reason for the rise of questions, even appearing in the authoritative *Handbook of Attachment*, about the status of the disorganized classification, and pressure for an approach to infant attachment pathology with greater granularity than a single encompassing category and as such with greater relevance to routine clinical practice (e.g. Lyons-Ruth & Jacobvitz 2016). A further factor contributing to questions about the disorganized category has been the gradual retirement of the generation of researchers trained by Mary Ainsworth. Part of Ainsworth’s legacy was a focus on categories as the best heuristic for conceptualizing the logic of individual differences. With the retirement of her immediate students, this position has become less taken for granted. A prominent case is that of Roisman and Fraley, who have argued that disorganized attachment should not be regarded as a distinct category. They propose that disorganization instead forms part of a dimension of infant anxiety about an attachment relationship. In 2011–12, Roisman assumed Alan Sroufe’s place as Director of the Minnesota Institute of Child Development and the Minnesota Longitudinal Study of Risk and Adaptation. This replaced a collaborator of

Ainsworth's and staunch defender of the categorical approach to infant attachment with a sceptic about the category as head of the most influential ongoing longitudinal study of attachment. Researchers from social psychology working on attachment, such as Mikulincer and Shaver (2016), have agreed with the dimensional premise of Roisman and Fraley, but proposed instead that disorganized attachment represents the ramification of incompatible approach-seeking and avoidant strategies for responding in attachment relationships.

However, the different critical discourses about the disorganized attachment category have coiled around one another without really connecting, in part because published discussion has tended to be buried in brief remarks in methods sections of papers rather than attracting focal attention. Though it also made it more controversial and sensitive, my project proposing to examine the emergence of the category and its reception made sense as worthwhile to researchers in the context of i) perceived problems in the reception of the category by practitioners, policy-makers, and publics and ii) emerging conversations among researchers about how best to conceptualize its precise nature. Given that attachment researchers knew that there had been historical work done on Bowlby, it also seemed fitting to them to have scholarship address one of the most influential contributions of the next generation.

2. Contributory expertise

A second factor that helped lower the barriers to critical collaboration was my relative fluency with the writings of attachment researchers, which formed an odd kind of relevant expertise for developmental science. My training is as a sociologist, and I was a graduate student of researchers who generally used historical methods and close textual analysis. By no means, however, do all texts reward such a treatment. Derrida asks: "What is it in a 'great' work, let's say of Plato, Shakespeare, Hugo, Mallarme, James, Joyce, Kafka, Heidegger, Benjamin, Blanchot, Celan, that resists erosion? What is it that, far from being exhausted in amnesia, increases its reserve to the very extent to which one draws from it, as if expenditure *augmented* the capital?" (Derrida 1989, 845). Despite their location within a scientific discourse, there are some works of attachment theory that have this quality of giving more to the reader the more one reads them. Personally, I attribute it to their attempt to capture within formal categories the love we may have for a caregiver who has, at times, rejected, abandoned or hurt us when we needed them, and the reasons a caregiver may have behaved in this way. These are questions that yield more the deeper one has capacity to enter into them.

To give one example: before the disorganized attachment classification had been formally introduced, Main and Stadtman (1981) reanalyzed Ainsworth's narrative records of mother-infant interactions observed throughout the infant's first year. They found that the extent of the caregiver's aversion to physical contact had a strong association with conflict behavior in the infant. This is an important finding because caregiver aversion to contact at home was found to be almost ubiquitous among infants classified as avoidant in the Strange Situation (Ainsworth et al. 1978). The findings from Main and Stadtman's studies appear to suggest that infants classified as organized-avoidant in the Strange Situation – a form of "attachment insecurity," but nonetheless generally considered relatively low-risk – are disproportionately likely to show behaviors listed in the Main and Solomon indices of disorganization. Social workers and clinicians have at times used observation of behavioral disorganization by an infant in the family home as sufficient reason to warrant concern about a family (e.g. Shemmings & Shemmings 2014). This practice appears to be based upon the incautious assumption that the behaviors specified by Main and Solomon in any setting are a stable "symptom" of a child's experiences of a frightening and potentially maltreating caregiver. Attachment researchers have noted that the attention I called verbally and in print to Main and Stadtman's forgotten finding has been useful (Granqvist et al. 2016): it has helped them warn social workers and clinicians of the risks in assuming that the same assessment can be used indiscriminately across contexts; and it has helped them take heed of the fact that the Main and Stadtman study was in need of replication.

