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Published in:
The Cambridge Handbook of the Intellectual History of Psychology

Publication date:
2019

Citation for published version (APA):
Burman, J. T. (2019). Development. In R. J. Sternberg, & W. Pickren (Eds.), *The Cambridge Handbook of the Intellectual History of Psychology* (pp. 287-317). (Cambridge Handbooks in Psychology). Cambridge University Press.

11 Development

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The challenge of writing an intellectual history of development is that – in the twentieth century, which is to say *the period during which Modern Psychology underwent its major growth spurt* (esp. after World War II, when B. Fred Skinner (1904–1990) and Jean Piaget (1896–1980 dominated)[1] – development, generally, played second-fiddle to evolution. The result is that the developmental discourse has lately been primarily an evolutionary one: discussions of natural change in humans have been tilted toward the maturation and shaping of inherited traits, rather than the construction of novelties constrained by interactions between biology and context.[2] In other words, recent psychological thinking about development has been informed by the manufactured dichotomy of *nature versus nurture* when instead we might have been thinking in terms of *nature and nurture*.[3]

This dominance of evolution over development is most obvious in the history of biology. Indeed, recent scholarship has shown that the leaders of the modern synthesis of Darwinian natural selection with Mendelian particulate inheritance also worked actively to suppress serious considerations of development (see esp. Amundson, 2005).[4] The effect of this was to shift the focus from individuals to populations: the primary discourse in biology ceased to be about organisms or even groups of organisms, or their ecologies, and instead came to focus on probabilistic distributions of genes in populations.

Genes were thus taken out of their contexts, in the synthesized theory, and attributed with causal powers separate from those contexts. The norms of science then also followed suit: gene-talk was deemed scientific, in the biological

[1] Historians of psychology have had a tendency to focus on the early history of the discipline: our origin stories (see esp. Danziger, 1990, 1997). However, it is clear that the influence on the present of the period following World War II is ultimately much greater – and also very highly politicized (see Capshew, 1999; Herman, 1995; Pickren, 2007; Selcer, 2009; van Strien, 1997; also Solovey, 2013; Solovey & Cravens, 2012; Urban, 2010). For a history of the history of psychology in this period, see Capshew (2014).

[2] The rhetorical pendulum seems to swing between these two states, which are often referred to as "preformationism" and "epigenesis" (summarized by Maienschein, 2005).

[3] This has since begun to be discussed popularly: "GxE" (e.g., Shenk, 2010).

[4] J. B. S. Haldane (1892–1964), who was one of the architects of the modern synthesis, noted that the relationship between nature and nurture was once "one of the central problems of genetics" (Haldane, 1946, p. 197). This was then black-boxed until quite recently (see West-Eberhard, 2003).

discourse, and all else was dismissed.[5] This in turn meant that the new synthesis expected by contemporary biologists to extend and expand the modern synthesis, known colloquially as "evo-devo," was delayed in its emergence (Carroll, 2005; Laubichler & Maeinschein, 2007). It is certainly coming, however, because it turns out genes alone can't accomplish everything the dominant theory has demanded of them (Sapp, 1987, 2003; see also Moore, 2015).

The impact of this on psychology, as metatheory, is difficult to assess in full. Yet oversimplifying is easy: psychologists in the twentieth century often considered the base units of stimulus–reactions and temperament to be innate, with observed developmental changes treated as little more than the consequence of an experience-linked maturational sequence that shapes inherited reflexes in different ways to produce different variations on the same theme. Development was therefore not considered *causal* in the same way as inheritance. Studying different age groups instead became a way to assess variability in the expression of inherited traits, and especially of shifted timings (often interpreted in the psychological discourse as reflecting reduced or precocious intelligence), rather than the means to uncover fundamental processes of construction.

This is not to say, however, that nobody was ever concerned with such things. That is clearly not the case: the recent turn toward evo-devo has raised the profiles of several scholars who were once dismissed as dissidents (esp. as "Lamarckians" or "neo-Lamarckians"). In biology, for example, Conrad Hal Waddington (1905–1975) is now celebrated as a "revolutionary" and "pioneer" who was "ahead of his time" (Jamniczky et al., 2010, p. 553; see also Laubichler & Hall, 2008; Slack, 2002). And we can predict that psychologists with related evolutionary-developmental interests will soon receive similar treatment: Piaget for instance, whom Waddington (1975) cited,[6] but also – more recently – Gilbert Gottlieb (1929–2006), who is already considered a "pioneer" of the narrower field of developmental psychobiology (e.g., Bateson & Logan, 2007).[7]

In addition, however, I propose that such attributions sometimes say more about the receiving audience than they do about the original source. This is because historical subjects are often reinterpreted when the present perspective changes. Thus, for example, the work of Arnold Gesell (1880–1961) is today considered exemplary of the maturationist perspective. (Hence, the normative "Gesell Developmental Schedules," see Ball, 1977.) Yet this fundamental tenet

[5] This is exemplified in the hostility of the debate between camps represented, respectively, by Richard Dawkins (b. 1941) and Stephen Jay Gould (1941–2002) (see Brown, 1999; Sterelny, 2007).

[6] Piaget's biology has long been a source of problems for developmental psychologists. This has since been addressed by historians (see esp. Messerly, 1996; 2009; also Vidal, 1994). But I propose that the problems themselves are ultimately more interesting than the subject matter. So my intent is explicitly *not* to call Piaget a *pioneer*. Instead, I aim to use him as a source for a microhistory of scientific neglect.

[7] Gottlieb is best remembered today primarily for "probabilistic epigenesis" (see esp. Gottlieb, 1991, 1992, 2007). This, however, suffers from all of the same problems of visibility as Piaget's neglected biological theorizing. So the simplest move is to start there: of the two, Piaget is much better-known.

of introductory textbooks – not only in developmental psychology, but also in pediatrics – is now being questioned and complexified (e.g., Dalton, 2005). And the very fact of these reinterpretations is very interesting, historiographically (see also Wozniak, 2005). As a result, what concerns me here is the reverse of celebration: what I have elsewhere called "the neglect of the foreign invisible" (Burman, 2015).

On Method: Identify and Investigate Neglected Invisibles

The goal in identifying neglected invisibles is not to lament their neglect, but to investigate it (see also Burman, Guida, & Nicolas, 2015). In this case, with the history of development under evolution, that approach applies especially nicely. The biology-talk is a bit difficult, of course, since that is already itself foreign to most psychologists. But the rhetorical structure is at least something with which historians of psychology should already be familiar: the relationship between development and evolution in biology, especially after the midpoint of the twentieth century,[8] has been characterized in much the same way as that between applied psychology and experimental psychology at the start of the twentieth century.

