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RICERCHE

The "cognitive neuroscience revolution" is not a (Kuhnian) revolution. Evidence from scientometrics

Eugenio Petrovich[α] & Marco Viola[β]

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Abstract Fueled by the rapid development of neuroscientific tools and techniques, some scholars consider the shift from traditional cognitive psychology toward cognitive neuroscience to be a *revolution* (most notably Boone and Piccinini). However, the term "revolution" in philosophy of science can easily be construed as involving a paradigm shift in the sense of Kuhn's *The Structure of Scientific Revolutions*. Is a Kuhnian account sound in the case at hand? To answer this question, we consider heuristic indicators of two features of paradigm shifts: the incommensurability of ontologies; and a gap between scientific communities. Based on our evidence, we argue that no revolution has occurred (at least, not yet).
KEYWORDS: Cognitive Neuroscience; Cognitive Psychology; Philosophy of Science; Thomas Kuhn; Scientometrics

Riassunto *La "rivoluzione delle neuroscienze cognitive" non è una rivoluzione (in senso kuhniano). Evidenze scientometriche* – Complice il rapido sviluppo di strumenti e tecniche in neuroscienze, alcuni studiosi (in particolare Boone e Piccinini) intendono il passaggio dalla psicologia cognitiva classica alla neuroscienza cognitiva nei termini di una *rivoluzione*. Tuttavia, il termine 'rivoluzione' in filosofia della scienza è strettamente associato alla nozione di successione di paradigmi esposta da Kuhn ne *La struttura delle rivoluzioni scientifiche*. Obiettivo di questo lavoro è capire se effettivamente la concezione kuhniana offra una corretta descrizione di questa dinamica storica. In particolare, prenderemo in esame due indicatori euristici delle rivoluzioni kuhniane: l'incommensurabilità ontologica trai due paradigmi e la diversa composizione demografica delle comunità scientifiche. Sulla base delle evidenze scientometriche che prenderemo in esame, affermeremo che non è avvenuta nessuna rivoluzione (almeno per ora).
PAROLE CHIAVE: Neuroscienze cognitive; Psicologia cognitiva; Filosofia della scienza; Thomas Kuhn; Scientometria

[α]Dipartimento di Economia Politica e Statistica, Università degli Studi di Siena, Piazza San Francesco, 7-8 – 53100 Siena (IT)

[β]Dipartimento di Filosofia, Comunicazione e Spettacolo, Università degli Studi Roma Tre, via Ostiense, 234-236 – 00146 Roma (IT)

E-mail: eugenio.petrovich@unisi.it; marco.viola@uniroma3.it (✉)

1 Introduction

TEXTBOOKS OFTEN PARTITION THE FLOW of human history into different epochs, separated by remarkable events. For instance, most textbooks set the boundary between the Modern and Contemporary Ages in 1789, the year of the French Revolution. In a similar fashion, the history of scientific psychology is often presented as a series of revolutions. First, we had the behaviorists' rejection of introspection, a kind of parricide of founding fathers, including Wundt and Titchener. Then came the so-called *cognitive revolution*, which legitimized theoretical entities that mediate perception and action via the analogy between minds and computers. Then, more recently, the rise of cognitive neuroscience, which is often thought to represent a revolutionary break with classical cognitive psychology and its lack of interest in the neural implementation of cognitive processes.

The idea that the rise of cognitive neuroscience is a *revolution* seems implicit in many accounts of the phenomenon. Perhaps it is fueled by the rhetorical statements that have accompanied big science projects like the Human Brain Project or the BRAIN initiative. Sometimes, it is even made explicit in scientific articles. For instance, in listing tool-driven revolutions in neuroscience, Bickle declared that it remains an open dispute[1] whether the rise in cognitive neuroscience spurred by the rapid spread of functional brain imaging deserves the very label "revolution". But in the same year, Boone and Piccinini published a paper entitled *The cognitive neuroscience revolution*, describing a change of tides in science that remained, they claim, unbeknownst to many fellow philosophers.[2]

Given the vast influence of Kuhn's book *The structure of scientific revolutions*, talk of revolution may be easily interpreted in terms of shifts from one paradigm to another possibly incommensurable one. In this paper, we will claim that conceiving the transition from traditional cognitive psychology to cognitive neuroscience in terms of a Kuhnian paradigm-shift amounts to mischaracterizing it. Hence, given how easily talks of revolutions in the context of scientific progress may evoke the Kuhnian narrative of paradigm-shifts, we advise fellow philosophers and historians of the mind sciences to refrain from employing a revolutionary lexicon when describing and thinking about this transition. Apart for historical accuracy, we think that our concerns have broader implications. Our most salient fear is that, if the Kuhnian narrative is adopted by cognitive neuroscience textbooks and becomes the standard training framework, new generations of neuroscientists will be tempted to neglect the past psychological literature under the wrong assumption that this has nothing to do with their field. This could condemn them to "reinventing the wheel".

After presenting some theoretical considerations that contextualize our claim in the next section, we use scientometric data to defend it against the paradigm-shift view of the psychology-to-neuroscience transition. The notion of paradigm-shift is notoriously elusive to definition. As such, bibliometricians struggle to provide consensual operationalizations of this notion.[3] To circumvent this problem, we take a slightly more indirect approach, addressing two proxy properties diagnostic of paradigm-shifts: namely, that the two contrasted paradigms have different ontologies (incommensurability); and that they are pursued by different scholars (Planck's principle). In Section 3 we offer a brief recap of Kuhn's account of scientific progress and derive testable scientometric predictions for both incommensurability and Planck's principle. In the following sections (4 and 5) we show some data that argue against these predictions (for details on data and methods, see the appendix). Lastly, in Section 6, we hint that other models of scientific progress better describe the transition from classical cognitive psychology to cognitive neuroscience.

2 The shift from classical cognitive psychology to cognitive neuroscience

As we mentioned, Boone and Piccinini described the passage from classical cognitive psychology to cognitive neuroscience as a "paradigm shift".[4] Why? The main breaking point, according to the authors, is epistemological. During the *Ancién Regime* of traditional cognitive psychology the golden explanatory standard was a boxological framework confined to Marr's algorithmic and representational levels,[5] which prevailed irrespective of the physical implementation. This epistemological stance was based on a commitment to a functionalistic metaphysical framework according to which human minds were (or were interpreted as) universal Turing machines, with mental states often construed as sub-personal mental representations. The cognitive psychologist was not concerned with finding out what parts of the brains performed some cognitive operation since, as Putnam famously put it: «We could be made of Swiss cheese and it wouldn't matter».[6] Instead, cognitive psychologists sought to provide functional explanations, i.e., decomposing a complex behavioral task into several stages of cognitive processes occurring at the sub-personal level, whose nature and relationships were often represented by boxes and arrows, respectively.[7] Ideally, the same model that describes how a cognitive operation can be exploited by human beings could be used to program a software that performs the same task.

By contrast, in the new age of cognitive neuroscience, respectable explanations need to be cast in terms of multilevel neurocognitive mechanisms.

Abstracting from some relatively fine-grained differences, the most influential definitions of mechanistic accounts[8] share the idea that to provide a mechanistic explanation of some phenomenon means to explain how it depends on component operations that result from the workings of component parts that exhibit the right kind of spatial and temporal organization. In the context of the transition from cognitive psychology to cognitive neuroscience, box-and-arrow designs are thus no longer sufficiently explanatory: they are at best sketches of mechanisms,[9] placeholders for a complete account given in terms of the neural structures and processes implementing a given operation.

