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The Function of Literature in Psychological Science

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Abstract

The recent reform debates in psychological science, prompted by a widespread crisis of confidence, have exposed and destabilized the so-called myth of self-correction, that is, the problem that most scientists perceive their disciplines as self-correcting without engaging in actual practices that correct the scientific record. In this paper, building on the idea of self-correction as a myth, I propose another myth common to psychological science: the myth of self-organization. The myth of self-organization is the idea that scientific literature will organize itself into something the community adding to it would recognize as systematic knowledge; while the actual members of those communities do not engage in effective ways of organizing it. I argue for the existence of the myth self-organization by taking a historical look at how the scientific literature was construed by psychologists during the 20th century. In my view, the literature, and behaviors of scientists related to it, becomes a social institution exerting influence over the science it belongs to. I conclude with a critical discussion of self-organization through the debates about preregistration and theory formalization in psychology's reform movement.

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Running head: *THE FUNCTION OF LITERATURE IN PSYCHOLOGICAL SCIENCE*

The Function of Literature in Psychological Science

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What is the function of psychological literature, or for that matter, of any scientific literature? This question will hardly receive a singular answer, but I hope even attempting to provide one will prove to be illuminating for the ongoing conversations about psychological reform. In this paper, I will discuss what psychologists perceive as the function of their literature. I will focus on the ongoing conversations about preregistration and theory within the reform debates spawned by the replication crisis in psychology. My aim is not to advance an argument within these debates. Instead, I aim to provide a venue for psychologists and other interested scholars to think about psychology's reform as a movement which is destabilizing the social institution we commonly refer to as the scientific literature.

Why focus on the conversation about preregistration if I am interested in the function of psychological literature? Preregistration represents one of the central publishing innovations reformers in psychology rally around in order to change the social practices of research (Chambers, 2013), which will in turn lead to a better replication rate and a "healthier" literature. By instituting a new article format, the Registered Report, or just encouraging a new type of behavior where scientists preregister their studies on their own, the reformers are hoping to change the face of psychological literature, and by extension, of psychological science. If they succeed, anybody perusing the literature will be able to tell, at a glance, the difference between exploratory and confirmatory research (Wagenmakers et al; 2012); the usage of inferential statistics will be constrained to become interpretable (Nosek et al, 2018); and the communities of researchers will be able to trust that what is found in the literature is replicable and reproducible. This is the kind of future, or even a utopia (Nosek, Spies, & Motyl, 2012), the reformers hope for if the methodological reform actually sweeps over psychology at large and changes the research practices of most psychologists. In this way, preregistration initiatives and the conversations scientists have about the merits and demerits of preregistration present an entry point for describing a more abstracted notion: The cumulativeness of psychological science.

If preregistration, in some form, is widely instituted within psychological science, the reformers hope this will reform the literature to finally serve its nominal function of being a correct, curated record of replicable known phenomena within a scientific discipline. My argument is that this nominal function of the literature is not self-evident. Psychologists, as positivistic scientists, are socialized to expect it, and if a group of psychologists claim it is not being achieved, this creates a rupture. In other words, it creates a crisis. Even more so, different arguments put forward during crisis change what the nominal function of the literature should be. Should the literature be a repository of robust empirical reports? Or a representation of systemized knowledge in a scientific field? Or both?

The dynamics of reform largely pit psychologists in two porous groups; the reformers and the status-quoers. The status-quo psychologists (e.g. Gilbert et al, 2016; Fiske, Schacter, & Taylor, 2016) do not see the need for reforming their statistical and methodological practices, or do not even necessarily agree that there is a replicability problem in psychology. They are often pegged as the old guard, the academics who built their careers within the methodological regime which is now being labeled as degenerative. The reformers, on the other hand, are the new kids on the block, or at least were ten years ago when the talk of reform started. They largely argue that psychology has problems with researcher degrees of freedom (Gelman & Loken, 2013), questionable research practices (Simmons, Nelson, & Simonsohn, 2011), and in general sloppy standards of inference (Nosek et al, 2018). These problems lead to the scientific literature being cluttered with irreplicable results, and with no way of identifying what should be the real phenomena of interest, and what should be discarded.

We can associate broad opposing positions about the function of the literature between the status-quoers and reformers. Reformers do not think the scientific literature is self-correcting (Vazire & Holcombe, 2020). Status-quoers believe that the institutions put into place – namely volunteer review managed by expert editors – ensures the self-correction of science, at least in the long-run. Reviewers and editors, as the status-quo view goes, are already in place to put a stop to blatant transgressions against methodological rigor and ensure the scientific record to pivot in a constructive direction, over a long enough period of time. Large part of the reform effort was actually supplying arguments against this view, as the reformers seem to collectively say: The existing journal system breeds bad epistemic practices. For the status-quoers, the literature is a repository for individual studies and this repository possess the emergent property of self-correction. For the reformers, the literature is a repository of individual studies that must be corrected and pruned in order to provide a trustworthy record.