Indeed, it is now well-understood that Edwin G. Boring (1886–1968) intervened – over Lewis Terman's (1877–1956) explicit objections – and pushed early modern psychologists' view of scientific psychology away from application to reinforce experimentation (O'Donnell, 1979). His textbook, *A History of Experimental Psychology* (Boring, 1929, 1950), was then so influential that it changed the entire discipline: it provided a solid and unambiguous foundation on which to build the science we have today. This then redistributed funding and top talent, and generally restructured psychologists' norms and values (see, e.g., Teo, 2013b). For something similar to have happened in other academic areas should therefore be no surprise.[9] Indeed, the recognition that such things have happened should be exciting: there's a huge amount for historians to investigate, most of which we can't presently see, but much of which also has great relevance to contemporary thought and practice.

[8] The "modern synthesis" gets its name from Huxley (1942). But it came together in the two decades prior. And in the two decades before that, Darwin and Mendel were considered to be in direct competition (see Sapp, 1990). In other words, there has been a lot of change reflected in the intellectual history of evolution. The subsequent suppression of development from the evolutionary discourse has been examined especially by Amundson (2005).

[9] This is commonly examined in terms of national differences, such as the omissions in importing Wundt's experimentalism into the United States (e.g., Blumenthal, 1975, 1977; Leahey, 1981). Or the differences between American and French styles of intelligence testing (Carson, 2007, 2014). Or indeed in the changes undergone as psychology has moved across national boundaries, and then "indigenized" as the foreign is made local (Danziger, 2006; Pickren, 2009; also Baker, 2012). But it need not be. Indeed, this is why I am not referring to "foreign invisibles," here, but "neglected invisibles." (They are neglected because they *feel* foreign, not because they *are* foreign.)

Of course, there's also a further point of interest here. The scholarship describing the effect of Boring's intervention on the future of psychology has been instrumental in the emergence of the history of psychology as a specialty within psychology. In particular, what Boring did has since been labelled by so-called New Historians as being normatively bad: he wrote a *biased* history, in the sense that he was driven by his proexperimental agenda to present evidence in a particular way that aligned with his stance in a dispute regarding disciplinary politics (Kelly, 1981). And that's just not how we do things now (following Furumoto, 1989; Young, 1966; see also Brock, 2017). But we can indeed learn from his impact: Boring's history constructed the subject we now take for granted (cf. Danziger, 1990).

In short, therefore, the idea here – in identifying a neglected foreign invisible, then seeking to make visible what can not be seen[10] – is to flip Boring on his head. At the same time, however, I aim to proceed in a way that contemporary historians will accept. Thus: if we recognize that *the present perspective is biased* by disciplinary politics, then *we can look for what's missing from our histories* in the primary sources that informed them (cf. Harris, 1997; Rutherford, 2015; also Burman, 2017). Of course, the goal is still to have an impact. The result is then consistent with the way historians of biology see their contributions to contemporary biology: not as antiquarianism, but rather as deeply relevant to science (e.g., Maeinschein, Laubichler, & Loettgers, 2008; Peterson, 2016). The primary methodological virtue is therefore *contextualism*, rather than *historicism* in a strict sense. And demonstrating that is the purpose of this chapter: by placing a relevant history in context, intellectual historians can contribute to contemporary science without falling prey to presentist cherry-picking.

My Target: Piaget's Hazardous Hypotheses

In what follows, I focus on the most-neglected aspect of Piaget's late theory. He described this as his "hazardous hypotheses" (Piaget, 1980, p. 113).[11] These represent a generalization of his earlier stage theory of child development: why children's reasoning begins with sensorimotor exploration (touch and movement) before it becomes concrete (object-driven) and then only later formal (applying to imaginary objects). But before getting to those details, we must also consider a crucial historiographical question: why should any scientific hypothesis be hazardous to someone of even Piaget's stature?

Briefly: Piaget's late hypotheses breached the boundaries of what was then considered *normal*. Not only did he posit that the same general constructive process is responsible for both evolutionary and developmental change, but he also suggested that chance plays only a marginal causal role. This was in direct conflict with the dominant theory of natural change advanced within biology

[10] Or to make audible that which can't be heard (Burman et al., 2015).
[11] The original phrase, in French, is *deux hypothèses risquées* (Piaget, 1974 (1980), p. 103).

and then imposed on other disciplines (which he dismissed as "mutationism"). Piaget then distanced himself further from the modern synthesis by arguing that neither evolution nor development is random or blind – because organisms don't respond to pressures randomly or blindly. He also proposed to unify discussions of change at the individual level with those of change at the level of populations, for unknowing and knowing biological organisms alike. And he did this by leveraging a concept that had long-ago been ejected from biology: a form of Vitalism influenced by Piaget's (1914) earliest adolescent readings of Auguste Sabatier (1839–1901) and Henri Bergson (1859–1941), then later updated, which he came to call "equilibration" (see Burman, 2016; Gallagher, 1977; Moessinger, 1978).

In his psychological theorizing, Piaget had limited the role of equilibration to serving as the engine underlying the stages of cognitive development. This was then typically paired with *assimilation* and *accommodation*: the processes of bringing stimuli in for interpretation, and of changing the structures through which those stimuli are interpreted when stimuli and structure disagree (discussed by e.g., Piaget, 1962; Piaget & Inhelder, 1969b). But in the mid-1960s, he also – as he put it – "began to doubt the existence of stages" (in Piaget & Garcia, 1974, p. ix). So he reworked his theoretical approach, and updated the logical and biological metatheory informing it (see Burman, 2013, 2016). He constructed new arguments using a combination of intellectual history and philosophical argumentation (Piaget, 1971a, 1979, 1980). Then he clarified the means of equilibration's operation in psychology (Piaget, 1970, 1985, 2001). And he extended it to explain how scientific knowledge develops too (Piaget, 1967; Piaget & Garcia, 1989).

The hazardous hypotheses themselves were tied up with a series of biological conjectures. These are explained most directly in a book entitled *Adaptation and Intelligence*. Thus, for example, Piaget (1980) asserted: "evolutionary transformations of adaptive significance (not, therefore, just any mutation) are closely bound up with new patterns of behavior" (p. 113). He also observed that form and function are highly correlated: "Fishes swim, birds fly, and man himself owes a good deal of his intelligence to his hands" (p. 113). He then offered a chicken-and-egg problem: "Must we accept . . . that all these specialized organs were formed independently from the patterns of behavior that they subsequently entailed?" (p. 114). Or, to put the problem slightly differently: *in causing the origin of new species, could behavioural change precede natural-selective change?* Examining our natural history suggested, to Piaget, that this was indeed the case. In one of his more vivid illustrations, he reminds the reader that fish once moved from the water to the land, incontrovertibly, and that they therefore did something new – very clearly and unambiguously – relative to their prior evolutionary history (p. 115). It's this novelty that then had to be explained.