Now, the mechanistic epistemological framework traced by Boone and Piccinini may not reflect current practices in cognitive neuroscience. Indeed, mechanistic explanation is more a regulative ideal subscribed to by philosophers than the standard epistemology used in present-day neuroscientific research. And while some philosophers agree with Boone and Piccinini that interlevel mechanistic explanations must be the gold standard to which cognitive neuroscience aims,[10] others have protested that mechanistic explanations need not be the only explanatory account in neuroscience[11] or that, in trying to catch up with the challenges of complexity, nuanced mechanistic frameworks ultimately give up on their signature heuristic strategies, i.e., decomposition and localization.[12]

Of course, some neuroscientists also share the urge to have interlevel mechanistic explanations. John Krakauer and colleagues are so convinced of the need for an integrated mechanistic understanding of the mind that they make a plea for it.[13] Indeed, their urge to make this plea seems to reveal the fact that aiming at full-fledged mechanistic explanations has become more the exception than the norm. In fact, most neuroscientific studies simply settle for comparing different processes in terms of their neural correlates. Perhaps this gap will be filled sooner or later, thanks to novel tools provided by cognitive computational neuroscience that simulate some dark corners of the brain that could not be examined otherwise.[14] Or maybe neuroscientists will end up sidestepping explanatory aims and rather focusing on mind-reading, i.e., predicting mental states from neural data – a trend that Yarkoni and Westfall suggest and against which Weiskopf protests.[15]

3 Scientific revolutions as Kuhnian paradigm-shifts

But let us assume for the sake of discussion that the "cognitive neuroscience revolution" has paved the way to the Eldorado of mechanistic explanation. Would this constitute a good reason to invoke such a burdensome term as "revolution"? As we mentioned, the term "revolution" is heavily weighted in philosophy of science, by the specific meaning it was given in Kuhn's masterful book, *The structure of scientific revolutions.*[16] As is well known, Kuhn's book depicts the history of mature sciences as a sequence of paradigm shifts. Each paradigm provides some exemplars that set the lexicon and the agenda of a scientific community, thus enabling it to accumulate progressive knowledge by doing normal science. Whatever does not fit into the scope of the paradigm, or contradicts it, is treated as an anomaly, and provisionally swept under the rug. However, when too many anomalies stack up, the dominant paradigm may end up being challenged by newer ones, until one of them manages to establish itself as the new dominant paradigm. And then normal science can restart.

New paradigms may fail to account for some phenomena that old ones could account for (and vice versa), because they have a different ontology. This is due to Kuhn's commitment to a certain degree of theory-ladenness in observation. In simple terms, he admits the possibility that no neutral ground may exist for objectively comparing the merits and flaws of different paradigms. Due to this *incommensurability*, the battle between paradigms does not hinge on purely rational grounds, but ultimately requires some leap of faith. However, leaps of faith are rare. Kuhn embraces Max Planck's idea that «a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it».[17] This principle has been eponymized as *Planck's principle* and gets efficaciously expressed by the dark slogan "science advances one funeral at a time".

Having evoked the Kuhnian framework, we cannot help pondering the following question: does the passage from classical cognitive psychology to cognitive neuroscience count as a genuine revolution *sensu Kuhn*? Boone and Piccinini do not explicitly endorse such a bold claim[18]. On the contrary, some passages in their paper suggest that they embrace a more prudent view of evolutionary and progressive shift rather than the kind of rupture envisaged by Kuhn. First, they follow Piccinini and Craver[19] in considering boxological explanations of cognitive science as sketches of mechanistic explanations. And second, they highlight that «rather than reinventing the wheel, [cognitive neuroscientists] began incorporating behavioral protocols from cognitive psychology and applying those protocols to experimental setups in which neural activity could be monitored in both humans and model organisms».[20] And yet, a reader may well interpret their position in a strong Kuhnian sense as they write: «cognitive science as traditionally conceived is on its way out and is being *replaced* by cognitive neuroscience»[21] and, by explicitly calling the transition a “paradigm shift”,[22] they somehow wink at a Kuhnian narrative.

Notwithstanding what Boone and Piccinini claimed or might claim, the question remains: is the transition from classical cognitive psychology to cognitive neuroscience a *bona fide* paradigm shift? Unfortunately, there is no straightforward answer to this question. Those willing to tell this story based on a Kuhnian script will have no trouble finding nice quotes that support such a narrative. On the one hand, some so-called “ultra-cognitivist neuropsychologists”[23] refrain from conceding any evidential weight to neuroimaging data. When doing so, they love to quote Jerry Fodor, a paragon of old-style cognitive psychology: «If the mind happens in space at all, it happens somewhere north of the neck. What exactly turns on knowing how far north?».[24] On the other hand, we find a scientifically minded philosopher like Anderson[25] suggesting that the huge mass of neuroscientific data can - and must – be used to prompt a complete refurnishing of our mental category. He remarks that what he has in mind is not just a “revision” (gradually negotiating extant categories), but rather a revolution (jettison the extant categories and start from scratch based on solid neural foundations). Now, there is no doubt that both positions have been *influential* and have promoted considerable debate in the field. However, being influential in a field does not necessarily imply being *representative* of the mainstream. Indeed, it is entirely possible that such views were influential precisely because they were extreme, thus setting the outer boundaries of the common view.

One way to avoid this kind of “cherry-picking fallacy” may lie in switching from a qualitative to a quantitative approach. One problem of implementing such approaches in the case at hand is that the Kuhnian notions of “revolution” and “paradigm” are notoriously elusive. It is very hard to define them. Let alone to operationalize them in formal terms. To circumvent this conceptual issue, some library scientists have prudently settled for proxy measures of paradigm similarity, such as ascertaining whether scholars cite the same documents[26] or use common keywords.[27] In the remainder of this paper, we will adopt a similarly indirect approach. Aided by some bibliometric data, we will argue that thinking in terms of Kuhnian revolution misrepresents what actually happened in the wake of cognitive neuroscience. Rather than trying to operationalize the notion of paradigm itself, we look for traces of the two signatures of paradigm shifts that we outlined above, namely the *incommensurability* of the two paradigms and the separateness of their communities, as posited by *Planck’s principle*. A detailed explanation of how data were gathered and processed is provided in the Appendix.

4 Are cognitive psychology and cognitive neuroscience ontologically incommensurable?

According to some scholars, the attempt to use neuroimaging techniques to localize cognitive function is at best premature because we still lack an agreed-upon ontology of cognitive constructs.[28] Indeed, the very neuroscientist who heralded the debate on cognitive ontology, Russell Poldrack, concedes that by projecting the wrong set of psychological categories onto the brain we might end up reifying them. As proof of concept, he browsed the literature to find studies that might be reinterpreted as functional localizations of the mental faculties posited by Sir Francis Gall’s phrenological theory.[29] But unlike detractors of neuroimaging, who consider this to be fatal evidence that neuroimaging tools are worthless, Poldrack takes a reformist stance: instead of blindly localizing extant faculties, he proposes, neuroimaging techniques must be used to critically reassess them. However, while Poldrack seeks to pursue this reform by means of some sort of reflexive equilibrium between psychological taxonomies and neuroscientific data, other scholars argue for a drastically bolder approach, where extant psychological categories are jettisoned, and neural co-activations are taken as *prima facie* evidence of some common underlying psychological dimension.[30] This quarrel, whose roots can be backtracked at least to the Churchlands’ pleas for eliminativism,[31] is now commonly referred to as the debate on cognitive ontology.[32]

Now, while the radical take on cognitive ontology would promote the kind of ontological incommensurability that we would expect to follow a Kuhnian revolution, the fulfillment of its promise seems hindered by several conceptual problems. To put it simply, patterns of neural activities can hardly be individuated in a way that is totally shielded from any psychological assumption whatsoever.[33] And even if the project of a fully bottom-up, brain-based reform of our cognitive ontology turns out to be viable, our feeling is that the ontology of cognitive neuroscience has not cut the umbilical cord yet.

In order to move from feelings to data, we can muster some indirect evidence of ontological commonality/divide between cognitive psychology and cognitive neuroscience. The most obvious approach would be based on keywords or on some other forms of literature mining from either abstracts or articles full texts.[34] However, remarks concerning the high polysemy of psychological constructs across different research groups[35] make us skeptical of this approach. The deep structure of ontology may remain rather stable despite differences in labels, whereas apparent similarity in labels may hide major differences in their meanings. Therefore, we bargained on a rather intuitive approach taking another route that, while less direct, we deem to be more prudent: measuring citation exchanges. Citations signal, if not direct knowledge exchange, at least participation in a common "discussion".[36] This is why they are commonly used in scientometrics to map the structure of research fields and their mutual relations. When incommensurability happens, according to Kuhn it is associated with communication breakdowns between specialists: in terms of citations, this can be operationalized as a lack of mutual citations in these specialists' publications. By contrast, robust citation flows can be interpreted as a proxy for scientific information communication between fields and, thus, the lack of incommensurability barriers.