These two groups are porous because they are not institutionalized. They represent broad epistemic ideals around which psychologists orient themselves. There are known exemplars for each, but most psychologists have complex opinions about the state of their science which do not perfectly align with one or the other. However, in the arguments put forward about reform in psychology, in particular about preregistration, we can read off the kind of epistemic goals that motivate psychologists. I especially draw attention to preregistration, because within the debates about preregistration in the last two years, the polarized dynamic between reformers and status-quoers received a third party: preregistration-skeptic reformers, or at least, reform-fellow-travelers. What happens to the function of psychological literature if there is a third party, which disagrees with the need for mandated preregistration, but agrees that there is a problem with the literature?

We may say that the reformers up to now have dismantled the myth of self-correction on the level of the literature, with their focus on replicability, questionable research practices and statistical testing of hypotheses. However, this new brand of reform-fellow-travelers trained their sights on a different myth, the one of self-organization of the literature. I call self-correction and self-organization myths because I believe they are widely held beliefs across the many communities of researchers in psychology. The myth of self-correction, which was harshly criticized and dismantled by reformers, implied that if journal review just continued apace, sloppy or fraudulent research results would disappear from the literature on their own. The myth of self-organization, on the other hand, implies that if the literature is continuously populated by methodologically robust and inferentially sound studies, this mass of empirical reports will at some point in the future be organized into systematic knowledge (usually called theory).

Keeping these two myths in mind, I will argue that the function of psychological literature is going through cycles of criticism, organized as the opposition between three broad and porous groups of researchers. First, the preregistration-minded reformers criticized status quo psychologists that the literature, under the methodological regime in place up to the 2010s, is not self-correcting. The preregistration-minded reformers argued that this function is a myth; there is no self-correction as an emergent property of a scientific literature, there are only individual scientists who will actually pursue correcting the record. This argument, at least among the reformers, is uncontroversial. Then, the preregistration-skeptics criticized the reformers that in dismantling the myth of self-correction they fell for a more insidious false belief about the function of psychological literature, namely the myth of self-organization. The reformers, by pursuing the reform focused on replicability and statistical testing, had hoped that if the literature was policed and pruned from bad studies, this healthy literature would allow for self-organization of psychological knowledge into theory. As I will show by following the preregistration-skeptic views on theory, this is also a myth about the function of the literature which still begs dismantling.

Preregistration and its discontents

As preregistration has been gaining steam and successfully expanding among editors of psychology journals, a group of psychologists and cognitive scientists have articulated a position arguing against it, or at least the outsized role it seemed to occupy in the reformist “fixing” of psychology. The culmination of this criticism of preregistration was the short paper “Preregistration is redundant at best”, which after rounds of discussion on social media received a less controversial title of “Is preregistration worthwhile?” (Szollosi et al, 2020). The authors’ argument is straightforward: “[T]he diagnosticity of statistical tests depend entirely on how well statistical models map onto underlying

theories, and so improving statistical techniques does little to improve theories when the mapping is weak” (Szollosi et al, 2020, p.1) What they stress is the difference between scientific inference and statistical inference; and that increasing the robustness and transparency of statistical inference through preregistration will remedy little if the theories underlying statistical hypotheses and models are not shored up. Now, if the point of psychology was just amassing effects, preregistration was the intervention of choice. Such a view, though, buys into the myth of self-organization and it seems that the preregistration critics were rebelling against that: Piling effects will not lead to theory.

The paper, despite its provocative first title, was a straightforward argument about the limits of preregistration. The authors called attention to a deeper problem revealed by the implementation of this new publishing format, an issue with psychological theory. Transparency and preregistration were important improvements, but, as the authors argued, will remain ineffectual to solve the deeper issue of poor scientific inference due to meager theory. The argument in the paper reads as a typical extension of the reformist literature in psychology, critical but engaging with a “new” practice (preregistration) and inspecting its advantages and disadvantages, with a somewhat polemical tone.

Despite that, the paper met an uproar of backlash among many reformers.¹ The Twitter² “hot takes”, discussions, threads summarizing the paper and agreeing or disagreeing with parts of it; represent a true window in the social and scientific dynamics within the reform movement itself. It also shows that the movement, as an analytical category, is far too monolithical for the kinds of positions gathered under it. The polemics and “flaming” on Twitter show how damning the identification with the “old guard status-quoers” is in the new – primarily online - community gathered around scientific reform. Strongly disagreeing with something as central as preregistration represents a position that cannot be entertained and for the culprit to still be seen as a good-faith supporter of psychology’s reform. In the extreme, being fingered as a reform denier by one of the prominent reformist voices might even attract harassment, ridicule or future citation exclusion from the growing reform literature.

The debates after the Szollosi paper are an example of the social dynamics of critique and disagreement mediated by scientists communicating through social media. For an analysis especially touching on the so-called “tone debate”, see Derksen and Field (2021) and Morawski (2020). The

¹ For a more contextualized view on preregistration from one of the authors, and also a comment on the social media kerfuffle over it, see Navarro (2020b). See also the extensive comment by Gervais (2020), who also expands on the role of theory in these conflicts within the reform movement.

