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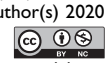

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On Kuhn's case, and Piaget's: A critical two-sited hauntology (or, On impact without reference)

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Abstract

Picking up on John Forrester's (1949–2015) disclosure that he felt 'haunted' by the suspicion that Thomas Kuhn's (1922–96) interests had become his own, this essay complexifies our understanding of both of their legacies by presenting two sites for that haunting. The first is located by engaging Forrester's argument that the connection between Kuhn and psychoanalysis was direct. (This was the supposed source of his historiographical method: 'climbing into other people's heads'.) However, recent archival discoveries suggest that that is incorrect. Instead, Kuhn's influence in this regard was Jean Piaget (1896–1980). And it is Piaget's thinking that was influenced directly by psychoanalysis. Psychoanalysis then haunts Kuhn's thinking through Piaget, and thus Piaget haunts Forrester through Kuhn. To better understand this second site of the haunting—which is ultimately the more important one, given the intent of this special issue—Piaget's early psychoanalytic ideas are uncovered through their interaction with his early biology and subsequent turn to philosophy. But several layers of conflicting contemporary misunderstandings are first excavated. The method of hauntology is also developed, taking advantage of its origins as a critical response to the psychoanalytic discourse. As a result of adopting this approach, a larger than usual number of primary sources have been unearthed and presented as evidence (including new translations from French originals). Where those influences have continued to have an impact, but their sources forgotten, they have thus been returned. They can then all be considered together in deriving new perspectives of Forrester's cases/Kuhn's exemplars/Piaget's stages.

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John Forrester (2017[2007]: 25) recalled, in 'On Kuhn's case', that *The Structure of Scientific Revolutions* set his direction in life. In that essay, though, he also sought to characterize the process of finding other selves, and other truths, in considering issues of identity: himself both an historian and a philosopher (and a lover of psychoanalysis), and Kuhn both an historian and a philosopher (and, Forrester argued, a kind of psychoanalyst too). These three tangled but non-identical threads—history, philosophy, and psychoanalysis—then needed to be separated. As he explained,

> Kuhn was an historian insofar as he functioned according to his psychoanalytic model of understanding [which he referred to as 'climbing into other people's heads'], and ceased to be one, becoming something which he called a philosopher, when he used another model. (ibid.: 31)

This first approach is now standard, historiographically, albeit without the psychoanalytic frame. History is historicist, rather than presentist. And the other model is, famously, the source of misunderstandings when moving across boundaries and ruptures: 'incommensurability' (see Kuhn, 1999, for an updated yet brief explanation).

Kuhn's influence, of course, was massive; well over a million copies of *Structure* have sold. Yet he also helped to shift historical narratives away from individual scientists who stand on the shoulders of giants (into whose heads one might wish to climb). Indeed, histories of science are no longer presented as heroic struggles by Great Men striving toward essentialized truths about a unitary world. Instead, the contemporary approach to historiography is very nearly sociological. This can be interpreted in different ways, some of which he found 'absurd' (Kuhn, 2000[1992]: 110).[1] But the narratives that result are typically about the group, in some way, rather than its leaders or their obsessions: how *whatever it was* came to be organized by or around socially mediated concerns or interests, and how this organization in turn affected its members' beliefs and actions in relation to their subject matter (both factual and interpretive).

This post-Kuhnian approach is now so widespread that individual names don't even mean what they used to. Instead of recalling people, names now stand in for the groups that identified themselves in relation to their joint interests and the external sources of power which shaped them. 'Pavlov', for example, no longer refers solely to the Nobel Prize-winning Russian physiologist. It also refers to the Pavlovian physiological factory, the people who passed through it, the replication and production and managerial processes they followed, and the funding sources and authorities that supported those efforts (Todes, 2002, 2014). Similarly, the contemporary meaning of 'Skinner' is more a function of popularization than it is of individual effort (Rutherford, 2009). As a result, too, the use of such a name can be contextualizing for others less well known; hence 'before Skinner' (see, for example, Rutherford, 2017). This is also the case for 'after Wundt', which itself often signals a higher-order meta-narrative about the Americanization of something that looked quite different in its original sources (following, for instance,

Blumenthal, 1977; Leahey, 1981; Tweney and Yachanin, 1980). So too does 'after Boring' implicate psychology's changing view of itself, having consequences for those who participated (for example, Kelly, 1981; O'Donnell, 1979; Rutherford, 2015; Winston, 1998).

In other words, the use of a name no longer implies that what follows is biography. Biographies exist, certainly, but these are usually less about the individual and more about that person's interactions with their context. Post-Kuhnian histories therefore aren't *reverential*; they're *referential*. And indeed it's these references that define the names themselves: who those people were—but as a reflection of who and what else they were with, how, and why—and reflected in their shared language (quantified by Green, Feinerer, and Burman, 2015a, 2015b). The resulting historical narratives then derive their meanings from those relations (Kuhn, 1970; see also Burman, 2017).

We see this approach reflected in Forrester's writings: *Language and the Origins of Psychoanalysis* argued that the entire psychoanalytic endeavour was not an offshoot of biology or neurology, but of linguistics and philology, and that its purpose was the examination of socially and culturally linked meanings in relation to the analysed self. *The Seductions of Psychoanalysis* examined the import of this approach for post-Freudian philosophy; *Freud's Women*, its and his relation to women and femininity, both personal and professional; *Dispatches From the Freud Wars*, the conflicting actions and legacies of different psychoanalytic interest groups; *Truth Games*, its relations with deception and value (viz. 'lying on the couch'); and, most recently, *Freud in Cambridge* presented a series of microhistories about the social life of Freudianism as it was transplanted into a new context where Freud himself never had a local presence.[2] In short: these are histories of meaning, and of how meanings change, with reference to various names that represent positions traceable to Freud without necessarily including him personally.

The challenge, in referential history, is what to do when an influence comes without a reference to which it can be attached. The result, in such cases, isn't then so much history as it is *hauntology*.[3]

We see something of this in Forrester's (2017[2007]: 26) reflections on Kuhn, as he said directly: 'I have increasingly come to be haunted by the uncanny sense that my problems are more akin to his than I myself, and certain others, would immediately recognize'. From this perspective, the case of Kuhn then provides more insight into the case of Forrester than the reverse (ibid.: 43–58). Yet in rereading Kuhn through Forrester, I nevertheless came to a new appreciation not only for how Kuhn influenced Forrester's thinking, but also for how he still haunts ours: the historical Kuhn sought to understand how certain truths could be understood to be rational, and accepted as such by the relevant expert-group (paradigms), while the philosophical Kuhn sought to understand how disagreements between otherwise rational positions could arise (incommensurability) and be overcome (through translation and language-learning). But this all then reduces to two issues with which both analysts and historians still need to engage: the choice of what to approach or avoid, reflected in method, and the overcoming of the remaining distance. It was then Kuhn's (1970) methodological revision of paradigm into the exemplar and disciplinary network—at the behest of critics including former Wittgenstein student Margaret Masterman (1910–86) and fellow Harvard alumnus Dudley

Shapere (1928–2016) (174, n. 4)—that most affected Forrester (2017[2007]: 45–6). Indeed, it's from this that he derived his view of the 'case' (ibid.: 49–51).

Following these reflections, Forrester's (2017[1996]) earlier approach to 'thinking in cases' (plural) can be understood as an invocation to consider the meaningful relations between *several* examples taken by the relevant consensus to be exemplary (that is, representative, characteristic, archetypal). The interpretive focus must then not be on a solitary world-spanning narrative afforded by a single case history, but on the plurality of worlds afforded by an entire course of analysis.[4] And so too must the analyst consider multiple ways of worldmaking, which Forrester did by invoking not only Kuhn but also Foucault, Wittgenstein, Winnicott, and others (see Bar-Haim, 2020). In the writing of *Structure* itself, though, these multiple perspectives were drawn from psychological—not psychoanalytic—sources.