After all, except for a first "revolutionary" stage, in which the early Copernican astronomers directly challenged the Ptolemaic theory, we do not expect to find discussions of epicycles in astronomy. Nor do we expect to find Paracelsus' writings mentioned in present day chemistry journals. On the contrary, robust citation trading between psychological and neuroscientific journals would signal that the two disciplines share a *lingua franca*, contrary to what you would expect if they were incommensurable.[37]

Yet, as bibliometricians know very well, rather than by being defined by some fixed level of reality,[38] scientific disciplines (such as "psychology" and "neuroscience") are best seen as historical entities; indeed, in bibliometric mappings they present themselves as spurious clusters, with overlapping areas, that change shape over time. Moreover, when philosophers of cognitive science speak of "psychology" and "neuroscience", often they have specific subfields in mind, namely cognitive psychology (i.e., the study of sensory systems or higher mental faculties in individual healthy adults) and systems neuroscience. However, experimental cognitive psychology is just a part of the broader field of psychology, together with other areas like social, educational, or clinical psychology. And within neuroscience, systems neuroscience (roughly corresponding to cognitive and affective neuroscience) is not alone. As stressed by John Bickle, contrary to the misperception of many philosophers, molecular and cellular neuroscience – the more biomedical branches of the discipline – make up mainstream neuroscience.[39]

A rough approximation of the structure of the citation exchanges between Psychology and Neuroscience is displayed in *Figure 1*. The figure is a science map which spatially represents relationships between the journals indexed in the Web of Science (WoS) database (as of December 15, 2021) under

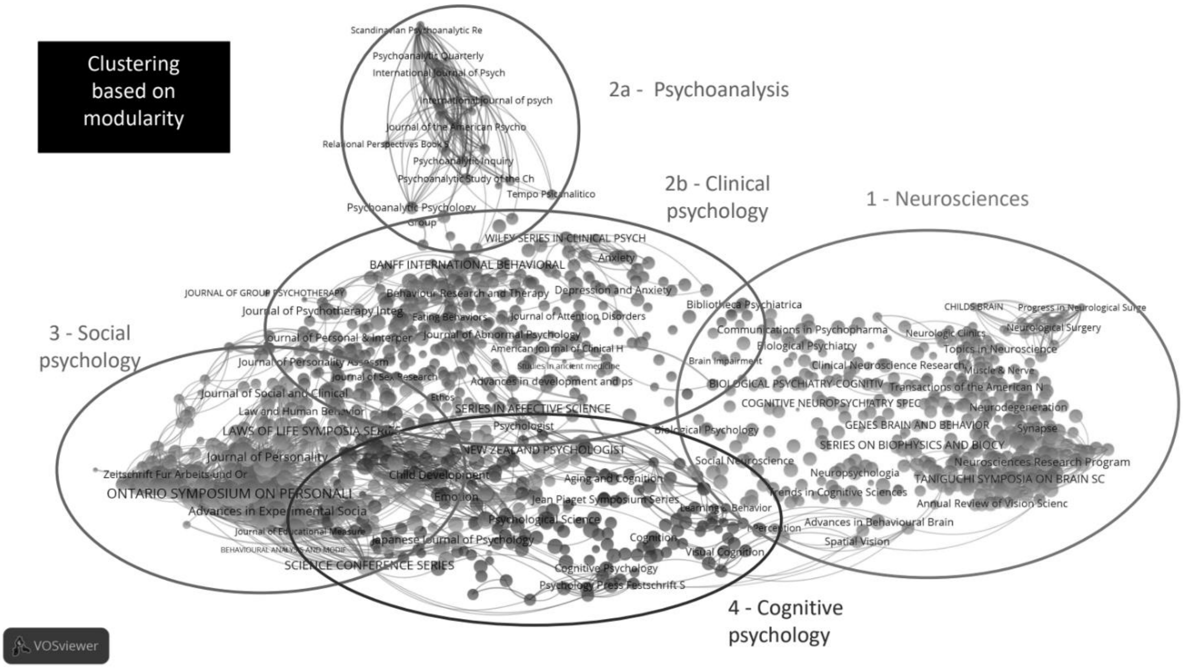

Figure 1. *A science map representing the citation landscape of journals categorized as* PSYCHOLOGY *or* NEUROSCIENCES *(or both) according to the Web of Science database.* The similarity between two journals is based on the number of journals they both cite and on the intensity of their citations, so that journals with a similar citation profile are placed together on the map (journal bibliographic coupling). The thickness of the links represents the similarity of pairs of journals (only links with strength > 0.3 are shown), whereas the size of the nodes is proportional to the sum of the strength of the links incident to the nodes. Clusters of similar nodes are individuated by the Leiden clustering algorithm and are represented by the different colors of the nodes. Labels were proposed by the authors based on their domain expertise. For more details, see the Appendix. Software: VOSviewer (cf. N.J. VAN ECK, L. WALTMAN, *Software survey: VOSviewer, a computer program for bibliometric mapping*). A colored interactive visualization (without our labels) is available at https://tinyurl.com/2osz3dfp

the categories PSYCHOLOGY (N = 1028), NEUROSCIENCES (N = 473), or both (N = 50).[40]

Even considering all due caveats regarding possible biases induced by the database or by the visualization, Fig. 1 tells us a few things.[41] The majority of exchanges between Psychology and Neuroscience – including the 50 journals belonging to both categories – lie at the interface between the yellow cluster (which we propose to label "cognitive psychology") and the red one (which we label "neurosciences"; see Figure A1 in the Appendix). Now, the very fact that journals of cognitive psychology still exist and undergo florid editorial activity contradicts a strong Kuhnian ideal: if there had been a revolution, the paradigm we used to call cognitive psychology should have vanished, and another called cognitive neuroscience should have arisen from its ashes. Instead, what we see is that both coexist and trade citations. Moreover, some journals are classified as pertaining to both fields (although based on their citing behavior, we included most of these in the red cluster).

While this map provides a nice panoramic picture of the citation landscape, it is worth zooming in on some finer-grained details. To do so, we leave aside the clusters we imposed on the map for the sub-categories of Psychology journals based on WoS classification: *PSYCHOLOGY (UNLABELED), APPLIED, BIOLOGICAL, CLINICAL, DEVELOPMENTAL, EDUCATIONAL, EXPERIMENTAL, MATHEMATICAL, MULTIDISCIPLINARY, PSYCHOANALYSIS, SOCIAL*. (No sub-areas of NEUROSCIENCES are provided by the WoS database. See the Appendix for the descriptive statistics). We see that the percentage of citation output targeting NEUROSCIENCES journals vary greatly within these subcategories: roughly 2.05% of the citations in the 131 journals of APPLIED PSYCHOLOGY point toward NEUROSCIENCES journals. In contrast, NEUROSCIENCES journals get the lion's share of 24.3% of citations from the 29 journals classified as BIOLOGICAL PSYCHOLOGY. This might be rather unsurprising, given that BIOLOGICAL PSYCHOLOGY is conceptually very close to NEUROSCIENCES – but let us stress again that the very existence of such overlaps does not fit comfortably with a Kuhnian account. However, the citations in EXPERIMENTAL PSYCHOLOGY journals (N = 126) of NEUROSCIENCES journals is also fairly high, at around 17.2%.