Another case where historical work attracted interest from research psychologists was when I found in the John Bowlby Archive a number of finished-but-unpublished writings, primarily addressing the nature of trauma. These had been barely discussed by historians (an exception is van der Horst 2011), and were unknown to attachment researchers or Bowlby's heirs. The writings include a full monograph by Bowlby on psychological theory; reflections on his experiences working clinically with evacuee children; and a painfully candid retrospective account of what he felt were his main limitations as a clinician. There was also a manuscript speculating on the relationship between child mental health and later physical health, a short work of political philosophy delivered as a seminar series in the 1960s, and a long discursive essay drawing on legal theory in thinking about the implicit contracts made within families. I have published a historical article reflecting on these works in *Attachment & Human Development*, the home journal for empirical attachment research (Reisz et al. 2018); this article has been cited by subsequent experimental research (e.g. Peltola et al. 2019). And with the encouragement of the trustees of the Bowlby's estate, I have been working this year with Kate White from the Bowlby Center to bring these writings into print as an edited volume for Routledge.

From 2013 onwards, correspondence with some attachment researchers became regular. In particular, Judith Solomon, Mary Main, and Erik Hesse were hugely supportive of my work. At the start, they seemed surprised but also intrigued by the very idea that their work would receive sustained historical attention. However, over time my impression is that this sense faded, and instead they were surprised and intrigued by the past itself, sharing my excitement at ideas or research findings that became available again when studying their old and unpublished writings or data. In turn, their authority over the interpretation of the disorganized attachment classification, as authors of the foundational work on the topic, meant that I had powerful gatekeepers. There was a tendency for empirical researchers to regard historical scholarship as decorative at best for the real work of science. But there was another tendency to regard history as powerful and dangerous. In negotiating these tendencies, Main and Solomon were important advocates for me with colleagues. This was entirely despite the fact that my publications tended to have, in part, a critical and objectifying aspect, more often displeasing than pleasing for the researchers under discussion. The situation was likely helped by the fact that, as well as this critical aspect, my writing also conveys some sense that I want attachment research to be the best that it can be, rather than agnosticism or scepticism about the whole enterprise. Though naturally there are significant disagreements about what "best" actually entails, the basic perception that I am at least well-meaning has helped lay the ground for trust. I suspect that this has been more about tone than about the content of what I write (see also Mykhalovskiy et al. 2019).

Slowly over years, conversations about the history of research on disorganized attachment developed out into open-ended conversations with a growing number of attachment researchers as they pursued thinking about theory specification and development. Some researchers have come to call on me as one point of access to the field's memory. Though by no means has everyone been welcoming. I have experienced disciplinary boundary-policing from some researchers; some are evidently dismayed that – without "earning my spurs" through developmental scientific research – I have any standing with their peers. I suspect that this has been especially the case among those experimental researchers with less interest to begin with in the philosophy of science and related questions. However, additionally, other relationships were soured at points by my presumption or naivety in conversation, and others soured by poorly judged evaluations I made of research or theory. To take one example: in an oral history a few years ago, I made an outright error in discussing an aspect of Ainsworth's research, and I then misunderstood the nature of a methodological innovation made by my participant due to lack of statistical knowledge. Towards the end of the interview, I asked a question based on work by another researcher, without awareness of subsequent failed replications. My participant was polite and professional, offering his knowledge to me as an evident outsider. However, he has subsequently seemed, in good faith, genuinely baffled as to why his colleagues have been interested in further conversation with

me or sometimes seek my views. To give a further example, another attachment researcher broke off contact after seeing my published evaluation of a study conducted by a colleague, which in retrospect was over-hasty. Over time, she has become friendlier but I think remains wary of me and of my work.