Forrester (2017[2007]: 38) cited Kuhn's such influences as 'Piaget and Kant, Bruner and Postman'.[5] He then developed the Piaget connection further, and placed it in continuity:

> What started with the experience of the gestalt switch and incommensurability developed via the interest in Piaget's genetic epistemology and experiments in perceptual transformations into reflections on learning. . . . It then shifted again towards the philosophy of language, theories of reference and meaning, towards attempting to show how scientists could live in a different world after a revolution. (ibid.: 42)

He continued, while at the same time leaving the lineage of traceable references and coming instead to reflect his own:

> Whatever the complexities and changes over time of Kuhn's psychological orientation, the connection I think I have established is quite sufficient: Kuhn's most prized skill, that of 'climbing inside heads,' was linked to his personal psychoanalytic experience and remained his to the end. (ibid.)

The result is clear: for Forrester, Kuhn—and post-Kuhnian history—is psychoanalytic. However, a recent archival investigation suggests this can be complexified. It found that Piaget's influence on *Structure* was originally much greater than we now know, much earlier on (before Bruner and Postman), and that his presence also declined practically to invisibility as the manuscript was revised prior to publication.

This observation isn't mine. Rather, it comes from Peter Galison (2016), who noticed and realized Piaget's importance after a careful study of Kuhn's papers held in the MIT Archives (especially the 1949 notebook). Afterward, he expressed his surprise at the change across the different versions of the project:

> [Whence] Jean Piaget, in my view *the* central figure in Kuhn's early work, the model for stable, coherent, conceptual structures broken by periods of acute disturbance, where meanings become unmoored? Vanished with barely a trace, other than a brief reference in the preface. (ibid.: 63; original emphasis)

Of course, we can be psychoanalytic in explaining this: we could suggest that Piaget was *repressed* or *suppressed* or even *censored*. And indeed there's some supporting evidence for such an interpretation. For example: David Kaiser (2016: 79) noted Kuhn's reported anxiety about meeting Piaget at Berkeley in the 1950s. But to take this tack would be to climb into Forrester's head, rather than working to understand the original interests of Kuhn's that Forrester later felt came to haunt him. So we ought instead to approach the problem through Kuhn.

This isn't easy. Indeed, Alex Levine (2000) previously noted some of the potential pitfalls of a Piagetian reading of Kuhn. Other authors have also observed a bias in psychologists' readings of Kuhn more generally (see, for instance, Coleman and Salamon, 1988; Driver-Linn, 2003; O'Donohue, 1993). And I reviewed some of the connections between their published and unpublished works in response to an essay suggesting that Piaget could offer a 'remedy' for some of the problems in Kuhn (Burman, 2007; commenting on Tsou, 2006).[6] More than a decade later, such a focus continues to seem to me to be the way forward.

Still, I accept Forrester's (2017[2007]: 28) suggestion that psychoanalysis played a role in Kuhn's formation. He documented its presence in Kuhn's life, via his mother, friend, and son. Forrester also cited suggestive comments regarding Kuhn's failure at nail-biting (ibid.: 27), imagined neuroses (ibid.: 28–9), and remembered hatred of an analyst whom Forrester in turn called 'extremely effective' (ibid.: 30). In what follows, though, I will instead proceed down the investigative path suggested by Forrester's *endorsement* of Kuhn: looking to the primary sources, and—as he did, famously, in figuring out how to make sense of Aristotle after having just been trained to the doctoral level in a completely different physics—trying to understand these sources from within.

Quite significant, in this respect, is what Kuhn said in his presentation at Piaget's institute in 1966: 'it was Piaget's children from whom I had learned to understand Aristotle's physics' (Kuhn, 1977[1971]: 21). And perhaps less significantly: 'I am proud to acknowledge the ineradicable traces of Piaget's influence' (ibid.: 22).

Because archival research is *de rigueur*, I will also offer my own to supplement Galison's. Here then, from a letter that I found in an apparently unrelated collection, is Kuhn—before *Structure*—in his own words and without any of the pressure to perform in the above: 'I think there is no contemporary from whom I have learned any more than I have from Piaget' and 'it is the experiments that I have found most useful, even though my interest in them had derived from a desire to construct an epistemology of my own' (pace Forrester, 2017[2007]: 42; pace Levine, 2010: 375). He also went on to refer to what soon afterward became *Structure*: 'I am an old fan of his, have found many of his experiments extraordinarily illuminating, and actually now have a manuscript awaiting revision in which I contrast some of his results with a terribly important episode in the development of medieval and early classical dynamics' and 'If the manuscript that I was wrestling with last summer ever gets into a form that I dare show to people, you will be able to see better what I mean'.[7]

On the basis of such evidence, one might be forgiven for suggesting—in echo of Forrester—that it was *Piaget* who set *Kuhn's* direction. Indeed, as Kuhn put it himself in his 1966 talk:

> Almost twenty years ago I first discovered, very nearly at the same time, both the intellectual interest of the history of science and the psychological studies of Jean Piaget. Ever since that time the two have interacted closely in my mind and in my work. (Kuhn, 1977[1971]: 21)

This seems conclusive, especially in light of the emphasis that Forrester (2017[2007]) put on that period (and particularly the Aristotle episode). However, as Galison notes, it is also basically unknown to contemporary audiences. If what follows must therefore be a hauntology, rather than an intellectual or philosophical history with extra steps, then that is the first site of the haunting. The second site comes through the influence of psychoanalysis itself. Yet while Forrester is clear about its influence on himself, and somewhat less convincing about its importance to Kuhn, this is not such an obvious thing to say about Piaget. Nor is it especially clear, at present, why one might want to say it. That he served for eight months, in 1921, as analysand to Sabina Spielrein (1885–1942) seems today—mistakenly—like little more than trivia (for details, see Vidal, 2001[1995]).[8]

Still, I do think that Forrester (2017[2007]: 42) was onto something interesting. It's just that he was wrong about the link being *direct*. And thus recognizing this two-sited haunting—Piaget of Forrester (through Kuhn), and psychoanalysis of Piaget (to Kuhn)—affords new perspectives that show us new things of all three. To excavate these positions sufficiently, though, will take some serious digging.

Climbing into Piaget's head

The French-speaking Swiss biologist turned experimental philosopher of children's knowledge-claims, Jean Piaget (1896–1980), is today one of the big names of contemporary psychology. For many decades, he has been included on lists of psychological 'eminences' (see, for example, Diener, Oishi, and Park, 2014; Haggbloom *et al.*, 2002; Heyduk and Fenigstein, 1984; Korn, Davis, and Davis, 1991; Myers, 1970). Of course, where he ranks on such lists is dependent in part on who you ask (Green and Martin, 2017). But he is always mentioned. Why? (As a result of what conditions did his name come to mean what it now means?)

Very briefly: after the unexpected Soviet success with Sputnik in 1957, the Americans were spurred on to seek out any ally or source that could help them win the Space Race—which is to say, gain Air Superiority (the High Ground) in the age of missile-based nuclear weapons—and thus also to win the Cold War. Following the passing of the National Defense Education Act (NDEA) in 1958, virtually unlimited resources were liberated by Congress and directed toward figuring out how to produce scientists, technologists, engineers, and mathematicians at a rate surpassing their production in the USSR (Urban, 2010). It was then through this great need that Piaget's writings regarding children's intellectual development were popularized (especially by Flavell, 1963; Hunt, 1961; see the archival documents described in Müller, Burman, and Hutchison, 2013). And these popularizations ultimately revolutionized both psychology and education, in the American sphere of influence, with knock-on effects in adjacent disciplines (see, for instance, Murray, 1979).