² For following the myriad of takes on reformist Twitter, see the Altmetric page of the paper: <https://www.altmetric.com/details/69591120/twitter/page:1>

reform movement is a newly minted social organization among psychologists. On one hand, the reform is extremely horizontal and accessible through social media like Twitter and the network of blogs and podcasts acting as nodes in that ebb and flow of social media chatter, and on the other with newly established elites within that seemingly flattened organization. The members of the new elites are those who have social media accounts with thousands of followers which command the audience of large swaths of the reform-curious psychologists across the globe. Or more traditionally, who have managed to make metascience careers out of reformist methodological innovations, attracting funding and landing publications in traditional venues of prestige; or, lastly, are themselves leaders in the platformization of openness (Andrews, 2020) in psychology.

Twitter in particular represents one of the liveliest channels for this kind of communication (another I will not go into here are Facebook groups), and it gives rise to some new behavior when it comes to scientific debate. Potentially every online public conversation, especially when one or more of the participants are members of the new social media elite, becomes a venue for propagating views about epistemic ideals through the discipline. Robustness, cumulativeness, transparency, objectivity, progress; all the familiar signposts of the epistemological underbelly³ of a science become rallying cries and points of contention in short spurts of 240 characters, or longer threads espousing more elaborate views. Sometimes they are discussed explicitly as philosophical fundamentals for empirical research, more often they are just implied through conversations about research design, technical implementation, coding, platform-use, ethics or integrity. Many participate in these conversations which forge minor reform celebrities sometimes even drawn from early career researchers or students. More importantly, many more in the semi-passive audience read and pick up parts of these conversations and bring them back to their research groups, departments and classrooms, either silently not to antagonize their potentially hostile seniors, or openly as supporters of a new movement aimed at fixing psychology. What do we learn from this about the function of the scientific literature in psychology, and the myth of self-organization? Primarily two things: one from the content of the preregistration kerfuffle and the other about the medium of the conversation.

The content of the preregistration debate - exemplified by the social media ripples around the Szollosi paper - shows how the myth of self-correction and the myth of self-organization prop each other up. If science does correct itself, be it through the naïve expectation of the status-quoers that it will literally self-correct as a system or the more sophisticated preregistrationist reformist view that it will take actual work by scientists doing the correcting; if it does get corrected, then this

³ I call it underbelly here because even working scientists who do not engage in philosophical research about epistemic goals and values still have some sort of implicit epistemology indigenous to their disciplinary community (see Flis, 2019 for indigenous epistemologies in the reform movement).

correction will result in a “cleaned-up” scientific record which will at some point in the future allow for organization into something that many will finally recognize as systematized knowledge. The Szollosi paper problematizes the first myth by exposing the second one. The literature will not self-organize or be organized by the community because the production of sophisticated theory in psychology is meager. Instead, conspicuous correction will just reveal that the emperor has no clothes. The improved statistical inference will show the extent to which psychological theory and related scientific inference are poorly developed and practiced by psychologists. Psychologists seem to be disciplinarily steered away from gaining skillsets for theorizing or working as theoreticians within the bounds of the discipline. Theoretical work, by and large, does not secure funding, academic positions, venues for publication or access to training graduate students (Gigerenzer, 2011; Borsboom, 2013; Smaldino, 2020b).

The medium of conversation about preregistration, on the other hand, tells us that the polemical aspect of the scientific literature was moved from the published record into the sphere of social media, often to the chagrin of established psychologists in the status quo camp (see the Fiske “methodological terrorists” episode; Derksen and Field, 2021). Not only have the polemics moved from journals to blogs and podcasts, but they seem to have been amplified by polarization through social media debate. Indeed, as discussed by Alex Csiszar in his history of scientific journals (2018), the new Open Science movement, criticism of peer review and radical criticisms of the publishing industry have started segmenting the many functions which have been collapsed into the scientific literature from the nineteenth century onwards. The research fronts and unsubstantiated speculative claims moved to preprint databases; the fiery polemics and attacks to the social networks, blogs, and podcasts. Preregistration, if successful in its sweep through psychological science, will further explicitly segment the record into the speculative “exploratory” research and the fodder for hypothesis-testing in the “confirmatory” part.

As is common with social-epistemological change, the transformation is piecemeal, disorderly, and uneven. But it points toward the literature coming into the full glory of its function as imagined by the reformers. It is to be curated, cleaned, corrected, retraced, trimmed and pruned as information; a collection of methodologically sanitized knowledge. However, this new literature will only work properly if the critics-from-within-the-reform pointing out the mismatch between statistical and scientific inference are not correct. And so, controversial papers are penned, Twitter filled with mockery, and voices are pushed to the reformist margins because they are too radical for a strategic fight over the soul (preregistered or mathematically modelled?) of psychology versus the status-quo inertia looming large.