In the terms of Collins and colleagues (2016), my contextualist knowledge of the history of attachment texts has served as a flickering but real-enough kind of “technical referred contributory expertise”: an ability to contribute to a domain’s development, despite different training and appearing as an outsider, based on having knowledge understood as accepted currency in that domain. In my case, this was an ability to contribute to conversations about concepts in attachment theory and their uses (for instance the relationship between the construct of disorganized attachment and neurodevelopmental disorder). A point of comparison, and a role-model for my own work, is Sally Sheard. Sheard’s academic appointment is split between the Department of Public Health and Policy and the Department of History at Liverpool. Her scholarship as a historian of developments in British health policy and the role of the civil service became itself a form of technical referred contributory expertise in conversations with the Department of Health as part of the *History & Policy* network, and in her collaboration with the Chief Medical Officer (Sheard & Donaldson 2006). Possession of referred contributory expertise, however it is sourced, has the potential to reduce barriers to critical collaboration, and may be a prerequisite. In Sheard’s case, there are evident synergies between knowledge of the history of what has worked well and poorly in the history of health policy, and a government Department with a high rate of circulation of civil servants, reducing institutional memory.

3. *Compatible philosophies of time*

Klein (2010) and Kalela (2011) have observed that collaboration between disciplines depends upon their relative commitments and compatibility, and this relates to the wider question of in what way or ways, exactly, historical knowledge is useful or seen as capable of being timely. The importance of perspectives on time for how scientists appraise research possibilities has also been highlighted by Arpin and Granjou (2015). In this regard, another factor that reduced the barriers to collaboration has been that, despite their very substantial differences, history and attachment theory have points of overlap in their philosophies of time. Additionally, this concern with time is central to the “epistemic identities” of both fields, which makes mutual intelligibility and conversation easier (cf. Osbeck & Nersessian 2017). As Foucault (1970) and Steedman (1995) have noted, the constitution of history and the psychology of the child as modern disciplines have an affinity in their constructions of the past as both discontinuous with the present and as never fully over. Historical sociology, at least as I learnt it, and attachment theory, as I have encountered it, express this affinity in a number of ways. They agree that we make our homes on top of and within the ruins of our pasts, and that this shapes what we can build, where, and with what stability. A consequence is that both perceive that important aspects of our lives are often best regarded as byproducts of the past, rather than immediately functional and well-judged in the present – but that this byproduct can be used or adapted responsively, that contingency is material and runs deep. In making sense of this contingency, both disciplines have significant ontological commitments that emphasize the social basis of the self, and the effects of this for the knowing subject. Whereas similarity or difference in methodology or attitudes towards “positivism” are often assumed to be decisive in organizing relationships between disciplines, my experience has been that a shared philosophy of time has been more significant.

These compatibilities were an important bridge for holding conversations across epistemologies. I had been trained within a form of historical sociology that saw as its primary task the critique of assumptions within the practices under scrutiny. This could certainly include the evaluation and arbitration of knowledge claims, treating knowledge as a constituent feature and practice within the world rather than solely a reflection of it (see Jardine 2004; Baert et al. 2011). Such an

approach sits within a broader trend within historical epistemology towards consideration of the past specifically in light of the present and present concerns (Oreskes 2013; Weidman 2016; Loison 2016). A premise of this position is that close consideration of the discontinuous past is valuable because it can tell us about ourselves, and open up other ways of responding to the present – what Chang terms “extension” as an ambition of complementary science. Attachment researchers had an aligned position, but took an additional, different step. For them, consideration of the (developmental) past offers perspectives that can be tested as hypotheses regarding better ways of understanding and responding to the present.

My first direct dealings with this perspective were during an oral history with Judith Solomon. I had been asking about changes to her thinking over time regarding how cognitive maturation impacts behavioral disorganization. She offered her thoughts, laying out two different pathways that might be in play. She then asked me “well, what would you predict?” I was rather taken aback. I had certainly not expected to be asked for my perspective. Yet for Solomon, hypothesis-generation occurs out of a network of concepts that organizes lines of curiosity. For her, any question about attachment concepts and the shape of the literature, to the degree that these might specify the concepts more precisely or differently, was necessarily and obviously hypothesis-generating. The relationship between conceptual specification and hypothesis generation is an ordinary part of psychological practice (Smith 2005). From around 2013, I found myself in conversations about this ordinary work. My feel for the landscape of the published and unpublished literature was perceived as, though certainly as less valuable than an empirical study, potentially relevant. In turn, over the years a sense that my work could function as complementary science has changed the kinds of problems that I am drawn to. For instance, seeing my historical work as relevant to the conduct of future empirical study and clinical practice led to recent collaborative work with Mary Main, Erik Hesse, and colleagues on a published paper attempting more precisely to specify the relationship between disorganized attachment and autism spectrum disorder, which has been something of a thorny issue for researchers and child clinicians.

