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Abstract

This chapter examines the criteria exposed by Stephen Jay Gould's original paper on *just-so stories* to sustain such a charge. I show that Gould's concerns were neither directed to narrative explanations nor were they ineluctably linked to their narrative quality. Then I analyse how advocates of narrative science have met the challenge. I identify two basic defensive approaches: the vindication of explanatory narratives in cases where the historical, contingent and causally complex nature of the phenomena demand a narrative approach and an *unveiling* strategy showing how there's a narrative *behind* each law-like generalization or nomological explanatory formula. The chapter's concentration on the argumentative moves of the discussants helps clarify their positions. Moreover, the argumentative quality of their object of study (scientific reason-giving practices) is also emphasized. I claim that the *dialectical requirement* of openness to collective survey and discussion is what may prevent *just-so charges* for any kind of explanatory model.

21.1 Introduction

The recent interest shown by philosophers of science and scholars in related fields concerning the narrative qualities of our scientific explanatory practices has not sufficiently addressed a widespread reluctance to recognize narrative's epistemological significances. On many occasions, this reluctance is marked by the derogatory use of the '*just-so story*' label (Gould 1978; Gould and Lewontin 1979) to signify that narrative explanations – or other narrative reason-giving practices (cf. Olmos 2019) – do not meet the epistemic criteria required for scientific appraisal.

In many philosophical *forums*, it is now standard to present a stark opposition between allegedly genuine scientific explanations – invoking a well-established and well-delimited, ideally law-like account, which is amenable to formalization, perhaps including a causal mechanism¹ – and *just-so stories* – reconstructive,

¹ Although there are important differences and entrenched discussions between philosophers of science who emphasize the role either of laws, of formalizable statistical relations, or of

typically untestable, conjectures of what *just-in-fact* may or may not have happened to cause a particular phenomenon. However, the problem with the widespread use of this opposition is that it tends to create a strong and somewhat easy association between the noun *story* and the qualification *just-so*, so that every attempt to approach explanatory and justificatory tasks through narrative form within the sciences is easily and cursorily dismissed with the *just-so story* derogatory term. In the worst cases, the use of this summary label even tends to prevent further discussion, acting as a dialectical blockade.²

In this chapter, I examine the roots of the *just-so charge* by going back to its now classical source in Stephen J. Gould's original paper (1978) from which it spread in the history and philosophy of science (HPS) field as a negative evaluative term. As it is well known, in choosing this denomination, Gould was inspired by Rudyard Kipling's collected children's tales, *Just So Stories* (1902), containing twelve whimsical etiological fables.³ Gould tried, thus, to convey to the general public the idea of the unscientific and boldly imaginative nature of his own target examples, namely evolutionary biological accounts.⁴

However, a careful reading of Gould's piece shows that his concerns were neither directed to narrative explanations nor were they, in any case, ineluctably linked to their potential narrative quality. For Gould, assessing an allegedly scientific account as a *just-so story*, was to issue a negative evaluative judgement

describable causal mechanisms in the conformation and appraisal of scientific explanations, all these conceptual possibilities share a ring of respectability within mainstream programmes of study that discussions on narrative models have not yet fully attained. The concerns of the so-called 'new mechanists' (Craver and Tabery 2019) seem closer to narrative science discussions than are the more traditional emphases on nomological and Bayesian models. And yet, important suggestions made from the narrative ranks might, as Crasnow (2017) shows, improve and qualify the mechanistic approach.

² As several scholars have noticed, even if this dichotomy and its association with narrative explanatory models does not usually appear as such in published papers on epistemology or philosophy of science, it is still a widespread prejudice that is academically very effective. See, for example, Currie and Sterelny (2017: 16 n. 7): 'These complaints are not often found in the published literature, but both of us have met it *regularly* in conversation, and one of us *regularly* in referee's reports on his narrative-based explanations of hominin evolutionary history' (my emphasis on '*regularly*').

³ Available at www.gutenberg.org/files/32488/32488-h/32488-h.htm. Kipling's work includes stories such as 'How the Rhinoceros Got His Skin' or 'How the Leopard Got His Spots' and tries to respond to children's typical pressing questions by providing fantastic accounts of how a certain individual of a species (the fable's protagonist) got a particular trait that's now common to all in the tradition of the etiological fable. One of the most renowned tales in the book, 'The Elephant's Child', stands out as interestingly self-referential regarding the book's own theme, as the protagonist child elephant, full of 'satiabile curiosity' (as Kipling's child-like spelling runs), gets its unattractive but very useful trunk precisely for asking questions and being inquisitive.

⁴ The success of Gould's felicitous denomination is obviously also due to the coincidence between Kipling's themes and evolutionary biological research. On the narrative difficulties of making particular evolutionary accounts, see J. Beatty's paper (Chapter 20). A recent paper by Hubálek (2021) on *just-so stories* focuses precisely on the central role of the particularities of evolutionary science in the configuration of this topic.

regarding its claim to epistemic relevance (as being a bold and so-far unwarranted conjecture) or to point out its way of presentation as avoiding further discussion and further testing (being an unfalsifiable, self-contained hypothesis).

