

CONSTRUCTIVE HISTORY

From the standard theory of stages to Piaget's new theory

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Abstract

This project demonstrates how Historians of Psychology can contribute to the future of Psychology from within the Department of Psychology (rather than from departments of History, the History and Philosophy of Science, or Science and Technology Studies). To do this, I focus on the claim that Jean Piaget's last works constitute a "new theory," while also showing how this labelling was appropriate. This is discussed briefly in the introduction. The first chapter is also quite simple: it follows the turn toward "locality," and uses autobiography to show why a psychologist might want to pursue advanced training in history. This approach is then reflected in the second chapter, where Piaget's autobiography is used to situate what followed in his own studies. The third chapter reflects this at an again-higher level, comparing an American history of Piaget's biography with a Genevan history (but augmented with new archival research). In addition to revealing new details about his life, this also highlights a difference in historiographical sensibilities at work in shaping the discipline. The fourth chapter then shows that this generalizes. It reviews the most famous case of an instance where a series of texts were "indigenized" during their importation into American Psychology (viz. Titchener's importation of Wundt). To confirm that the same thing occurred with Piaget, I introduce a new technique inspired by the Digital Humanities. In short: I show in quantitative terms acceptable to Psychologists what Historians would be more inclined accept from a study of primary sources. Two examples of this more-traditional kind of history are then presented. In chapter five, I consider a change in Piaget's appeals to a formalism associated with Kurt Gödel. In chapter six, I look at how this change informed

Piaget's return to biology (and his subsequent updating of the Baldwin Effect). And the conclusion re-examines the original claim in light of everything else discussed. The ultimate result, though, is not only a new way to consider Piaget's standard theory of stages. I also present a new way to understand his broader view of the development of knowledge. This also in turn informs a new way of doing history, presented in the Appendix.

Dedicated to my wife

LAURA

to my sister

JACQUIE

to my nephew

GREGORY

and to my parents

MARGOT & TONY

who have each, in their own way, helped me more than they know

Acknowledgements

A reflection. That's what this is; of all those who have helped or influenced me. And I don't just mean this section. The entire project reflects different interactions with different people on different topics, in different places, and at different times. Indeed, they have shaped the results that you now hold. Granted, the beginnings of the project are discussed in more detail in Chapter I. But that's just because it was necessary there to walk readers through my own journey from Psychology to History—in case they hadn't yet taken that journey themselves. Here, though, I can be more personal.

The work that informed this dissertation was conducted with a cohort of graduate students at York University in Toronto. First inspired by interactions with Cathy Faye, Jason Goertzen, Kate Harper, and Paula Miceli. Then in collaboration with Laura Ball, Ryan Barnhart, Marissa Barnes, Jenn Bazar, Elissa Rodkey, Kelli Vaughn, and Jacy Young. And then later with Arlie Belliveau and Eric Oosenbrug, along with my teaching assistants Ben Zabinski and Dan Lahham. They all played a role in the development of these ideas (or related thinking upon which subsequent work was based). Sometimes they also served as foils, but all were always supportive. And I am grateful to each of them, separately and together. (Our Historiography Breakfast meetings were especially valuable.)

Of course, it was the professors who often set the agenda that we explored. I will talk in some depth about two earliest influences—Jordan Peterson and Jan Sapp—in Chapter I. But the explorations that informed this project, after they had made their mark, were guided primarily by my doctoral advisor: Christopher Green.

Chris makes fun of me sometimes for “wooing” him when I was still a master’s student in another department. But I’m very glad I did it. The resulting apprenticeship was extremely gratifying: we created a blog that attracted hundreds of thousands of readers (*Advances in the History of Psychology*), we explored—and helped to construct—a new field of inquiry (the digital history of psychology), and we created a laboratory that has now published more than a dozen articles (the PsyBorgs Lab).

Chris deserves a big thank you for supporting my journeys to all of the places I thought looked interesting or relevant, both intellectual and geographical. And so too do my other committee members: Thomas Teo and Fred Weizmann, who himself also provides some continuity from my MA in Interdisciplinary Studies. I am grateful for that continuity too, especially since this project both extends and transcends that earlier work.

The other full-time faculty in the History and Theory Area in the Department of Psychology, along with Chris and Thomas, are also due both credit and thanks: Mike Pettit and Alex Rutherford. Together, they provided the context in which I developed. (Mike also served as Dean’s Rep for my MA, and then chaired my dissertation defence.) Of course, several other faculty from friendly areas and departments contributed to my formation outside of the regular coursework: David Jopling, Martin Fichman, Bernie Lightman, Juan Pascual-Leone, and David Reid, as well as Anton Yasnitsky and Marga Vicedo from the University of Toronto. But two in particular must be marked out for special thanks.

I served as Stuart Shanker’s teaching assistant for five years, and he taught me a great many things. Key among them is the absolutely critical importance of making all

research *relevant*. These lessons came primarily through my service in various capacities at his research institute: first as research assistant for three years, then as associate director for one year (during which time I helped him to create the Canadian Self-Regulation Initiative), and ultimately as research director for a final year. I learned a tremendous amount. And furthermore, from another perspective, he paid for a lot of what follows. I will be forever grateful.

An opportunity arose, at the end of the project, for me to take up a position in Geneva as an assistant to Marc Ratcliff in the Piaget Archives. This followed an invitation to speak at the FAPSE Centennial in 2012, as well as a ThinkSwiss Research Scholarship from the Swiss Embassy in 2014. And I was delighted to join his team.

Marc is a rare scholar: he is fluent in a half-dozen languages, has PhDs in both developmental psychology (with Montangero at Geneva) and the history of science (with Bynum at UCL), and is a genuinely caring and supportive person who is interested only in doing good and interesting work with good and interesting people. He has built a fabulous team, and am grateful for their support. (I am especially thankful to Ariane Noël, and also Nathalie Delli-Gatti, whose support and friendship made my life in Geneva possible.)

I am looking forward to working with this team for many years. And I am also glad to call Marc a friend.

There is also a wider circle of friends of colleagues without whom this project would have been much more difficult. Most notably, however, are those at the Jean Piaget Society (which is not fan club, but is rather “the society for the study of

knowledge and development”).

JPS is my home-conference. This is partly for the subjects discussed at its meetings, and its audience of interested participants. But its stalwart members are the main reason. Chris Lalonde, Ashley Maynard, and Eric Amsel have become good friends. But I am also glad for such supportive and generous colleagues such as Mark Bickhard, Tom Bidell, Jan Boom, Nancy Budwig, Robert Campbell, Jeremy Carpendale, Brian Cox, Colette Daiute, Ayelet Lahat, Cynthia Lightfoot, Ulrich Mueller, Larry Nucci, Bill Overton, Pete Pufall, Geoffrey Saxe, Elliot Turiel, Abel Hernandez-Ulloa, and Phil Zelazo. And, of course, I must also thank and recognize Michael Chandler and Jeanette McCarthy-Gallagher as extraordinary mentors.

My work has been supported over the years by several sponsors, funders, and awards. It is my privilege now to recognize them, and thank them. I was recruited into the doctoral program in Psychology in part through a York Graduate Award in 2007, although I had not yet completed my MA. This was followed by two awards from the Jean Piaget Society: the Pufall Award in 2009 and the International Emerging Scholars Award in 2010. (The latter was provided formally by the Jacobs Foundation.) In 2011-2012, I was supported by an Ontario Graduate Scholarship. In 2012, I was honoured to be named a finalist for the Anne Anastasi Graduate Student Research Award by Division 1 of the American Psychological Association. In 2013, I received the Ambassador Gary J. Smith Award for research with an international focus. And in 2013-2014, I was supported by the Norman S. Endler Research Fellowship and the Pierre Elliott Trudeau Fellowship.

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Selections of what follows have appeared in print elsewhere, although the versions here have typically also been substantially rewritten. Part of Chapter II is under review for an upcoming special issue of *Spontaneous Generations*, a slightly longer version of Chapter III was published in *History of Psychology*,* Chapter IV is slated for inclusion in a special issue in French that is being put together by my group in Geneva, Chapter V is in press at *Theory & Psychology* (and was itself reworked from a draft included in my MA thesis), an abridged version of Chapter VI was published in a special issue of *New Ideas in Psychology*, and the Appendix is in press at *Estudios de Psicología: Studies in Psychology* (and was co-authored with Marc Ratcliff). I am grateful to the publishers for permission to include these essays here, as well as to the cited archives for permission to quote from their collections.

Chapter III draws the most heavily from archival sources. The material there is from the Harvard University Archives, for which permission to quote was sought and granted. That said, however, the revision of Chapter V and the expansion of Chapter VI would not have been possible without sustained access to the Piaget Archives at the University of Geneva. Nor would the Conclusion have been. Other published works

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include reference to material housed at the Cummings Center for the History of Psychology at the University of Akron, and I am presently developing new works drawing on material from the Yale University Archives and the Rockefeller Archive Center.

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INTRODUCTION

Piaget's new theory, and how to find it

“PIAGET’S NEW THEORY” IS fascinating (see esp. Beilin, 1992b). The label itself is certainly a part of its draw. It is suggestive of future possibilities; of advances on ideas regarding child development that had their greatest societal impact in the West during the widespread and dramatic education reforms that followed Sputnik (Bliss, 1995; Herman & Ripple, 2002; Hsueh, 2005; F. B. Murray, 1992; see also Marchand, 2012; Stendler, 1965). But it’s also exciting because the works behind that label remain largely unknown to English audiences: there is indeed something there to be discovered, if only one could understand the texts (see esp. Smith, 2009a).

That the appellation exists at all is actually kind of wonderful. It suggests that those who can read the French originals might be able to build directly on the legacy of one of the Twentieth Century’s most prominent theorists. This would then have implications for all of the areas that Piaget influenced at the height of his popularity in the 1960s and ‘70s: education and developmental psychology, of course, but also philosophy and psychiatry too (see Almy, 1979; Beilin, 1992a; J. S. Hall, 2000; Lovell & Shayer, 1978; F. B. Murray, 1979; Voyat, 1977).

Still, that the New Theory continues to be “new” nearly forty years after the

author's death, in 1980, has always struck me as peculiar. There seems to be something implied about contemporary Psychology by the fact of its constituent works' unremarked-upon existence. And maybe also something about Psychology's relationship to both its own history and History as a discipline. So that's what this dissertation is about: Psychology's connection to its past, but using Piaget as a source of concrete examples and evidence. Its contribution is therefore also to those two areas: how we "do History," in Psychology, and how contemporary psychologists might understand Piaget better as an historical subject.

In Chapter I, though, I do little more than introduce myself, the project, and how I got here. In short, it's autobiography: motivation. But it's also a necessary disclosure of potential conflicts of interest, and—perhaps surprisingly—it's methodologically important. This is because "localities" are becoming increasingly significant to historians of science (see Chambers & Gillespie, 2000; Shapin, 1988-2007/2010). And, indeed, that interest is beginning to influence the history of psychology as well (e.g., Carson, 2007, 2014; Green, in prep; Teo, 2013b). Thus, it plays a role here too: *everything is situated*, including the narrating historian (i.e., *me*).

In Chapter II, I therefore introduce Piaget by referring in part to *his* autobiography. But the narrative there is presented with the recognition that the subject is already a very well-known person: much of what is discussed amounts to little more than a review of others' scholarship, reflected through the prism of Piaget's own self-presentation in material that he wrote himself. I push on this a little bit, of course, but not hard.

This is intended to be a gentle start. It's in Chapter III where I actually start to "do" History in a more serious way: I refer to more complex primary and archival sources, fill in some gaps in an existing published peer-reviewed study, and then compare that study to another that's not accessible in English. Note, though: this comparison wasn't done because my doctoral program has a language requirement. It doesn't. Instead, I did it to set up what follows. In short: I tried, in a general way, to make a specific problem clear—why it could be that there's a New Theory—so that we could begin to think about that problem in more general terms.

The problem, briefly put, is this: our perspective is skewed by how we are situated in our own context. The view of contemporary psychologists is therefore not just incomplete, but bent in a particular direction. Indeed, following the turn to focus on "localities," this can be put in slightly different terms: the meanings of foreign texts are reshaped to fit local interests (cf. Callon, 1986). In other words, texts are "indigenized" as they are moved (see Danziger, 2006; Pickren, 2009b). And the result is that the "foreign" remainder is omitted, dismissed, glossed over, mangled, or neglected.

Chapter IV shows using some simple quantitative tools that this occurred during the importation of Piaget's ideas into American Psychology. But first it reviews how such an argument could be accepted by contemporary Historians of Psychology. This is necessary because, in the History of Psychology, quantitative argumentation is itself often considered to be a "foreign" approach. (Historians don't infer; they demonstrate, usually by citing original primary source texts.)

Accepting this, the quantitative demonstration is used simply as a formalism

illustrating a more general argument. The actual structure of that argument is provided by a history: I review the process whereby early Historians of Psychology realized that a similar claim could be made regarding the importation of Wilhelm Wundt's writings during what is now considered to be the generally-accepted birth of Modern Psychology as a discipline.

The importation of Wundt's work into American Psychology is a special case, with real rhetorical value, because the histories delving into it originate in the professionalization of the History of Psychology itself. Indeed, what I will call the "Wundt Argument"—that changes occurred in what Wundt meant, as his work and influence were carried by Titchener from Leipzig to Cornell (and from there to the rest of the United States)—is foundational in a way that will be hard to dismiss as "foreign." This is very useful in setting up what follows after: the formalism then helps simply to show that the Argument itself can be generalized, and it is therefore presented in the simplest possible terms.

The structure of this generalization is important. Otherwise, the rhetorical move might seem like a sort of sleight of hand. So I want to be clear from the outset.

After laying out the Wundt Argument, I refer to a quantitative analysis by one of the early professional Historians of Psychology. This showed that certain of Wundt's works were overlooked during the importation of his works at the founding of Modern Psychology. (As he put it, "the *Völkerpsychologie* fared poorly" [Brožek, 1980, p. 106].) Following this, I then show how newer methods can demonstrate the same thing—in more detail—of Piaget's works. Piaget thereby provides a second instance of the

“indigenization process” that occurred with Wundt’s works. And this suggests a more fundamental process, underlying both importations, which I will call the “Indigenization Argument.”

Briefly put: when meanings move between localities, changes are both inevitable and demonstrable. Wundt was changed, and so was Piaget. They were “Americanized” (see e.g., Rowland, 1968; cf. Rieber, 1980b, pp. 147-149). That then affords a second—very different—instance of really “doing” a history of Piaget. Indeed, it shows explicitly *why* there is a New Theory.

Let’s put this in the simplest possible terms: there is a “new theory” because there is a “standard theory” (see e.g., Beilin, 1989b, pp. 95-101; 1992a, pp. 192, 197-198, 202; 1992b, p. 10). In other words, there is something that is generally understood of Piaget. This even has sufficient technical vocabulary attached to it—“accommodation,” “assimilation,” “conservation,” “constructivism,” “equilibration,” “stages,” etc.—that specialist dictionaries were produced by insiders to help readers parse those meanings (see e.g., Battro, 1966/1973; Legendre-Bergeron, 1980; also Montangero & Maurice-Naville, 1994/1997). But changes in these technical definitions have remained largely unexamined; readers simply assume they understand, because the later meanings overlap with others already known from earlier works.

In short, the “standard theory” is represented by the “Piaget” referred-to in textbooks. This is also the “Piaget” to whom Vidal (1994) referred in choosing to entitle his early biography *Piaget before Piaget*. To wit: there was a man (in Vidal’s case, a boy) before there was a legend. And yet it is this legend upon whom even the highest-quality

secondary sources and collections typically focus: “the famous developmental psychologist” (e.g., M. Chapman, 1988a; Gruber & Vonèche, 1977/1993). As a result, divergences from this known character—including even those that the man himself later undertook as “one of the chief ‘revisionists of Piaget’” (Piaget, 1968/1970, p. 703n)—*appear* “new” to those who can’t see them as having arisen from within their own local context.

This contrast is wonderfully revealing, especially in terms of how it helps us to look at the later works that we don’t presently understand. But, from all of this, we also derive an explicit contribution by History to Psychology: as with Wundt, the “Piaget” we see is not the Piaget who was. There’s a lot more to know.

The resulting efforts are not “postmodern,” however, as some commentators have suggested dismissively (e.g., Kose & Fireman, 2000). Such approaches simply reflect different aspects of what it means to do History as a specialist of the history of psychology: the proper application of the right tools and methods, in engaging with primary sources, *to see what cannot be seen from the present vantage*.

Following the articulation of this contribution, and History’s role in making it, the goal in the last chapters is then to begin to deliver on the implied promissory note. Thus, Chapters V and VI chase specific opportunities afforded collectively by Chapters II, III, & IV. The intent, though, is not to present the New Theory itself. This has already been ably done, and the results have remained invisible (e.g., Beilin, 1992b; Davidson, 1988; also Acredolo, 1997; Bickhard, 1997). Thus, instead, my intent here is simply to make those “foreign invisibilities” easier to see (cf. Burman, Guida, & Nicolas, 2015).

After this, the Conclusion wraps everything up. But it does so with a twist: it engages critically with the means by which the New Theory was originally identified, by tracing the original sources that influenced the secondary literature that applied the “new” label. As a result, it seems to question what could be perceived as the foundation of this very project. Yet it ends by returning to where we begin in what follows in Chapter I: how it is that History has a contribution to make to Psychology, and the importance of localities in understanding meaning. The Appendix then, in turn, shows how the resulting approach could also make a contribution to History itself: I return to Piaget as a biographical subject, and use the dissertation’s larger epistemological contribution—the identification of levels in Piaget’s later works, replacing the stages of his earlier works—to propose a new kind of history.

CHAPTER I

Locating the study

A DOCTORAL DISSERTATION IS finished three times: once when the proposal is approved and the candidate can see the eventual manuscript in their mind's eye, a second time when the relevant conference papers have all been presented and the candidate's colleagues can see the eventual manuscript in their minds' eye, and finally when the candidate abandons the envisioned manuscript and hands in something simpler. This final step seems crucial. It is not a defeat, though; it is a milestone. Understanding the difference is the last lesson every doctoral candidate must learn: *the dissertation is not the book.*

I wish I had figured this out sooner. But the speed of my progress through York's doctoral program in Psychology is certainly not because no one told me. (The same words were spoken while I was finishing up my MA in Interdisciplinary Studies: "*Save it for the book!*") Their meaning must simply have passed me by. And that, in fact, is partly what this dissertation is about: the difference between words and meanings—texts and contexts—but with specific reference to how Jean Piaget (1896-1980) has been understood as belonging to the history and future of psychology.

Of course, I have benefitted tremendously from what I have heard of what I was

told. This project would not have been possible without the advice, guidance, and support of a huge number of people. (Many of them are named in the Acknowledgments.) At the same time, however, the project itself is also the culmination of a much longer personal journey that can be traced especially to three influential moments during my undergraduate training in psychological science at the University of Toronto (Hon.B.Sc 2004).

These three moments set up the story that follows by answering a simple question: “*How did I get to the History of Psychology?*” This then seems like a useful place to start, here, because my goal is ultimately to show how others with a similar background—those interested in scientific psychology, including even those whose interests incorporate the brain¹—can, should, and will want to undertake a similar journey. It also reflects an important aspect of my larger argument: *just as Histories must be put in context, so too must be the Historians who present them.*

To achieve this goal for the project as a whole, this chapter moves through three partially-overlapping institutional localities: the undergraduate program in Psychology at the University of Toronto (1999-2003),² the master’s program in Interdisciplinary Studies

¹ The neuroscientific version of the argument that follows is developed in (Burman, 2012b; 2014.)

² I actually started in Commerce and Finance (B.Com), but that didn’t last. Because I was also working in the tech industry at the time, I was curious to know why dot-com businesses with no revenue were worth so much. (Recall that, at that time, even small companies with no prospect of profitability could be worth billions on paper.) But the professors in the area dismissed my questions as meaningless: “Those investors are irrational, and thus irrelevant—a fool and his money are soon parted.”

I didn’t find this answer very satisfying. And since that was my life, the question didn’t seem meaningless. So I asked them who studied those kinds of questions. The inevitable answer: “I don’t know; maybe psychologists?” That’s when I decided to pick up psychology as a minor, although I ultimately left finance entirely.

The only professor from that time who had any continuing influence on me was Dr. Andrey Feuerverger. His first-year Statistics of Stock Markets seminar was a revelation: using the big UNIX cluster to model trading strategies against historical data, and then defending the outcomes as non-random, was a

at York University (2005-2008),³ and the doctoral program in the History and Theory of Psychology also at York University (2007-2014).⁴ They provide the context to make sense of my current position, which—to my surprise and delight—now finds me working at the Piaget Archives in the University of Geneva, Switzerland (2014-present).⁵ More importantly, though, they also lay out the process of my discovery of the value of History for Psychology.

Of course, it goes without saying that Geneva is the ideal location from which to continue my research. (This is where his papers are!) But I will not talk here in detail about the Piaget Archives, or what I have found there. That's for the future. My goal here is to consider the past, so we can see the future more clearly. Indeed, this is what I consider to be the primary role served by History in Psychology (which is distinct from a History of Psychology that exists outside of the Department of Psychology).⁶ Everything

fantastic introduction both to computer-assisted quantitative reasoning and the deeper philosophical requirements of hypothesis-testing. I hope one day to return to that original interest, but armed with new tools. (My first original research project was on exactly this topic, developing an argument—in response to his prompting over several years of mentorship—that we ought to model the market as if its participants could be located along a boundedly-random continuum of irrationality: “The development of the self as an economic actor” [unpublished].)

³ The foundation for my graduate work was also strengthened immeasurably by my time as an associate producer at the Canadian Broadcasting Corporation (2004-2006). And it was necessary to stay, after I was accepted into the graduate program at York, because I received no funding in the first year of my MA. But because this was an experience unlikely to be shared by other developing psychologists, it distracts from the story that I wish to tell. It therefore won't be discussed further.

⁴ I withdrew in 2014, in good standing, to complete my dissertation without the burden of tuition fees.

⁵ This move was first supported by the Swiss Embassy through the ThinkSwiss Research Scholarship. The Fondation Jean Piaget then provided a supplementary bursary, to help defray the cost of living in Geneva. My research is presently supported by a grant from the Fonds National Suisse, subside n° 100011-146145.

⁶ This split also seems to parallel the divide in History between internalism and externalism. Typically, externalist histories look outward to put things in the context of their social, political, and cultural times. Internalist histories, then, do the reverse: they typically focus on the subject matter to the exclusion of context. This, then, is a challenge for historians of psychology because psychologists—quite reasonably, it seems to me—would prefer that “their historians” speak about “their subject.” And the

here reflects that interest, and therefore only select aspects of my research output from recent years will be discussed.

Articles intended as contributions to a History of Psychology that could exist outside of the Department of Psychology have been published elsewhere (e.g., Burman, 2012b, 2012d, 2014). So have pieces intended to establish my credibility in the broader fields to which I would like to contribute as a Professor of Psychology (e.g., Burman, Guida, et al., 2015; Burman, Green, & Shanker, 2015; Green, Feinerer, & Burman, 2013, 2014, 2015a, 2015b; Matthews, Burman, & Murtha, 2014; Nicolas, Andrieu, Croizet, Sanitioso, & Burman, 2013). Yet these all relate to this project only indirectly, so their results will not be examined here in detail.

What this project is, ultimately, is a history of Piaget. But it is a particular kind of history. It is not about the man (biography), nor is it really about his ideas (intellectual history). It is also not about the social, political, or cultural contexts in which the man lived or his ideas were conceived (externalist history). Rather, I am interested in *how we have understood Piaget*. This departs from the disciplinary norm, but it does so in ways that I intend to be both useful and revealing.

Such an approach could be structured as a “public understanding of science” (e.g., Grant, 1998; Rutherford, 2009). Yet again, though, I take a different path; one closer to “knowledge translation” (see Kitto, Sargeant, Reeves, & Silver, 2012). But it is also not an examination of translation in a strict *linguistic* sense (e.g., Jurczak, 1997; Smith, 1981,

present fashion among historians is for the reverse. There are many ways to address this conflict. The approach taken here is one of them. Others are discussed in Chapter 4.

2009a). My interest is not in *errors*, but in *differences*. The project is therefore about *us*, in a sense, and—especially—our relation to older scientific ideas that may still have important continuing implications for new research, practice, and policy (cf. Carson, 2007, 2014).

In pursuing *difference*, my discussions will typically adopt two perspectives: *what we know* (as well as how and why), and *what we can't clearly see* (as well as how we see and why). These two perspectives are then brought together here through a larger consideration of what it is that Historians of Psychology are doing *in Psychology*. In other words: the project is an examination of the doing of the History of Psychology, *for Psychology*, but using histories of Piaget to make the surrounding discussions more concrete.

It has been a long journey. Learning the history of psychology takes time, because there is a lot of material to cover. But learning the History of Psychology⁷ is more complex still, because—in addition to the content—it has its own additional disciplinary norms and values. Surprisingly, however, this place where I now find myself is very similar to where I first started: I have found to my delight that history can be as rigorous as the best of scientific psychology. It's just different. As a result, it seems clear that there are real contributions to knowledge to be made by History to Psychology. We need only to see them.

⁷ This capitalization—“history of psychology” and “History of Psychology”—is intentional. The intent is to follow Graham Richards' (2002) distinction between little-p psychology and big-P Psychology: the subject (content) and the discipline (form).

1. Historical developmental psychology

So, then: how did I get to History? It started in an unusual place: in my introductory course to Developmental Psychology at the University of Toronto, in the Winter semester of 2002. The professor suggested as one of our essay options that we compare and contrast a contemporary study with an historical one.⁸

This is not, strictly speaking, History. In most cases, the results will be influenced too much by the contemporary view (“presentism”). So let’s instead be clear about the impact that this had: had it not been for the resulting experience, I would likely never have sought out any higher-level historical courses. Also, I did not choose immediately to write about Piaget. (This is not a love story.)

Piaget’s name was no more meaningful to me then than any of the other names mentioned in Marty Wall’s big Intro course at Convocation Hall. His was just one among many, and I wasn’t so clear about those either. (When I first chose the major, I remember vividly that an upperclassman teased me when I mispronounced Rorschach as sounding more like “Dvorak,” whose name is known either as a classical composer or the inventor of a high-performance computer keyboard, according to the audience.) Piaget’s name was also associated with some spooky language that seemed not to make sense in light of anything else I had learned to that point (e.g., assimilation, accommodation, etc.). Thus, instead, for no reason in particular that I recall—except perhaps because I wanted to

⁸ This puts it at the middle of my third year of undergraduate studies, not long after I had decided to leave Finance for Psychology. My justification for the change was that, if I was going to understand the crash of the dot-com stock market bubble (in which I had participated and about which I felt quite strongly), then I would need to understand something about human behavior and decision-making. This was reinforced later that year by the awarding of a Nobel Prize in Economics to Daniel Kahneman, a former UBC psychologist who had moved to Princeton in the mid-1980s.

begin at the beginning, and had read about him in Richard Dawkins' and Daniel Dennett's popular writings about evolution—I chose to focus on Charles Darwin's (1877) biography of his infant son, William Erasmus Darwin (1839-1914), as mediated through his notebooks of observations and several intervening decades.

Given what I knew then about the elder Darwin (viz. that he had “discovered” evolution), it was surprising to me that he had written something that was both psychological and developmental. William (“Doddy”) was also conceived at around the same time as his father's ideas about Natural Selection, and so the son seemed to me to be the lesser of the two contributions. Still, though, Charles was both a famous naturalist and a new father. His observations of his son's development would therefore be interesting regardless of the issue's ultimate historical importance.⁹ And it would meet the requirements of the paper. Thus, it was decided: Darwin.

The library's then-new website showed that the university owned only one copy of the biography. This was held in the Thomas Fisher Rare Book Library, which is located at the base of the neck of the famously turkey-shaped Robarts Library. So that's where I went.¹⁰

After the porter relieved me of my knapsack, a librarian sat me down in the reading room. It was dimly lit, with booked-lined walls that reached far overhead; more a

⁹ I became more positive about children once my nephew was born.

¹⁰ Of course, I now know that this is an article and not—as the library website still says today—a book. I also now know that the piece had been reprinted several times (e.g., Darwin, 1877/1971, 1877/2010). At that time, however, I knew nothing. More important, however, is that the larger lesson also didn't come from the words themselves; rather, it came from the interactions inspired by the material object that provided form to the content. Put differently: the more important meaning, for me, was not found in the text. It arose from the context.

cavern—or a small cathedral, for the worship of knowledge—than my mental image of a library. (There were no flickering fluorescents overhead, nor any chewing gum long ago ground black underfoot.) The books themselves were unexpected too: ancient tomes of various sizes, some of which were so large that they needed to sit flat on special oversized shelves the size of dinner tables. Then the librarian brought me the biography, although I should probably say, “The Darwin.”

I expected a photocopy, not an historical artifact. I wasn’t there to “do History.” I was just there to pick up an old pre-scientific document so I could critique it; to tear it up (figuratively). I’m certain that I even asked her to make me a copy, rather than risking damage from my handling the original.

“No,” she said, to her credit. “You have to do this properly.”

The pages were almost too delicate to touch: they were brittle to the point of being crispy. They were also translucent in places, presumably where others had touched them with oily fingers. But I was committed to leaving The Darwin in as good shape as I had received it. So, as the librarian watched from a nearby alcove, I decided that I should use a piece of acid-free paper—if such a thing existed—to turn each page as gently as possible without leaving a mark.

When I asked at the desk, she had some prepared for me. It seems to me now that she had probably been waiting with it, ready to swoop in if required, but wanted first to see what I would do before intervening. I returned to my carrel with the papers; a pilgrim with a passport.

While I sat there, in my first encounter with History (and wondering what to do

next), I noticed a distinctive smell. It was quite pleasant, and tasted in the air almost like vanilla. This was completely unlike the off-putting chemical smell of the glue in the new journals made available for review in Robarts' and Gerstein's Current Periodicals rooms. Electronic journals had also been introduced, of course, but the coverage was spotty and the websites poorly-constructed. As a result, it often seemed simpler in those early days to go browse in person. Besides, nobody had wifi yet, and very few of the university's rooms had Ethernet sockets to plug-into. I also usually didn't know what I was looking-for, so thumbing through the volumes was often the way to go: scholarship by accidental discovery. But not with *The Darwin*.

When I finally figured out how to turn the pages without bending or even touching them, and started reading, I was surprised by what was written. Darwin's report wasn't at all like the other texts that I had read for that class, or any other. But there were similarities.

He engaged with an earlier published study. He spoke of behaviors that he had observed at different ages. And he concluded from his observations that the earliest of these behaviors must have been the result of inherited reflexes. He was also very careful in attributing to his son the adult labels for emotions and cognitions, when it was not at all clear that those were what his son was experiencing.

This was not a foreign thing, as I expected from the way in which I had come by it. It just seemed overly informal. It was a baby-biography, rather than a scientific study of child development. But it also seemed more cautious, in some respects, than many

more-modern texts that I had read.¹¹ For this reason, and also because it was not possible to dismiss Darwin as “ignorant” for failing to follow contemporary practices (Dawkins and Dennett had presented him as biology’s Greatest Genius), I was forced to consider a different perspective: *according to whose standards should this text be judged?*

This was almost certainly not what my professor intended. And, in fact, I do not remember what I wrote in my essay. (Nor do I remember the grade it received.)¹² These don’t matter, though, for I had been gifted a much more important lesson: a practical introduction to historical investigation, guided by a trusting and forgiving librarian and inspired by a proper scientist of developmental psychology. I was thus launched, but not in the usual way.

History was not for me a way to *escape* scientific psychology. History was rather a means of *engaging with* the discipline’s lost and forgotten territories; a way to look for treasures that might have been overlooked by the discipline’s map-makers (see e.g., Keegan & Gruber, 1985; Lorch & Hellal, 2010). History was a way forward, even as it looked back.

2. Seeking the meaning of ‘meaning’

The second influential moment came the following year, in the Winter of 2003, in

¹¹ I later learned the name for failing to heed this caution, but it’s worth saying it directly now: Darwin was careful not to commit what William James (1890/1981) soon afterward called “the psychologist’s fallacy” (i.e., the replacement of a subject’s reported experiences with one’s own). Note, though, this could easily be equated with a general version of historians’ admonition against “presentism” (i.e., the replacement of an historical context with a contemporary one). Because this is then shared by both disciplines, it seems uncontroversial to suggest that the lesson is therefore important. Regrettably, however, it is also one that many psychologists—including even developmental psychologists—now seem to miss: *children are not adults in miniature*.

¹² My transcript shows a B for the course, which is pretty good. But it’s not awe-inspiring.

a graduate seminar that a favorite professor—Jordan Peterson—invited me to audit. He called it, simply, “Maps of meaning” (following the title of his then-recently published book [Peterson, 1999]).

The topic was systems of organized belief in human thinking, from a partly neuroscientific perspective, but examined anthropologically through myth and world literature. It was also without most of the infrastructure usually carried over into such discussions from the philosophy of science. But we did read Thomas Kuhn’s (1962/1993) *Structure of Scientific Revolutions*. And Kuhn had written about how he had struggled to understand Aristotle. That caught my attention: *this guy had had a similar experience as mine, a year before, when I had been trying to understand Darwin!*

Kuhn’s reasoning was straightforward: this ancient philosopher, Aristotle—who had been so influential that he was referred-to for centuries as, simply, “The Philosopher” (Ackrill, 1981)—had been wrong about so much that modern physics had shown to be the case. But it was not possible to dismiss him as *ignorant*, because his stature was greater even than Darwin’s. Aristotle’s wrong-ideas were therefore things to take seriously; the ignorance was *ours* (of his reasoning), not *his* (of scientific discoveries made long after his death).¹³

Kuhn decided that he needed to figure out how and why what Aristotle had said made sense at the time as a thing to think; how Aristotle’s knowledge claims were justified, given what was believed at the time. And this basic insight seemed more

¹³ I learned later that holding historical people to present-day standards combines two historiographical sins: “presentism” and “anachronism.” But I didn’t know that at the time. And no one told me. So it was an important (personal) rediscovery.

important, to me, than the specific details of his resulting theory regarding the organizational role played by “paradigms” in the advancement of science and their “revolutionary” replacement after the accumulation of “anomalies.” Indeed, Kuhn’s organization-of-perception story could be interpreted in many different ways (e.g., Fuller, 2000). And that, I think, was ultimately my professor’s point in sharing the material: Kuhn’s writings are useful, to psychologists, as things-to-think-with (see e.g., Peterson & Djikic, 2003; Peterson & Flanders, 2002; also Peterson, 2013).

In retrospect, this also applies to the course itself: it is not its *content* that now seems to have been most important to my formation. Rather, it was the course itself that was valuable; a sustained interaction, but exemplified by a single moment.

I remember specifically that, at one point, my professor stopped mid-sentence during an extemporaneous lecture. He interrupted himself to say that Kuhn’s paradigms-and-revolutions were inspired in part by Piaget’s studies of the stages in children’s cognitive development. This, then, was an opportunity: one could make a better thing-to-think-with if the underlying influence could be clarified. But there was a problem, and a reason why no one had done the work.

“It’s too bad,” he said, “that no one here speaks French and could explain to us what else Piaget said that could still be useful.”

He then remembered the idle banter from one of our lab’s breakfast meetings (which were also a real highlight of my undergraduate experience). He turned to me, with an expectant look.

“You speak French, don’t you?”

That then became my mission: to find, understand, and explain the seemingly impossible connection between Piaget's view of children and Kuhn's view of science. But my focus at the time was primarily on Kuhn, rather than on Piaget. Kuhn was the one who had influenced my professor's perspective, and thus had also shaped the experiments that we were doing in his lab. And Kuhn was the one who had had a similar experience to mine at the Fisher Library. In addition, I found his attitude—the serious treatment of things that don't make sense—very appealing. It was therefore Kuhn who guided my work.

I followed our course-readings from *Structure* with my own study of *The Essential Tension* (Kuhn, 1977) and the then-still-new *The Road Since Structure* (edited by Conant & Haugeland, 2000). I also chased an unpublished book hinted-at in interviews and other essays, which Kuhn had tentatively titled *The Plurality of Worlds: An Evolutionary Theory of Scientific Discovery*. Failing to get a copy (it had been removed from the archive that held it), I then also read quite a lot more at the intersection between evolutionary biology and developmental psychology, since both Kuhn and Piaget had seemed to me to lean in those directions.

These readings led to my first scholarly publication: a book review, arranged by my professor, in which I appealed to those same biological ideas in order to simplify a new advance in psychological theory which suggested that perception and cognition ought to be conceived as being more deeply situated in acting, feeling bodies: “enactivism” (Burman, 2006). But, again, I learned more than content as a result of the effort.

The commitment required of writing for publication surprised me. Even a finished course paper is little more than a draft of what an editor will accept for peer review. And I had no idea that there could be more than one round of review. Or that this further scrutiny, after a revision, could still produce a rejection letter.

This has now happened to me a handful of times, of course, and I consider “responding to rejection” to be an important part of *the work* of this avocation. Indeed, the material that appears here as Chapter V was reviewed, revised, resubmitted, re-reviewed, and then rejected by six different journals before being accepted for publication in the seventh.

For the book review, though, all of this editorial heavy-lifting was done internally. And the experience prepared me for what would come later: my professor made it clear that a first piece is only a “first” piece if it is followed by a second, and a third. A fourth is then reflective of a pattern—hinting at the possibility of a proper program of research—and, in any case, after doing all of that the writing itself is no longer so daunting. He said much the same thing about drafts too: a “shitty first draft” is just a place to start, after which everything is made easier. (He was referring, I think, to Lamott, 1994.)

Still, it took me a long time to figure out what my shitty first draft was supposed to be about. After reading all of the Kuhn that I could find, I turned to Piaget. Unfortunately, though, he had written a lot more—in French—by at least an order of magnitude. (His collaborators pushed the number of potentially relevant primary sources above two orders of magnitude beyond Kuhn’s own daunting output, most of which I still

haven't finished reading.) Worse, so much of it also seemed unrelated to my interests. The secondary sources didn't help much either, because what my professor wanted me to do hadn't been done. There were only hints of something unseen, not the clear roadmap that students come to expect from textbooks.

I just couldn't find a way forward, aside from more reading. It was a grind, with no real end in sight. But then things were helped along immensely by the publication of an article to which I could respond directly. The sub-title makes its relevance obvious: "Piaget vs. Kuhn on scientific progress" (Tsou, 2006). In short: I got lucky.

This article took my seemingly-infinite problem and reduced it to a handful of specific individual points—on fewer than two-dozen pages—that I could engage one-by-one. I was then also able to reorganize my readings, and focus on what had been missed in this new piece that struck me as most important.

This gave me an easy shitty first draft. And that became my second scholarly publication (Burman, 2007b). As I worked on it, though, the larger project itself also began to change: questions regarding "maps of meaning" and "architectures of belief" became questions regarding how educational systems had used Piagetian ideas to encourage children to hold justified beliefs (Burman, 2008a). And then this new question was formalized and generalized, but using Kuhnian terminology, so that the underlying issues could be engaged more rigorously: *how might a fractured epistemic landscape of partially-conflicting beliefs be encouraged to unify?* (Burman, 2009a; following discussions with a senior doctoral student in the York HT doctoral program [Goertzen, 2007; 2008]).

In other words, I had undertaken a philosophical project using historical sources having special relevance to psychology and education. Yet, aside from my exploration at the Fisher Library, this was at first totally uninformed by any proper or sustained training in historical method. And that was a gap that needed to be addressed: reading a lot and speaking French are both necessary to develop a deeper understanding of Piaget, but neither is sufficient.

There was also a further problem: I had hated my first properly “historical” course, and it nearly ruined me for History forever after. I was saved only by my early inoculating experience with Darwin, as reinforced afterward by Kuhn. I therefore clung to Kuhn as someone who seemed to know what History was good for. Of course, this then itself became something of a frustration to my doctoral advisor, when we met later, because my lasting apparent-allegiance reflected a psychologically-orthodox—but not an especially historicist or contextual—reading of Kuhn’s works (see e.g., Green, 2004b; also Green, 2015b, pp. 2-3).¹⁴ But this can now be put in terms that are consistent with the larger argument of this dissertation: I had not yet learned how to use historical sources to leave my present-context. I was still deeply situated in the background provided by my experience and training, and was struggling to find ways of seeing beyond it.

¹⁴ A further observation regarding the importance of locality and personal interaction: the author to whom Green responded, Erin Driver-Linn, had previously studied under Jordan Peterson—the very same favourite professor who had inspired my own project. He had moved from Harvard, where she had had him as an advisor in 1995, to U of T in 1999. Thus, his sphere of influence moved with him. And, indeed, so did that of my first mentor in history; I met him at U of T soon afterward, and then we moved north to York. It is there that I then met Green, and wooed him—“vigorously,” he says—to serve as my doctoral advisor. I also met Fancher, recently retired from the York doctoral program, who had written the dozen-pages about Piaget that helped me to see my project in the first place.

3. Finding history

I took my first course on the history of psychology at the same time as my introduction to Developmental Psychology: in the Winter of 2002. But the course was nothing like my encounter at the Fisher Library. In retrospect, though, it provided an excellent example of what *not* to do when teaching the course: the “history of psychology” is not identical with the doing of “History of Psychology” (see Note 7).

In his lectures, the professor—a scientist who shall remain nameless—presented reviews of infamous mistakes by famous dead people who were all ignorant in different ridiculous ways. This comedy of errors was then reflected on the tests, for which we were required only to memorize a seemingly unending series of names, ideas, and dates. The tests themselves then consisted of drawing lines between the trivia across different lists presented in columns. This seemed to me like it would have been very unpleasant to grade, since there was no meaning to engage-with, but I’m pretty sure that I once saw a TA with a stack of acetate sheets: if the lines on those maps overlapped with what had been written on the page, then the “history” represented was deemed “correct” by geometric comparison.

It was, in short, a nightmare. I doubt I did very well, and am disinclined to check.

That said, however, the course wasn’t a complete waste of time: I was exposed to several interesting historical writings, including by some proper Historians of Psychology. Among them was a deeper and more rigorous discussion of Piaget than what had been presented in my other textbooks (Fancher, 1996, pp. 425-436). And this was

eventually quite helpful. But, again, it was not sufficient.¹⁵ Much more important was what happened after I completed the requirements for my degree: an opportunity to participate in another graduate course, but in History. That was utterly transformative.

The University of Toronto has a fantastic post-graduate system for recent academically-inclined alumni who have not yet been accepted to graduate school. New degree-recipients can register for a year of additional courses as a non-degree “special student” without needing to have all of the required prerequisites, so long as the professor gives permission. This meant that, with my degree in hand, I could fill-in some of the larger gaps in my education without first having to decide on a specific trajectory. (I still thought at the time that, because of my work experience, I might do an MBA and maybe become some sort of investment banker [see Note 2].)

The opportunity couldn't be passed-up. So I took a neuroscience “wet lab,” held in adjoining autopsy rooms at the U of T Medical School. My goal there was to understand better what it was that we were talking about when we attributed “architectures of belief” to the brain. And this quickly disabused me of the notion that brains are like machines in any way except metaphorically. (I had some experience repairing and upgrading desktop computers, and this course helped me to realize—in a very hands-on way—that there is nothing in a human brain that is remotely like the

¹⁵ Returning to those dozen pages later—and rereading my own marginalia (including the comment, “This is crap!” [on p. 435])—led me to reconsider Piaget, and my project, from a slightly different perspective: both Kuhn and Piaget were interested in the development of justified knowledge-claims, as well as how these knowledge-claims develop and evolve, but they used different methods in undertaking their enquiries. Piaget's investigations into how children's thinking changes could therefore be understood as seeking similar insights as Kuhn's historical examinations of how science changes. And that, in fact, was exactly the subject of one of Piaget's later books: *Psychogenesis and the history of science* (Piaget & Garcia, 1983/1989).

insides of a computer: anatomical areas blur together, things aren't always in the right place and can sometimes even be missing, and the convenient color-coding of machines and textbooks is of course completely absent in the wetware.)¹⁶ Indeed, from that point on, I wondered if an “organismic” approach to studying humans might be more promising than a “mechanistic” one: this was clearly a metaphor, and adopting an alternative perspective might be more productive. Of course, it's also possible that I was pushed in that direction by my increasingly-deep readings of Piaget in French. (Note that Piaget's organicism is not typically reflected in the English translations [see esp. Jurczak, 1997]).

To pursue that nascent thought, I also took a course in biological anthropology. This highlighted the similarities and differences between humans and the other great apes, but without any of the potentially problematic meta-theoretical baggage of a course in evolutionary psychology (which U of T did not offer at the time). And I approached a very senior visiting professor—a former Canada Research Chair from another school, filling in for a sick friend—about participating in his upcoming graduate seminar in the History of Biology. After a lengthy phone call, in which I explained my concerns about the dubious value of memorizing names and ideas and dates (while also describing my own independent studies of Kuhn and Piaget), he gave me his blessing. Jan Sapp thus became my first proper mentor in History.

Jan treats the History of Biology as an interdisciplinary endeavor (requiring

¹⁶ I later took advantage of this background to present my own version of Kuhn's paradigms, thereby linking his writings with the “new neurohistory” and parallel developments in “neurophilosophy” (Burman, 2012b, 2014).

mastery of both History and Biology), and so was happy to help me to use his course to organize and contextualize a study of Piaget's biology (see e.g., Messerly, 1996; 2009; also Ottavi, 2001/2009). He also supported my use of French primary and secondary sources, because he speaks the language fluently and had even done his doctorate under Canguilhem's former student, Camille Limoges, at the Université de Montréal. More important, however, is that Jan doesn't care a whit about trivia.

But that's not what was most important. There's a crucial further detail.

Because Jan was just visiting, he didn't know the campus. He therefore needed someone to help him after class to find his car in the dark, a task often made more difficult by the inevitable snow. But because it never took me long to figure out where he had parked from his description of the nearby buildings (I had lived on campus for two years), the walk then meant that I could get extra lessons for as long as our after-class conversations interested him.

These lessons ranged widely. And I took every advantage. What sources he mentioned, I'd grab from the library on my way home and read right away. Then I would be prepared, in the following week's lesson, to pick up where we left off. Our class—properly, a seminar—was run in a similar way.

Jan enforced few rules, and he guided the flow of the discussion to where it seemed to want to go. But one rule was sacrosanct: "Do the fucking work!" (I can quote this verbatim, because it is underlined several times in my notebook from the time.)

What Jan meant by this was both specific and clear: read the texts, pay attention to those instances where something seems strange, and then try to figure out what that bit

means. In other words, he wanted us to try to understand the assigned readings both for *what they said* and for *the reasons why* they said them. (He said that he had no patience for the philosophers' habit of making things up, just to see if the results were interesting.) In short, his course was exactly what I had been looking-for.

Unlike many seminars at that level, where the pattern of reading is a book or more a week (often examined only superficially and with the help of published reviews), Jan's approach was to have us read only a few chapters at a time. But he himself knew these chapters intimately, because they were from a book that he had only just published (Sapp, 2003). And he wanted us to read them very deeply. He also routinely pushed us to pursue the ideas he described by turning to the primary sources that he had cited. He even assigned the facsimile first edition of Darwin's (1859/1964) *Origin of Species* for that specific purpose.

So our readings became explorations. Everyone brought a different interpretation of the difficult bits of the assigned material, supported by different supplementary (primary) texts, and we debated the issues arising.

History was alive in that seminar. It was a riot!

The best part was that Jan got the occasional date wrong. These lapses were a delight to discover—and especially to catch in the moment—because he would grump about the error theatrically before laughing, scratching his head or waving his glasses, and then redirecting us to a more important question: *Ah, but why does it matter that this was published on that date and not another?* (In retrospect, I'm fairly certain that he sometimes gave the wrong dates on purpose as a kind of test; to see if we were working,

because remembering dates always seemed to be easier when they meant something.)

More than being serious fun, however, History is—for Jan—a way of *doing science*. His histories explain and situate and contextualize, but they also advance the fields that he examines and discusses and interrogates. They are *relevant*. And they are *exciting*: not just a protection against the doom of repetition, but also a way to see the possible paths that science might take in the future (see e.g., Sapp, 2005, 2007).

This, as it turns out, is not how everyone does history. But it's how I first learned it, and how I apprenticed as an Historian when I followed Jan back to York University. He then supervised my master's degree in Interdisciplinary Studies, with Fred Weizmann (an historian of psychology) and Matthew Clark (a translator of Ancient Greek, although not of Aristotle). I was then able to continue my explorations, and worked hard to fill in the gaps in my understanding. It was wonderful.

After completing the graduate program (MA 2009), I returned full-time to psychology equipped with some new and unexpected skills. But I also chose specifically not to return to U of T to continue my former professor's project. I was no longer focused on augmenting his observations regarding Kuhn's relevance for psychological theory. This, though, was for good reason: the theoretical project he had set had become for me historical. And York is—objectively¹⁷—the best place in the world to be if what you're interested in is the History of Psychology.

¹⁷ According to the APA's PsycNET interface to the PsycINFO database, which reports the affiliations of authors whose works are indexed by the service, "York U" was the #1 institution publishing journal articles tagged with either the "History of Psychology" index term or the "2140 History & Systems" subject classification code in both 2013 and 2014. The same is true for psychology's "grey literature" database, which includes newsletters, magazines, and conference talks: PsycEXTRA shows that YorkU was the #1 institution producing "History of Psychology" in 2009, 2010, and 2012.

4. Why “constructive” history?

This, so far, is not a history in the way that would be recognizable to someone who had taken my first course at U of T on the history of psychology. It is certainly not the history of a Great Man, nor so far of even mediocre ideas. (By the start of my MA, I don’t think that I had really had a properly “academic” idea worth mentioning.) But there’s a good reason for doing it this way, especially as part of the introduction to a dissertation such as this: it is clear to me that the ideas that I chose to pursue in my doctorate were made possible by my earlier interactions, which in turn were made possible by the institutional contexts in which those interactions took place (cf. Shapin, 1988-2007/2010).

In other words: it seems to me that the ideas that resulted from these efforts can be understood only by their being properly situated. (Indeed, as a writer of my own history, this feels correct: in preparing these words, I have gained a great deal of perspective about why I did what I did, and the resulting insights then helped me to explain those ideas more clearly.) Yet it also follows that this should generalize: an approach to explaining the genesis of normal ideas, developed by a normal person, ought to be equally applicable—following the principle of symmetry (see Bloor, 1976/1991, 2014)—even to Great Ideas developed by Great Men (cf. Latour & Woolgar, 1979).¹⁸

Thus, briefly put: had it not been for these early interactions, the resulting text would have come together in a very different way. It would also likely have had a very

¹⁸ It’s worth mentioning here that “Great Man histories” are no longer acceptable as History. This is yet another challenge to be navigated by Historians who seek to serve Psychology. The need for “relevance” has been recognized, however, and ways forward are starting to be charted (see esp. Ball, 2012).

different character.