This was foreign, and so neglected, because it was in conflict with the dominant scientific view: according to the modern synthesis, it is mutations in genes (genotype) that cause changes in bodies (phenotype) that in turn enable the colonization of new territories (adaptive radiation) and divergence through natural selection (speciation). But Piaget (1980) wanted to go the other way: "constructive conduct" (p. 114). And his biological experiments seemed to support the exploration of that possibility: there was more variation in phenotype than there should have been, given the dominant theory, and these variations could enable a different kind of selection at higher levels than those of the genes (Piaget, 1965, 1966). These could then enable phenotypical changes, in adults, that might mimic the effects of genetic mutations before the actual existence of those mutations. Normal natural selection could also then cause those higher-level changes to become fixed in a lower-level evolutionary sense: evolution preceded by behavioural change, and enabled by previously unaccepted developmental plasticity.

This plasticity is now recognized to be an important driver of evolutionary change (West-Eberhard, 2003).[12] But such proposals have also only recently become acceptable as science (Piatelli-Palmarini, in press). Decades before, Piaget's attempts were dismissed (Piattelli-Palmarini, 1980, 1994). His biologizing also didn't fit with psychologists' view of him as a psychologist (Messerly, 1996, 2009; also Ratcliff & Burman, 2017). So the related aspects of his late theory have remained beyond the boundaries of our contemporary understanding, despite attempts to explain subsequent advances on the well-known earlier stage-theoretic contributions to psychology (e.g., Beilin, 1992a, 1992b; Burman, 2013, 2016; Campbell, 2001, 2009, in press; Davidson, 1988). In short: what is most neglected of Piaget's late theory is the fundamental process that unifies evolution and development in the domains of both biology and knowledge – for individuals and groups. Or rather, if you'll forgive me the coining of a term, it informs a still-broader new synthesis: *evo-devo/psych-know* (extending Jablonka & Lamb, 2014).

Piaget's Inheritance from Baldwin

The way in which Piaget developed his generalized view of the process of equilibration involved an engagement with over a hundred years of scholarship: a massive review of material he might use to inform his argument. However, his primary influence in this who is known to contemporary audiences was James Mark Baldwin (1861–1934).[13] Conveniently, Baldwin is also well-known to both psychologists and biologists (see Broughton, 1981; Richards, 1987, pp. 451–503; Valsiner & van der Veer, 2000, pp. 138–176; Wozniak, 2009). So that's an ideal place to start.

[12] The one reference to Piaget, here, is incorrect (West-Eberhard, 2003, p. 707). The proper reference for the work cited is to (Piaget, 1929), to which Waddington (1975) also referred.

[13] Their connection has been well-studied (see esp. Cahan, 1984; Cairns, 1992).

Baldwinian Selection

Baldwin's arguments preceded the modern synthesis of Darwin with Mendel that now dominates the biological discourse. His goal was instead to provide a mechanism to fit in the space *between* Darwinian and Lamarckian theory, but in terms acceptable to Darwinians. He called this "a new factor in evolution" (Baldwin, 1896a, 1896b). The proposal was also necessary because biologist August Weismann (1834–1914) had recently shown that the Lamarckian individualistic view was untenable: only germ cells (sperm and egg) carry information into the next generation, and no amount of determination to change the body could have any direct import (Weismann, 1889).

For psychology, and for social scientists interested in finding an evolutionary role for action and learning, Weismann's findings were problematic: they subjugated the possibilities for future generations to the whims of history, random chance, and the outcomes of competitions far above the level of individual effort (Cravens & Burnham, 1971).[14] They also drew a much sharper line than had existed before between "inherited" and "acquired" behaviours (Johnston, 1995). From the perspective of contemporary biology, however, it was foundational.

Weismann's work was so important in setting biology on its current track that it is today considered the origin of what later became the modern synthesis between Darwin and Mendel (Sapp, 2003, pp. 68–69, 92–94). Indeed, even before this had been solidified as doctrine, Piaget (1993) recognized "Weismannism" as a "neo-Darwinist" hypothesis (p. 40). From this perspective, too, Baldwin's proposals have always been at least a bit unorthodox: in responding to Weismann with a new factor, Baldwin set himself apart from what soon afterward became the mainstream. But what had he proposed? Briefly put: that development can alter evolutionary trajectories.

Baldwin's papers presenting his "new factor" synthesized a series of earlier comments into a single short and coherent narrative. He also began by asking a simple question, which contemporary readers will recognize as also having been of interest to Piaget: "What is the method of the individual's growth and adaptation?" (Baldwin, 1896a, p. 444). He then explained what he meant by this, and gave the sought-after solution a name:

> Looked at functionally, we see that the organism manages somehow to accommodate itself to conditions which are favourable, to repeat movements which are adaptive, and so to grow by the principle of use. This involves some sort of selection, from the actual ontogenetic variations, of certain ones – certain functions, etc. Certain other possible and actual functions and structures decay from disuse. Whatever the method of doing this may be, we may simply . . . apply the phrase, "Organic Selection," to the organism's behavior in acquiring new modes or modifications of adaptive function with its influence of structure. (Baldwin, 1896a, p. 444)

[14] On the influence of Lamarckism on American social science, see Stocking (1962). And on "social Darwinism" more generally, in the United States, see Hofstadter (1944).

In other words, the organism makes “selections” of the causes of its own future actions. Some of these lead to good results, and it is beneficial – in a Darwinian sense – that those couplings between actions-and-results should be repeatable. So the organism selects them more often. The resulting mechanism is then “new,” however, because this “selection” is not made across generations.

Every individual in the population performs this new kind of selection in their own unique situation. In this way, they accommodate themselves to their local situation. The result *seems* Lamarckian because the proposed factor requires only *use* and *disuse.* But it is also compatible with Darwinian theory because such a facility could easily have arisen through natural selection: inherited capacities produce both relevant and irrelevant outcomes, and organisms will derive advantages by being able to select the relevant causes of those successful outcomes.

Still, though, the choice of name wasn’t clear about this. It was therefore soon after renamed “functional selection” (see e.g., Baldwin, 1902, pp. 94, 165–167; 1909, pp. 15–20). Thus, selections are made – naturally, and without outside intervention – in favour of functions that fulfil a need, regardless of their cause. This renamed form of the argument is then also more obviously compatible with the later turn by evolutionary theorists toward genes, even while it applies more broadly than just to particulate inheritance. To wit: *if it works, use it* (cf. Gould & Vrba, 1982, on “exaptation”; see also Gould, 1991; Lloyd & Gould, 2017).

Circular Process

Baldwin’s first paper didn’t provide a mechanism for how this new form of selection might occur. Instead, his argument was grounded in an assumption: “we simply assume what everyone admits in some form, [namely] that such adaptations of function – ‘accommodations’ the psychologist calls them . . . – *do occur*” (Baldwin, 1896a, p. 444; emphasis as in the original). The means by which this might happen was then proposed after reviewing the myriad observations this assumed factor would need to address. That came in the second paper, and he called it the “circular reaction” (Baldwin, 1896b, p. 543).