Piaget's name thus became synonymous with those Cold War interests. In recognition of the spectre that came to haunt Forrester's thinking in cases, the resulting approach to cognitive development might also be called *thinking in stages*. Yet this can be made clearer: the American goal, via the NDEA, was to speed children's passage through those stages toward a career in a STEM discipline (especially after Bruner, 1960).

Piaget himself, though, didn't care about that; he dismissed it as 'the American question' (in Hall, 1970: 31). To many Americans' surprise, he also didn't identify as a developmental psychologist or even as a child psychologist. Instead, he called himself a 'genetic epistemologist'.[9] And his methods for collecting and interpreting the data that informed his stage theory had their origins in biology, philosophy, and—before he turned to psychology—the Swiss style of psychoanalysis.

This psychoanalytic influence was brief, but important. As Piaget explained its potential to the Binet Society, in December of 1919, during his earliest-known (published) psychological presentation:

> Psychoanalysis affords a quite advanced theory of unconscious development. Mental development, on the other hand, has been studied by metric methods (which I do not have to praise here at the Binet Society). But the correlation between these two developments is still full of mysteries. (Piaget, 1920b: 58)

This was just after he left the Zurich psychoanalytic circle to join Binet's former collaborator, Théodore Simon (1873–1961), for what we would now call a postdoctoral fellowship in psychometrics. And the combination can now be said to have been foundational for what came after. We can even see his presentation to the Binet Society, in retrospect, as a notice of intent regarding what became a decades-long investigation of mental development using a modified psychoanalytic interview. Yet the story of Piaget's origins usually omits that chapter.

Piaget himself was not especially helpful in telling this story: his autobiographical writings present an idealized version of himself and his theory, such that those who rely too heavily on these sources operate under a 'biographical illusion' (Vonèche, 2001: 226; see also Vidal and Vonèche, 1983; Vidal, 1994: 5–9). Thus, for example, in his contribution to the influential *A History of Psychology in Autobiography* series, Piaget (1952: 244) referred to his early methods as involving not *psychoanalytic* but *psychiatric* questioning.[10] Later, he also summarized his whole psychoanalytic period in one sentence:

> After my doctorate I spent several months at Zurich studying psychology with G. E. [sic] Lipps and Wreschner[11] and some psychiatry with Bleuler, but without making much headway. Then I left for Paris. . . . (Piaget, 1971[1965]: 9)

As a result of this diminishment—or, perhaps, of a *repression* to rival Kuhn's own—Piaget's psychoanalytic side is usually glossed over in even high-quality histories of his early career.[12] The dominance of idealizations and popularizations over primary sources then causes errors and exaggerations to continue to be repeated. (Such as the persistent 'Piaget wrote a "journal article" about an albino sparrow when he was ten' origin myth

[after Piaget, 1952: 238; by way of remedy, see Piaget, 2010[1907]].) So there's some work to do in clearing these away before we can get properly started.

Scholarly histories as 'mental equipment' for head-climbing

Just before citing Piaget explicitly as a psychological influence, Forrester (2017[2007]: 41) highlighted Kuhn's (1977[1964]: 263) suggestion that the shared perceptions of scientific communities are grounded in their 'mental equipment'. It's this that then both limits and gives access 'to information which is simultaneously at hand and yet somehow inaccessible' (ibid.: 261). And if what follows must be a hauntology of Kuhn, using a variation of his method that influenced Forrester, then starting by reference to such equipment seems necessary here too. (What do we know that we can use to help us climb into their heads?)

The best book in English on the subject of Piaget's earliest thinking is *Piaget Before Piaget* (Vidal, 1994). This took advantage of archival sources to investigate his early years in Neuchâtel, Switzerland, from his birth in 1896 to his activities surrounding the completion of his *doctorat ès sciences* in natural history (collecting and characterizing snails) in 1918. In other words, the book is about Piaget *before* the stage theory of child development that made him famous. Hence the titular 'before *Piaget*'.

Yet this first volume of the many yet to be written focussed primarily on Piaget's early biology. His turn to psychoanalysis occurred immediately afterward: he moved from Neuchâtel to Zurich in 1918, to Paris in 1919, and then to Geneva—where he did the work that eventually made him famous—in 1921. As a result, there's a lot more to the story.

Vidal's book was soon supplemented by a collection that was translated into English more than a decade later: *Jean Piaget and Neuchâtel* (Perret-Clermont and Barrelet, 2008[1996]; further documenting the 'Zeroeth Piaget' [Burman, 2011]). This expanded beyond his early biology to describe important additional details regarding his formative years, including unearthing the archival evidence of an unfinished second doctorate in philosophy.

Had this higher doctorate been completed, the associated archival papers would have provided a helpful philosophical bridge connecting Piaget's later interests in psychoanalysis and psychology with his earlier snail-hunting (viz. his work describing where to find which species, and his early speculations about what methods to use in determining species-membership at a time before the universal acceptance of Mendelian particulate inheritance [published as Piaget, 1921]). Still, we do know the title he proposed—also in December of 1919: 'an essay on value judgements and biological method in the sciences of the mind' (Liengme Bessire and Béguelin, 2008[1996]: 66). And knowing this makes the connection between his early psychoanalysis and later psychology clearer, especially in light of his subsequent studies of children's value judgements in games like marbles and hopscotch; better known as his book on moral development (Piaget, 1932).

However, he didn't finish this second doctorate. And despite heroic efforts by the archival team, many crucial details were still lacking at the time of publication. As they put it: 'this episode seems to be a critical one in Piaget's development, which of course makes the sketchiness of our knowledge about it all the more frustrating' (Liengme

Bessire and Béguelin, 2008[1996]: 67). Because they also seem not to have published anything further on the topic, in either French or English, we can infer that the archival paper trail then ran cold.

Such is often the case with archival research; preserved records are inevitably partial. It was therefore also inevitable that both books would leave questions unanswered. Indeed, as Vidal (1997: 124) said soon afterward, 'A satisfactory answer to the question "How did Piaget get to psychology?" is still missing' (in response to Harris, 1997; see also Vidal, 2000). He himself then remained active in filling the gaps, including on the psychoanalytic side (see, for example, Vidal, 1986, 2001[1995], 2002a, 2002b). But his interests have subsequently turned to other topics. As a result, there is still much to do.

A paradox, and after

When we dig into this high-quality material, we find that there's a lot more to the early Piaget than one would expect from the textbook histories that are echoed in the secondary literature. By taking even a brief look, however, we find more than just new things. Indeed, there's even a paradox. And it's one that affected Kuhn too, as we see in his own recollections of his earliest explorations:

> I read a good deal, beginning with his *Mouvement et vitesse*. And I kept thinking, my, these children develop ideas just the way scientists do, except—and this was something I felt Piaget did not himself sufficiently understand, and I'm not sure that I realized it early—they are being taught, they are being socialized, this is not spontaneous learning, but learning what it is that is already in place. (interviewed in Baltas, Gavroglu, and Kindi, 2000[1997]: 278–9, citing the French original of Piaget, 1970[1946])

This misunderstanding—that Piaget was unaware of socialization, or dismissive of it—is terribly important in terms of characterizing the split between the popularization and the primary sources. For that reason, it also has to be our first step as we work our way backward into Piaget's psychoanalytic roots.