Additionally, conversations with Solomon and then also with other attachment researchers, have changed my experience of the distinction between the products of the sciences and those of the humanities. Historical sociology is an interpretive activity, concerned with understanding meanings and social practices, and aiming to analyze and defamiliarize its subject matter. However knowledge is itself a practice, and can be used to different ends (Nissen 2000). Historical analysis and defamiliarization of concepts or methods may also have relevance for experimental scientific activity, to the degree that the results of historical work seem to speak to a perceived question or problem, appearing as relevant to and constructive for “core business” for scientists rather than as a distraction. The key limiting factor is then the research capacity to concretely apply alternate perspectives within scientific practice, with time and resources always scarce.

4. The distributed skills of a team

Finally, a fourth factor that lowered barriers to collaboration was access to increased research capacity, in the form of receiving a New Investigator Award from Wellcome in 2014 to pursue research on the emergence and reception of the disorganized attachment classification. Interdisciplinary research is generally hard to find funding for, not least since reviewers tend to evaluate applications by discipline-specific values (cf. Bromham et al. 2016). By contrast, Wellcome seeks to fund research demonstrating a capacity to think beyond a specific discipline, and selection panels are generally multidisciplinary. Receiving this Award was an unusual opportunity, and has helped me personally and professionally in any number of ways. These have included supporting travel for meetings with Main, Solomon, and other attachment researchers; helping me move institutions to be closer to my family; and raising my standing among the research psychology community, for whom grants are, for good or ill, associated with status. Links to other Wellcome-funded researchers helped provide close-up access to models for

pursuing research between the humanities and sciences (e.g. Duschinsky & Macnaughton 2018). The feeling of being supported also gave me the hope and courage to trust in my sense of how collaborations could be pursued. Wellcome funding also supported a five-day conference for the researchers doing active experimental work with the classification, one output of which has been the Granqvist and colleagues (2017) “consensus statement” on disorganized attachment and its implications for clinicians and policy-makers.

However, by far the most important change made possible by the Award from Wellcome was the capacity to form a team with the skills needed for translating historical questions into complementary science publishable in psychology journals. Personally I lack the skills to take questions to a point that empirical results can be published in scientific journals. The initial group supported by the Wellcome Award has expanded further as other researchers have subsequently gained their own funding to work on the project. Most have prior training in applied disciplines: developmental psychology, clinical psychology, or social work research. The group has a distribution of skills and knowledge repertoires (Andersen 2016), though rarely does a work package require only one discipline. I find it rather remarkable (heartcatching if I’m honest, though this is difficult to say) to have such bright people committed because of, not despite, the uncertainties of this project, and the challenges of learning and combining methodologies. Part of the oddity of our group is that the usual “helpmeet” hierarchy (Balmer et al. 2016; Albert & Laberge 2017) between humanities and science in interdisciplinary work is reversed, with the consequence that work is conducted much more in the spirit of the open-ended quality of the humanities. I try to make the group a supportive commons of our difficulties and failures, so that no mistake feels irreparable. This includes several meetings every year – termed “surviving and thriving meetings” – dedicated to discussing how we might individually and together navigate the challenges of interdisciplinary work. However, working outside of disciplinary forms of valuation, I personally find the hiring process quite fraught. And I remain worried by the professional risk that these researchers have taken in joining the project, and the extra pressure placed by their need to simultaneously keep publishing in their home discipline in order to preserve credentials for a future labor market, and prospects of job security, largely organized by disciplines (Jones & Oakley 2018; Lindvig 2018). This is surely among the most important barriers to interdisciplinary working for early career researchers, and I have seen the impact it can have on colleagues’ confidence.

Some collaboration in thinking about the construct of disorganized attachment, at least with Mary Main and Judith Solomon, would likely have happened without the Wellcome New Investigator Award. And maybe some other collaborations would have come, just more slowly, and in a mode more complementary than scientific. It seems unlikely that I would have been in a position to co-author articles for mainstream developmental science journals. Without question, the chance to work with a team with distributed skills for bringing together history with developmental science has been catalytic for the development of an ongoing program of work in complementary science. The focus of this work has been to ask about the stabilization and use of the disorganized attachment classification, and to appraise the implications of research using this classification for practitioners and policy-makers. This has included asking new questions of secondary data already collected by attachment researchers, who have been generous in arranging access for us to their datasets, and sharing practical expertise in conducting analyses (cf. Larregue 2018).