There is nothing “innate” about the ways or directions in which my thinking has developed. Indeed, this is a big part of the reason why I have called the conclusion to this journey a “constructive” history: people, and the ways in which their interactions are organized, matter in ways that extend beyond the restricted sense that Kuhn discussed (which doesn’t apply cleanly to psychology anyway [see Greenwood, 1999; also Gholson & Barker, 1985]). But the label itself will also seem to some to be a peculiar choice, so I will explain it.

My first reason for the title is to reflect what I learned from Jan: if you are a master of History, and also of the Science about whose scientists you are writing, then you benefit from a perspective that is not accessible to either position alone. This also highlights patterns and differences and gaps and opportunities that would otherwise be invisible.¹⁹ As a result, the writing produced can be framed in such a way as to be meaningful to both audiences—historical and contemporary, Historian and Scientist—even as the work itself is thoroughly embedded in and informed by a rigorous and historiographically-correct study of sources from the past.

Put another way: “constructive history” is History in service of science, but not beholden to Science. It follows its own rules for its own reasons. This then helps readers to construct new understandings, both of the past and of their position relative to it. And

¹⁹ This approach is properly due to Gaston Bachelard, who is connected to Jan through Canguilhem (his academic grand-father). The similarity to Kuhn then comes through Bachelard’s influence on Alexandre Koyré (whom Kuhn cited). But the resulting approach isn’t correctly called “Kuhnian.” It reflects a much older tradition, which also influenced the epistemes of Foucault (who himself also studied under Canguilhem).

that is helpful in inspiring new perceptions that can lead to new investigations.

The second reason for the title is to pay homage to Michael Chapman (1947-1991), who followed a similar approach in his *Constructive Evolution: Origins and Development of Piaget's Thought* (M. Chapman, 1988a). Indeed, his history was intended explicitly to advance science. But he was not an Historian: he never published an original historical study in a professional History journal.²⁰ He also doesn't fit perfectly into my people-and-places rhetorical structure, because he died too early to influence me personally.

Chapman's influence on me instead came through his book: an object of his creation that I encountered in his stead. (Rather than influencing me directly as an "actor" who—like Jan—could have corrected my misunderstandings, I experienced his influence via an "actant" that required my interpretation [see Latour, 1996, pp. 2, 7-8, 15-16; also Burman & Nicolas, revised & resubmitted, p. 12; cf. Burman, 2012d]). But he did study briefly at York. And it was at York where he discovered Piaget, through the influence of one of Piaget's former students: Juan Pascual-Leone. (I have also myself had the pleasure of receiving his corrections over many years.)²¹ Chapman then decamped for the US for

²⁰ The core journals in the History of Psychology are *History of Psychology*, *The Journal of the History of the Behavioral Sciences*, and *History of the Human Sciences*. Historians of science also typically publish their best work in *Isis*, so works about psychological topics of exceptional historical quality are sometimes found there. My own core work has so far been limited to *HoP* (7) and *JHBS* (2). That said, however, I have also published in theory journals (e.g., *New Ideas in Psychology* and *Theory & Psychology*) and developmental journals (e.g., *Child Development* and *Intelligence*). This is good news, given our goal: historians aren't limited to publishing in History journals.

²¹ Pascual-Leone left Geneva in the early-1960s, and so—because my focus has been primarily on the later works of Piaget—his interests and mine run parallel to each other. I have benefitted from his insights, of course, but I do not follow the neo-Piagetian school that he inaugurated with his own student recruited up from U of T: Robbie Case (1945-2000). And, in fact, I first met Juan at the defence of one of Jordan Peterson's doctoral students. On the committee was also Phil Zelazo, who later became president of

his doctorate and subsequent postdoctoral studies, and later took up a position as a proper developmental scientist at the Max Planck Institute for Human Development, in Berlin, where he completed his book.²²

This book was one of several secondary sources that I read after completing the requirements for my undergraduate degree. In fact, it is one of only two books about Piaget on my reading list from that time that I now remember well. This is perhaps because Chapman's book has been especially influential in shaping my thinking, while also serving as something that I could respond to. (The other is an edited collection of primary source texts, which is so useful that I keep marked-up copies of it in both Canada and Switzerland [Gruber & Vonèche, 1977/1993].)

Why was he so important? Chapman (1988a) was one of a small number of North American outsiders to be led to a study of this history by a sense that we had not understood something of value in our original importations; that there is an "unknown Piaget" (p. 2).²³ And although his work was informed by Piaget's self-presentations in his own autobiographies—which I later came to understand is not strictly-speaking correct as an historiographical practice (for reasons discussed by Vonèche, 2001; cf. Burman, Guida, et al., 2015)—its publication was an important move in the direction of

the Jean Piaget Society and an important mentor to me in thinking about how American Psychology functions as a discipline.

²² Biographical details are from Chandler and Carpendale's (1992) thoughtful obituary, published in *Human Development*. I have had the good fortune to spend some time with Chapman's former colleagues from the University of British Columbia, especially Michael Chandler, and as a result have come to a much better appreciation of depth of the personal and disciplinary loss of his early death. It is an honour to follow in his footsteps.

²³ This was then the title I chose of the first symposium that I organized as a graduate student, for a meeting of the International Society for Theoretical Psychology held in 2007 at York's Keele campus.

articulating and then addressing the gaps in our understanding.

Much that was written afterward has benefitted enormously from Chapman's efforts. It is also worth noting, however, that Chapman was himself responding to John Flavell's (1963) massively influential outsider-history: *The Developmental Psychology of Jean Piaget*. And it's this book that provides the implicit background for virtually every English-language examination of Piaget written in the last half-century.

Flavell's book is today held primarily responsible for what has variously been called the "resurgence," "revival," and "rediscovery" of Piaget by American audiences (Müller, Burman, & Hutchinson, 2013, p. 52). But as Piaget himself noted in the foreword:

It seems clear that Professor Flavell is more interested in the experiments than in the theory, which sometimes gives me the impression—perhaps not of having been misunderstood, but, if you will—of having been understood on certain issues more from without than from within. (Piaget, 1963, p. viii)

In other words, Piaget noted a difference introduced between the interpretation and the source. This kind of observation is exactly the sort of signpost I now look for. And, indeed, it afforded Chapman's approach too: "*to understand Piaget from within*" (p. 1; his emphasis).

Chapman sought to do something more than review the parts of Piaget's project that seemed relevant to his audience of contemporary developmental scientists. He instead sought to do with Piaget what Kuhn had done with Aristotle, and which I had tried fumblingly to do with Darwin's baby-biography (and then later also with the

relationship between Kuhn and Piaget [in Burman, 2007b]).

To take on this daunting task, Chapman used Piaget's autobiographies to organize his narrative as an external reconstruction of the internal monologue represented by Piaget's hundreds of publications and collaborations (see the bibliography published by the Fondation Archives Jean Piaget, 1989). That then provided an entirely new and different view of his subject. It was not a story shaped by how Piaget's ideas had been received, as Flavell's was; Chapman showed instead that Piaget's thinking had developed, and how.

Chapman showed that Piaget had constructed his theory a piece at a time, through collaborations and wide-ranging interdisciplinary explorations. This included stops and starts; even false-starts. In other words, Chapman's book showed *change*. And it showed *growth*. By successfully taking on Flavell (who had at the time just recently received the Distinguished Scientific Contribution Award from the American Psychological Association), Chapman also showed that Histories can change too.

That is then a further reason for my choice of title: "constructive" implies changes in the Historical record in a way that's quite different from the negative connotations associated with "revisionist" history (cf. Kose & Fireman, 2000). And because Chapman used the term to describe Piaget's theory, its repetition here reflects an important aspect of what I will be discussing. But it also works as a description of Chapman's own approach to the doing of a history about something not well-understood: such a history reflects what came before, but from the "inside," and so it necessarily reveals areas not previously considered relevant.

The final reason for the choice of title is to reflect the influence of the Dean of contemporary History of Psychology: Kurt Danziger, who was one of the founding members of my doctoral program and about whom more will be said in the chapters that follow. It suffices simply to say, here, that the title reflects his own in *Constructing the Subject* (Danziger, 1990). And although Chapman couldn't have cited this book, since his own preceded it, their goals were similar: among other things, Danziger wrote about "the unknown Wundt" (see also Danziger, 1980/2001).

The cause of our not-knowing about important aspects of the founder of our discipline, explained Danziger, was the way in which Wundt was imported into American Psychology; that the understanding of Wundt was "constructed" as a result of social interactions and differences in context. I will make the same argument about Piaget, but in a way that I intend will generalize to all similar such importations (Chapter IV). Thus, here, Wundt and Piaget serve as examples of something more fundamental: the process whereby *understandings* are constructed, which is also what interested Piaget (discussed esp. in Chapter V & VI). This in turn also marks out the fundamental role for Historians as knowledge producers: to provide access to the contexts in which "the unknown" aspects of known-interest can become meaningful in new ways (see Appendix A).

In short: my work here emulates and builds upon those whose own efforts and interests inspired me. That said, however, it has been such a long time since I read all of them that their inspiration may now be more notional than direct. Still, I am delighted to recognize their influence. To the extent that readers see them reflected here, I also hope that my own efforts withstand the comparison. But any errors are of course my own.

CHAPTER II

Jean Piaget

WHO WAS JEAN PIAGET? The simple answer is that he was a twentieth-century French-speaking Swiss professor (1896-1980) whose writings, collaborations, and students influenced a great deal of contemporary thinking about how children develop—especially regarding their “cognitive development” (see e.g., Beilin, 1992a; Beins, 2012; Martí & Rodríguez, 2012; Morra, Gobbo, Marini, & Sheese, 2008). Indeed, Piaget has been accepted for decades as one of the very most influential contributors to psychology (e.g., Diener, Oishi, & Park, 2014; Haggblom et al., 2002; Heyduk & Fenigstein, 1984; Korn, Davis, & Davis, 1991; Myers, 1970; Perlman, 1980). Moreover, this influence has been so great that his name, as well as the eponym “Piagetian,” have become virtually synonymous with the study of “developmental stages.”²⁴ Delve deeper than this, however, and things start to get complicated (e.g., Beilin, 1992b; Kitchener, 1986; Lourenço, 2016; Smith, 1993; Tryphon & Vonèche, 2001).

It turns out we have *misunderstood* a great many things about Piaget and his research program (see esp. Lourenço & Machado, 1996; also Jurczak, 1997; Smith, 1981, 2009a). We have also been *blind* to a great many things (Bickhard, 1997; Bond &

²⁴ This is discussed in innumerable works. However, it is most explicit in (Piaget, 1962b, 1971c).

Tryphon, 2007; Gallagher & Reid, 1981; Vuyk, 1981). But for those seeking to contribute to the areas that Piaget influenced, these continuing errors and invisibilities are actually where the best opportunities are to be found. The challenge, then, is to figure out how to see them. That, in a nutshell, is the task for this dissertation (for the New Theory).

In this chapter, however, my purpose is simply to introduce Piaget. I begin by adding some depth to what everyone already knows of his origin story. Then I delve deeper to examine certain aspects of his early work that he later noted were crucial in setting up the work that was popularized as “stage theory” (esp. by Flavell, 1963; Hunt, 1961; see Müller et al., 2013). And then I discuss how all of this makes sense, theoretically, in order to provide a solid foundation upon which to build.

In the next chapter, I continue to build this biographical foundation by examining how Piaget’s earliest contributions were understood at the time. But I do this a little differently: by looking at how Piaget’s works were received by both Americans and the Swiss, and then comparing and contrasting those perceptions, we are able to highlight some similarities and differences in perception. Then, in Chapter IV, I extend this discussion of difference-through-comparison and provide a new method of identifying new opportunities for Historical work.

My intent for the new method is to provide a tool that it will generalize beyond Piaget to highlight similar opportunities of other influential authors of roughly equivalent stature. (All that is required by my new method is for a substantial number of an author’s texts to have been translated, so differences between the portfolios can be identified.) This then allows for the simpler identification of what has been missed. But we must

begin at the beginning. That is where the first misunderstandings appear, and these then cause a series of blindnesses that block our access to the opportunities that await us.

1. Piaget, Psychologist?

The greatest misunderstanding, reflected in most texts, is that Piaget was “a psychologist” (e.g., Rotman, 1977). The hard truth, however, is that he never received a degree in the subject. In fact, psychology was covered only briefly in his high school curriculum, as part of the philosophy course taught by Arnold Reymond in 1913-1914 (Schaller-Jeanerret, 1996/2008, pp. 43, 45).²⁵

Indeed, Piaget barely received a formal university education at all: he took only five semesters of courses at the University of Neuchâtel between the end of high school and the awarding of his doctorate in 1918 (Schaer, 1996/2008, p. 52). Thus, what he said in the concluding lines of one of his later autobiographies—“I will die without real diplomas” (Piaget, 1950-1976/1976, p. 43; trans by M. Chapman, 1988a, p. 264)—is therefore literally true, and not just the false modesty of an eminence.

That said, however, Piaget did indeed begin his career *credentialed* with a doctorate. This was in Natural History (Schaer, 1996/2008). And it was earned primarily as a result of his publications, which were in turn derived from nearly a decade of increasingly-meticulous fieldwork collecting and cataloguing snails in the Swiss Alps.²⁶ Yet his view of species got him into trouble with his professional community, because he

²⁵ The syllabus indicates the following was covered that year: “Psychology, facts of consciousness. Psychological analysis of judgement [sic]. The emotions. The will. Reflections” (Schaller-Jeanerret, 1996/2008, p. 43). For more detail on Reymond, see Ducret (1984, pp. 361-378).

²⁶ I initially thought that he might have been eating them (*escargots*), and thus his catalogues of where to find the different species could have been considered a form of animal husbandry. But the collections found in the desk drawers in his home office were mostly all too small for eating.

had come down on the wrong side of the Mendelian uprising that eventually resulted in the “modern synthesis” of evolutionary biology (Ducret, 1984, pp. 67-207; Vidal, 1994; see also Sapp, 1990; 2003, pp. 117-129, 143-156). As a result, he was effectively ejected from the field: Piaget became “notorious... as a bad systematist” (Vonèche, 2003, p. 5).

Intending to change his path, in 1919, Piaget submitted a proposal for a second—more traditional—PhD in philosophy. This was to be with Reymond (Vidal, 1994, p. 219). And his goal, reflected in the title of his proposed dissertation, was to apply the biological methods from his earlier work to the analysis of human values (Liengme Bessire & Béguelin, 1996/2008, p. 66). But he never completed the project (Piaget, 1965/1971, p. 10). Instead, he continued his pattern of thinking-by-publishing. His turn to psychology-proper then came via a public search for methods that would be compatible with his biological approach, and an avalanche of reported results.

2. Piaget’s turn to psychology

After receiving his doctorate in 1918, Piaget undertook what we would today call a postdoctoral fellowship in Zurich. There, he met the leaders of the dissident Swiss school of psychoanalysis (including Carl Jung and Oskar Pfister), as well as Eugen Bleuler. And from them he learned clinical interviewing. But he soon became “restless” (as he put it in an autobiography published thirty years later), and so abandoned the post after little more than a semester (Piaget, 1952a, p. 244). He then undertook a second postdoc in Paris, where he first began working with children at Alfred Binet’s former

laboratory-school at Grange-aux-Belles in Paris (P. Harris, 1997; Vidal, 1997b).²⁷ He held this post until his return to Switzerland in 1921.

Piaget's job at the Binet Lab was to standardize British intelligence tests for use with French populations. This took advantage of the advanced statistical training from his background in Natural History (Vonèche, 2003, p. 6). But boredom, curiosity, and a lack of direct supervision led him to apply his new clinical skills (set out in Piaget, 1920a, 1920b). And this pushed his work in an unexpected direction.

As is well-known to all those who have read a textbook description of Piaget's origin story, his interest was not at all in the content or organization of the intelligence tests themselves (as perhaps it should have been). Instead, he was interested in *whatever-it-is* that *produces* the results that are then measurable by intelligence tests. This he later explained, with the benefit of thirty-years' hindsight:

I engaged my subjects in conversations patterned after psychiatric questioning, with the aim of discovering something about the reasoning process underlying their right, but especially their wrong answers. I noticed with amazement that the simplest reasoning task... presented for normal children up to the age of eleven or twelve difficulties unsuspected by the adult. (Piaget, 1952a, p. 244)

In other words, he found that children's reasoning is fundamentally different from adults' reasoning. Or, put another way, some of what adults consider obvious is *literally unthinkable* earlier on. This at-the-time-unexpected finding then became the focus of his

²⁷ One sometimes reads that Piaget "studied with" or "worked with" Binet. This is an error: although Piaget was indeed hired to do work with intelligence tests at Grange-aux-Belles, they never actually met. Binet died in 1911, when Piaget was 15 (Wertheimer & Maserow, 1980). See Piaget (1975) for his reflections on Binet's contributions (also Nicolas et al., 2013).

research.

Piaget thus began cataloguing the difficulties that normal children have with different kinds of questions, as well as the patterns in their speech as they talked about them. Yet note the similarity to his earlier studies in Natural History, when he spent his time cataloguing alpine snails: *what do you find when you look?*

In other words: despite the use of the psychological subjects, this was still a biological project.²⁸ We can make this clearer—and highlight the foreignness of it—by posing a purposefully distant-sounding version of his hypothesis: *Do different children, at different ages, respond as if they belong to different “species”?* (Or, following Haeckel and consistent with Hall: *Does development from child to adult recapitulate the evolution of the human species from ape to man?*)²⁹

This early approach—which we might call “transitional,” because Piaget did indeed eventually get to psychology-proper (Vidal, 1997b; see also Appendix A)—is something that I have begun reconstructing using the collection of snail shells at the Royal Ontario Museum in Toronto (Burman, 2010). But to discuss that work in detail here would pull us badly off track, given our goals, so we will leave it aside for now and return to it in a future article.

Suffice it to say that Piaget appealed to a combination of methods drawn from natural history and psychoanalytic interviewing (or, as he put it, “psychiatric”

²⁸ Mitchell Ash (2003) notes that, following Ribot, the early French view of psychology was that it was properly an outgrowth of biology—rather than of philosophy (p. 254). Given this, Piaget’s proposed application of biological methods makes more sense as “psychology” in the way that we mean it. To see that difference as a similarity, we need only translate through the different national contexts.

²⁹ This characterization, it seems to me, is accurate only for Piaget’s earliest work (see also Vonèche, 2003, pp. 4-5). For more on Haeckel and Hall, see Green (2015a).

interviewing) as a result of his suspicion that the found-difference between adult- and child-reasoning might generalize; that groups of children answer certain kinds of questions in quite dramatically different ways that would nonetheless be formalizable in Natural Historical terms. His attempts to demonstrate with rigor that this was the case, perhaps to avoid a repeat of his ejection from his first discipline,³⁰ then afforded the myriad experiments that collectively constitute the empirical evidence for his “standard” stage theory of development: that the “sensorimotor” reasoning of infancy gives way to the “preoperational” reasoning of early childhood, followed by the “concrete operational” reasoning of late childhood and the “formal operational” reasoning of adolescence.³¹ But it’s the larger program that he continued to develop, especially after he returned to his earlier interests when it was shown in the early 1960s that he hadn’t been entirely wrong about speciation in the mid- to late-1910s (discussed in Chapter VI).³²

Thus, Piaget’s project was not initially a *psychological* project in the way that we mean the term. And it did not end up as a psychological project (see Appendix A). We simply call him a psychologist because he created several methods now used by psychologists, and eventually ascended to a Chair in Psychology (see e.g., Papert, 1999;

³⁰ I was once told by a former student of Piaget’s that his prolificacy was the result of religious inspiration; that he felt “called” to do this work, but also that he was unable to talk about this calling out of a fear that the admission would lead to his work’s dismissal. Although I have no additional evidence for this, it does seem to merit some further examination. Perhaps there will be some mention of the family’s religiosity in his wife’s papers, now held at the Piaget Archives in Geneva but still unprocessed.

³¹ That this got attached to the narrow age-ranges associated with North American school grades is almost certainly a result of reading the translations of Piaget’s works through the influence of Lewis Terman and his attaching “mental age” to the results of intelligence tests (see P. D. Chapman, 1988b). This is sufficiently important to merit its own discussion, however, and so will be examined in a future work (as a sequel to Nicolas et al., 2013).

³² As it turns out, too, “the species problem” is not so clear-cut as is implied by most discussions of this aspect of Piaget’s program. Richards (2010) reports that more than twenty competing species concepts are used in grouping individuals as belonging to a meaningfully-identifiable collection.

Vonèche & Vidal, 1985; Voyat, 1977; also Chapter 3). Given the extent of his impact on the discipline, this appellation is perhaps also obligatory as an honorific (cf. "Distinguished Scientific Contribution Awards," 1970). But recognizing this alone is not sufficient to produce a depth of understanding that could make new opportunities accessible. Indeed, it is quite possible that there is nothing more of value to be found in Piaget's explicit discussions of child psychology (but see Ratcliff & Burman, 2015; Conclusion). Those looking for inspiration, and new opportunities, therefore need to delve deeper. (Or, if you prefer: because the low-hanging fruit have all been plucked, we need new tools to help us reach higher [cf. Blumenthal, 1998, pp. 78-80].)

3. Delving deeper (to reach higher)

The preceding lines add depth to the usual textbook-presentation of Piaget's origin-story.³³ We see the influence of his background in biology (Ducret, 1984; Vidal, 1994, 1996/2008; also Messerly, 1996; 2009). And we see the origins of his methods (Bond & Tryphon, 2009; also Duveen, 2000; Mayer, 2005; Opper, 1977). In a sense, however, it was his earlier influences that were more important in laying the foundations for the later work that we understand least-well.

For example, he felt—in retrospect—that an early realization that *content* follows *form* was especially crucial to setting up his project: that the former is *structured* by the

³³ Of course, his origin-story is actually more complex still: Piaget was, simultaneously with his work at the Binet Lab, attending lectures by Pierre Janet at the Collège de France (Amann-Gainotti, 1992; Amann-Gainotti & Ducret, 2002). In these, Janet argued for a “genetic” (developmental) approach to psychopathology. And he presented a kind of stage model of the decline into mental illness, which Piaget then reversed and combined with Baldwin's related ideas regarding “genesis” to inform a stage model of development. But because neither Baldwin nor Janet left any archives, these waters have become muddy with time. For this reason, they will not be discussed here; attempts to clarify the direction of their mutual influences are underway elsewhere (Burman & Nicolas, revised & resubmitted).

latter, and also that questions regarding the justification for particular decisions could be used to provide evidence of that structuring. This is the thread that I will follow here.³⁴

The relevant lines from his first autobiography are typically edited for length, and presented only partially (if mentioned at all), but they are so important for all that follows that I will repeat them here fully. As he recalled, thinking back to his early course in philosophy that was to set his path away from biology:

A lesson by A. Reymond on realism and nominalism within the problem area of “universals” (with some reference to the role of concepts in present-day science) gave me a sudden insight. I had thought deeply on the problem of “species” in zoology and had adopted an entirely nominalistic point of view in this respect. The “species” has no reality in itself and is distinguished from the simple “varieties” merely by a greater stability. But this theoretical view, inspired by Lamarckism, bothered me somewhat in my empirical work (viz., classification of mollusks). The dispute of Durkheim and Tarde on [sic] reality or non-reality of society as an organized whole plunged me into a similar state of uncertainty without making me see, at first, its pertinence to the problem of the species. Aside from this the general problem of realism and of nominalism provided me with an overall view: I suddenly understood that at all levels (viz., that of the living cell, organism, species, society, etc., but also with reference to states of conscience, to

³⁴ Granted, this is not an especially historicist manoeuvre: we are explicitly following his then-presentist observation—from his autobiography of thirty years later—in order to focus our investigation on a particular aspect of Piaget’s project that seems both poorly-understood and potentially useful. However, adopting this strategy allows us to skip over material that is not relevant to the project at hand. The resulting history is therefore partial. But that’s okay, as we discuss below.

concepts, to logical principles, etc.) one finds the same problem of relationship between the parts and the whole; hence I was convinced that I had found the solution. There at last was the close union that I had dreamed of between biology and philosophy, there was an access to an epistemology which to me then seemed really scientific! (Piaget, 1952a, pp. 241-242)³⁵

The importance of this realization, as Piaget reflected back on his own journey to that point nearly four decades later, was that many of the various concerns that troubled him—as a youth whose outlook was influenced jointly and severally by Protestantism, Bergsonism, and the horrors of WWI—could be rationalized simply by relating parts to wholes. (Swiss Calvinists are a kind of Protestants, which are a kind of Christian, whose beliefs are in turn compatible with Bergsonian immanence, and yet these are then in conflict with the fact of War which itself seems to be compatible with biology [e.g., Piaget, 1916; 1918/1993].) Indeed, from this perspective, even the obviously separate domains of biology and knowledge could be understood as being different parts of the same whole (and they are in turn also wholes with their own parts). In a sense, too, both could also be considered alive; they are changing, on their own and in response to outside forces. The question was, *How?* (Bergson's *élan vitale* was continuous, while what Piaget saw was—at the time when they discussed it—discontinuous [Piaget, 1965/1971, p. 101n3].)

³⁵ Unfortunately, Piaget's original writings on nominalism have been lost (Vidal, 1994, p. 86). While there is the possibility that they could be found among the materials recently donated to the Piaget Archives, the practicalities of funding such endeavors have meant that the focus is on processing materials from after WWII (Ratcliff & Burman, under review). For now, we are therefore limited to what he recalls in his later autobiography. This can of course be supplemented by other related early writings (e.g., Vidal, 1992). But the ideal is always to examine the original primary source documents.

Piaget's goal, following his Paris postdoc, thus became to examine how the different forms of human thinking develop, and therefore also how different contents (including the conflicting values of his proposed PhD in philosophy) could be justifiably held. As he explained:

At last I had found my field of research. First of all it became clear to me that the theory of the relations between the whole and the part can be studied experimentally through analysis of the psychological processes underlying logical operations. This marked the end of my "theoretical" period and the start of an inductive and experimental era in the psychological domain which I always had wanted to enter, but for which until then I had not found the suitable problems.

(Piaget, 1952a, p. 245; referring especially to Piaget, 1918)

As we would now describe it, in slightly different terms, Piaget's "suitable problem" was that form can be best inferred from studying *the necessary*; conclusions following from the process of a child's reasoning that, as a result of their formal properties, could not be any other way (see Smith, 1993, 1997, 2009b).³⁶

He then continued his reminiscence, connecting together all of his early interests in a way that goes beyond strict formalism:

Finally my aim of discovering a sort of embryology of intelligence fit in with my biological training; from the start of my theoretical thinking I was certain that the problem of the relation between the organism and environment extended also into

³⁶ At the unveiling of a bust dedicated to Reymond at the University of Lausanne, in 1944, Piaget is reported to have said: "My own research on the genesis of logic in the child has been inspired to a large extent by his teaching, the genetic study prolonging the historical study itself" (qtd. in Schaller-Jeanneret, 1996/2008, pp. 47, 50n44).

the realm of knowledge, appearing here as the problem of the relation between the acting or thinking subject and the objects of his experience. Now I had the chance of studying this problem in terms of psychogenetic development. (Piaget, 1952a, p. 245)

This problem was examined objectively through a series of empirical studies, with Piaget continuing his research upon his return to Switzerland in 1921 at the Rousseau Institute's *Maison des Petits* in Geneva (see Chapter III).

Piaget had thought this project would take only five years (Piaget, 1952a, p. 255; also Piaget in Tanner & Inhelder, 1956-1960/1971, p. 32). Yet the result, thirty years later, was the emergence of a larger research program that he called “genetic epistemology” (see esp. Piaget, 1950; see also Appendix A). He then pursued this program, and continued to revise it, until his death (see also Piaget, 1968/1970, 1977b, 1980c).

Conveniently, for those seeking opportunities, this program has been very poorly understood. The greatest interpretive mistake—by far—was that it related somehow to the role of genes in knowledge, and thus that Piaget was a “neo-maturationist” (noted by Piaget in Evans, 1973, p. 39; Piaget, 1982a, p. xiii; also by Cellérier in J.-C. Bringuier, 1977/1980, p. 81; Voyat, 1977, p. 343). Of course, we now know—from his view of speciation, but especially because of his ejection from Natural History for being an anti-Mendelian—that such an interpretation could not possibly be correct. But the confusion over terms might indeed be understood as reflecting a change in the meaning of *genetic*, between English and French, which in this case refers to “genesis” rather than “genes”

(Piaget in J.-C. Bringuier, 1977/1980, p. 40). Piaget’s program was therefore, properly, an application of biological and psychological methods for philosophical ends: he asked, broadly, *How can knowledge be understood to grow, develop, and evolve in whole and in part?* (see esp. Piaget, 1967/1971; Piaget & Garcia, 1983/1989).

4. An introduction to genetic epistemology

For Piaget, each newborn enters the world without knowledge: their minds have a basic *form* (afforded by e.g., inherited instincts), but no *content*. Reflexes and random movements then allow them to engage with their environments. And this is key: it is from these interactions that knowledge is *constructed*.

This point must be emphasized before we continue. Knowledge, for Piaget, is not a “copy” of the world. Indeed, explaining the genesis of *new* knowledge—how it is that a child comes to know something about anything, and then extends this in unexpected ways—was the primary goal of his research program. As he explained later:

The problem we must solve, in order to explain cognitive development, is that of *invention* and not of mere copying. And neither stimulus-response generalization nor the introduction of transformational responses can explain novelty or invention. By contrast, the concepts of assimilation and accommodation and of operational structures (which are created, not merely discovered, as a result of the subject’s activities), are oriented toward this inventive construction which characterizes all living thought. (Piaget, 1968/1970, p. 714)

The infant’s earliest explorations are thus carried out—unknowingly, naively, and without conscious intent—using the tools of assimilation alone: a structural body

integrated with a functional sensory apparatus, which can bring things and experiences “in.”³⁷ But, crucially, these explorations are undertaken in the real world, and without any of the operational “wholes” that emerge later. Thus, even the “a priori” categories of experience need to be reinvented (Burman, 2011; citing Piaget, 1925).

That said, however, Piaget’s infant is more *disorganized* than *empty*. Smell, touch, vision, and proprioception all exist, but they act separately in different unintegrated domains. Structures and functions are therefore all “sensory” and “motor.” None are “abstract.” Indeed, this means that none are yet *mental* in the way we conceive of this as adults (although there is indeed “something it is like to be” because these structures and functions come with basic reflexive qualia included, which themselves then develop into the more complex kinds of affectivity [see Piaget, 1962a; Piaget, 1954/1981; also Gouin-Décarie, 1962/1965; 1978]).³⁸

Still, in healthy children, structural-functions (sensori-motor apparatus) work fairly consistently. The mechanical action of the hand becomes more precise, with practice, but it does not change in kind. The same can be said for smell. And vision. Yet

³⁷ The simplest definition that Piaget provides for assimilation is by way of analogy to eating: “Assimilation is chiefly a biological concept. By digesting food, the organism assimilates the environment; this means that the environment is subordinated to the internal structure and not the reverse.... A rabbit that eats a cabbage doesn’t become a cabbage; it’s the cabbage that becomes rabbit—that’s assimilation. It’s the same thing at the psychological level. Whatever the stimulus is, it is integrated with internal structures” (in J.-C. Bringuier, 1977/1980, p. 42). Of course, it goes without saying that the biology of eating is inherited.

³⁸ My response to the philosophical Knowledge Argument is therefore that “the redness of red” is a consequence of—jointly—the inherited sensory apparatus of the perceiver *and* their developmental trajectory to the point of their perception (in response to Jackson, 1982; see also Ludlow, Nagasawa, & Stoljar, 2004). The novelty of “redness” upon first seeing “red” then is not a function of knowledge, but of the abstractions constructed from that knowledge. These abstractions are then what give “redness” its meaning. My paper presenting this response remains unsatisfactory to me, however, and so it has never been submitted for review. Still, I have indeed tried coming at it in a different way (see esp. Burman, 2014). I hope to have opportunity to develop this thinking further in the future.

it is their *use* in the resulting explorations that enable the infant to *abstract* “objects” from the environment in a systematic way: this sensation in domain-*x* (touch) and that sensation in domain-*y* (smell) co-occur regularly with that other sensation in domain-*z* (vision). These co-occurrences can then be tested gropingly—which is to say, unintentionally—through circular repetition, such that they ultimately become expected (Burman, 2008a).

This expectation is the result of something new that the infant does not discover in the world. It is instead the result of the invention of a new kind of action: an “empirical abstraction” (see Moessinger & Poulin-Dubois, 1981). This is the construction of a new internal structure—a new supervening whole—that unifies aspects of disparate experiential domains into a functional mental unit. New experiences can then be “assimilated” to that structure, rather than to reflexes. And what an object “means,” forever afterward, is a function of an interaction between the object itself and the subject’s ways of structuring the results of its past explorations.³⁹

Briefly put: the infant’s world is one of “sensations,” rather than “objects.” This is then where we begin to encounter the most difficult aspects of Piaget’s vocabulary.

Structures are wholes, *ex hypothesi*, and so each is “equilibrated.” When new parts are discovered that must necessarily belong to a given whole, however, the relevant

³⁹ This was taken up in detail in the unpublished causality project (c1964-c1968) that I recently discovered at the Piaget Archives in Geneva. A conference talk about the project, and the discovery, has been presented in English (Burman & Ratcliff, 2015). And a chapter reporting on how the materials were found has been prepared in French (Ratcliff & Burman, under review). But the discovery is too recent for us to have done any more than this. Of course an English paper is being discussed, yet—because this will probably need to include translations, and thus it will be the first “new Piaget” to appear in over a quarter-century—the process of preparing that material is complicated by the need to secure rights, etc. An attempt at a new formalization was presented in (Piaget & Garcia, 1987/1991).

structures are “disequilibrated” and then “accommodated” to capture these parts and integrate them. The structures therefore change in response to found-exceptions. The results of this, however, are not themselves “real” (cf. J. Richards & Von Glasersfeld, 1980). They are internal reconstructions of external phenomena; a kind of name by which the real can be referred-to, having both formal properties and affective elements.

There are lots of small restructurings throughout childhood: any time an outcome doesn't match expectations, and this results in a change, we call this “learning” (see esp. Gallagher & Reid, 1981). But “development” is usually described more broadly as being characterized by a small number of important large-scale changes. And although Piaget talks specifically and at length about how small changes become large changes (in exactly the way you would expect from a biologist concerned with species), it is the large-scale that has drawn the most attention. This is then how his “stages” came to be understood (even though he talks in detail about sub-stages).

The first major structural re-equilibration, from what Piaget called the “sensorimotor” stage to the “pre-operational” stage, shifts the infant's entire world from being one of “sensations” to being one of “objects.” (Recall, however, that objects are *abstracted* from sensations: they are “real,” but not in the same way meant by philosophers who bang on tables.) This process of augmenting-equilibration then continues, building up from that new “objective” foundation.

The young child interacts with the “objects” found in their world (and can thus begin to perceive “illusory objects”). Yet note that their existence is still attached to her sensation of them. Adults interpret this as “egocentrism” (see Kesselring & Müller,

2011). But that is a misnomer resulting from Piaget's early experience with psychoanalysis. And he later apologized for the way in which that term misled early commentators (Piaget & Inhelder, 1948/1956, p. 220).

The point was not that young children are *more selfish* than adults, but rather that they live in a different kind of world that they construct (see esp. Piaget, 1926/1929). Objects are not simply identical with sensations; they are constituted by them. The sensations are what *things* are. With experience, however, children begin to abstract *different ways* of interacting with these things. And the objects themselves then become more real than the sensations that report their existence, at the "concrete operational" stage, because they are discovered to follow higher-order laws (e.g., conservation of mass, of volume, of number, etc.). Note, also, that social interactions follow the same pattern (Piaget, 1932/1932).

Ultimately, the rigidity of this lawfulness is escaped: abstractions come to be made of abstractions, such that the "objects of thought" themselves become imaginary and arbitrary. This is the "formal operational" stage typical of early adolescence (Inhelder & Piaget, 1955/1958). But even reflections on imaginary objects are grounded in the original empirical abstractions that started it all (see esp. Piaget, 1975/1985, 1977/2001). Systematically-organized, and mechanically- or mentally- (which is to say "operationally-") manipulated, but still a *reflection* of the real (see also Piaget & Garcia, 1987/1991; Piaget, Henriques, & Ascher, 1990/1992).⁴⁰

⁴⁰ This distinguishes Piagetian constructivism from Social construction. In short: for Piaget, structures are not arbitrary. They are "necessary" (see esp. Smith, 1993, 1997). In a future project, I would like to examine whether and how his concept of "pseudo-necessity" applies to cases of social construction:

5. Reaching higher

The move from the development of children's reasoning to the evolution of scientific reasoning follows the same structural pattern. Both begin from nothing, and thus everything we understand today was produced by this process of augmenting-equilibration (viz. from sensations to objects to ideas). But this is easier to explain by reference to mathematics. And Piaget had a favorite anecdote, in this connection, about a friend who also happened to be a mathematician:

When he was four or five years old—I don't know exactly how old, but a small child—he was seated on the ground in his garden and he was counting pebbles. Now to count these pebbles he put them in a row and he counted them one, two, three, up to ten. Then he finished counting them and started to count them in the other direction. He began by the end and once again he found ten. He found this marvelous that there were ten in one direction and then in the other direction. So he put them in a circle and counted them that way and found then once again. Then he counted them in the other direction and found ten once more. So he put them in some other arrangement and kept counting them and kept finding ten. There was the discovery that he made. (Piaget, 1964, pp. 179-180)

The friend's childhood discovery, in short, was *number*: "He had discovered that the sum ten is independent of the order of counting" (Piaget, 1972, p. 6).

Number supervenes on a countable set in the same way that the abstraction of a

it still *feels* necessary, in the same way that apparently-justified beliefs feel obvious, but it is not so formally (see esp. Piaget, 1977/1986, 1981/1987, 1983/1987).

whole supervenes on a grouping of parts. It is both discovered in nature, and constructed of nature: it is what we might call a “justified collection,” in the same way that a species is a defensible grouping of non-identical individuals (never mind their genes).

Number, in other words, requires an action not already present in the countable set: naming. From this first naming, new actions are then made possible—but by reference to that operational name, rather than to the underlying action of counting (or the phenomenal qualities of the things being counted). These names can then become coordinated by laws, such that the names themselves become only placeholders. Thus: the operation $1 + 1$ is “two,” necessarily, because the rule of addition refers to the bringing together of countable sets describable using those numbers such that the resulting set has the combined number of countable objects. But, crucially, no counting is required by the use of the operation; the rule coordinating the names replaces the action. Or, rather, the action is subsumed in the rule applied.

From this perspective, number is both real *and* imaginary. And the rules for manipulating numbers are also both real and imaginary. Yet they are imaginary in the constrained way of logical necessity. Indeed, Piaget (1941/1952) found that number and logic develop together (see also Smith, 1999).

Science is similar, but the fact of the existence of scientific knowledge in history also played a role in Piaget’s argumentation. To wit: it is simply the case that adults living together socially know something rather than nothing, and that this knowing has both a history and an impact. (Note that this impact of scientific knowledge was manifested most obviously in Piaget’s own development by the widespread

commercialization of electric light during his youth and early adulthood.)⁴¹

This core part of his research program was articulated in his inaugural lecture as a new professor at the University of Neuchâtel in 1925. There, he took on Kant: “epistemic categories are necessary,” he argued, “but not innate” (my paraphrase [Burman, 2011]).

As he put it:

For Kant, space—nay, Euclidean space (the only kind known in his time)—was an *a priori* form of sensibility; that is to say, Euclidean space was a form (*forme*) which imposes itself on the mind and which is imposed by the mind on the world. But the evolution of geometry in the nineteenth century showed that Euclidean space was simply one of many possible forms of space. Euclidean space therefore lost the character of necessity given to it by Kant. The problem is therefore extremely complex: to identify what in the notion of space comes from the activity of the mind, and what is derived from external experience. (my trans of Piaget, 1925, pp. 195-196)

In other words, the fact of historical change—and, in this case, the discovery that *there are more species of “space” than assumed by Kant*—showed that the way that we had conceived of this previously-fundamental aspect of human perception (for Kant, a necessary and *a priori* category that makes all other knowledge possible) was incomplete. Instead, our conception of “space” is part of a larger changeable group that we can name and then refer-to at the level of the whole.

⁴¹ Note that GE, which was founded as General Electric in 1892, was “born” just four years before Piaget. The tungsten filament used in modern bulbs was invented in 1904: when Piaget was eight.

Piaget continued: “What we have said of space could be said of all the Kantian *a priori*” (my trans of Piaget, 1925, p. 196). This then provided the basis for a whole series of experiments: if our scientific understanding of space can evolve and change, then the understanding of “space” also cannot be innate in children. This was ultimately shown in *The child's conception of space* (Piaget & Inhelder, 1948/1956). And, indeed, Piaget eventually showed the same thing of the other Kantian concepts as well: time, causality, etc.—through more than thirty books, spanning a half-century of continuous publication (1927-1987, incl. posthumous works).

Note, however, that Euclid was not *replaced* in this sequence. Piaget is therefore not talking about “progress” through falsification:

Euclidean geometry did not become even slightly “false” when non-Euclidean geometries were discovered; it was simply integrated into a larger structure as a particular case. The error [Kant’s] lay in believing that it was general; it became a particular case—a particular case among other structures—and there wasn’t a shadow of regression. Nor of abandonment. Not a single one of Euclid’s theorems was abandoned. (Piaget in J.-C. Bringuier, 1977/1980, p. 48)

In short: changes in the scientific understanding of space meant that Euclid’s view was recognized as a necessary part of a larger whole. Children develop in much the same way, but starting from much more primitive foundations: “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” (Piaget in J.-C. Bringuier, 1977/1980, p. 92).

From this perspective, Piaget’s research program is both less complicated and

more complex than Psychologists now think. It is about how we come to know: how “novelties” emerge, when they are not already “in” the system. And this is both a developmental and an historical problem. His use of poorly-understood terms—and his own accepted mistakes, such as with the use of the term egocentrism—then reflects his own inheritance and his locality. In other words: his works use the concepts that he had access to, and he changed them as he gained access to newer concepts.

“Schema,” for example, is a Kantian inheritance: how (developing) categories are applied to reality. But the process by which categories develop also reflects Piaget’s thinking about species: equilibrated wholes with interchangeable parts (see Chapter V). The categories themselves then serve as the means by which the world is sampled (through assimilation). And the extent to which the results are unexpected results in structural change (accommodation).⁴² Development therefore consists in coming to approximate the demands of reality, given the inheritance of systems adapted for a different time and place.

For Piaget, in short, development is an extension of evolution. And behavior is its engine. (This was the original title of Piaget, 1976; translated as Piaget, 1976/1979.) Still, the development of children does not recapitulate the evolution of the human species. The trajectories are simply parallel paths, and—because both are alive—they each always follow the path of least resistance (i.e., development proceeds along lines charted by a combination of the complexity of encountered-objects and the power of the extant

⁴² In biology, this is achieved by making babies. (According to the Biological Species Concept: if two groups of individuals belong to the same species, then their babies can make babies—if not, they can’t.)

schemas and structures, which is itself a function of past constructions).⁴³

6. The plan for what follows

To keep things as simple as possible, in what follows, I will begin at the beginning: with few snapshots of Piaget as he was first seen by his contemporaries (Chapter III). This then provides a solid foundation for the further inquiry into what opportunities may still exist by highlighting some similarities and differences in how Piaget has been perceived by different groups at different times (i.e., to show, like Kant's view of space, that our conception of Piaget is not "necessary"). I follow this, in Chapter IV, by introducing a general method that can help Psychologist-Historians to identify differences more easily, taking inspiration from work ongoing elsewhere at the intersection between Psychology and the Digital Humanities (e.g., Burman, Green, et al., 2015; Green et al., 2013, 2014, 2015a, 2015b). Together, the first chapters can then be understood as an articulation of the historiographical challenge faced by contemporary American psychologists in engaging with this kind of historical material: our present perspective affords certain kinds of opportunities, but obscures others.

Following this, two different kinds of more-traditional histories (presented in Chapters V & VI) deliver on some of the new opportunities made visible by these first inquiries. The first examines how a change in Piaget's thinking about logical formalism made it possible to conceive of the assumptions of his "standard theory" in new terms. (This is important because Piaget's changing formalisms altered how he modeled the

⁴³ Note that, from this perspective, the role of *education* is twofold: to teach skills to increase the power of structures, but also to reduce the complexity of encountered-objects. To differentiate this from "scaffolding," I called this "chaperoning" (Burman, 2008a). Of course, that now can be simplified in light of the preceding.

“necessity” upon which his methods relied.) And the second delves still deeper, to examine how Piaget’s later related works updated the view of “genesis” imparted to his early thinking by the pre-Mendelian thinking of James Mark Baldwin (discussed in Piaget, 1982b; see also Cahan, 1984; Cairns, 1980).

Yet those results are still themselves just a beginning. They also need to be situated in order to be more broadly meaningful (especially if this project is going to be more than an accounting of the myriad ways in which Piaget has been misunderstood [see e.g., Lourenço & Machado, 1996; also Jurczak, 1997; Smith, 1981; 2009a]). That is the task for the next two chapters.

CHAPTER III

The neglect of the foreign invisible

THIS CHAPTER IS INTENDED first as a contribution to historiography—historical method—and only second as a contribution to the history of developmental psychology. It picks up where we left off in the last chapter, and therefore provides some further biographical details about Piaget. But the focus is shifted. It is therefore intended to be a discussion, primarily, of *the doing* of the history of psychology: my focus is on *form*, not *content*. Here, this takes the form of an argument about a weakness of American psychology in an increasingly globalized world. That then sets up my broader argument in Chapter IV, as well as the broader contribution of this dissertation.

Briefly put: American psychology, including its associated approaches to the history of psychology, is not adequately equipped to benefit fully from the contributions of “foreign” scholars. To make the resulting (narrow) argument clear, two archive-driven microhistories are reviewed, contrasted, augmented with new archival research, and synthesized: Yeh Hsueh’s (2004) examination of the nomination process at Harvard that led to the awarding of an honorary doctorate to Jean Piaget in 1936, and Marc Ratcliff’s and Paloma Borella’s (2013) examination—in French—of a similar process that resulted in Piaget’s hiring at Geneva in 1929 and his eventual promotion there in 1940.

Comparing the authors' different approaches to similar content shows that we need to broaden our methodological sensibilities. This will allow us to see high-quality "foreign" contributions for what they are. It's also made clear, here, that several interesting insights result if we do. Among them: although Piaget's theory is today mistakenly criticized for being asocial, and this serves as justification for countering his early works with Vygotsky's posthumous critique, it emerges from these archival studies that Piaget may have in fact *chosen to present* himself and his work as non-sociological (when this was not the case) for reasons unrelated to his intellectual project. Such examples then broaden the discussion from the neglect of the "foreign invisible" to include suppression—even censorship (by self or other)—which in turn reflects the primary problem afforded by internationalization: by what standards are we to judge the contributions of "foreigners" into "our" discipline?

Thus, my purpose with this chapter is threefold: (1) to highlight a disciplinary problem, relating to "our"⁴⁴ treatment of "the foreign," (2) to demonstrate some of the consequences of this problem, and (3) to highlight some of the associated epistemological issues that could be most easily addressed by more clearly articulating what it is that we're doing in our present approaches. The first is accomplished by considering the internationalization of psychology and of the history of psychology: the problem arises

⁴⁴ My use of "our" here is intentional, as is my informal tone, because I wish to speak plainly and directly to the dominant power in the discipline: English-speaking authors who publish regularly in major American journals like this one. "We" are a big part of the problem, and only "we" can fix it. Conveniently, however, "we" then also benefit: not everything that is of interest to us is visible from our vantage, and some of it requires the perspective of "the foreign" in order to rise above our horizon (see e.g., Burman, Guida, et al., 2015; Burman & Nicolas, revised & resubmitted; Nicolas et al., 2013).

from our continuing to follow “American”⁴⁵ sensibilities even as we become more open to international topics and “foreign” authors. The second examines the invisibilities that result from the resulting clash of disciplinary cultures, by comparing and extending two published microhistories. And, the third, by pointing out that celebration (of which historians are rightfully wary) has an inverse: neglect, and worse (of which we seem unaware).⁴⁶

My point, in short, is that we are being blinded to “foreign” contributions by disciplinary norms that are at the same time being left unarticulated. We don’t typically

⁴⁵ I have put “American” in quotes because it isn’t intended as a national attribution. In this case, for example, “American” includes those Canadians who adopt a professional literary persona in order to write and publish in the dominant style, in some cases even unconsciously adopting American spellings in their “non-professional” writings (e.g., by accidentally but consistently writing “behavior” instead of “behaviour” when the result is nonetheless intended for a Canadian audience). From this perspective, “foreign” then means everyone who doesn’t adopt this persona—regardless of whether or not doing so is their choice. (I am grateful to Stephen Retallick for pointing this out in my own writings for Canadian audiences.)

⁴⁶ This choice of term is far from optimal: it is fraught with unwanted celebratory connotation. My intent is not to argue that there are Great Men who have been *disrespected* by our treatment of their *legacy*. Rather, my intent is to suggest a more passive meaning, for which we are unfortunately lacking a precise term: the opposite of “celebrated,” which is less active than “ignored” while also ideally having the sense of an unknowing dismissal—without consideration—as an act performed unconsciously by an individual, or a group, to another (or to the products of that other group’s efforts) especially as a side-effect of the extant norms. An approximation is provided by “pretermitted,” but this still includes too much intentionality; an active disavowal of illegitimate heirs, who are known but denied.

If I could be allowed license to coin such a term, I might suggest “an-aesthetic” (following Burman, 2012b, 2014). By this, however, I would mean the following: *contrary to the extant aesthetic in such a way as to cause a numbing of one’s faculties*. This then has the desired connotation: *receiving audiences are “an-aesthetized” by their prior knowledge and conflicting norms, such that new and divergent potential importations are overlooked, unnoticed, and generally unattended-to*. And this is consistent with whole other bodies of historiographically-relevant literature (e.g., the “interessement” of Callon [1986], although our sought-after term would then become the impenetrably dense “uninteressement” or “disinteressement”).

I recognize, however, that coining new terms is awkward and to be avoided if possible. For the convenience of the reader, therefore, I will continue to use “neglected” in full knowledge that the choice to do so is flawed and the term itself potentially misleading. Thank you to Jacy Young for being the first to point out the problem, and especially to Shayna Fox Lee and Kelli Vaughn for each separately suggesting “premit,” as well as to Eric Charles for insisting on the usefulness of “neglect” over my continuing objections. David Jopling subsequently suggested “disciplinary blinkering,” and I have to admit that this is also appealing.

see this as a problem because the consequences are invisible *to us*: we follow our training, our judgment, and our intuitions to celebrate “good” history while ignoring “bad” history. But we can leverage the awareness in other areas close to us, where the problem is clearer. With the consequences then made visible—or, alternatively, with silenced voices made audible⁴⁷—we can begin to address the cause and work toward fixing the problem: “we” are neglecting a large and valuable source of contributions that meet many of our criteria for “good” history but do so simply in a “foreign” way.