The circular reaction is not only recursive, but it is also explicitly Vitalist: it is a property of life itself that living things move toward “the good,” and away from “the bad.” This alone is sufficient to drive a kind of change, as Baldwin (1896b) explained: “These [movements] then give renewed pleasure, excite pleasurable associations, and again stimulate the attention and *by these influences the adaptive movements thus struck are selected and held as permanent acquisitions*” (p. 543; his emphasis). The resulting learning could in turn drive evolutionary change in ways that further reinforce this kind of movement, setting up an evolutionary trajectory for ever-increasing acuity in functional selection:

> The intelligent use of phylogenetic [evolutionary] variations for functional purposes [for “the good”] in the way indicated, puts a premium on variations which can be so used, and thus sets phylogenetic progress *in directions of*

> *constantly improved mental endowment*. The circular reaction which is the method of intelligent adaptations is liable to variation in a series of complex ways which represent phylogenetically the development of the mental functions known as memory, imagination, conception, thought, etc. We thus reach a phylogeny of mind which proceeds in the direction set by the ontogeny of mind. (p. 547; his emphasis)

This, however, isn't simply a justification for a role to be played by psychology in evolutionary biology. It is also a deeply biological argument.

The functional selection of even minimally cognitive abilities returns a (formerly dismissed and previously Lamarckian) direction to evolution and development: toward the good, the interpretation of which is in turn afforded by each organism's orientation in the moment. Organisms then explore new environments according to their preferences, and the descendants of those which survive these explorations undergo evolutionary change in ways that are in line with the causes of those preferences – including learned behaviours.

This is an important leap forward, theoretically, especially in terms of finding a place for psychologists alongside biology at the top of the scientific hierarchy. Yet he continued. And the result was a reversal of the biogenetic law famously espoused by Ernst Haeckel (1834–1919), who now has the reputation as the preeminent Continental populariser of Darwinian ideas (Richards, 2008). In other words, Baldwin's new factor reversed Haeckelian recapitulation of evolution in development while at the same simplifying the explanation of natural change. As he put it:

> We thus reach a phylogeny of mind [evolution of mental abilities] which proceeds in the direction set by the ontogeny of mind [including child development], just as on the organic side the phylogeny of the organism [evolutionary biology] gets its determinate direction from the organism's ontogenetic adaptations [embryological and developmental change]. And since it is the one principle of Organic Selection [Functional Selection] working by the same functions to set the direction of both phylogenies, the physical and the mental, the two developments are not two, but one. (Baldwin, 1896b, p. 547)

Thus, physical change and mental change can be understood as being driven by the same circular mechanism. But developmental change need not recapitulate the evolutionary history of the species.[15] Rather, development – especially the further development of intellectual capacities – foreshadows evolution's future advances by opening up new territories in which different inherited structures can become functional, used, and selected-for.

This role for psychology in evolution bears repeating in different terms, because Baldwinian selection is often presented by biologists as a shield *against* the neo-Darwinian pressures that would otherwise be responsible for the production of novel forms following the mutation of genes. (This is also referred to as

[15] This puts Baldwin's view of development in direct opposition to that of G. Stanley Hall (see Green, 2015). We can therefore ask new questions about the role played by disciplinary politics in his ejection from the discipline (see Wozniak, 2009; Wozniak & Santiago-Blay, 2013).

"masking," see e.g., Deacon, 2003, p. 92; 2005, pp. 110, 113.) Briefly, then: genetic predispositions are inherited, but these only *become* functional in specific situations and contexts (cf. Gould & Lewontin, 1979, on spandrels). Because vital movement is then also always toward "the good," according to the organism's perspective in the moment, different predispositions provide different benefits according to where that organism finds itself. And this opens up new possibilities for further exploration at the individual level, which can in turn prompt population-change at the group level: explorers have babies that have babies, in that new territory, and nonexplorers don't (because they're not there).

Genetic Can Refer to the Genesis of Novel Forms, Not Just to Genes

The circular reaction will be familiar to those who have only read Piaget (esp. 1952, 1954, 1962). Yet this is too specific an inheritance, and Piaget's version was – for most of his career – much narrower than Baldwin's (which extended even to aesthetics). Indeed, in reflecting later on Baldwin's influence, Piaget (1982) said that he considered "the global idea of genesis" (p. 83) to be the most important among the senior man's influences. And because both authors found themselves outside of the dominant biological theory, this is what they both meant by the term *genetic*. This is then also in turn why their writings are broader than the contemporary evolutionary epistemologies proposed and espoused since the modern synthesis: neither Baldwin nor Piaget was concerned with the application of "blind variation and selective retention" as a metatheoretical method, but instead were interested in the growth of knowledge as a natural extension of human development itself (see discussion by Apostel, 1987; Hooker, 1994; Kesselring, 1994; Vonèche, 1985). And both offered similar processes to explain that growth.

Circular Becomes Cybernetic

The circular reaction was, for both Baldwin and Piaget, the general engine of adaptation. However, the historian must assume change as a function of method; to do otherwise is now considered "ignorant" (Teo, 2013a, p. 842). Knowing this, we must look for it. And so we find it in the later works that I find especially interesting: the version of Baldwin's circular reaction that Piaget used in his late theory was provided by Waddington, whom Piaget appreciated especially for his "cybernetic approach" (Piaget, 1979, p. 47; also Piaget, 1971b, p. 93; 1971d, p. 50).

Waddington's cybernetics are discussed across multiple volumes (see esp. his 1977). However, the version that Piaget chose to cite in the discussions of his hazardous hypotheses was presented in *The Strategy of the Genes*; in the same chapter, in fact, in which Waddington (1957) explained his now-famous epigenetic landscape, which he entitled "the cybernetics of development" (pp. 11–58).

Both of these concepts – epigenetics and cybernetics – are tied together, in Waddington's discussion that Piaget cited, and thus one cannot be considered without the other. Contemporary discussions typically omit the cybernetic aspects, however, so this aspect of the history of development under evolution can also be considered a neglected invisible.[16] And because Waddington showed clearly how the modern synthesis needs to be updated to include development, providing a review is consistent with my goal of contextualizing the extended new synthesis that's coming. So I will go through the relevant material in more detail than I might otherwise for something that's better-understood.

The chapter begins with a very simple discussion of development, in its embryological form, picking up from Weismann and starting with the process whereby a fertilized egg differentiates into distinct parts: "regionalisation" (p. 11). Once separated, these parts begin to change in character: "histogenesis" (p. 11) or, alternatively, "physiogenesis" (p. 12). And then these cell lines become recognizable as body-parts, which continue to develop: "morphogenesis" (p. 11). In other words, a set of undifferentiated cells of shared origins grows into separate specialized cells, organs, and eventually into a single body composed of many distinct and yet interrelated parts. Thus, collectively, these processes are describable as "individuation" (p. 13). The challenge is then to explain how the obvious and easily observable discontinuities emerge. This is the realm of epigenetics (pp. 13–14). And its fundamental problem is akin to that of genetics; namely, that of explaining speciation as *the emergence of novelty* (p. 14).