Briefly: this is the 'Piaget' (1926[1923], 1928[1924]) who was criticized by Vygotsky (1962[1934], 1987[1934]). But that is also the same man who, in 1936, received an honorary doctorate from Harvard as part of the Tercentenary celebrations (four years *before* Kuhn's first year there).[13] This is then paradoxical because—although Piaget's lecture was entitled 'Principal factors determining intellectual evolution from childhood to adult life' (Piaget, 1937)—he received that award for his *sociology* of child development (Hsueh, 2004).

This interpretation by the Tercentenary organizers wasn't wrong: Piaget's peers in Switzerland had a similar view of his work at the time, and his full professorship at Geneva was initially as the Chair of Sociology (Ratcliff and Borella, 2013; discussed in English by Burman, 2015). In other words, there are two conflicting interpretations of the early Piaget. And the one that's less deeply embedded in the sources has since come to dominate our contemporary understanding. (Kuhn's too.)[14] But this 'anti-Vygotskyian Piaget' is indeed misleading in a fundamental way that ultimately serves as a barrier to

deeper understanding.[15] There are deep conflicts between the primary and secondary sources that have yet to be adequately resolved.

In short: Piaget is both well known and, in certain respects, totally unknown. Thus, in Vidal's (1997, 2000) words, we need a 're-reading' of the early Piaget that influenced Kuhn (that is, prior to the popularization that changed the meaning of his name). The simplest way to do this, in the sense of manifesting this most-neglected foreign invisible—which is to say the unknown early sociologist of child development who was influenced by biology, philosophy, and psychoanalysis—is to follow his early philosophy of part-wholism. This was made most explicit in his autobiography. And even though that is a potentially problematic source, we can at least start there and then continue to work backward into the other less problematic primary sources so that they can show us new things we couldn't otherwise have seen.

Piaget's part-wholism

The following autobiographical passage is typically quoted in discussions of the early biological and philosophical influences on Piaget's theory (for instance, in Burman, 2016: 757; Chapman, 1988: 15; Messerly, 1996: 7). There, what he intended—whether originally or in recollection—was clearly meant to stand as a scientific universal:

> I suddenly understood [at around the age of 18 or so] that at all levels (viz. that of the living cell, organism, species, society, etc. but also with reference to states of conscience [sic], to concepts, to logical principles, etc.) one finds the same problem of relationship between the parts and the whole. . . . There at last was the close union that I had dreamed of between biology and philosophy. . . . In all fields of life (organic, mental, social) there exist 'totalities' qualitatively distinct from their parts and imposing on them an organization. Therefore there exist no isolated 'elements'; elementary reality is necessarily dependent on a whole which pervades it. (Piaget, 1952: 241–2)

Reading this out of his pre-psychoanalytic context and into his later psychological work, we can propose that children are to be considered—at least by the autobiographical Piaget, post–World War II, even if perhaps not by the young man 'before *Piaget*'—as individual parts of a larger social whole. Yet his part-wholism was also more complicated than this. And the easiest second step backward is to look at the book that Kuhn himself read, which he reported on in his 1949 notebook: *Mouvement et Vitesse* (trans. as 'The child's conception of movement and speed': Piaget, 1970[1946]).

Again, briefly: one would be tempted, as an adult with a good grasp of the physical world, to operationalize movement as 'displacement' and speed as 'average displacement over time'. A series of observations of displacements along a path, in succession, are then parts to the whole that is a movement in both space and time. But interpreting this sequence as *speed-in-the-way-we-mean-it* reflects an operation that must develop. Indeed, Piaget found that young children first see speed in terms of a spatial 'overtaking' of one part with another (Piaget, 1970[1946]: 281, 293). Only later do they see these parts moving in relation to a coordinate whole (ibid.: 286–7, 304–5). Understanding a young child's apparently relativistic[16] interpretations therefore requires climbing into

that child's head, and then looking out at the world from their perspective (in the same way that Kuhn realized he had to climb into Aristotle's head to understand his physics).

The cognitive (popularized) version is somewhat more obvious in Piaget's (1952[1941]) book on how children learn to reason with number: 'the child can perceive either the whole or its parts, but not the whole and one of its parts simultaneously' (ibid.: 174) and 'as soon as the child envisages one part separately, the whole is destroyed' (ibid.: 172). In an earlier book that was translated afterward, Piaget (1954[1937]) wrote that the young child's key realization—related to Conservation (described in his Harvard address [Piaget, 1937: 248])—is that they are themselves a part of a larger comprehensible whole: 'the universe becomes a coherent whole in which effects follow causes which are independent of the subject and in the midst of which the activity itself must, in order to intervene in the structure of things, submit to objective laws that are both spatial and temporal'. Still earlier, Piaget also applied part-wholism to the social systems inside of which children develop: 'we shall call "ideal" every system of values which constitutes a whole . . . and we shall call "values" the particular values related to this whole or the means making it possible to attain this goal' (Piaget, 1952[1936]: 10). This connects us to the topic of his unfinished second doctorate. But the same interest is equally apparent in the slightly earlier book on moral development, where there is a great deal of discussion about whether all members of a classroom ought to be punished for the misbehaviour of a single member (Piaget, 1932). And it's discussed throughout his first article reporting explicitly on the research conducted at the Binet Lab in Paris, where it's even included in the title: 'Essay on Certain Aspects of the Development of the Notion of Parts in the Child' (Piaget, 1921).

Some theory as a climbing aid

This doctrine of part-wholism can at least be treated as having been accepted by the author—the later *Piaget* we know—as foundational for what came afterward. We see that he also described it earlier than in the later books for which he became known. Indeed, we even find it in his oft-cited early novel, which was written soon after the oft-quoted teenage classroom insight about levels and totalities (Piaget, 1918; summarized in English by Gruber and Vonèche, 1993[1977]). So let's have some theory to help us read these historical texts.

To treat children as parts to a whole is obviously true in a trivial sense. It's clear that the child is the juvenile form of the adult, and that every adult was once a child. Children are thus parts of the whole that is our future as a biological species.[17] But this is also true in a way that really matters for the epistemological programme that Piaget supported with psychological methods, which Kuhn—like many others—misunderstood: the construction of knowledge always takes place in a social milieu that imposes an organization on learning, which is to say *priorities* (ideals and values). Thus, as Piaget explained in one of his last psychoanalytic articles of this early period: 'thinking is disciplined by social life' by learning signs-as-instruments that can be used linguistically, and also by learning language-as-a-conceptual-structure that anchors and regulates the meanings of these signs in an external logic which is itself a 'condition for the existence of common thought' (Piaget, 1933: 406).[18]

Had Kuhn seen this duality—instruments and conditions—in his formative readings, then he might not have had to revise his paradigm concept into exemplar and disciplinary matrix in the postscript added to the second edition of *Structure*. The split would have been clear from the start. Yet it is also clear that, for Piaget, the construction of knowledge follows *not only* from exposure to objects-to-be-learned (what for Kuhn became exemplars) *but also* from coming to adopt the meanings-that-organize (pressures [discussed below], and later implications, from the experiential matrix). Thus: a child can be either underexposed in a particular domain, or under-disciplined in it, and then development will lag behind (discussed in detail in Piaget, 1941).[19]

As I pointed out in connection to the American Cold War focus on speeding up education, however, the fact of this delay was not—for Piaget as it was for Kuhn (with incommensurability)—the main philosophical problem that needed to be addressed. Instead, Piaget's focus was on the underlying constructive process. That's what needs to be contextualized further if we are to understand new things, and if we are to uncover more of what comes along with his mostly unknown psychoanalytic roots.

Sociality and snails

Behind this more explicitly social view of Piaget's genetic epistemology—his constructive theory of knowledge, of which children's cognitive-developmental stages were only a part—is an important and misunderstood premise: the environment in which the child finds herself biases the construction of her knowledge in that direction. This is a direct import from his early biological research with the snails of his first doctorate. As a result, however, we see that genetic epistemology is not *just* about truth; the summit to be achieved after a climb (pace Smith, 1993).[20] In order for true-knowledge to develop, the child must first have access to it; it must be valued.