Theory-talk and psychologists bootstrapping themselves to the truth

During the twentieth century, psychologists have found a way to explain away the intractability of their constructs. The consensus is largely that psychological phenomena are ontologically complex. This complexity can be accommodated with the seemingly humble conclusion that psychology is unfinished as a scientific project – and it has been unfinished for half a century, if not longer. More so, the ontological complexity is contained by a methodological straightjacket. Methods and statistical inference have stabilized across the subdisciplines of psychology (Flis 2018, p. 1-41). The stabilization spread through research designs of correlational and experimental psychology, circumscribing both applied and basic research, and served by construct validity and frequentist null hypotheses significance testing. Empirical inquiry continued unabated during the second part of the twentieth century, incrementally adding bits and pieces to the literature which will be organized at some point in the future. At the end of 2021, that future is still not now.

The replication crisis, and the researchers pulling it forward with criticism and innovation, have cast a critical look at the project of empirical overproduction without trustworthy correcting mechanisms. They have exposed the myth of self-correction built into methodological and inferential infrastructure of quantified psychology. They have called for methodological rigor and tried to redefine what it is. Also, they have called for new social practices of research. Change in the subdisciplines is slow but steady. The reform continues apace. Some already declared victory (Nelson, Simmons, and Simonsohn, 2018). Some fear the reform has run out of steam without achieving its goals (Inzlicht, 2020). The metascience spawned by the replication crisis also continues; papers are published, the literature prodded and inspected, as are the behaviors of scientists.

Within this context, in the last few years, a new perspective has slowly coalesced around conversations about theory in psychology. This perspective is moving the limelight from replication to theory construction. The discussion of the Szollosi paper was one of the examples of this new perspective – a polemical jab at preregistration, the eponymous innovation spearheaded by the preregistrationist reformers. The talk of theory was first explicitly articulated as a new perspective arguing against the methodological focus of the reform movement at large. It has also exposed a truism known to most psychologists: What counts as theory, how theory is developed, and how data is interpreted; all these practices around theory differ among different psychologists. Theory does not mean the same thing for all psychologists. Within the reform movement, since its early days were dominated by an elaborate conversation about conceptual replication and the misuses of loose theoretical interpretation in social psychology, the mainstream reformers have become extremely cautious when somebody invokes “we need more (work on) theory!” It seems like the cognitive

modelers, coming from a different research tradition, were the unwitting victims of that older conversation.

In the way that replicationist criticism revealed the myth of self-correction, and tried to remedy it by inaugurating a new publishing practice of preregistration, the new talk of theory in the reform movement is tracing the contours of the myth of self-organization. The mathematical psychologist Danielle Navarro (2020b), through an intellectually stimulating text connecting statistical inference, Feyerabendian philosophy of science, and generative art; articulated the position undergirding the work of the new theoreticians as a way in which scientists “bootstrap themselves to the truth”:

This kind of “virtuous cycle” is how we hope our scientific processes unfold. We rely on our theoretical insights [...] to design experiments [...] that allow us to modify our theories, design new experiments, and so on. The interplay between these two components is – we hope – the process that allows our theories to better approximate the truth, and our experimental results to better target the phenomenon of interest.

However, it seems that at least at first, the reform movement was not a big enough tent to allow researchers interested in theory in, at least when they were explicitly critical of something as central as preregistration. I would argue that the pushback in the reform movement against this way of centering theory is not surprising because of at least three compelling reasons. Firstly, the ongoing reform of methodological practices in psychology is already hard. Reformers act strategically in trying to propagate their innovations through the communities of researchers, and they often meet resistance. Going off message, by privileging theories, risks watering down a point that is already difficult to explain or accept. Secondly, focusing on theory and deemphasizing replication sounds strangely familiar: like the rehashing of the “conceptual vs direct replication” debate between the status quo and reform-minded social psychologists. This is especially the case when it comes from a perceived out-group, fellow travelers to the reform but not outspoken advocates. Thirdly, and more profoundly, saying that the methodological innovations centering replication will not solve some supposedly deeper issue verges on the border of apocalyptic. It would mean that the accepted research practices have an even deeper systematic problem than the lack of methodological rigor. It would mean a reform of research practices will not cut it. An even deeper change in how research is organized would be needed; something much more similar to a revolution. The message on the wall is truly disturbing: Improving psychology’s methods will not be enough.

The space in the reform movement which the new theoreticians carved out for themselves is the one calling for plurality and diversity in science, not only as a political desirable but an epistemic necessity. Berna Devezer and colleagues (2019) articulated this position in a highly technical paper

that models the behavior of scientists adopting different research strategies (theory testing, novelty-seeking, boundary testing, and replication) for converging on scientific truth. Their results, counterintuitively, show that:

[T]he link between reproducibility and the convergence to a scientific truth is not straightforward. A dominant research strategy producing highly reproducible results might select untrue models and steer the scientific community away from the truth. Reproducible false results may also arise due to bias in methods and instruments used [...]. While both reproducibility and convergence to a scientific truth are presumably desirable properties of scientific discovery, they are not equivalent concepts. In our system inequivalence of these concepts is explained by a combination of research strategies, statistical methods, noise-to-signal ratio, and the complexity of truth. This finding further indicates that issues regarding reproducibility or validity of scientific results should not be reduced down to QRPs or structural incentives.