These conditions, I claim, should not be confused or equated with the discursive and causal narrative quality of an explanatory scheme or excluded from the realm of more classically understood explanations that could also, in the mentioned senses, be *just-so* as well. The narrative quality of many of our scientific reason-giving practices is not, in and of itself, a way to avoid the identification of causal relations (even mechanisms, depending on how we define them; cf. Crasnow 2017) or to exclude further discussion or testing (Al-Shawaf 2019). On the contrary, it might be part of what is hypothesized of certain scientifically interesting phenomena, both in the sense of making them dependent on long-term processes (as witness the timely *historicization* of certain natural or social enquiries, at a certain point in their development) or on a complex, highly contextual and somewhat indeterministic causal web that's *better* rendered in a narrative form.

In what follows, I will carefully examine and analyse Gould's points and then come back to current discussions regarding the use of narratives and narrative reason-giving modes within the sciences where advocates of the epistemic relevance of the topic have felt the need to meet the challenge of the *just-so charge*. I identify two basic defensive approaches. One is the vindication of the use of explanatory narratives in cases where the historical, contingent and causally complex nature of the phenomena involved demands an approach that would avoid the strictures of classical models. When these conditions obtain, scientific narratives might be *less just-so* (i.e., less bold, less self-contained) than their too-narrowly understood mechanistic or easily formalizable rivals. This is basically what is claimed about their case studies by Crasnow (2017) and Currie and Sterelny (2017). Nonetheless, authors working along this line, usually propose some kind of collaboration or integration between these different epistemic modes and tools.

The second kind of vindication⁵ of narrative science follows, instead, an unveiling (somewhat genealogical) strategy, hinting at a deeper level of narrativity. Scholars taking this approach (Richards 1992; López Beltrán 1998; Rosales 2017) try to show how there's a narrative – or at least a narrative kind of rationality, in W. Fisher's sense (1989) – behind (or before) each law-like generalization or nomological explanatory formula. This kind of narrative, that depicts and delimits the scenarios in which the particular nomological expression might acquire some sense and specifically become useful for drawing scientific conclusions,⁶ is usually obscured and disregarded in its current application as a validated theory. However, it may always re-emerge when the formula comes under scrutiny as an explanatory

⁵ Which, as can be seen, can be traced back to the early 1990s and, thus, antecedes the current discussions of the 'new mechanists'.

⁶ Toulmin's (1953: 51–93) characterization of 'scientific laws' not as traits of nature but as restrictedly applicable and practical inference rules might be of help here.

principle (sometimes, as in Rosales's case study, in comparison with rival theories), which makes it a crucial part of its deep understanding.⁷

As in previous contributions (Olmos 2018; 2019), I approach all these topics with the tools and conceptual framework of argumentation theory, that takes into account the argumentative nature of our discursive, explanatory and justificatory practices in terms of reason-giving, reason-asking and reason-discussing activities. The philosophers of science whose works I examine support their claims with (obviously non-demonstrative) reasons and concentrating on their argumentative moves in these discussions helps clarify their positions.⁸ But we must also take into account that their very object of study (scientific explanation and justification) is also of an argumentative, reason-giving nature. The way the grounds of *this* argumentative activity – i.e., scientific justificatory or *forensic* practice, in John Woods's (2017: 143–144) terms – should be assessed is what is finally at stake in philosophical discussions regarding the use of narratives in science and their alleged vulnerability to the *just-so charge*.

A final step that the argumentative approach might help us take is based on the *naturalistic* assumption that scientific argumentative activities are already intrinsically normative and evaluative in nature, so that, even if philosophers might discuss the criteria for the acceptability of the concerned claims and *explanantia*, there is already an intra-scientific evaluative activity going on, whose most basic rule, well beyond the strictures of any aprioristic model, is the *dialectical requirement* of (a posteriori) openness to collective survey and discussion.⁹

A well-understood *just-so charge* will have more to do with possible violations of this basic rule than with the textual and formal characteristics of proposed and supported scientific arguments and explanations.

21.2 The *Just-so Charge*

As already mentioned, the now classical reference for the *just-so charge* is palaeontologist and evolutionary biologist Stephen Jay Gould's (1978) critical article on what he saw as the excesses of certain trends in sociobiology, published in the *New Scientist* under the title 'Sociobiology: The Art of Storytelling'. For Gould, the question was whether and when evolutionary

⁷ As Mary Morgan has defended (2001: 369), 'the identity of the model is not only given by the structure (or the metaphor), but also the questions we can ask and the stories we can tell with it'.

⁸ A genuine *locus classicus* for the argumentative (as opposed to demonstrative) nature of philosophical discourse is Friedrich Waismann's 'How I See Philosophy' (Waismann 1968: 30).

⁹ According to Hansson (2017), for example, a minimal criterion of science would be: '*Science is a systematic search for knowledge whose validity does not depend on the particular individual but is open for anyone to check or rediscover*'.

scientists (sociobiologists, particularly) were being excessively speculative and overconfident with their imaginative accounts about the historical origins of the traits they studied.