The first challenge in addressing this fundamental problem, in embryology, is that of resource-allocation: if genes are to produce their functional substances (proteins), then these substances must be formed of raw materials. The rate at which production can occur, at different sites in the cell nucleus, is then a function of competition with other production processes *and also* of the concentration of these raw materials (p. 16). The future state of production is therefore driven not only by an initial inheritance,[17] but also by all production past and present. Modelling this change during development then requires the recognition of "a 'feed-back' mechanism" (p. 17) such that those organs better able to

[16] The neglect isn't universal (see Gallagher, 1977). But I find it interesting that, rather than being historically informed, the essays that include both epigenetics and cybernetics instead typically take the form of philosophical argumentation (e.g., Molenaar & Raijmakers, 2000). This is problematic: authors who make that connection without incorporating a strong intellectual history risk appearing anachronistic, or disconnected from their lineage, even while their theoretical advances seem to be directly in keeping with the intent of the original sources. Indeed, the connection Waddington (1957) made between cybernetics and open systems theory (p. 24) is also consistent with the contemporary interest in dynamic systems by developmental psychologists (see e.g., Boom, 2004, 2009; Garcia, 1999, 2000). But such contemporary extensions are easily dismissed – for their apparent presentism – even though their narratives could have been framed in such a way as to make them more historiographically acceptable.

[17] Of course, just recognizing this type of inheritance – "cytoplasmic inheritance" (see Sapp, 1987) – is already a problem for the modern synthesis. And as Sapp (2003) notes, it was intentionally ignored; omitted from consideration on purpose (p. 114).

acquire resources from the cellular environment will also be better able to compete for those resources in the future. And this in turn leads to an "exaggeration of initial differences" (pp. 16, 19).

The second challenge is that of "canalised paths" (p. 19). This reflects the observation that organ development can be perturbed, even dramatically, and yet still produce functional outcomes. In other words, there seems to be some sort of regulatory compensation at work. And its degree varies, such that some organisms can be pushed further than others – to the point of a threshold – before there are major functional consequences downstream (p. 20). Here, though, Waddington lamented the lack of precise mathematical tools to describe these processes formally (pp. 22–23). But he cited Ross Ashby (1903–1972) as a potential source, who is today recognized as a pioneer of cybernetics.[18] However, he also pointed to Ludwig von Bertalanffy (1901–1972) for the application of such maths to the "open systems" (p. 24) found in biology.[19] It is then in this sense that Waddington meant the phrase "developmental pathways" (p. 26).

A developmental pathway, for Waddington, is an abstraction from a multidimensional surface in "phase space" (p. 27).[20] It is this that then affords the meaning of his famous illustration of the "epigenetic landscape" (p. 29). He illustrated this with a three-dimensional drawing, in which an organismic marble can be seen following different developmental paths. Briefly: the marble can follow only certain paths and end up with a functional set of organs, but these paths are also themselves regulative: "if while the system is moving along a certain trajectory it is pushed slightly out of its course it will tend to compensate for this disturbance and to reach eventually the same end state as it would normally have done" (p. 30).

Of course, this was not intended to be a rigorous explanation of development. The epigenetic landscape's hills and valleys are tools for thought: their depth and breadth are useful for providing a mental picture of regulative plasticity (pp. 30–31). The bottom of the valley represents the normal equilibrated state under the represented developmental conditions. This can then be described with reference to Ashby's homeostat, except – because the system is dynamic – Waddington introduced the term "homeorhesis" (p. 32). And that implies flexible stability under changing conditions; a gyroscope, rather than a thermostat.

A further term to reflect this equilibrated process, but for the valley rather than the developing organism that travels through it, is "creode" (p. 32). Thus, for example, the Haeckelian theory of recapitulation can be understood as applying to the creode rather than to the organism: previous genetic change that alters the timing or shape of the present epigenetic landscape (p. 33). The

[18] On Ashby, see Pickering (2010, pp. 91–170).

[19] Piaget also cited Bertalanffy, but – in his biological writings – preferred Paul Weiss (1898–1989). On their connection, see Drack, Apfalter, and Pouvreau (2007).

[20] The simplest explanation of phase space that I know was provided by Gleick (1987).

depth of a creode then represents "the intensity of its homeorhesis" (p. 34), which can also be referred to as its "homeorhetic cross-section" or "canalisation cross-section" (p. 34).

The simplicity of the epigenetic landscape betrays the complexity of the underlying network of influences (pp. 34–35). It therefore has the potential to misrepresent the functional collectivity of production processes, both genetic and biochemical. And of course interventions typically affect multiple variables simultaneously (pp. 36–37).

Waddington was careful to remind the reader that his illustration is an oversimplification: projecting a multidimensional object in three dimensions risks drawing equivalencies between different types of stress (p. 38). There is also a great deal of imprecision in the processes being modelled: "developmental noise" (p. 39). This can be reflected in the representation of the creode: a relatively flat bottom implies "adults which vary somewhat around some mean value" (p. 40).[21] But it can also be reflected as imperfections in the sphericity of the organismic marble itself: a more chaotic pathway can be represented by including bumps on both surfaces, according to whatever is suggested by the relevant experimental results. He then suggested some additional related terms, although without going into further detail: "developmental stability," "stabilising selection," and "selection for repeatability" (p. 41). These seem to me to be veiled references to Baldwin, but he didn't make that explicit in this chapter.[22]

He did, however, continue with "matters of terminology" (p. 41). Indeed, he focused on ensuring clarity for systems in equilibrium. As he explained, returning – implicitly – to connect his reviewed terms with the cybernetic and systemic ideas provided by Ashby and Bertalanffy:

> The Greek word [homeostasis] . . . does not seem very apposite in this context, since there is nothing static about development; we are not dealing with the maintenance of a steady state but with the attainment of some particular end-state in spite of temporary deviations on the way there. (p. 42)

He continued:

> The main argument against adopting the term "developmental homeostasis" arises, however, from the fact that the evolution of a canalisation is, in some ways, antagonistic both to genetic homeostasis and to physiological homeostasis. The more narrowly canalised is the development of a character, the less will changes in gene frequency come to phenotypic expression, and the less will be the tendency to genetic homeostasis. (p. 42)

Continuing to use the same term with contradictory meanings at other levels then produces statements that are "highly paradoxical" (p. 42), resulting in "confusion" (pp. 42, 43). This is then the justification for his insistence on "canalisation" and "homeorhesis" (p. 43). He also responded to objections:

21 One is tempted to render this in probabilistic terms. However, that is properly Gottlieb (1999, 2007) rather than Waddington (1957).
22 This connection is made clear in an earlier essay (Waddington, 1953).