Attachment and detachment

Much of my activity on the project has been edged with excitement, but also some fear of being outmatched, of not being up to the challenges of doing this work. One very significant aspect of this is the limits of my skills and training, for example to supervise adequately some of the work of my team, entailing the creation at times of quite large supervisory networks for doctoral projects.

Another aspect has been the percussive blasts that keep opening the project's scope, as unexpected developments occur, even when these are really good, such as finding Mary Ainsworth's papers from after her move from Johns Hopkins to Virginia, which had previously been thought lost by historians. These papers include her detailed longitudinal home observations, and her ethnographic notes from her fieldwork in Uganda. Yet, as Callard and Fitzgerald (2016) among others have identified, one of the most challenging aspects of collaboration as a historian has been the potential tensions between being critical and being useful, whilst also in a subordinate discipline. The role of critical collaborator has been, in my experience, one that has felt precarious, ever two bad decisions away from ending, rather than something settled. It is true that the role has, over time, been stabilized by the relationships that have developed since my first round of oral histories with attachment researchers in 2012–13. Yet these research relationships have had to withstand the accumulating clutter of my requests and mistakes and attempts at rectification. All relationships do, of course. However, the nature of my position amplifies this issue, not least the desire to offer critical perspectives. It is through asking awkward questions, I think, that I can be most useful. Otherwise the risk is that complementary science will decay into complimentary science.

A qualified detachment is helpful for constructive intervention (Anderson 2001; Fullbrook 2003). However, where detachment becomes uncoupled from attentiveness, as it easily can when there are barriers to immersion, the result can be overextended claims by historians to be “debunking” science. An example is Vicedo's debunking of attachment theory from Bowlby and Ainsworth to the present, without empirical consideration of the generations after these founding figures. And whereas Vicedo describes attachment research as impervious to criticism, in my experience attachment researchers have been amenable to criticism when it is informed and constructive. Without doubt there are attendant interpersonal, epistemological, and affective tensions, and sometimes the need for picking one's battles or playing the long game. I suspect that it helps that I evidently care about attachment research work and its effective reception, despite being an outsider. However, the already-existing interest in interdisciplinarity among many senior attachment researchers was also a relevant factor, in part a legacy of Bowlby's own construction of attachment theory from ethology, psychoanalysis, and cybernetics. This interest has facilitated a surprising degree of two-way engagement, including publication of a co-authored paper with Judith Solomon and the sociologist Monica Greco appraising the political commitments of attachment research and why exactly Deleuze and Guattari, highly critical of other established psychological theory, were remarkably favorable about attachment theory and treated it as an exception (Duschinsky et al. 2015b).

One of the primary forms of attachment-based intervention with families facing difficulties in relation to their parenting is video-feedback (Juffer et al. 2017). Researchers found that showing caregivers filmed exemplars of “ideal” parenting did not serve as a useful model, and instead lowered self-esteem and self-efficacy. However, for a friendly individual to show caregivers a film of their own behavior with their child, drawing attention to interactions in the film and what stemmed from these moments, had different consequences. This technique has been repeatedly found to have a powerful effect on the adult's behavior, improving caregiver sensitivity and the child's later mental health. Video-feedback has been a common frame of reference used by attachment researchers in understanding my work. The metaphor is one which highlights the acknowledged value of looking together at the recent past, with joint attention to how things occurred and what then ensued, as a basis for considering whether there might be other ways of acting in the present and future. It may be uncomfortable to undergo, but the intended tone of video-feedback interventions is resolutely constructive and encouraging. History is here envisaged as a screen or mirror for the present: always an interpretive activity, but nonetheless stable and common enough to permit joint attention and reflection.