Gould's target cases included, in the first place, certain explanations of animal behaviour that he analysed as solely based on their 'consistency with natural selection' or 'adaptationism' (Gould 1978: 531). As his first example, Gould picked up David Barash's explanation of the greater aggressiveness of male mountain bluebirds towards other males approaching their nest before rather than after eggs have been laid. According to Gould, Barash's proposed *explanans* to this phenomenon (that he documented, also according to Gould, with rather scarce data) was that this behaviour was advantageous (and so adaptive) as long as it reserved aggression (a costly attitude) to periods where the male was not yet sure to have passed on his genes. This was 'consistent with the expectations of evolutionary theory' (Gould 1978: 531) and that was nearly all there was to it.

Gould's criticism of this case included demands for more data and more tests, exploration of alternative (also testable) explanations and, most significantly, a call for certain control or restraint on the part of the scientist in drawing further conclusions based on his hypothesized *explanans*. Barash was particularly due criticism as he had gone so far as to suggest this was also a way to understand human foibles regarding adultery (Gould 1978: 531). Barash's *hasty jump* from mountain bluebirds to humans brought him near the realm of *sociobiology* and so allowed Gould to introduce his real main target.

This was Gould's illustration of a just-so story where the story-like quality of the negatively evaluated *explanans* was not really in its narrative nature (which was somewhat missing here), but rather in its far-fetched imaginative play. So Gould's complaints were directed neither against reconstructive *historicized* explanations that, he assumed, were the goal of evolutionary theory in general, nor against the indication of complex (multifactorial), entangled (non-linear) and somewhat indeterministic causal webs behind a target phenomenon. On the contrary, he explicitly opposed panselctionism or panadaptationism and advocated a less rigid version of natural selection that would 'grant a major role to other evolutionary agents (genetic drift, fixation of neutral mutations, for example)' (1978: 531).¹⁰ Although he displayed some irony about the changing styles of evolutionary stories presented in biology – showing the workings of

¹⁰ Professor J. Huss (Chapter 3) kindly suggested that a way to understand the place of this piece within Gould's maturing conception of evolutionary theory is as belonging to a transitional stage between his attempts at a nomothetic and computable approach to palaeobiology (Gould et al. 1977) – that Huss (2009) has studied as the 'MBL Model' – and his definitive emphasis on historicity, contingency and causal pluralism exposed in his classical *Wonderful Life* (Gould 1989). See also Turner and Havstad (2019).

a favoured kind of adaptive factor as it gets theoretically trendy – he seemed to assume this feature as a rather inevitable condition of scientific development.

Now, what was exactly Gould's problem with sociobiology in particular? Regarding the aforementioned mountain bluebirds example, Gould said that it presents 'a perfectly plausible story that may well be true. I only wish to criticise its assertion without evidence or test, using consistency with natural selection as the sole criterion for useful speculation' (Gould 1978: 531). This is the first important thing: it is not a question of the stark unsuitability or unacceptability of a certain *methodology*, in the sense of a certain way of supporting a scientific content (a kind of reason) but of its *sufficiency* to establish its conclusion. That, according to Gould, this was happening in sociobiological explanations of human behaviour allegedly *more than* in other areas of evolutionary theory could be attributed to two additional difficulties met by this particular disciplinary approach: the little observational evidence available,¹¹ and, more significantly for our purposes, the *reductionistic* option for one specific kind of causal explanation (biological adaptive selection) for such highly complex phenomena as human behavioural traits whose etiological history was surely more entangled than that.¹² Along with this last point, the strictly selectionist sociobiological historical explanations of human behaviour Gould had in mind could be charged with the *just-so* label *precisely* for being more reductionistically mechanistic (based on the single principle, 'if adaptive, then genetic') than assumedly and sophisticatedly *narrative!*

The derogatory label, moreover, was the more emphatically attributed by Gould as he perceived that certain ideas taken from the theories advanced by sociobiology were currently being used to uphold practical and political implications as based on what he saw as their *hasty conclusions* (Gould 1978: 532). So here a certain *pragmatist modulation* of what seemed to start as a purely epistemological concern comes to the fore.

To sum up, according to my analysis, Gould suggested three (not completely independent but yet distinguishable) criteria to be taken into account for a *just-so charge* and none of them has to do with either the explanatory *historicization* of phenomena (their being explained not by the workings of constant laws of nature but by the particular detailed history behind them in conditions that may not be repeated) nor with the suggestion that the phenomena involved might be better understood contextually, taking in account the complexities of its entangled causal web, rather than in isolation. Quite the opposite, I would remark.

¹¹ As the saying goes, 'behavior doesn't fossilize' (Kurzman 2012).

¹² Including 'cultural evolution', with rather different causal workings, according to Gould (1978: 533).

These three *criteria* amount to: (a) a charge of *theoretical justificatory insufficiency*; (b) a charge of *unwarranted reductionism*; and (c) a charge of *prescriptive justificatory insufficiency*. Let us analyse these three points separately.