They also show that populations of scientists with diverse strategies “will discover the truth quickly”⁴ while “[i]f populations are largely homogeneous, with one research strategy dominant over others, then the scientific population tends to perform poorly on at least one of the desirable properties which might mean forsaking reproducibility or delaying discovery.” In other words, prioritizing replication over other strategies is a bad choice for a community of researchers in the long run. More speculatively, communities adopting diverse strategies and welcoming a plurality of perspectives will be epistemically more efficient.

The reform movement is a microcosm for this call for diversity – the preregistration reformers are faced with the need to welcome other perspectives if they hope to be truly effective. Devezer’s call was a double whammy for the reformers. It became a locus for the issue of diversity within the reform movement regarding theory versus replication, but also for the problem of #bropenscience (Whitaker & Guest, 2020). It was an opening through which modelers from cognitive science could claim space for their understanding of what theory was, an understanding quite distinct from the conceptual versus direct replication conversation happening until then (Gervais, 2020; Devezer et al, 2020; Guest & Martin, 2020; Van Rooij & Baggio, 2020a; Van Rooij & Baggio, 2020b; Navarro 2020b). Even though, coming from cognitive science they were just spelling out the common practices within their fields, they were actually articulating a counter-perspective developing on the margins of the reform movement (others articulating these perspectives include Robinaugh et al, 2020; Fried,

⁴ Truth is presupposed as singular and reachable by available methods by the authors and built as such into the model. It’s an open question what various departures from realism would mean for this approach.

2020a, 2020b; Smaldino, 2019, 2020b; Muthukrishna & Henrich, 2019). I use marginal here to signify that the perspective was not mainstream among the preregistrationist reformers. It was explicitly rejected in the beginning as the preregistration debate shows. For cognitive scientists, there was nothing marginal about it; this is what their communities were doing all along.

What does recentring theory have to do with the myth of self-organization which is the topic of this paper? I would argue that talking about the mismatch between statistical and scientific inferential practices, in the way that Navarro, Devezer, van Rooij, and Guest and Martin write about; or the lack of mapping of one to the other, is precisely an articulation of how incremental empiricism of twentieth century psychology ultimately failed at arriving to systematic knowledge as judged by the psychologists themselves. Here we have psychologists who say shoring up the methods and statistics producing the massive psychological literature will not make that literature into something theoretically organized. In other words, changing practices to ensure the correction of the scientific record will not turn the scientific record into a repository of systematic theories. Such an improvement will require different kind of work.

What is this different kind of work that will be needed? While following the debate in the reform movement, a classical treatment of the social dynamics in a very different scientific community came to mind – Andy Pickering’s (1984) description of the discovery of weak neutral currents in particle physics. Pickering gives a detailed and convoluted account of how weak neutral currents came into being as a phenomenon in particle physics in the 1970s. What made me think of his description when reading the psychologists argue about the primacy of theory and replication is the way Pickering (1984, p. 112) describes theoretical and experimental practices as symbiotic. The scientists practicing experiments and the scientists developing theories propped and pushed each other forward when gathered around the same phenomenon, each group generating new problems for the other to work on. The social organization of the community was such that it pitted theoretical work versus empirical work not as adversarial, but as both a check and a jumping off point for the other. Very similar to what Danielle Navarro calls “bootstrapping to the truth”, just for constructivist Pickering, that was bootstrapping to the mutually constructed phenomenon.⁵

I don’t think psychologists need to model their communities to 1970s particle physics. Specific social arrangements which allow for both empirical and theoretical work can take a myriad of forms in the sciences, and many have nothing to do with such a rigid distinction. Take for example Hans Jorg Rheinberger’s description of experimental systems in biology (1993). Whatever the particular type of

⁵ Van Rooij and Baggio (2020a) make a similar suggestion, with an added theoretical cycle which assesses the plausibility of theories even before they are faced with any empirical content.

social arrangement for research, psychologists – especially reform-minded psychologists – need to take up the explicit goal of securing a space for theoretical work in the social fabric of the psychological sciences. Journals need to be created or propped up, curricula changed to accommodate for training in theory formalization and modelling, and article formats devised for purely theoretical work which will interface with the massive empirical literature that is constantly being (over-)produced.⁶