As before, we find this if we go looking for it. For example: in his essay on 'genetic logic and sociology' that was published originally in 1928, then reprinted in the third French edition of a collection published in 1977 and translated in 1995 as part of *Sociological Studies* (Piaget, 1995[1928]). This essay was also cited explicitly in the moral development book (Piaget, 1932: 65, n. 2). And it is so revealing of this neglected aspect of Piaget's thought that it was described by the editor of the English translation of *Sociological Studies* as affording the book's 'central question' (Smith, 1995: 1). This therefore seems like a useful third step as we work our way backward into this invisible history.

Piaget (1995[1928]: 185) began this essay with a clear statement: 'the development of the infant is an adaptation of its mind to its social and physical surroundings'. But I want to take the discussion that follows in a very different direction than did his English editor.[21] Indeed, I'd like to recognize the reference from moral development and thereby connect this essay explicitly to that slightly later book and then read across the grain of both of these texts. Thus, I propose reading that earlier sentence through this later one: 'through imitation and language, as also through the whole content of adult thought which *exercises pressure* on the child's mind as soon as verbal intercourse has become possible, the child begins, in a sense, to be socialized from the end of its first year' (Piaget, 1932: 26; emphasis added). And I'd like to suggest that the idea underlying both

of these psychological passages could equally have been written about Piaget's snails: his source from just before his turn to psychoanalysis, which also provides us with the background against which to interpret its influence.

This is a reversal of our expectations, today, but Piaget did the very thing that I am suggesting we do: he explained the adaptive behaviour he observed in his *snails* by comparing them with pre-linguistic *infants*. This is so surprising that I've translated the entire passage, which should then be read through the lenses of a biological—or, more precisely, zoological (natural historical)—audience from the early 20th century:

> Let those who are unfamiliar with contemporary psychology be reassured: there will be no intervention here by vital essence or soul. Psychology is the science of conduct.[22] In man, whose conduct is accompanied by directly knowable states of consciousness, there is a benefit to analysing comportments both from within and from without. Human intelligence can thus be studied both by examining the external reactions of the body (in the baby, for example, who does not yet speak but nevertheless learns to conduct himself in space by endless experimentation with the surrounding objects) and by examining the internalized activity that is judgment or reasoning. In animals, whose consciousness we ignore (and about which we are ignorant), psychology is limited to a study of reactions: reflexes, habits, trial and error gropings, the eventual invention of new processes, etc. The psychology of the *Limné* snails will therefore be of their reflexes, unconditioned or conditioned, of their acquired habits, memory, etc. (Piaget, 1929: 448)

He continued, adding a further detail that should have informed all subsequent readings of his work:

> There is therefore no mystery in any of this, and I don't see why one would be afraid of a word. But if the word 'psychology' bothers the reader, replace it with 'physiology of the nervous system' or whatever you like: my conclusions will remain the same. (Piaget, 1929: 448)

From this, it seems clear that Piaget was speaking simultaneously in several registers: his biology was psychological, and his psychology was biological.[23] Thus, he didn't always mean what his later readers—or translators—might have thought: treating Piaget's psychology as a reflection of his biology, and vice versa, reveals things of his earliest thinking that we couldn't see before (see also Jurczak, 1997).

Pursuing this further, we find new clarifications of terms that had previously looked solely psychological. For example: although an evolutionary 'adaptation' is hereditary, across generations, Piaget (1929) used the term 'accommodation' when such a change occurred at the level of the individual. Also: when he spoke of adaptations, he referred to reflexes (*réflexes*). But when the discussion was of accommodations, his talk turned to habits (*habitudes*).[24] Piaget's study of children also afforded a further epistemological problem that his snail studies could not: 'one cannot speak of the child without asking if logic is something social and, if so, in what sense' (Piaget, 1995[1928]: 185).[25] His solution then blended the three approaches—social, biological, and logical—in such a

way that I wonder if the resulting collection of essays shouldn't have been entitled *Socio-Ecological Studies*.[26]

Truth and consequences

The beliefs of others put pressure on the developing child, but it is only by learning cooperation—reciprocity, which is to say the rhythmic consistency of turn-taking—that one achieves a *method* for *acquiring* the truths that can ultimately and forever relieve this pressure (Piaget, 1995[1928]: 208; original emphasis). Yet this doesn't mean that what cooperative children actually learn will be true, because acquiescence in the face of external pressure—the uncritical acceptance of socially imposed constraints—only inflicts shared *beliefs* (ibid.; original emphasis). It does not itself provide the impetus to strive for truth as a shared value.

In this connection, Piaget's (1932) early view of moral judgement contrasts the 'heteronomy' of obedience to authority with the self-determination of 'autonomy'. This has since been discussed in detail in the scholarly literature on both education and moral development (see, for example, Kamii, 1984; Kohlberg, 1975, 2008[1963]).[27] And we can think of it in relation to what Kuhn (2012[1962]) inherited from Piaget as being akin to the relation in *Structure* between 'normal science' (individually heteronomous activity) and 'extraordinary science' (individually autonomous activity). Yet by combining this known-view with the usual role played by logic in discussions of truth, we also derive a more nuanced understanding of the means by which autonomy can be achieved outside of crisis situations.

Logics

Following Piaget's (1995[1928]) line of thinking, the missing piece linking cooperation with autonomy in the search for truth would seem to be that provided by the primary cognitive achievement of adolescence: abstract reasoning (Inhelder and Piaget, 1958[1955]; Piaget, 1922, 1954, 1956, 1970).[28] This then manifests both as an acceptance of formal necessity and as a rejection of inconsistency. And it thereby becomes a counterforce to artificially imposed constraints, providing a means for differentiating and then rejecting 'pseudo-necessities' (Piaget, 1986[1977], 1987[1983]). But the resulting thinking is still not always or necessarily directed toward *the truth*.

Truth must be held as a value for truthfulness to emerge, in reflection of that societal ideal, during individual development. Otherwise the abstractions enabled by formal operational reasoning get directed instead toward other ends, such as manipulating the accepted conventions and dogmas of the law separate from the norms and values of morality (for example, Piaget, 1995[1944]). We see this concern developed further in Piaget's comments in a little-known post-war seminar presentation at UNESCO, where he contrasted 'primitive' with 'modern' societal modes of development:

> The upbringing of the adolescent in primitive societies ruled by tribal custom tends essentially to conformity. On the one hand, intellectual conformity: there is nothing to induce in him the habit of reflection or the critical spirit, for in every field (from true techniques to

> mystical representations and from magic to the causal explanation of phenomena) his thoughts are ready-made for him and he bows to the collective notions of the tribe handed down from generation to generation. And, on the other hand, moral conformity: sacred duties and ritual prohibition (tabus [sic]) leave only the narrowest margin to action not governed by rules. (Piaget, 1947: 3)

Of course, I see a great deal of the 'primitive' in contemporary society too. We also find it in Kuhn's conservatism (Fuller, 2000). But this is because Piaget is using an old meaning of the term.[29] Recognizing that, the present concern applies equally well to his comments about practicalities in that same essay: 'if the pupil's intelligence is subordinated to a master's authority in the sphere of knowledge, the pupil's moral conscience cannot in other respects free itself to build a rule of conduct suited to the problems of the day' (Piaget, 1947: 4).