The interplay of interdependence and independence, of criticism and contribution, is exemplified by my relationship with Mary Main and Erik Hesse. My research in their personal papers led them to decide to deposit their papers (320 boxes) with the Wellcome Library Archive, to sit with

the Bowlby Archive, available to the public and to scholars. I have a great deal of affection for them, fueled by the personal generosity with which they have treated me over six years. Not least, Main and Hesse have displayed an overall commitment to encouraging me as a researcher even when aspects of my work disappoint them or seem ill-judged. This support has extended to my research group, with sustained attempts made by Main and Hesse to help these early career researchers treat their research as an open-ended adventure. Though the demands on their time are intense, and I try to be reasonable, I often feel a desire to tow Main and Hesse in particular directions to attend to the things I am working on regarding the history of infant disorganized attachment. In turn they have demonstrated a desire to tow me away from fruitless and towards fruitful questions about their work, not least to really attend to how the infant construct relates to its correlates in other attachment assessments in later childhood and adulthood. Yet they have spoken to me of their priority that I retain a capacity to be critical of their work, with what they regard as a historian's proper objectivity. My guess is that we are each a bit worried and a bit reassured by our independence from one another, but pleased and surprised too by what this sometimes allow us to bring one another.¹ I have become familiar with their slightly-pained smiles as, for example, I lay out for them the history of how an imprecise phrase in one of their papers led to confusion and misapplications, and suggest some clarifications that could be made to researchers and clinicians. In turn, they will give terrific feedback on a draft of an article I have written about their work or that we are coauthoring, and then expect to see the next version, will give terrific feedback, and expect to see the next version, etc. This (at times very protracted) process has resulted in work that is more qualified and more useful than at the start, and on every occasion also *more* rather than less critical of their earlier work, especially as the extraordinary attention to detail at the heart of Main's contributions to developmental psychology gets turned on her own earlier thoughts.

Two examples can help illustrate ways in which I have attempted to conduct work that is both critical and useful. One has been efforts to help researchers on disorganised attachment learn from and respond to their reception. For instance, I arranged to interview Alan Sroufe, Miriam Steele, Erik Hesse, Mary Dozier and Pehr Granqvist about the meaning of the construct of infant disorganised attachment and its implications for clinicians and social workers in assessing and supporting families. Using money from the Wellcome Award, the interview was filmed and placed on YouTube, to compete with the many other videos already up that contain misrepresentations regarding the classification.² The difficulties in making this video critical, useful and succinct were later mirrored in the challenges of work to make a published consensus statement from the leading developmental scientists working with the classification (Granqvist et al. 2017). My doctoral students, Sarah Foster and Helen Beckwith, had conducted focus groups with practitioners working with children and families about their use of attachment theory, and were engaged in further vignette-based and Q-sort study of practitioner reasoning about attachment. These studies have identified the practical advantages of hazy appeals to attachment for professionals; this finding suggested a more precise and helpful model than merely regarding publics as having 'misunderstood' the work of experts (Pettit & Young 2017). Their empirical research was useful to Pehr and colleagues for thinking about how to effectively explain the potential relevance of the classification for practitioners whilst also cautioning against assumptions about the classification that diverged from the available evidence.

¹One small example: in an influential 1999 paper, Hesse and Main develop a theory about the implications of conflict between fear and other motivational systems. As something of an aside in the paper, there is a speculation about conflict between fear and aggression, attributed to "T. Johnson." Tucker Johnson was, it turned out when I asked, their family dog, whose dignity and intelligence made occasions when he displayed conflict behaviors stand out as especially visible to his humans.

²Infant Disorganized Attachment: The Key Questions, <https://www.youtube.com/watch?v=UY7hhVvKGOo>

A second example of my attempt to combine being critical and useful relates to Chang's (2004, 236) recommendation that complementary science "can seek to generate scientific knowledge in places where science itself fails to do so." Observational attachment data are relatively time-consuming and expensive to collect, with the result that sample sizes in developmental attachment research are generally not large and therefore underpowered for fine-grained analyses. For a long time this has limited possibilities for asking psychometric questions about the construct, with the result that questions of relevance to clinicians about the potential for different forms of disorganized attachment have seemed marginal to the field's core business. Yet the situation has altered with the availability of datasets of sufficient size, such as the Generation Squared longitudinal study led by Carlo Schuengel and Mirjam Oosterman. Lead researchers associated with these datasets have been interested in the questions raised by history as complementary science, and generous in developing collaborations to slowly explore the data together. My own, rather post-structuralist interest in the status and deployment of the disorganized attachment classification meshed with their concern with psychometric rigor in the articulation and use of psychological constructs. A tenet of their positivism is that if a question is testable, important enough, and data is available, then it should be supported and pursued if resources can be found to do so.