Some preregistrationist reformers have indeed taken up this call, e.g. Scheel and colleagues (2020). Where you can still find the remnants of the reformist internal conflict about the role of replication versus theory is in the reference lists. In the above cited paper, many of the central actors (Navarro, Van Rooij, Devezer, Guest and Martin) are not cited and their work on reformist theory is not acknowledged. The exception is the work of Eiko Fried and Paul Smaldino. How come? The reason behind this is complex, and only truly known to the authors. I am not trying to infer intent of the authors here, just observing how such citing decisions factor in the ongoing dynamics of conflict of opinion within the reform movement. Observing from the side, Van Rooij, Guest and Navarro are often pegged as outsiders because they come from the cognitive modelling and mathematical psychology side; as is Berna Devezer considering her heavily model-centric work (and her philosophical and historical sensibility). Fried, on the other hand, is a researcher in psychopathology with a keen theoretical interest, bridging the chasm from the psychologists' side. Smaldino is in cultural evolution, applying this perspective very innovatively to social phenomena in scientific communities. Interestingly, Fried and Smaldino are also the only two men in the otherwise ignored group of theoreticiennes. Ultimately, it might be that the opposing camps around preregistration in the reform discussions represent a good case study for polarization in scientific communities, a topic of considerable interest in social epistemology (O'Connor & Weatherall, 2018). Even if a middle-ground position develops, it still leaves the question which side will reap the benefits of articulating this position on theory, and have the opportunity to lead the way forward.

The theory-minded reformists have already made a first step in claiming the need space for theory. In the first batch of papers explicating the new call to mathematically formalized theory, a number of “tutorials for theorizing” have already been published. Olivia Guest and Andrea Martin (2020) explained how to model through the “pizza problem”, and also provided examples of how outputs of

⁶ The new theoretical reformers seem to be deeply aware of the issue with institutional functioning when it comes to theoretical work. Smaldino (2020b, p. 6) even keeps the option open that it might be insurmountable for psychological science: “Perhaps the field of psychology has been so thoroughly devastated by the problems of replicability and generalizability that its lessons should be discarded, and a replacement field should be built anew on firmer foundations.” This kind of “reconfiguration instead of unity” was already suggested by the historian Chris Green (2015) when he discussed the problem of disunity of psychological science.

psychological research could be theorized, specified and implemented through a model-centered approach. Paul Smaldino wrote a tutorial on how to translate verbal theories into formal models, aimed specifically at the “model-curious” (Smaldino, 2020a). Iris van Rooij and Mark Blokpoel (2020) also wrote a tutorial for theory formalization, the whole instructional text written in the form of a compelling dialogue between fictional characters Verbal and Formal. These show awareness that modelling and formal theorizing are new practices which need to be introduced.

The social organization of research

Sociologists and historians have been keenly aware of how the social life of scientists and their literatures are entangled. In Latour and Woolgar’s (1979/1986) classical account, the authors follow an imagined anthropologist investigating a very peculiar tribe; that of scientists in a lab. And the said anthropologist observes some fascinating behavior by the locals: Lab equipment and materials were used in a deliberate way by the group in order to produce graphs and text that were then organized into papers. The lab served as a place where the material was transformed into the symbolic; the doing of experiments into literature. Nowadays, detailed histories of the scientific literature, the journals and articles in it, are shedding new light on the history of scientific publishing (Csiszar, 2018; Baldwin, 2018; Fyfe et al, 2017) and its role in science. These historians conceptualize the literature as one of the social institutions in science, institutions which have been developed, tweaked, changed and refashioned depending on the needs of the myriad of actors involved in publishing, editing, and reviewing the content but also earning from the trades and industries associated with it.

The role played by the literature itself, as a collective conception, is not only a reflection of the scientific community adding to it. It is also a part of society at large, and some wider trends in the history of media, or information in general. Twentieth and twenty-first century scientists are often under the impression that science, and by extension scientific publishing, are somehow extricated from the lowly trends like media culture in their society, but that couldn’t be further from the truth. Alex Csiszar (2018, p. 2) masterfully exposes the internal contradictions that have accumulated through the history of negotiations of what is the function of scientific literatures in the social life of nineteenth century science: “How did it ever become possible to suppose that scientific journals could be both permanent archive and breaking news, both a public repository and the exclusive dominion of experts, both a complete record and a painstakingly vetted selection? And what are the stakes in deciding that this no longer makes sense?”

Within the reformist talk about replicability, one of the contradictory aspects from Csiszar’s description of the scientific literature seems to have trumped all other: The idea that the literature is

a complete record; something that needs to be pruned by retraction in case of malfunction (malfunction through fraud, or just lack of care i.e. “rigor”). Csiszar (2018, p. 286) - sensitive to the sensibilities of the nineteenth century when scientific journals were formed as part of a turbulent media culture hoisted by new publics and formats in wider society outside of universities and learned societies - has a unique vantage point for observing such structural aspects of our own quandaries at the beginning of the twenty-first century:

The conception of the scientific literature as a total source of scientific information contributed to and was bolstered by the idea that modern society entered a new epoch in the twentieth century, the information age. Scientific papers – public, countable, and seemingly able to retain their truth value far from their context of production – were among the most exalted instantiations of the morselized character of this abstract sense of information that was purported to pervade society. Like the scientific journal itself, invocations of information often fused together several different, sometimes contradictory ideas, including facts about particular phenomena, a substance that was transferred in any act of communication, and a mathematical theory of signal and noise.