From this perspective, it is clear that the child's potential for complex scientific reasoning—truth-seeking unencumbered by local constraints—is itself a *modern ideal* that underlies Piaget's epistemological project.[30] It is therefore also a trait of his 'epistemic subject' (the idealized knower), rather than of all 'individual subjects' (children) who were examined in schools and laboratories around the world (see Niaz, 1991). In short, autonomy is a *virtue* in reflection of that ideal: a *goal* toward which our pedagogy should strive, but *subordinated to* higher ideals that undergird the developmental pathway as *norms* (such as 'truth'; see Burman, 2013, 2016, 2019).

That said, we need a word more about logic: in addition to providing order to cognition, logic for Piaget also provided the formal means by which one could describe the conjugations of accepted or implicit rules that produce outcomes upon which everyone can agree (such as those in marbles or hopscotch). But one need not *reason logically* in order to follow a set of rules. Instead, as he wrote early on: 'mathematical reasoning consists of spatial and numerical constructions, the rules of which are not rules of logic but the propositions previously accepted' (Piaget, 1926: 454; expanded upon in Piaget, 1952[1941]). Such propositions can be treated, in other words, as exemplary of a way of acting; conventions adopted as values *before* being reflected in cognitive operations.[31]

It is for this reason that *logics*, for Piaget, can develop. He then generalized this across multiple domains. As he put it most memorably: 'logic is the morality of thought' and 'morality is the logic of action' (Piaget, 1932: 404). Yet this is also clearly not logic in the way meant by the philosophers who later criticized him for his misuse of their formalisms (see, for instance, Quine's complaint in a letter, also from 1960, about 'Piaget's persistent and evidently incorrigible stupidity over matters of logic' [quoted in Burman, 2016: 760]).[32] Instead, logic to the child is just another form of developmental pressure. Its impositions can be renegotiated until they become *necessary*.

Further clarifications

Delving into these early works provides a much more nuanced view of the theory that was popularized as part of the American response to Sputnik. Some of the resulting insights are surprising. For example: these early works are clearly not about *cognitive* rules. At least, not in the way that we now mean the word (see Green, 1996). What the

child learns is instead akin to how a snail adapts: in response to the pressures of the socioecological milieu, the body—including the 'physiology of the nervous system'—develops in such a way that reflects its life-history to that point.

The resulting view of development is not, of course, *representational* in the way we now think of it. What is learned is not *information*, in the sense used today. That's too linguistic. And Piaget's early theorizing had to work simultaneously at multiple levels, including those prior to language (viz. for non-linguistic snails as well as for pre-linguistic children). So operational structures should instead be thought of as being written directly into the body. Indeed, as he explained of his snails: 'the shell of a *Limnée* [pond snail] constitutes the most authentic psychological document we have describing the history of its proprietor' (Piaget, 1929: 327). Language—the civilizing achievement of human evolution—then builds on that embodiment, which develops (see the contributions by Cellérier, Inhelder, and Piaget in Piatelli-Palmarini, 1980[1979]).

Embodying operations in this way imparts stronger theoretical force than would a solely cognitive or linguistic rule. They're structures. Literally. It's clear from this, too, that responses are *impelled* before they're *considered*. The result is that there are also stages of development in other areas besides cognition. Indeed, Piaget explained this in his last explicitly psychoanalytic essay of the early period:

> Thinking (reason), like feeling (affect), has a history. This is to say that it evolves in its structure and not solely in its contents. The steps of this evolution can be characterized by means of a system of 'stages,' with the understanding that such a classification is always artificial. (Piaget, 1933: 405)

Again, we find what could now be considered a notice of intent in the primary sources that preceded the popular translations. Yet we can also reverse the direction and return even this to the snails of the earliest Piaget before psychoanalysis: it is affect that indexes the discordant pressures of the human context, but at much faster timescales than can be observed of the snail shells that contract in response to the tension of holding tightly onto stones in rough waters (calcifying to provide the stable 'psychological documentation' of the history of each snail's individual development).

In engaging with children, rather than with snails, I propose that this is what the early Piaget sought to assess: *what are you holding onto?* Because unlike snails, which hold evermore tightly to stones as the turbulence increases, humans hold not only onto objects but also to people and to ideas. And that, to me, is a much better reflection of Piaget's early biological and psychoanalytic origins—transmitted to Kuhn and haunting Forrester—than the contemporary version of his questioning method that's reflected in even the best and most critical textbooks (viz. *How do you justify your knowledge-claims?*). Briefly put: documenting this history of tensions, and their attendant beliefs and values, is what those interviews were about.

To what end?

We are thus led to Piaget's original biological turned philosophical turned psychoanalytic and later psychological problem: how to define a developmental scale of held-

values, reflecting the pressures of societal norms, that is at the same time scientific in the strong sense afforded by studies in biology? As he mused, very early on, in his philosophical novel:

> Supposing that psychology wanted to draw up a scale of values, a number of difficulties would arise. The principal among them is that all of the values are relative to each other. If I were to decide to place 'action' above all others, then the values of 'existence' and 'knowledge' would find their worth only in relation to this norm. And vice versa. (Piaget, 1918: 166)

After Piaget's departure from biology, however, this relativism became interpretable—psychoanalytically—as an individual patient's actual and documentable history of resolved and unresolved conflicts. A held-value then became an internal reflection of an external pressure that was *felt to exist* (whether it actually did exist or not) but which could also, in its higher form, be reflected upon and expressed in catharsis to expel any residual stress. And it seems to me that it's also this that led Piaget to turn away from psychoanalytic *methods*, in proposing what he referred to as an 'urgent' experimental study: 'the advance is that the cathartic process is itself in contradiction with the psychoanalytic conception of censorship: it is, in a sense, at the moment when the complexes become conscious that the subject is cured of them' (Piaget, 1920b: 54).

Mental development, which served as the model for Kuhn's lineage of scientific paradigms and revolutions, thereby became—for this early psychoanalytic Piaget—a process of moving from unconsciousness to greater consciousness, and of overcoming the conflicts between them (Piaget, 1920b: 55). These conflicts, though, could be both intellectual and moral (ibid.: 56). And from there his famous turn, while working in Binet's former lab, toward asking children to reflect on their answers when responding to tests of intelligence. But it's these additional references that make those questions so interesting, as he reflected in his later autobiography: 'from the very first questionings I noticed that though [the] tests certainly had their diagnostic merits, based on the number of successes and failures, it was much more interesting to try to find the reasons for the failures' (Piaget, 1952: 244).

That is ultimately the influence of psychoanalysis on Piaget. And Kuhn saw its value in this respect too, as Forrester (2017[2007]: 30) noted. Importantly, though, interviewing is not all that mattered to Piaget that came from this period. Its *combination* with his other early interests is what's important for what came after: the broader matrix of interconnected meanings—the *plurality* of inter-referring concepts. It's also those additions that haunted Kuhn.

Asking children and scientists to justify the truths they hold to be self-evident enables a study of the different ways of justifying knowledge-claims that are *held to be true* without concern for whether they are actually correct. They only need to *feel* true to those who hold them: stable, safe, solid, and secure. Like a rock to a snail in rapids.[33] The resulting scale of possibly pathological and irrealistic *kinds* can then be understood as representing different *epistemological species*, which is to say varying interrelated parts that can be described collectively as wholes. And these species can be understood to exist in different socioecologies that pressure them. Like the different snails that could be

found along the mountain paths that Piaget walked as a doctoral student, which he then collected, grouped, and categorized (see his dissertation, accepted in 1918 and published as Piaget, 1921).