To give an illustration of such work at this odd post-structuralist/positivist interface: Sophie Reijman, a postdoctoral researcher in my group, and I conducted participant observation of four training institutes for researchers and clinicians learning to code the classification; I also conducted qualitative analysis of >300 cases of infants seen in the Strange Situation, from a variety of samples. Coding notes have to date been regarded as mere ephemera by the field, but from the perspective of the history of science they are classic materials (Lenoir 1998), and together with the observations of training for coders gave a good picture of the cogs and wheels inside the workings of the classification and the meaning ascribed to the different behaviors. Together these forms of qualitative analysis of the classification helped us develop a picture of the meaning of the different codes within the Main and Solomon indices and how they are used, and what is made visible and what gets hidden by the coding process (Reijman et al. 2018). Sophie is now exploring differences in the antecedents and sequelae of different behaviors in a large longitudinal dataset.

Conclusions

Chang identified three kinds of benefits that may be gained for science from work in history and historical sociology: increasing scientists' critical awareness; recovery of former ideas and showing their contemporary relevance; and identification of hypotheses for testing. Yet there are very significant challenges to achieving any of these goals, for instance in my work as a historical sociologist working in relation to attachment research. Barriers identified by Callard and Fitzgerald – among them inequalities of status, disparities in resources, differences in epistemology and goals, and limits to patience and trust within relationships – have been significant for my work and require daily thought and navigation. This article identified four factors that served to lower the barriers identified by Callard and Fitzgerald. At the point that my project began, there were perceived problems in the reception of the disorganized category, as well as emergent debates about how best to conceptualize disorganized attachment. If attachment researchers had perceived no problem, then they may well have given time to supporting historical work, for instance as participants in oral histories. However, their goals would have been more constrained, perhaps focused on ensuring that the field was represented accurately and, ideally, well. At most, they might have been receptive to the use of history to increase perspective-taking and critical awareness. However, my experiences would lead me to believe that when i) a perceived problem of conceptualization and/or reception for scientists and ii) technical referred contributory expertise from historical research is recognized, there is a strong basis for joint goals and collaborative

work. This would suggest that perhaps some priority might be given to differences in goals among the barriers identified by Callard and Fitzgerald.

Differences in method and epistemology, and especially the potential for haughty positivism from scientists, have often been treated as especially important in the literature on interdisciplinarity. This has not been my experience. This barrier has been dropped by the compatibilities in the philosophy of time between attachment research and historical inquiry, contributing to recognition of the technical referred expertise and the acceptability of recovery of ideas from the past. As a consequence, a critical study of the history of concepts and methods can be regarded as a potentially worthwhile activity by both parties. For me as a historical sociologist, this history provides a superbly rich case study relevant to major current concerns in my discipline such as the history of emotion in the human sciences, and sociological debates about psychological categorization. For attachment researchers, my historical work offers an unusually detailed – albeit sometimes uncomfortably objectifying, overpedantic, and strangely judged – appraisal of matters that are seeing or may warrant appraisal anyway. This has raised the standing of historical work from “sideshow, with the potential to harm perceptions of the paradigm” to “somewhat relevant, with the potential to be interesting, though still a reputational risk.” The outputs of historical research are certainly not as valued as the outputs of an experimental study on the same topic, but at best may be relevant to thinking about and interpretation of such studies and hypothesis-generation. Further barriers to interdisciplinary work put in place by inequalities of status and disparities in resources were lowered by the generosity of important gatekeepers. The Wellcome New Investigator Award took these barriers down yet further, especially by supporting the development of a research team with a range of skills and training. The barriers posed by limits to patience and trust within relationships have been an obstacle, and my naïve fumbling and outright mistakes at times have made this obstacle yet higher. But the experience of working together on joint projects has eased this barrier over time with collaborators, as the relationships have gained ballast against the strains put on them. Though dependent on the fragile, mutual and ongoing work of aligning goals and finding research capacity, it is these relationships that now form the primary support for sustaining my work in complementary science.

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