The “morselized” view of information meshed well with the idea of a scientific article as just one of the many building blocks in the bigger structure of science. And indeed, the natural sciences embraced it wholeheartedly. Psychology, especially in North America, was following these trends from the early years of the twentieth century. The precursor of the APA style was already suggested in 1929, and some twenty years later formalized in the first Publication Manual of the American Psychological Association. According to the historians Matthew Sigal and Michael Pettit (2012), following Blair (2003), the manual arose as a specific technology institutionalized to combat information overload.

Information overload “pertains to the period when a discipline or a community of scholars begin to produce more knowledge than any particular researcher can reasonably be acquainted with” (Pettit & Sigal, 2012, p. 1) In some of the older sciences, information overloads might date as far back as the sixteenth century, prompting the “production of a host of new genres of books, such as indexes, and bibliographical practices designed to retrieve already existing knowledge” (Pettit & Sigal, 2012, p. 2). What are the peculiarities of systematizing knowledge in disciplines like psychology which have expanded into their first information overloads at the cusp of the information age? The myth of self-organization is precisely the kind of a peculiarity that quantified social sciences disciplined in the twentieth century (Isaac, 2012) would experience. The same technologies – be they abstracting journals like *Psychological Abstracts* or *PsycINFO* it later turned into (Benjamin & VandenBos, 2006)

– operate and serve very different social and epistemic roles in fields that went through a series of information overloads and their coextensive regimes of theoretical and experimental practices comparatively recently, during the twentieth century.⁷

Common criticisms of psychological theory compare the state of the discipline's theories to physics, identifying the lack of quantification (especially quantified point-predictions, see Meehl 1967) as the biggest problem. In my view, that is putting the cart before the horse. Quantified theories are a product of scientific communities which exhibit social organization which allows for interaction between theoreticians and empiricists, pitting the progression of one group's research agenda on the condition that they provide fodder for the other side. No such thing exists in quantified social science like psychology, because psychologists value empirical overproduction above all else, and have no identifiable groups of researchers that are not empiricists.

Psychologists' empirical overproduction

The rhetorician of science Charles Bazerman put forward the idea that psychologists who subscribe to the structure of writing prescribed by the APA style implicitly accept the idea that their research is adding to the incremental encyclopedism of their (sub)discipline. Incremental encyclopedism, as Bazerman (1987, p. 139) defines it, is:

With the article primarily presenting results, constrained and formatted prescription, authors become followers of the rules to gain the reward of acceptance of their results and to avoid the punishment of nonpublication. Accepting this role, they subordinate themselves to the group endeavor of gathering more facts toward an ultimately complete description of behavior – a project of incremental encyclopedism. As behaviorism gradually gained influence, authors began presenting results as ends in themselves, to fill gaps in other results, rather than as potential answers to theoretical questions. In the mid-twenties [1920s], introductions rapidly ceased to raise problems and began to give a codified review of the literature, with each item associated with a specific contribution. The experiment to be reported in the article was presented simply as a continuation of prior work. After a brief period when close analysis of the literature was allowed in small print, disagreements over theory, results, or formulations in the previous literature tended no longer to be discussed. Articles tended to be treated as accumulated facts, literature reviews in the articles tended

⁷ Not only that, but the development of *PsycINFO* as a database of psychological knowledge might have been one of the experiments testing the idea that would be exported to the rest of the disciplined sciences as the century progressed. For informed speculation in this regard which still awaits corroboration through empirical historical work, see Burman (2019). Burman asks some pointed questions about *PsycINFO* in the Cold War context and the role of NSF and maybe CIA in its development.

to lack synthesis, problem-orientation, or interpretation. [...] This communal vision – much narrower than the traditional “shoulders of giants” formulation diminishes the role of the individual as a thinker.

In other words, Bazerman’s view was that the format and style of the experimental report; and later the research article; was a crucial element in the epistemological system of psychology of the time. The format expressed what psychologists perceived as a fundamental feature of their discipline, namely that it progressed by adding small pieces of the whole picture of behavior bit by bit. If enough of these bits were accumulated incrementally, they would form true pictures of the objects of research.

For the participating behaviorists, the project was building true pictures of human (and animal) behavior. As behaviorism waned, the article format it inaugurated persisted. And I would claim that the incremental empiricism behind it also stuck. It stuck on because the idea that there will be convergence to formalized theory waned, and was replaced with the myth; an excuse that even if nobody could imagine such a convergence, it was okay to continue the empirical overproduction because that in itself would be enough in the future. Psychology could still be counted among the forward-looking positivistic sciences, and its lack of theory had a promissory note for future exculpation. Psychologists basically said: the knowledge is unfinished with no known way for convergence at this point, but if we continue on, we will persevere by way of accumulation of empirical descriptions.⁸ With the rise of inferential statistics and null hypothesis testing in the middle of the twentieth century (Gigerenzer et al, 1990) alongside construct validity theories (Slaney, 2017), this incremental encyclopedism transformed into the language of effects. If enough effects were accumulated, some kind of systematic knowledge would arise from that collection.