Gould rejects the presentation of a particular historical, reconstructive explanation (not necessarily presented through a fully fledged *narrative*) as solely supported by its inner plausibility (understood as ‘consistency with evolutionary expectations’). This would be a charge of *theoretical justificatory insufficiency* and can be mitigated by additional future evidence that would still use that initial plausibility as one of the reasons adduced in favour of the supported hypothesis or by a more humble presentation that would consciously advance it as a plausible hypothesis and offer it to the scientific community, assuming that a lot of research is yet necessary to issue a judgement on it – being still a valuable contribution for some reason (e.g., its novelty).

So this criterion (a), demanding additional robustness for establishing scientific theories, is not much more than a reminder of the collective rules of scientific research, organized scepticism, public scrutiny and assumed fallibilism. This much has been acknowledged by other scholars responding to Gould’s piece: ‘The goal should not be to expel stories from science, but rather to identify the stories that are also good explanations’ (Kurzban 2012). What Gould is asking for here is just ‘more evidence’.

Gould’s complaint may be, then, argumentatively modelled as requiring for the sought-for conclusion (the assertion of the hypothetical reconstruction) a *conjunction of additional arguments* (Marraud 2013a: 59–62) that, significantly, does not have to drop at all the initial one. Figure 21.1 represents such a conjunction of reasons justifying an evolutionary hypothesis, presented this time through a narrative, neither *solely* on the basis of its *narrative coherence* nor disregarding such coherence’s contribution in supporting the conclusion.

I have used similar diagrams in previous works (Olmos 2019; 2020a). They are based on Marraud’s (2016) interpretation and development of Toulmin’s model (1958). In addition to the representation of co-orientated reasons (signalled by the connective ‘besides’), they combine the use of justificatory reasons (allied with connective ‘so’) and explanatory reasons (allied with connective ‘that’s why’) and, whenever needed to clarify both kinds of inferential steps, either justificatory or explanatory *warrants* (in Toulmin’s sense) are provided in side boxes. Gould’s concern here is structurally similar to Ian Hacking’s criticism of the sufficiency of *abduction alone*, i.e., of the explanatory power of a hypothesis including an existential posit, to establish a ‘realist claim’ regarding the theoretical entity posited by it (Hacking 1983: 271–272). As I have shown elsewhere (Olmos 2018: 50), Hacking’s suggestion might be argumentatively modelled as requiring for the sought for conclusion (the assertion of the hypothesis) a *conjunction of*

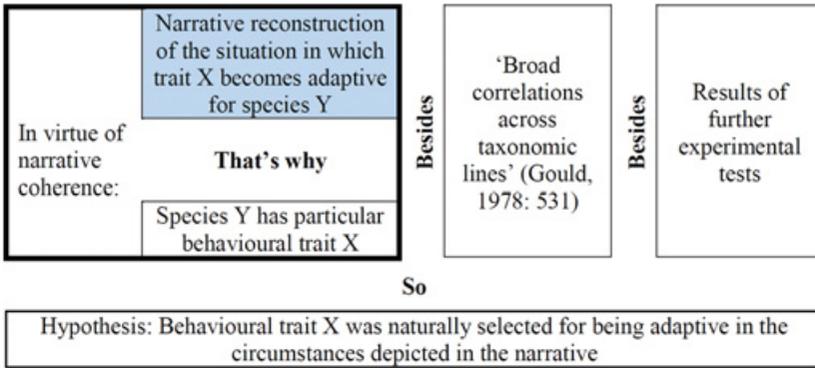


Figure 21.1 Conjunction of reasons justifying an evolutionary hypothesis

arguments, specifically including further experimental evidences amounting to the detection and manipulation of the posited entity.¹³

Gould also criticized the concentration on just one kind of causal mechanism (strictly understood natural selection in terms of adaptiveness is, as could be expected, Gould's *usual suspect*) on which to base a historical account of a complex phenomenon. This would be a charge of *unwarranted reductionism*, that is rather more serious as it might prevent rather than encourage research along other lines and easily provide a sense of overconfidence in a particular kind of explanation, precisely for its neat identification of one well-delimited responsible mechanism.

Criterion (b) is Gould's main epistemological point – although criterion (c) may be his main motivation. It is more a caution against *selectionist reductionism* (i.e., *pansélectionism*) than any other thing, which is consistent with his well-known position in evolutionary biology (Gould and Lewontin 1979). The caution works this time as the conclusion of an *a fortiori* argument (Marraud 2013b) based on his own reservations with pansélectionism in accounting for biological traits. We could reconstruct this *a fortiori* rationale behind Gould's case (see Figure 21.2).

This reconstruction, chosen for its clarity at this point in the discussion, could be much more refined if we take into account that it really works

¹³ Laith Al-Shawaf has recently engaged in a defence of evolutionary psychology (Al Shawaf, Zreik and Buss 2018; Al Shawaf 2019) trying to respond to *just-so stories* charges, along lines rather coincident with my own analysis of criterion (a). The *just-so charge* would be misplaced when evolutionary psychologists do not only concoct and present their storied hypotheses but continue their experimental research and generate and test novel empirical predictions. Al-Shawaf claims that most published research in evolutionary psychology provides evidence and arguments *enough* along these lines and cannot be accused of presenting theories as *just-so* accounts.