1. Internationalization and the rhetoric of universals

Jeffrey Arnett (2008) is one of several contemporary psychologists to point out how much of “Modern Psychology” is skewed toward “American” interests, methods, and subjects. He also provided empirical support for his argument—that 95% of the world’s population is ignored, even as they are described—by examining the nationalities of the contributors, population samples, and editorial boards of six premiere journals published by the American Psychological Association. Briefly put: he showed that these samples we rely upon in studying and describing human behavior and mental processes are unrepresentative of humanity as a whole.

⁴⁷ This is my reading of Danziger’s (1994, 1997a) view of the future of the history of psychology—twenty years ago—as being found in feminist scholarship and internationalization. This is because they both present the views of outsiders who are nonetheless recognizable as being a part of the discipline: either women psychologists or the psychology of women, and foreign psychologists or the psychology of foreigners. This is the direction in which I have taken some of my own historical work, but with what I hope will be received as a friendly amendment: I do not view feminist scholarship and internationalization to be separate domains. Simply put: both seek to find the Other, and then to represent that perspective. Indeed, on this basis, I believe that their approaches can be unified as seeking to make audible subaltern “voices” (see e.g., Burman, Guida, et al., 2015). The historian’s challenge is then one of providing an adequate translation of that voice (the Unheard Other), so that it can be made “sensible” in a context other than its own. That psychology itself seems simultaneously to becoming more open to foreign perspectives is then a bonus.

A related critique has described psychology's narrow focus on particular populations as, acronymically, WEIRD: "Western, Educated, Industrialized, Rich, and Democratic" (Henrich, Heine, & Norenzayan, 2010a, 2010b). This view proposes that the assumption in psychology is that these "standard subjects" are representative of all human variation, but then also argues that—when a fuller sampling is taken—our experimental populations instead often appear as outliers. Thus, universal claims cannot be generalized from the narrow to the broad. And although some psychologists now accept this, many still believe that the brain can be appealed-to for universals: all human brains were shaped by the same distant selections-and-reinforcements of evolution, so—in theory (or, more precisely, according to evolutionary meta-theory)—all of the "species-typical traits" ought to be shared. Yet the same concerns apply there as well: contextual differences are becoming ever-more biologically relevant, with the rise of epigenetics and evo-devo, and so the rhetoric of universals then fails again (Burman, 2013b, 2014; Chiao & Cheon, 2010; Lickliter & Honeycutt, 2013; Robert, 2004, 2008). Psychology is thus biased toward "American" perspectives narrowly, and Western perspectives broadly. Worse still, this failure also biases the intuitions that guide our work (Stitch, 2010).

If we accept this premise, at least in the abstract, then we must also accept that it applies—following the principle of symmetry (Bloor, 1976/1991, 2014)—to historical research as well. Indeed, just as Arnett (2009) suggests this as a problem of Modern Psychology's philosophy of science, so too would I like to raise it as a problem of the History of Psychology's philosophy of history (cf. Teo, 2011). We would therefore have

a better view if we could take a broader perspective: our participant pool may well have stagnated, and so more mixing could therefore be in everyone's interests (cf. Burman, 2009a).

Of course, this is easier said than done: "American" psychology did away with the foreign language requirement of doctoral education as the legacy of an earlier educational system, no longer used and no longer necessary (S. Rosenzweig, Bunch, & Stern, 1962). We therefore have a problem without fully recognizing it: a pain, but one about which "we" are not properly aware. The goal here is therefore to make this pain more explicit: to share some of what is already obvious to our "foreign" colleagues, while at the same time building toward a deeper understanding of the issues under examination in the larger whole of this project.

2. Disciplinary problems

The elimination of the language requirement no doubt improved times-to-completion. And, for that, monolingual doctoral candidates are undoubtedly grateful. The unintended consequence, however, has been that most psychologists trained since the 1960s now miss something important in considering "foreign" sources: it's not just the *words* that need translating. What words *mean* is also in part a function of *sensibility*, which is often invisible in the text itself: tacit knowledge assumed of the reader by the author, which is then lost in translation (Polanyi, 1958/1962, 1966/2009).⁴⁸

⁴⁸ Polanyi's (1966/2009) famous line in this connection is "we know more than we can tell" (p. 4). I suggest that this can be formalized: our communications are always necessarily embedded in a larger system of meaningful implication, and indeed social pressure, which reflect the extant norms (cf. Burman, 2009a, 2012d, 2013b, 2014). By analogy to Fechner's (1887/1987) generalization of Weber's Law, we can thus suggest that "sensibility" accepts contributions within a just noticeable difference from those norms. Everything captured within this distance is then what is missed in translation; it's what's implied, but not

The best-known discussion of the emergence of this tacit background, in psychology, was provided by Kurt Danziger (1990) in his book *Constructing the Subject*. There, he described how the methods of contemporary “American” psychology came to be institutionalized and accepted as “the way in which members of the discipline defined their professional project” (p. 133). His subsequent book, *Naming the Mind* (1997b), did similar work for “American” psychology’s vocabulary. Together, though, they showed that the discipline’s ways of *doing* and *describing* are social practices; norms to be assimilated during each scholar’s professional development, which constrain what’s possible in contextually-appropriate and -adaptive ways (cf. Burman, 2013b, pp. 368-370).

In other words, the way we do things—indeed, the way we *think about* psychological phenomena and act upon them as Psychologists and Historians of Psychology (and as people who experience psychological phenomena subjectively)—is not a reflection of natural or universal categories (see Hacking, 1995, 1996, 1998, 2007; Martin & McLellan, 2013; Rose, 1990, 1984-1994/1996). That they are thought to be is just an assumption, embedded in a history (see e.g., Rutherford, 2011; Rutherford, Vaughn-Blount, & Ball, 2010; Teo, 2009; Teo & Febraro, 2003; Weizmann, 2004).

stated, in the original communication. As a result, in translation, the non-explicit aspects are left invisible: the primary source of “neglect” (see Note 46).

This stretching of a psychophysical law for historiographical purposes may go too far for some readers. But as a rhetorical device it seems to make the point clearly. The stretch is also consistent with Thurstone’s (1927) broader “law of comparative judgment,” which he intended explicitly to apply to *qualitative* judgments. From this perspective, historiographical sensibility is therefore rather like an “attitude” or “value” that could in principle be measured (cf. Thurstone, 1928, 1929). The challenge for historians—or perhaps for philosophers of the history of psychology—is thus to define the bona fide historiographical criteria necessary for producing high-quality outcomes, regardless of differences in international sensibility, and then allow for creativity within those bounds.

Thus, the Discipline could easily be different (e.g., Burman, Guida, et al., 2015; Nicolas et al., 2013). And indeed, elsewhere, it is (e.g., André, Bloch, & Bornstein, 1994; Bacopoulos-Viau, 2012; Carson, 2007, 2014; Guillin, 2004; Hogan & Vaccaro, 2006; Lombardo & Foschi, 2003; Parot, 2000).

3. International sensibilities

Recognizing the failure of *universalism* led to the study of *indigenization* (Danziger, 2006, 2009). This is the postcolonial view that the spread of psychological knowledge is projected out from centers of disciplinary power and into the foreign periphery (see also Pickren, 2009b). The result, however, is not a transmission of “pure” knowledge (e.g., Shapin, 1988-2007/2010). Instead, messages are reconstructed—massaged—to fit more consistently with local interests (e.g., Burman, 2012d; Carson, 2007, 2014).

For the discipline we recognize today in English-speaking North America as “Modern Psychology,” this is generally accepted to have begun in the late-nineteenth century with a projection of knowledge out from Germany: Wilhelm Wundt (1832-1920) attracted hundreds of students to his laboratory in Leipzig, and many of them in turn used what they learned to found new psychological laboratories at their home institutions (see Benjamin Jr, Durkin, Link, Vestal, & Acord, 1992; Tinker, 1932). That said, however, Wundt’s influence was also imported into the U.S. in a very particular way: Edward Bradford Titchener (1867-1927) popularized a decanted version of Wundt’s experimentalism that omitted the social embeddedness required of the original program (see esp. the chapters collected in Rieber, 1980a; Rieber & Robinson, 2001). This

indigenized reconstruction then made possible the particular character of its replacement. Indeed, it was Titchener—and not Wundt—to whom John Broadus Watson (1878-1958) responded in introducing his “Behaviorism” (J. B. Watson, 1913, p. 164; see also Larson & Sullivan, 1965; O'Donnell, 1985). It was thus also in this way that “American” psychology separated itself from its European roots: “The ‘behaviorist revolution,’ as it has so often been called, marks the point at which the ‘new’ psychology achieved full American citizenship” (Koch, 1985, p. 25).

More recently, the center of power has shifted from Europe to the United States. And because the “American” style of experimentalism⁴⁹ has since been accepted as the dominant approach to the study of psychological phenomena, worldwide, the projection is therefore now *out* from U.S. centers and *into* the periphery of the rest of world. But many other national psychologies still continue to operate in the background; as with Watson’s response to Titchener, they even produce the occasional “backlash” that then takes root (see e.g., Teo, 2013b; Weidemann, 2013).

In other words, there are *many* ways of *doing* Psychology. One can even put this in stronger terms: there is *necessarily* a plurality of psychological voices and approaches (Teo, 2010a). Or, stronger still: “all psychologies are indigenous psychologies” (Marsella, 2013). Yet this then implies—again following the principle of symmetry—that

⁴⁹ Green’s (1992, 1996) observations in this connection are especially interesting: the “American style” includes variations on themes that are substantially different from their original sources. Some importations—such as Operationism (or, equivalently, Operationalism)—were even subsequently repudiated by their originators. To foreigners who are familiar with those original sources, this can make “American” psychology seem very weird indeed. (And yet those who wish to participate in the discipline must learn to delight in the Emperor’s finery, lest they be dismissed as “unfit” or even “ignorant” to question it.)

there must also be a plurality, including perhaps even national differences, in *the doing* of *the History of Psychology*.⁵⁰ It follows too that this will occasionally be marked by clashes in sensibility, and even backlash, in which high-quality material is omitted from consideration for reasons unrelated to merit or quality (cf. the recent debate between Robinson, 2013a; Robinson, 2013b, 2014; and Danziger, 2013; Teo, 2013a; also Brock, 2014a; Pettit & Davidson, 2014). Indeed, we see such a clash exemplified in the “American” reading of an important new biography describing Jean Piaget’s (1896-1980) nomination to the professorship in Geneva that he held for most of his career (Ratcliff & Borella, 2013). This will serve as the narrow illustration of our argument, while highlighting some of the benefits of adopting a broader perspective in the following chapter.

4. The problem posed by the new biography

At issue here in this chapter is, primarily, *the way in which* Ratcliff and Borella (2013) responded to an important implicit question: “How did Piaget get to psychology?” (Vidal, 1997b, p. 124). This is because what they said in the English-language abstract to their French-language article is highly problematic for “American” audiences: “His nomination [to the chair in psychology] put many obstacles in his way, which the young

⁵⁰ Teo (2013b) points out, for example, that the very focus on Wundt is a function of American historiography (via esp. the influence of E. G. Boring [see also Kelly, 1981]). He suggests that an equally acceptable alternate approach would be provided by German historiography: shifting the story away from Wundt to other “founders,” or to the Prussian requirement that as of 21 August 1824 psychology be a required course at university (p. 4). He also suggests that Danziger’s influence in American history of psychology is a reflection of his subject’s—Wundt’s—central position in the American historiography of psychology (p. 4n7). Yet my intent is not to suggest an alternative origin story. Rather, I propose that this is simply the case everywhere: there will always be “a founder” to be celebrated, but who the person is that receives this label is a function of the interests of those who apply it.

Neuchâtel conqueror had to surmount” (p. 1).

Briefly put: presenting Piaget as a great conqueror of psychology is not acceptable to the present sensibilities in the History of Psychology as it is done by Americans and published in top English-language journals. Yet how such an influential person came to adopt and then influence the discipline is indeed an important question. There is, therefore, a gap in scholarship to be addressed (see Ball, 2012; also Appendix A).

If the new biography did not address this gap, at least partially, then there would be no story to tell. But it does; ably. And “we” would have dismissed it as celebratory; to “our” loss. More troubling still, the authors recognize that their framing is awkward for this audience. Yet this recognition is itself useful: the resulting paradox—the *appearance* of celebration, *in translation*, given the *intent not to celebrate* in the original—affords all that follows.⁵¹

First, therefore, we must make the contribution clear: we must contextualize the new biography by reviewing the relevant parts of what we presently know in English. Yet the examination of the process whereby Piaget received the honorary doctorate provides more than a review of *whatever it was* that came to be celebrated. The associated archival documents also provide us with a deconstruction of the process of *assessing* a contribution as *worthy* of celebration. The resulting schematic is then applied to the new

⁵¹ When I first had the suspicion that the article by Ratcliff and Borella (2013) might be dismissed, I simply wrote and asked about the authors’ intent. The answer is revealing of exactly the problem that I hope to remedy, and also of the awareness among “foreign” authors that such a problem exists: “I’m afraid you’re right for the conqueror stuff - I mean it sounds awkward [sic] for the anglo audience” (Ratcliff, personal communication, 22 November 2013). He then clarified: “My approach is NEVER hagiographical or celebratory.... My goal is to attempt to render a sense of the mindset... never to glorify the character” (my trans of Ratcliff, personal communication).

biography, so that it can be appropriately evaluated: it is situated, deconstructed, and discussed according to a standard previously applied to its subject. Finally: we examine the consequences (across several epistemic levels), as well as what to do about them. And that in turn sets up the discussion in Chapter IV.

5. What we presently know

It is difficult to escape the influence on “American” audiences of what has been called Piaget’s “resurgence,” “revival,” and “rediscovery” in the late-1950s and early-1960s (see Müller et al., 2013, p. 52; discussed by Bond & Tryphon, 2007; Elkind, 1979; Hsueh, 2005, 2009; Kessen, 1996; Voyat, 1977). Most historical examinations are written through that lens—often referred-to, more simply, as “Piaget rediscovered” (following Duckworth, 1964)—with early works “anticipating” the findings and arguments of later influential works. Still, there is a handful of high-quality historical sources to which we can productively turn. And we can simplify the examination further by turning to one particularly well-documented microcosm.

5.1. Harvard. Contemporary English-reading audiences were reintroduced to the *first* American Piaget, as a separate entity from the rediscovered *second* American Piaget, primarily through Jerome Bruner’s (1915-2016) recollections of his time at graduate school in the 1940s. This provided a less rosy view than most readers at the time had come to expect. As he recalled:

We all knew about Piaget. I cannot remember a time when I didn’t! I had read him on language and thought, on moral judgment, on physical causality [i.e., the early translations]. I had found him fascinating, but not as a *theorist*. He was an

astute observer of children and his observations were intriguing—particularly those about egocentrism. It never occurred to any of us graduate students at Harvard that he had any bearing on anything aside from the phenomena to which he addressed himself. The controversy surrounding his work was of that boring kind in which pedants complained about the small number of children he observed. That was “dust bowl empiricism.” Piaget was simply a self-contained one-man show. (Bruner, 1983, pp. 133-134; emphasis as in the original)

In other words: Piaget was *known* before the Rediscovery, but he was not *celebrated*. Instead, Bruner recalled, the students’ enthusiasm at the time was for Sigmund Freud (1856-1939).

This is consistent with other reminiscences. For example: Hsueh (2004) notes that Henry Murray (1893-1988), a long-serving member of the Harvard faculty, recalled that Freud had originally been the psychology department’s first choice for the Tercentenary proceedings in 1936 which ultimately awarded honorary doctorates to both Piaget and Carl Jung (1875-1961). Yet we also see from Hsueh’s in-depth study of the nomination papers held by the Harvard University Archives that the situation at the time was much more complex than these reminiscences suggest. And, indeed, the process of coming to understand the place and contribution of Ratcliff and Borella’s (2013) new biography—about how Piaget was raised to his professorship in psychology at Geneva—can be simplified greatly by reviewing what we know about how he was nominated for that honorary doctorate.

5.2. Piaget’s honorary doctorate. The Harvard nomination papers offer a wealth

of detail: not only do we get a glimpse of how an elite group of psychologists viewed their own discipline at the time, but we also see how much attention psychological phenomena had attracted from outside the discipline (from e.g., biology and education). To make this clearer, Hsueh's (2004) narrative has been simplified in Table 1.

[For the table, see over...]

Table 1. A tabular simplification of Hsueh's (2004) narrative, highlighting the top nominees from each relevant selection group.

Psychology			Education		Biological Sciences	Executive Committee
Oct 1934 (p.22: Boring)	Dec 1934 (p.23: Boring)	Oct 1935 (p.25: Allport)	Nov 1934 (p.26: Holmes)	Nov 1935 (p. 25: Holmes)	Dec 1934 (p. 23: Henderson)	Nov 1935 (p.27)
Lashley [‡]	Lashley [‡]	McDougall	Thorndike [†]	Terman	Pavlov	Terman
Jung*	Jung*	Freud	Dewey [†]		Janet*	Piaget*
Köhler	Janet*	Stern	Terman			
Thorndike [†]						
Tolman						
(Terman						
[p.26])						
Koffka						
Luria						
Stern						
Wertheimer						

* Recipients in 1936

[†] Previously awarded, and therefore ineligible

[‡] Harvard faculty, and therefore ineligible (Elliott, 1999, p. 164)

This presents something of a Who's Who of early-1930s American Psychology. Yet it is interesting to note that Jung received an award, and not Freud, for two reasons *unrelated to merit*: not only was Freud thought to be too old to travel (p. 22), but Harvard president James Bryant Conant (1893-1978) was also reluctant—for personal reasons—to support his nomination (p. 22 [citing Murray]). It is even more interesting to note, however, that the nomination process as it affected Piaget went much the same way. There are thus two additional factors, *beyond merit*, that need to be examined with particular attention when considering such celebrations: “health” (which is about *the subject* of celebration) and “internal politics” (which is about *the decision makers* and *their process* of determining who or what *should* be celebrated).

Taking this perspective, we then see that Piaget emerged as a candidate only after Ivan Pavlov's (1849-1936) declining health prevented him from undertaking the long overseas journey from the Soviet Union to the U.S. east coast (p. 25). We see also that there was no mention of Piaget's own health problems; nor were there any mentions of his interactions with Sabina Spielrein (1885-1942), who served for a time as his analyst (see Vidal, 1995/2001; also Vidal, 1994, pp. 162-181). We will therefore turn to the second factor, “internal politics,” which Hsueh (2004) discussed in detail.

5.3. Decision-making and internal politics. Pavlov was older than Freud, and his late withdrawal from the Harvard nominations process—in October of 1935 (less than half-a-year before his death in February of 1936)—did not come as a complete surprise.⁵²

⁵² The Tercentenary records show that Pavlov had indicated in February of 1935 that his attendance was always uncertain: there was a possible conflict with an important meeting in Spain, and also he had concerns regarding the seasickness that would result from braving “the Atlantic autumnal storms” (Pavlov, 1935). His nominator, Walter B. Cannon, was then encouraged to use his influence to convince

But it was incredibly inconvenient: the committee had held out hope, until the eleventh hour, that he would come. This was because they intended for Pavlov, a winner of the Nobel Prize, to provide leadership in anchoring the symposium on “Factors Determining Human Behavior” (p. 26).⁵³ With the Tercentenary looming, and the symposium scheduled for September of 1936, they then had less than a year to fill the resulting void.

Hsueh (2004) reports that the committee felt pressured to find a similarly interdisciplinary scholar with comparable interests, especially those relevant to educational and social psychology. They quickly worked through an initial list of nominees, but disqualified them all. The committee then sought a second round of suggestions. And a third. Ultimately, they produced the last list themselves:

the names of Jean Piaget and Lewis Terman [1877-1956] were put forth together, not only as possible replacements of [sic] Pavlov to give a keynote speech to the symposium but also as most [sic] qualified candidates whose work had important implications in the fields of education, human learning, and intelligence, the same lines of inquiry that Pavlov inspired. (p. 26)

In short, Piaget and Terman were initially on equal footing. From the Table, however, it is clear that Terman should have been preferred: he had a much broader base of support. (Of course, only correcting the misunderstanding of Lashley’s nomination being for

Pavlov to choose to attend anyway. (For more on Cannon and Pavlov, see Todes, 2014.) By August, however, this seemed very doubtful: “he has told his son that his recent trip to London was his last journey except, perhaps, the trip to Madrid” (Cannon, 1935a). By the time the third round of nominations was sought in October of 1935, however, a memorandum in the files indicates that Cannon thought the odds of Pavlov attending had improved (*Memorandum on Pavlov*, 1935). This perhaps explains the committee’s delay in seeking a replacement.

⁵³ The papers presented in this session were ultimately published together in a volume of the same name (Harvard Tercentenary Publications, 1937; see also Elliott, 1999, pp. 163, 165-167).

Terman—rather than Tolman [p. 26]—made this obvious.)

We can suggest in retrospect that both men would have acquitted themselves admirably, although Terman certainly more intelligibly. And the committee knew this, as I discovered when I retraced Hsueh's steps in the archive: "Piaget speaks English very poorly and could hardly have the general influence in the symposium and the round table which Terman might well have" (Wilson, 1935b). Indeed, that it was Piaget and not Terman who ultimately received the nomination is somewhat peculiar when examined without the exuberance of the Rediscovery. As Hsueh (2004) reports, "neither the psychology nor the education faculty seemed enthusiastic about him" (p. 28).

Hsueh's archival work suggests that Piaget was in fact no one's first choice. Instead, he was known by committee members as a result simply of their connection to the Rockefeller-funded Hawthorne Experiments conducted out of the Harvard Business School (pp. 31-34; see Gillespie, 1991; Hsueh, 2001, 2002). This then provides a partial answer, related to internal politics, to the question of how he received the nomination. But the Hawthorne Experiments were at best instances of industrial or organizational psychology, not educational or social psychology: the connection from Hawthorne to the committee, and its goals, therefore can't be the whole story.

Fortunately, the committee's decision became clearer when I had the opportunity to immerse myself in the archives, with the benefit both of hindsight and of being able to build on Hsueh's work. In short: Terman seems *not* to have been *rejected*. Instead, we find the second part of our sought-after answer in the committee's comments on his health: "Terman is fully worthy of the honorary degree and it would be very nice to give

him one because he has been handicapped by tuberculosis (as I am informed) and has had to lead a rather quiet life” (Wilson, 1935a).

Terman was dropped soon after. And I saw no other reason given. Thus, we might ask: after the trouble with Pavlov, could they risk it with another less-than-perfectly-healthy candidate? It seems not.

While it is the case that Piaget’s champion on the committee thought him more “inspired and novel” (qtd. in Hsueh, 2004, p. 27), the man reflected in the nomination papers is clearly not a *Great Man* whose recognition was *inevitable*. Indeed, I discovered that the committee saw their choice of Piaget as a “gamble” (Wilson, 1935b). But it seems to have been the safer option. And, they noted, it also fit with President Conant’s goal of balancing established scholars with younger investigators whose contributions were not yet well-known (Hsueh, 2004, p. 30).

Still, Piaget seems like a lightweight when compared to the other candidates. The nomination papers make the differences clear: Pavlov was “the most eminent physiologist Russia has produce[d],” “awarded the Nobel Prize in 1904,” and “a highly stimulating influence in physiology, psychology, and philosophy” (Cannon, 1935b). Pierre Janet was “without a doubt the most distinguished French psychologist and psychopathologist,” “generally regarded as having founded psychopathology as a separate discipline,” “a well-loved public figure... brilliant lectures,” and “in good health” (Boring, 1934, p. 2). And Jung was “one of the important pioneers of the investigation of personality,” “A man of universal erudition and true wisdom,” “at the summit of his career,” and “one of the deepest and most provocative thinkers of the

contemporary world” (Boring, 1934, p. 3).

By comparison, Piaget’s biography was—to put it mildly—more subdued. His nomination does not shine as brightly:

His interests and training involve natural history, philosophy and sociology as well as psychology. He has the true imagination of a creator. His work is that of systematic research upon the conceptual content and mode of functioning, and mode of apprehension of the child mind. These things he has studied in their developmental characteristics. He sees the development of children’s concepts upon a background of sociology and has in his recent volume, for example, investigated such concepts as retributive and distributive justice, collective responsibility, etc., in their genetic phases and set up hypotheses which seem to correlate the data regarding the origins and successive transformations of these concepts during the developmental process. Other illustrations could be given to show that his work has important implications for the field of social learning although it is not a study of the learning process as such. (Wilson, 1935b; referring to Piaget, 1932/1932)

So then: Piaget was imaginative and creative and systematic, but not a *Great Man* like Pavlov, Janet, or Jung. He was, however, the last man standing. And they needed a body to fill a space in the schedule. Thus, Piaget—just one month after celebrating his fortieth birthday—received the honorary doctorate originally intended for a Nobel Laureate more than twice his age.

While these discoveries are in keeping with the present “American”

historiographical *preference* for non-celebratory narratives, it is awkward for an “American” *reading* of the new biography by Ratcliff and Borella (2013). This is because they *seem* to present the Harvard doctorate as an irony: the rationalization for *framing* Piaget’s tortured nomination to a full professorship at Geneva as being the story of a “conquering hero” (pp. 16, 18). And this framing no longer seems justified (even if it had been acceptable). Yet following the heuristic extracted from the Harvard nomination process—again appealing to symmetry—their *apparently* celebratory essay can be examined in two ways that extend beyond the fundamental merit of their study: their study’s underlying “health” *as history* (which is about *their evidence and its presentation* [i.e., how they have done their work]), and the “internal politics” of the receiving audience (which is about *us*).

6. What’s new in the new biography?

Ratcliff and Borella (2013) locate Piaget in the years immediately after his first moves into psychology, but also immediately before he had begun to publish the books in the “infancy trilogy” for which he later became most famous (viz. Piaget, 1936/1952, 1937/1954, 1945/1962). It includes a depth of archival work that puts their study on par with *Piaget before Piaget* (Vidal, 1994) and the best chapters in *Jean Piaget and Neuchâtel* (Perret-Clermont & Barrelet, 1996/2008). It is also “correctly” motivated:

Jean Piaget is best known for his theory, the scientific impact of which was—in its time—greatest among psychologists. [Yet its very success] has generated a kind of epistemological obstacle; as if Piaget’s work is, by its intrinsic qualities, ahistorical in nature. Little work has been done to counter this attitude. (my trans.)

of Ratcliff & Borella, 2013, p. 1)

Indeed, it is this problem—contemporary psychology’s ahistoricism regarding matters of theory (cf. Teo, 2013a)—which then affords their approach: a combination of cultural history and microhistory, applied to the life-course of an historical subject. In short, they provide some of the story of how the theory came to be.

The result is not a biography in the traditional sense, because Piaget is not really their subject. Instead, Piaget provides the pivot around which their story turns. (It is a biographical microhistory.)⁵⁴ The resulting study, like the study of the Harvard nomination process, is therefore properly about internal politics. And that is not only acceptable, as history, but it can also be revealing of broader issues not presently visible (see e.g., Green, 2002, 2004a, 2007).

6.1. Geneva. The history of the hiring institution, the Rousseau Institute, is not well-known in English. We know that it was founded in 1912 by Edouard Claparède (1873-1940) as a research-based teachers’ college, but against the University of Geneva’s regulations and thus of necessity with the assistance of a private foundation (see Claparède, 1925, p. 93). We know too that the institute became attached to the university in 1929, when it affiliated with the Faculty of Letters, and also that its focus then began to shift away from pedagogy and toward psychology (Hofstetter, 2004). Yet we know little else; the Institute itself has been the subject of a proper inquiry only infrequently, and is rarely mentioned in English aside from in the occasional book review (see e.g.,

⁵⁴ In his response to the draft of this essay, Ratcliff (personal communication, 3 December 2013) suggested Jill Lepore’s (2001) “reflections on microhistory and biography” as being broadly representative of his approach.

Jahoda, 2013). We can therefore fill another gap in “American” scholarship by providing some of that additional context here. Note, though, that—if we are not to reinvent the wheel—we must do this by relying on “high-quality”⁵⁵ secondary sources published in French.

To that end: Rita Hofstetter has written the most comprehensively, and the most recently, about the history of the Genevan approach to “the educational sciences” (see e.g., Hofstetter, 2009, 2012a, 2012b; Hofstetter, Ratcliff, & Schneuwly, 2012; Hofstetter & Schneuwly, 2007). Her *habilitation*—entitled, *Genève: Creuset des sciences de l'éducation* [Geneva: Crucible for the sciences of education]—is especially illuminating for our purposes. She writes:

Relations between the Institute and the University were not limited to the Faculty of Letters. Instructors from other Faculties (medicine, law, and the sciences) continued to teach courses at the Institute, or to open their lecture halls to its students. And the ties stayed strong between the Institute and the Laboratory of Psychology in the Faculty of Sciences, during this period, even if only to provide a joint edition of the *Archives de psychologie* and to organize various courses, conferences, and congresses (my trans of Hofstetter, 2010, p. 265).

She continues, highlighting both the difficult financial circumstances and the importance of international assistance after WWI:

⁵⁵ It’s worth reiterating that this judgment has two aspects: one is about the work under consideration, and the other is about the criteria for judging quality. Someone else might therefore disagree with me, and suggest another source that ranks more highly according to their perspective. And, indeed, at the individual level this disagreement is fine: the debate is constructive, and everyone benefits. It is only at the collective level that it becomes problematic: blind, unexamined, uncritical, prejudicial—systematically biased. That is what I am targeting.

Of course, this collaboration with the University was primarily the result of personal relationships and initiatives, and received little formal support.... Instead, the State's large deficits forced it to consider cuts, which led even to the suggestion that the Laboratory of Psychology be relocated. Opposing this, to maintain its proximity to one of its most precious assets, the Institute undertook to rent back the space. This was made possible by the support of the Rockefeller Foundation.... (my trans of Hofstetter, 2010, p. 266)

Hofstetter thus situates the continued operation of the Rousseau Institute in the 1920s, and therefore also the hiring of Jean Piaget, in a larger context that includes the projection of U.S. power and interests through the operations of its charitable foundations (see also Parmar, 2012; Solovey, 2013; Solovey & Cravens, 2012). And this in turn connects back with other things we know from later on, in terms of how Piaget and Inhelder were supported in the period leading up to the Rediscovery (see e.g., Inhelder, 1989; Bronckart, 1980; Hsueh, 1998, 2009). Now, then: to the new biography.

6.2. Who was this candidate for professor? After what we would today call “postdoctoral studies” in Zurich and Paris, Piaget was hired in May of 1921 to serve as *chef de travaux* (director of studies)⁵⁶ at the then-still-independent Rousseau Institute. He also volunteered to serve as *Privat-Dozent* (outside lecturer) for a course at the University of Geneva examining “experimental research on the child” (Ratcliff &

⁵⁶ This is the translation provided in Piaget's (1952a) first English-language autobiography (p. 245).

Borella, 2013, p. 5n21).⁵⁷ At the same time, and in parallel, Claparède—who was at the time Professor of Experimental Psychology in the Faculty of Sciences—proposed to the Faculty that Piaget teach a class on “experimental psychology” (p. 5n22). These proposals were sent up the administrative ladder, variously discussed, and received formal approval from the Senate in July of 1921 (p. 6n26).

This arrangement turned out to be conducive to Piaget’s productivity: by 1925—which is to say, before his thirtieth birthday—he had published two books and 24 articles on psychological and philosophical topics (p. 6n28). He had also turned down a call from his *alma mater*, the University of Neuchâtel, to take up the post of *professeur ordinaire* (full professor). His reason was that the proposed workload was “crushing” (p. 6n29). Yet this refusal was met with a return offer for a “reduced chair” (p. 6), combining some of the responsibilities of the then-vacant Chairs in Philosophy and Sociology but leaving much of his time free for research.

Piaget accepted the revised offer in April of 1925, resigning from his position as *Privat-Docent* at Geneva (p. 7n35). But he stayed active at the Rousseau Institute, returning fortnightly for meetings and to oversee continuing research projects (p. 7n37), especially at its laboratory-school: the *Maison des Petits* (Piaget, 1952a, p. 246; see also Perregaux, Rieben, & Magnin, 1996).

When Claparède fell ill in the winter of 1927-1928, the Dean of the Faculty of Sciences at Geneva asked Piaget to serve as his replacement (p. 8n43). Piaget assented

⁵⁷ To preserve and reflect the quality of the archival research reported, references to material supported by primary sources will cite page and footnote number in the published original. All quotations from this article, including of sources quoted, are translations by the author.

easily. As part of this return, however, Piaget also asked that Switzerland's premiere French-language newspaper—the *Journal de Genève*—publish some of the details: “because,” he explained, “I have many friends in Geneva and would be happy to remind them of my existence” (qtd on p. 8). (This practice of “self-presentation” is one that he later continued, albeit sometimes with less success [p. 9], and it is something to which we will return briefly below.)

In June of 1928, with Piaget set to return to Neuchâtel at the end of his contract, the recuperated Claparède proposed that a new Chair be created—in “the history and psychology of sciences” (p.8n46)—with the goal of replacing himself over his sabbatical and at the same time providing a permanent home for Piaget in Geneva. This proposal was warmly accepted by the Faculty in July, but for Piaget's request that he remain at the rank afforded him at Neuchâtel (p. 9n48). Instead, it was decided in September that he would be offered the lower rank of *professeur extraordinaire* (contractually-limited visiting professor).

6.3. Conflict. We thus find the origins of the “obstacles” to be “surmounted” and “conquered” that are so rhetorically problematic for “American” audiences. Briefly put: in Switzerland, university governance is partially integrated with state governance. Appointments at the University of Geneva are therefore made first by the relevant Faculty, and then they are confirmed at the Ministry of Education for the Canton of Geneva (*Département de l'Instruction Publique* [DIP]).⁵⁸ But that's not how Piaget

⁵⁸ I am grateful to Marc Ratcliff for providing the correct English rendering (personal communication, 3 December 2013), which I otherwise would have translated directly as “the Department of Public Instruction.”

proceeded: the Rousseau Institute was also overseen by the DIP—prior to its by-then-imminent affiliation with the university (Hofstetter, 2010, pp. 223-241)—and so he appealed through what may well have seemed to him to be his institution’s proper hierarchy.

Because it was the head of the DIP, Albert Malche (1876-1956), who had approved in September of 1928 the Faculty’s plan to create the new Chair (p. 9n49), it was then Malche to whom Piaget appealed in October in an attempt to secure his desired appointment (p. 12). Yet this meant that the same administrative process—to define the terms and conditions of the new Chair which Piaget would occupy—were undertaken simultaneously at two different levels of governance, each of which did not have the support of the other.

Malche proceeded to create the Chair at the rank of full professor, but without any input from the Faculty. He supported this decision by noting that Piaget had not only held the rank of full professor in Neuchâtel, but also that he had recently been called to a Chair in psychology at Liège in Belgium (p. 12n70). His intent was therefore probably strategic: to keep a promising young talent in Geneva. The result, though, was “a dreadful scandal”—as Piaget put it in a letter to Claparède in December of 1928—“having the effect of a pebble [tossed] in a pond” (p. 12n73). One of Piaget’s earlier supporters, the botanist Robert Chodat (1865-1934), even turned against him: Chodat called the proposed appointment “parasitical” (p. 13n74). And while the change of heart might have been in part because his own son was being considered for the same promotion (p. 13), his primary objection was no doubt to an outsider being parachuted in at a higher-than-

expected level in an out-of-the-ordinary way as a result of an impending institutional affiliation. Indeed, Chodat's sentiment seems representative of the Faculty's general response to the Administration's meddling.

In January of 1929, Piaget chose to accept the position as defined by the Faculty: three years probationary at the lower rank, teaching one hour per week, with the bulk of his salary to be paid by the Rousseau Institute. As he explained: "I prefer the confidence of my colleagues over a title that they have not given me themselves" (p. 14n82). He also rejected a possible alternative: "I confess that I prefer a limited Chair in the history of scientific thought in the Faculty of Sciences than the same Chair as full professor in the Faculty of Letters" (p. 14n82). His justification: "I cannot conceive the history of scientific thought except as enlivened by its continued contact with the science that makes it [possible]" (p. 14n82).

6.4. Promotion. Piaget's contract with the Faculty of Sciences was renewed in 1932. He then received the honorary doctorate from Harvard—which was not in fact a doctorate of *science*, but rather a doctorate of *letters* (Elliott, 1999, p. 165n)—in 1936. Despite this honor, however, he remained unpromotable at the University of Geneva: internal politics, in the Faculty of Sciences, would not allow it. The earlier scandal's waves rippled ever on.

When Piaget was eventually raised to full professor, in February of 1940, it was to a Chair in *Sociology* in the Faculty of Social and Economic Sciences (p. 17n99). Then Claparède died, in September of that year, and the Faculty of Sciences again found itself in need of a replacement. In Claparède's place, Piaget was therefore named Director of

the Rousseau Institute and full professor of Experimental Psychology *in the Faculty of Sciences* (p. 17n100). The promised position was thus delivered, but not because it was inevitable. He also certainly didn't take it by force: nothing was "conquered," in the way that this word is usually meant in English.

7. So what?

At the time of his nomination at the University of Geneva, in 1929, Piaget had authored four books. These are known in English by their translated titles: *The language and thought of the child* (Piaget, 1923/1926), *Judgment and reasoning in the child* (Piaget, 1924/1928), *The child's conception of the world* (Piaget, 1926/1929), and *The child's conception of physical causality* (Piaget, 1927/1930). And it is true that these garnered some attention at the time, but not as contributions to developmental theory.

To the extent that the books were celebrated, it was for their methods (see e.g., Blumer, 1930; Isaacs, 1929, 1931; Lynd, 1927; E. Murray, 1931; Stone, 1929, 1930; C. O. Weber, 1927; also Bain, 1937, p. 432). It was these methods that then informed the Hawthorne Experiments' approach to interviewing, and they that in turn launched the Human Relations movement and thus provided the foundations for what we now call "human resource management" (Hsueh, 2001, 2002; see also Bond & Tryphon, 2009; Duveen, 2000; Mayer, 2005; Opper, 1977). As we have seen, it was also this—a program totally disconnected from Piaget's early studies of child development and unrelated to his later program in genetic epistemology—that connected up with the internal political context at Harvard that led to his being awarded the honorary doctorate in 1936.

Yet this is not what ultimately makes the new biography important for

“American” audiences; it is not that the young Piaget was able to overcome—or “conquer”—the challenges that he faced. Rather, the biography’s importance derives from the recognition that the meaning of “Piaget” in “American” psychology was for several decades a reflection of the same few works that the nominations committees at Harvard and Geneva had considered: these books, along with the companion volume on moral development (Piaget, 1932/1932), were the only major works made available to English audiences until after WWII. We can therefore use the committees’ deliberations to provide a new perspective on the state of Psychology—and Piaget’s place within it—before the blinding explosion of Rediscovered interest in the 1950s and 1960s.

In short: studying the nomination committees’ archival records can serve as a shield against the influence of any later celebratory (or backlash-negationist) sentiment. The resulting reframe of the new biography’s contribution is then also more consistent with the present “American” historical sensibility—often called by adherents “the new history” (see Furumoto, 1989, 2003; Teo, 2013a)—because it places both articles’ depth of archival research in their social and cultural context. Here, though, that doesn’t mean *ignoring* the contents of Piaget’s intellectual contributions; instead, we must treat those contributions as belonging to the context in which they were received *as* contributions (i.e., “health” and “internal politics”).

8. What new does this contribute?

As a result of all of this, we are now able to shift from *form* back to considerations of *content*. We are therefore now able to answer—at least partially—Vidal’s (1997b) question: “How did Piaget get to psychology?” (p. 124). Indeed, from the perspective

afforded by the new biography, it's a relatively short story. Piaget got to Psychology, in the sense that he was recognized with a Chair in the area at his home institution, in the same way that he received his honorary doctorate from Harvard: directly following the death of a preferred senior colleague (first Pavlov and then Claparède), and only in part as a result of the quality or impact of his work.

This is a very different view than that afforded by the Rediscovery hagiography. It is also a very different view than we might have come-to had we dismissed the new biography as inappropriately celebratory. Given this, and everything reviewed to this point, we must therefore conclude that the new biography is not only “healthy” as history but also that it is too important for “us” to disregard.

Still, an objection—anticipated from those interested primarily in Piaget’s intellectual output—is that this examination has *ignored* the ideas that were meant to be placed in context: nothing here discusses what he said, when, or why. Yet it is clear that Piaget did *not* have an impact at the time on what we now would consider the “obvious” areas of his influence (such as early childhood education [Beatty, 2009]). Indeed, Lawrence Kohlberg (1927-1987)—who is known today primarily for advancing the project introduced in Piaget’s (1932/1932) *Moral judgment of the child*—later claimed that his primary theoretical influence was actually James Mark Baldwin (1861-1934 [Kohlberg, 1982, p. 280]). Thus: as with the Hawthorne Experiments, Piaget’s influence on Kohlberg seems to have been primarily methodological. (Recall too that this is consistent with Bruner’s autobiographical reminiscence of Piaget’s place at Harvard at the time.) *Those* are therefore the ideas that matter here; at *that* time, for *this* story.

Reflecting also on how the nomination processes compare, at Harvard and at Geneva, it is interesting to note the similarities: an unexpected proposal was met with resistance, but supported by administrators (i.e., it was “healthy”), with disgruntlement in the resisting departments (i.e., it conflicted with “internal politics”). This structure has nothing whatsoever to do with Piaget specifically, or his theory. Yet it is also interesting to note the common conclusion of the time, which does: that Piaget’s “real merit” was as a *Sociologist*.

This was surprising to discover once, but it appeared three separate disconnected times in the course just of preparing this chapter: at Harvard in 1936, at Geneva in 1940, and also previously at Neuchâtel in 1925. (Note also the reviews of his early works in sociological journals [e.g., Bain, 1937; Blumer, 1930].) This is rather remarkable, and completely unexpected given how Piaget is commonly perceived.

8.1. Self and social. Piaget’s theory is understood in English today to be distinctly asocial (even anti-social). This has long been known by Piaget scholars to be incorrect (see e.g., Kitchener, 1981; 1991, 2000, 2009; Smith, 1995; also Lourenço & Machado, 1996, pp. 150-151). Yet, in the blinding exuberance of the Rediscovery, the role of “early social theorist of development” in the American psychological pantheon was instead given over to Lev Vygotsky (1896-1934).

Is that now in question? Perhaps.

The elevation was supported by Vygotsky’s (1934/1962) apparently-devastating critique of Piaget’s early theory. And the corrections provided there then served as the justification for using one theory in counterpoint to the other. But this now looks solely

like a function of celebration: Piaget's theory was virtually ignored until the early-1960s, and such a critique could therefore only gain the importance it came to have in light of that reflected interest. That the celebratory perspective of Vygotsky's supremacy has continued is then itself a function of an associated neglect, because most readers have remained unaware that Piaget (1962/1979, 1962/2000) responded. Indeed, he made it clear in his comments that the concerns identified thirty years before had been subsequently addressed: "on certain issues I find myself more in agreement with Vygotsky than I would have been in 1934, while on other issues I now have better arguments for answering him than would previously have been the case" (Piaget, 1962/2000, p. 242).

Still, the fact remains that Piaget never explicitly *assumed* the mantle of sociality after he had addressed Vygotsky's critiques; cooperation, for him, became instead "co-operation" (in the sense of developing individuals following the same trajectory, as a result of interactions with the same shared set of influences).⁵⁹ It now seems possible to suggest, however, that Piaget might have *chosen* to present his work as non-sociological; that this focus may have been part of his early trouble with the Faculty of Sciences, so he minimized it in favor of increasing formalism. (Is it possible that Titchener could have

⁵⁹ What Piaget said in this connection is useful, both for his view of the social and for his view of the influence of a dominant sensibility: "Under the influence of Durkheim, which was after all immense in France, I wasted a fantastical amount of time trying to decide between the social and the individual. That lasted up until the moment I realized that cooperation could be spelled 'co-', 'operation.' When the Durkheimians put the whole burden on society, they commit the incredible mistake of forgetting that, without the nervous system, nothing at all would happen. So, since Durkheim didn't dare to say that the nervous system was a social product, it was necessary to recognize its existence prior to society and social forces. Today, one can no longer imagine the power of the Durkheimians had. Their imperialism was such that my friend, Leon Bopp, jokingly told me, 'You'll see, next they are going to annex minerology!'" (Piaget, 1982b, p. 85).

done similarly, in making his importations of Wundt? [see Green, 2010].)

In addition to the archival documents examined in the microhistories discussed above, and consistent with the observation that Piaget actively engaged the media in constructing his public image, we find the following presentations of self in his autobiographical writings: he dismissed his early concern with the social environment and language as “peripheral” (Piaget, 1952a, p. 246).⁶⁰ He also explicitly denied knowing that he had been nominated to the Chair in Sociology at Geneva that he ultimately accepted (Piaget, 1952a, p. 253). Furthermore, we know from comments included in the preface to *Sociological Studies* that he had resisted publishing a collection in book form of his sociological writings (Piaget, 1943-1963/1965; expanded second edition translated as Piaget, 1977, collecting together works published 1928-1960/1995). And, in an interview conducted at the end of his life, he limited the effect that the very social Baldwin had had on his developing theory to, solely, “the global idea of genesis” (which he said twice [Piaget, 1982b, p. 83]).

Of course, these indications are not conclusive. But even the suggestion that it might be the case—that Piaget may have actively sought to present his work as non-sociological, perhaps in reflection of his perception of the Faculty’s ideal “scientific persona” (cf. Vonèche, 2001; for discussions of method, see also Bordogna, 2005; P. N. Campbell, 1975; Paul, 2014)—requires a very different investigative approach than that

⁶⁰ Piaget clarified at the Macy conference held that year, in response to a question from Margaret Mead: “the environment does not change the order of succession of these operations, but it can accelerate, return, or, even entirely block further development” (in Abramson, 1954, p. 156). In this, he was consistent. As he explained twenty-five years later, in his last-known interview: “I believe that cultural factors advance or delay, but do not modify, structures. I also believe that structures always have a biological point of departure” (in Voyat, Inhelder, & Piaget, 2011, p. 21).

previously pursued.

For the innumerable contemporary scholars seeking to build on a synthesis of Piaget's and Vygotsky's theories, the obvious approach before the contrast afforded by the new biography would have been to set up the two theorists in dialectical opposition: Piagetian theory as *thesis*, and Vygotskian theory as *antithesis*, with the commenting author providing a new synthesis. Now, however, we are drawn instead to look for the ways in which Piaget—a man seeking the approval of the Faculty of Sciences at Geneva—may have purposefully obscured his theory's sociality. Given that he had indicated in his response to Vygotsky that the antithetical criticisms had been addressed, we can also come to no other conclusion than that the sought-after synthesis may already exist in his subsequent writings.

8.2. Neglect, backlash, and suppression. Shifting perspectives now from a focus on the *particular* to a focus on the *discipline* (from *it* to *us*), it is clear that the rise of Vygotsky is interpretable as a response to the popularity of Piaget; that his works may well have been caught in an updraft of the post-Piagetian backlash, which is itself nicely encapsulated by the term “Piaget bashing” (see Seagrim, 1981, p. 422; used more recently by Valsiner, 2012, p. ix). Yet the larger issue raised by these studies is not that of backlash. Instead, it's the reverse that seems to matter more in the grand scheme of things: a stronger neglect, in the form of *suppression*.

The connection between the two concepts is straightforward: were it not for suppression, there could be no backlash. Change would simply happen normally—incrementally—as areas of accidental neglect were discovered and addressed. Yet the

result need not be revolutionary, in the sense described by Kuhn (1962/2012) in *The Structure of Scientific Revolutions*. Indeed, we know that Kuhn's model doesn't apply cleanly to Psychology (see e.g., Greenwood, 1999). Still, there is *something* here that needs to be explained (as I argued previously in Hobbs & Burman, 2009). I would therefore like to reintroduce the notion as one belonging properly to the indigenization-colonization discourse; of the importation and reconstruction of "foreign" influences in new contexts, with specific reference to "local" interests and norms, leading to different views of the same phenomena and followed eventually by the subsequent masking of both the original source and the imported thing's remaking (see e.g., Burman, 2012d).

Why must this occur? Briefly put, it's because psychology—and psychological meta-theory—is manipulable as a result of its fundamental nature as a normative body of knowledge. Despite pretensions to universality, psychology remains a moral science (pun intended). And this brings neglect with it as a natural consequence: certain thoughts are "thinkable," and others are "unthinkable." But these sensibilities then also suppress "foreign" contributions, in every sense of that word. And I suggest that it is this suppression which produces the backlashes that have been observed as "the rise of indigenous psychologies" (see Allwood & Berry, 2006). I also suggest, however, that it is doing more than this.

9. Hurt and remedy

Where there are different norms, there are different possibilities. Thus, what we accept as Psychology shapes how we approach psychological topics. Yet I propose that this acceptance is also a form of colonization: it is a frame of mind, or an implicit stance,

adopted by those who wish to participate. This then *imposes* the norms that Danziger (1990, 1997b) described as having arisen historically in “American” psychology. And it is that which then causes the neglect which leads to problems for “foreigners,” even as professional development is otherwise unaffected.

To wit: if a text is deemed by insiders to be “foreign,” in the sense that it is not consistent with the present set of norms that govern the discipline, then it will be dismissed as “not relevant” or “out of date” (or of “poor style”). It will therefore be excluded and ignored (perhaps as “Sociology”).⁶¹ Yet this then implies also the reverse: that older texts which are found to anticipate a present aesthetic can be celebrated as “ahead of their time” (or “genius”). And this suggests that such dismissals are indeed the flipside of celebration, and thus that they are something about which historians should be wary: they do not index differences in quality (“health”), necessarily, but differences in context (“internal politics”). Indeed, they are “sign-posts” (Ball, 2012, p. 79).

Dismissing “foreign” contributions solely for reasons of incompatible *style* is highly problematic: readers operating from within the “American” sensibility miss important details, the recognition of which would help in the pursuit of other seemingly unrelated interests that are nonetheless valuable. What we have discovered about Piaget makes this clear. Yet there is even, to put it in stronger words, an ongoing *hurt* to be remedied: a consequence of what we might call, for lack of a better term, “scientific chauvinism” or even “epistemological prejudice” (cf. Teo’s [2010b] “epistemological

⁶¹ Piaget (1982b) said something similar of Baldwin’s position in biology as a psychologist: “Do you know that among biologists the fact that Baldwin was a psychologist is completely ignored? It is a point that I had to check out for myself. Biologists do not take psychologists seriously. If they had known that Baldwin was a psychologist, they wouldn’t even have read him!” (p. 84).

violence”).⁶²

The obvious solution is that we ought to encourage psychologists to “think bilingually”—as indeed I did in discussing these problems during my first visit to Geneva (Burman, 2012e)—but the elimination of the language requirement in the “American” psychological curriculum presents an unsurmountable institutional barrier. It also now seems clear that this would have been too narrow a solution. Other obvious routes are to encourage and support multiculturalism (see Piaget, 1966/1976) and interdisciplinarity (Piaget, 1966/1967, 1966/1969, 1970/1973, 1978, 1979). Yet there is no universal institutional mechanism which could provide for this in the education of “American” psychologists, except perhaps the History and Systems course that was—until recently—required explicitly for APA accreditation (see Benjamin, 2001, p. 738).