In reflecting on what this means for Forrester's cases, I suggest that post-Kuhnian analysts and historians share values with Piagetian developmental psychologists—or rather, since this now has a misleading popular meaning, with 'genetic epistemologists'—who use the collected evidence to climb into the heads of others and thereby attempt to understand the world according to *the view from within* (cf. Burman, 2012a). And even if not falsifiable, directly, the resulting biologically informed theory is still scientific in much the same way as Darwinian theory is scientific: it is a powerful 'metaphysical research programme' with ties to 'situational logic' (using the language introduced by Popper, 2009[1974]).[34]

Logic, of course, is a fraught topic when it comes to Piaget. One of the contributions of this hauntology, therefore, has been to show how this is a result of our misunderstanding how children's apparently logical action is anchored first in social norms: values individually, and ideals socially, that can be followed in such a way as to produce results that can be analysed formally without the requirement that the description's formal operational (cognitive, internalized) equivalents actually be held by any of the children described. Kuhn (1970) then shared an aspect of this, albeit without direct reference, by proposing that paradigms be reconsidered as exemplars that collectively afford a disciplinary matrix of meanings. This constitutes the set of norms governing what is acceptable in both stated-fact and avowed-interpretation. And so I propose that we can read something similar out of Forrester's Kuhn-inspired and Piaget-haunted approach: thinking in cases constrains our analyses, and—if considered in their plurality—force the resulting abstractions to remain attached to the underlying sources only so long as their influence continues to be felt.[35]

Author's Note

My original draft was presented in the Forum of the Theory and History of Psychology Department at the University of Groningen, and I benefited enormously from the resulting discussion: the opinion at that time was that my connection to Forrester was weak. Despite some revisions before peer review, the reviewers then agreed with my colleagues and students. I am therefore especially grateful to the editors for suggesting that the draft be reworked to incorporate Kuhn. That also pushed me toward the hauntological approach presented here, and enabled me to synthesize my original manuscript with an update then in progress of a much earlier essay on similar themes. This update had been presented separately at the annual meeting of the International Society for the History of Philosophy of Science (HOPOS), also held in Groningen. And so Kuhn was relatively easy to work in. Related to this, I thank the Cummings Center for the History of Psychology for permission to quote from material held in their collection (which is not accessible elsewhere).

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Notes

1. Post-Kuhnians who think themselves beyond Kuhn, perhaps even as a result of such dismissals, will want to note his later comment about the famous realization reading Aristotle: 'My difficulty was not that these anomalous passages were more obscure than the others I had read, for they were not. My problem was rather that I could not believe a person of universally acknowledged intelligence could have written them: they seemed not so much obscure as *absurd*. I read on, however, with steadily increasing frustration until one memorable day when I suddenly saw a pattern in the passages that had seemed anomalous. All at once the text made sense to me, but only if I changed my way of reading' (Kuhn, 1999: 33; emphasis added). That, to me, is Kuhn's primary contribution. And, it seems, to Forrester too.
2. The references for these are Forrester (1980, 1990), Appignanesi and Forrester (1993), Forrester (1997a, 1997b), and Forrester and Cameron (2017).
3. The origin of this is Derrida (1994[1993]), who defined 'hauntology' as a transformative linguistic act that 'consists always in attempting to ontologize remains, to make them present, in the first place by *identifying* the bodily remains and by *localizing* the dead' (ibid.: 9, original emphasis) and also as 'an interpretation that transforms what it interprets' (ibid.: 63). In developing this further as a useful contemporary method, we can read the invisibility of *a* haunting *différance* as requiring the unearthing of linguistic bodies which differ with a received interpretation. Hence, we must find forgotten or unknown texts to give the new interpretation a referentially anchored meaning. As he explained, 'We will designate as *différance* the movement according to which language, or any code, any system of referral in general, is constituted "historically" as a weave of differences' (Derrida, 1982[1968]: 12, original emphasis). My reading of what this entails has therefore been enacted here: a larger than usual number of references and quotes and translations have been provided to inform my argument, and they have been presented strategically in order to reveal and then remove layers of different aspects of our misunderstanding that produce what *amounts to* a haunting. (So if you don't like Derrida, then how about Scooby-Doo?)
4. In this, I follow the final revised title of Kuhn's last (unpublished) book: *The Plurality of Worlds*: *An Evolutionary Theory of Scientific Development* (see Hoyningen-Huene, 2015).
5. The mentions of Piaget are in footnotes, in *Structure*, while those of Bruner and Postman are directly in the body. Kant isn't mentioned directly. Elsewhere, though, Kuhn did indeed describe himself as 'a Kantian with moveable categories' (in Baltas, Gavroglu, and Kindi, 2000[1997]: 264). He also described his last (unpublished) book as espousing 'a post-Darwinian Kantianism' (Kuhn, 2000[1991]: 104). This needs to be developed further (see Kuukkanen, 2013).

6. I notice in retrospect that I wasn't explicit in providing the name of Piaget's synthesized version of Kuhn's paradigm with Foucault's episteme: *le cadre épistémique*, which got rendered in translation variously as 'the epistemic frame of reference' and 'the epistemic framework' (see throughout Piaget and Garcia, 1989[1983]). Also relevant is my subsequent discovery of over 100 unpublished experiments, by Piaget and his colleagues, on how children learn to represent physical causality. They were to be collected as *Travaux sur la causalité*, which is referred to in the notes of that same book as '3 vols. to appear' (ibid.: 283). But they never did appear. A description of the discovery process—a literal archaeology of the piles of papers preserved in Piaget's home office—has been published in French, and resulted from a collaboration between the archival team who removed the papers to the archives and the historian who then took advantage of their efforts at protecting the piles' order-relations (Ratcliff and Burman, 2015). An expanded and updated English text is in preparation. These unpublished manuscripts are especially noteworthy, in this context, because the experiments they describe advance the original studies that inspired Kuhn. And also because Kuhn was invited to participate in the theoretical seminar that Piaget organized to inform his team's efforts. (Kuhn's presentation at that meeting is the source of several of the direct quotes here.)
7. Kuhn, letter to John Flavell, 28 October 1960, John Flavell papers, Archives of the History of American Psychology, Cummings Center for the History of Psychology, University of Akron, Akron, OH, Box M2991, Folder 14. This letter was regarding what later became *The Developmental Psychology of Jean Piaget*, in which he also discusses some of Piaget's impact on his own thinking. (Note that this refers to a version of the manuscript from *before* what Hoyningen-Huene (2015) calls 'proto-*Structure*').
8. Spielrein is profiled by Appignanesi and Forrester (1993) in *Freud's Women*, where her connection to Piaget is described on pp. 223–5. Note, however, the common error there of saying that he 'had studied psychology with Alfred Binet in Paris' (ibid.: 224). Binet died in 1911, and Piaget was hired to work at the *former* Binet Lab in 1919. (For reflections on this period not included in his other autobiographies, see Piaget, 1975.)
9. Piaget (1971[1970]: 1) described his research programme as follows: 'Genetic epistemology attempts to explain knowledge, and in particular scientific knowledge, on the basis of its history, its sociogenesis, and especially the psychological origins of the notions and operations upon which it is based'. My preference, however, is for Chapman's (1988) explanation. This is both simpler and more direct: 'The goal of this discipline was investigation of the origins of knowledge' (ibid.: 1), which is to say 'how new forms of knowledge and reasoning come into being' (ibid.: 336). Note of course that 'genetic' here means 'genesis' rather than 'related to genes' (Burman, 2012b: 285, n. 1; see also Burman, 2019; Burman, Bazar, and Weizmann, 2020). And note too that the theoretical changes made after the popularization have been referred to not only as 'Piaget's new theory' (Beilin, 1992; pace Smith, 2017), but also—to draw attention to his updated biological meta-theory—as 'epigenetic epistemology' (Burman, 2013, 2016).
10. His method goes by two names: it was called 'the clinical method' until World War II (approximately), and was afterward called 'the critical method' (see, for instance, Bond and Tryphon, 2009; Mayer, 2005). But contrary to the hauntologist's instinct, it was not a *lie* for Piaget to refer to psychoanalysis as psychiatry. Indeed, in his first psychoanalytic paper, Piaget (1920a: 18) offered a reason for this naming: in France, psychoanalysis was—at the time—ignored by all *except psychiatrists*.