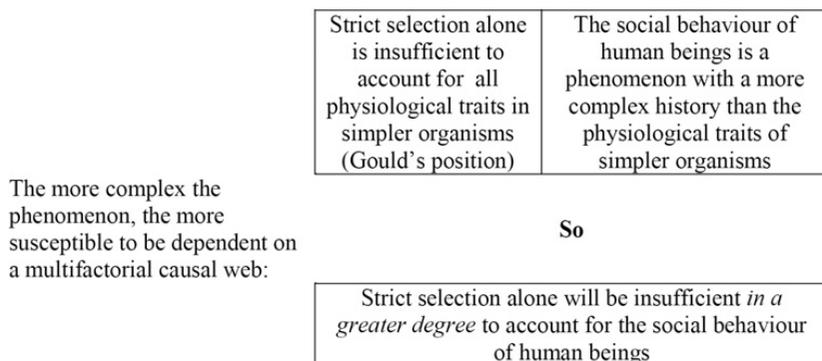


Figure 21.2 A fortiori rationale behind the charge of unwarranted reductionism

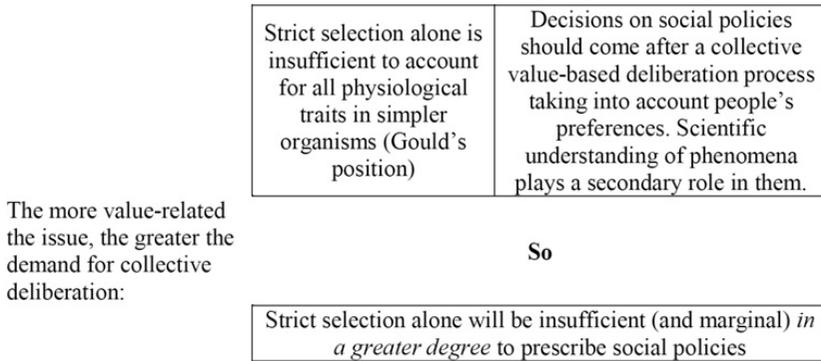
meta-argumentatively (Marraud 2013b: 10–12) as a comparison between two reason-giving acts (two explanatory accounts) that are connected by a *scalar topos* of the kind ‘the more . . . , the more . . .’ acting as the warrant of the comparison. This topos, makes possible that the characteristics attributed to one of these reason-giving acts (its being *insufficient* or *inadequate* in a case) be transferred *in an increased measure* to the compared one on the basis of some condition that places them in different positions on the comparison scale.¹⁴

So the problem this time with sociobiology’s ‘art of storytelling’ is that it has *overconfidently* picked up a scientifically well-defined and well-understood *mechanism* (the ‘selection of beneficial traits’) and used it as a guide to reconstruct (allegedly too simplistically) the causal origins of some of the most complicated and intractable phenomena available. The *just-so charge* arises here as a charge against misguided scientificism, not against *narrative science*.

Gould finally warns us against a perceived as hasty use of theoretical results from natural science to support practical (even political) decisions. This would be a charge of *prescriptive justificatory insufficiency* that should be weighed in its own merits and according to pragmatic reasons modulated by considerations of risk (among other things).

The significance of criterion (c) (*prescriptive insufficiency*) can be understood (see Figure 21.3) as based on an additional a fortiori line of reasoning: ‘if strict

¹⁴ The meta-argumentative variety of a fortiori arguments is a scalar version of the meta-argumentative interpretation of analogy (cf. Woods and Hudak 1989) allowing the *simple transfer* (with no increase) of the characteristics attributed to an argument to another argument on the basis of their similarity.



The more value-related the issue, the greater the demand for collective deliberation:

Figure 21.3 **A fortiori rationale behind the charge of prescriptive insufficiency**

selection alone is not enough to account for the actual social behaviour of human beings, how could it be enough to prescribe social policies?’ I have again to thank Professor Huss for reminding me of Gould’s membership of the Harvard left-wing group ‘Science for the People’ that held strong positions against the use of scientific results in justifying oppressing policies. This is consistent with my contention that criterion (c) was Gould’s main motivation in making his case against sociobiology.

As already said, none of these hints at assessment criteria has really much to do with the *story-telling quality* of assumedly *narrative* models of explanation. After defending the scientific credentials of evolutionary psychology (see n. 13, above), Al-Shawaf asks himself ‘why do so many people persist in the notion that evolutionary psychological hypotheses are just-so stories?’ (Al-Shawaf 2019; cf. Al-Shawaf, Zreik and Buss 2018: 9). He attributes this mainly to the inescapable fact that evolutionary psychology (as astrophysics, cosmology and geology for that matter) has a central historical component and that historicity tends to be associated with untestability.

So it seems that the noun *story* easily attracts the charge *just-so*. Even if the *historicity* of the phenomenon addressed by many scientific disciplines is not a contested issue, the prejudice against *storied accounts* remains strong enough in many *forums* so as to extract protestations of scientific soundness in those addressed. Scholars interested in narrative science and narrative models of scientific justificatory practice feel, therefore, compelled to answer *just-so charges* even if those charges, when carefully examined, have not much to do with the particular characteristics of narratives and may even be based on just the opposite traits.