In fact, it seems to me that the History and Systems course could be the perfect vehicle for teaching this valuable and necessary “cultural sensitivity.” Indeed, the kind of pluralistic thinking that is encouraged when this course is done well—encapsulated in the broader epistemic sensitivity required to engage and understand ideas that are hard to see and understand from the present perspective—ought to generalize easily to other areas of the discipline (see e.g., Ball et al., 2013; Beins, 2011; Yanchar & Slife, 2004). Another benefit is the much-reduced ethical burden in history, since most of the human subjects involved are already in the ground. In short: if we accept Hamlet’s observation that death is the undiscovered country, then what is History if not exactly the guide our students

⁶² To put this in different terms, “foreign” scholars have become the subaltern speakers of professional history of psychology (cf. Spivak, 1988). The remedy thus proposed is therefore necessarily a “political” one (following Spivak, 1998). And while I find the necessity of political action personally distasteful, the cause—asymmetry in treatment and inconsistency in action—is far worse.

need? (cf. R. I. Watson, 1979).

10. Final thoughts

“Modern Psychology” is already being pushed in this direction. Indeed, the best single book-length comparative study by an Historian of Psychology is John Carson’s (2007) examination of the differences between American and French approaches to intelligence-testing (see also Carson, 2014). Yet witness, too, the recent publication of two new volumes intended to aid instructors in helping their students to think internationally: *Internationalizing the Psychology Curriculum in the United States* (see esp. the chapter by Pickren, 2012) and *The Oxford Handbook of the History of Psychology: Global Perspectives* (Baker, 2012). Two new textbooks for the undergraduate course in History and Systems—reflecting these very concerns—are also available (Pickren & Rutherford, 2010; Walsh, Teo, & Baydala, 2014). But there is as yet no similar text that describes international differences in *historical sensibility*; differences in the doing of the history of psychology, or what to do in encountering them.

This is perhaps a reflection of Danziger’s (1990) popularization of David Bakan’s (1967) accusation that psychologists suffer from a kind of method-fetishism, which he had memorably called “methodolatry” (p. 158). Given their power and influence in the History of Psychology, the subsequent neglect of methods-talk among Historians of Psychology is only natural. Indeed, the discussion to this point about the appropriate response to recognizing the Americanization of psychology—among professional historians of psychology—has been primarily about internationalizing historical *content* (see esp. Brock, 2006). But we need also to extend this discussion to *process*.

To be clear: I am not calling for a *celebration* of methods. Rather, my intent is simply to encourage greater reflexivity in considering our practice as historians (cf. Morawski & Bayer, 1995; see also Morawski, 2005). In short: we must situate ourselves, as well as our subjects. This is necessary because our practice—the process of *doing* History—is implicitly “American,” even in considering international subjects. And, as a result, we neglect valuable “foreign” contributions.

Where, then, are the lines to be drawn between social norms, individual aesthetics, and *the right way to do things*? Surely there must be criteria according to which scholarly quality can be judged without regard for the way in which its *product* has been *packaged*. We need only articulate them more clearly.

The present intuition in “American” History of Psychology seems to be that standards are set during one’s education as a result of reading “good” examples of “good” history (see e.g., Vaughn-Blount, Rutherford, Baker, & Johnson, 2009, pp. 123-125). But that normative approach then leads to the abstraction of heuristics—such as the dismissal and rejection of *apparently* celebratory works—which can clearly result in poor historiographical decision-making when considering “foreign” contributions. In short, we are left with a problem: identifying high-quality scholarly material cannot be merely a matter of taste, as if considering a fine wine for either the cellar or the bin (cf. Shapin, 2012a, 2012b). And the need will only become more urgent: the History of Psychology is growing rapidly outside of the United States (Pickren, 2009a). If we don’t adapt, it is conceivable that we could someday be left behind (even excluded from our own discipline).

So, then: should the editors of “American” journals reject potentially-valuable “foreign” contributions simply because they *feel* un-American upon first reading? Surely not, for reasons that are obvious to “foreign” authors. As Ratcliff put it—in English—in our discussion of his new biography of Piaget:

In French *Le "jeune conquérant neuchâtelois"* [the young conqueror from Neuchâtel], as according to the original abstract was in tune both with language style and conceptual needs. of course it is an issue of connotation, which a French native speaker is at ease with. And the connotation of this conqueror is simple: not at all celebrative as you rightly saw, but, how would you call someone who was able to "enroll", as sociologists of science call it, more the 350 people to collaborate closely with him ? Are we just authorised to use the politically correct word "enroll" because it reflects the connotation of our time?; this is just a paved way for *censorship*. (Ratcliff, personal communication, 22 November 2013; my emphasis, with all other typography as in the original; including implicit references to Callon, 1986; and Ratcliff, 2010)

While the purpose of disciplinary norms may indeed be to censor “unthinkable” contributions, that—in this case—should not be the intent. The intent should rather be to identify the best studies that contribute to knowledge in the best way.

High-quality material is clearly being neglected accidentally (or rather, as I have heard my students say, it is being filtered out “accidentally-on-purpose”). Thus, my suggestion is simply this: as we reexamine our sensibilities regarding “foreign” content, and refocus away from WEIRD participants, so too must we confront our biases about

“foreign” process and presentation. I will now provide a method by which this might be achieved in specific cases, although its goal is not to assess quality; rather, its purpose is to identify differences.

CHAPTER IV

Quantifying neglect, formalizing the argument

QUESTIONS REGARDING THE STANDARDS of contemporary History, as it is done today by contemporary Historians of Psychology, are—from the present historiographical perspective—*empirical* questions. This is because contemporary Historians deal in evidence; usually papers, like letter correspondence and unpublished manuscripts, which are typically held in specialist archives. The material traces left by the actions of past Historians can also thus be examined. And this is how History itself changes: such examinations can inform present-day decisions about how best to present new historical arguments.

In a strict sense, however, questions of norms are also beyond the scope of this dissertation. This is not a history of the History of Psychology. Rather, my intent here is to use a small case in order to speak about a larger issue. I have been following the norm of using evidence to pursue that goal. But this chapter takes a different approach: it uses evidence, but not in the form of the earlier comparative “microhistory.” (Small stories about a big issue.) Instead, I present a new method that enables a generalization of the Wundt Argument for application to Piaget. The result is a quantitative examination that will be acceptable to Psychologists, while at the same time replicating the form of an

argument that has been very important to Historians of Psychology.

To put it plainly, “Wundt” was reconstructed in a new context: his foreign-feeling interests were neglected, and suppressed. The resulting reconceptualization then stood-in for the original source: Wundt was actively *Americanized* by his American followers (Rieber, 1980b). *Their* critics then dismissed *him* as well; his followers were fruits of a poisonous tree, and so critics burned the whole orchard down. It’s then this, as I mentioned in Chapter III, that gave us Behaviorism (O’Donnell, 1985).

Of course, though, this is not about the *object* “Wundt.” The man himself didn’t actually move. It is about *meaning*, and thus also about *implications*: for an Americanized Wundt to be “the founder” of American Psychology constrained what the founded discipline could be defined to be. Subsequent action was then directed down one path, rather than another: toward “experimental” psychology, rather than a more “applied” approach (O’Donnell, 1979).

The book that summarized and presented all of this in a single narrative—*Constructing the Subject: Historical Origins of Psychological Research* (Danziger, 1990)—has become, along with its author, one of the central influences of contemporary History of Psychology (Teo, 2013b, p. 4n7; see also Brock, Louw, & van Hoorn, 2004). So my proposal follows simply and incrementally from there: something similar happened with Piaget, and the results pushed Developmental Psychology in a direction that was more consistent with the American audience’s interests than with the original source’s intent.

Briefly put: Piaget’s texts were imported into a post-Titchenerian Behaviorist

context, during the “rediscovery” period of the 1950s and 1960s, and their meanings were remade in the image of the Cognitive Revolution. (This, in Geneva, is not a controversial suggestion [see e.g., Voyat, 1977].) That then explains the lop-sidedness of Piaget’s legacy (Bond & Tryphon, 2007). And it also, in turn, provides a plausible cause for subsequent concerns regarding the existence of a “new” theory: insiders became aware that the contemporary view of Piaget’s works was skewed, and went looking for what had been missed—in the same way as Wundt historians did (see esp. Blumenthal, 1975; 1977; Danziger, 1979, 1980; Leahey, 1981; also Araujo, 2016; Klautke, 2013).

Thus, my goal in this chapter is to provide a general version of this Wundt Argument that will be acceptable to both Historians and Psychologists. To do this, I have replicated Brožek’s (1980) citation analysis of Wundt’s impact using books by Piaget (cf. Bond & Tryphon, 2007). But because I also want to use this to make an argument about *form*, rather than *content*, I have also gone a step further: my purpose here is not simply to report facts, but to use them as evidence. Thus, I aim to present Piaget as a second instance of a general “Indigenization Argument,” and to provide the means to replicate the discovery with other authors in other contexts.

1. Generalizing from Wundt to Piaget

Brožek (1980) situated his study as part of the Wundt Centennial, but explicitly distanced himself from its celebrations: “[his] aim was (and is) to contribute new factual information, not to join the chorus of Wundt’s admirers or critics” (p. 103). To gather his data, he then examined the first 90 volumes of the *American Journal of Psychology* for citations. But he did so by hand: none of the automated tools that now exist could be

appealed-to then (cf. Green & Feinerer, 2015).

Brožek's (1980) intent was to determine the impact of Wundt's individual book-length contributions, as well as a "rate of decay" (p. 104). The result was that he demonstrated a "decline of Wundt's glory" (p. 104). More interesting for our purposes, however, is the ranking he produced of Wundt's individual books.

This, for Brožek, was a way to confirm that the well-known *Grundzüge der physiologischen Psychologie* was indeed Wundt's "bestseller" (p. 105). And Brožek equates its popularity—with 61% of all citations in his sample—with its "relative importance" (p. 106). In retrospect, however, contemporary Historians would likely provide a different interpretation.

We know from subsequent work that Brožek's aside about the *Völkerpsychologie* having "fared poorly" (p. 106) would soon be taken very seriously. (Its neglect is the basis of the Wundt Argument.) Seen from that perspective, his quantitative data can then be understood as providing evidence not of each book's "importance," but of their "reception." This then enables his study to be interpreted through the lens of indigenization that characterizes the Wundt Argument: evidence of what, in Wundt's writings, the audience of American importers found most interesting or useful. That is what I have done below for Piaget. Further analyses then highlight differences in the reception of those same texts by different groups, differentiating between *content* (message) and *form* (massage). (Or, to put it in the terms of Chapter III: *health* and *internal politics*, although I do not use those terms here.)

To enable such a comparison, I have collected the citation counts provided by

Google Scholar for all of Piaget’s books in French, as well as for their English and German translations.⁶³ This is a bit skewed toward the present view, of course, because Google has access only to those materials that have been digitized. (And also because the disciplinary “style” for citations has changed over the period of study [see Sigal & Pettit, 2012].) But this at least should be skewed consistently: Google has no particular interest in one aspect of Piaget’s output over another, so its data should be more than adequate for the purposes of my demonstration. (I do not duplicate his calculation of a “rate of decay,” however, because Google Scholar does not provide time-based citation data; interested readers are directed instead toward Google Books’ NGram Viewer, although this has its own problems [see Pettit, 2016].)

2. Toward a more formal comparative history

The great benefit of this approach is the very large ‘n,’ which ought to appeal to Psychologists. Indeed, I found 104,814 citations distributed across the three languages. (See **Figure 1**.) And this then affords a kind of null historical hypothesis: if Piaget’s texts were not indigenized in the same way that Wundt’s have been shown to be, then their impact—as assessed by citation counts—should be the same in French, English, and German. (Or rather, the impact would be the same relative to the total number of citations made in that language.) Needless to say, however, that is not the case.

⁶³ Books and pamphlets are included according to the categorization produced by the Fondation Jean Piaget in Switzerland: <http://www.fondationjeanpiaget.ch/fjp/site/bibliographie/index.php>. The best source for titles in all three languages is Richard Kohler’s online bibliography: <http://www.richardkohler.ch/piagetbiblio.htm>

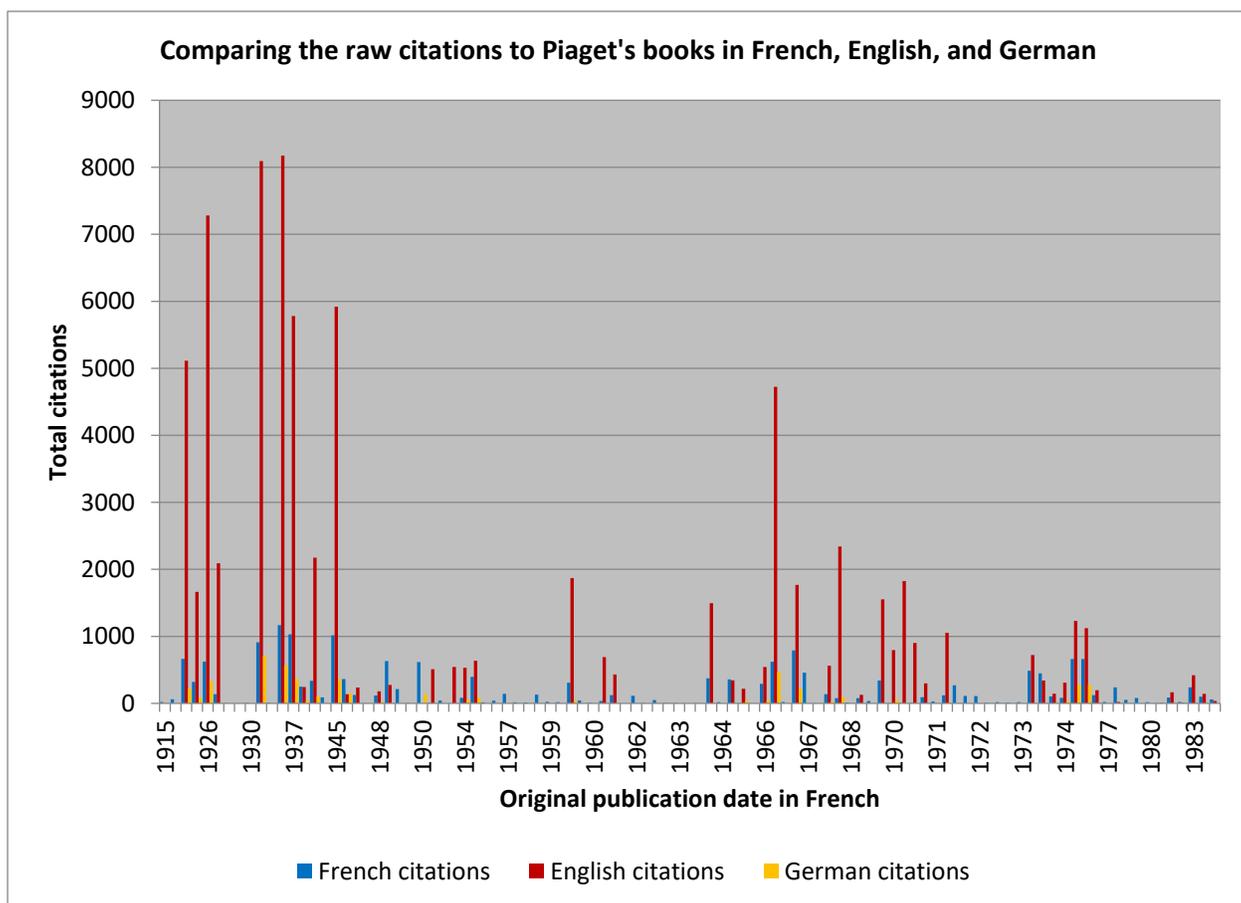


Figure 1. Piaget's raw citation data, with no manipulation. Dates are according to original source publication. There is one exclusion: *The Psychology of Intelligence* (Piaget, 1947/1950), for reasons explained below.

2.1. The raw data, and the cooked. To replicate Brožek’s (1980) study as closely as possible, the first step is to present a simple ranking using the raw data from Google. Yet this already highlights a problem: at the time these data were first collected, in early-2013, there were thousands of citations reported by Google for the English edition of *The Psychology of Intelligence* and very few for the French original (Piaget, 1947/1950). And yet when a double-check was performed in early-2015, and continuing to this day, the situation was reversed: thousands of citations for the French, and very few for the English.

This seemed fatal for the approach, at the time of discovery, but problems like this are actually fairly common in the digital humanities. Corrections to “raw” data are almost always required. And this need is so pervasive that one commentator recently pointed out that “raw data is an oxymoron” (Gitelman, 2013). All such data, in other words, is at least lightly “cooked.” (Indeed, Brožek’s [1980] study is full of selections and exclusions.) Still, it’s worth noting that no other book seems similarly affected. We will therefore simply recognize Piaget’s *The Psychology of Intelligence* as being akin to Wundt’s *Grundzüge* in its impact—#1 in French and German, and #3 in English—and move on to a deeper analysis using the data we do have. A brief word, though, about what it is that we are excluding.

As Piaget’s books go, *The Psychology of Intelligence* is relatively straightforward. It was developed first as lectures for a course that he ran at the College de France in 1942 as part of his resistance to WWII: “at an hour when university men felt the need to show their solidarity in the face of violence and their fidelity to permanent values” (Piaget,

1947/1950, p. vi). Publication and translation were then delayed until after the war was over.

This was the first of Piaget's books to appear in English since the translation of *Moral Judgment* almost twenty years before (Piaget, 1932/1932). Given this, it's then no wonder that the book was so influential: it marked Piaget's return to English psychology. In German, however, the situation was somewhat different.

Translated immediately after the war, and published in 1948, *Psychologie der Intelligenz* was the first-ever German-language edition of one of Piaget's books. Indeed, it was the first of nearly three dozen translations. It can therefore be understood to occupy the somewhat special position of having provided the first impression of Piaget for German audiences. Simply put: every book published afterward would have been read through its influence.

Comparing the reception of the three editions, the relatively lower status of the book's English translation can perhaps be attributed to the popularity of John Flavell's (1963) introductory textbook: *The Developmental Psychology of Jean Piaget*. This is widely recognized as having been the first major work to make Piaget accessible to English-speaking audiences. The two books also cover very similar territory. Commentators on the draft of 1957 even dismissed it as having been skewed in its perspective by that book's recent translation (Müller et al., 2013, p. 52). And this in turn provides a possible explanation for Piaget's book's slightly lower ranking on our Brožekian bestseller list: the two books competed for citations in English, but—because no translation of Flavell was made—they did not in French or German. (With Flavell's

book's citations added to Piaget's, the combination then ranks #1 on the English list.)

From this perspective, what "Piaget" means for the contemporary audiences is *old*: his ideas are WWII-era. And everyone knows, of course, that huge amounts of work have been done since then. So why would a contemporary Psychologist bother with such an archaic text?

At the same time, Piaget's view is also clearly *foreign*. Rather than treating intelligence in psychometric terms, he considers it to be the consequence of a developing sequence of "operational" structures and groupings. (More on this in Chapter V.) That's not archaic; it's weird.

For an Historian, though, this finding is ideal: as an introduction to the ideas that informed the "standard theory," one could do much worse than *The Psychology of Intelligence*. Now, though, we want to see beyond its influence. Excluding it from our analyses is therefore not only necessary for practical reasons, but it's also very useful: we won't be blinded by it, and our illustrations of the quantitative data won't be compressed by its much-greater impact.

2.2. Basic figures. To derive the remainder of a bestseller list, we can report some basic figures. Thus, for example: the means and standard deviations for citations in French, English, and German were 186.2 (261.1), 1461.4 (2114.5), and 172.6 (182) respectively.

Immediately, this suggests a powerful reason for why European funding agencies are so anxious to have their grantees publish in English-language journals: the impact in English is eight times greater than in French or German, for the same input. But the

differences also provide us with a simple way of assessing the impact of books across different languages: it's clear that each language group needs to be treated separately, before they can be compared. Thus, those books that received more citations than two standard deviations above the mean in each group are listed in **Table 2**, along with their translated titles.

The consistency is remarkable: *The Origins of Intelligence in Children* (Piaget, 1936/1952) and *The Moral Judgment of the Child* (Piaget, 1932/1932) appear in all three lists, and—if one is interested only in reading the Great Books by Psychology's Great Men—they are clearly key companion texts to read alongside *The Psychology of Intelligence*. But they are also very early texts. In fact, with only one exception related to the French reception of *Biology and Knowledge* (Piaget, 1967/1971), all of the “bestsellers” were published prior to the end of WWII. The exception need not be discussed here, however; it will appear several times later, and its importance will be discussed in detail (especially in Chapter VI).

Table 2. Piaget’s “bestsellers” by citation count (total citations in that language)

French	English	German
<i>La naissance de l'intelligence chez l'enfant</i> , 1936 (1167)**	<i>The origins of intelligence in children</i> , 1936/1952 (8174)**	<i>Das moralische Urteil beim Kinde</i> , trans 1932/1954 (711)**
The origins of intelligence in children, trans 1952		<i>The moral judgment of the child</i>
<i>La construction du réel chez l'enfant</i> , 1937 (1029)**	<i>The moral judgment of the child</i> , 1932/1932 (8093)**	<i>Das Erwachen der Intelligenz beim Kinde</i> , 1936/1969 (567)*
The construction of reality in the child, trans 1954		<i>The origins of intelligence in children</i>
<i>La formation du symbole chez l'enfant</i> , 1945 (1016)*	<i>The child's conception of the world</i> , 1926/1929 (7280)*	
Play, dreams and imitation in childhood, trans 1962		
<i>Le jugement moral chez l'enfant</i> , 1932 (911)*	<i>Play, dreams and imitation in childhood</i> , 1945/1962 (5921)*	
The moral judgment of the child, trans 1932		
<i>Biologie et connaissance</i> , 1967 (791)*	<i>The construction of reality in the child</i> , 1937/1954 (5781)*	
Biology and knowledge, trans 1971		

2.3. Citation density. A simple next step is to calculate a slightly different kind of bestseller list by accounting for the number of years since publication. This then allows us to get to a more temporally-sensitive version of what Brožek (1980) referred-to as “the relative ‘weight’ of... individual publications” (p. 103).

Citation density indexes impact per year in which it was possible to cite a text. This can be calculated easily by dividing the total citation count by the number of years since publication. That then provides a time-weighted “citation density” metric that enables the comparison of works produced across a long lifespan, although it will be especially useful when we compare translations made of the same text that were

themselves published at different times. It also provides a better assessment of “perceived importance” than does the raw citation count: a more important text will have a greater number of citations per year, even though a more recent text may have fewer total citations.

We can again use means and standard deviations to narrow in on the most significant books in the group. Thus, to summarize: the original French works were cited an average of 3.5 times per year since publication (with a standard deviation of 4.6 citations per year), the English translations 28 times per year (33.2), and German 4 times per year (4.1). Focusing on the works that received citations at rates greater than two standard deviations above the mean then gives the results in **Table 3**.

Table 3. Piaget's "bestsellers" by citation density (cites per year since publication)

French	English	German
<i>L'équilibration des structures cognitives</i> , 1975 (17.9)**	<i>The origins of intelligence in children</i> , 1936/1952 (136.2)**	<i>Die Psychologie des Kindes</i> , 1966/1977 (13.4)*
<i>Equilibration of cognitive structures</i> , trans 1978 and 1985		<i>The psychology of the child</i>
<i>Biologie et connaissance</i> , 1967 (17.6)**	<i>The psychology of the child</i> , 1966/1969 (109.8)*	<i>Das Erwachen der Intelligenz beim Kinde</i> , 1936/1969 (13.2)*
<i>Biology and knowledge</i> , trans 1971		<i>The origins of intelligence in children</i>
<i>La naissance de l'intelligence chez l'enfant</i> , 1936 (15.4)**	<i>The construction of reality in the child</i> , 1937/1954 (99.7)*	<i>Das moralische Urteil beim Kinde</i> , 1932/1954 (12.3)*
<i>The origins of intelligence in children</i> , trans 1952		<i>The moral judgment of the child</i>
<i>La psychologie de l'enfant</i> , 1966 (13.5)**		
<i>The psychology of the child</i> , trans 1969		
<i>La prise de conscience</i> , 1974 (12.8)**		
<i>The grasp of consciousness</i> , trans 1976		
<i>Réussir et comprendre</i> , 1974 (11.8)*		
<i>Success and understanding</i> , trans 1978		
<i>Logique et connaissance scientifique</i> , 1967 (10.2)*		
[untranslated]		
<i>Introduction à l'épistémologie génétique</i> , 1950 (10.0)*		
[untranslated]		
<i>Psychogenèse et histoire des sciences</i> , 1983 (8.3)*		
<i>Psychogenesis and the history of science</i> , trans 1989		

The results are interesting: the French originals are both more numerous and more diverse than the English or German translations. And although the highest “density” books from the English and German lists are contained within the French list, the French list also includes many more recent books.

Two of the identified French texts haven’t been translated. Of course, this isn’t especially meaningful in terms of differentiating impact across groups (because interested foreign scholars would have had to cite the originals). But there are some things here that we ought to mention. Actually, the French #1, *L’équilibration des structures cognitives* (better known as *The equilibration of cognitive structures* [Piaget, 1975/1985]), requires some explicit discussion.

This book is the first theoretical summary of the New Theory period: it reviews and integrates the first three volumes of experiments published immediately after the changes in Piaget’s logic and biology that are discussed at length in Chapters V & VI (Piaget, 1974/1976, 1974/1978, 1974/1980b). It also represents Piaget’s return to theorizing the primary constructive mechanism required of his system: “*équilibration majorante*,” which—lacking explicit access to the updated logical framework discussed in Chapter V—the English translators rendered unhelpfully as “optimizing equilibration.”

As has often been noted, this translation is insufficient to convey the intended meaning. Among other things, it misses the transcending step implied by the level-change that was made more explicit in Piaget’s later untranslated discussion of dialectics (Piaget, 1980b). Yet even with that reference in-hand, this book would have been very difficult to render in another language: as a summary of new ideas, it assumes a huge amount of

material that is not discussed explicitly (much of which had not yet been published). This then had real consequences in moving its meaning across contexts.

In English, this book has the dubious distinction of having received two translations. To put it plainly: the first attempt was “seriously defective” (Smith, 2009a, p. 28). What’s worse, though, is that the earlier translation has actually received more citations than the higher-quality replacement. The result is that the bulk of readers in English have actually not read even an approximation of the text read by French audiences.

Given the book’s position on our second bestseller list, this is itself quite significant. But it’s not the only book with problems. Indeed, one of Piaget’s better-known translators, Eleanor Duckworth, told me in 2010 that *Origins of Intelligence*—which sits at #1 on both of our English bestseller lists—also needs to be replaced (noted in my translation of Ducret & Schachner, 2011, pp. 1n-2n). Doubtless there are many others too: the original translators of *Equilibration* and *Origins* were responsible for several volumes, including the bestselling *The construction of reality in the child* (Piaget, 1937/1954). It therefore seems likely that many of Piaget’s most important books contain substantial divergences from the originals.

Recognizing this, it’s obviously possible that “indigenization” isn’t the only intervening effect at work here. As with Wundt, there’s some incompetence too. Unfortunately, however, that is a very difficult thing to quantify. For this reason, I will concentrate solely in this chapter on what can be shown with the evidence that can be mustered. I will then use the following chapters to follow the example of contemporary

Historians and delve more deeply into what cannot be shown quantitatively.

2.4. Comparing across language groups. Listing top-ranked publications is easy so long as one has access to the data. But we want to compare all of Piaget's books, in multiple languages, and in a systematic and direct way. As a result, we must now begin to move beyond what Brožek (1980) attempted, although—since the technology is quite different today—we do so simply by taking advantage of the available computational tools.

In addition: the recognition that all such data are “cooked,” to one extent or another, enables us to play with the numbers in ways that are more revealing than would be the raw citations that most such studies focus on. Yet the resulting manipulations are mathematical, rather than statistical. And they are as simple as can be managed: although more complex manipulations might reveal more, their rhetorical power would decline in proportion to their complexity. The resulting approach may therefore seem a bit foreign to Psychologists, but it should be close enough for comfort. It is also consistent with work presently being done by Psychologists and Historians working in the digital humanities (see e.g., Burman, Green, et al., 2015).

The first manipulation is a kind of scaling. This is necessary if we are to compare across language groups, but it requires only that the citation count for each book be rescaled relative to the most highly-cited book in that group. This then allows for the relative impacts in each language to be compared without concern for the external causes of differences in citation patterns (e.g., the number of people working in each group, citation styles, etc.). We can then consider internal differences, and use them to identify

new questions worth asking.

This first manipulation is easily done in an Excel spreadsheet using the LARGE function: with book title, publication date, and raw citation count in the first three columns, a new column is calculated by dividing the relevant cell in each row (the individual book's citation count) by the largest number from the "raw" column (the book with the highest citation count in that language). Once defined, the formula can then be populated across the rows. Of course, Excel will automatically increment the cell-references during this process. And although this is convenient for updating the reference to the "raw" column, it introduces an error inside the function. As a result, the function needs to be corrected for all rows. With this done, the corrected column can then be copied and pasted into another column (or to another sheet) for application with another language group.

Presenting these results graphically is relatively simple. The tallest bar of each color represents the highest-impact text in that language group. But the results are powerful: they afford the first qualitative test of our null hypothesis. Simply put: had the impacts been the same in each language, then the bars in **Figure 2** would be flat three-across for each book in all three languages. That is clearly not the case.

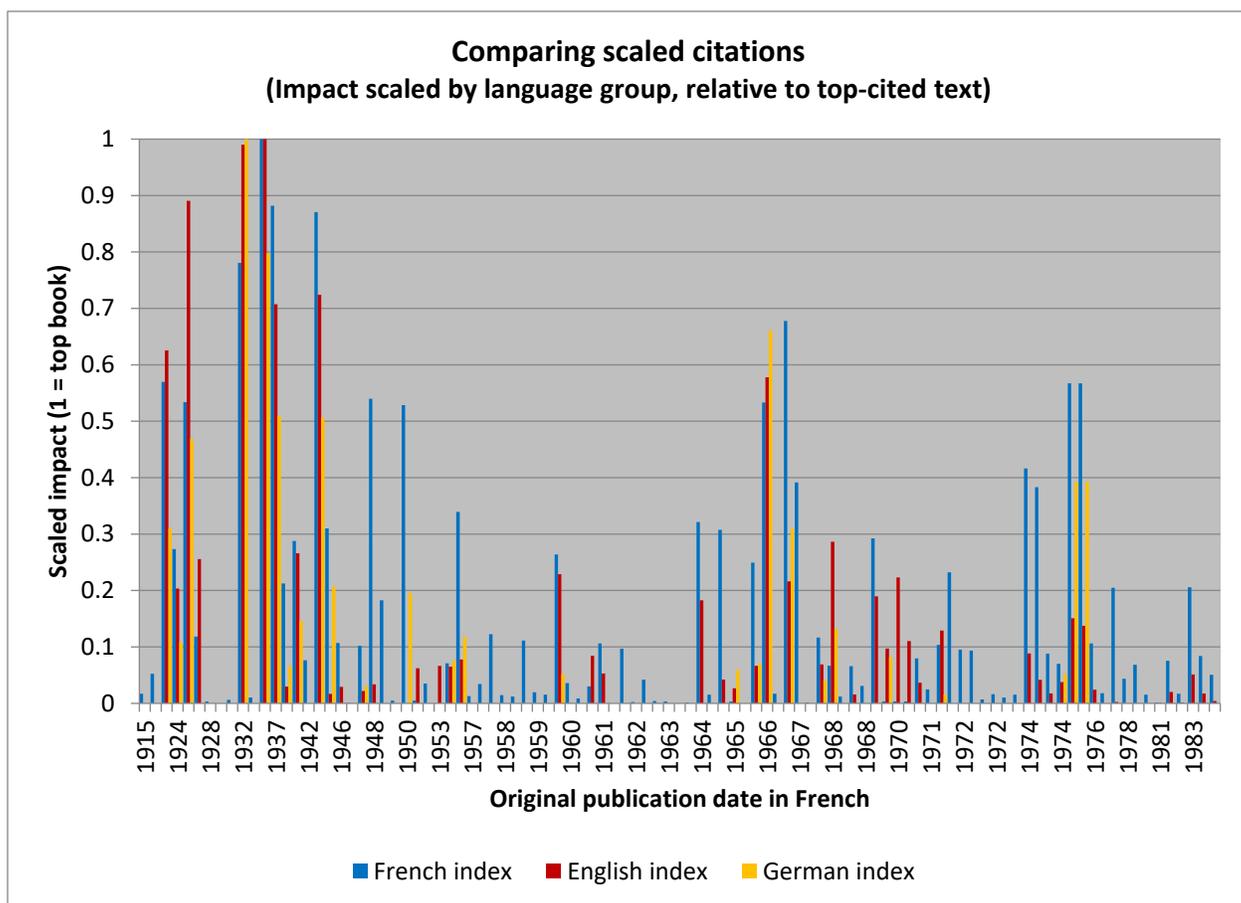


Figure 2.

To make the point conclusively, one might be tempted to add error bars and asterisks showing significant differences (to represent the degree of certainty one has that Google's sample is representative of the population of published citations). But these graphic are already overly complex. And it's not necessary to focus on close calls or near-misses: this is an investigative tool, not a replacement for History. An example will suffice to make the value of such an approach clear.

Looking to the first experimental books of the New Theory period—*The Grasp of Consciousness* (Piaget, 1974/1976) and *Success and Understanding* (Piaget,

1974/1978)—we see that they are widely cited in French, and yet hardly known at all in English. But because these data have been scaled relative to impact in each language, this difference should not be much smaller. If texts were received according to their innate importance, rather than according to the meaning that they have in a particular scientific context, then the scaled impact would be very similar. This would also apply to all books, rather than just the few from early in Piaget’s career.

This needs a word more: only the early books were received at similar levels in all three languages. The blue bars illustrating the impact that these texts had in French are much taller later in his career, especially in the 1970s: the period of the New Theory. That is suspicious, but nothing more. We must dig deeper.

In addition to rescaling and comparing the raw citation data, we can also rescale and compare the calculated citation densities. (**Figure 3.**) The effect is then to normalize the impact relative to publication date in each language. We are then able to see the earlier bestseller list in a new light: individual bestsellers seem often to be bracketed on either side by other high-performing texts, as if certain topics or times became momentarily fashionable.

Something to note quickly: because there were two versions published in English of *Equilibration of Cognitive Structures*, it appears twice in the graph. This is the double-bar located at 1975. Even despite this potential confusion, however, it’s clear that the French texts are much more highly represented more recently. Indeed, with only a few exceptions, the most densely-cited English and German texts are still mostly older: pre-WWII. To say more, though, we will need to continue to dig.

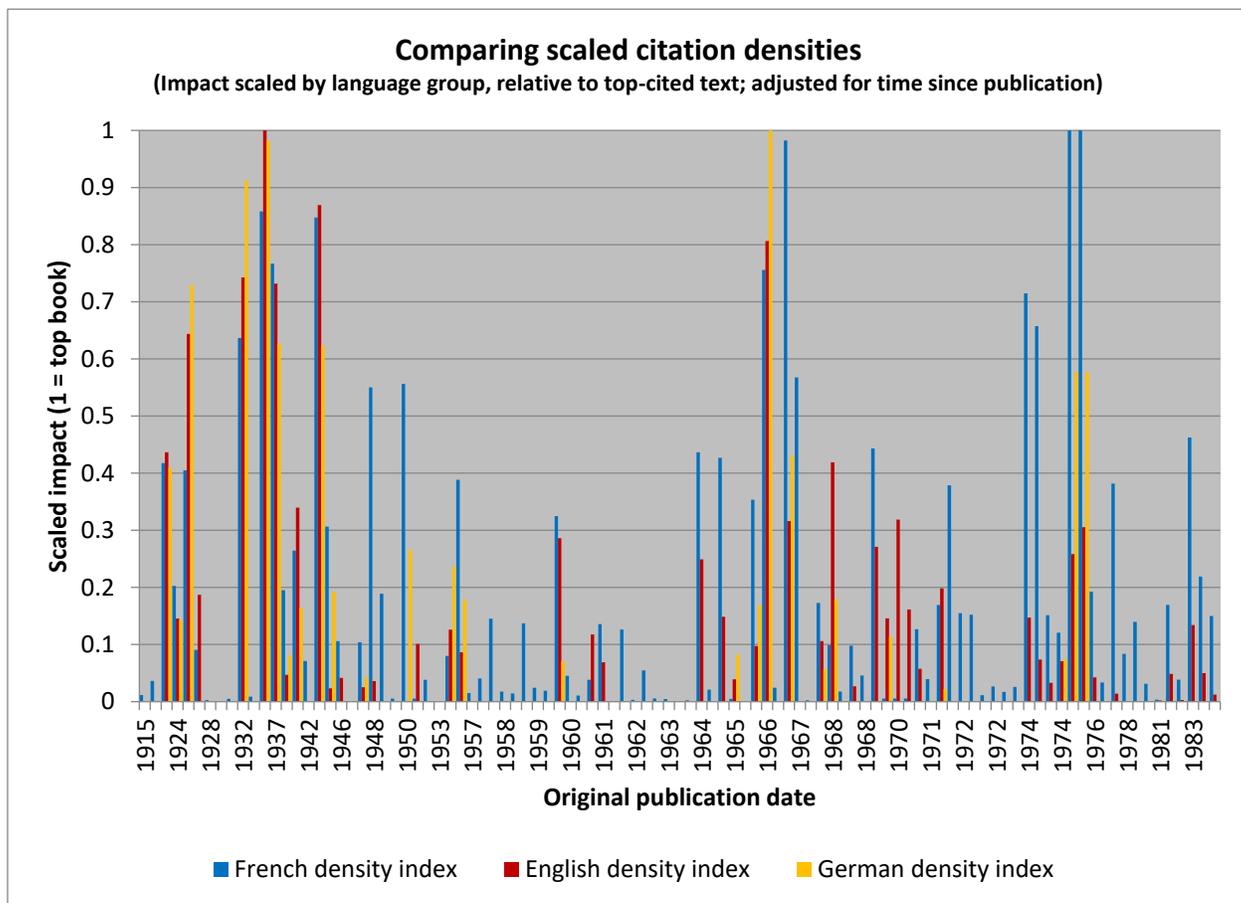


Figure 3.

2.5. Controlling translation impact with source impact. To assess

“indigenization” is to make a comparison between source and target. For Wundt, this was done historically: the early New Historians realized that the understanding of the target group (American psychologists) had gaps relative to the source (German psychological texts). Brožek (1980) then showed this both quantitatively and qualitatively by making lists. My visualizations in replicating his work with Piaget have been more complex. But

the resulting comparisons have so far been very similar. We can go much further.

To build my argument, I have added a new layer of complexity at each step. Until this point, however, they have each been simple. Now, though, we must do something relatively complex. Briefly put: making a quantitative comparison requires controlling for the impact of the source texts in the source language while at the same time presenting the impact of the target translation in the target language.

This can't be done using the raw citations. Instead, we need to use the scaled language-controlled data. In what follows, we are therefore leveraging everything done to this point.

To construct the illustration in **Figure 4**, I first removed from consideration those works for which there is no translation in the target language. Then I constructed a new formula, which required several columns. I subtracted the scaled language-controlled source (French) impact from the scaled language-controlled target impact (separately in English and German), and then—to preserve the scaling—I used the ABS function and the LARGE function to divide by the highest-density text in the target language. This enabled the division to use the absolute value of the product of the subtraction, which then ensured that the results remained within the expected range for a scalar comparison (+/- 1).

The resulting illustration is a kind of scaled “over-under.” Where values are negative, sources are cited more highly than translations. That the overall trend is more negative than positive, for both target languages, then suggests that—although individual texts may have been cited at higher rates—the relative impact of Piaget’s writings were in

the aggregate reduced in translation. This is then consistent with our intuition in considering **Table 3**. In short: the impact of the scaling is higher on the low side (-1 vs -0.75).

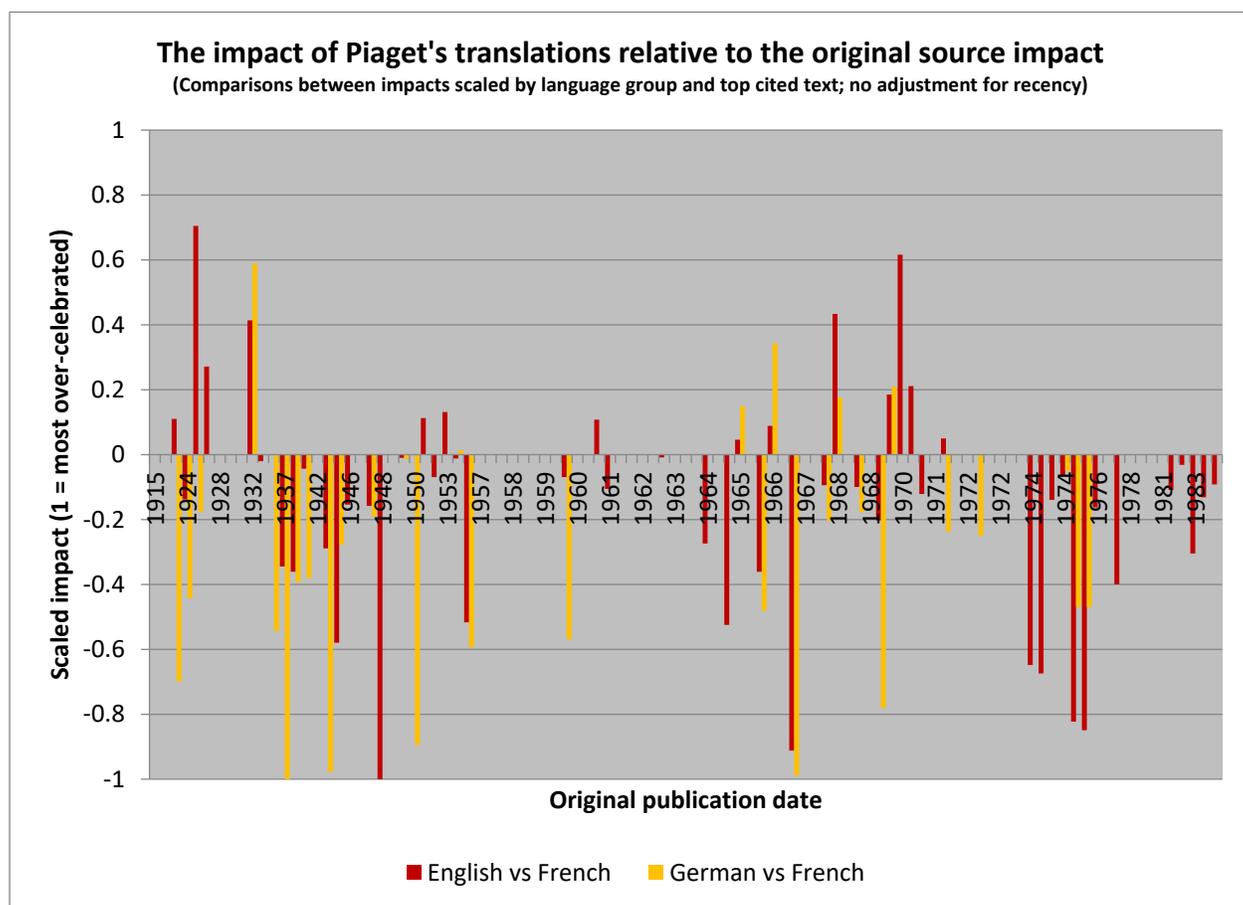


Figure 4.

These results also provide the means to construct two new lists, which will be useful in simplifying the illustrations. Yet these lists are quite different from the earlier ones. Rather than identifying “bestsellers,” as Brožek (1980) did, we can instead identify those books which have been “celebrated” in translation, and those which have been

“neglected.” By this I mean of course that their reception in translation is at odds with their reception in the original French. Following contemporary Historians’ discomfort with “celebratory” narratives, however, I will instead refer to these texts “over-rated” and “under-rated” (relative to the impact of the source text in the original language).

Inclusion on the “over-rated” list suggests that rather too much fuss has been made of these books in translation, relative to their original impact (see **Table 4**). And with the exception of *Genetic epistemology*—which is a very short collection of four lectures intended for an American audience, and hence quite citable by English-speakers (Piaget, 1970/1971)—both texts are known to us from earlier analyses: both *The child’s conception of the world* (Piaget, 1926/1929) and *Das moralische Urteil beim Kinde* (known in English as *Moral Judgment* [Piaget, 1932/1932]) are on our first list of bestsellers (**Table 2**), and the latter is on our second list too (**Table 3**).

Table 4. Piaget’s most “over-rated” texts (by raw indigenization score)

English vs. French

The child’s conception of the world, 1926/1929
(0.70)*

Genetic epistemology, 1970/1971 (0.62)*

German vs. French

Das moralische Urteil beim Kinde, 1932/1954
(0.59)*

The moral judgment of the child

By contrast, inclusion on the “under-rated” list suggests that rather less has been made in translation of those texts than perhaps was warranted given the way in which they were received in French (**Table 5**). Surprisingly, no German texts were identified by this process. But the later work from the second French bestseller list is included on the English side of this new list (**Table 3**). So are the two translations of *Equilibration of*

cognitive structures. (Note, though, that this effect goes away when citations for both versions are added together; the entry in the table is struck-out.) And one book is entirely new to us.

Table 5. Piaget’s most highly “under-rated” texts (by raw indigenization score)

English vs. French

The child’s conception of space (-1)*

Biology and knowledge (-0.91)*

~~The equilibration of cognitive structures (-0.85)*~~

German vs. French

[none reach the level of 2SD]

This new book, *The child’s conception of space* (Piaget & Inhelder, 1948/1956), was discussed briefly in Chapter II as part of the discussion situating Piaget’s genetic epistemology as a programmatic experimentally-derived response to Kant. For this to have been received so underwhelmingly by English-speaking audiences then reinforces Piaget’s impression, in commenting on Flavell’s (1963) introductory textbook, that the view presented of his work in English was more psychological than epistemological—while his own perspective was the reverse (see Piaget, 1963, pp. viii-ix).

From a psychological perspective, this book on space presented *yet more results* from the Genevan factory: replications of things already known or suspected from earlier works, albeit often using interesting new methods to demonstrate them. From an epistemological perspective, however, the book was quite significant: it connected Piaget’s psychological research with problems in the history of science, and delivered on the promissory note included in his inaugural speech as the new Chair in Psychology at Neuchâtel (Piaget, 1925). It also did so experimentally, rather than through theoretical argumentation.

In French, the book is included on the Top 10 list. (It also only just narrowly missed the cut-off for inclusion in **Table 2** as a bestseller.) In English, however, it ranks in the middle of the second tier. It is, in other words, perceived as having been of middling importance.

2.6. Controlling for language while using citation density. Similar results can be derived from these data after taking citation density into account. (**Figure 5**.) Yet, curiously, no book is “over-rated” from this perspective (**Table 6**). It seems that taking historicity into account eliminates this category from both the English and the German texts. From there, delving deeper would require stepping out of the internal dataset to enquire as to why certain texts were omitted from translation. This is important, but outside the scope of this chapter. (However, I will discuss untranslated works in what follows.)

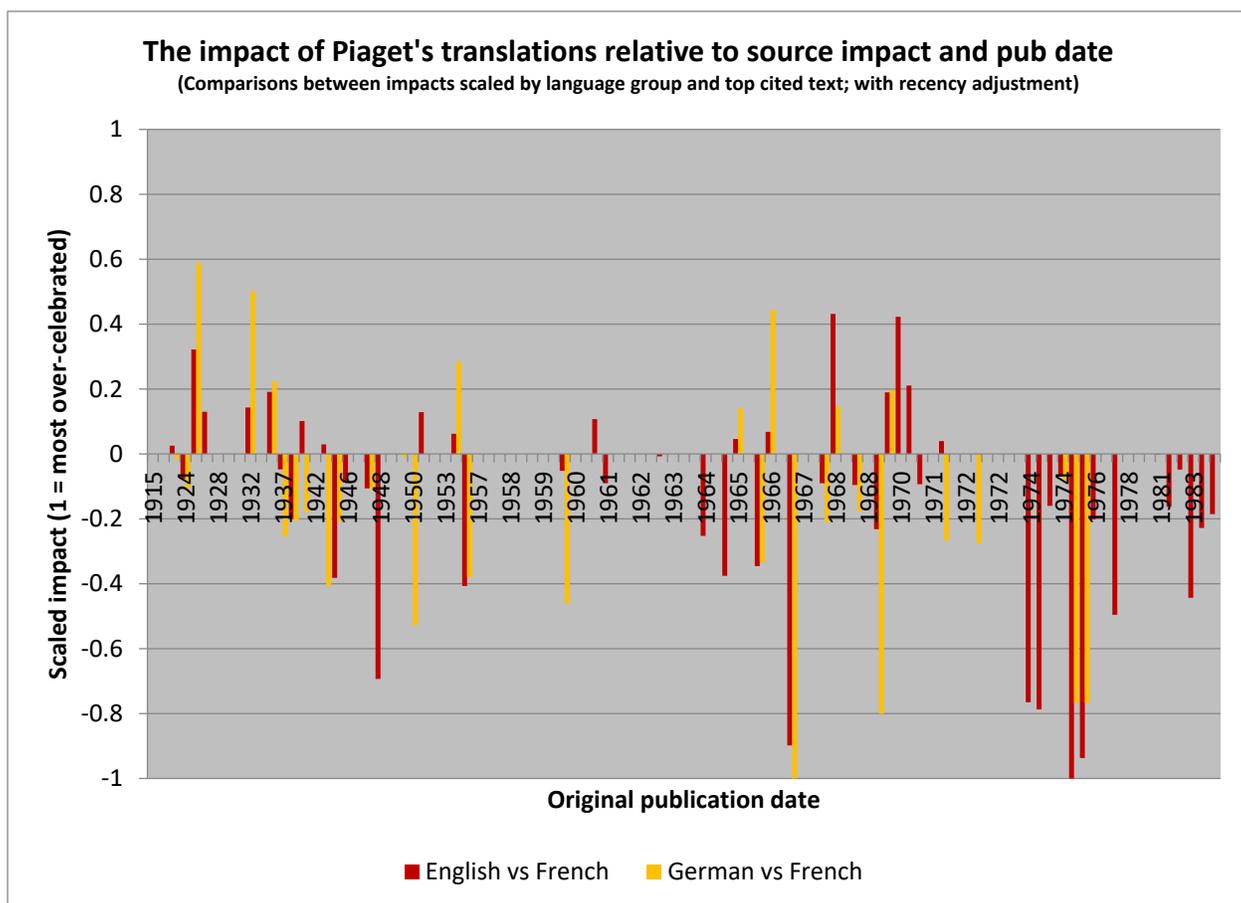


Figure 5.

Table 6. Piaget's most "over-rated" texts (by density-weighted indigenization score)**English vs. French**

[none reach the level of 2SD]

German vs. French

[none reach the level of 2SD]

The main difference in using citation density, as compared to raw counts, is that more recent books are highlighted (**Table 7**). Thus, for example, *Biology and Knowledge* (Piaget, 1967/1971) is identified as a highly "under-rated" text in both English and German, while *Success and Understanding* (Piaget, 1974/1978) is listed only for English. Both will be discussed in detail in the following chapters. (The two translations of *Equilibration of cognitive structures* were identified as well, but this effect disappears when their counts are combined.)