In comparison, PSYCHOLOGY journals get a lot less citations from NEUROSCIENCES: on average, barely 0.7% citations found in NEUROSCIENCES journals target PSYCHOLOGY journals. Here, too, the percentage varies greatly based on the PSYCHOLOGY subfield: while PSYCHOANALYSIS journals are almost never cited (0.003%), EXPERIMENTAL PSYCHOLOGY gets some attention, i.e., 2.13% of the total citation output. To compare,

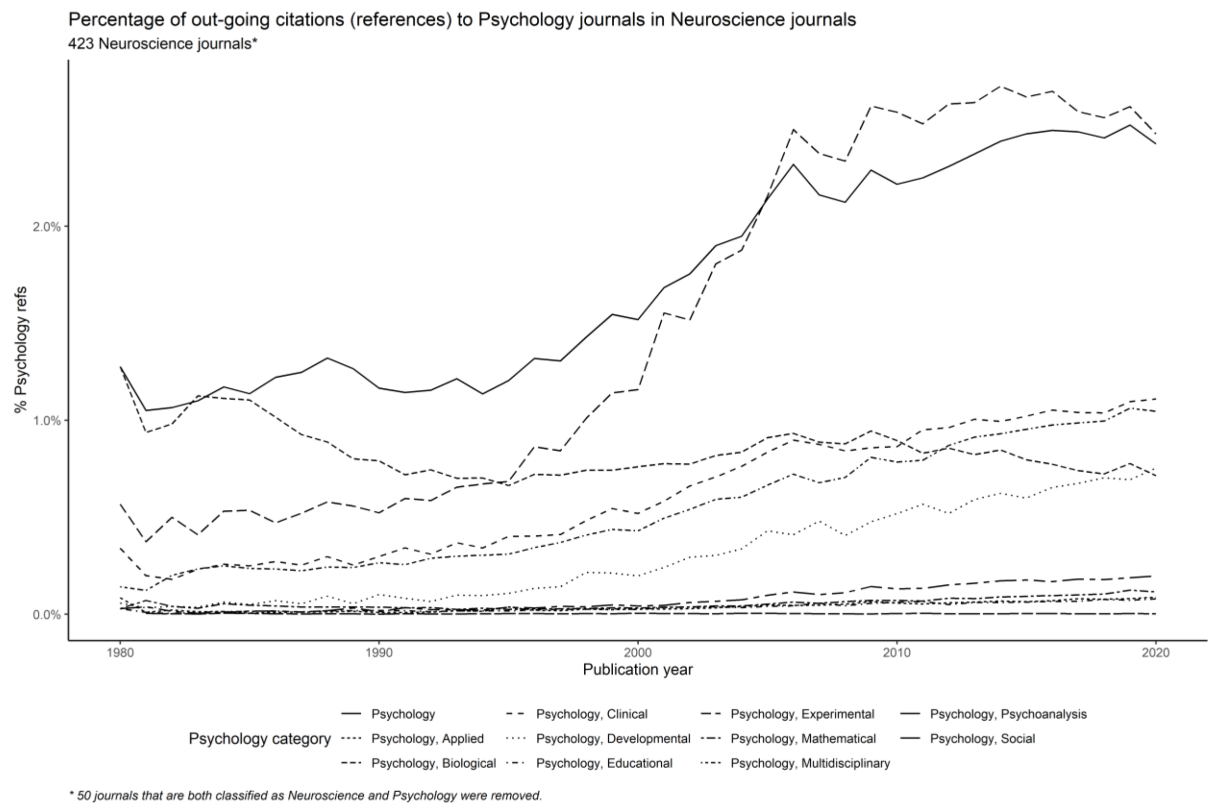

Figure 2. *Percentage of references in neuroscience journals pointing to psychology journals over time.* The percentage is calculated over the total references for each year. Note that the 50 journals that were classified as both neuroscience and psychology were removed from the citing journals to avoid overestimation.

consider that about 47% of the total citations from NEUROSCIENCES journals point toward other NEUROSCIENCES journals, 13% to CLINICAL NEUROLOGY, and 9% to BIOCHEMISTRY & MOLECULAR BIOLOGY.

This imbalance may reflect several factors. One of them is probably an asymmetry in the prestige and power of biomedical disciplines *vis-à-vis* the social sciences, which is periodically lamented by cognitive scientists coming from a psychological background.[42] But we are reluctant to assign all the weight to socio-political explanations of this kind because other factors may weigh in, such as different publishing and citation cultures. Disentangling them goes way beyond the scope of our article.

Let us then come back to our initial question: is the citational landscape we briefly surveyed suggestive of the kind of ontological incommensurability that a Kuhnian revolution would entail? We are inclined to say no. Intuitively, the imbalance in the citation quota going from EXPERIMENTAL PSYCHOLOGY to NEUROSCIENCES and vice versa might suggest a hierarchy of power that can somehow be squared with some kind of revolutionary narrative, in which experimental psychologists are seen as the remaining defenders of an old, defeated paradigm, and hence "pay" their citation tribute to the winners, i.e., the Neuroscientists.[43]

Suggestive as this picture seems, it quickly fades once we account for another dimension, that we have not discussed thus far: the temporal dimension. If a revolution had occurred, we would expect that people in the winning paradigm – a sub-area of Neuroscience – would stop, or at least show a decrease in citations of the old paradigm – somewhere within the field of Psychology. But the opposite is true. That is, the percentage of citations from NEUROSCIENCES to PSYCHOLOGY has increased (cf. *Figure 2*). In particular, the number of NEUROSCIENCES journals that quote EXPERIMENTAL PSYCHOLOGY journals tripled during the years of the supposed Revolution.

Rest assured that an advocate of the "subjugation of Psychology" scenario outlined above will insist on noting that the quota of citation going from Psychology to Neuroscience is an order of magnitude higher. And it progressively increased over the same 40 years – in EXPERIMENTAL PSYCHOLOGY it almost doubled. And yet, while these considerations may still count as evidence for some prestige/power asymmetry – or for a fascination with some sort of positivist-flavored reductionism (if you prefer a theoretical reading over a socio-political one) – we think that they hardly support the claim that the rise of cognitive neuroscience promoted incommensurability.

5 Are cognitive psychologists and cognitive neuroscientists different people?

Let us now turn to a more direct question: namely, are the communities of cognitive psychologists and cognitive neuroscientists made up by distinct researchers? Recall that Planck's principle allowed for some conversions, but not many, since «it rarely happens that Saul becomes Paul. What does happen is that its opponents gradually die out, and that the growing generation is familiarized with the ideas from the beginning».[44] Some rare exceptions are also tolerable: for instance, a single scholar may continue to contribute to both cognitive neuroscience and cognitive psychology without invalidating the claim of revolution. But if we take the claim seriously that, in general, cognitive psychologists and cognitive neuroscientists adhere to competing paradigms in the Kuhnian sense, we should consider this scholar to be a lonely multidisciplinary genius stoically resisting the modern drive to specialization, a kind of bipolar Dr. Jeckyll and Mr. (or Ms.) Hyde that works both for and against each team. Conversely, if the proportion of authors that work for both teams is not negligible, a likelier explanation is that there have never been two competing teams after all.

Our personal intuition strongly advises us that nothing like the Planck principle obtained during the so-called cognitive neuroscience revolution. Quite to the contrary, we suspect that scholars who identify as psychologists routinely get employed in neuroscience departments, if only to switch back to psychology departments. And the same holds for scholars who think of themselves as neuroscientists.

A similar blend, we suspect, can be found in publication venues. To verify our intuition, we had to turn to another database, Dimensions. In fact, compared to WoS, Dimensions allows for more accurate identification of authors based on standardized IDs.[45] Another interesting feature of Dimensions is that, instead of using the journal sector as a proxy for the disciplinary identity of the paper it contains, Dimensions leverages a machine learning algorithm that assigns a paper to a given scientific field, based on (a simplified version of) the ANZSRC Fields of Research classification.[46] The focus is on three fields of interest: Psychology, Neuroscience, Cognitive Science. According to the classification performed by the algorithm, from 1980 to the present day (December 15, 2021), 2,388,493 papers have been published in Neuroscience, 2,421,160 in Psychology, and 713,394 in Cognitive Science. How many of them were authored by scholars whose name figures in more than one list? Were things different in 1980?