21.3 Defenders of Narrative Science Meet the *Just-so Charge*

Among the papers included in the 2017 special issue on ‘Narrative Science and Narrative Knowing’ of *Studies in History and Philosophy of Science*, edited by Mary S. Morgan and M. Norton Wise (Morgan and Wise 2017), it is significantly Sharon Crasnow’s (Crasnow 2017: 6–13) and Adrian Currie and Kim Sterelny’s (Currie and Sterelny 2017: 14–21) papers that make an explicit mention of the *just-so story charge* and try somehow to respond to or minimize it.

These two papers stand out in the collection as taking a meta-methodological approach while analysing their respective case studies. Both engage, particularly, in an epistemological appraisal of narratively dense and detailed accounts as opposed to certain efforts to base explanations regarding historically problematic phenomena (the 1898 Fashoda colonial incident, or the evolutionary development of human cooperation, respectively) in too restrictedly understood causal mechanisms or trajectories, amounting to formal models of explanation.

For these authors, narratives help better explore and understand the very causal relations expressed by those allegedly explanatory formal formulae, their contingent nature and the alternatives available at each historical turn. Both make reference to John Beatty’s (2016) ideas about *what narratives are good for*, namely dealing with contingencies, alternative possibilities and the particulars of historical turning points, that are still the focus of Beatty’s own contribution to the aforementioned special issue, in which he states that: ‘Narratives are about not only what actually happened, but also what might have’ (Beatty 2017: 31).

But what both papers finally depict in their case studies is not really a situation in which a narrative account of some phenomenon opposes a rival (in the sense of theoretically divergent) *narrowly mechanistic* account of the same phenomenon. The point is rather that certain kinds of identified or hypothesized causal links or mechanisms are *better* understood (explored and discussed) under a narrative rendering than under the crystallized mode of a formal formula or strict inference licence. So, as I will emphasize in the next section, the opposition (or comparison) is not so much *mechanisms vs narratives* but *narratives vs formalizable laws*.

Sharon Crasnow’s paper focuses, in particular, on a case of historical political science, a discipline that, in principle, already accepts its narrative nature. Nevertheless, recent philosophical discussions regarding the requirement for scientific explanations to be based on causal mechanisms, emphasizing, moreover, the use of individual case studies as devices for *causal process tracing*, tend to be read as bringing social scientific disciplines to a point in which narratives might be *dissolved* in favour of a bounded search for discrete pieces of evidence that allow the operation of such allegedly well-identified, well-delimited mechanisms.

This is what Crasnow calls the ‘inferential reading of process tracing’¹⁵ that she opposes to the enriched use of narrative accounts of *those same* mechanisms and processes. Her claim is that narratives in political science might, in fact, help tracing causal processes and identifying mechanisms in a *better* way than restricted inferential-type readings, precisely because narratives focus on exploring and discussing contingencies and alternatives (Beatty 2016) and, thus, help making credible and understandable the inner and detailed workings of the very causal connections involved. Contrariwise: ‘process tracing as a search for diagnostic pieces of evidence fails to capture the way that a mechanistic account seeks to address the inter-relationship of the parts in a way that narrative elements of case studies can’ (Crasnow 2017: 8).

Crasnow acknowledges that narrative approaches to science have been challenged with the *just-so charge* that she equates with the notion of biased *cherry picking* (i.e., suppressing disconfirming evidence or biasedly selecting confirming evidence). Her suggestion to avoid *both* the problem and the charge is finally to *add substance*, detail and discussion of alternatives to narrative accounts, making them, if anything, even *more narrative*.¹⁶

One worry often raised about the use of case studies is the idea that it may devolve to cherry picking or just-so stories. This is indeed a concern but one that can be addressed by requiring that all of the relevant details of the case be considered and not just those that are relevant to the favored hypothesis. In order to assure that these details are addressed, alternative hypotheses – different ways that the story could have gone, different paths that could have been taken, different mechanisms through which the case can be understood – need to be explored. (Crasnow 2017: 10–11)

The four virtues that Crasnow ultimately associates with the narrative discussion of causal links or *process tracing* (i.e., closure, connectivity, elimination of alternatives and examination of counterfactual options) (Cranow 2017: 10–11) seem to be doing the work of avoiding too simplified accounts based on the biased selection of a restricted kind of evidence. This is more than consistent with Robert Richards’s (1992: 41–42) suggestion that narratives (as opposed to *nomological* models of explanation) are the adequate vehicle for ordering and

¹⁵ On the concept of ‘process tracing’, see Andrew Hopkins’s and Sharon Crasnow’s chapters (Chapters 4 and 11).