The myth of self-organization of the scientific literature, for scientific psychology, fills in for the lack of social organization for producing theory, arising as a social norm forged in the twentieth century information overload. This also explains why other disciplines, especially in the natural sciences, do not subscribe to the myth of self-organization or are positioned to resist it. They inherited social norms and institutions for organization of knowledge from previous regimes stabilized under information overloads long forgotten. It is not only that “mature” sciences are mature because of the level of formalization of their theory. More importantly, they allow for scientists with diverse

⁸ For a version of this view, see Greenwald (2012). Not only that Greenwald explicitly articulates the importance of methods development and application over theoretical analysis (or, as a type of theoretical analysis), he also bases his view on the analysis of Great Persons, the Nobel Laureates and connects it to his view of scientific progress. I would like to thank Patrick Forscher for drawing my attention to this paper.

epistemic orientations to cooperate under the umbrella of the same discipline. Not only the production of empirical reports, or of abstract theory, but both and everything in between.⁹

Conclusion

In this paper, roughly three groups of actors were identified participating in the talk about reform and theory: status-quo psychologists, preregistration-minded reformers and theory-minded reformers. These groups are analytical fictions I use for sorting out the conversation in content but also follow its social dynamics. It seems that all three groups adhere to the idea that theory is important. For status-quo psychologists, the accumulation of effects leads to flexible interpretation and what Eiko Fried calls “weak theories” (Fried, 2020a, p. 23). They are fast and loose with their theoretical interpretation of empirical effects, amending those interpretations or ditching them altogether depending on need. For status-quoers, theories are verbal descriptions so they can be disposed of without much consequence. In my analysis, the status-quoers buy both in the myth of self-correction and the myth of self-organization. Adding to the literature, without prepared infrastructure for correction or organization, serves the goals of psychology’s incremental encyclopedism. The preregistration-minded reformers developed their positions in response to the status-quo psychologists. Realizing the abysmal irreplicable state of the collected effects in the literature, they set out to formalize how the justification of hypothesis testing operates in psychology. Basically, they tried to fix the criteria for what gets to enter the literature.

The literature would be fixed by a rigorous application of the new infrastructure for correction of the record; preregistration the most useful among them. This would provide a literature that would be useful at some point in the future. The theory-minded critics articulated their position in response to the preregistration-minded reformers. Coming from cognitive science and computational modelling, they saw little use in mandating preregistration across the board. The gain in transparency would be nice in itself, but considering the weakness of psychological verbal theories – the weakness present both in the status-quo looseness with hypothesis testing and preregistration-minded “robustness” – this improvement would not help in dismantling the myth of self-organization. Even if the scientific record is corrected and policed, the communities will be sifting through a morass of weak verbal theories with scarce a candidate worthy of the complicated affair of testing hypotheses.

Psychology’s literature would be fixed only if it is populated by a new brand of formal theory, and its caregivers taught, encouraged, and enabled in formal theory construction and modelling.

⁹ For a contrasting point, see Peterson and Panofsky’s (2021) articulation of the difference between integrative and diagnostic replication, and how that difference interfaces with “task uncertainty” in different scientific disciplines.

What are the directions for scientific psychology that can be read from this theory-talk?

Psychologists, across the board, will need to start a conspicuous mathematization of their verbal theories and the formal modelling of phenomena of interest. As a historian, I wonder whether this new zeal for theory will really provide a new direction for psychological science? I truly hope so. In some social sciences, modelling has been a very constructive force (e.g. Morgan, 2012). But then I think of a comparatively rarely mentioned paper by Thomas Kuhn, written in 1961 before the famed *Structure of Scientific Revolutions*. In it, Kuhn draws the distinction between the traditional physical sciences and the Baconian ones. Traditional sciences included astronomy, mechanics and optics, while the Baconian sciences gather the study of heat, electricity, magnetism and chemistry. The traditional sciences, according to Kuhn, experienced a profound mathematization – through relatively reliable measurement and formalized theory – already in the seventeenth century. While in the Baconian sciences “[s]uccessful quantification [...] had scarcely begun before the last third of the eighteenth century and only realized its full potential in the nineteenth” (Kuhn, 1961, p. 188).

Kuhn’s view was quite somber. Mathematization and formalization could not be forced. They needed to wait for the development of sophisticated enough measurement devices and the conceptual ecology which would be qualitatively rich enough with potentially measurable phenomena. And even though the mathematization of the physical sciences was a huge source of their epistemic success, it could not be forced. As Kuhn (1961, p. 190) put it: “The full and intimate quantification of any science is a consummation devoutly to be wished. Nevertheless, it is not a consummation that can effectively be sought by measuring. As in individual development, so in the scientific group, maturity comes most surely to those who know how to wait.” Has the incremental empiricism of twentieth century psychology produced sophisticated enough measurement and a bank of phenomena theoretically-minded psychologists can work with? The answer to that question will dictate whether the myth of self-organization will have been only identified within the ongoing reform movement or fully dismantled. Or, even more apocalyptically, if psychology survives as a science we still might recognize.

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