11. This should be Gottlob Friedrich Lipps (1865–1931) and Arthur Wreschner (1866–1932), as noted—in French—by Ducret (1984: 497).
12. Appignanesi and Forrester (1993: 224) report that Piaget 'concealed the identity of his analyst [Spielrein] for a long time, indeed he never directly acknowledged that it was her'.
13. The initial enthusiasm in the department of psychology was to give this recognition to Freud, but James Bryant Conant—Harvard's new president and, later, Kuhn's mentor—discouraged this in favour of a fuller search (see Burman, 2015; Hsueh, 2004).
14. Kuhn's 1949 notebook indicates that he started not with *Mouvement et Vitesse*, as he later recalled, but with one of the earlier books criticized by Vygotsky: *Judgment and Reasoning in the Child* (Piaget, 1928[1924]; see Galison, 2016: 51). Yet this book was then later dismissed by Piaget, along with his other early psychological works, for having been incautious in its language:

> I published them without taking sufficient precautions concerning the presentations of my conclusions, thinking they would be little read and would serve me mainly as documentation for a later synthesis to be addressed to a wider audience. . . . Contrary to my expectation, the books were read and discussed as if they were my last word on the subject. (Piaget, 1952: 247)

15. Few people are aware that Piaget responded to Vygotsky (see Piaget, 1979[1962], 2000[1962]). There are also plenty of examples of his social thinking (see especially Piaget 1995[1977]). A once harder-to-find example from an archival source, from the same era, is quite direct: 'man is a social creature and society alters, develops and perhaps wholly creates certain mental processes' (Piaget, 1964: 7). This is now freely available online.
16. This characterization is intentional: Piaget's project was inspired by direct questions from Einstein (see Sauer, 2019).
17. This is explicitly *not* a Haeckelian recapitulationist view (see Burman, 2019; pace Levine, 2000; pace Vonèche, 2003).
18. Piaget (1933) then mentioned Saussure (p. 406) and Freud (p. 407), but he did not cite specific works.
19. Piaget had a specific word for when these delays arose: *décalages*, which was translated initially into English as 'shiftings' (see, for example, Piaget, 1932: 56, n. 2).
20. Piaget's later formalization of equilibration—thus also his updating of assimilation and accommodation—makes it clear that he talks about things being *functionally true*, relative to how they have been tested and found lacking, which is quite different from them being *True* formally according to extensional Truth-Table logic (see Burman, 2016).
21. This is justified in part because the essay was not original to the *first* French edition of the book. Instead, it was added as the first chapter of the *expansion* that—together with the first part—became the *third* French edition and from there the first English edition. So there are alternate interpretations to the one the editor presented, at least one of which I aim to uncover here.
22. I have chosen to be slightly inelegant in translating the original (*la science des conduites* and *les comportements*) to follow Piaget's connection to Pierre Janet, and the tradition of rendering Janet's French as 'conduct' and 'comportment' in English to mark its difference from American Behaviorism (see Amann-Gainotti, 1992; Amann-Gainotti and Ducret, 2002).
23. Indeed, they build on each other. As a result, his sociology is also psychological and—at a remove—biological too (see Piaget, 1918: 173). This interdisciplinary approach continued into

his later work: 'Every psychological explanation comes sooner or later to lean either on biology or on logic (or on sociology, but this in turn leads to the same alternatives)' (Piaget, 1950[1947]: 3).

24. For those who use digital aids to conduct their textual archaeologies, it's worth noting that Piaget (1929: 448) cited 'Pawlow' in this connection rather than 'Pavlov'.
25. Or, put another way: 'what is the nature of the agreement of minds which guarantees logical truth (as opposed to other possible sorts of agreement) and what is the nature, collective or individual, of the instruments of thought by means of which an individual, even isolated and momentarily contradicted by all others, may demonstrate a logical truth or the existence of a fact?' (Piaget, 1995[1950]: 35).
26. This would have been in keeping with the other works from that period: the publication of the first French edition of *Sociological Studies* in 1965 followed Piaget's return to active research in biology in 1964 (see Burman, 2013, 2019).
27. Note that the classic paper by Kamii (1984: 411) includes an illustration with a misleading error: the locations of the labels, heteronomy and autonomy, are reversed.
28. It will surprise readers that the psychologist most famous for his studies of 'obedience to authority', Stanley Milgram (1974), had earlier served as co-translator of Piaget's book with Inhelder on formal operational reasoning (Inhelder and Piaget, 1958[1955]). Why this happened isn't exactly clear. The primary translator—Anne Parsons, daughter of the Harvard sociologist Talcott Parsons—completed her doctoral dissertation in 1955 on the popularization of psychoanalysis in France and the United States. This was written in French, and she had studied with Piaget in Paris (Breines, 1985: 815). It is also noteworthy, in this connection, that Piaget's lessons at the Sorbonne in 1953–4 involved a return to earlier psychoanalytic interests (Piaget, 1981[1954]; see also Meljac and Diener, 2000: 45). And so it seems acceptable to place the translation of the formal operations book in this context. However, it is not yet clear to me why or how Milgram became involved; perhaps because he was a PhD student in the Department of Psychology, while Parsons was at the Medical School. Further research is required.
29. I found this in the UNESCO Archives, and it is now available freely online. But note that the use of the term 'primitive' follows a definition that was borrowed from Lévy-Bruhl, which Piaget later decided ought to be replaced with reference to Lévi-Strauss (see Piaget, 1995[1965]: 23; Piaget and Grinevald, 1983[1973]). That this update is often missed has produced some unfortunate misunderstandings, especially when contemporary readers have sought to use his quotes about 'primitives' to support arguments with which he would not have agreed. (See the discussion of Haeckelian recapitulation in Burman, 2019.) That said, of course, the sources of such misunderstandings are certainly of historical interest and should not be white-washed (see Jahoda, 2000).
30. Piaget later tried to accommodate the encumbrance in his formalism by appealing to relevance logic (Piaget and Garcia, 1991[1987]). But this project was left unfinished at the time of his death (see Davidson, 1993; Ducret, 1988).
31. Kuhn's reading notes from 1949 miss this: 'The Piaget reading is useful primarily in shaping my own general view. It can't be transplanted too literally for the kids haven't got the logical criteria of the adults I deal with' (quoted in Galison, 2016: 51). Both children and adults adopt societal ideals as values, and so both can be understood to behave logically even if they have both not internalized the formal reasons for their actions.
32. This was Kuhn's sense too, as I discovered in the archival letter cited above: 'I have always found Piaget weakest in his discussions of logic. I have to say that he simply does not know at

all what the contemporary developments that he criticizes are about'. See Kuhn, letter to Flavell, 28 October 1960, p. 1.

33. In his later psychological writings, Piaget (1950[1947]: 4) cited Claparède in this connection: 'The individual only acts if he experiences a need' (see translation in Burman, 2008: 181–3; also Piaget, 1974).
34. Lakatos (1970: 184; original emphasis) noted that his '*scientific* research programmes' are in this tradition, and cited Popper in that connection. In other words, the use of the label is not problematic for those who are concerned with the scientific status of Piaget's contributions.
35. Piaget later developed this further in his studies on 'reflecting abstraction' and 'morphisms' (Piaget, 2001[1977]; Piaget, Henriques, and Ascher, 1992[1990]). But there is no space here to go into his later work. More research is required.

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