> The main objection, which might justly be urged against the term "canalisation" is that it is derived from an analogy or metaphor, in which three dimensions are used to express the properties of a system which really involves a multi-dimensional phase space; it may therefore suggest too concrete an image to be suitable as a name for the abstract quality to which it refers; but this seems a less important failing than those involved in the alternative term homeostasis. (pp. 43–44)

He then distinguished between physiological homeostasis, developmental homeorhesis, and genetic homeostasis. Homeorhesis thus provides the dynamic connection between two otherwise fairly stable systems, as a function of its level's greater plasticity. And so we can think of the individual's history being written into their body (during development), just as their ancestors' histories were written into the distribution of their inherited genes (by evolution).

The epigenetic landscape itself, however, is explicitly the representation of a formalism (p. 49). Even if the facts were different, he explained, the model itself would therefore retain its usefulness: "systems of essentially the same formal properties might be produced by processes of quite a different kind" (p. 49). This is then part of what seems to have appealed to Piaget, who reworked his own formalisms several times (Burman, 2016). Indeed, Piaget (1979, pp. 44, 54) cited Waddington's (1957) illustration on page 56, which – at first glance – I initially mistook for a neural network of the sort that had been examined by the Genevan cybernetics team (in Cellérier, Papert, & Voyat, 1968). Reading carefully, however, we see that it is a demonstration of how the same genotype can produce different phenotypes as a result of changes in the regulation of its components (cf. Hinton & Nowlan, 1987).

Impact on Piaget

Waddington's impact on Piaget was enormous: their interactions led to a complete reworking of the biological metatheory underlying Piaget's psychological theory (Burman, 2013). However, this seems disproportionate: very few others had such a singular impact on the late theory that we can't see (cf. Burman, 2016). So we must put their connection in context before examining what it meant for Piaget's hazardous hypotheses.

Piaget met Waddington in Geneva at a meeting hosted by the World Health Organization (WHO) that was held in 1964 and then followed up with a second meeting in 1965. As Piaget soon afterward recalled of his first impressions:

> in the course of discussion about the regulations of development, he made a very profound comparison between epigenetic construction and a progression of geometric theorems in which each is rendered indispensable by the sum of those preceding it, though none is directly derived from the axioms underlying the original one. (Piaget, 1971a, p. 14)

He continued:

> The comparison of epigenesis with a progressive mathematical construction comes home to us all the more forcibly because the growth of elementary

> logico-mathematical operations during the ontogenesis of intelligence in a child raises the same problem of preformation or epigenetic construction as that which forms the basis of discussion about causal embryology. (Piaget, 1971a, p. 14)

This, as it turns out, is a critical generalization of the formal model represented by the epigenetic landscape. And it is indeed at the crux of Piaget's hazardous hypotheses: the same formalism can be used to describe embryological development, children's mental development, and the history of mathematics (see Burman, 2016).

Until we find all of the relevant archival documents, we can take direction only from Piaget's report. And this is very brief. Still, though, it is useful in situating what came after. Thus, for example:

> There are, as I think we established at Geneva in 1964 during our symposium on developmental regulations, three main factors in organic growth: programming by the genome, environmental influence, and equilibration or autoregulation factors. The two last are, properly speaking, neither hereditary (since they impose themselves *motu proprio* in terms of situations) nor acquired from outside (since internal regulation is involved). (Piaget, 1971a, pp. 35–36)

Piaget then grouped himself with Waddington, whom he identified as "the embryologist," against the objections of an unnamed physiologist:

> At the conference just mentioned, one leading embryologist seemed to adopt my point of view, though saying he needed time to think about it, whereas a famous physiologist openly expressed disagreement, pointing out that regulations or equilibrations are the direct expression of the causal interactions involved, each of whose elements are either predetermined from the genome onward or acquired under environmental influence. (p. 36)

He continued:

> The physiologist was probably right as far as his own field was concerned, because it is a fact that homeostatic regulations do not contain the necessary regulatory organ. . . . But the embryologist and even the psychologist (myself) were perhaps right, too, the former because he was thinking of epigenetic growth, . . . and myself because I was thinking of cognitive functions. (p. 36)

The regulatory organ found in embryology and cognitive development – but not found in physiology – was for Piaget *equilibration*, but reconceived as "homeorhesis" in Waddington's (1957) terminology: dynamic regulative flow. Indeed, from then on, Piaget cited Waddington in this connection.[23]

Piaget concluded his report on the conference by picking up Waddington's reference to Bertalanffy. This, though, was treated in levels in an open system:

> Here, then, is the conclusion of this summary of our guiding hypotheses. The living organization is an equilibrated system (even if one avoids the term and

[23] Lacking the relevant archival documents, the strength of the connection can be shown by tracing it through published sources (see e.g., Piaget, 1970, p. 710; 1971a, pp. 19, 23–25; 1971c, p. 47; 1971d, p. 49; 1972, p. 60; 1973, p. 52; 1985, p. 4; Piaget & Inhelder, 1969a, p. 122; also in Evans, 1973, p. 8).

> substitutes Bertalanffy's "stable states in an open system"). But this organic equilibrium only represents a relative sort of stability in those very fields where it is best protected. The genome is isolated to the maximum degree from its environment, although it cannot be so completely; its equilibrium is nevertheless upset by mutations, etc., despite these ideal conditions. The epigenetic system is more open, but it finds its equilibrium by means of a number of processes, among them homeorhesis. Physiological systems are even more "open," and yet they react by homeostasis of the interior environment – an environment all the more remarkably stable as the various animal groups are evolved and differentiated. The role of the nervous system is to be open to external stimuli and to react to them by means of its effectors. . . . Finally, behavior is at the mercy of every possible disequilibrating factor, since it is always dependent on an environment which has no fixed limits and is constantly fluctuating. Thus, the most highly stabilized equilibrium forms found in any living creature, namely, the structures of intelligence, whose logico-mathematical operations have been of inescapable importance ever since human civilization reached the stages of being consciously aware of them. (Piaget, 1971a, pp. 36–37)

In other words, Piaget's appeals to Waddington – and their joint appeals to Bertalanffy – have the effect of connecting the stages of children's mental development to the levels of physical development and evolution that Waddington had identified (see also the illustration in Waddington, 1959b; extended by Burman, 2013, p. 369).

Constructive Conduct

Piaget's hazardous hypotheses used this updated view of equilibration to connect all of these levels under one constructive process. Thus: when operating in a hierarchical multilevel biological system, a disequilibration – or "perturbation" in Waddington's terms – is addressed initially at the level of greatest plasticity: individual behaviour. It then affects all of the lower levels in turn, but in a particular way.

An unexpected discovery first affords a learning opportunity. That's Baldwin. If the necessary lesson isn't learned, then the perturbation alters morphogenesis in development: the body changes in ways that reflect the unaccommodated pressures. That's Waddington. And then, if still unresolved, the pressure can become life-threatening. The extent to which that life-threatening pressure is experienced differentially by the population, the following generations' distribution of genes will accommodate it by means of natural selection. That's the modern synthesis of Darwin and Mendel.