Table 7. Piaget's most highly "under-rated" texts (by density-weighted indigenization score)**English vs. French**The equilibration of cognitive structures,
1975/1985

(-0.94)*

Biology and knowledge, 1967/1971 (-0.90)*

Success and understanding, 1974/1978 (-0.79)*

German vs. French*Biologie und Erkenntnis*, 1967/1974 (-1.0)*

Biology and knowledge

3. Toward a generalized Indigenization Argument

It seems reasonable to conclude from the material reviewed thus far that Piaget was affected by the same sort of importation process as was Wundt: changes were made in the meaning of his research program while it was being re-interpreted for a new

audience, and certain materials were stressed, deemphasized, or left out according to the audience's interests. The goal now is to show this more conclusively; to remove as much of the time-related variance as possible from a simple temporal model of the texts' impact, such that what remains can be attributed solely to error or indigenization.

3.1. Raw citation data. Beginning at the simplest possible place, we start with the basic figures. The mean publication date and standard deviations for the books examined are as follows: 1959.8 (16.3) in French, 1969 (16.5) in English, and 1973.4 (7.4) in German. Given that Piaget was born in 1896, the very late average in the original French is surprising: for most people, "retirement age" is not the midpoint of a career. Beyond this, however, there is very little of substance to say about dates alone. Still, even treated as trivia, these data merit a few comments.

The earliest translations into English all fall more than two standard deviations from the mean on the low end. This supports the accepted belief that Piaget's works were later rediscovered: the early translations fall far outside the expected range, and were followed by a decades-long gap. What is more interesting, however, is that I recall—but cannot now find the reference—that Piaget dismissed his earliest works prior to *Moral Judgment* as juvenilia. That this matches up exactly with our findings is coincidental, of course, because the choice of two standard deviations is conventional. But it is certainly made more interesting by our having identified *The child's conception of the world* (Piaget, 1926/1929) as having been highly over-rated by English audiences (**Table 4**).

On the high end, all of the books published in English and French fit within two standard deviations but for one exception: the recently completed edited volume on

“reason” (Henriques, Dionnet, & Ducret, 2004). This project was started in the last year of Piaget’s life, and was assumed until recently to have been abandoned. That belief was clearly incorrect. Unfortunately, however, as with many of Piaget’s later works, this new book has not yet been translated (but see Piaget, 2004/2006).

The German case is somewhat more complex. Two books in our sample sit beyond two standard deviations from the mean on the distal side: *Das moralische Urteil beim Kinde* in 1954 (known in English as *Moral Judgment* [Piaget, 1932/1932]), and *Die Bildung des Zeitbegriffs beim Kinde* in 1955 (*The Child’s Conception of Time* [Piaget, 1946/1969]). The first is an obvious choice for an early translation: it is one of Piaget’s best-known books in both English and French. (And yet also highly overrated in its German reception [**Table 4**].) But the second seems a very peculiar choice. It is considered a lesser work in English, and of slightly better than middling importance in French. Yet it is tied to the Kantian program mentioned earlier, and—given the American discomfort with grand theories—that is perhaps why it was considered more “relevant” to the German audience.

However, there is also a contextual element here that is worth considering: Einstein died in April of that same year. And it was his questioning of Piaget on this topic in the early 1930s that had originally inspired the project (see Sauer, in press). So perhaps its early publication in German was related to this connection; interest reflected by context. To know for sure, of course, we would need to examine the relevant papers at the Piaget Archives. Unfortunately, these are not among the processed papers.

Two German translations also sit above two standard deviations on the proximal

side: *Die Entwicklung des inneren Bildes beim Kind*, translated in 1990 (*Mental imagery in the child* [Piaget & Inhelder, 1966/1971]), and *Intelligenz und Affektivität in der Entwicklung des Kindes*, translated in 1995 (*Intelligence and Affectivity* [Piaget, 1954/1981]). Neither is especially well-known in English.

The first is a study of spatial and geometrical reasoning, and can thus be considered a sequel to *The child's conception of space* (Piaget & Inhelder, 1948/1956). The second is an extended treatment of lecture notes from a course presented at the Sorbonne in 1953-1954, in which Piaget discussed the “energetics” and “structuring” of reason. This is of course in conflict with the “cognitive” interpretation that American audiences apply in interpreting Piaget, but the reason for the German publisher’s choice of this volume over another is not clear. (Again, those papers are not yet accessible at the Piaget Archives.)

3.2. Periodization of raw citation data. In addition to the identification of books published at unexpected times (which is so superficial as to be almost not worth mentioning), we can use the raw citation data to get a sense of the relationship between time-of-publication and impact in each language. This then highlights certain features of Piaget’s different publishing careers, such as the long gap prior to the “rediscovery” in English and the large cluster of translations in both English and German in the late-1960s through the late-1970s. (**Figure 6.**) But it’s the regressions that provide us with our sought-after model.

An astonishing 42.2% of the variance in the English raw citation counts can be accounted-for simply by the date of publication ($r = -0.65$). The German figure is lower,

but still substantial: 15.9% ($r = -0.40$). But compare these figures to the French data: only 6.4% of the variance in citations can be accounted for by date of publication ($r = -0.25$). This difference is very suggestive.

In short, it does indeed appear as though something happened in the movement of these works from French into English and German: there is a strong order effect. (Works published earlier received more citations.) Our goal, in the following manipulations, is then to eliminate this effect from the simple temporal model. The remaining variance can then be attributed to other factors, such as indigenization.

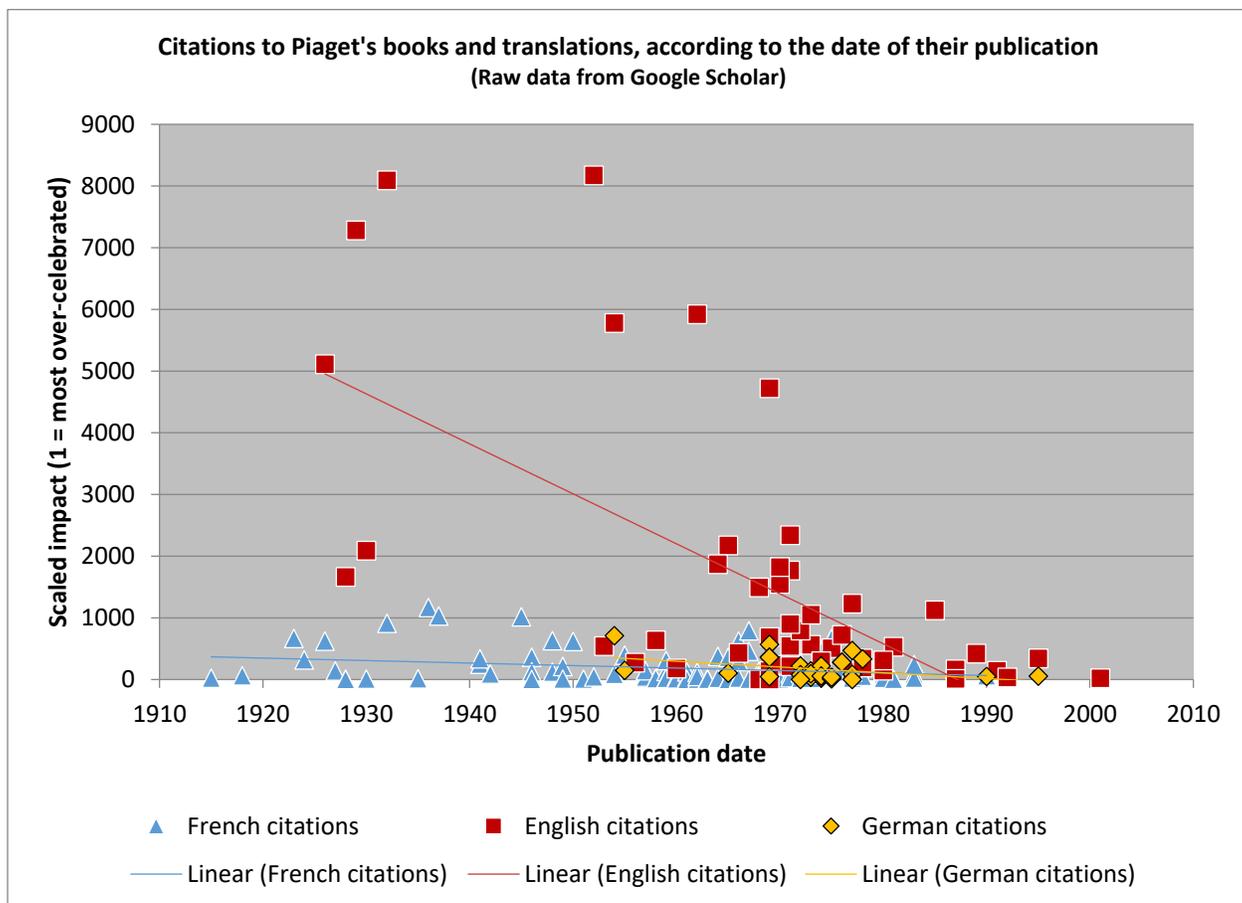


Figure 6.

3.3. Periodization of citation density. The shift from raw citation counts to citation density has immediate and readily-apparent consequences, especially in English (Figure 7). The slope of the regression is reduced massively (from $m = -82.9$ to $m = -1.0$). And the modelled variance is reduced to 23.4% ($r = -0.48$). In German, the change is proportionally similar (from $m = -9.3$ to $m = -0.1$). And variance is reduced to 4.2% ($r = -0.21$).

It's with the French data, however, that our goal is first achieved. The slope is

nearly flat (from $m = -4.0$ to $m < 0$). The variance is also reduced practically to zero ($r = -0.04$). In other words, when citation density is taken into account, there is no relationship between the impact of a text in its source language and its original date of publication. The order effect is thus effectively removed. From this, we can then proceed confidently to re-examine the earlier language-controlled analyses: with these manipulations applied, differences in impact will be due to differences in reception.

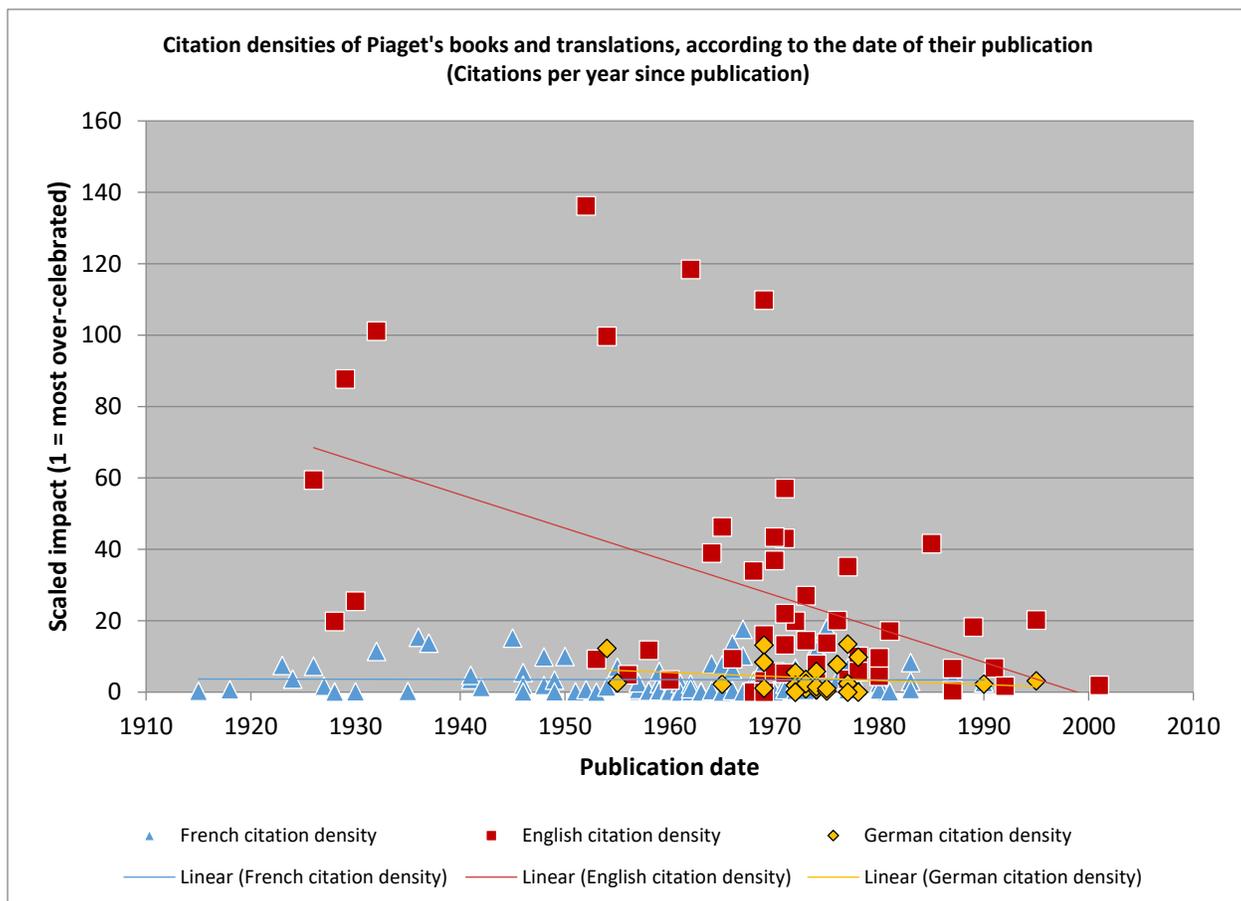


Figure 7.

3.4. Periodization of raw citations, controlling for language. By controlling for both language and publication date, we eliminate differences in the quality or importance of the underlying text. The focus is then entirely on how the audience has perceived the text. And the results are again dramatic. (**Figure 8.**) In English, the slope of the regression is flatter still (from $m = -0.97$ to $m = -0.001$). And the variance is reduced to 14.1% ($r = -0.38$). In German, the slope is almost perfectly flat ($m < 0$). Variance is effectively eliminated ($r \approx 0$). However, it's possible that the clustering of the bulk of German translations in the 1970s could be masking the effect. To be sure, we need to

look at citation density when controlled for language.

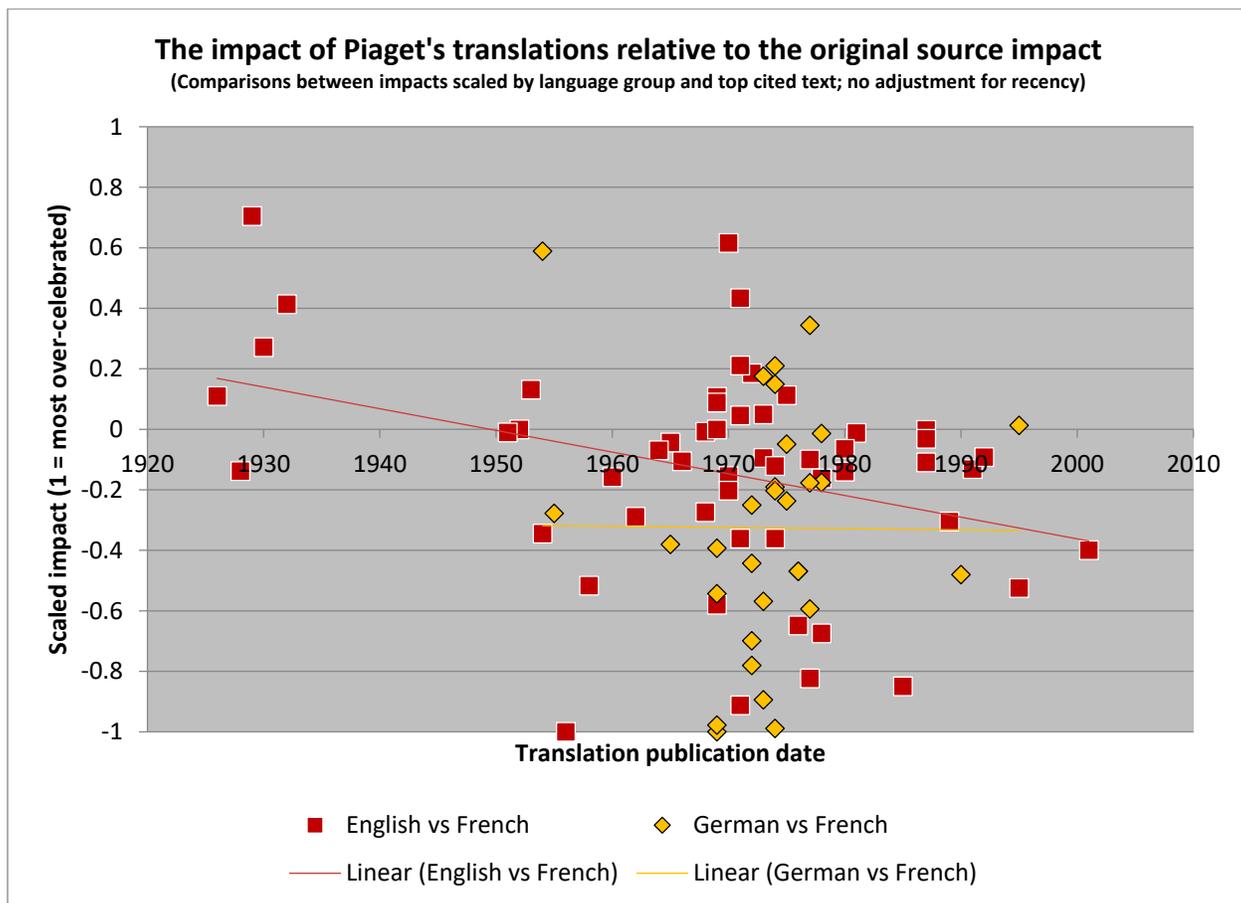


Figure 8.

3.5. Periodization of citation density, controlling for language. The same analysis using citation density shows no appreciable change: the slopes are effectively the same, and the variance is only minimally different (from 14.1% to 13.7% for English). (**Figure 9.**) From the perspective of trying to control for impact per year, this suggests

that the relative impact per year in French is the same as the relative impact per year in English. As a result, no further change is made in the model. And that in turn implies that the remaining variance is due to factors not accounted for by differences in time or language.

Eliminating the first five English translations as outliers has only minimal effect: the slope is effectively unchanged (from $m = -0.007$ to $m = -0.009$). And variance drops only minimally: from 13.7% ($r = -0.37$) to 8.7% ($r = -0.29$). In other words, it seems safe to suggest that we have achieved our goal: with citations per year and language taken into account, publication date is no longer relevant—in German. But the English data show an order effect that is sizeable enough to be of interest to Psychologists. This then suggests that there is more of Piaget that could be gleaned from deeper readers, and that this contribution is being masked by the extant understanding.

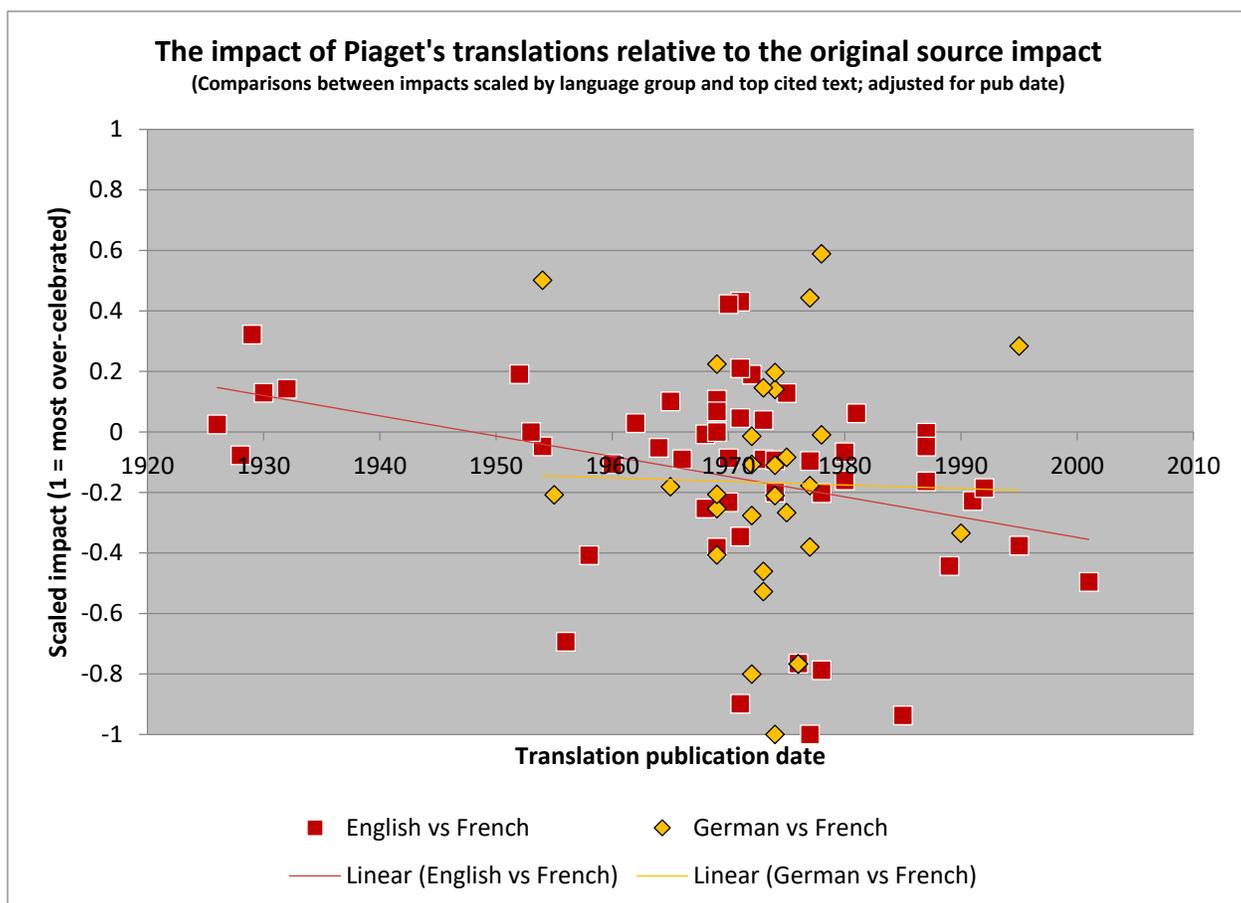


Figure 9.

4. Insights and questions arising

Generalizing from the work done by New Historians examining Wundt's works, and applying these insights to an examination of Piaget's works, I have shown that it is possible to go much deeper than previously thought with a quantitative analysis of the impact of historical texts. Indeed, on the basis of the work done here, I am confident that we can declare an indigenization effect: when both the impact in the source language and the publication dates are controlled-for, the order in which texts appeared in translation accounts for a small but significant amount of the impact that texts had on the English-

speaking audience.

That said, however, my purpose in doing this work was not to suggest that Piaget's status ought to be on a par with Wundt as a Founder of Modern Psychology. (This would be celebratory, and therefore forbidden by the norms of the discipline.) Rather, I think it's *the argument* that has been strengthened by this examination. This stronger version of "The Wundt Argument" can then be called "The Indigenization Argument." That not only formalizes some of the discussion in Chapter III, but it also suggests that the comparison of reception could easily be replicated with the other importations of works by other authors.

In short, therefore, the more general version of the argument is this: when meaning moves, different audiences will interpret it in different ways (cf. Burman, 2012d). In other words: differences in reception reflect differences in the audiences' interests, and we can use those identified differences in order to learn something new about those audiences.

To that end, for example, we can extend the methods above to take a further step and compare the language-controlled impacts against each other. This then identifies only one book that is over-rated by the English relative to its impact in both French and German. And the identified-text is exemplary exactly of the way in which the American view was criticized in Geneva: *Science of education and the psychology of the child* (Piaget, 1930-1965/1970).

4.1. The American Question. Piaget lamented the American obsession with using the experimental findings of developmental and child psychology in order to design

interventions that might speed up educational progress. He called this *the American Question*. And he explained his concern clearly in a magazine interview:

Is it a good thing to accelerate the learning of these concepts? Acceleration is certainly possible but first we must find out whether it is desirable or harmful....

No one has made studies to determine the optimum speed. (in E. Hall, 1970, p. 31)

He continued, responding to a question about preparing students for entry into the workplace in a more efficient way:

It is difficult to decide just how to shorten [the children's] studies. If you spend one year studying something verbally that requires two years of active study, then you have actually lost a year. If we were willing to lose a bit more time and let the children be active, let them use trial and error on different things, then the time we seem to have lost we may have actually gained. Children may develop a general method that they can use on other subjects. (in E. Hall, 1970, p. 31)

These are all empirical research issues that he is raising. He treated the question seriously as a question for science to resolve. For me, though, that misses the deeper point. I think there is an historical reason for why this is the “American” question.

4.2. Pursuing the issues arising. French and German parents are equally interested in having their children “do well” in life. But the American national obsession with “doing better” that manifests itself here seems actually to be something different.

Indeed, I propose that the American Question is not about children, but about War. In particular, I suggest that it is tied up with the series of policy changes that led

among other things to the Rediscovery. Namely: the massive governmental shift in response to Sputnik that produced, among other things, the National Defense Education Act of 1958. That, however, is an Historical topic for another time. My focus, for the moment, is on pursuing the “foreign invisibles” related to the interests of Psychologists that have been hinted-at in the texts identified above.

In pursuing this goal, in Chapter V, I lay out the formal structures behind the New Theory. Its discussion therefore precedes the experimental works summarized in *Equilibration of cognitive structures* that were included in the French citation density list: *The grasp of consciousness* and *Success and understanding* (**Table 3 & Table 7**). But it also made them possible.

Another of the key texts discussed from this list is the untranslated *Logique et connaissance scientifique* (**Table 3**). This was edited by Piaget in response to what he called “the Gödelian crisis,” and in fact his appeals to Gödel changed just prior to its publication. That change then also provides the narrative thread that we follow throughout the chapter.

This thread then continues in Chapter VI, which builds directly on the logical framework. But it focusses on ideas introduced in *Biology and knowledge* (**Table 3, Table 5, & Table 7**). These ideas were then developed further in other seemingly-disconnected writings, including *Psychogenesis and the history of science* (**Table 3**).

The result on the psychological side is the New Theory. But the biological side should not be dismissed. These works articulate the basis for a new and powerful evolutionary-developmental theory which has not been received in a way that recognizes

it as such. That then has implications for Psychology that tie into changes now underway in contemporary Biology. My goal is therefore to show more clearly how those ideas make sense, so that they are not lost in translation. This first requires examining the changing formalism underlying Piaget's theory (Chapter V), and then we can consider the biology (Chapter VI).

CHAPTER V

First foreign invisible: Piaget's neo-Gödelian turn

WHILE PIAGET'S EARLY WORKS were being "rediscovered" in the United States, in the late-1950s and early-1960s, he was simultaneously working on a complete revision of the underlying theory using approaches drawn from biology, cybernetics, and something he came to call "the epistemology of logic" (see esp. Piaget, 1967b; also 1970a, p. 487; 1970/1972, pp. 63-68). The result of these efforts was the construction of a new meta-theoretical framework formalizing the *processes* driving developmental change (Gallagher, 1972/1977; also R. L. Campbell, 2009). And this then informed more than a decade of new experiments (see Ducret, 2000).

This later work extends far beyond the limits of the earlier stage theory of child development (popularized esp. by Flavell [see Müller et al., 2013]). For this reason, it is referred-to in the secondary literature as "Piaget's new theory" (see esp. Beilin, 1992b). The most well-known aspect of this is his unfinished move toward "relevance," and its formalization in a logic of meanings (Piaget & Garcia, 1987/1991). And thus that is what we will examine here, albeit by contextualizing that move rather than reviewing the contents of the book that presents it (see Ducret, 1988).

Of course, Piaget's attempts to formalize his theory—and especially his writings

on logic—have been the subject of innumerable critiques. Yet most of these have been presented by outsiders who failed to fully understand his intent (see e.g., Piaget, 1963). What follows therefore pursues a quite different goal than that typically sought: even as we recognize the present interests of the discipline, we focus on providing the view “from within” (following M. Chapman, 1988a, p. 1).

The task is accomplished by tracing the ways in which Piaget himself described the formal aspects of his theory. And we simplify this by focusing on a particular change in his appeals to the works of Kurt Gödel (1906-1978). The resulting history is then of necessity incomplete: there are many more aspects of the New Theory that need to be developed further, and a change in formalism was not the only change that occurred. (In the next Chapter, for example, we will discuss some of the similarly-timed changes in Piaget’s biology.) Yet what follows does usefully clarify some of what Piaget said about the assumptions that underlie his later works, and so some new translations are also provided where space permits.

To be clear, though: we are not imputing to Piaget any variation of the loose and relativistic crypto-Gödelian interpretations that have subsequently come to be criticized for their abuse of the primary source texts (see e.g., Franzén, 2005). Instead, we trace the emergence of a “neo-Gödelian” view: a view that makes sense of Piaget’s (1967/1971) later observation that “Gödel’s theorems supply impressive arguments in favor of constructivism” (p. 80). And while we first find these arguments in Gödel’s own writings, their influence on Piaget came through the efforts of his French-speaking contemporaries. But, to get to these, we must begin at the beginning.

1. Kurt Gödel and his proofs of “incompleteness”

The program that interests us emerged from Gödel’s (1929/1986) doctoral dissertation. This included a proof of the “completeness” of first-order predicate calculus: it demonstrated the non-contradiction of arithmetical methods and thereby provided a firm foundation for the proof-making activities of mathematicians. (He showed that their tools are trustworthy in a strong sense: *proofs*, if constructed using such methods, are *true*.) Building on this, he then undertook the project that became synonymous with his name: a multi-part proof of the “incompleteness” of all general systems of number theory that are sufficiently complex to allow arithmetical methods to function within them without inconsistencies. That work was presented in 1930, published—in German—in 1931, and accepted as his *Habilitationsschrift* in 1932.⁶⁴

The key assumption of Gödel’s (1931/1986) discovery is that *statements about arithmetic*, if formally presented, can themselves be *treated as* arithmetical propositions. Using a technique based on prime factorization that he invented for this purpose (now called “Gödel numbering”), he showed that certain properties of these meta-mathematical statements could be understood to correspond to truth, falsity, provability, etc., in a systematic way. He then showed that these statements could be manipulated using standard arithmetical methods: they could be made to make strong and trustworthy claims, including about themselves. These claims could thus be shown to be either true or false.

⁶⁴ For additional biographical details, see esp. *Logical Dilemmas: The Life and Work of Kurt Gödel* (Dawson Jr., 1997). Each of the articles cited from Gödel’s *Collected Works* also have introductory notes, which often provide more useful specifics about any individual piece than the glosses provided elsewhere.

The incompleteness project took this one step further. By building on the Liar Paradox—typically presented as, simply, “this statement is false”—Gödel was able to use his numbering technique to set up a self-referential contradiction: “If this statement is true, then it is false.”⁶⁵ And this corresponds to an arithmetical impossibility (i.e., $1 = 0$).

The discovery that such a claim could be proven afforded a peculiar conclusion: “truth” and “proof” are not formally identical. This was then generalized: not every *statement* can be proved to be true or false, within the system that contains it, despite the completeness of that system’s *methods*.

Gödel showed, in other words, that *every formal system* above a certain degree of complexity is afflicted by “undecidable” propositions. (This was demonstrated by his “first” incompleteness theorem.) Furthermore, if such a system *claims*—of itself—that it is complete and consistent, and thus not internally contradictory, then it also suffers from the Liar Paradox and is therefore necessarily *incomplete* and *inconsistent*. (This was demonstrated by his “second” incompleteness theorem.) The details of how this was accomplished, exactly, have been discussed by several well-known secondary sources (most famously by Hofstadter, 1979; but earlier, and most influentially, by Nagel & Newman, 1958). But it’s what happened afterward that’s of special interest to us.

Gödel’s (1931/1986) proof was important because it explicitly undermined a key assumption of Bertrand Russell (1872-1970) and Alfred North Whitehead’s (1861-1947) *Principia Mathematica*. This had been intended to provide a solid justification for the

⁶⁵ Gödel explained this directly to Hao Wang, who reproduced the explanation in *A Logical Journey* (Wang, 1996, pp. 82-83).

relationship between mathematics and logic: if formal systems were complete, Russell and Whitehead argued, then all of mathematics—and, indeed, all of mathematical philosophy (perhaps even all human knowledge)—could be adequately described with, and fully reduced to, logico-mathematical first-principles without any loss of meaning. (This position is called “logicism,” and is traceable ultimately to Gottlob Frege [1848-1925; see van Heijenoort, 1967; also Smith, 1999].) After Gödel’s discovery of incompleteness, however, this goal became formally untenable: there is no single, static formal system that can decide all mathematical truths.

This eventually made Gödel famous. That said, however, the initial reception of his result was chilly (see Dawson Jr., 1997, pp. 53-79). Although it has since come to be interpreted as having undermined David Hilbert’s (1862-1943) *Principia*-invigorated program to provide a formal foundation for all of mathematics, it did not start out that way. Indeed, Gödel had set out originally to *contribute to* Hilbert’s program (Davis, 2005; Feferman, 2008). Yet aside from a handful of professionals (including John von Neumann [1903-1957]), virtually no one noticed his result at the time.

The first major expositions of “incompleteness” for a non-specialist audience were published twenty-five years later (Nagel & Newman, 1956, 1958; see Feferman, 2007). That’s when the idea really began to take off. But that’s not how Piaget encountered it (see Note 72).

2. On the changing implications of incompleteness

The relationship between Piaget and the evolving Gödelian view is a complicated thing to trace. Although Piaget and Gödel almost certainly met at the Institute for

Advanced Study, when Piaget was a visiting fellow there in 1954, there is no correspondence between them held in the accessible collections at any of the archives I consulted. (This is not in itself especially meaningful, though: the majority of the collection held by the Piaget Archives is unprocessed and not available for consultation [Burman, 2013a], although things are indeed expected to open up starting in 2018 [Ratcliff, personal communication, 24 Jan 2016].) As a result, we can only trace the changes in the larger discourse in which they both participated: preparatory spadework, in anticipation of future in-depth archival excavations (e.g., Heinzmann, Trognon, & Tremblay, 2014; Ratcliff & Burman, 2015).

It is toward this end that I undertook a variation on the approach taken by Michel Foucault (1926-1984) in his early works: an “archaeology.” I traced Piaget’s appeals to Gödel in order to identify the “ruptures” that mark out the sedimentary layers separating different “discourses” (see esp. Foucault, 1969/1972). And this in turn led me to examine the changes in the formal justification for Piaget’s theory of “grouping” (*groupement*)—which can be defined most usefully here as *that formalism the contents of which are “equilibrated”*—in order to help the mass of archaeological findings cohere. The result is a narrative that extends our present understanding of how Piaget’s New Theory came to be.

2.1. The importance of logic. The value of a formal logical framework is that it enables the valid inference of *truth* or *falsity* about a conclusion drawn from a set of propositions that are themselves known to be either *true* or *false*, but without reference to the content of the claims represented (i.e., if you have trustworthy methods, then the rest

follows necessarily). And, indeed, this can be put in terms understood of Piaget's "standard" theory of developmental stages. To explain this, though, we must make explicit an assumption inherited from his earliest works: a "stage" is the product of internally-consistent groupings of "structures," inside of which transformations can be made which do not change the quality of the whole (i.e., they are "equilibrated"). These structures then produce actions that either lead to the desired outcome or don't (i.e., they are *functionally* true or false).

This assumption, on its own, is supported by Gödel's *completeness* theorem: transformations are formalizable as arithmetical statements. In application, it is further supported by the argument that formal principles can be used to understand the operation of the mind (see Boden, 2006). The result is that Piaget's "structures" (referred-to variously in translation as "schemes," "schemas," and "schemata" [see Brown, 2001, pp. 181, 189n2]) are theorized as coherent action-producing systems with internal workings having formal characteristics.

Thus, to infer the existence of a particular structure, a Piagetian developmentalist might engage a child in a series of questions related to what is now referred-to as a Conservation task. For example: *When it is true that a volume of water is moved between a tall-thin-cup and a short-fat-cup, then what must also be true about the volume of water during the transformation?* And then, crucially, in terms of the Piagetian method of differentiating kinds of groupings in the children's responses: *Why?* (see Bond & Tryphon, 2009).

Such an approach is all well and good for constructing a classification, because—

contrary to expectations at the time—children at different ages do not all reply in the same way. But things then began to fall apart as a result of how Piaget (1942, 1949, 1952b) formalized this insight further, in an attempt to *explain* his otherwise-acceptable empirical descriptions of child development by positing the existence of different kinds of structures in operation at different ages: “our real problem is to discover the actual operational mechanisms which govern behaviour [sic], and not simply to measure it” (Piaget, 1953, p. xviii).

Briefly put: if the stages of child development are the product of grouped-structures, then Piaget reasoned that the expected outcomes for each stage could be formalized in a kind of behavioral “truth table” listing all of the possible transformations. Different stages would thus have different possible outcomes associated with them, as a consequence of what must follow necessarily given their different operations. (A young child’s response to a problem like Volume could then be internally-coherent, even though incorrect; the result of their truth table indexing only the level of the water in each glass, rather than coordinating height, width, and depth.) Gödel’s *incompleteness* theorems suggested, however, that—even if such a table could be constructed for each stage—then there would still be functional *truths* of that system which could not be contained within the structural table as *provable* statements: children would be able to produce responses and behaviors that didn’t belong to their “appropriate” stage. (There would, in other words, be unevennesses in development: *décalages* [which he had in fact discussed in detail earlier—albeit in species terms—in Piaget, 1941, pp. 251-270].) All of the stage-tables, not just the “formal operational” one, would therefore be functionally incomplete.

And thus so too would be the theory that produced them. QED.

In short: it would have been devastating for Gödel's discoveries to have become widely understood within developmental psychology before Piaget had prepared a response. The entire formal foundation of Stage Theory had been undermined, using the same language in which it had originally been supported.

For Gödel, however, proving the incompleteness of complex formal systems was just a result: a necessary consequence. And, contrary to what one might now think after reading about it in popular commentaries, he didn't just stop working on or with incompleteness once he had proved that Russell and Whitehead—and subsequently Hilbert too—had been wrong to insist that logicians could reduce all of mathematical philosophy (perhaps even all human knowledge) to logico-mathematical first-principles. Thus, we must now ask: *How did Gödel's ideas change, and how did these changes enable the emergence of Piaget's later functional-structuralism?*

2.2. From “Gödelian” incompleteness to a “neo-Gödelian” hierarchy. In December of 1933, Gödel delivered a lecture at Cambridge in which he argued for the continuing importance of finding a means to achieve Hilbert's goal of grounding mathematics on firm foundations (published as Gödel, 1995b; see Feferman, 2008). He also argued that, because of incompleteness, a proof of non-contradiction would have to be found by “constructive” means. (In mathematics, a formal system is “constructive” if it produces the object it intends to prove: the proof is in the production, not in the inference.)

Gödel then delivered a similar lecture at Yale, in April of 1941, in which he

described how “intuitionistic” logics—which treat mathematics and logic as internally-consistent tools for thought, rather than as revelations of the natural order—could be considered “constructive” in this way (published as Gödel, 1995a; see also van Atten, 2014; van Atten & Kennedy, 2003). Still though, as he later explained of the work’s reception at the time: “Nobody was interested” (Gödel qtd in Wang, 1996, p. 86).

A much longer (and more formal) version of the Yale talk was published, in German, in December of 1958. And it had such an impact that it became known among logicians as the “Functional interpretation” (Gödel, 1958/1990; see Avigad & Feferman, 1995). That is also the basis for what we are calling the “neo-Gödelian” perspective, as it came to influence Piaget through the writings of Gödel’s French-speaking contemporaries.

2.3. The Functional interpretation. Gödel’s Functional interpretation was published at a time when interest in his early work was on the rise (via esp. Nagel & Newman, 1956, 1958). His purpose, however, was not to preempt the various misunderstandings that would emerge following its popularization. Instead, he approached a quite different problem: *Where do the insights mathematicians rely upon in constructing new proofs come from?*

His solution is once again too complex to go into here in detail, but he introduced the effort simply enough: “in the proofs we make use of insights... that spring not from the... properties of the sign combinations representing the proofs, but only from their *meaning*” (Gödel, 1958/1990, p. 241; italics as in the original). In this view, proofs do not result from the arrangements of the letters and numbers and symbols which comprise

them. Nor are they a function of the contents of a truth table, in which a result could be looked-up. (Such a result would be “trivial.”) Instead, the elegance of proof-making is a function of the theorist’s competence in *manipulating* the implications *entailed* by those symbols; by *the theorist’s ability at transforming* the symbols’ signified relations, using accepted operations, to say something *new and non-obvious*. In other words, from this perspective, *success* in the genesis of mathematical knowledge is a function of the mathematician’s *understanding*. It is not a function of the *symbols* themselves.

This is interesting in itself, and Piaget’s work certainly did move in that direction in the first published experimental works of the New Theory period (esp. Piaget, 1974/1978). But Gödel’s intent was different: to formalize this fundamental mathematical concern, and thereby make the meaning of “meaning” more rigorously examinable.

To do this, Gödel relied on the notion of “recursion” to distinguish between levels of complexity in mathematical explanation: a proof requiring one operation is simpler than one requiring a transformation of that operation. (Similarly: a statement that relies on recursive self-reference is more complex than one that doesn’t.) “Meaning,” from this perspective, could thus be understood as that *abstraction* which is reflected down through the levels of this many-layered system, from the nuanced understanding of the constructing mathematician to constrain the elegance-in-formalization of the final proof. The result is so, therefore, because it *must be* according to what it *says* and *also* because of what it *means*. Once constructed, the proof then simply *is*: it exists, whether or not there is a formal system to make sense of it.

In contrast to earlier logicist conceptions of the activities involved in proof-

making, Gödel’s Functional interpretation implies that the necessity of results is constrained both “bottom-up” (by the denotation of the given symbols) *and* “top-down” (by the understanding, competence, and insight of the proof-making mathematician). In other words: meaning-*signification* is projected upward, while meaning-*implication* is projected down.⁶⁶ All proofs thus exist in a middle realm, between the structures which comprise them (having signification) and the functions that they have (having implication).

Piaget’s later works contain elements of all of this. Indeed, it is our contention that the two approaches are intimately related: the “neo-Gödelian” insight of the Functional interpretation—that there are levels of relative incompleteness, and that these exist in a hierarchy—informed Piaget’s reconstruction of his “standard” theory at a new level with greater scope. But without direct evidence of contact, between Piaget and Gödel’s Functional interpretation, how can such a claim be supported?

3. Archaeology and its results

To conduct a Foucauldian archaeology is, in broad strokes, to adopt an agnostic approach to history. Instead of bringing my own interpretive lens to the events as I perceive them to have occurred, the following analysis has been guided simply—as it were—“by the fact that words have happened, that these events have left traces behind them” (trans by Gutting, 1989, p. 228). These words have thus been excavated, laid out, explained as clearly as possible, and presented as the product of an evolving “discourse”

⁶⁶ I am indebted to Tyson Gofton for highlighting this crucial distinction, and to Jagdish Hattiangadi for helping me to understand it.

(Gutting, 1989, pp. 244-245). Of course, because there can't be a discourse without discussants, I have also traced the influence of specific individuals as well. For reasons of space, however, only the key figures of the neo-Gödelian excavation are introduced (see also Ratcliff, in press-a). And, once again, we must begin at the beginning: with the “groupings” that provide the basis for Piaget's stages.

3.1. Back to “grouping.” Piaget's theory of grouping originated in his earliest studies. As I mentioned before: in around 1914-1915, when he was in his late-teens, he realized—while half-paying attention in a course on logic taught by Arnold Reymond (1874-1958)—that the coherence of biological species as “wholes” could be defined as being a consequence of the relationship between their individual “parts.” He then began to generalize this insight to all systems with similar whole-part relationships. As he explained later in his autobiography:

I suddenly understood 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; hence I was convinced that I had found the solution.... 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, 1952a, pp. 241-242)

This, he felt, could provide the basis for a scientific epistemology: if knowledge claims could be treated as formal wholes, in the same way as could the biological species

examined to that point in his early studies,⁶⁷ then strong logical tools could be used in the examination of different conflicting knowledge-claims.

Coupled with the training he subsequently received in clinical interviewing at Zurich in 1918, this insight is the source of his interest in grouping children according to their own justifications: *why* the Parisian children he examined in 1919-1921 responded to his test questions in particular ways, rather than *whether their answers were correct*. It also thereby provided the basis for his “standard” theory of stages: different groupings of children, at different ages, justify their knowledge claims in different ways that are nonetheless coherent relative to each other. The later *Traité de Logique* (1949) then attempted to formalize these results more completely. That is also where Gödel is mentioned in Piaget’s writings for the first time.

3.2. A misleading appeal. In the *Traité*, a “grouping” (*groupement*) is defined formally as the theoretical intermediary between a “mathematical group” (*groupe*) and a “mathematical lattice” (*réseau*). It was therefore intended as a way of recognizing and preserving the relationship between a whole and its parts (Piaget, 1949, pp. 91-103). This combination enables several parts to be grouped as wholes at different levels, each of which is then coherent and separate from the other, but with lower levels subsumed by the functional “operations” of the higher levels.

This move is similar to Russell and Whitehead’s replacement of Frege’s “general set” with a hierarchy of set-types, in *Principia Mathematica*, as a way to avoid self-

⁶⁷ For more on Piaget’s early life, see Vidal (1994), Perret-Clermont and Barrelet (1996/2008), and Ratcliff (2010). On his intellectual influences, see esp. Ducret (1984).

referential contradictions like the Liar Paradox. But a simpler way to explain the result is to refer back to Piaget's early background and training in biology.⁶⁸

Just as a *species* is composed of individual organisms, collected together in a group, so too is the next higher level up (genera) composed of individual species. And so it goes: up from species and genera to families, orders, classes, phyla, and kingdoms (extending up still further to *life* and even to *existence*). This is then logically coherent, relative to the definition of a grouping, because each rank in the taxonomic hierarchy can be formalized in ways that are consistent with Gödel's original *completeness* theorem: a species is complete, for example, because its members form an inter-breeding (intra-translatable) functional whole. Higher taxonomic levels are also complete in the same way because they are relatable by reference to their evolutionary history (as an inter-generational lineage of structural transformations), while at the same time remaining qualitatively (functionally) distinct in the present: by definition, couplings between members of different species produce no fertile offspring (cf. "vicariance" in Note 73). Piaget's approach in the *Traité* thus attempted to provide the formal means to describe complex nested wholes within a single interconnected *structure d'ensemble des parties* (typically translated as "structure-of-the-whole," or "structural whole," but more appropriately rendered as "power set" [Campbell in Piaget, 1977/2001, p. 122n]).

The philosophical challenge posed by this formulation is in determining its relationship to Piaget's later constructivism, especially given what he came to say about Gödel's arguments providing support for it. Although Gödel is indeed mentioned in the

⁶⁸ The most comprehensive source on Piaget's biology is Messerly (1996, 2009).

Traité de Logique (and referred-to again in a similar way in the 3-volume *Introduction à l'épistémologie génétique* [1950]), it seems clear from those texts that Piaget had not yet digested the implications of Gödel's incompleteness for groupings: early on, Gödel is for Piaget simply a footnote in the history of logic. He is not *used*, early on, in the same way that he is referred-to later. As a result, there is no formal support within the early conception of grouping for species-change—or indeed stage-change—except by forbidden Lamarckian mechanisms, which Piaget (1952a) had indicated at the time as being a source of discomfort (p. 241).

The problem with this early version of the theory, then, is this: although the children's different groupings of knowledge-justification could be shown to exist empirically, at different age-ranges, there was no biological explanation for developmental change between the groupings except by the old Haeckelian doctrine of "recapitulation" (as Piaget was criticized by e.g., Gould, 1977, pp. 144-147). From this perspective, earlier stages of evolution are complete wholes through which development simply proceeds; they are given, *a priori*. And while this could indeed move children through the required evolutionary-developmental lineage, in the sense of recapitulating different "species of mind" at different "stages of development," it did not have associated with it an acceptable constructive cause (aside from "re-equilibration," which remained inadequately defined until after the neo-Gödelian turn).⁶⁹

That said, however, there was indeed a possible non-constructive cause:

⁶⁹ Piaget (1975/1985) noted that *Equilibration of Cognitive Structures* represents "a complete reworking" (p. xvii) of the models presented in an earlier book: *Logique et Équilibre* (Apostel, Mandelbrot, & Piaget, 1957). The discourse examined here is therefore part of how that earlier material came to be describable as "inadequate."

“maturation.” And this explains the initial impression by several Americans, during the “rediscovery” period, that Piaget’s works were “neo-maturationist” (noted by Piaget in Evans, 1973, p. 39; also in Voyat, 1982, p. xiii). Yet that is only really a problem if species, and thus also stages, are *real* in a strong sense. While this seems to be required by Piaget’s logic, that interpretation is undermined by his biology.

As Piaget (1952a) had noted in his autobiography, he was a “nominalist” when it came to species definitions. (A species is what one *calls* a grouping of intra-translatable organisms, not what that grouping *is*: “The ‘species’ has no reality in itself and is distinguished from the simple ‘varieties’ merely by a greater stability” [p. 241].) Yet this perspective conflicts with the fundamental assumption of the logical model to which he appealed: hierarchical “types” are separate and distinct. They are formally *real*: they exist independently from our thinking about them, like Platonic objects. And that’s how Gödel conceived of them (see Davis, 2005; van Atten & Kennedy, 2003). But that’s not how Piaget used them. Instead, for Piaget, even the *a priori* categories of experience are constructed. This is a fundamental assumption of his research program (see esp. Piaget, 1925, 1950, 1968/1970).

In short, it seems that it was Piaget’s *appeal* to logic in subsuming both his original biological interests and the results of his subsequent psychological experimentation to a larger epistemological structure—and specifically the reference to “groups” and “lattices” in defining *groupement*—that was misleading: as meta-theory it

departed from his avowed “nominalism,”⁷⁰ and introduced hard separations where he needed smooth gradations. This is because it is only through the continuity of species that it would be possible to “move” from one grouping to another on the basis of assimilation and accommodation alone. Therefore, from this inconsistency we see that Piaget’s early logic was incompatible with his intent. Indeed, as he recognized explicitly much later, his formalisms needed to be “clean[ed] up” (qtd. in Piaget & Garcia, 1987/1991, p. 157; see also Piaget, 1971a).

3.3. Beth and the changing formal discourse. We can now consider dismissing claims that Piaget’s early attempts at formalism were anything more than a tool for thinking with. As he put it later: philosophizing in this way is a source of “wisdom,” to be sure, but also one of “illusions” (Piaget, 1965/1971). Reflection must be tempered by engagement with reality, as well as with the facts of historicity and development. Hence his life-long insistence—referring to, among others, James Mark Baldwin (1861-1934)—on the need for a “genetic [constructive] logic” (e.g., Piaget, 1928/1977/1995, p. 184; 1970/1972, p. 15). This, however, was not a concern shared by the logicians who read his work.