In absolute terms, the number of authors who have published at least one paper in two or even three fields has increased hugely during these four decades: the number of those who have published at least one paper in both Cognitive Science and Neuroscience increased by +801%; in Cognitive Science and Psychology by +580%; and in Psychology and Neuroscience by a massive +1808%. Moreover, we see an increase of +726% in authors who have contributed at least one paper to each of the three fields (cf. *Table 1*). For the most part, this inflation was simply driven by the number of active scholars in each of the three disciplines. But indeed, the very fact that this increase affects *all three* disciplines seems at odds with a Kuhnian reading of the cognitive neuroscience revolution, which would rather predict a significant increase in authors writing Neuroscience papers at the expense of increases in the authors of Psychology papers, and especially Cognitive Science papers.

Table 1. *Author statistics.* The table provides a comparison of the number of authors who have signed at least one paper in each of the three fields we are examining (Cog = Cognitive Science; Psy = Psychology; Neuro = Neuroscience), as well as the number of authors who have authored a paper in at least two fields, or in all three. The classification of fields is taken from Dimensions (see the Appendix for more details).

Areas	# in 1980	# in 2020	Delta	Delta %
Cog	11 478	79 061	67 583	+589%
Neuro	42 281	408 513	366 232	+866%
Psy	28 293	397 879	369 586	+1306%
Cog & Neuro	3136	25110	21974	+801%
Cog & Psy	8293	48095	39802	+580%
Psy & Neuro	3648	65956	62308	+1808%
Cog & Psy & Neuro	2244	16288	14044	+726%

Even when normalizing over the total number of authors (of at least one paper) in Psychology and Cognitive Science, the number of authors that also published (at least one paper) in Neuroscience increased, respectively, from 13% to 17% and from 27% to 32% (fig. 3). These are not small percentages, and they continue to increase. Had a revolution occurred, the road to Damascus would be way too overcrowded.

6 If not a revolution, then what?

To recap, in the introductory section we wondered whether the rise of cognitive neuroscience, which is sometimes construed as a revolution (most notably by Boone and Piccinini), would qualify as a Kuhnian revolution, in which one paradigm is abandoned for another. We then hastily pointed out, in the following section, that this is not an easy question to answer, since the notions of “paradigm” and “revolution” have no straightforward definitions – let alone operationalization. However, we noted, two kinds of features may be taken as heuristic cues that some sort of Kuhnian paradigm-shift has occurred: incommensurability and Planck’s principle. These two features cannot be measured directly either. But by spotting an increase in citation exchange we noted that, rather than supplanting cognitive psychology and burning bridges to psychological communities, the rise of cognitive neuroscience actually strengthened communication with psychology – the contrary of what one would expect from incommensurable paradigms. Moreover, even accounting for the rise in absolute numbers of scholars involved in the study of mind and/or brain, the percentage of researchers authoring works in multiple fields has increased. Again, this pattern of data invites a narrative of integration not revolution.

Admittedly, our arguments are far from conclusive: we could only tackle the issues of incommensurability and Planck’s principle using proxy data, which inherit the biases and distortions embedded in the WoS and Dimensions databases. We do not exclude the possibility that someone will find new data, or even come up with a new interpretation of ours, proving that the cognitive neuroscience revolution is a revolution in a strong, Kuhnian sense. They are very welcome to do so, but we think that the burden of proof lies on their shoulders. Until convincing counterarguments are provided, it is better to avoid speaking of revolutions. The influence of (the early) Thomas Kuhn looms way too large over philosophy of science for us to use word “revolution” without immediately bringing his thesis to mind. In fact, authors who talk about revolutions in neuroscience sometimes feel the urge to suspend their judgments on the revolutionary status of cognitive neuroscience,[47] or supplement some caveats in a footnote.[48] But since, in some editorial formats, footnotes are separated from the relevant text by several pages, a reader may miss them. And even if she reads them properly, the ergonomic principles regulating memory could easily conflate “a not-necessarily-Kuhnian revolution” with “a revolution” plain and simple, which may end up acquiring some feature of Kuhnian ones a few months later. If a Kuhnian notion of revolution is then taken seriously in defining the syllabi for new generation of neuroscientists, it could legitimize neglecting knowledge accumulated over decades of boxological cognitive psychology. Thus, we deemed an explicit rejection of this hypothesis based on arguments that can be publicly debated to be potentially useful. Moreover, our rejection has motivated the search for another narrative. What narrative?

Theoretical considerations and the pattern of scientometric data we sketched above jointly invite us to think of the passage from cognitive psychology to cognitive neuroscience as one of progressive expansion and integration, rather than abrupt rupture.

Of course, we are not the first to hold such as a view. It is similar to what Bechtel had in mind when he described the *vertical expansion* of the original cognitive science research program[49] happening over twenty years ago. Or what led the neuroscientist Henson to downsize the fuss (both positive and negative) over neuroimaging when he claimed that «functional neuroimaging data simply comprise another dependent variable, along with behavioural data, that can be used to distinguish between competing psychological theories».[50] And indeed, why should the data collected using a functional hemodynamic technique be qualitatively different from those we get from an eye-tracker? More recently, even Miłkowski[51] has argued that computationalism – that is, the core commitment of old-fashioned cognitive science – has not been overturned by neuroscience, but rather evolved within it. Otherwise, a notion such as canonical neural computation would never enjoy the popularity it has nowadays.[52] According to Miłkowski, the account of scientific progress that best fits with this transformation is that of *research tradition*.[53] We are sympathetic to this choice, because research traditions (unlike Kuhnian paradigms or Lakatos' research programs) allow for evolutionary changes to their core commitments.

Whatever notion one picks to describe it, the resulting picture is one where researchers can muster and compare data from different sources so as to analyze cognitive phenomena from a variety of perspectives. Take the research program undertaken by Keltner and Cowen. They seek to go beyond the claustrophobic taxonomy of emotions left by Paul Ekman's (purely psychological) legacy, Basic Emotion Theory. And they do so by reporting consistent patterns from a huge variety of sources, ranging from facial and vocal expression to neural markers.[54]

We want to point out that, by promoting the idea that the passage from first wave cognitive psychology toward cognitive neuroscience was more fluid that revolutionary talk would imply, we by no means intend to downsize its significance or potential. Indeed, we acknowledge that new techniques have yielded not only precious new data, but also prompted us to rethink some key concepts.

Arguably, a revolutionary narrative would make it harder to account for the diachronic relationship between these disciplines. Thus, let the sleeping Kuhnian concept lie.

7 Appendix: Data and methods

Data for the bibliometric analyses were extracted on December 15, 2021 from Clarivate Analytics Web of Science (WoS) database (https://www.webofscience.com/) and Digital Science Dimensions database (http://dimensions.ai/).[55] Queries were made through the SQL relational database system hosted by the Centre for Science and Technology Studies (CWTS) at Leiden University, using the most recent versions available of Web of Science and Dimensions. Micro data cannot be made