¹⁶ Equating the *just-so charge* to a charge of *cherry picking* is a charitable (and dialectically fruitful) choice as it concedes that a narrative account (as well as any other scientific account) might be in need of further and more detailed justification regarding unmentioned or unqualified evidence (i.e., additional arguments). It would be part of a normal scientific evaluative discussion to check any account for *cherry picking*. As I have already said, sometimes the *just-so charge* tends to work in a more prejudiced and dialectically blocking way against certain modes of presenting scientific accounts. Responding to such attempts at blockade by charitably acknowledging that one is being asked to make a better case is a rather reasonable strategic move.

weighing (downgrading and emphasizing) the contributions of possibly many different causal links that could be invoked to account for a historically situated event.

Currie and Sterelny (2017) conduct an even bolder and more committed defence of the benefits of scientific story-telling. Their recipe, though, for obtaining such benefits without incurring *just-so charges* is a bit different. First of all, they are prepared to *defend speculation*, not anymore a vice whenever it yields the appropriate kind of Lakatosian fruitfulness (Currie and Sterelny 2017: 16). It is, precisely, on account of the value attributed to such fruitfulness that they acknowledge that Gould and Lewontin were probably right (or at least consistent) when criticizing strictly adaptationist hypotheses, because, as I have already remarked, these may tend to prevent rather than encourage research along other lines:

Gould & Lewontin's complaints about adaptationist reasoning is in part clarified by this distinction: the charge of 'just-so' storytelling is in effect the charge of idle speculation: adaptationist hypotheses fail to open new investigative routes and actively discourage them (here is not the place to consider whether such a charge is plausible). (Currie and Sterelny 2017: 17 n. 11)

A second step in Currie and Sterelny's defence regards *coherence* as an epistemic virtue. Mere *internal coherence*, so to say, might be insufficient – although not thereby *negligible* in this respect – to support a historical reconstruction of the causal web leading to an explanandum-phenomenon. But, insofar as such a reconstruction is pressed (by scientific method and community) to cohere with all kinds of constraints, issuing from material discoveries, other reconstructions, general theories, etc. such *extended coherence* becomes a noticeable achievement. This idea may be understood as amounting to appraising *consilience* as a kind of master scientific virtue (Weinstein 2009) or, alternatively, as demanding from us a sufficiently flexible, assumedly multifactorial and open-ended, model of scientific argumentative assessment (Olmos 2020b) in which the contributions of different strategies (some possibly more narrative than others) may be weighed and, at least to a certain point, harmonized.

This last idea is much in line with Currie and Sterelny's final suggestion that their defence of scientific storytelling aims more at integration than substitution. The virtues and benefits of narrative approaches should *combine* with the virtues and benefits of formal models and the possible shortcomings of each of them be compensated by the other. And this is so because, as they try to show (although they do not express it with these words), some kind of *just-so charge* could be attributed to both. There might be *just-so stories* but there are also *just-so formal explanatory models* insofar as they unwarrantedly claim to be self-standing explanations.

This is what purportedly happens in their discussed example of a neatly modelled *threshold-dependent* explanation of the emergence of punishment in early human communities (sociobiology again). Such a clean, self-standing explanation, leaning on the pristine comprehensibility of the mechanism invoked is exposed as a *just-so* attempt, insofar as it is not taking into account enough contextual constraints as the emergence of other factors leading to human cooperation. What's missing here (according to Currie and Sterelny) is a good integrative narrative that's lost in the *decompositional* strategy of formalized (*narrowly mechanistic*) models:

Highly complex explananda like the evolution of human cooperation are resistant to approaches which depend solely on the decomposition and abstraction which enables modellers to probe aspects of constituent dynamics in isolation. For highly complex, multi-factorial, and multi-stage causal trajectories there are no master-models to be had, and so we must instead combine narratives and models, allowing us to navigate between the trade-offs generated by complexity. (Currie and Sterelny 2017: 20)

No wonder that Currie and Sterelny come to agree with Gould and Lewontin's complaints. Their *just-so* criticism, even if it was coupled with the noun *story*, was not directed towards the *storied* character of the accounts they criticized, but to their overconfident self-standing reliance on just one supposedly well-known and well-comprehended natural mechanism.

21.4 The Narrativity behind Nomicity

A somewhat different strategy to appraise narrative models of scientific explanation and justification is the one that exploits a kind of *genealogical* argument based on the idea that there's a narrative (or at least a narrative kind of rationality; cf. Fisher 1989) *behind* (or before) each law-like generalization or nomological explanatory formula that – even if it may be rather opaque and disregarded in its current application as a validated theory – may always re-emerge when the formula comes under scrutiny as an explanatory principle.

The point here is not that there's a *story* behind its *establishment* that may make it more understandable or even be part of its justificatory framework. These kinds of ideas would pertain to either the history of the discovery and acceptance of particular scientific laws and theories or more generally to what I have called the narrative account of scientific experimental and research activities (Olmos 2020a; cf. Meunier's paper in this volume on 'research narratives', Chapter 12). In this sense, there are recent significant case studies of how scientists themselves use a narrative rendering of their *interventions* and experiments (e.g., Mary Terrall's (2017) account of Réamur and Trembley's 'tales of quest and discovery' or M. Norton Wise's (2019) work on Faraday's series). However, this is not what I specifically want to focus on here.