Evert Beth (1908-1964), for example, lampooned him: the big book that formalized these early proposals—*Traité de logique* (Piaget, 1949)—was called “mediocre” and “negligent.” Worse, Beth described it as redolent with “failures hidden by pretenses of technicality capable of impressing only a reader naïve in logic” (my trans

⁷⁰ Beth (1960) later reminded Piaget of the importance of this nominalism in a letter that discusses, among other things, both Gödel and constructivism (p. 2; citing Chapter 16 of Beth, 1959).

of Beth, 1950, p. 258). Indeed, this was a common reaction among professionals. Willard van Orman Quine (1908-2000) even referred, in correspondence with Beth, to “Piaget’s persistent and evidently incorrigible stupidity over matters of logic” (Quine, 1960; see also 1940a, 1940b).

Still, Piaget’s response was conciliatory: recognizing the failure, he invited Beth to collaborate on a project aligning their perspectives (Piaget, 1951, p. 244). This led to a sustained correspondence (Heinzmann et al., 2014). And that in turn led Beth—and ultimately Quine too—to join the International Center for Genetic Epistemology as a member of Piaget’s advisory board (Burman, 2012c, p. 284).

Beth participated in symposia at the Center in 1956, 1959, and 1960 (Beth & Piaget, 1961/1966, p. 5). They also undertook a series of writing projects together. These then informed an important book: *Mathematical Epistemology and Psychology* (Beth & Piaget, 1961/1966).

In this book, Piaget’s appeals to Gödel followed Beth’s. Indeed, Beth wrote his entire first half of the book before Piaget wrote his own half in response (p. xxi). They then commented on each other’s work, revised their respective halves, and wrote the general conclusions together. In other words, after the failure of the *Traité* to achieve his aims, Piaget showed that Quine’s assessment was incorrect: rather than being truly incorrigible, in matters of logic, Piaget followed Beth’s lead.

In the final published text, Beth discussed the results of Gödel’s 1931 paper on incompleteness in great detail: he introduced the proof historically, albeit using more complex and rigorous terms than we have used here, and developed it following in the

style of Gödel’s application of recursive self-referential meta-mathematical statements (Beth in Beth & Piaget, 1961/1966, pp. 54-55; also pp. 70, 120-122). He then generalized the results to all formal systems, showing that even a new system brought in to replace an earlier one (proven to be incomplete) will itself be incomplete in a different way.

This is not yet “neo-Gödelian” in the way I intend to mean the phrase, but it comes close: Beth cited Gödel’s (1944/1990) first extended philosophical statement, which expanded upon his earlier critique of Russell and introduced into print the notions of “construction” that would later be formalized in the Functional interpretation (Beth in Beth & Piaget, 1961/1966, p. 112). He then pointed to work showing that unprovable truths could become provable—with their supervening system “rendered more adequate” (Beth in Beth & Piaget, 1961/1966, p. 122)—through the addition of new axioms. He also indicated that the resulting system would be “more powerful” (Beth in Beth & Piaget, 1961/1966, p. 59). Yet he was clear: one can never predict, in advance, what the innovations will be. Otherwise, they would already exist; a new proof would have been constructed. (Or, in biological terms, a new species introduced: it would exist, whether or not there is a taxonomic system to make sense of it [cf. Note 73].)

3.4. Ladrière and levels of relative incompleteness. In December of 1960, less than a year before the publication of Beth’s book with Piaget and almost exactly two years after the publication of Gödel’s Functional interpretation, a French-speaking Belgian logician—Jean Ladrière (1921-2007)—published in the same journal a commentary in French examining the same sorts of problems discussed by Beth. And he began, as did Beth, by explaining Gödel’s result of 1931. But it is where Ladrière took

the discussion afterward that seems to have pushed Piaget toward the neo-Gödelian perspective that came to characterize the New Theory.

There are two ways to read Gödel, explained Ladrière in 1960, referring only to “recent research” (p. 287).⁷¹ The first is to disabuse oneself of the notion that one fully understands the implications of the operations used in constructing any individual proposition. (This, as it happens, is consistent with Piagetian methods [see Bond & Tryphon, 2009].) The second is to posit an open system with indefinite extension:

If this second way is taken, one is brought to envision an infinite, and even transfinite, hierarchy of systems.... On the first level, one can formalize a given domain; then, while grounding oneself on this first level, one can then formalize a larger domain, and so on. One can thus also go as far as one wants, but without ever arriving at an end. (my trans of Ladrière, 1960, p. 299)

This second response to the necessity of incompleteness, for Ladrière (and for Piaget’s subsequent understanding of Gödel), was therefore the necessity of constructivism itself. Systems shown to be lacking at one level are simply reconstructed at a higher one. Ever larger in scope; ever broader in reach.

3.5. Piaget’s uses of Ladrière. While I have not yet found any reference by Piaget to Gödel’s later works (there is only the rupture in Piaget’s *use* of Gödelian ideas), the earliest citation that I did find by Piaget to the Ladrière commentary is from a subsequent article that was published the year of Beth’s death. Indeed, while arguing for

⁷¹ Ladrière (1960) doesn’t cite Gödel’s Functional interpretation directly (he cites nothing directly in that piece and includes no references), but it seems likely—since his own article was also published in *Dialectica*—that this paper is included in the “recent research” to which he did refer (p. 287). The reference was then made explicitly later (see Ladrière, 1981, pp. 298-299).

the use of the limits of formalism as a means to bridge the gap between logic and psychology—and thereby also provide the means to “reintroduce an operator constructivism which refers... to the subject’s activities” (the original intent of the *Traité de Logique*)—Piaget referred specifically to “the great work by Ladrière” (Piaget, 1964/1971, p. 135).

Piaget expanded on these initial comments in a large volume that he edited in response to what Beth had earlier called “the Gödelian crisis” (Beth in Beth & Piaget, 1961/1966, p. 53; see Piaget, 1967c, p. 8). There, he highlighted Ladrière’s suggestion that “formal systems are the abstracted objects [*objectivation*] of mental activity” (my trans of Ladrière, 1960, p. 321; cited by Piaget, 1967b, p. 378). And, more importantly for our purposes here, Piaget pointed explicitly to Ladrière—not Beth, although he is mentioned—as the source of the insight that *levels of relative incompleteness* must exist in a *hierarchy* (Piaget, 1967b, p. 383; citing Ladrière, 1960; 1967). He also repeated Beth’s comment about power, but in slightly different terms: in Piaget’s interpretation, each higher level is “stronger” than the last (e.g., Piaget, 1964/1971, p. 146; 1967/1971, p. 319; 1970/1972, pp. 67-68, 70, 90; 1977/1986, p. 307).

That said, however, the most significant discussion of Ladrière’s contributions is made in *Structuralism*.⁷² Those passages are so revealing of the reasoning informing Piaget’s later works that I will provide an extended retranslation of the most relevant section. Then we will build, from there, toward a conclusion.

⁷² This book also contains the only reference I have found by Piaget to Nagel and Newman’s (1958) popularization of Gödel’s theorem (p. 33n). Yet that reference was added by the translator. And because the change in Piaget’s appeals to Gödel precedes this usage, it would be inappropriate to suggest that it was they who caused it.

3.6. New translations from *Structuralism*. Piaget began by blending the insights of logic with the facts of biology, integrating Ladrière's neo-Gödelian perspective into his earlier framework:

The first point of interest of such observations is that they introduce, into structures, the notion of greater or lesser strength and weakness (relative to the domains in which they are comparable). The hierarchy thus introduced therefore suggests the idea of construction, just as in biology the hierarchy of characters suggested evolution. Indeed, it seems reasonable that a weak structure uses more elementary means and that more powerful forces correspond to instruments whose development is more complex. (my trans of Piaget, 1968, p. 30; cf. Piaget, 1968/1971, pp. 33-34)

He continued, updating his earlier descriptions of how knowledge evolves:

The second fundamental lesson of Gödel's discoveries is... that to complete a theory in the sense of demonstrating its non-contradiction, it is no longer sufficient to analyze its presuppositions. It has also become necessary to construct its replacement. Before this, one could justify believing in a lineage of theories as a kind of beautiful pyramid, resting upon a foundation of self-sufficiency; the lowest stage the most solid, since it had been formed of the simplest instruments. But if this simplicity itself becomes a sign of weakness, and reinforcing a stage requires the construction of its replacement, then the overall consistency of the pyramid as a whole is in reality suspended from its peak. With this height itself unfinished (and having to be unendingly high), the image of the pyramid must

then be reversed and—more precisely—replaced by that of an upwardly broadening spiral. (my trans of Piaget, 1968, pp. 30-31; cf. Piaget, 1968/1971, p. 34)

In other words, the requirement for a firm (Hilbertian) foundation is here replaced. There need only be an initial spark—an inclination—followed by construction driven by exploratory behavior (see esp. Piaget, 1976/1979)

Piaget finished his exposition in *Structuralism* by abridging—and then finally citing—Ladrière’s 1960 article. Here, again, the levels are made explicit:

The idea of the structure as system-of-transformations thus becomes interdependent with a constructivism of continuous formation. However, despite its general significance, the reason for this is simple: one can draw from Gödel’s results important considerations regarding the limits of formalization. One can show, for example, the existence of levels—in addition to the formal levels—that are distinct levels of semi-formal and semi-intuitive knowledge... that are awaiting, so to speak, their turn at formalization. The frontiers of formalization are thus mobile, or “vicariant,”^[73] and are not closed once and for all like a wall marking the limits of an empire. J. Ladrière proposed the ingenious interpretation: “we cannot survey, at one glance, all the operations possible of thought....” (my

⁷³ “Vicariant” is erroneously translated as “vicarious” in the English edition. This is a regrettable error, and one repeated in another New Theory-period work that would also otherwise be extremely important (Piaget, 1977b, pp. 353-356). But we can make the correction: it is properly to be understood as an allusion to the evolutionary process in which a new species first emerges from an earlier grouping: “vicariance,” such that—prior to that moment of speciation—members from either proto-grouping can be substituted for each other without functional impact (see also Piaget et al., 1990/1992, pp. 45n, 54-57, 91-109, 224-226). Otherwise, change occurs solely as a function of drift.

trans of Piaget, 1968, p. 31; citing Ladrière, 1960, p. 321; cf. Piaget, 1968/1971, pp. 34-35)

From this perspective, Piaget's view of children is not unlike Gödel's view of mathematicians: novelty emerges from the projection of meaning, and the results take on a life of their own according to the logic extant at the levels they pass through along the way. Note, too: we can't always predict the outcome, even if we can see all the inputs.

3.7. Implications for the standard theory. In the sequel to *Structuralism*—a short volume published in the same series, and translated as *The Principles of Genetic Epistemology*—we begin to see the emergence of the New Theory's version of the well-known stages of child development. Here, though, the hierarchy of levels acquires traits associated with both “form” and “content.”

Sensorimotor structures are forms in relation to the simple movements they coordinate, but content in relation to the actions that are internalized and conceptualized at the next higher level. Similarly: ‘concrete’ operational structures are forms in relation to sensorimotor actions, but content in relation to the formal operational level of 11-15 years. And these are in turn just content with respect to operations acting on them from still-higher levels. Likewise, in the example given by Gödel, elementary arithmetic is a form that subsumes as content the logic of classes and relations... and it is itself content... from the perspective of transfinite arithmetic. (my trans of Piaget, 1970b, p. 84; cf. Piaget, 1970/1972, pp. 67-68)

Concrete operational reasoning—thinking about objects that can be felt and acted upon as

parts and wholes, like the water in a glass—is therefore made possible by the sensori-motor and pre-operational structures that come before it: reaching, grasping, manipulating, etc. Similarly, formal operational reasoning about “imaginary objects” (like those involved in mathematics) cannot be achieved without first inventing the notion of “an object” that it relies upon and refers-to.

Piaget expanded on similar themes in his discussion of a talk delivered by Ladrière at the International Center for Genetic Epistemology in 1970 (published in 1973). There, he explained, Ladrière had mentioned two other things that were of particular interest. The first relates to this notion of possibility *made possible* by past constructions:

in the deductive branches of knowledge, one always finds “opacities” to illuminate, whether as a result of paradoxes or—more generally—because, once a system is constructed and has become internally consistent, it remains to find the reason for its existence and for its global [higher-level] properties. (my trans of Piaget, 1973, pp. 215-216)

Indeed, for Piaget, the discovery of such “opacities” is an invitation for enlightenment. More importantly, though, it was also after this that the pursuit of such “possibilities” came to serve in partnership with the dictates of “necessity” as a key driver of exploration-without-end (Piaget, 1977/1986; citing Ladrière, 1973, pp. 55-56).

In the second highlight, we see this insight extended to explain how new knowledge—nominally, “an explanation” (but treated functionally, in the abstract, as *a reflection producing desired outcomes*)—is actually constructed:

Ladrière's response is doubly instructive with regard to what we call reflecting abstraction (that which proceeds from operational coordination and not from objects) and the reciprocal assimilation between superior [higher-level] and inferior [lower-level] structures.... Explanation consists in deriving from the preceding structure that which is reorganized on a higher plane, while also enriching it by disengaging what it contained implicitly and then reassembling those functions in a new structure. (my trans of Piaget, 1973, p. 216)

This insight was then developed further in a book on exactly this topic: *Studies in Reflecting Abstraction* (Piaget, 1977/2001).

We end our discussion of what Piaget learned from Ladrière's neo-Gödelian interpretation of incompleteness by referring to how he used these ideas to make the connection back to biology and psychology:

The relationship between two levels is neither one of reduction from ulterior [further] to anterior [closer] nor of simple subordination from one to the other [lower to higher], but is rather one of reciprocal assimilation.... The explanatory relationship between the superior [higher-level] system and the inferior [lower-level] system is a reciprocal assimilation in the sense, not—of course—of identification, but of mutual dependence, and thus of a sort of integration according to the biological or psychological significance of the result. (my trans of Piaget, 1973, p. 216)

In short: the formal processes are the same, for mind and body, but they have their action each at their own level. These levels also complete each other, and make each other

possible, such that the level of biology is as necessary to that of the mind as the mind is to that of knowledge (see esp. Piaget, 1967/1971; Piaget & Garcia, 1983/1989).

4. Conclusion

Before the publication of the Functional interpretation, and the subsequent commentaries by Beth and Ladrière, Gödel was to Piaget simply a footnote in the history of logic. (Gödel's results on *completeness* supported Piaget's use of truth tables, but were no more interesting than that.) Afterward, however, changes in the meaning of *incompleteness* provided the means to reconstruct the Genevan research program at a new level. And it was outward from there that the New Theory was built.

That said, of course, it is important to note that Piaget delighted in his role as “one of the chief ‘revisionists of Piaget’” (Piaget, 1968/1970, p. 703n). We are, in other words, discussing change within a lineage. There is therefore no hard or transcendent *philosophical* rupture here, despite what is implied by Beilin's (1992b) use of the term “new” in referring to the New Theory. Yet there is indeed an *historical* rupture, in the sense meant by Foucault (1969/1972). And that is why it was examinable using an “archaeological” method: tracing evidence through historical layers, identifying the boundaries, and seeking influences that can be named.

Curiously, though, the result of identifying the neo-Gödelian turn is that we now see that the New Theory is not really a theory of “stages” at all. Rather, it is a theory of constructive processes in which stages play a role as part of the hierarchy of levels but—being potentially infinite in scope and extension—lose their interest as a fundamental feature of the theory. Indeed, from this perspective, the New Theory is not at all about

what children *can't* do at different stages of their development. It is rather about what happens before, during, and afterward.

Piaget explained this in an interview, but assuming a background that has until now remained implicit:

Too many people take the theory of stages to be simply a series of limitations. That is a disastrous view. The positive aspect is that as soon as each stage is reached, it offers new possibilities to the child. There are no “static” stages as such. Each is the fulfillment of something begun in the preceding one, and the beginning of something that will lead on to the next. (in Piaget & Duckworth, 1973, p. 25)

Of course, this is likely to be something of a surprise. But it can be made consistent with what we know by referring to the results of our archaeology.

Reflecting back on Piaget's nominalism, one might now suggest that “stages” might always have been little more than a name for something much more dynamic and complex than the name itself implied. This is of course a philosophical claim, rather than an historical one; akin to Piaget's own musings about the meaning of “species” (see Vidal, 1992). Still, it follows that a constructive mechanism was always underneath the descriptive label. And the neo-Gödelian turn seems to provide such a mechanism, at least formally. From this, though, it also follows that it's the *structures* that count. And so the insights of the neo-Gödelian turn apply not only to cognitive functions like *memory* (Piaget & Inhelder, 1968/1973), but also to *affect* (Piaget, 1954/1981, 1970/1976) and *the will* (Piaget, 1962c). What, then, is the result?

We are left with a relatively simple theoretical statement: change is driven by the assimilation of the world to structures, followed by the accommodation of those structures when they are found to be incomplete. This is a minor variation of the old assimilation-accommodation-equilibration story that is presently reflected in textbooks. But then the New Theory adds a new layer of complexity: change occurs within and across levels of decreasing relative incompleteness and increasing scope, resilience, or power (see esp. Piaget, 1980b).

That said, however, children—or, more generally, “developing knowledge users”—don’t *experience* this as a kind of Popperian conjecture and refutation. This is because they *assume* their structures are complete (i.e., they are “egocentric” [see Kesselring & Müller, 2011]). Proofs of incompleteness must therefore be delivered in a “relevant” way; they need to be “meaningful,” according to how this has already been defined in the extant context. And that can now be characterized, formally, according to how found-incompletenesses relate to the extant levels from which structures derive their significations and their implications (see esp. Piaget & Garcia, 1983/1989, 1987/1991; Piaget & Henriques, 1978; Piaget et al., 1990/1992).

This has important repercussions. Among other things, we come to see “the social” in a different guise: it’s just another level (Burman, 2013b). And while even mentioning this may sound like it extends our discussion beyond the limits of what Piaget actually said, that is not the case (Burman, 2015). Indeed, as Piaget put it explicitly during the neo-Gödelian turn: “society is the supreme unit, and the individual can only achieve his intentions and intellectual constructions insofar as he is the seat of collective

interactions that are naturally dependent, in level and value, on society as a whole” (Piaget, 1967/1971, p. 368; also Piaget, 1977/1995). The turn toward “logics of meaning” that are considered to characterize the New Theory can thus now be reinterpreted as a way of modelling this necessary context-dependence. Yet these are, in large part, new issues requiring substantially more discussion; another view can be had by examining the changes to Piaget’s biology that occurred just afterward.

CHAPTER VI

Second foreign invisible: Update of the Baldwin Effect

AT THE BEGINNING OF PIAGET'S career, when he received his doctorate in natural history from the University of Neuchâtel in 1918, the maturation of children was thought by many developmentalists to recapitulate the evolution of the human species (Koops, 2015b; Noon, 2005). This was due primarily to Ernst Haeckel (1834-1919), who was in his time the most influential continental popularizer of Darwin's ideas about evolution (see Sapp, 2003, pp. 31, 36-41; also Gould, 1977; Levit, Hossfeld, & Olsson, 2014; R. J. Richards, 2008).

In psychology, Wilhelm Preyer (1841-1897) was explicit in his endorsement of his Jena colleague's recapitulationism. For example: in *The Mind of the Child* (1882/1888), he explained that understanding child development requires understanding the evolution of the human species (see the special section edited by Eckhardt, Bringmann, & Sprung, 1985, pp. 175-280). In the U.S., G. Stanley Hall (1846-1924) thought similarly (Green, 2015a). As did both John Dewey (1859-1954) and James Mark Baldwin (1861-1934; see respectively Fallace, 2011; Young, 2013, p. 357).⁷⁴ Yet

⁷⁴ Note, though, that Piaget dismissed Hall's version as "weak" (Piaget, 1928/1977/1995, p. 201), "strange" and "false" but nonetheless "valuable" (Piaget, 1945/1962, p. 156), and—eventually—"quite out of date" (Piaget & Inhelder, 1966/1969, p. 60). Instead, he preferred the theoretical approaches of Baldwin and Pierre Janet (see e.g., Piaget, 1928/1977/1995, p. 201). That is the thread we follow here, although

recapitulationism was ejected from evolutionary theory in the early 20th century during the lead-up to the “modern synthesis” of Darwinian selection with Mendelian inheritance (Churchill, 1980).⁷⁵ And so too was Piaget, from biology (Vonèche, 2003).⁷⁶

This disciplining of biology in favor of Evolution came at the expense of Development. Indeed, it is now recognized that Development was actively suppressed from the Evolutionary discourse following the modern synthesis (Amundson, 2005).⁷⁷ The focus was instead on “genes” as the primary cause determining organisms’ present and future physiology. Yet the result was that evolutionary biologists also came to ignore individual organisms even as they took dominion over them: evolution was reconceived as a phenomenon of populations, in which shifting distributions of genes drove the changes observed in physiology (see Sapp, 2003, pp. 147-151).

By the 1950s, however, so-called “dissidents” from the modern synthesis had begun to explore the reincorporation of development into evolutionary theorizing (see e.g., Gilbert, 1994; Levit, Hossfeld, & Olsson, 2006). Or, from another perspective, ideas

following the convention in English of focusing on Baldwin (e.g., Cahan, 1984; Morgan & Harris, 2015; Wozniak, 2009). (For Janet, see Amann-Gainotti, 1992; Amann-Gainotti & Ducret, 2002; Burman & Nicolas, revised & resubmitted).

⁷⁵ Hamburger (1980) noted that this ejection happened earlier in the disciplining of Development than it did in that of Evolutionary thinking, and that that is part of what pushed the two fields apart: “Roux, a student of the major German prophet of evolution, Haeckel, and with impeccable credentials as a selectionist... broke away from Haeckel in the matter of recapitulation. He founded experimental embryology or *Entwicklungsmechanik* in the 1880s as a deliberate countermove against Haeckel's categorical verdict that phylogeny is the sufficient cause of ontogeny, and that there is nothing else to explore in this matter. Roux's decisive move from ultimate or remote to proximate causes... started the alienation of embryological from evolutionary thinking” (pp. 98-99).

⁷⁶ Piaget quoted Preyer directly, albeit in the French translation of his book: *L'Âme de l'Enfant* (see esp. Piaget, 1936/1952, pp. 30n, 63). Baldwin quoted him in English (e.g., Baldwin, 1896b, p. 540).

⁷⁷ Hamburger (1980) was more charitable: “I do not imply a criticism of the originators of the modern synthesis for their neglect of developmental genetics. On the contrary, I would assert that it has always been a legitimate and sound research strategy to relegate to a 'black box,' at least temporarily, wide areas that although pertinent would distract from the main thrust. No great discoveries or conceptual advances are possible without this expediency” (pp. 99-100).

regarding development were themselves starting to be “modernized” (e.g., Noble, 2015). And these changes in turn enabled Piaget to “return,” as he put it, to his “first loves as a biologist” (my trans of Piaget, 1950-1976/1976, p. 40).

Of course, this chapter is not really intended to be about Piaget’s biology (see Messerly, 1996, 2009; Moessinger, 2000). Nor is it about the ideas his biology inspired (see Carey & Gelman, 1991; Langer & Killen, 1998; Parker, Langer, & Milbrath, 2005). Rather, this is a continuation of my earlier examination of the discourse in which his theory was reconstructed: because changes were also occurring in other disciplines that he had relied upon in building his “standard” theory of stages—such as Logic (Chapter V)—his return to earlier interests helped to reinvigorate the entire framework that had originated in his first works. And this is ultimately what led to the emergence of the New Theory, which is of course my focus in this project.

Here, therefore, I examine another example of a change in Piaget’s meta-theory that made the New Theory possible. This is then given additional contemporary relevance by showing how the change also resulted in the extension, and ultimate updating, of the “Baldwin Effect” (so named by Simpson, 1953). After tracing some of Piaget’s more recent biological influences, we then see in addition how a deeper reading of those authors can be used to augment our understanding of Piaget’s later proposals that built upon them. And this includes further developments of his so-called “neglect” of the social (pace Morgan & Harris, 2015). The result is then a totally new view of the New Theory, and an entry-point to understanding how the open hierarchy of levels applies more broadly than simply as a merely-semantic replacement for stages (see also

Appendix A).

That said, however, the result of this discussion need not be a “New Piagetian” approach. (Indeed, if that were my goal, I would coin a more distinctive term without a similar-sounding competitor in “neo-Piagetian.”) Rather, the larger justification for this discussion relates to contemporary changes presently underway in Biology, which themselves have their origins in many of the same works that were cited by Piaget. Briefly put: after many years of separation, Evolution and Development are now being synthesized anew to provide an approach known colloquially as “evo-devo” (see esp. Laubichler & Maehslein, 2007). Indeed, from this perspective, Evolution and Development ought to be conceived-of as being on the same continuum (Carroll, 2005; Robert, 2004).

These ideas are now making their way slowly into Psychology, which otherwise still remains largely gene-centric in its biological meta-theory (see e.g., Chomsky, 2010; Griffiths, 2007; Lickliter & Honeycutt, 2003, 2013; Masterpasqua, 2009; Meaney, 2010; Robert, 2008; Szyf et al., 2009). As a result, the timing seems right for considering how the earlier incarnations of these contemporary ideas were used by Piaget to inform his “stages,” and how they were updated in the New Theory. We can then project the results forward, to replace the notion of an evolutionary-developmental continuum with one of levels in a system (cf. Hooker, 1994). In the process, we also thereby reconnect the discipline itself with its historical roots in Haeckel’s recapitulationism, as well as with the related idea that similar mechanisms are at work in evolution, development, learning, and even the advance of knowledge itself (see Koops, 2015a; cf. Piaget, 1967/1971; Piaget &

Garcia, 1983/1989).

1. An introduction to the Baldwin Effect

The simplest place to start in this penultimate chapter is with the Haeckel-influenced background that informed and then was updated in Piaget's later biological works: "the Baldwin Effect" (Depew, 2000; Young, 2013). This is now understood, generally, to be the process whereby learning alters evolutionary trajectories. But it is also important to note that the proposal originated before the modern synthesis of evolutionary biology, that it was named afterward, and that it didn't gain the currency it now has until the late-1980s (i.e., after Hinton & Nowlan, 1987). It therefore has both an historical aspect and a contemporary one, with boosters and skeptics actively involved in exploring the continuing importance of both perspectives (see B. H. Weber & Depew, 2003; Turney, Whitley, & Anderson, 2007). In order to understand Piaget's use of those same ideas that now are popular for different reasons, we therefore need to step out of the present perspective and try to see it from the inside.

1.1. Context. The intellectual context in which the Baldwin Effect was introduced was very different from today's. Inheritance was understood to be necessary, but its mechanisms had not been discovered: genes *existed*, but they were not *known*. As a result, discussions were broader; based more on principles than particles. And, in this, two important theorists loomed especially large: Darwin, of course, and—before him—Jean-Baptiste Lamarck (1744-1829).

Today, Darwin is the archetypal hero of such stories: an explorer who encountered the unknown, and returned with the principles that inform virtually all

discussions of contemporary evolutionary thinking. These principles were then formalized following their synthesis with the discovery of the action of particulate genes, providing the basis for the system now called “neo-Darwinism” (i.e., the product of continued advancement after the modern synthesis). But the name we know is not the man that was. As with Piaget, there was a Darwin before “Darwin” (e.g., Gruber, 1974; cf. Vidal, 1994). And I don’t just mean this biographically.

Recognizing that even Darwin has a context provides us with an uncontroversial way to get to the Baldwin Effect, as well as to a clearer understanding of how it contributes both to evolutionary and psychological theory, why it has occupied a precarious position in the public understanding of biology (including by psychologists), and why Piaget’s update was in turn so easily misunderstood. Simply put, Baldwin occupies the space *between* Darwin and Lamarck. He is therefore located in the grey zone between the great hero and the great villain of contemporary evolutionary theory. Yet because Lamarck is himself a part of Darwin’s context, we can begin with the later thinker and then work backward. This reverses our usual trajectory, but is necessary given the focus of this chapter: my primary goal is to show something new of Piaget, not of Darwin or Lamarck (the discussion of whom in what follows should be itself uncontroversial).

1.2. Darwin and Lamarck. Darwin’s main contribution was twofold: to present evidence for evolution in a coherent and very rigorous way, and to provide a mechanism that could explain that evidence simply. The evidence that Darwin accumulated is now all but forgotten. But his primary mechanism is well-known: “natural selection” (discussed

most popularly by Dawkins, 1976/1989; Dennett, 1995).⁷⁸

Natural selection is driven by a simple observation: over the long-term, organisms that *can* make more babies *will* make more babies. This then eventually puts pressure on other organisms that compete for the same resources (including members of the same species), such that those organisms which are less able to compete for those resources will also be less able to make babies that can in turn make babies. The result is that those organisms that are most often observed in nature are the descendants of the most fecund earlier ancestors: future generations reflect the results of past competitions. This reproductive prowess then also defines “success” and “fitness” in both the Darwinian and the neo-Darwinian frameworks: they make more.

Most important, however, is that the physiology of those future generations reflects *the inheritance of the traits* that enabled their ancestors to out-compete their rivals in relevant baby-making domains. The changes in physiology seen in the fossil record can therefore be explained by differences in the relative competitive strength of different traits that altered the likelihood of one’s babies having babies (e.g., sight, foresight, and sociality). That same mechanism then also explains why there are so many different kinds of organism with so many different kinds of traits: each individual observed by the naturalist reflects the entire history of selection pressures leading up to the moment of that observation.

This is obviously a powerful proposal: every present-day organism reflects their ancestors’ ways of living in the world. And Darwin showed this through innumerable

⁷⁸ He offered others too, like “sexual selection,” but the primary proposal will be our focus here.

natural-historical studies. (My favorite bit of Darwin trivia is that he published *four* separate full volumes on *barnacles*.) But it was also not an entirely original proposal. Darwin's big idea relied on five earlier big ideas, each of which was equally as rigorously presented and defended as his own:

1. Organisms that are physiologically similar to each other can be considered to be related according to the degree of their similarity, and therefore they can be given similar names (Linnaeus).
2. Organisms that are found in fossils located in adjacent geographical strata, and which are physiologically similar to each other, are the same species even though the paleo-ontological record itself sometimes shows discontinuities and catastrophic endings (i.e., "extinctions"). Because the most recent fossils are physiological similar to organisms found living in the world, they thus reflect the same origin (Cuvier).
3. The Earth shows evidence of massive geological changes occurring gradually over vast timescales not accounted-for by a literal reading of the Biblical tale of Creation (Lyell).
4. Competition occurs naturally as a consequence of population growth outstripping resource growth (Malthus).
5. Evolution occurs naturally as a result of the preservation of traits that are well-used (and the elimination of traits that aren't used), such that the direction of change is from the simple to the complex. A consequence of this is then the gradual accumulation of modifications passed from generation to generation: "the

inheritance of acquired characteristics” (Lamarck).

The combination of these ideas, when coupled with the evidence collected, then affords the Darwinian conclusion—the origin of species by means of natural selection—as a consequence. Yet it is the last influence that is of the most interest to us.

Briefly put: Darwin provided a simpler mechanism for natural change than had Lamarck. It explained more and demanded less. Evolution is driven by inheritance down the generations, but not because the willful actions of great individuals rewrite the fortunes of all those who follow. Thus, famously: an individual giraffe’s *efforts* at stretching to reach the high branches of scarce trees is not the *cause* of evolutionary change, because—according to Darwinian theory—it is not that *effort* itself that drives change. Rather, long-necked giraffes sometimes have longer-necked babies. These babies can then reach higher, and so they are able to gain access to more scarce resources and have more babies which might themselves have longer necks. As a result, longer necks are then also “fitter” relative to the need (and subject to constraints like managing blood pressure).

This seems straightforward today. Yet it was also only a simple corollary of the other proposals upon which Darwin built. With an old Earth, for example, it is not necessary for change to occur as quickly as Lamarck had thought. A slow accumulation of traits—over a stupendous number of years, rather than a few thousand—could plausibly account for the variety observable both in the wild and in the fossil record. The earlier progressivism of Lamarck’s proposal was also weakened, because change became relative to the selection pressures imposed by competition rather than undertaken by

sheer force of will leading to use and disuse. Evolutionary advancement thus lost its directionality: not “progress toward,” but “progress from” (see also Burman, 2008a, p. 176n). But the role of the individual changed as well, because the locus of evolution shifted from individual effort to species-specific traits held in different quantities by high-achieving families.

From here, we see the direct connection from Darwin to a dark history of the intentional selection of people by other people: “eugenics” (see esp. Bashford & Levine, 2010; Fancher, 2001). We also see the influence on psychology of this style of thinking through Darwin’s cousin—Francis Galton (1822-1911), who actually coined that term—and his correlational studies of how “eminence” runs in families (see Fancher, 1983; 2009; also Diamond, 1977). These interests were then carried forward in not-always-obvious ways, although especially through intelligence-testing (esp. Fancher, 1985; Gould, 1981/1996; Zenderland, 1998; also Minton, 1988, pp. 51-55, 143-150; Weizmann, 2010).

In short: several other important areas of psychology, beyond developmental and child psychology, are bound-up tightly with the issues that are discussed in this chapter. Of course, our goal is to follow a different path than other historians in examining this lineage of ideas (e.g., Degler, 1991; R. J. Richards, 1987; Tucker, 2002). Before moving a step closer to Piaget, though, I can’t resist an historian’s aside. Specifically: it is curious that both theories—Darwin’s and Lamarck’s—are so consistent with the demands of the context in which they were proposed.

The French view, provided by Lamarck, can be located directly in the French

Revolution that toppled the aristocracy during The Terror. Yet this upper echelon of society remained in place in England, and Darwin's family (the Darwin-Wedgwood-Galtons) aspired to reach its lofty heights through their great works. (Note that Darwin himself was a gentleman, but not an aristocrat: he inherited wealth, and thus opportunity, but not position.) It is then still-curiously that an attempt to find a path between their two positions was provided by an American: Baldwin, the son of a southern slave-freeing abolitionist, sent north to be educated at the College of New Jersey (now called Princeton University) and eventually hired there in 1893 to serve as the Stuart Chair in Psychology. (For some further details about Piaget's own context, but presented following the levels-perspective developed in this project, see Appendix A.)

1.3. A new factor. Baldwin's goal was to provide a mechanism to fit in the space between Darwin and Lamarck, but in terms acceptable to Darwinians. He called this "a new factor in evolution" (Baldwin, 1896a, 1896b; see also 1896c, 1897). And it was necessary, for a philosopher with interests in psychology, because August Weismann (1834-1914) had just 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 effort to change the body could have any import (Weismann, 1881-1888/1889).

For psychology, and for those interested in finding an evolutionary role for action and learning, Weismann's findings were problematic: it subjugated the possibilities for the individual to the whims of history, and past competition, while also drawing a much sharper line than had existed before between "inherited" and "acquired" behaviors

(Johnston, 1995). From the perspective of biology, however, it was foundational. Indeed, 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 (Sapp, 2003, pp. 68-69, 92-94).⁷⁹ As a result, from this perspective, the Baldwin Effect has always been at least a bit unorthodox: in responding to Weismann, Baldwin set himself apart from what soon afterward became the mainstream. But what had he proposed?

Baldwin's two papers introducing the "new factor" synthesized a series of earlier publications into a single short and coherent narrative. He began by asking a question: "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 favorable, 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)

⁷⁹ Piaget was influenced by this as well: his later writings interpret Lamarck through Weismann's germ theory (Morss, pp. 70-71n16; see also pp. 64-65).

In other words, the organism has to make selections between the causes of its own actions: some lead to good results, and it is beneficial that those couplings between actions-and-results should be repeatable. The resulting mechanism is then “new” because this “selection” is not made across generations. Simply put, it was more a general view: successful *actions* reproduce to make more of themselves.

Every individual in the population performs this new kind of selection in their own unique situation. It is in this way that 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 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.

From this perspective, only the results matter. Baldwin’s is therefore a “functional” argument (see Green, 2009; also Beilin, 1983). And although the selection is made at the level of the organism, it is also a purely automatic (natural) process. No will is required to drive the use or disuse. It is therefore not Lamarckian in a problematic sense.

Still, though, the choice of name wasn’t clear about this. It was also perhaps too close to “organismic selection.” It was therefore soon after renamed “functional selection” (see e.g., Baldwin, 1887-1897/1902, p. 165n; 1902, pp. 94, 165-167; 1909, pp. 15-20). And this is just as well. It’s much clearer: selections are made—naturally, and without intervention—in favor of functions that fulfill 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: *if it works, use it*. (The coincidence of its naming with Gödel’s “functional interpretation,” discussed in Chapter V, also foreshadows Piaget’s update.)

1.4. Mechanism. Baldwin’s first paper didn’t provide a mechanism for how this selection would occur. Instead, his argumentation 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). A mechanism was only proposed after reviewing the myriad observations this assumed factor would need to address. This then came in the second paper: the “circular reaction” (Baldwin, 1896b, p. 543).

The circular reaction is both recursive and vitalist: life moves toward “the good,” and away from “the bad.” Individual adaptation is then the result of life’s many iterations. But Baldwin also pointed out that social organisms don’t do this randomly, because they have access to a form of non-biological inheritance that is made possible by imitation.

Imitation is a special kind of copying. And it is wholly unlike the copying of genes.

Genes are copied chemically. Strands of inherited-particulate (DNA) split in half, and then each half is used as a template. Using the machinery of the cell, this template is then read and the amount of particulate is doubled directly. Imitation, however, is different.

Imitation is an endogenous reconstruction of an exogenous source: a reproduction

by internal structures of something observed externally—with no template (Burman, 2012d)—which is found to have beneficial functional consequences (or not) and then used (or not). As a result, new variations do not emerge by “mutation.” Instead, they reflect their origins. In a way analogous to the discussion in Chapter IV, imitations “indigenize” the external object and re-represent its features according to the functions of internal structures. Yet Baldwin’s new factor then applies equally as well: the imitating organism “moves toward” those variations with pleasurable results, and “moves away from” others. As this is repeated, the organism then produces “habits” of internalized imitations-and-variations (as a learned-alternative to the “instincts” evolved by blind-variation-and-selection).

Note, though: because the imitated behaviors exist, they can be *assumed* to be functional. They must produce “good” ends, or they would have been selected-against. Copying them then requires no special “will.” And thus the imitator need only imitate and move in the direction of “the good.” The rest follows from there: “These 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*” (Baldwin, 1896b, p. 543). Thus, imitations persist across generations to the extent that they are not pressured away: an acquired characteristic, inherited by other-than-Lamarckian means (i.e., inherited indirectly through social learning).

1.5. A further step. Baldwin didn’t stop there. His goal was not simply to provide a new evolutionary mechanism. He was also making a contribution to psychology.

Thus: to the extent that pleasurable results, easily achieved, are less costly than those which come at the end of a long competition, the principle of movement provides a means of exploring new territories not “foreseen” by evolution. And that in turn—as a result of Darwinian competition—provides a new evolutionary impetus toward wider exploration, as well as toward greater intelligence. As Baldwin put it:

The intelligent use of phylogenetic [evolutionary] variations for functional purposes [for “the good”]... 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. (Baldwin, 1896b, p. 547)

The development of even minimally-cognitive functions then returns a direction to evolution, which is in turn afforded by each organism’s individual psychology.

This is already an important leap forward for psychologists. Yet he continued. And the result was the reversal of Haeckel’s recapitulationism:

We thus reach a phylogeny of mind [evolution of mental abilities] which proceeds in the direction set by the ontogeny of mind [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).

In other words, physical change and mental change are driven by the same mechanism. But developmental change also does not recapitulate the evolutionary history of the species. 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 creative role of the mental in evolution bears repeating in different terms, because the Baldwin Effect is often presented by biologists as a shield against the selection pressures that would otherwise be responsible for the production of novel forms following the mutation of genes. (This is also referred to as “masking” [see e.g., Deacon, 2003, p. 92].) Briefly: genetic predispositions are inherited, but these only *become* functional in specific situations and contexts. Because vital movement is always toward “the good,” different predispositions provide different benefits according to where the organism finds itself. And this then opens up new possibilities for further exploration.

The way in which this proposal was made compatible with gene-driven neo-Darwinian theory is discussed next. And it was by referring to that version that Piaget updated the Baldwin Effect: he leveraged the neo-Gödelian hierarchy of levels to provide a general mechanism that is as compatible with neo-Darwinian (particulate) theory as Baldwin’s view was with Darwinian (principled) theory. Before we move another step toward this, though, it must also be said that the publication of these proposals by Baldwin coincided with the publication by other authors of similar ideas, and all were

collected together subsequently—by G. G. Simpson (1902-1984)—under Baldwin’s name well after all of their deaths. (Hence, “the Baldwin Effect” [Simpson, 1953].)

However, the multiple origins of the idea itself is not relevant for our purposes because it was Baldwin whom Piaget read and discussed in the greatest depth and detail.

2. Genetic relates to the genesis of novel forms, not to genes

I quoted Piaget (1982b) in Chapter III as saying that he considered “the global idea of genesis” (p. 83) to be the most important among his inheritances from Baldwin. And because both authors found themselves outside of the modern synthesis, this is then what they both meant by the term “genetic.”⁸⁰ This is also in turn why their writings about “genetic epistemology” 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 meta-theoretical method, but instead were interested in the growth of knowledge as a natural extension of human development itself (see discussion by Apostel, 1987; Vonèche, 1985). And both offered similar mechanisms to explain that growth. Indeed, one might say that Piaget’s early writings were less an *inheritance* from Baldwin, and more of an *imitation* (see e.g., Cahan, 1984; Cairns, 1992). Of course, our interest is in his later variations.

2.1. The biological meaning of equilibration. Functional selection appears in both Baldwin’s and Piaget’s writings, most notably in the form of the recursive “circular

⁸⁰ (In this sense, genetic is therefore a “false friend” in the linguistic sense: the same term is used to refer to different ideas, but separated by enough time and context that those differences become invisible behind the word itself [Burman, 2012a].)

reaction” (Parker, 1993; Sánchez & Loredó, 2007). This is the source of ideas regarding the constructive pairing of “assimilation” and “accommodation,” which both Baldwin and Piaget used to explain the genesis of novel forms. It also provided the basis for Piaget’s well-known developmental triad: assimilation-accommodation-equilibration. Yet this language can also itself be misleading. (For example: Piaget’s triad is not the equivalent of Fichte’s thesis-antithesis-synthesis, despite Piaget’s frequent appeals to dialectics, because for him equilibration is a process and not a result [see also Burman, 2008a, p. 174].) As a result, we must define our terms carefully. Again, though, we can do this most simply by translating them through Piaget’s biological origins.

Thus: if the environment never changes, then the organism is *born* equilibrated. There is no circular reaction at the level of the organism; the inherited evolutionary history of the species has shaped its individual fitness, into which it must simply mature. Assimilations of environmental stimuli are easy: the responses are innate, and the organism needs only to manifest its destiny. But if the environment does change (perhaps because the organism itself has moved), then the organism *must respond* if it is to *retain* its fitness: first it must assimilate the effects of any change it encounters, and then it must move in such a way so as to accommodate its way of life to that change. In other words, the organism must *adapt*.

This is a core tenet of Piagetian theory: biological systems are always re-equilibrating (see the discussions by Boom, 2009; M. Chapman, 1992; Ducret & Cellérier, 2007; Gallagher, 1972; Moessinger, 1978). It is also a core proposition of the Baldwin Effect: it is possible to change how selection pressures affect an organism by

learning to respond differently to aversive stimuli, such that subsequent evolutionary change might follow in the path laid out by prior behavioral and developmental changes. But an explanation for how this could work that was also consistent with neo-Darwinian (gene-driven) theory remained elusive well into the 1950s. Indeed, no less astute an observer than Simpson claimed—at a conference held in 1956—that the origin of adaptation was both “the central problem” and “the prime problem” of evolutionary biology (Roe & Simpson, 1958b, p. 338; Simpson, 1958, p. 521). This is because adaptation is not really a problem of species; it is rather a problem of individuals, and thus was dismissed by the modern synthesis.

The modern solution to this problem—one that is just now beginning to be popularized—was outlined originally by two authors: the Russian-speaking Ivan Schmalhausen (1884-1963) and the English-speaking Conrad Hal Waddington (1905-1975). Piaget cited them both, but preferred Waddington for reasons discussed below. Yet this was contrary to the disciplinary norm at the time: Schmalhausen was preferred by Theodosius Dobzhansky (1900-1975), one of the main architects of the modern synthesis (Gilbert, 1994). And that in turn provides part of the further justification for this history: it is Waddington who is now celebrated as the father of modern “epigenetics,” and who is thus also one of the grandfathers of the new synthesis between evolution and development in “evo-devo” (see e.g., Noble, 2015; Slack, 2002; van Speybroeck, 2002).⁸¹

⁸¹ The prefix *epi-* means “above” or “beyond.” Epigenetics, for Waddington, therefore means “above genetics” (see Figure 2, below). An older variation is *epigenesis*, which should be understood specifically in contradistinction to *preformation* (Young, 2013; also Sapp, in press). To compare the two—epigenesis and epigenetics—see van Speybroeck (2002). For an examination of how related ideas have been used in psychology, see Kitchener (1978).

2.2. “Epigenetics.” Waddington called his version of the Baldwin Effect by a different name: “genetic assimilation” (Waddington, 1953b, 1953a). Briefly put, he proposed the means whereby an acquired character could be copied from the phenotype to the genome, such that it could be reproduced in a subsequent generation even after the earlier cause had been removed.

These proposals were dismissed. Indeed, for Simpson (1953, p. 116), it was not necessary to appeal to other-than-orthodox processes in order to explain change. At the same time, however, Simpson noted that it was not correct to dismiss *Waddington* for having appealed to forbidden Lamarckian mechanisms. Simpson was simply not convinced by the explanation, just as he had not been convinced about the necessity of the Baldwin Effect. Waddington also himself defended against such a reading of his work, while simultaneously making clear how his proposals were themselves distinct from the Baldwin Effect (see also Crispo, 2007). The result was then a way to achieve *the goals* sought by Lamarckians, but by more acceptable means (see also Waddington, 1957, 1959a, 1961a).

Waddington appealed to a similar notion as had Gödel in grounding his Functional interpretation of 1958: circular recursion in the form of feedback, but across levels, such that natural selection could be understood as acting at a different level than that of the operation of individual genes. In his view, too, development could also be understood to have a role in exposing inherited traits to different selection pressures. Indeed, it was this inclusion of development in evolutionary theory that could achieve the sought-after solution to the problem of adaptation. He explained:

the existence of feed-back systems in development... makes possible the appearance of another feed-back system, in relation to natural selection and the environment, which results in the genetic assimilation of acquired characters; and this exactly mimics, by quite another mechanism, the type of result which Lamarck and others have wished to explain.... (Waddington, 1962, p. 96)

In other words, adaptive change occurs *because* different levels of biological organization interact.

From this perspective, the engine driving change is not the individual's high-level efforts in the world, nor is it their repetition. (Therefore, it is not "Lamarckian.") Instead, the mechanism is to be found in much lower-level interactions: just beyond the level of the genes. Yet, as a result, the organism's place in its ecology—the source of selection pressures that have their effect at this low level—can be conceived as belonging to the same interconnected and interacting system as its ways of responding to selection pressures: learning, development, and evolution (see esp. Waddington, 1959c; also 1977).

2.3. Impact on Piaget. This insight provided the biological basis for Piaget's (1974/1980a, 1975/1995) later biological proposals that were most harshly criticized (see esp. Piattelli-Palmarini, 1979/1980; 1994; also Deacon, 2005; Parker, 2005). And indeed, the reception of those ideas was not unlike the initial treatment of Piaget's logic. The biggest difference is that, in biology, he had no disciplinary insider to assist him.

There is, in short, no Ladrière in this story. Waddington's role was rather akin to Beth's (serving as a kind of reorienting catalyst), and there was therefore no one of similar stature and influence to take the second step. Yet, in terms of how the changes

still-ongoing in Biology affected his thinking, that is the end of the story. Its beginning can be found at an in-house conference held in Geneva in 1964, when Waddington provided the means to connect Piaget's interest in biology with the changes in his conception of logic that we have called the neo-Gödelian turn. As Piaget recalled: "he made a very profound comparison between epigenetic construction and a progression of geometric theorems" (noted in Piaget, 1967/1971, p. 14).⁸²

The question then under discussion is by now familiar: *From whence do novelties originate? Are they innate, or do they arise from a more complex process?* Piaget then grouped himself with Waddington, whom he identified as "the embryologist," against the objections of an unnamed physiologist:

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.... (Piaget, 1967/1971, p. 36)

The regulatory organ found in embryology and cognitive development—but not found in physiology—was for Piaget *equilibration* itself. This then became a fluid kind of homeostasis, but without a static set-point: "homeorhesis," in Waddington's (1957, 1961b) terminology, and Piaget cited him in this connection from then on (see e.g.,

⁸² Why it was, exactly, that Waddington was invited is unknown. If those papers are held at the Piaget Archives, they are in the unprocessed collection.

Piaget, 1966/1971, p. 47; Piaget, 1967/1971, pp. 19, 23-25; 1968/1970, p. 710; 1968/1971, p. 49; 1970/1972, p. 60; 1970/1973, p. 52; 1975/1985, p. 4; Piaget & Inhelder, 1969, p. 122; also in Evans, 1973, p. 8).⁸³

The effect of this encounter was so great that, soon after the conference, Piaget returned to biology *ex professo* and began publishing again in biological journals (Piaget, 1965b, 1966).⁸⁴ These renewed studies—of the same snails from his doctorate, about which he had published nothing for nearly four decades, and also of plant development—were then intended to provide the material counterpart to the formalisms discussed in Chapter 5. To understand that, however, we need to reach back once again to the very beginning of the standard theory.

3. From snails to children, and back again

As I have said, Piaget's standard theory was informed originally by a conception of the logic of grouping: underlying a grouping's organization as a whole is a formal cause that is in turn describable as a *structure*. But this wasn't initially a psychological insight. It started as a musing on the relations between parts and the whole they belong to,

⁸³ Homeorhesis is the broad category inside of which the better-known “canalization” is a specific type (see Waddington, 1957, pp. 32, 43-45, 58, 149-150, 189). Today, however, this idea is typically referred-to as relating to “developmental pathways” (see e.g., Gottlieb, 1991, 1997).

⁸⁴ Before this, by which I mean after 1929, Piaget was not working a professional biologist. Indeed, he even referred to himself in 1975 as a “novice” (in Piattelli-Palmarini, 1979/1980, p. 60). The use of the expression, however, is necessary to draw attention to this because he also said in 1975 that he had never left biology: “I haven't come back to it—I've never left it” (in J.-C. Bringuier, 1977/1980, p. 110). Indeed, without the distinction it provides, it is hard to make sense of this relative to his claim in 1976 that he had made a “return” to his “first loves as a biologist” (my trans of Piaget, 1950-1976/1976, p. 40). Yet, with it, we can simplify easily: biology was always important to Piaget, for inspiration, but he did not work as a biologist for very long after the completion of his doctorate. Instead, he turned to philosophy with a view to applying biological methods to the study of human values (Liengme Bessire & Béguelin, 1996/2008). He then found work in a variety of fields, including as an experimental psychologist (Ratcliff & Borella, 2013; discussed in English by Burman, 2015). But it was not until after the conference of 1964 that he chose to once again publish his thoughts regarding biology.

in the greater context of his work in natural history cataloguing snails. It was then this that led to Piaget's rejection of the singularly upward causation demanded by the Mendelians—from genes to phenotype (from parts to whole), with no possibility for the downward influence argued-for by Lamarckians (from whole to parts)—followed by his professional embarrassment and ejection from the discipline (Vonèche, 2003).