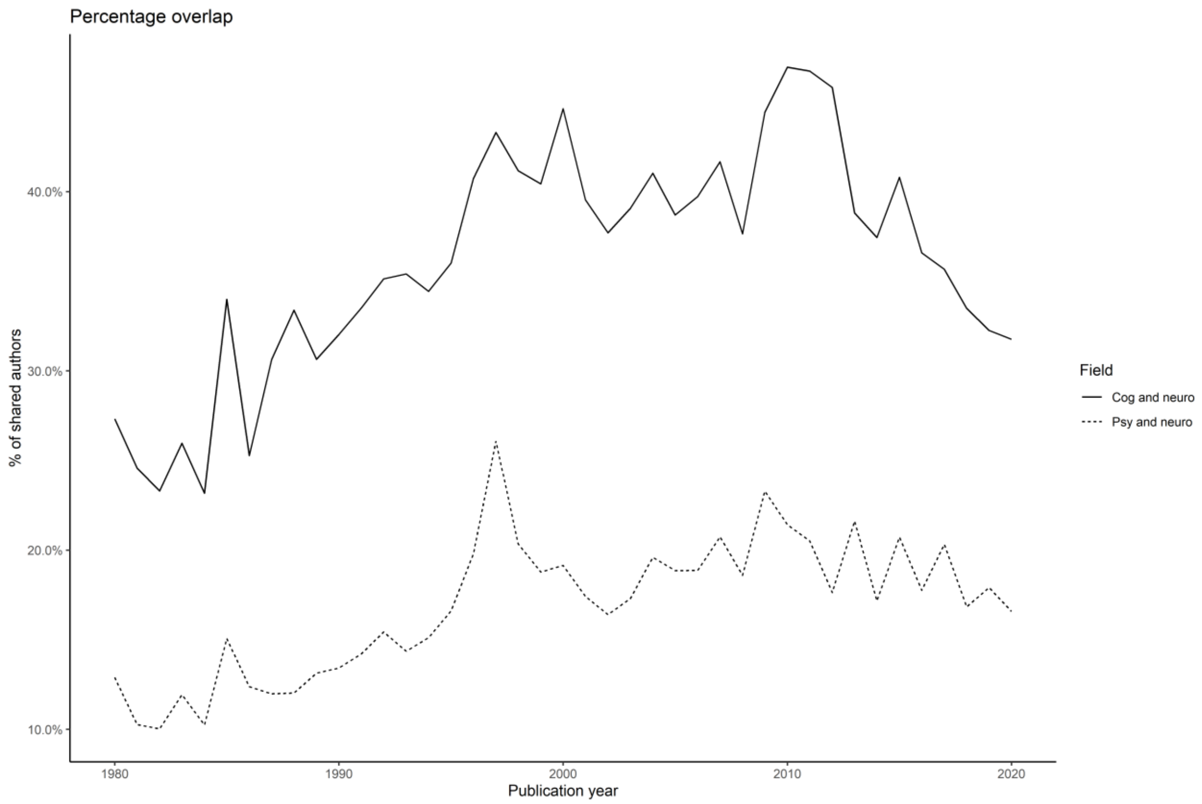

Figure 3. *Shared authors between psychology and neuroscience, and cognitive science and neuroscience.* Percentage of authors who published at least one Neuroscience article (based on Dimensions categories) as well as one Psychology article (dotted line) or one Cognitive Science article (solid line), amongst authors who published at least one paper in the respective fields.

publicly available due to their proprietary nature.

Web of Science data were used to generate the journal bibliographic coupling map in Figure 1 and to analyze citation flows between psychology and neurosciences. Dimensions data were used to produce author statistics.

7.1 Field delineation in WoS

Web of Science adopts a journal-level classification system, meaning the basic unit of the classification is the journal, not the individual publication. Each journal covered by Web of Science is assigned to at least one of the 254 WoS research categories.[56] For the field of psychology, Web of Science includes 11 research categories: "PSYCHOLOGY", "PSYCHOLOGY, APPLIED", "PSYCHOLOGY, BIOLOGICAL", "PSYCHOLOGY, CLINICAL", "PSYCHOLOGY, DEVELOPMENTAL", "PSYCHOLOGY, EDUCATIONAL", "PSYCHOLOGY, EXPERIMENTAL", "PSYCHOLOGY, MATHEMATICAL", "PSYCHOLOGY, MULTIDISCIPLINARY", "PSYCHOLOGY, PSYCHOANALYSIS", "PSYCHOLOGY, SOCIAL". 1028 journals are assigned to at least one of the previous categories. 208 (20%) are assigned to more than one category. No journal is assigned to more than 4 psychology categories. For the field of NEUROSCIENCES, Web of Science includes only the category "NEUROSCIENCES". 473 journals are assigned to this research category. 50 journals (corresponding to 5% of psychology journals and 11% of neuroscience journals) are assigned to both neurosciences and at least one psychology category. Hence, our dataset includes 1451 unique journals that are classified either as psychology or as neurosciences (cf. *Table A1*).

The downside of WoS classification system is that all publications from a certain journal inherit the categories of that journal, so that the within-journal subject variation is lost. This may produce inaccurate classification for multidisciplinary journals covering a wide range of subjects. However, for bibliometric analysis at the field level, such as the one presented in this study, journal-level classification is commonly considered sufficiently reliable and appropriate for science mapping purposes.[57]

7.2 Journal bibliographic coupling map

Science maps are visual representations of the structure and dynamics of scholarly knowledge. They aim to show how disciplines, fields, journals, authors, keywords, or publications relate to each other.[58] For this study, we used journals as the unit of analysis and citation-based relations among journals (journal bibliographic coupling) as links.

Specifically, we generated a vector representation of the 1,451 journals in our dataset by extracting from the WoS the list of journals cited by each journal with the corresponding number of citations received by the citing journal. For instance, if journal A cites journals X, Y, and Z 10, 20, and 30 times, respectively, its vector representation in the cited space is $\mathbf{a} = [10,20,30]$. If journals B cites journals the same three journals but 10, 0 and 400 times respectively, its vector is, accordingly, $\mathbf{b} = [10,0,40]$. In this way, a citing-cited journals asymmetric matrix C was generated, with 1,451 citing journals on the rows citing 30,529 distinct cited journals on the columns. Each element o_{ij} of the matrix contained the citation from journal i to journal j. Clearly, when i = j, the element equals the number of journal self-citations.

Then, the similarity between each pair of citing journals (i.e., the rows of C) was calculated using the association strength coefficient[59], which is defined as:

$$A(c_{ij}, s_i, s_j) = \frac{c_{ij}}{s_i s_j}$$

Table A1. *WoS dataset descriptive statistics*

WoS category	N Journals	N pubs	N references
PSYCHOLOGY	206	331,980	5,875,970
PSYCHOLOGY, APPLIED	131	155,036	2,712,486
PSYCHOLOGY, BIOLOGICAL	37	92,328	1,652,999
PSYCHOLOGY, CLINICAL	197	273,512	5,333,478
PSYCHOLOGY, DEVELOPMENTAL	120	168,776	3,852,113
PSYCHOLOGY, EDUCATIONAL	88	81,492	1,447,034
PSYCHOLOGY, EXPERIMENTAL	146	255,901	5,802,181
PSYCHOLOGY, MATHEMATICAL	18	31,676	361,938
PSYCHOLOGY, MULTIDISCIPLINARY	187	386,069	5,702,875
PSYCHOLOGY, PSYCHOANALYSIS	29	39,876	165,651
PSYCHOLOGY, SOCIAL	110	122,537	2,715,541
NEUROSCIENCES	473	1,568,006	44,047,235

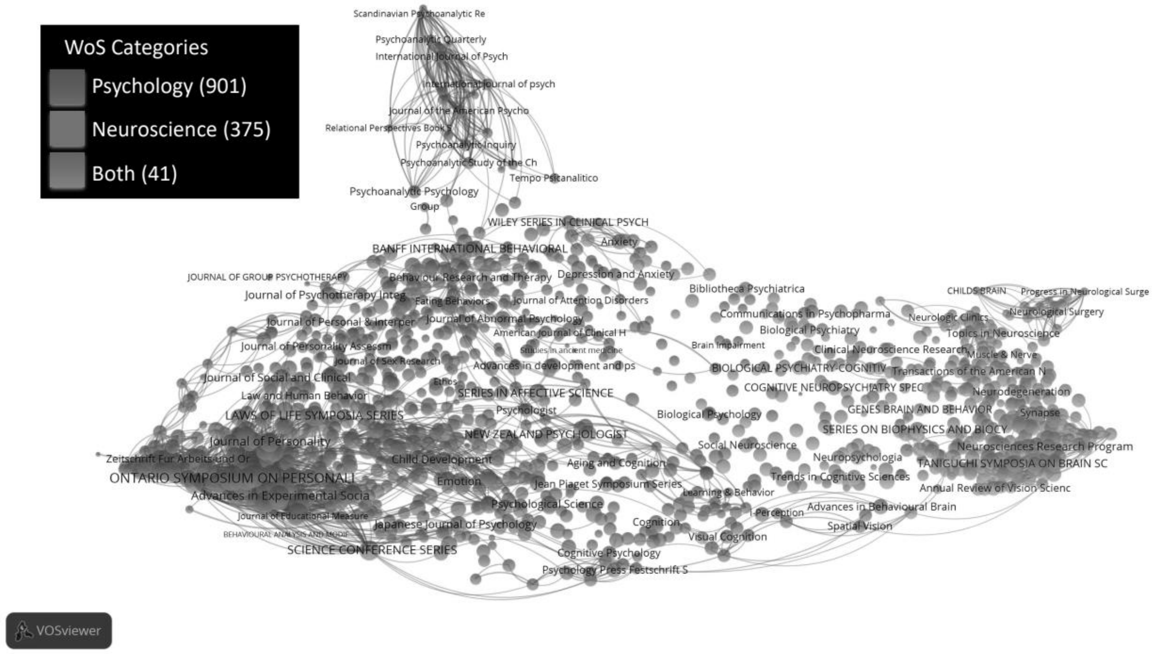

Figure A1. *Journal bibliographic coupling map with colors representing WoS research categories.* Green corresponds to psychology, red to neurosciences, and blue to journals classified in both categories. A colored interactive visualization can be found at https://tinyurl.com/2278t32b