The result of this feedback is that an earlier higher-level developmental accommodation can become evolutionarily fixed: a change in genes, but preceded by changes in development. But that's not all. Changes in morphogenesis can also mimic the effects of mutation: due to developmental change within a plastic reaction range, a normal bodily phenotype is altered to accommodate the extant pressure. This can then make it appear as though the abnormal phenotype has copied what would normally be the result of a genotype ("phenocopy"). And this abnormality can then itself be reproduced at the level of the genes – and thereby made normal – as a result of natural selection ("genocopy"). However, the use of these copy-terms seems to have caused more problems than the ideas they represent.[24] So I will instead follow what I think so impressed Piaget when Waddington mentioned the parallel to mathematical construction.

A Mathematical Illustration of Constructive Conduct

The transmission of pressure from one level to another is problematic under the modern synthesis. Yet the metatheory that guided Piaget was clearly different. Indeed, his metatheory assumes a hierarchy of levels that can interact under certain conditions (Burman, 2016). And the way that makes the most sense to me for how this occurs, of all the possible ways I've seen discussed, is to refer to the French approach to long-division that I learned in elementary school.[25]

In maths, as in the levels approach to evo-devo, action is taken at the first level of plasticity. The remaining selection pressure affects each lower level only to the extent that the problem remains unresolved at higher levels. Thus, for example: dividing 3 by 2 gives 1 with a remainder of 1, which is carried over to the next level beyond the decimal (i.e., "the next decimal place"). This then gives 10 divided by 2; or 5, with no remainder. And that in turn provides a complete resolution of the problem. At the higher level, before the decimal, we have 1; at the lower, 5. Hence: 3 divided by 2 equals 1.5. No further action is required; there is no further pressure to solve the problem.

If the first level is – metaphorically – that of Waddington's (1959b) exploitive system, then the second can be treated as if it were the epigenetic level. So the change posed by this problem occurs at those two levels alone: once resolved, there is no remainder to carry over to still-lower levels. The resolution, 1.5, is perfectly adapted to the pressure applied by the problem of dividing these two numbers.

A slightly different resolution arises by dividing 2 by 3. This remainder is never resolved, and it continues to be transmitted to the next level: 0.666 . . . forever. This lack of resolution is not only a problem for the individual mathematician, who could be forced into imprecision by rounding the number off, but it is also problem for the set of *rational* solutions. (A new, higher-level concern above the level of the individual mathematician.) That, however, can be resolved by a different kind of response. We might call it a new invention above Waddington's exploitive level: colonizing a new conceptual territory to explore, which will then be inherited by future generations of mathematicians who can build upon it (cf. Burman, 2013, p. 369).

[24] For an edifying discussion of the meanings of "phenocopy" that also has no connection to the possible confusions which Piaget may have introduced, see Oyama (1981).

[25] Special thanks to Mme Shortliffe at the *École publique John Fisher* in Toronto for having taught these lessons so effectively that they would remain close-to-hand even decades later.

Such numbers are still *rational*, but they are distinguished from integers by being called *fractional* (⅔). The further invention of a new convention also enables the repetition after the decimal to be represented by use of a symbol: $0.\overline{6}$. Both of these resolutions then resolve the problem of division in a new way that is totally satisfactory. No further action is required, and new kinds of operations can be undertaken without loss. (Although ⅓ + ⅔ = 1 is probably to be preferred over $0.\overline{9}$, for the sake of elegance and clarity, even though they are equivalent by convention.)

More complicated numbers, such as *irrationals*, cannot be easily represented fractionally. A classic example is the long edge of a right-angle triangle. Dividing this long edge (Plato's *hypoteínousa*) by one of the short edges also produces remainders that continue forever, but – unless the triangle is *special* – these decimals are nonrepeating. This prevents the use of the invented-symbol that can refer to repetition. And, again, that poses a problem both for the individual mathematician and for the mathematical system used to provide solutions: the long side is incommensurable relative to the other two sides, in the sense that there is some loss in making the comparison. Therefore, another new invention is required to resolve the pressure.

Here, we find in history something still-more complicated: Pythagoras' theorem ($a^2 + b^2 = c^2$). Of course, this then also required the further invention of a symbol in order to represent the length of the triangle's problematic long side: $c = \sqrt{a^2 + b^2}$. Yet now, as a result, we have new conceptual territories to explore. This constructive process of exploration, disequilibration, and accommodation then continues: every unresolved problem encountered under the presently extant mathematics produces a new invention. (Soon after referring to Waddington's comparison of epigenesis and maths, Piaget (1971a, p. 15) mentions the invention of the *imaginary* number, i, such that $i^2 = -1$.) This is because the remainder – the unaddressed pressure – must always be accounted for without loss. And the order of the operations that produces both the problems and the solutions provides a constructive lineage of epistemological species: from simplest to complex, but without the problematic Lamarckian baggage.

Note, though, that *any* resolution provides a *response* regardless of whether the *solution* provided is actually *correct*. Indeed, from this perspective, corrections must come from outside the functional system in order for the appropriate adaptation to occur: feedback from the teacher to the student, perhaps, or further selection pressure from the immediate environment (such as a new proof). For students, though, this in turn affords a further teachable moment: highlighting the original problem's reversibility (*1.5 * 2 = 3*) and thereby encouraging reflection at a higher level of abstraction than the operations originally used, and thus also a greater understanding of the structure of mathematics itself.

For most organisms, of course, the further interaction is usually not so "constructive." Mostly, unresolved pressures mean they die. But that's Darwin and Mendel, and the rest follows clearly from the combination from Baldwin and Waddington. Indeed, from this perspective, Piaget's arguments do not seem at all controversial. The first level of responding to perturbing pressures is always behavioural, and the second level is always learning. Piaget's further proposal is simply that we carry this insight down through the hierarchy of levels that his colleagues in adjacent disciplines had talked about in more detail: if a learned response is insufficient, then the remaining pressure will affect morphogenesis and eventually gene distributions.

This is ultimately why *behavior* is, for Piaget (1979), the *motor of evolution*. Of course, the translator didn't render the original French title of that book – *Comportement Moteur de l'Évolution* – in this way, but that was a mistake.[26] To reiterate: for neo-Darwinian evolution to occur, populations must encounter perturbations that have no resolution at any level higher than that of the genes. Or, to put it another way: for the distribution of genes to change, the organisms must be exposed to sufficient selection pressures to kill off a significant proportion of their group's membership. Of course, that same observation doesn't apply to adaptation. This occurs at a higher level, but following the same process. And so too, proposed Piaget, do the advances made by scientific development in response to found-remainders and the exploration of new territories (see esp. Piaget & Garcia, 1989).

Rejections

None of these proposals were immediately accepted. Baldwin's functional selection was only recognized as a contribution to evolutionary theory half-a-century later, and two decades after his death, when it and a number of related proposals were jointly called "the Baldwin effect" (Simpson, 1953). This then itself went largely unappreciated until the 1980s (following Hinton & Nowlan, 1987). Now, though, it is considered an important part of the evolutionary story, with self-proclaimed "Baldwin boosters" serving in support (see Weber & Depew, 2003). But it is still not part of the story of development. Today, that role is instead played by Waddington.