Biology, however, was changing. The intervening decades highlighted the one-sidedness of the gene's eye point-of-view. And it was Waddington who ultimately explained the continuing importance of Piaget's original work, referring to his own formulation of the phenomena captured by the Baldwin Effect:

The most thorough and interesting study of genetic assimilation under natural conditions concerns the well-known pond snail *Limnaea stagnalis*. It was made by the great Swiss psychologist Piaget, and was begun in 1929 before he had taken up the study of psychological development in man for which he has become so well known. (Waddington, 1975, p. 92; alluding to Piaget, 1965b)

In other words, Piaget's original *tentatives* that had been dismissed as incorrect at one time were redeemed at another: the trans-generational changes that Piaget had observed in shell shape, which he had posited as being due to the snails' adaptation to turbulent water and strong currents, could indeed be accepted as having been acquired and conserved across generations. The problem was then to find an acceptable explanation for how that happened.

3.1. From snails to psychology. The importance of this to psychology is that—despite having had to abandon his earliest ideas as anti-Mendelian at a time when Mendel

was in ascendance, and thus having been forced to move on from his first loves (Vidal, 1992; Vonèche, 2003)—the influence of Piaget’s earliest biological thinking on his psychological theories cannot be as easily dismissed as his earliest logical ideas. Although his later biological proposals were rejected in a similar way to his earlier appeals to logic, the situation as regards his biology is quite different: Piaget began his career in biology, and was trained in it, while he only ever dabbled in logic. This merits some further discussion.

For the snails of Piaget’s doctoral studies, the grouped-deformation of each individual’s shell was caused *materially* by tension in the muscle used to hold onto a stone in rough water. Briefly: the rougher the water, the greater the tension, and the larger the deformation. Over time, Piaget (being an anti-Mendelian) then thought that this might itself lead the deformed snails to join a different species-grouping. His biometrical methods also supported this view explicitly (Burman, 2010; Vidal, 1994).

After his departure from Biology, these early methods were applied to his studies of belief justification in children. Thus, similarly: for the children of his earliest studies of intelligence in Binet’s former laboratory-school at Granges-aux-Belles in Paris, the differences between age-groupings in answering test questions could be conceived-of as having been caused by differences in the “mental tension” that Pierre Janet talked about in the lectures that Piaget had attended at the College de France (Burman, 2007c; Valsiner & Van der Veer, 2000). So, in short: children hold on tightly to their early justifications, then eventually give up, let go of childish things, and thereby “re-equilibrate” to a higher “species of mind” (with the underlying mechanism remaining

undefined).

3.2. From snails to stages. If one were to construct an assessment tool based on the synthesis of Piaget’s early biology with the theory of mental tension that he learned from Janet, the result would be a conception of change that is consistent with stages. Indeed, Piaget talked about Janet’s work in exactly these terms (Burman & Nicolas, revised & resubmitted).⁸⁵

From this perspective, the only difference between a “series of groupings” and a “sequence of stages” is the existence of a set of transformations which unite them in a lineage. Piaget’s resulting studies showing these transformations—a natural history of childhood, following the broad pattern set out in his dissertation (*viz.* what you find when you look)—were then reported in dozens of books. And it’s these that informed his standard theory.

Of course, the results of that work came to be widely criticized (see e.g., Morra et al., 2008). Yet the changes that followed the popularization of these criticisms also led to a series of alterations to the theory itself, shifting the focus from the fact of “stages” to the question of “process” in their emergence. And this, in a nutshell, is the problem of “genesis.” It is also the most important of Piaget’s theoretical inheritances from Baldwin (see Piaget, 1982b). That puts us back on track.

4. A new look at the problem of genesis

After the in-house conference in 1964, an important subtext in Piaget’s writings

⁸⁵ This is almost certainly why he referred to Janet as his “true mentor” in psychology (see Amann-Gainotti, 1992; Amann-Gainotti & Ducret, 2002).

was to refer to these new biological ideas in terms of their causal role in generating novel forms (e.g., Piaget, 1967/1971, p. 36). He even referred to the possibility of replacing then-standard conceptions of linear causation with new ideas regarding “cybernetic circuits” inspired by Waddington’s use of feedback (Piaget, 1967/1971, p. 294n). This was then followed by a book in his house-series, *Études de l’épistémologie génétique*, on cybernetics and epistemology that included a paper that had garnered severe criticisms at the in-house conference in 1965 (noted by Piaget in Cellérier, Papert, & Voyat, 1968, p. 2). And he followed that up with his own book—again citing Waddington (Piaget, 1974/1980a, p. 10)—in order to explain his view of how these mechanisms might work in better-known biological systems.

Until recently, however, all of this made very little sense in terms of Piaget’s workflow in psychology: although he did indeed revisit his earlier writings on the child’s understanding of causality—in two short volumes (Bunge, Halbwachs, Kuhn, Piaget, & Rosenfeld, 1971; Piaget & Garcia, 1971/1974)—his publications from this time do not reflect the same kind of experimental intensity that characterize his subsequent New Theory writings (e.g., Piaget, 1974/1976, 1974/1978, 1974/1980b). Indeed, there is a gap in his non-theoretical publications at exactly this time: between 1968 and 1974. Although his collaborators continued to publish in his house-series, his own productivity dropped off uncharacteristically. But this is where archival study becomes so important for historical projects.

Although there isn’t space here to go into detail, I recently discovered a series of unpublished manuscripts among the newly-processed materials at the Piaget Archives.

These report on over one hundred experiments that seem to have been inaugurated in the period immediately following the 1964 conference. And they are all about causality.

So far, I have identified three full books' worth of material that appear to have been intended for publication starting in around 1970, along with a partial fourth book. The reconstruction of these books has since become a major project in its own right (Burman, 2016; Burman & Ratcliff, 2015; Ratcliff & Burman, 2015). But suffice it to say, here, that many of the concerns that later characterize the later New Theory are reflected there in embryonic form.

In Chapter 5, I argued that these concerns arose as a result of a new interpretation of Gödelian incompleteness: change occurs across a hierarchy of levels as a result of the proof of the incompleteness of functional structures. This was a history of Piaget's "logico-mathematical" argumentation; a discussion of *formal* causation. There, he appealed to the works of logicians: Gödel, Beth, Ladrière, and others. In his subsequent biological works, however, Piaget appealed instead to biologists: Waddington, of course, and also to Paul Weiss (1898-1989). In other words, he began to move toward explanations incorporating *material* causation. And now we see that the unpublished psychological experiments looked at how causality itself comes to be represented during child development; not a study of how a knower's relationship with the world could be conceived-of, but of how the world pushes back. To put it in terms more consistent with its origins in the material presently under consideration: *Piaget sought to understand how the world inflicts selection pressures on developing organisms, and forces them to change in ways that accommodate—and, indeed, anticipate—those pressures.*

We are now moving beyond Piaget's earlier nominalism: perturbations caused by selection pressures aren't dismissible in the same way as a convention about what to call a thing. These effects are real, although not in the sense of logical realism—for reasons previously discussed (see also J. Richards & Von Glasersfeld, 1980). Rather, they provide the functional proofs of the incompleteness of an organism's extant structures. By examining his appeals to the changes being made in Biology, we can therefore see that the emergence of the New Theory was embedded in a much larger discourse than that examined earlier.

4.1. Weiss' hierarchy. In constructing the biological aspects of the New Theory, Piaget's appeals are exactly consistent with what we would expect from having excavated the neo-Gödelian turn: he cites authors especially for their thoughts about the interactions across levels in multi-level systems. Indeed, Piaget mentions both Waddington and Weiss—in the same sentence—in his published “afterthoughts” to a paper presented by Inhelder at the Alpbach Symposium held in Austria in 1968. From his perspective, their views can be synthesized with his own as different levels reflecting the same fundamental set of principles:

From the embryological regulations, whose fundamental stage Paul Weiss called “reintegration”, or from the numerous cybernetic circuits described by Waddington at the heart of his “epigenetic landscape”, up to self-regulations which the study of mental development is continually bringing to light, we find a quite remarkable functional continuity. And if we remember that the thought operations, with their anticipations and retroactions (operational reversibility) also

constitute regulations, but that they are “perfect” regulations (with precorrection of errors and not only correction after the event), we are struck by the generality of these vital fundamental processes, whose knowledge is just as indispensable to the psychologist as to the biologist (Piaget, 1969, p. 158).

In other words, Piaget thought that Weiss and Waddington had provided tools for thought from which psychologists could benefit. But what was it, in particular, that had caught his eye?

Weiss’ (1969) view, as presented in his talk at Alpbach, was informed by a study of the lifespan of neurons and the macromolecules which constitute them. He noticed that the unitary mind is maintained despite the turning-over of its constituent elements. At the same time, however, these elements do not—as individuals—have any meaning except as part of this whole. (They have no innate “essence.”) Thus, he reasoned, the whole can be reified as a supervening structure which completes the lower level neurons by providing the means of organizing them: they have stability as a whole in consequence of their membership in that grouping. The mind is thus *not only* a consequence of neuronal *activity*, but *it is also* a product of neuronal *organization*. Its properties are afforded bottom-up (by the biology and chemistry of neurons), just as it is constrained top-down (by their organization).

A similar realization applies to genes: genes are not innately meaningful, as life-essences, but rather become meaningful as a consequence of their relationships to other living things. Alone, genes are incomplete. As Weiss explained:

...genes, highly organized in themselves, do not impart higher order upon an

orderless milieu by ordainment, but [rather are themselves] part and parcel of an ordered system, in which they are enclosed and with the patterned dynamics of which they interact. The organization of this supra-genic system, the organism, does not ever originate in our time by “spontaneous generation”; it has been ever present since the primordial living systems, passed down in an uninterrupted continuity from generation to generation through the organic matrix in which the genome is encased. The organization of this continuum is a paradigm of hierarchical order. I have schematized it [see Figure 10]... by concentric shells which, in this case, coincide with physical enclosures. The diagram is self-explanatory. The profusion of arrows indicates pathways of all possible interactions that must be taken into account in studying the dynamics of this system, organism. (Weiss, 1969, pp. 37-38)

Although this was illustrated as the relationship between each level of the organism relative to its environment, we can also understand it schematically using a more general perspective of the levels of our interaction with the larger world: from molecules to minds, from neurons to neighborhoods, and—as he explained in an earlier essay (Weiss, 1960)—from observed data to scientific theories too.

Weiss’ connection between the process of biological assimilation and theory construction is exactly consistent with Piaget’s program of research in genetic epistemology (noted by Piaget, 1976/1979, p. 57; see esp. Piaget & Garcia, 1983/1989). Indeed, Piaget saw the similarity as a further endorsement of the kind of thinking that informed his standard theory of stages: “This parallel is so close to what I have always

argued apropos of cognitive assimilation that there is no need for me to press the point” (Piaget, 1976/1979, p. 57).⁸⁶ The two can therefore be connected; at least in Piaget’s mind, one a continuation of the other. But, importantly, Weiss didn’t reciprocate. After Waddington’s endorsement, Piaget was left to fend for himself.

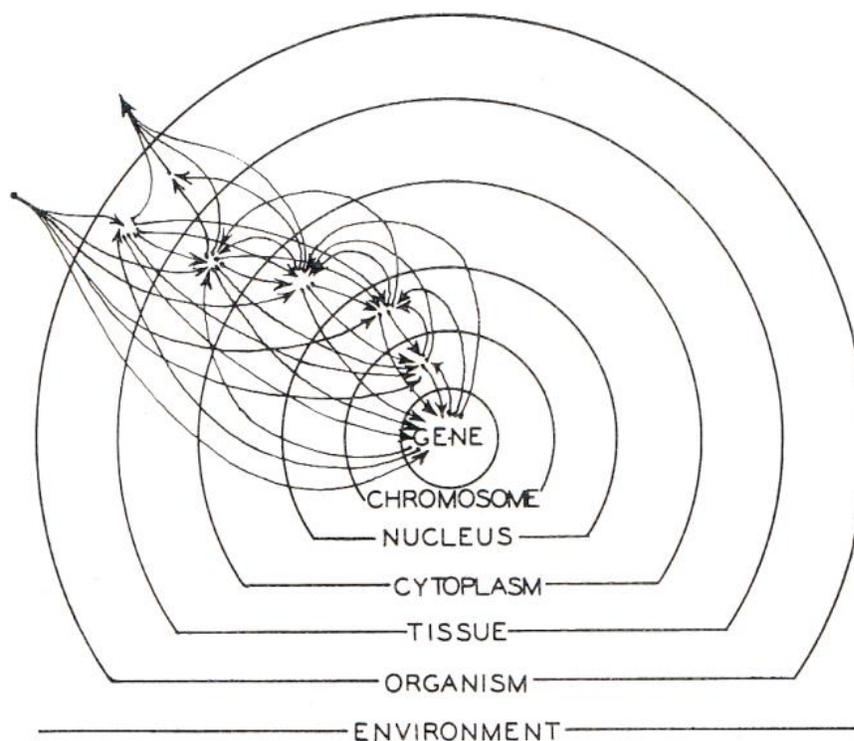


Figure 10. Paul Weiss’ “self-explanatory” illustration of the hierarchy of levels. Reprinted figure with permission from Weiss, *Reviews of Modern Physics*, 31, p. 18, 1959. A copy of the image is also included in his *Hierarchically Organized Systems*, which Piaget later cited directly (in Piattelli-Palmarini, 1979/1980, pp. 197, 387; Weiss,

⁸⁶ Their agreement runs deep. And not just on substantive issues. For example, Weiss (1960) even makes the same parallel as Piaget in comparing learning with eating: “Knowledge grows like organisms, with data serving as food to be assimilated, rather than merely stored” (p. 1716; cf. Piaget, 1960/1973, p. 70; see also Piaget in J.-C. Bringuier, 1977/1980, p. 42).

1971, p. 40). The original is copyright 1959 by the American Physical Society (reuse license #3944070900942).⁸⁷

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4.2. “Phenocopy.” As Piaget returned to Biology, he conducted a systematic review of all of the ideas developed since his doctoral training which could also be applied to the ideas that he had first pursued in making the transition from Biology to Psychology. (This was published under the title, *Biology and Knowledge* [Piaget, 1967/1971.]) It is perhaps unsurprising that the result of his survey was a reciprocal endorsement of Waddington’s work. But it is also interesting to find, in Waddington’s writings, some of the sources and ideas discussed in Piaget’s later biological books.

For example: Waddington discussed “phenocopy” at some length, as part of his discussions of the Baldwin Effect and genetic assimilation.⁸⁸ Unfortunately, Piaget’s version of this has been badly misunderstood. And it seems this was entirely his fault. The gist, though, is that selection pressures can be understood to have their impact according to the levels—and the functional reaction ranges of the pressured structures—which come to be perturbed (i.e., their plasticity).⁸⁹

Recalling Weiss’s view of biology as an ordered system—which Piaget cited only after endorsing Waddington—these reaction ranges can be understood to operate at different levels in a neo-Gödelian hierarchy. In certain special circumstances, this separation of levels can then have the effect of “copying” a specific phenotype to the level of the genes, such that an adaptive change comes to be preserved across

⁸⁸ François Jacob defines this simply: “Phenocopies in biology are modifications due to the environment that imitate genetic effects.... But this represents only a slight variation within the realm of what the genotype allows. In the case of the small animals from the bottom of Lake Geneva [Piaget’s snails], the observed variations are always those allowed by their genotype. One always remains within the working margin authorized by the genes, and one simply has a slight variation due to temperature, osmotic pressure, and so forth” (in Piattelli-Palmarini, 1979/1980, pp. 61-62). Piaget wanted to go a step further. This chapter describes how he sought to do so.

⁸⁹ The best contemporary source for this kind of argumentation is West-Eberhard (2003).

generations. The result is then a Lamarckian outcome by different means (Piaget in J.-C. Bringuier, 1977/1980, pp. 112-113). And that, of course, was not allowed unless one is both extremely careful and very convincing.

The controversy that resulted, and the confusion, is nicely represented in the discussion that followed Piaget's debate with Noam Chomsky at Royaumont in 1975. However, it is especially noteworthy that Nobel laureate François Jacob—whom Piaget had previously cited (in 1967/1971, p. 294n; 1974/1980a, pp. 8, 62n)—objected simply that “phenocopy” did not have the meaning Piaget intended (in Piattelli-Palmarini, 1979/1980, p. 62). Indeed, the word suggested by subsequent critics is “genocopy” (see e.g., Deacon, 2005). With that clarified, the rest of the confusion then diminishes to mere trivia.

4.3. On the abuse of a discipline's specialist vocabulary. Of course, for the dismissal of Piaget's biology to come down to the choice of a word seems ridiculous. We must therefore pursue it: *was Piaget aware of both terms, and of the distinction between them?*

With the terminological suggestion in-hand, we find it easily in the primary sources. It was even included directly in the *preliminaries* of one of the main books under discussion at Royaumont. There, he wrote: “Konrad Lorenz, writing of behavior patterns which become hereditary in certain species of ducks.... proposed the term ‘genocopy’” (Piaget, 1974/1980a, p. 11n; citing Lorenz, 1963/1969, pp. 72-73). This reference was then also included in Piaget's response to Jacob: “I would like to question this point [i.e., Jacob's objection to the use of the term ‘phenocopy’]... When Lorenz gives an example,

he calls it ‘genocopy’ to indicate the difference... Anyway, this is a mere detail” (in Piattelli-Palmarini, 1979/1980, pp. 63-64).

This dismissal is important for understanding what happened in the reception of his later biology: he dismissed a possible error in the choice of a technical term—a name—in favor of focusing on what to him was the underlying meaning. Indeed, in this sense, Jacob was exactly right to object. But it also seems that Piaget knew about the terminological problem in advance. He just didn’t care.

Of course, this is also an easy thing to check. Piaget didn’t always provide explicit references, but he did in this case. Simply following the citation, we therefore find the following in the English translation of the cited text by Lorenz:

For those interested in the laws of heredity and phylogenetics it may here be said that the process described above is the exact opposite of a so-called phenocopy. We speak of this when through extrinsic individually acting influences, an appearance, a phenotype, is produced which is identical with one that, in other cases, is determined by hereditary factors. In ritualization, a newly arisen hereditary disposition copies forms of behaviour formerly caused phenotypically by the concurrence of very different environmental influences. We might well speak of a genocopy. (Lorenz, 1963/1966, p. 59)

The two terms are therefore opposites, and this was known to him. At Royaumont, however, Piaget justified his continuing stubborn use of the problematic term by referring back to Waddington: “If the term *phenocopy* gives rise to alternative definitions, then let us simply speak of genetic assimilation in the sense used by Waddington, who also

admits that there can be replacement of a phenotype of the same form” (in Piattelli-Palmarini, 1979/1980, p. 63).

Disregarding the words used, is this appeal itself incorrect? On its face, it seems not. But Piaget continued, citing Weiss, and then said something that would have seemed at the time to be contrary to neo-Darwinian orthodoxy:

Now, as for the relation between phenocopy and genome, I would like to remind you of Paul Weiss’s profound remark: that when we say “related to the genome,” this may have two completely different meanings: the first one being “determined by the genes” and the second one “compatible with the genes,” which is not the same thing. It is precisely in the case of phenocopies that *there are new hereditary formations compatible with, but not determined by, previous genes.* (in Piattelli-Palmarini, 1979/1980, p. 63)

This would have been totally unorthodox at the time, as several commentators pointed out (summarized by Piattelli-Palmarini, 1994). But no longer.

What Piaget was talking about—functional equivalence given structural difference—can be understood as arising directly from the formalisms of the neo-Gödelian turn: the same outcome can occur by different means. This reflects the whole-part relations captured by Piaget’s use of the term “vicariance” (Chapter V). It also appeared later in psychological guise as “morphisms” (see esp. Piaget et al., 1990/1992). And that is a key part of the New Theory that has proven very difficult to understand (Acredolo, 1997; Bickhard, 1997; Davidson, 1988).

Still, I don’t really want to be pulled off track just yet by this interesting

foreshadowing in his late-biology of that still-later development in his psychological writings, even though the biological underpinnings are now being returned to by biologists. My intent here, as we move toward a conclusion, is rather to point to the quibbling over terms; what Piaget referred to in his *preliminaries* as “terminological difficulties” (Piaget, 1974/1980a, p. 11). In short: in his view, the word he used was not as important as what he meant for this labelling to refer to. With that clarified, we can then explain his proposal in more detail.

4.4. Back to nominalism. From this we are returned to Piaget’s nominalism, discussed in Chapter 5. We even see that a focus on this wrong word may well be what led his critics astray. Yet we also see that Piaget did not himself accommodate to the pressure his peers exerted, as he did with his logic. He did not change in response to criticisms, and even persisted in his use of this inappropriate term in later writings:

There has been some question as to whether the genotype copies the phenotype or vice versa. As a partisan of the first view, Lorenz has sought to clarify matters by proposing the term ‘genocopy’ in connection with a form of behaviour [sic] in certain ducks which was at first phenotypical and then became hereditary. But it is now standard usage to speak of phenocopy when referring to simple instances of an initial phenotype being copied by a later genotype. (Piaget, 1976/1979, pp. 70-71)

Yet this at least helps us to make the point about perturbations—and the corrections of experts (including relative experts, such as in the relation between teachers and students)—becoming selection pressures.

In the sense that knowledge-production is a kind of cultural evolution, Piaget's biological contributions to knowledge have effectively died. As a result, contemporary scholars cannot observe them in situ. They must instead excavate those remnants that continue to be preserved in our cultural archaeological record, and then put them in context, in order to correctly interpret what the fact of their former existence might mean. And relative to the amount of ink spilled as a result of what seems to have been a simple misunderstanding—resulting not from substance, or even from rhetoric, but from diction—I suggest that it would have been much easier and more efficient just to do this historical heavy-lifting. We don't even need to delve very deeply to find easy questions worth asking that would have led to a similar place as we now find ourselves.

For example: *Why did Piaget (1976/1979) entitle his last biological book *Comportement Moteur de l'Évolution*? This strikes exactly at the very heart of the issue. Indeed, I think it was horribly misleading for the translator to render that title as *Behaviour and Evolution* (intending perhaps to allude to Roe & Simpson, 1958a). They should have preserved the original: *Behavior, Motor of Evolution*. Why? Because of what Piaget meant.*

Briefly put: without exploring the unknown, organisms cannot encounter selection pressures to which they aren't already adapted. Their structures cannot therefore become perturbed (disequilibrating), and they cannot therefore be updated. Or, to give this a neo-Gödelian spin: without exploring the unknown, the assumption of the perfection (completeness) of an adaptation cannot be falsified. This then prevents accommodation and the construction of a broader basis for action. In other words: without the openness

required of exploration, Piaget argued, there can be no development and no evolution.

There is only maturation and mutation (cf. Jablonka & Lamb, 2005/2014).

5. Making sense of the intent

Of course, we can dig much more deeply here. Indeed, pushing issues of language aside, we come to see something quite different (pace Piattelli-Palmarini, 1994).

Following Waddington and Weiss—which is to say, in a hierarchical multilevel biological system—a “perturbation” is addressed initially at the level of greatest plasticity: individual behavior. If the organism’s learned responses do not resolve the imposed pressure, its remaining effects then feed-back through the hierarchy. First, this affords a learning opportunity. Then it alters morphogenesis in development. (Snail shells change shape in rough water, and plants grow to different heights.) And then, if still unresolved, the pressure can become life-threatening. The extent to which that life-threatening selection pressure is experienced differentially by the population, the following generations’ distribution of genes will then come to accommodate it by means of natural selection.

The result is that an earlier higher-level developmental accommodation can become evolutionarily fixed: a change in genes, but preceded by changes in development.⁹⁰ And so an earlier phenotype (due to developmental change within a

⁹⁰ This then provides an alternative explanation for “punctuated equilibrium” (Gould, 2007). Briefly: random mutations accumulate in the genome that have no relevance to the extant selection pressures. (There is an accumulation of structures without functions.) When the pressures change, however, those organisms that are pushed beyond their reaction ranges—beyond their developmental plasticity—experience physiological stresses that then highlight the functional effects of previously-irrelevant mutations. Most will still be irrelevant. But those mutations that confer a relative advantage will tend to have benefits that increase the likelihood of bearing offspring. These offspring then carry those mutations forward. Note, though, that the Baldwin Effect serves in this case as a “shield” against selection pressures

reaction range) is “copied” at the level of the genes (due to natural selection). The difference that previous generations expressed during development then disappears in later generations; they develop following their own trajectory.

The way to think about this “transmission of pressure” that makes the most sense to me is to refer to the French approach to long-division that I learned in elementary school. Granted, this is an analogy. But it is very similar to what Piaget seems to have meant: the two seem to me to be, in his terms, “isomorphic” (cf. Piaget, 1963/1968, pp. 188-189).

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, with no remainder: 5. And that in turn provides a resolution of the problem.

At the higher level, before the decimal, we have 1; at the lower, 5. Hence: $3 / 2 = 1.5$. Once resolved, there is no pressure to continue carrying over a remainder to still-lower levels.⁹¹ The resolution, 1.5, is perfectly adapted to the pressure applied by the problem of dividing 3 by 2. (It replicates no matter how many times you check.)

A slightly different resolution can be had by referring to convention: $2 / 3$, for example, can be “rounded” at any level. According to the needs of the moment—which,

(see e.g., Deacon, 2003, p. 92; 2005, pp. 110, 113). So do the reaction ranges. Indeed, that’s the importance of “plasticity” for evolution (West-Eberhard, 2003). Put in terms of the New Theory, plasticity constrains the chain of relevance so that structural changes can accumulate without immediate functional effect. Note, though, that this also in turn implies that most adaptations are in fact “exaptations” (Gould, 1991; Gould & Vrba, 1982).

⁹¹ 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.

referring to Piaget's ultimate turn to "relevance" in seeking a logic of meanings, we now might refer to as the *relevance relation* afforded by context (Piaget & Garcia, 1987/1991)—a different response will provide the sought-after resolution: 0.7 vs 0.67 vs 0.667. All are conventionally-equivalent; again, "isomorphic."

Note, though, that *any* resolution provides a *response* regardless of whether the "resolution" provided is actually "correct" (i.e., "the solution"). Indeed, from this perspective, corrections must come from outside the system in order for the appropriate adaptation to occur: feedback from the teacher to the student, or further selection pressure from the organism's immediate environment. For students, though, this in turn affords a further teachable moment: highlighting the original problem's reversibility— $1.5 * 2 = 3$ —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. But for most organisms, the further interaction is usually not so "constructive." (Mostly, they die.)

In any case, from this perspective, Piaget's arguments do not seem at all controversial. Of course the first level of response is behavioral, and of course the second level is learning. (That's the Baldwin Effect!) His proposal is simply that we carry this insight down through the hierarchy of levels that his colleagues in adjacent disciplines—Gödel, Beth, Ladrière, Waddington, and Weiss—had talked about.

This is why behavior is the motor of evolution. For evolution to occur, populations must to encounter perturbations that have no resolution at any higher level. For the distribution of genes to change, they must be exposed to sufficient selection

pressures to kill off a significant proportion of their group's membership. (By definition.)

But that same observation doesn't apply to adaptation. That occurs at a higher level.

(Also, now, by definition.)

By parsing Piaget's meaning, we also then make sense of other comments from 1975 that—in a reversal of the common understanding from half-a-century earlier—seem to turn his earlier sympathy to Haeckelian recapitulationism on its head:

I believe, with Baldwin and with Freud as well, that the child is more primitive than *any* adult, including prehistoric man, and that the source of knowledge lies in ontogenesis. 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. (in J.-C. Bringuier, 1977/1980, p. 92)

This is quite a change. And it's an important one to note explicitly, given how Piaget's otherwise apparently Haeckelian tendencies have been misunderstood (pace Oosterdiekhoff, 2012, 2013).

To be clear: from this perspective, 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. Those changes may simply not be conserved: sometimes, the lessons learned are forgotten.

This all now seems consistent with contemporary writings on developmental

systems (see esp. Oyama, 2000a, 2000b) and developmental plasticity (see esp. West-Eberhard, 2003). But there's also an important epistemological insight here, with relevance to the doing of history.

It seems Piaget encountered in his biology a variation on the problem he encountered with his logic: he didn't make his case "properly," according to the conventions of the disciplinary sandbox in which he was playing. He was a stranger in a strange land; a "foreigner," whose contributions were then "neglected" (see Note 84; also Chapter 4). Indeed, it seems safe now to conclude that the inspiration he found during these interdisciplinary travels—which then in turn informed his psychological works—is why psychologists have had such trouble understanding them. In short, this is why there is an "unknown Piaget" (M. Chapman, 1988a, pp. 2-6).

6. Beyond genesis

It would be premature, given the direction of contemporary research, to end our story there. Indeed, Waddington and Weiss did more than provide an "existence proof" of the formal levels that Piaget used to ground his later works on reflecting abstraction, generalization, correspondence, etc. They also provided the means to incorporate the social, an important aspect of Piaget's theory that is still not well-understood.

Waddington explained, referring to his own illustration giving a level to each of genetics, natural selection, epigenetics, and behavior:

The four-membered evolutionary system... must, of course, continue to operate in the human species, as in every other. However, the salient feature about man—perhaps one might say that it is his defining characteristic—is that he has

developed, to an enormously higher degree than is found in any other species, a method of passing information from one generation to the next which is alternative to the biological mechanism depending on genes. This human information-transmitting system is, of course, the process of social learning.

(Waddington, 1961b, p. 70)

In Waddington's presentation, however, cultural evolution was for humans separate from biological evolution: "a second evolutionary system superimposed on top of the biological one, and functioning by means of a different system of information transmission" (Waddington, 1961b, p. 70).

It is important to note that this did not anticipate the later neo-Darwinian "memes as idea viruses" proposal (discussed by Burman, 2012d). Citing Piaget explicitly, in a later essay, Waddington instead attempted to explain how teaching and learning—not ideas themselves as carriers of inherent meaning—might be conceptualized in a biologically-coherent fashion:

We need to consider carefully the requirements for any process which will make it possible for information to be transmitted from one generation to the next by teaching and learning. For such a process to be effective, it is not only necessary for a language to be developed in which the information can be expressed, but it is essential that the recipient should be brought into a frame of mind in which he is prepared to receive the information which is transmitted to him (Waddington, 1962, p. 111)

He continued, building upon the results of experiments informed by Piaget's standard

theory to present a new argument for how such receptivity develops:

The human infant soon after birth seems not to be able to distinguish between itself and its surroundings. It exists, apparently, in a truly solipsistic universe in which it is the world and the world is part of it. The development of a mentality which is prepared to receive information transmitted from outside by language demands, of course, the breakdown of this solipsistic unity. The readiness to receive a transmitted message implies that there appears within the infant's mind some psychological system which carries the authority which is necessary for the information not only to be taken in, but to be allowed to have meaning.

(Waddington, 1962, p. 112)

In other words, Waddington argued that it is this acceptance of authority—the source of extant conventions—which allows children to develop their thinking: it allows higher levels to correct the incompletenesses that must necessarily exist at lower levels.

This is consistent with Piaget's (1935) much earlier reference to Claparède's "*instinct du conforme*" in a discussion of theories of imitation (p. 1). Indeed, his discussions of authority lead in a similar direction (e.g., Piaget, 1932/1932, 1933/1995). But recognizing the implications of the neo-Gödelian turn, we can now push this somewhat further. For example: we can suggest that the level of adults "completes" the level of children, albeit imperfectly. (The reasons imposed by grown-ups cannot themselves be faultless, and aren't even always good, but they're almost always imposed

anyway.)⁹² Of course, we also see the possibility for children to construct their own social norms through interactions with each other (cf. Piaget, 1932/1932). And we see that, inevitably, new adults—adolescents—will rebel against the tyranny of the older generation’s “just good enough” as further cognitive development constructs the tools to identify the incompletenesses of the adult world (cf. Inhelder & Piaget, 1955/1958). This then becomes either a fundamental motivator—the impetus to use behavior as a motor for idealist change (i.e., to overcome those aspects of the world that are deemed absolutely intolerable)—or it becomes a powerful justification for apathy and cynicism (Burman, 2008c).

As Waddington implies, however, it’s not the simple existence of authority itself that’s most interesting. It’s rather what its role in “completing” implies about human nature.

The argument which I wish to advance is that the authority which is required to make possible the socio-genetic method of transmitting information, and the authority which is involved in the development of the ideas of ethical good and bad, are two aspects of one and the same type of mental functioning.... If this is the case, the fact that man is the sort of creature who goes in for having ideas right and wrong is an essential part of the same mechanism which makes it possible for

⁹² At around the same time as the original version of this chapter was accepted for publication, a new book was published by Paul P. L. Harris (2012) that develops an important set of related ideas. Yet he framed his contribution in explicit contrast with Piaget’s (p. 5). This, however, is useful to us: because he cites only three early works, published originally in 1923-1937, the dismissal serves to highlight the contemporary value of this sort of history. Indeed, in his most recent discussion of Piaget and Baldwin, he repeated the old “Piaget neglected the social” trope that was addressed in Chapter 3 (see Morgan & Harris, 2015). Here, however, we pursue a different approach of the same “neglected” material. When then find that the neglect is ours.

him to transmit information by teaching and learning. (Waddington, 1962, pp. 112-113)

This argument is developed in greater detail in *The Ethical Animal* (Waddington, 1960) and in his later Gifford Lectures (Waddington, 1972a, 1972b, 1973a, 1973b). But it is the coincidence of Piaget's appeals to Waddington's and Weiss' presentations of biological levels with the neo-Godelian turn in Piaget's use of formalisms in defining levels that makes it especially important to us in terms of understanding the changes that occurred subsequently in the New Theory.

From this perspective, we can borrow Waddington's illustration of the levels in his "four-membered evolutionary system" and then "read in" to the New Theory an augmented version that takes into account his subsequent addition of a higher set of social levels (see Figure 2). We then see that the meaning of each individual's behavior is situated in a context defined in part by their social groupings' ideals and criteria for membership. We also see that, even granting that biological change and social change are partially uncoupled, each grouping's possibilities are constrained by the past decisions made by its members; that groupings inherit the histories constructed by their members' past actions, and that choice is constrained by those inheritances, which we might call norms. These norms then provide the socio-ecological milieu into which each new generation develops. And their effects are transmitted, as perturbations, to affect not only the development but also the stress levels—and perhaps even the survival rates—of those children who inherit them.

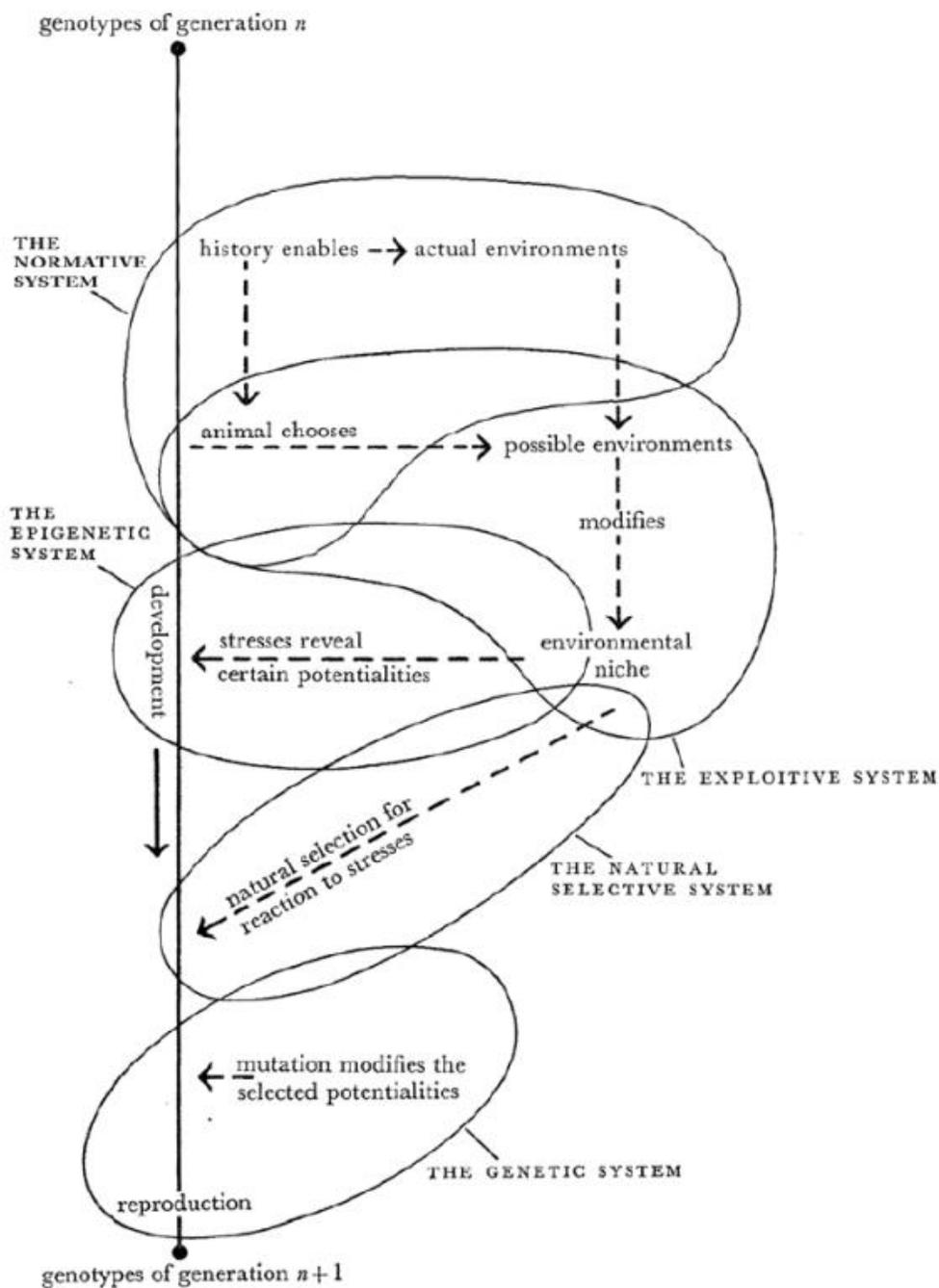


Figure 11. My accommodation of Waddington's (1959c, 1959b) four-membered evolutionary illustration adds his new top-most level, which I have here called "the normative system" (following Piaget's reply to Derrida at the Cerisy conference of

1959 [in Piaget, 1965a, p. 49]). This extends Waddington's earlier schematic to include the interactions that he describes in subsequent writings, and in a way that is consistent with Piaget's use of related material. Individual choice, here, is therefore not the primary cause of stress. Instead, contrary to what Waddington originally thought, that role is given over to history: the inherited effects of all past choices, which make certain futures possible and others not (cf. Hackman, Farah, & Meaney, 2010). Choices in the present then also shape possible futures, which are in turn inherited by later generations. Thus: the present is inherited (necessary), while the future is constructed (possible). Adapted by permission from Macmillan Publishers Ltd: *Nature*, 183, p. 1638, 1959 (license # 3944080117221). Reprinted with permission from Burman, *New Ideas in Psychology*, 31(3), p. 369, 2012 (license # 3942341238615).

7. Conclusion

I showed, in Chapter V, that Piaget's later works were informed by the recognition of a formal hierarchy of levels. This, therefore, is a further extension of those efforts; a second step along the bridge to Piaget's New Theory, but excavating some of his appeals to sources in Biology rather than to those in Logic. As a result, we see that Piaget's later biological writings—and his updating of the Baldwin Effect—can be tied to a larger formal discourse on the genesis of the logic that informs children's reasoning. Here, though, psychological development becomes a kind of extended embryology: development outside the womb, bathed in the child's local social ecology.

Yet what, after this, does *development* mean? In short: it is the process whereby the organism becomes sensitive to stimuli, and which shapes the responses it gives in response. This then recasts his old intelligence-testing work in new light. As Piaget explained in his 1971 address to the first meeting of the American society named in his honor:

The organism is sensitive to a given stimulus only when it possesses a certain competence. I am borrowing this word from embryology in the sense in which Waddington has used it. He has referred to the influence of an inductor.

Waddington has shown that an inductor which modifies the structure of the embryo does not act in the same way at all levels of development. If the inductor is present before the embryo has the competence to respond to it, the inductor will have no effect at all; thus, it will not modify the structure. The embryo must be at a point of being competent to respond to the inductor before the inductor can have

its effect. (Piaget, 1972, p. 5; cf. Piaget, 1967/1971, p. 22)

In other words, what *development* is—from this perspective—is the process not just by which the organism becomes responsive to external causes, but it is also the means for that organism to access more than the simplest forms of *linear causality*.

He continued, framing this proposal in terms that would be more understandable to the primarily English-speaking and Behaviorist-thinking audience:

Stimulus-response is not a one-way road, unilateral scheme. A subject is sensitive to a stimulus only when he possesses the scheme [structure] which will permit the response. In other words, the sensitivity to the stimulus is the capacity for response, and this capacity for response supposes a scheme [structure] of assimilation. We again have to create an equilibrium between assimilation, on the one hand, and accommodation to a given or an external stimulus, on the other hand. (Piaget, 1972, pp. 5-6)

Then he returned to the problem of genesis that he inherited from Baldwin: “The relationship can also be described as circular which again poses the problem of equilibrium, an equilibrium between external information serving as the stimulus and the subject’s schemes or internal structure of his activities” (Piaget, 1972, p. 6; see also Piaget, 1982b).

From there, his solution was then to appeal to the neo-Gödelian hierarchy, but with constant feedback added: dialectical change across levels, coupled with periods of discursive change within levels (see Piaget, 1971/1977, 1980b). All that said, however, it is not correct, strictly speaking, to say that Piaget *intended* to update the Baldwin Effect

as a means to ground the New Theory. This would put the cart before the horse. Rather, Piaget simply appealed to the then-contemporary works that updated the ideas upon which his old developmental framework had been built: he returned to his roots, following an intriguing possibility suggested by a colleague, and then—reinvigorated—he reworked his old ideas to fit with how biological knowledge had changed in the intervening years.

Because Piaget's standard theory had been based on the very same idea which underlies the Baldwin Effect (viz. the circular reaction), his later use of biological ideas updating that notion (e.g., Waddington's and Weiss' use of cybernetic feedback across levels) simply replaced Baldwin's role in his work. It is then in that capacity that we can say that the Baldwin Effect was updated. *We can read it out of Piaget's transitional writings.* But to do so, we need to see through historical lenses; we must ignore the distortions due to his translators' errors—as well as his own infelicitous word choices—and instead dig deeper to find our own understanding in Piaget's sources and arguments. In so doing, we then find the means to reinterpret Piaget's problematic writings on phenocopy (and indeed all of his discussions of interactions across levels) in terms that are more consistent with contemporary attempts to synthesize the new Biology with Psychology. We also see how the social finds its place in that proposal.

In a talk that I had the pleasure to attend, Michael Meaney (2009) referred to such efforts as illustrating “where nature meets nurture.” Yet, in pursuing this thought, contemporary psychologists typically turn for insight to the works of Gilbert Gottlieb (1992, 1999, 2007; see esp. Logan & Lickliter, 2007). In introducing this alternative

lineage and its implications, however, I believe we are also justified in examining Piaget's older sources as untapped resources. This, then, is also consistent with how biologists have come to treat those older sources as well (see e.g., Drack, Apfalter, & Pouvreau, 2007; Drack & Wolkenhauer, 2011; B. K. Hall & Laubichler, 2008).

In taking on this task, we come to an appreciation of the New Theory as being more than “a logical hermeneutics of action” (Beilin, 1992b, pp. 1, 6). It is also more complex than the picture afforded by the neo-Piagetian “dialectical Piaget” (so called by Morra et al., 2008, p. 6). Instead, we see that Piaget's standard theory has been updated to include both epigenesis and a formally-supported version of its associated hierarchy of levels. Elsewhere, I have therefore proposed that the New Theory merits being called by a new name that more adequately reflects these additions: “epigenetic epistemology” (Burman, 2007a, 2008b, 2009b, 2009c, 2013b). This then has the double benefit of referencing the broadest form of Piaget's standard theory, genetic epistemology, while at the same time highlighting his debt in the new theory to Waddington, Weiss, and others whose works are now being rediscovered by contemporary biologists. Thus, as ideas regarding epigenetics—past and present—become increasingly popular, perhaps the interest reflected by the proposed name will lead to a new reading of those most-promising later works by Piaget that we least understand.

CONCLUSION

PRIOR TO THIS PROJECT, the start of the New Theory had been dated to 1974-1975. This was marked by the publication of *Grasp of Consciousness, Success and Understanding*, and *Experiments in Contradiction*, as well as an important project led by Piaget's chief collaborator—Bärbel Inhelder—on development and learning (Inhelder, Sinclair, & Bovet, 1974/1974; Piaget, 1974/1976, 1974/1978, 1974/1980b). These experimental results were then summarized in *Equilibration of Cognitive Structures* (Piaget, 1975/1985). Yet these of course also all have dates when work began, and those dates then push the start of the New Theory back into at least the late-1960s.

That said, however, we now know from detailed archival study that the situation was much more complex than had been thought previously (by e.g., Burman, 2008b). The first books of the New Theory were preceded immediately by a series of unpublished studies, dating from the mid-1960s, which reported the results of over a hundred psychological experiments on the child's developing representation of causal relations (Burman & Ratcliff, 2015; Ratcliff & Burman, 2015). These were introduced in print in a short first volume, but they were also clearly intended to be published in full. As Piaget said explicitly: "We are in possession of about a hundred studies, completed and written

up, which we expect to divide into separate small books bearing on the essential points of causal explanation” (in Piaget & Garcia, 1971/1974, p. x). But many of these studies were withheld for now-unknown reasons.

I have since found three complete books, first handwritten by Piaget and then typed, returned, and corrected—each with a clear narrative arc, properly introduced and having a formal conclusion—as well as what looks like a partially-completed fourth book. I suspect that these experiments were intended to test the move by biologists toward models of non-linear causation, and that it was the complexity of the resulting discussion—including talk of “morphisms” (functional similarities across levels despite structural differences)—that led to their being shelved (Burman, 2016).

Indeed, on the basis of this archival work, I now suspect that the works of the New Theory can be understood as attempts to explain, simplify, and extend those earlier unpublished experimentally-derived insights. If this suspicion is correct—that they all relate to an attempt to discover the emergence of different “cybernetic circuits” at increasingly-powerful levels in both cognitive development and the evolution of knowledge itself—then the turn toward the New Theory can be pushed back directly to the conference in 1964 when Waddington visited Geneva (Chapter VI).

Of course, because I have discussed the importance of context for understanding meaning (from several perspectives across multiple chapters), it is clear that Waddington’s comments were themselves interpreted through a particular lens: an updated logical framework—formalizing the new hierarchy of levels as a replacement for the foundation underneath stages (Chapter V)—which itself has multiple names and dates

associated with it. Moving backward, these include: Ladrière in around 1960, Gödel in around 1958, Beth in around 1956, and many others besides (see Ratcliff, in press-a, in press-b). Piaget’s visiting fellowship in 1954 at Gödel’s home institution, the Institute for Advanced Study in Princeton, is also potentially implicated. So is Beth’s critique of 1950 that led to the collaboration that enabled Ladrière’s later contributions to be received in a meaningful way (Chapter V).

These names and dates form a lineage that extends back decades prior to 1974-1975. Some even extend back before the founding of Piaget’s International Center for Genetic Epistemology in 1955 (see Appendix A). But it’s also not the names and dates themselves that matter; it’s what they imply. (It’s what they *mean*, and what *we can do* as a result.) But how far back should we go?

If some of my peer reviewers had their way, the emergence of the New Theory would be traced right back to Piaget’s autobiographical novel of 1918. Although absurd to me, this seems justified for them because that is the first explicit discussion of “equilibration” in Piaget’s writings (see e.g., Boom, 2009, p. 135; M. Chapman, 1992, pp. 40-41; Vonèche, 1992/1996). But this suggestion has never made sense to me because the meaning of equilibration has not been stable. Indeed, I have shown that to be the case (esp. Chapter V & Chapter VI).

Granted, the insistence on equilibration as a defining feature of Piaget’s theory can perhaps be attributed to a difference in disciplinary sensibilities—between History and Theory—regarding the treatment of “continuity” (see Teo, 2013a, p. 842). If that is correct, then where the focus falls in a discussion is what signposts the disciplinary norms

to be applied in judging the associated arguments. And so, from this perspective, I certainly do understand the Theoretical desire to focus on the apparently-continuous thread that extends back from equilibration to the circular reaction and thus also to Baldwin. Still, one reviewer went well beyond this interest: in their letter accompanying the editor's rejection of an earlier version of Chapter V, they suggested that the fact of equilibration having been included in Piaget's autobiographical novel of 1918 might itself justify calling Piaget the original and unrecognized inventor of the idea of incompleteness that later made Gödel famous.

This, to me, is a ridiculous claim. The impact of incompleteness on Piaget's thinking is traceable directly. There is also no evidence of any impact of Piaget on Gödel's thinking in this regard. Indeed, given what's accessible in the relevant archives, no Historian could ever suggest anything of the sort. Still, if such a claim was possible for an otherwise-respected colleague to make, we need not stop with Piaget. Following the spirit of their suggestion, we might even suggest that the reviewer was wrong about who should receive the credit: Piaget clearly owed Baldwin a debt, especially in his early works that relied on concepts related to "genesis" that Baldwin introduced as part of his proposals regarding Functional Selection (Chapter VI).

Pursuing this idea then finds us running headlong into Baldwin's "genetic logic" (see esp. Baldwin, 1906; 1908, 1911; also 1915). And this work is indeed similar, if considered loosely, to some of Piaget's later writings about biology and logic. (Otherwise, I could not have suggested that Piaget updated the Baldwin Effect.) Yet we can also then expect that Baldwin's proposals would have been received and dismissed in

the same way as were Piaget's, for the same reasons. In short: even if incompleteness had been invented earlier than the version in which Gödel presented it, we have no evidence of such a thing having been presented in a way that would allow the contribution to be received by the relevant community. That is therefore not the impact we seek to explain, put in context, and make useful. We are, in short, not seeking "firsts."

1. And thus, "new"

From this perspective, "new" is not an historical object in the same way that a "first" is. Rather, "new" is a name; an attribution. And this is actually consistent with what my colleagues in Geneva have said of my insistence on pursuing the New Theory as the focus of my research. Because, to them, that phrase itself has no essential meaning. And I accept that, in no small part because I accept both the fact of change and the textual evidence of Piaget having described himself as one of his own "chief revisionists" (Piaget, 1968/1970, p. 703n2). But that doesn't mean the New Theory isn't "real," in the sense that it could have an impact that's "new" to an important audience. Piaget's standard ideas are still used; thus, their update would be functionally relevant (cf. Chapter VI).