Where c_{ij} is the common number of out-going journal citations shared by journals i and j (i.e., the journal bibliographic coupling strength, which is obtained by the post-multiplication of C for its transpose C^T), s_i the total number of citations given by journal i (i.e., the i-row total in C) and s_j the total number of citations given by journal j (i.e., the j-row total in C).[60] In the example above, the association strength between **a** and **b** is equal to

$$\frac{1300}{60\times 50} = 0.43.$$

The similarities between each pair of journals were arranged in a symmetric similarity matrix of order 1451 × 1451 which was then visualized as a network in Figure 1 using VOSviewer. Nodes represent the citing journals and links their mutual association strength. Note that VOSviewer shows only the biggest component of the network ($n = 1341$ journals on 1451). The VOSviewer algorithm, which is based on a modified version of Multi-Dimensional Scaling,[61] places the nodes of the network by considering their similarity, so that similar nodes are placed closer in the visualization. The size of the nodes was proportional to the total link strengths (i.e., the sum of the weights of the incident links of the node). Lastly, the Leiden clustering algorithm,[62] which is based on the maximization of a variant of standard network modularity, was used to individuate clusters of similar nodes, that were distinguished with different colors (the resolution parameter of the algorithm was set to 1).

Based on the journal titles and our domain expertise, the various clusters or their sub-areas were then mapped to psychology disciplines and labels were overlaid on the map to obtain the final visualization. In the alternative visualization (cf. *Figure A1*), by contrast, the WoS category of the journals is represented by the color of the nodes. Note that the blue journals, i.e., the journals belonging to both psychology and neurosciences, are placed in the red cluster when VOSviewer clustering is used (cf. *Figure 1* in the article). The schema in *Figure A2* provides an overview of the entire methodology.

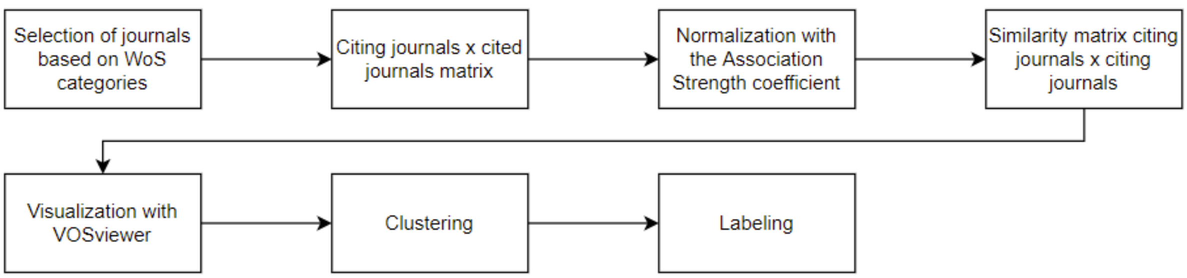

Figure A2. *Methodology for the construction of the journal bibliographic coupling map*

7.3 Field delineation in Dimensions

Unlike WoS, Dimensions uses a publication-level classification system. The system is based on several categorization schemes used by funders and institutions around the world, which are extended to new publications by a machine learning algorithm.[63] In this study, we used the Field of Research (FoR) system, from the Australian and New Zealand Standard Research Classification (ANZSRC), which covers all areas of research.[64] The original FoR system had three hierarchical levels (divisions, groups, and fields) but the implementation in Dimensions uses only divisions and groups. For psychology, we selected the groups "1701 Psychology" (2,421,160 publications) and "1702 Cognitive Sciences" (713,394 publications), whereas we left out "1799 Other Psychology and Cognitive Sciences" because of its residual nature and scarce numerosity (3,670 publications). For the neurosciences, we selected the group "1109 Neurosciences", which contains 2,388,493 publications.

7.4 Author analysis

Compared with WoS, Dimensions provides more accurate data for authorship analysis because it assigns standardized IDs to the authors of publications, creating author profiles. This is crucial for merging variants of the same name and to disambiguate homonyms. The identification of authors is performed algorithmically by Dimensions, using a combination of data including existing person IDs (e.g., ORCID), name variants, affiliation data, research topics, journals, co-authors, and active years. Unfortunately, this procedure does not always succeed. According to the website,[65] Dimensions adopts a conservative strategy for author disambiguation: if there is any doubt about whether two name variants refer to the same person or not, Dimensions does not merge them. However, our experience with the database shows that, especially with very common names, incorrect merging can occur.

Due to these limitations with Dimensions, the author statistics should be taken as including a certain margin of error. Nonetheless, this margin appears to be very small in proportion to the size of the dataset and therefore does not invalidate the statistical analysis.

Notes

1 J. BICKLE, *Revolutions in neuroscience: Tool development*, fn 2.
2 Cf. W. BOONE, G. PICCININI, *The cognitive neuroscience revolution*.
3 Cf. H. SMALL, *Paradigms, citations, and maps of science: A personal history*; M. WANG, L. CHAI, *Three new bibliometric indicators/approaches derived from keyword analysis*.
4 W. BOONE, G. PICCININI, *The cognitive neuroscience revolution*, p. 1513.
5 Cf. D. MARR, *Vision*.
6 H. PUTNAM, *Philosophy and our mental life*, p. 291.
7 Cf., e.g., R. CUMMINS, *The nature of psychological explanation*.
8 Notably cf. S.S. GLENNAN, *Mechanisms and the nature of causation*; P. MACHAMER, L. DARDEN, C.F. CRAVER, *Thinking about mechanisms*; W. BECHTEL, A. ABRAHAMSEN, *Explanation: A mechanist alternative*.
9 Cf. G. PICCININI, C. CRAVER, *Integrating psychology and neuroscience: Functional analyses as mechanism sketches*.
10 Among the most influential philosophers who have endorsed the mechanistic framework as *the* ideal for neuroscientific explanations we find W. BECHTEL, *Mental mechanisms: Philosophical perspectives on cognitive neuroscience*; G. PICCININI, C. CRAVER, *Integrating psychology and neuroscience:* D.M. KAPLAN, *Explanation and description in computational neuroscience*.
11 Cf. D.A. WEISKOPF, *Models and mechanisms in psychological explanation*.
12 Cf. M. SILBERSTEIN, *Constraints on localization and decomposition as explanatory strategies in the biological sciences 2.0*. For a different take on this issue cf. D.C. BURNSTON, *Getting over atomism: Functional decomposition in complex neural systems*.
13 Cf. J.W. KRAKAUER, A.A. GHAZANFAR, A. GOMEZ-MARTIN, M.A. MACLVER, D. POEPPER, *Neuroscience needs behavior: Correcting a reductionist bias*.
14 Cf. N. KRIEGESKORTE, P.K. DOUGLAS, *Cognitive computational neuroscience*.
15 Cf. T. YARKONI, J. WESTFALL, *Choosing prediction over explanation in psychology: Lessons from machine learning*; D. WEISKOPF, *Data mining the brain to decode the mind*.
16 Cf. T.S. KUHN, *The structure of scientific revolutions*.
17 M. PLANCK, *Scientific autobiography and other papers*, pp. 33-34.
18 Gualtiero Piccinini (personal communication) confirmed that he is not committed to a strong Kuhnian reading.
19 Cf. G. PICCININI, C. CRAVER, *Integrating psychology and neuroscience: Functional analyses as mechanism sketches*.
20 W. BOONE, G. PICCININI, *The cognitive neuroscience revolution*, p. 1526.
21 *Ibid*., p. 1509 - emphasis ours.
22 *Ibid*., p. 1513.
23 Cf. e.g., M.P. PAGE, *What can't functional neuroimaging tell the cognitive psychologist?*; M. COLTHEART, *What has functional neuroimaging told us about the mind (so far)?*; T.A. HARLEY, *Why the earth is almost flat: Imaging and the death of cognitive psychology*.
24 J.A. FODOR, *Let your brain alone*, p. 69.
25 Cf. L. M. ANDERSON, *Mining the brain for a new taxonomy of the mind*.
26 For instance, Small recounts how he construed co-cited papers as an approximation of Kuhnian exemplars. Yet, he also points out that Kuhn himself found this maneuver puzzling. Cf. H. SMALL, *Paradigms, citations, and maps of science: A personal history*.
27 Cf. M. WANG, L. CHAI, *Three new bibliometric indicators/approaches derived from keyword analysis*.
28 R. LOOSEMORE, T.A. HARLEY, *Brains and minds: On the usefulness of localization data to cognitive psychology*.
29 Cf. R.A. POLDRACK, *Mapping mental function to brain structure: How can cognitive neuroimaging succeed?*, tab. 1.
30 M.L. ANDERSON, *After phrenology: Neural reuse and the interactive brain;* L.F. BARRETT, A. SATPUTE, *Large-scale brain networks in affective and social neuroscience: towards an integrative functional architecture of the brain*.
31 P.M. CHURCHLAND, *A neurocomputational perspective: The*