Of course, Waddington's (1953) version of the Baldwin effect was also dismissed at the time. Yet he recognized the problem that a dissident approach would have, and actively defended against a Lamarckian reading of his work. The result was then a way to achieve *the goals* sought by Lamarckians, and by Baldwin, but by more acceptable means (Waddington, 1959a; see also Crispo, 2007). Still, though, it is only recently that Waddington's proposals have been accepted as providing the basis for a new synthesis (see e.g., Laubichler & Hall, 2008).

[26] The choice to render it instead as *Behaviour and Evolution* was perhaps intended as an allusion to Roe and Simpson (1958), who had indicated that providing an acceptable explanation for adaptation was the "central problem" of evolutionary biology (p. 338; also Simpson, 1958, p. 521).

Piaget's synthesis of Baldwin and Waddington has since been called "the baby in the Lamarckian bath" (Deacon, 2005). But in its time, like Baldwin and Waddington, it was also dismissed. And for similar reasons. This is clearest in the discussion following his debate in 1975 with Noam Chomsky (b. 1928) and Jerry Fodor (1935–2017). Yet even the constructive comments from his supporters there focused on a point of possibly confused terminology – related to phenocopy versus genocopy – rather than his larger substantive point (see Piattelli-Palmarini, 1980).

Briefly put, this is: without exploring the unknown, organisms cannot encounter pressures to which they aren't already adapted. The structures that must cause their explorations cannot therefore become perturbed (disequilibrated), and therefore they cannot be altered except by mutation. This then ignores the developmental role played by accommodation, and the reequilibration of constructive conduct, thus also it dismisses the evolutionary usefulness of functional selection and the construction of a broader basis for action in response to the demonstration that one's structures are functionally incomplete (discussed by Burman, 2016). By contrast, accepting the responsiveness of the equilibratory mechanism across levels – and of homeorhesis – enables the updating of Baldwin's original arguments: it provides a ratchet not only for developmental and evolutionary change, but also for epistemological change. Without it, too, there's only chance and luck. And then novelties can be unravelled as easily as they are formed; there is, as another theorist put it, no "generative entrenchment" (see Wimsatt, 2007).

Conclusion

My examination here has been of neglected arguments from the past that also have the potential to advance contemporary thinking, especially regarding the new synthesis of evolution and development. What is especially interesting is that the neglected aspects relate equally to embryology and mental development and scientific change. They are also supported by strong formalisms. In other words, the neglected invisible here is an argument regarding the processes of natural change in the human realm – biological, psychological, and epistemological – that is supported by decades of evidence but also goes beyond this to incorporate strong theoretical supports drawn from logic and biology.

This in turn affords an interesting set of observations about the doing and teaching of history, for which we might refer to the historians who are known to have been influenced by Piaget – especially Thomas Kuhn (1922–1996), whose last unpublished book is thought to contain an evolutionary-developmental argument (see Marcum, 2015; also Burman, 2007). But because space is limited, I would prefer to end with a caution related to the notion that prompted the chapter: the perceived relationship between evolution and development, and how disciplinary politics shape how we perceive the past. In this, I am particularly concerned by the lingering influence of Ernst Haeckel (1834–1919), whose doctrine of recapitulation still looms large (see Koops, 2015).

After all of this, we can consider Haeckelian recapitulation as a kind of maturationist view: evolution comes first, and then development repeats the stages that were previously discovered. Yet this has since been reversed by the epigenetic arguments presented by Baldwin, Waddington, and Piaget. And so we can expect the extended new evo-devo/psych-know synthesis will replace Haeckel with a more constructive dissident; perhaps even by his Jena colleague, William Preyer (1841–1897), whose *The Mind of the Child* (1889–1890) influenced so many of the early developmentalists who are now celebrated.[27]

Recognizing the politics that advanced Haeckel over Preyer, even in psychology, enables us to see critical comments made by others. Piaget, for example, was explicit in rejecting recapitulation:

> the child is more primitive than *any* adult, including prehistoric man, and that the source of knowledge lies in ontogenesis [individual development]. Any adult you choose, whether cave man or Aristotle, began as a child and for the rest of his life used the instruments he created in his earliest years. Consequently, in the field of knowledge – I'm not generalizing to every field – ontogenesis is basic. I would say that it's more primitive than phylogenesis [species evolution]. (in Bringuier, 1980, p. 92)

Piaget also made this point in a letter to Steven Jay Gould: "psychologically, the child explains the adult more than the reverse" (qtd. in Gould, 1977, p. 146).

Indeed, this seems to have emerged early in Piaget's approach, although the connection to Baldwin (1895) is not clear. As he explained in his inaugural lesson at the University of Neuchatel fifty years before: "Let us guard against returning to the simplistic idea of a necessary parallelism between the development of the race and that of the individual, a parallelism which biologists have shown to be equivocal and conjectural" (Piaget, 1925, p. 204; translated by Kitchener, 1985, p. 6). But it is also important to note explicitly, because otherwise such discussions can be easily misunderstood (pace Oesterdiekhoff, 2012, 2013, 2016).

To be clear: according to this neglected view, the development of children does *not* recapitulate the stages of evolutionary progress in the human species. Nor does the development of knowledge recapitulate the stages of cultural evolution. Instead, developmental change *precedes* evolutionary change: novelty is the result of exploration, not mutation nor effort nor creation.

Of course, this was all once nearly unthinkable as a thing to suggest: Baldwin's contributions were ignored for decades, Waddington was harshly criticized, and Piaget's remain neglected. It may still be hazardous to raise them

[27] For example, G. Stanley Hall (1846–1924) wrote the foreword to the American edition of Preyer's book: "Among all the nearly fourscore studies of young children printed by careful empirical and often thoroughly scientific observers, this work of Preyer is the fullest and on the whole the best. It should be read by teachers and parents even of older children, as the best example of the inductive method applied to the study of child-psychology" (Hall in Preyer, 1889–1890, p. xxiii).

outside of the protected confines of the emerging new synthesis. Time will tell: if I get tenure at my home institution, in the Department of Psychology, then we'll know the hazard has passed. Regardless, raising these issues is a big part of what I think the role of intellectual history *should* be.

Intellectual histories that don't take these suppressions into account reinforce the biased present in ways that are consistent with the political past. This politicized view then becomes normalized; the outcome made *obvious* in retrospect, and at the same time evermore entrenched. Yet this, I propose, is a more dangerous and pernicious form of presentism than that which typically concerns historians of psychology. Or, more properly, it is an encapsulated presentism: a reflection of an historical moment, closed off from examination and carried forward *as if* exemplary. For this reason, I propose, such encapsulations *require* investigation (e.g., Rutherford, 2015). If we don't look to the past with a critical eye, and put pressure on past interpretations now understood to be problematic, then we accept its impact – blindly and unthinkingly – on our own possibilities for the future.

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