Yes, the view of Piaget is different in English. I showed this conclusively in Chapter IV. The differences in interpretation between different language groups are also then consistent with the literature on indigenization: different books have different meanings for different groups. (Their contents weren't *copied*; the same words were *reconstructed* in different contexts, thus having different meanings and different impacts.) And it also reflects a similar kind of nominalism-problem that Piaget

encountered with the intersection between his biology and his logic: there's something underneath the name chosen, and that's ultimately what is most important for the communicating author (Chapter II, Chapter V, & Chapter VI). Yet recognizing that the author's intent and the audience's understanding does not force a retreat to relativism. Indeed, from an historical point of view, it's incredibly useful: we can look for evidence of similarities and differences (according to your disciplinary preference).

If you go looking for "Piaget's new theory" in the secondary literature, the most obviously relevant texts are by Harry Beilin (1921-2007). He published a well-known and -cited article situating the later works relative to what he called Piaget's "enduring contributions to developmental psychology" (by which he meant the sub-discipline of American Psychology devoted to studying the changes in cognition between birth and adulthood). He then discussed the output of Piaget's last years in greater depth in a harder-to-find chapter from the Jean Piaget Society Symposium Series, itself directly entitled "Piaget's new theory" (Beilin, 1992a, 1992b).

Digging deeper, we see that it was also Beilin who made the distinction between "the new theory" and "the standard theory" (Beilin, 1989b). And he was a founding member of the Jean Piaget Society itself. He then received their Lifetime Achievement Award in 2002. In other words, he was an "insider." And this can be problematic for histories: the view presented often comes with an agenda attached.

Granted, I am an insider too. This, though, is of a different kind: I work at the Piaget Archives in Geneva, and I was elected to serve on the Board of Directors of the same society that Beilin helped to found. But I am also an historian: although I first

trained as a psychologist at the University of Toronto (HonBSc 2004), my doctorate from York University is in the History and Theory of Psychology. In other words, I am trained to dig—and then dig deeper.

Thus: if we continue to delve into the secondary literature, after finding Beilin, we find Philip Minor Davidson (1951-2004). We then see that Davidson (1988) not only published his own relevant work that's directly related to what Beilin called "Piaget's new theory," but he also presented it at the Piaget Society before Beilin published his own papers (Davidson, 1984, 1986). He even co-edited the English translation of Piaget's important second-to-last book: *Toward a Logic of Meanings* (Piaget & Garcia, 1987/1991). And he continued to write about related issues until a long illness brought an early end to both his career and his life (Davidson, 1987, 1992, 1993).

Digging still deeper, however, we find that Beilin (1980, 1983) was already heading in this direction, and had even already used the phrase "new theory" (Beilin, 1986, p. 117). He also commented on Davidson's most directly-relevant article (Beilin, 1988). And he presented his own earlier talks on the topic at relevant societies, including the Society for Research in Child Development (Beilin, 1989a). So while we can point to the meetings of these associations—the Piaget Society and SRCD—as being important locations for interaction between these two popularizers of the idea that there is a "new theory," we cannot appeal to such a review to resolve any possible priority dispute: there is no "first" here. Rather, it seems these authors developed their ideas together (cf. Beilin, 1980, 1985).

2. A primary source

Examining the references in some of Beilin's earliest work on the subject, we find another possible source not cited in his later works: an English translation of a text by Piaget (1977b) that explicitly includes—three times—the phrase “new theory” (in the title, and on pp. 350, 351). This then appears, on its face, to be a smoking gun: a primary source written by the originating author of the material under discussion in the secondary literature, which also includes the term of interest. The objection by my Genevan colleagues, and the concern regarding relativism and competing perspectives, can then be countered: in history, textual evidence trumps all.

The article in question is the published version of a speech that Piaget had intended to deliver at the New York Academy of Sciences. He was unable to attend, however, and so Bärbel Inhelder gave the talk in his place. But it was not she who translated the version of the speech that Beilin cited. And that, as it turns out, matters more than anything else in understanding the fact of the New Theory as being a name referring to something that may or may not itself be new.

First, though: the contents of the speech. We can go from there to the more complicated question of whether and how, or why, the material presented was actually new.

The paper is part of the project on “possibilities” that began in 1975, ran for two years, and resulted in the posthumous publication of two books in the early-1980s (see Piaget, 2004/2006, p. 2; also 1981/1987; 1983/1987). In this case, however, the specific issue under consideration was the “groupements” that had provided the original

foundation for stage theory. That is then how this speech relates to the new theory: it presents a redefinition of the concept of grouping, which I explained earlier by making reference to Piaget's background in biology.

That said, however, this is not itself a purely theoretical paper. Instead, Piaget constructed his argument by reporting the results of a series of experiments: one examining the number of combinations that children provide when given the instruction to arrange some blocks in all of the random ways possible, another on how children respond when asked to cut up a piece of paper in all of the ways possible, etc. The results then reflect the same developmental sequence: different age-cohorts of children respond in different ways that nonetheless reflect a single constructive lineage.

For example: young children, aged four to six, present a half-dozen or so possibilities with little variability. (These objects can be arranged only in these ways, or the paper cut only thusly.) But older children, aged seven to eleven, provide an increasingly large number of ways of presenting the given options until around age twelve. After that age, though, the children give up after only a few attempts and conclude that the possibilities are infinite. Most important: they explain why it must be so.

This new research, Piaget explained, built on work from 1974-1975 on the notion of "morphisms" that I saw mentioned in the unpublished manuscripts and which were later expanded-upon in *Morphisms and Categories* (Piaget et al., 1990/1992). This in turn reflected a shift away from modeling operations on geometrical transformations toward considering them as comparisons. The objects examined are then not themselves altered,

either physically or in their arrangement, but instead have features abstracted from them such that analogies can be presented.

Simplifying somewhat from Piaget's later discussion, this becomes: *A is to B as B is to C* (a comparison), *because x, y, z* (a justification derived from an abstraction related to the comparison). The similarities between the objects compared then become akin to family relations, and these relations can themselves be abstracted to form new operational structures to be applied more generally. Thus: *the fact of A being the cousin of B, and B being the cousin of C, implies that A is also the cousin of C—not only by the transitive property of equality, but also because the meaning of “cousin” enables a vicariant comparison to any similarly-defined relation within that grouping* (see also Piaget et al., 1990/1992, pp. 45n, 54-57, 91-109, 224-226). Children can then use the *meaning* without understanding the underlying formalism, and they can explain it in a way that is consistent with that formalism even though they aren't aware that such a formalism could be used to simplify things further. The two resulting explanations are functionally equivalent, but exist at different levels.

In this way, Piaget explained, abstractions and comparisons both prepare the way for the operations examined in earlier works and reflect their functioning. The formalism sits at a *higher* level of abstraction than the meaning, which in turn sits at a higher level than the family relations (which are higher than the objects represented). The means of getting access to these higher levels is then circular. But that isn't a problem for functional selection, which assumes it (Chapter VI).

Unfortunately, it's not clear how Piaget derived his results. He didn't explain this

clearly in the speech, but we can add it here. Briefly, then: the inference of the existence of an operational structure at one level or another relies on the observer asking for the child's justification. If there is no description of cousin-relations, and especially of the *necessity* that this imparts to the compared-objects as a result of what "cousin" *means*, then the operational structure can't be inferred to exist despite the child's use of family comparisons. It is therefore the justification that operationalizes *knowledge*, for Piaget: *knowing why*, and not just *knowing that*. (Note, too, that this is different from the usual comparison between *knowing that* and *knowing how* [see Inhelder & Piaget, 1979/1980].)⁹³

From there, Piaget turns to discuss "groupings" directly. And that's where we find the source of the key phrase, "new theory," in the text. To delve into this further, however, we need to have a look at the French original. Although it's great to be working from any sort of primary source text, we need to locate ourselves in the unadulterated version because we have a reasonable suspicion that changes may have occurred.

3. French original

A footnote on the front page of the English version indicates that the French original would be published in the *Bulletin de psychologie* as part of a special issue celebrating an important colleague and collaborator of Piaget's (viz. Paul Fraise [see Appendix A]). But this issue never appeared. However, a chapter with the right title was indeed published in a book dedicated to that same colleague. Examining the text itself

⁹³ This difference is probably not coincidental: Gilbert Ryle served on the advisory board of the International Center for Genetic Epistemology (see Burman, 2012c, p. 284).

then raises a problem: the contents are different, at least initially.

The original does not include any of the material discussed until just after the introduction of the phrase “new theory” in the English version: there is none of the talk about the possibility project, its experiments, or the importance of the family relations mentioned above. (It seems that these were added solely for the New York audience.) Instead, the original jumps right in to a discussion of increasingly difficult formalisms. After that, though, the translation and the original overlap almost exactly.

Piaget (1977a) recognizes that his earlier attempts at logic were decried by logicians for their “inelegance” (p. 53), as he put it, but he explains that what he had intended by “groupings” was always meant to reflect the classification of species introduced by *Linné* (which is also the name given by the translator, but whom English audiences would know better as “Linnaeus”). The change in his theory—which the original refers to in French as “new formulations,” rather than “the new theory”—is then related to this.

Here, though, the earlier species-definition of stages is updated to take advantage of new axiomatizations by logicians. He cites these as showing that species-groupings are the equivalent of “categories” in the form provided by McLane and Eilenberg (discussed in Piaget et al., 1990/1992). Piaget then says that a category, in this sense, is what he meant by “morphism.” Two levels can thus be functionally isomorphic—their effects are interchangeable, like members of the same species—even if their structures are different.

This makes no sense from the point of view of neo-Darwinian biology. But remember, Piaget follows a different model. For him, two organisms can be

physiologically indistinguishable as a result of higher-level changes unrelated to change at the level of the genes. And it's this that allows him to proceed.

Piaget describes several examples of categories, including their formal definitions. These don't concern us in their details, but something here certainly does: the translator made an important mistake. Rather than rendering *les vicariances* as the category of "vicariances," this instead becomes "vicarious relationships." And that's something entirely different.

Vicariance was discussed briefly in Chapter V in order to reconnect the logical meaning used by Piaget with the biological meaning that is more easily understood today. (Recall that species are called "vicariant" because all members are intra-translatable within the grouping: the members of the grouping can make babies that can make babies.) Replacing this with "vicarious" therefore has a strange effect. Rather than being functionally identical with each other, and thus replaceable as parts relative to the whole, the members of a "vicarious" grouping become merely similar. They "feel" alike (cf. Burman, 2012b, 2014). But that's properly a *side-effect* of what Piaget meant (see e.g., Piaget, 1954/1981). And thus the translation loses the strength of the original formalism.

Inhelder's (1977) own speech at the same conference provides some additional detail, but has the benefit of having been translated by someone else. She thus refers to vicariance—rendered correctly—to explain the construction of the level-jump that has since come to be called *reflecting abstraction*:

How does the child build up these structures, and, especially, how, around the age of 11 or 12, does he attain a logical structure that goes beyond that of classes and

relations, beyond the double-entry matrices of the concrete operations period?

Piaget calls the mechanism by which this is achieved “reflexive abstraction.” [sic]

Such abstraction takes place when the subject derives from his actions and operations certain principles that lead to a new organization when he is confronted with a new problem. For example, at the level of concrete operations, the child is already capable of substituting one criterium for another (shifting), and of conserving the whole in whatever way it has been divided into parts (Inhelder and Piaget, 1964). This is a manifestation of what has been called “vicariance”.

Subsequently, by reflexive abstraction, this concept leads to the idea of a division of one and the same totality into all its possible parts. Through this abstraction, which is accompanied by a constructive generalization, the subject reaches the logicomathematical concept of the set of all subsets. (Inhelder, 1977, p. 336)

This makes the main theoretical issue of Piagetian later constructivism clear, and ties everything together: abstractions and generalizations occur through a process similar to Baldwin’s Functional Selection, but across and within neo-Gödelian levels. My illustration in Chapter VI makes this clear, but the resulting meaning is worth reiterating: lower levels constrain higher levels, and are constrained by them, such that possibility and necessity interact with exploration in the construction of novelty.

From this perspective, Piaget’s final turn away from extensional logics and toward relevant logics must be interpreted through a much deeper context. As Beilin (1992b) explained:

When Piaget (1980[c]) declared he had previously been in error in placing almost

exclusive stress on extensional, truth-table logic and that a more balanced theory of meaning was needed giving greater attention to intensional logic [see esp. Piaget & Garcia, 1987/1991], it appeared as though a radical and sudden transformation had occurred in his theory.^[94] It was indeed a radical change, but it was not occasioned by a sudden shift in the direction that Piaget's theory had been taking in the 10 years prior to that time. (p. 1)

My main contribution, then, is to push back the date by placing those changes in context. And I'd like to push that just a little bit further.

4. Translator's intent?

Nowhere in the French original did Piaget use the words "new theory." We also cannot ask the translator, Gilbert Voyat, what he intended by introducing the term. This is because he died soon after the translation was published, and his archival papers have not yet been found. But I would like to suggest, perhaps controversially, that his choice of labels was quite intentional. This is because he was an insider—a Genevan—who had decamped for the US and, I'm told, had intended to become Piaget's local representative in New York.

There are clues to this in his writings at the time. For example, in an article

⁹⁴ The difference between extensional logic and intensional logic is akin to the difference between *the fact of an object's existence* (the thing in itself) and *what that object implies* (as epistemic phenomenon). Truth tables can therefore summarize the logical transformations that an object can undergo, but cannot represent what those transformations imply. Also: since self-contradiction cannot be modeled in this way by a truth table, but can indeed be modeled as an implication, the logic of intension (which Piaget referred to as "a logic of meanings") is more applicable to his problem. Piaget's shift to the new theory can therefore be understood as a shift toward a kind of phenomenology, but one *caused by* the formal properties of the structures of the mind. This then also parallels Godel's shift toward Husserl in his own later works (see e.g., Tieszen, 1992; 2012; van Atten & Kennedy, 2003; also van Atten, 2006).

describing Piaget's impact in the US, Voyat (1977) talked about the "subtle distortion of Piaget's original aims" (p. 346). He then published a simplifying introductory textbook (Voyat, 1982). Yet he also identified an opportunity:

The intensity and breadth of the interest aroused [among teachers] in America by the findings of the Geneva school are truly remarkable. A host of individual teachers and the many "new schools" (sometimes called "Piaget schools") all claim to base their educational epistemology on operational theory. One might well ask how such enthusiasm is possible when the mainstream of American pedagogy is still in thrall to a militant and vigilant behaviorism. Part of the answer to this question is that the demands which American education has to meet on the pragmatic level tend to maintain the system in a state of permanent instability. Another point worth mentioning is the complementarity and resemblance between Piaget's approach and Dewey's, which continues to underpin a dissenting tradition in educational thinking in America. (Voyat, 1977, p. 346)

In other words, we can suggest that it was *teachers*—rather than psychologists—whom Voyat sought to target. But why? He explained directly:

an increasing number of educationalists find in this theory exactly what they are looking for, namely, a theoretical framework and the knowledge needed for the organization of their projected applications. (p. 346)

Still, though, there was a barrier in the way:

No unifying tendency is visible here save for the explicit invocation of Piaget's thought, and what we see is actually a large number of distinct trends with no

predominant orientation emerging for the time being. (p. 347)

Thus, Voyat saw a problem that could be addressed by taking advantage of the disparate groups' joint and collective allegiance to Piaget's name.

From this perspective, what would a unifying view need to do? It would need to adopt a high-level epistemological view, in the sense that it would need to be relevant *both* to teachers and to researchers in psychology and cognitive science who are seeking to understand the development of knowledge (p. 347). It would also need to reconnect with Piaget's biology in a way that would be acceptable, given the American "suspicion" of "disguised Lamarckism" (p. 348). And, most important of all, it would need to reincorporate interaction:

the concept of interaction is the one that remains the most mysterious to the American way of thinking. It is paradoxical to discover that this most vital element in operational [Piagetian] theory, in tandem with the notion of equilibration as an essential mechanism in cognitive development, remains the most alien of concepts for those schooled in the classical tradition of American psychology. (Voyat, 1977, p. 348)

I think, now, that this has been achieved simply by showing how Piaget came to present the ideas that Voyat led Beilin (and Davidson) to consider. Interaction is assumed of Functional Selection, and it is also required of the neo-Gödelian approach. That then also addresses the weird biological theory, and provides the means to see the development of knowledge through its various levels.

Still, there are some open questions. One relates to the interaction between Voyat

and Beilin. They were both based at CUNY. And thus there is the possibility for a local history that cannot be pursued without their archival and institutional papers. The timing of Voyat's labeling of the New Theory also coincides with the height of what has been called "Piaget bashing" (Chapter III). The choice of words could therefore easily have been a rhetorical strategy, related to the context of the time: a kind of literary sleight of hand to separate "the problematic Piaget being bashed" from "the Piaget who still has things to offer," even while knowing that they were the same person. Of course, it's hard to say this with certainty: we will only know once we have access to the relevant archival papers.

5. Final thoughts

The label—the "new" theory—is a term of art highlighting what is different in Piaget's later works from what American readers have come to expect. There is no great philosophical rupture at work, although there are of course many smaller historical ruptures to be found.

We are thus returned to the beginning, because it was in this sense that Beilin (1992b) explained the difference between the two, old and new:

Piaget's late additions extend the theory and at the same time conserve it. They extend the constructivist aspect of the theory by emphasizing the nearly unlimited possibilities in mind, and at the same time show the constraints that come from integrating the products of only some possibilities, and the procedures needed to actualize them. Thus, the developing rational mind is inherently productive, yet constrained, and we are reminded that the development of mind is subject

constantly to dialectical forces, the position that Piaget increasingly came to espouse in the later theory (p. 12).

In short, the “new” theory is more constructivist, more focused on possibility and necessity (i.e., on formal views of constraint), and—if such a thing were possible—more dialectical. It is consistent with the standard theory, but different enough to be of interest even to skeptics. And its differences included some elements that were very hard to understand without bringing back the missing context.

In the end, though, we can leave off quoting Inhelder in her introduction to Voyat’s interview of Piaget in February of 1980: “After this glimpse of recent work, I hope you will be convinced that Piagetian theory is not a closed system but is in a continual state of refinement and development” (in Voyat et al., 2011, p. 9). It is, in brief, a model of itself: changing, developing, and evolving. As he put it:

At all levels of development there are implications between actions or meanings; then there are dialectical relations that lead the subject to go beyond what he has already acquired. These spiralling [sic] constructions of a dialectical nature constitute what I have long considered to be the essence of the growth of knowledge. Development does not simply consist of ever new equilibrations, but of ‘augmentative’ equilibrations. (Piaget, 1980a, p. 6)

The ultimate lesson: one never finishes. The journey continues. And in terms of how this applies to my own research, I am looking forward to that journey with great anticipation.

APPENDIX A

Exploring the doing of history using levels

What follows is the pre-review manuscript of an article co-authored with Marc Ratcliff, my supervisor in Geneva. It was written in English by me, using notes and a short first draft written by him in French. And although it was his suggestion that we pursue the connection to Gödel that I outlined originally in Chapter V, it was my idea to have that inform an historiographical experiment. That then shaped the narrative arc, and I followed my standard method to fill in the details: “get out of the way, and let the story tell itself.” We discussed this second draft, and I revised it. The resulting manuscript has been submitted to *Estudios de Psicología*, following their invitation. Two versions will be published: the original in English (below), and a Spanish translation that the editors will arrange on our behalf. Where I have made translations of the French sources, the original text is provided in footnotes.

THE MOBILE FRONTIERS OF PIAGET'S PSYCHOLOGY

From academic tourism to interdisciplinary collaboration

“The frontiers of formalization are... mobile, or ‘vicariant,’ and are not closed once and for all like a wall marking the limits of an empire. J. Ladrière proposed the ingenious interpretation: ‘we cannot survey, at one glance, all the operations possible of thought....’” (Burman's [in press] trans of Piaget, 1968, p. 31; citing Ladrière, 1960, p. 321)

“...the epistemic framework of psychology in 1959 is not that of psychology in 1930; or even 1950. These new frameworks are also never imposed by outsiders, even should those outsiders be named Kant or Husserl. Instead, one day or another, insiders discover that they encounter a particular philosophy that suits them. On that day, they are brought together in greater unity. But to declare beforehand that what a psychologist does is not psychological, and exceeds the disciplinary frontier—I reject the premise” (our trans of Piaget, 1965a, p. 61).⁹⁵

A number of historians have shown that, early in his career, Jean Piaget (1896-

⁹⁵ In the original: “...les cadres de la psychologie de 1959 ne sont plus ceux de la psychologie de 1930 ou même de 1950. Mais ces cadres nouveaux ne sont jamais imposés du dehors par des non-techniciens, qu’ils s’appellent Kant ou Husserl. Qu’un jour ou l’autre on découvre que ces cadres rencontrent telle ou telle philosophie qui leur convient, ce jour il y aura davantage d’unité. Mais décréter d’avance que ce que fait le psychologue n’est pas psychologique et qu’il dépasse ses frontières, c’est un impératif que je refuse” (Piaget, 1965a, p. 61).

1980) drew heavily on multiple disciplines outside of what we now consider psychology. Whether psychoanalysis, mathematics, physics, biology, sociology, philosophy, anthropology or religion, the sources of his inspiration were many and varied (see e.g., M. Chapman, 1988a; Hofstetter et al., 2012; Vidal, 1994; Vonèche, 1992/1996). This is reflected clearly in the speeches he gave to various societies: until WWII, Piaget communicated the results of his work at meetings devoted to the discussion of psychology, philosophy, philosophy of science, biology, sociology, natural science, physics, and education. The spectrum of his publications was equally broad.

Given Piaget's wide-ranging interests, however, one might wonder about the disciplinary status of psychology in his professional circles in Geneva. Indeed, one might enquire specifically about its role and place at the Jean-Jacques Rousseau Institute for the Sciences of Education, which Piaget co-directed—starting in 1929—with founders Edouard Claparède (1873-1940) and Pierre Bovet (1878-1965). After all, it is via this institution that Piaget ultimately “got to psychology” after receiving a doctorate in natural history (Burman, 2015, p. 157; responding to Vidal, 1997b). And that is therefore what we examine in this article: the interaction between personal interest and institutional context, as well as the resulting implications in terms of possibilities for future action. Yet, at the same time, such an examination would also be too limited in scope. The institutional context itself had a context, both national and international, into which Piaget had to fit as well. We therefore attempt to go beyond the kind of institutional microhistory that is typical for such an examination, although we do so in a way that is perhaps a bit audacious for such a short piece.

Beyond presenting an interesting view of one of the key figures of developmental psychology, the essay's historiographical audacity derives from the identification of a change in the formalism underlying Piaget's theory of knowledge. Briefly put: the original theoretical foundation of his well-known stage theory of child development was replaced with an open hierarchy of levels (see Burman, in press). One consequence of this is reflected in the epigraph, and in our choice of title: the limits of what can be formally represented—which, in Piaget's later work, meant “the limits of what can be known”—aren't fixed. The effects of this realization can then be seen in Piaget's changing relationship with psychology as a scientific discipline, which we will discuss in detail. But the implications of that change also go much deeper, even informing his posthumously-published examination of the parallel between child development and the history of science (see Piaget & Garcia, 1983/1989; with commentary by Beilin, 1990; Kitchener, 1987). So, although unorthodox as “history,” we take the risk of having it inform our approach here too.

1. Our motivation and approach

Historians have argued since Leopold von Ranke (1795-1886) that everything that happens occurs in a specific context, and that this context must be reflected in the histories that are subsequently told.⁹⁶ Yet we might now reconsider this singular and amorphous “context” in terms of the identified hierarchy of levels: actions at one level are both constrained, and made possible, by the levels adjacent to it (see also Burman,

⁹⁶ The consequences of this are often referred-to as “the new history of psychology” (see esp. Furumoto, 1989; with recent discussion by Teo, 2013a).

2013b). Indeed, this seems now to have been the basis of Piaget's later turn toward "logics of meaning," in which he and his co-author argued that it is possible to model the effect that this "entailment" between functionally-interacting levels has on decision-making by taking note of relevance relations (Piaget, 1980c; Piaget & Garcia, 1987/1991; with commentary by Apostel, 1982; Ducret, 1988).

That said, however, the underlying insight—identifying levels, and then examining interactions across and between them—never really caught on specifically as an investigative tool.⁹⁷ We therefore attempt it here as a kind of historiographical experiment. In short, we aim to examine some of the levels and interactions in Piaget's own history in order to see his professional life in its contexts, but also to delve more deeply into an unexamined aspect of his broader theory as it existed at the end of his life. As a consequence, we also then come to a new appreciation of Piaget's relationship with his home discipline of psychology as its boundaries moved, and as he himself became increasingly involved in moving them.⁹⁸

We begin by examining the interaction between the two most basic levels implicated in our story. Piaget is thus first placed in the context of the primary institution devoted to psychological research with which he identified: the Rousseau Institute (see also Burman, 2015; Ratcliff & Borella, 2013). We then jump up a level to consider the

⁹⁷ Two names loom especially large when considering Piaget's influence on historiography: Thomas Kuhn (see Burman, 2007b) and Hayden White (see Miller, 1987). Yet because our approach takes advantage of insights not available to either of them, the results are quite different.

⁹⁸ If this attempt must be associated with specific methods, then we can call those methods "symmetry" and "reflexivity" (see Bloor, 1976/1991, 2014). By this we mean that the resulting approach applies equally to Piaget's successes as it does to his failures. It also applies both to his epistemology, and to ours. What follows is therefore as much the product of Science and Technology Studies as it is of the History of Psychology (but see Note 96).

context in which that local network was itself situated, and ask how Piaget's strategic decisions in representing its interests reflected the extant demands (see also Hofstetter, 2010). Those early successes led to new opportunities, first at the national level and then internationally. As a result, we are able to see that the same kind of thinking operated successfully at multiple levels. We then discuss when and how that thinking failed, and how Piaget accommodated his approach. The result is therefore a look at how Piaget defined psychology, in relation to the ways in which it was possible to do so acceptably, but also why he came to believe so strongly in the importance of formalized interdisciplinary collaboration after having been for decades an informal and happy tourist of philosophy, biology, etc.

2. The contexts of Piaget's early psychology

When Piaget first joined the Rousseau Institute, in 1921, it was not a "psychological" institution. The lessons given in psychology originated in several Faculties, so the discipline was not institutionalized at the time in the way that it has since come to be. Although the psychology laboratory there was among the world's first—and is believed today by Genevans to have been the very first created specifically in a Faculty of Science (in 1892)—it was also held apart in a now-unexpected way: the work produced did not have a clear "psychological" institutional identification.⁹⁹ Indeed, until 1968, no psychological dissertation from Geneva bore mention of an institutional

⁹⁹ Claparède also created, in 1937, a temporary (half day per week) neuropsychology service at the Hospital of Geneva. But he did so without informing the administration. This was then cited, simply, as "the laboratory at the hospital" (see Ratcliff & Ruchat, 2006).

“psychology.”¹⁰⁰ Instead, students were registered in the Faculty of Letters, Science, or Medicine.

There was of course the easily-recognizable journal, *Archives de Psychologie*, which was founded in 1901 by Claparède and Théodore Flournoy (1854-1920).¹⁰¹ They then edited it together for nearly twenty years; that is, until Flournoy’s death and his replacement on the masthead by Piaget in 1921 (Ratcliff & Hauert, 2006, pp. 104-105; Hofstetter et al., 2012, p. 68; cf. Burman, 2015, p. 157). However, when the journal published psychological research—from both Swiss and international scholars—the articles appeared alongside work in philosophy, anthropology, sociology, pedagogy, and even biology. The presented “archives,” such as they were, were therefore not “of ‘psychology’” in the much-narrower way that we mean this term today.

As for the broader political and economic context, between the Great Depression and the rise of Fascism in Europe, the local situation in Geneva became increasingly tense during Piaget’s early years there. When the Swiss army ultimately turned its guns on a crowd of anti-fascist demonstrators, on 9 November 1932, thirteen were left dead and sixty-five more were injured. This then also precipitated a crisis at the nearby Rousseau Institute, as the faculty there in turn became increasingly politically-polarized.

While the Institute’s directors, Bovet and Claparède, wanted to support a kind of militant activism against the excesses of the Right, Piaget sought instead to decouple the

¹⁰⁰ In the several monikers used from the founding of the Rousseau Institute until that of the Faculty created in 1975, the word “psychology” first appeared only in 1968 in the name of the “School of psychology and educational sciences” (see Hofstetter et al., 2012).

¹⁰¹ Indeed, it was Flournoy who had founded Geneva’s experimental psychology laboratory in 1892 (see Ratcliff, Borella, & Piguet, 2006).

Institute's scientific activities from any possible politically-motivated response (Hofstetter et al., 2012, pp. 59-62; also Hofstetter, 2010; Vidal, 1997a). This choice to adopt a strategic outward-facing neutrality—by accentuating the scientific objectivity of Genevan research (cf. Green, 2010)—then had important implications for the discipline that Piaget subsequently helped to promote. Briefly: to the extent that psychological research was itself seen to be purely scientific, rather than political, the Institute and its students would be safe from Fascist retaliation.

Still, though, the outbreak of WWII saw a dramatic decrease in the number of students enrolled in the Institute's teacher's college. The Institute's resources then declined by an equivalent amount. Claparède also died in 1940, and Bovet retired. The old regime thus ended. The founders were gone, and the other losses threatened to bring the institution down too. The very survival of the Rousseau Institute, as well as of its scattered and semi-camouflaged psychology (still without institutional recognition), thus became the primary issue faced by Piaget as its new Director.

3. Strategic movements

Starting in 1936-1937, Piaget had inaugurated what historian Jean-Jacques Ducret (2011) called his "classic program" (see for discussion Ratcliff, in press-b). This was an intensive investigation of the nature of mental "operations," informed by logical principles (see also Burman, in press). It was thereby intended to provide a solid foundation for his *experimental* study of children's knowledge. Yet, at the same time, it also provided the basis for his *theoretical* program in "genetic structuralism" (i.e., the investigation of the construction of novel causes [structures], and not just novel outcomes

[functions]). Indeed, this is the work that occupied the bulk of Piaget's time as a scholar until the 1950s.

With his elevation to Professor and Director, in 1940, a number of organizational changes were made as well. First, management of the Institute was split: Piaget would continue to advance the research program in experimental psychology, while a co-manager would oversee the ongoing training of teachers. Second, scientific research in psychology itself was split. Piaget then took on the oversight of all *fundamental* (basic, pure) research and—following the departure of several previously well-positioned researchers—assigned to André Rey subordinate oversight of all work in *applied* psychology (Hofstetter et al., 2012). This structure then informed every other endeavor that the Institute undertook, as well as the way in which psychology was itself defined in Geneva.

3.1. Research strategy, or strategic research? For Piaget, and thus also for the new regime at the Rousseau Institute, psychology was to be taken seriously for two main reasons. First, and in agreement with Wundt, because psychology is a laboratory science that identifies phenomena which can be described with reference to *laws*. But also, second, because its observations can be *formalized*.

Piaget explained this in an article published in 1941, in which he summarized his joint experimental and theoretical efforts with reference to the complementarity between law-driven research and his efforts at formalization. As he put it:

Psychologists have often sought to express the laws of behavior in mathematical terms, and even those of consciousness itself. But the measurement of

psychological fact has not yet exceeded the power of statistical law, which applies with certainty to all (and without prejudging the true nature of the underlying mechanisms that are described only probabilistically). It is in this way that the ‘metric scales of intelligence’ provide only broad statistical comparisons of the results achieved gradually by one’s intelligence during its evolution.^[102] But still, neither statistics nor metric scales allow for the direct measurement of intellectual operations themselves, or help to translate the fact of these operations into an analytical framework. And they certainly don’t detail the transformations that constitute development as such. (our trans of Piaget, 1941, pp. 215-216)¹⁰³

In other words, what Piaget sought was more fundamental than what was typical of psychological research at the time, and also quite different from what one would expect today: he sought “the changes in the formative processes that could explain [the gradual accumulation of acquired characteristics]” (our trans of Piaget, 1941, p. 216).¹⁰⁴

It is fascinating to see in the original sources that he pointed to factor analysis as a

¹⁰² This is certainly a reference to work done in the style of Binet and Simon, and Piaget soon afterward referred to both Binet and Terman together (see e.g., Piaget & Inhelder, 1948/1956, p. 68). For Piaget’s thoughts about Binet specifically, however, see (Piaget, 1975; also Wertheimer & Maserow, 1980).

¹⁰³ Or, as he put it in the original: “Les psychologues ont souvent conçu l'espoir d'exprimer sous une forme mathématique les lois du comportement et même de la conscience. Mais la mesure des faits psychologiques n'a guère dépassé jusqu'ici le niveau des lois statistiques, qui s'appliquent assurément à tout et ne préjugent pas de la nature intime des mécanismes dont elles expriment le résultat probable. C'est ainsi que les « échelles métriques de l'intelligence » sont des comparaisons statistiques encore bien grossières des résultats progressivement atteints par l'intelligence en son évolution, mais elles ne permettent en rien de mesurer ni même de traduire en un schéma analytique les opérations intellectuelles elles-mêmes, pas plus que le détail des transformations qui constituent le développement comme tel.” (Piaget, 1941, pp. 215-216).

¹⁰⁴ This translation reverses the negative in the original, for clarity here, but it keeps the intended meaning. The line provided is from the last half of the continuation of the earlier quote: “Il est vrai que l'on a pu représenter celui-ci par des lois exponentielles mais outre qu'elles ne différencient alors pas la croissance psychique de la croissance organique en général, elles décrivent à nouveau la sommation graduelle des acquisitions et non pas les modifications des processus formateurs qui pourraient les expliquer” (Piaget, 1941, p. 216).

possible way to engage with these issues, even mentioning Spearman by name, because Piaget is often accused today of having neglected statistical methods. But we also see what it was that he was striving for: the true challenge, he noted, was to understand the connection between identified statistical factors and the underlying causes that change during development (Piaget, 1941, p. 215n). To engage this question of causes, he then asked a theoretical question similar to that asked by Boole a century before: “At least in the study of the psychology of thinking, why not seek to apply those techniques of logical calculus that refer to the most general aspects of mathematics while at the same time remaining qualitative?” (our trans of Piaget, 1941, p. 216).¹⁰⁵

To an extent, the means by which Piaget would answer this question had already been provided by his earliest “classic” investigations on the development of mental operations. But to these he added a second experimental program focusing on lower-level processes; namely, perception (see Ratcliff & Hauert, 2006). This was then introduced, in the article of 1941, by contrasting the view of Gestaltists like Köhler—that basic perceptions reflect indecomposable wholes—with the view that had emerged in Piaget’s own experimental and theoretical work: that the development of intellectual operations could be characterized by a kind of reversibility, in whole-part relations, that is simply not allowed under the assumptions of the other theoretical system (Piaget, 1941, p. 285).

¹⁰⁵ This reorganizes the sentence for clarity, but also excludes a delightful turn of phrase in defining qualitative inquiry that we would like to provide separately: “where the immeasurable achieves the same precision as the measurable” (our trans of Piaget, 1941, p. 216). Here, though, is his question as written—in its entirety—from the original: “Pourquoi donc ne pas chercher à appliquer, au moins au domaine de la psychologie de la pensée, la technique opératoire de la logistique, qui rappelle celle des parties les plus générales de la mathématique tout en demeurant sur un terrain qualitatif où le non-mesurable atteint la même précision que le numérique?” (Piaget, 1941, p. 216).

3.2. At a higher level. Institutionally, both research programs—theoretical and experimental—also benefited from new editorial leadership at the Institute’s *Archives de Psychology*. Indeed, with the war obstructing the production and circulation of new knowledge, the journal (now co-edited by Piaget and Rey) was forced to refocus on publishing work produced primarily by the Genevans themselves. The once-international journal was therefore brought in-house, if only temporarily.

At the same time as these actions served Piaget’s research programs, they also contributed to the institutional legitimization of psychology in Geneva. And the research programs themselves—genetic structuralism and experimental psychology (combining on the one hand a constructivist and formalistic understanding of development while at the same time, on the other hand, testing the Gestaltist laws of perception in their developmental aspect)—were well-suited to this role. In short: they enabled the Institute to avoid the crackdowns that led, elsewhere, to the suppression of exactly Claparède’s and Bovet’s argued-for kind of resistance.¹⁰⁶ It was thus by balancing tradition and innovation, while at the same avoiding political scrutiny, that Piaget began to force upon Genevan psychology a particular and sustainable identity. (His own political resistance came through subtler means, which we will describe in greater detail in a future essay.)

3.3. Higher. Two still-higher levels were also constructed during the war. A new publishing venue to represent a unified Helvetian psychological discipline—the *Swiss Journal of Psychology*—was founded in 1942. This was then followed by the founding,

¹⁰⁶ The best known example of this is provided by the suppression of “pedagogical” works in the Soviet Union, 1936-1956, which is today better known for having included Vygotsky’s writings (leading to the erroneous belief that it was his work specifically that had been “banned” [see Fraser & Yasnitsky, 2015]).

in 1943, of a new professional association: the Swiss Psychological Society (known today acronymically and simultaneously, in French and German, as “SSP-SGP”). In other words, during WWII, psychology itself was nationalized in Switzerland (although “confederated” is a more appropriate term in the Swiss context).

As the two international stars personifying this new vision of psychology,¹⁰⁷ the French-speaking Piaget and the German-speaking Carl Jung (1875-1961) were given places of prominence in both organizations. Indeed, both are now remembered as key founders. Piaget then served as the first president of the SSP-SGP in 1943-1945. And he also served as a co-editor of the journal, with Jung, from 1942 to 1946.

It should be said, however, that the establishment of the society and the journal are not considered critical moments in either of Piaget’s or Jung’s personal biographies (see e.g., Piaget, 1967a, p. 145). They are rather more typically understood as a reaction by the Swiss academic community to the chaos of war; a closing-in of ranks, and overlooking of internal conflicts, in a moment of existential uncertainty. Yet to suggest this is of course to adopt a perspective that is higher than that of the individual, and higher even than the level of a national institution. It places the foundational events in an international context, relative to a World War, and one is therefore left to wonder if things might have turned out differently (i.e., if the way things are, or the way they are perceived to be, isn’t actually “necessary”).

That said, however, our purpose is not to point to the possibility of counterfactual

¹⁰⁷ They had also both received honorary doctorates, just five years before, at Harvard’s Tercentenary (see Burman, 2015, pp. 151, 153; Hsueh, 2004, pp. 21-24).

history. Rather, we would argue simply that adopting an epistemological approach that explicitly avows a reversible levels-oriented perspective enables one to adopt a broader view. Thus, just as we can jump up to the international level and place these foundational events in context, so too can we jump down a level and look for sources. Doing so in this case then suggests that the founding of these institutions could be seen as an endorsement, extension, and generalization of the local strategy of scientific neutrality and objectivity adopted in Geneva. After all, it was this that allowed the Rousseau Institute to function without having constantly to defend itself from political interference. And as a result, too, Swiss psychologists then came to benefit from the support of a new set of allies: medical doctors constituted nearly half of the new society's founding members.¹⁰⁸

3.4. Parallel developments. The two research programs inaugurated at the start of this period ended in 1955 and 1962, respectively, and both had a significant impact on the outward spread of Genevan psychology during the 1960s. Indeed, the results of the program in genetic structuralism—by then recast as the much broader “genetic epistemology” (which was itself also later updated [Beilin, 1992b])—had a global impact, especially during the resurgence in American interest in Piaget's work as part of the widespread education reforms after Sputnik (see e.g., Bliss, 1995; Herman & Ripple, 2002; Hsueh, 2005; also Müller et al., 2013). Although Piaget had already been known to Americans as a methodologist, he thus also came to be seen as a “theorist” too (see

¹⁰⁸ This is a very different outcome from the way similar alliances were forged in France, when e.g., Binet was working to create the metric scale of intelligence (see Nicolas et al., 2013).

Bruner, 1983, pp. 133-134).

Yet to truly understand what was going on at the time, and see the social and intellectual contexts that informed this work (while enabling its subsequent success), we need to jump up another several levels and then look down to compare similar processes in different contexts. To put it more simply: the postwar period was a time of broad disciplinarization for psychology globally. In other words, similar processes were being undertaken simultaneously in different contexts around the world. Indeed, as is well-known, the differently-institutionalized American Psychology boomed at this time (see Pickren, 2007). But so too did French Psychology, albeit in a different way. We then also see that Piaget's own efforts weren't confined just to Switzerland.

3.5. Piaget in France. In the aftermath of WWII, a vast editorial project began to come together in Paris under Paul Fraise (1911-1996). His goal was to unify the disparate strands of scientific psychology as it was conducted by speakers of the French language.¹⁰⁹ A supporting association was then created in 1951 (Fraise, 1988, p. 125). And this had, as one of its outcomes, a large 9-volume *Traité de psychologie expérimentale* that was co-edited by Fraise with Piaget (1963-1965).

In contrast to an earlier WWI-era synthesis by Georges Dumas (1866-1946), the new *Traité* was explicit in associating French psychology with the experimental sciences. In France and Belgium, the institutionally-recognized efforts of Piéron, Wallon, Fraise, Zazzo, Michotte, Nuttin, and their students also helped to reinforce this new identity. After WWII, French psychology thereby shifted in its fundamental orientation: less

¹⁰⁹ Fraise, of course, had his own contexts (see Carroy, Ohayon, & Plas, 2006).

philosophical, and more laboratory-based. Fraisse then went on to be elected president of the *Société Française de Psychologie* (1962-1963) and the International Union of Scientific Psychology (1966-1969).

One could certainly argue that this change to a laboratory-based approach reflects an Americanization of postwar French Psychology. (This is supported explicitly by Fraisse's recognition of the influence of E. G. Boring on his own historical perspective [see Fraisse, 1963/1968, p. 1n; and thus see also Kelly, 1981; O'Donnell, 1979].) The shifting of the disciplinary center from Europe to the US would also play a role in such an argument (see e.g., van Strien, 1997). But we prefer a more "polycentric" historiographical approach (Danziger, 1994, 2006; Pickren, 2009b; with commentary by Brock, 2014b). And we note that the French disciplinary turn toward laboratory science was consistent with Piaget's much earlier strategy from Geneva, rather than simply reflecting the outward spread of an inevitable American revolution (cf. Hobbs & Burman, 2009).

Briefly put: Piaget was with Fraisse at the center of it all, commuting regularly by train between Geneva and Paris to teach at the Sorbonne from 1952 until 1963 (see Meljac & Diener, 2000). He had also been president, in 1954-1957, of the International Union of Psychological Science (see M. R. Rosenzweig, Holtzman, Sabourin, & Bélanger, 2000). And as the director of the International Bureau of Education, 1929-1969, he had important ties to UNESCO's transnational program as well (Hofstetter, 2004, 2015; Hofstetter & Schneuwly, 2013; see also Selcer, 2009). Thus, whether or not the change in French Psychology was *caused* by the projection of Piaget's originally local

strategy to the national level through the founding of the SSP-SGP as a scientific society of laboratory-based psychologists—followed by the adoption of a similar strategy by similarly-inclined international others—it is a simple observation that Piaget was involved intimately in the disciplining of psychology as it was conducted in the French-speaking world. And focusing solely on the American influence is to be blinded to that other relevant level.

With this made clear, however, we are also led to an apparent contradiction: Piaget’s International Center for Genetic Epistemology (referred-to acronymically in French as “CIEG”) was not itself a *psychological institution*. We suggest, therefore, that Piaget’s founding of the CIEG was a further parallel effort to the disciplining of psychology. In short: he brought his earlier undisciplined tourism *inside*, and then institutionalized *that* in Geneva—rather than psychology itself—as the superordinate ideal of “interdisciplinarity” (see e.g., Piaget, 1964/1971; Piaget, 1966/1967, 1970a; also Darbellay, 2011; Vonèche, 1993).

4. The turn away from undisciplined tourism

Piaget published in biology until 1929, in sociology from 1928 until 1951, and in logic from 1937 until 1953. Yet these attempts to work simultaneously on several fronts met with a mixed reception: while Piaget’s efforts in psychology garnered great international acclaim, his earlier work in biology led to his ejection from the discipline (Vonèche, 2003). His work in logic also produced a similar initial reaction (Burman, in press; Ratcliff, in press-a; see also Apostel, 1982; Ladrière, 1982). And his sociology fared only somewhat better: although he occupied a chair of sociology at Geneva from

1939 to 1952, this has been largely forgotten (see Piaget, 1977, collecting together works published 1928-1960/1995; 1962/2000; also M. Chapman, 1986; Kitchener, 1991).

We suggest that the reason for his excursions into these foreign territories was the same as his reason for pursuing his “classic” research programs. To wit: to understand a thing, you have to see it from different perspectives.

This was reflected mostly obviously in his still-untranslated-in-English 3-volume *Introduction à l'épistémologie génétique* (Piaget, 1950). Indeed, in this new higher-order approach, participants would work to accommodate the main weakness of a disciplined psychology:

When a discipline—such as experimental psychology—separates from philosophy to set itself up as an independent science, this decision by its representatives should not be attributed to their greater seriousness or value. The choice is rather one of giving up on certain divisive topics. It is also a commitment, either by convention or by gentleman's agreement, to engage only with questions that can be answered by the exclusive use of certain common or communicable methods. As a result, there is in the constitution of a science a kind of necessary renunciation: a requirement to present as objectively as possible the results that were achieved (or the explanations that were pursued), and not to intermix these findings with concerns that while valuable must be left outside the disciplinary boundaries (*frontières*) as drawn (our trans of Piaget, 1950, p. 9).¹¹⁰

¹¹⁰ In the original: “Lorsqu’une discipline, telle que la psychologie expérimentale, se sépare de la philosophie pour s’ériger en science autonome, cette décision de ses représentants ne revient pas à s’attribuer, à un moment donné, un brevet de sérieux ou de valeur supérieure. Elle consiste simplement à renoncer à certaines discussions qui divisent les esprits et à s’engager, par convention ou *gentleman's*

In other words, what a science is—for Piaget—was *discipline*; shared norms, and a devotion to both observation and communication. Yet discipline also requires *restraint*.

This then in turn affords an interesting view of the demarcation problem: *science* as it is practiced by *scientists* is narrower, by common shared agreement, than the *scientific interests* of those who practice it. Yet Piaget also thought that great leaps forward came as a result of reflecting carefully on these narrow results, as those broader interests led otherwise disciplined individuals to stray into speculative territory. From this perspective, the development of knowledge then becomes a repeating two-step process: a discursive scientific phase, and a dialectical reflective phase (see esp. Piaget, 1980b). But here too was a problem: the methods to pursue this reflection had not yet themselves been formalized. The source of humanity's greatest advances in knowledge was therefore undisciplined; unscientific. Thus, in 1953, Piaget received the first of several grants from the Rockefeller Foundation to begin to address that very problem. And that support led in turn to the founding of the CIEG in 1955, which Piaget directed until his death in 1980 (see J. C. Bringuier, Gruber, Carreras, & Cellérier, 1977/1980; Bronckart, 1980).

From this perspective, it would be a mistake to suggest that the founding of the CIEG represented an abandonment of Piaget's efforts at disciplining psychology. Instead, the two go hand-in-hand. Indeed, as he later explained, he saw the interdisciplinarity institutionalized there as representing "the future of experimental sciences" (Piaget,

agreement, de ne parler que des questions abordables par l'emploi exclusif de certaines méthodes communes ou communicables. Il y a donc, dans la constitution d'une science, un renoncement nécessaire, une détermination de ne plus mêler, à l'exposé aussi objectif que possible des résultats que l'on atteint ou des explications que l'on poursuit, les préoccupations auxquelles on tient peut-être bien davantage par devers soi, mais que l'on s'oblige à laisser en dehors des frontières tracées" (Piaget, 1950, p. 9).

1971b, p. 539). This was later expanded-upon in a speech to the American Psychological Association:

Psychology, like all other sciences, can live and prosper only in an interdisciplinary atmosphere. Interdisciplinary relationships indeed exist, but they are still insufficient. In the science of human beings, it is clear, for example, that the study of intelligence brings up the problem of the relationship between thought and language—hence a collaboration between psychology and linguistics; and the current work of linguists on transformational grammar and linguistic structure in general is very promising for possible comparisons with the operatory character of intelligence. But this is an immense field to cover, and collaborations are only beginning. Likewise, there exist numerous relationships between data from the science of economics and of “conducts,” and game (or decision) theory, which was elaborated by economists, constitutes a very enlightening instrument for the analysis of “strategies” of behavior. But here, too, collaboration is only beginning. (Kamii speaking at APA for Piaget, 1978, p. 651)

In short, interdisciplinarity is the disciplined institutional solution to not being able to “survey, at one glance, all the operations possible of thought” (from the first epigraph). It serves psychology, and is served by it. To have its full impact on the discipline, however, interdisciplinarity must also be implemented by insiders: psychologists (from the second epigraph).

5. Conclusion

This essay was not intended to provide a comprehensive history. Rather, it was an

historiographical experiment. And, in that, it seems to have been successful: we now have access to new perspectives, as well as new questions worth asking, and we can see how their subjects and objects relate to each other. Granted, covering the same territory with greater historical rigor would require several indepth studies, some of which have already been published and are cited below. But the act of identifying those opportunities is in itself quite valuable.

The perspective provided by adopting Piaget's late epistemological framework is overly condensed, as history, but it is also nonetheless convenient as a tool for collecting together a large number of parts coherently in a short space. The result is also then, too, to place this kind of epistemological history (and thus the called-for deeper histories too) alongside more detailed empirical approaches as a way of reflecting on reported facts about the world. As Piaget explained:

no science can be placed on a single plane, and each one of them involves multiple and distinct epistemological levels. All sciences of nature, therefore, involve transcendental aspects... that are inherent in research itself and that are in constant movement and... impossible to substantiate or put down on paper once and for all. In fact, there exists a reflexive [circular, self-reflecting] progress in the sciences.... It consists of the constant delineation of new conditions of intelligibility, which are transcendental with respect to the content of later experience. (Kamii speaking at APA for Piaget, 1978, p. 649)

Adopting the levels perspective then shows why, for Piaget, epistemology was also necessary for psychology: it provides the disciplinary tools for reflecting on what it is that

psychology investigates scientifically.

The frontiers of psychology are therefore mobile first because new facts are discovered. But they move more quickly and further when meta-psychological reflection raises new questions and shows why and how they are worth pursuing as part of the normal conduct of the discipline. Although the discipline remains whole, its boundaries then move outward into new territories as a consequence of new thoughts becoming possible to think; a result of the “epistemogenesis of reasons” (see Piaget, 2004/2006; also Henriques et al., 2004). And it is interdisciplinary investigation—or, more broadly, exploratory behavior in general (cf. Piaget, 1976/1979)—that most often provides the impetus for this expansion. But we can of course also now say that Piaget’s own perspective is the result primarily of focusing on one level’s interactions with another, and both of these levels can be placed in their different contexts. This has of course been argued-for of his psychology, but not of his epistemological framework as a whole. The result is then a much more nuanced view, not only of Piaget’s life but also of his theory.

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