nature of mind and the structure of science. However, note that while Churchland mainly insists on the elimination of folk psychology in favor of a neurally inspired taxonomy, the debate on cognitive ontology nowadays is more focused on the categories of scientific (cognitive) psychology. Cf. J. Francken, M. Slors, *Neuroscience and everyday life: Facing the translation problem*; J. Dewhurst, *Folk psychological and neurocognitive ontologies*.

[32] Cf. M.L. Anderson, *Mining the brain for a new taxonomy of the mind*; A. Jannsen, M. Slors, C. Klein, *What is a cognitive ontology, anyway?;* M. Viola, *Carving mind at brain's joints. The debate on cognitive ontology*.

[33] Cf. E. Hochstein, *How metaphysical commitments shape the study of psychological mechanisms*; J.C. Francken, M. Slors, C.F. Craver, *Cognitive ontology and the search for neural mechanisms: Three foundational problems*.

[34] For a nice example of how topic modeling can be used to unravel the structure of a discipline in the context of the history of economic thought, cf. A. Ambrosino, M. Cedrini, J.B. Davis, S. Fiori, M. Guerzoni, M.A. Nuccio*, What topic modeling could reveal about the evolution of economics*.

[35] Cf. R.A. Poldrack, T. Yarkoni, *From brain maps to cognitive ontologies: Informatics and the search for mental structure*; J. Sullivan, *Coordinated pluralism as a means to facilitate integrative taxonomies of cognition*.

[36] H. Kreuzman, *A co-citation analysis of representative authors in philosophy: Examining the relationship between epistemologists and philosophers of science*.

[37] Cf. C.R. Sugimoto, S. Weingart, *The kaleidoscope of disciplinarity*.

[38] Cf. P. Oppenheim, H. Putnam, *Unity of science as a working hypothesis*.

[39] Cf., for instance, J. Bickle, *Reducing mind to molecular pathways*. We suspect that, on theoretical grounds, neglecting the biomedical branches of neuroscience may result in incorrectly interpreting some neural structures and processes in terms of psychological rather than homeostatic functions. For some comments and implications cf. also T. Shallice, *The declining influence of cognitive theorising: Are the causes intellectual or socio-political?*

[40] We employ small caps to refer to WoS categories. A few journals (127 from psychology, 98 from neuroscience, and 9 from both categories) are indexed in WoS database but are not displayed in Fig. 1 as they are not connected to the main component of the network (see the Appendix).

[41] Notice that these clusters were imposed by us based on visual scrutiny of the titles of journals and should not be confused with the sub-disciplines of Psychology provided by WoS database.

[42] Cf., e.g., T. Shallice, *The declining influence of cognitive theorising: Are the causes intellectual or socio-political?*; P. Legrenzi, C. Umiltà, *Neuromania*.

[43] The idea of citations as credits in science can be traced back to Merton and his normative school in sociology of science (cf. R.K. Merton, *The sociology of science: Theoretical and empirical investigations*). It has however been contested by social constructivist approaches to citation theory. For an overview of citation theories, cf. I. Tahamtan, L. Bornmann, *What do citation counts measure? An updated review of studies on citations in scientific documents published between 2006 and 2018*.

[44] M. Planck, *Scientific autobiography and other papers*, p. 57.

[45] Notice that while Dimensions allows for *more* accurate identification of authors compared to other extant databases, it is not completely immune from some types of misidentifications. See the Appendix for a discussion.

[46] https://www.abs.gov.au/ausstats/abs@.nsf/0/4ae1b46ae2048a28ca25741800044242?opendocument.

[47] J. Bickle, *Revolutions in neuroscience: Tool development*, fn 2.

[48] A.-S. Barwich, L. Xu, *Where molecular science meets perfumery: A behind the scenes look at SCAPE microscopy and its theoretical impact on current olfaction*, fn. 11.

[49] Cf. W. Bechtel, A. Abrahamsen, G. Graham, *The life of cognitive science*.

[50] R. Henson, *What can functional neuroimaging tell the experimental psychologist?*, p. 194.

[51] Cf. M. Miłkowski, *From computer metaphor to computational modeling: the evolution of computationalism*.

[52] Cf. M. Carandini, *From circuits to behavior: A bridge too far*.

[53] Cf. L. Laudan, *Progress and its problems*.

[54] For an overview, cf. A.S. Cowen, D. Keltner, *Semantic space theory: A computational approach to emotion*.

[55] The reviewers asked us to explain why we picked WoS rather than Scopus. The main reason is that Scopus has limited temporal depth; data from before 1996 are not reliable (cf. C.R. Sugimoto, V. Larivière, *Measuring research: What everyone needs to know*, chapter 4), such that Scopus contains less useful data than WoS. Morevoer, Scopus was not suitable for the temporal analysis in Figure 2.

[56] The list of WoS research categories is available at the URL: https://images.webofknowledge.com/images/help/WOS/hp_subject_category_terms_tasca.html

[57] Cf. L. Leydesdorff, I. Rafols, *A global map of science based on the ISI subject categories*; L. Leydesdorff, B. Hammarfelt, A. Salah, *The structure of the Arts & Humanities Citation Index: A mapping on the basis of aggregated citations among 1,157 journals*.

[58] Cf. E. Petrovich, *Science mapping*.

[59] Cf. N.J. van Eck, L. Waltman, *How to normalize cooccurrence data? An analysis of some well-known similarity measures*.

[60] Note that, since $S = [0, 1]$, the association strength can be considered as a normalization of the journal bibliographic coupling strength, which corrects for journal size.

[61] Cf. N.J. van Eck, L. Waltman, *Software survey: VOSviewer, a computer program for bibliometric mapping*.

[62] Cf. N.J. van Eck, L. Waltman, *Citation-based clustering of publications using CitNetExplorer and VOSviewe*; V.A. Traag, L. Waltman, N.J. van Eck, *From Louvain to Leiden: guaranteeing well-connected communities*.

[63] For more details, see https://dimensions.freshdesk.com/support/solutions/articles/23000018820-which-research-categories-and-classification-schemes-are-available-in-dimensions-

[64] For more details, see https://www.abs.gov.au/ausstats/abs@nsf/0/6bb427ab9696c225ca2574180004463e. Note that Dimensions implements the 2008 version of the FoR system and not the 2020 versions.

[65] https://plus.dimensions.ai/support/solutions/articles/23000018779-how-are-researchers-unified-disambiguated-in-dimensions-

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