The claim I want to examine is rather that any scientific law-like generalization would somehow depict and delimit the scenario of its own validity and applicability as based on considerations regarding the possibilities of isolating natural phenomena and letting them develop in a controlled setting and making them solely dependent on a relevant set of variables. Such scenarios and the assumptions that make them plausible and assessable would be *narrative* in the sense of describing what can be expected of either a spontaneous or a more or less controlled course of events. Invoking and exposing them in their narrative detail would be just what's needed whenever those generalizations, instead of being *just applied*, are discussed and weighed against alternative ones – which is something scientists involved in original research, as opposed to science teachers and appliers of scientific current theories, are expected to do.

Several authors have defended the interest of approaching and exposing such kind of *narrative* ground that, on the one hand, purportedly gives support to, and, on the other, is somewhat obscured by, scientific nomological formulae. I take Alirio Rosales's (2017) comparison between Ronald A. Fisher and Sewall Wright's mathematical solutions (i.e., nomological models) for certain problems of population genetics in terms of the diverse narratives that not only support them but give them meaning to be fairly understandable along these lines.

An even more theoretically committed contribution in this respect is Carlos López Beltrán's (1998) paper, centred on the combination of narrative and statistical explanations in biology and medicine. As other philosophers interested in *narrative science*, López Beltrán starts with the factual assumption that certain specific scientific areas and practices (his focus is on medicine and biology) make an extensive use of *narrative patterns of explanation* for their very particular, unique and eventful *explananda* (a clinical case or the evolution of a particular trait). But his most thought-provoking point is that the statistical numerical models that these same disciplines also construe still reveal their narrative warp and woof, as issuing from data collection practices whose particulars are more than present in their final presentation and effective use. López Beltrán situates statistical models midway between the particularity of the unique case and the universality of classical nomological generalizations and does so by invoking a fourth intermediary state between the unique and the statistical in the clinical-case based on typicality.¹⁷

¹⁷ López Beltrán uses here extensively the work of Spanish medical doctor P. Laín Entralgo, who, in 1950, published a book on the significance of *clinical stories* that has been invoked as a forerunner of contemporary approaches to *narrative medicine* (Charon 2006). The claim about the *typicality* of a case-narrative may become a claim for its *exemplarity* in the sense that it may allow drawing conclusions thereof that are more based on the saliency and usefulness of its traits than on the statistical probability of its real occurrence (Morgan 2007: 167). However, the

Thus, López Beltrán predicates the genealogical and conceptual *continuity* of narrative and statistic explanatory strategies – against their current alleged rivalry – and even places narrativity, especially narrative cognitive capacities, as grounding both:

The continuity between these two strategies I want to expose gives certain priority to the narrative one. I want to show that, not just historically but also conceptually, the efficiency of statistical procedures is based on the either explicit or implicit use of cognitive capacities associated to narrativity. That is, the use of statistics implies a (currently nearly always occult) narrative resource and makes the same kind of explanatory work. Both strategies try to establish more or less reliable connections between strictly unrepeatable singular events and the sought for syntheses and generalizations that motivate scientific research. (López Beltrán 1998: 275; my translation)

A major reference for López Beltrán is Robert Richards's (1992) seminal paper, so far probably the most radical defence of the ultimately *narrative* character of scientific explanation *in general*: 'When the barriers are down, we will see, not that historical narrative fails as a scientific explanation, but that much of science succeeds only as historical narrative' (Richards 1992: 40).

The idea of a generalized narrative approach to scientific practice that may be just temporarily and only very superficially *circumvented* by relevant simplifications is very present in Richards's radical proposal. For Richards, the narrative quality of scientific explanatory practice would be, somehow, at the bottom of any explanatory attempt in such a way that it is only when making certain simplifications and taking certain methodological decisions that some disciplines *just apparently* and for a limited range of phenomena succeed in leaving their narrative nature behind:

[e]volutionists cannot make many predictions of consequence. I should add physicists are not logically better off; their projected systems are usually simpler and, as far as circumstances go, dead. But they cannot more accurately predict the exact trajectory of a falling leaf on a blustery Chicago day than Darwin could have divined the rise and evolutionary development of the HIV virus. (Richards 1992: 36–37)

Richards placed his narrative approach to explanation in opposition to law-based explanatory models, but most especially to the attempt to understand less strict patterns under the epistemological dominance of the nomological

functions played by what is supposedly *typical* or *exemplary* in the assessment of the epistemic relevance of narratives may be varied enough (cf. Morgan 2019). For example, in Toker's (2017) study of Gulag's literature, fictional but supposedly sample cases function as representing what really happened many times and may be so discussed in a scientific setting as 'history'. In Meunier's (Chapter 12) 'research narratives', depersonalized accounts of what really happened once (and not exactly so) become epistemically relevant for a community inasmuch as they depict procedures that might be generally implemented.

model. Instead of considering such nomological models as the successful peak from which any degree of divergence would diminish the scientific quality of an account, Richards somehow maintains that keeping in touch with the narrative roots of our scientific explanatory attempts – instead of contemplating and appraising their skeleton-like yields – will in fact *improve* epistemological research.

My second claim goes further: it is that all explanations of events in time are ultimately narrative in structure. This means that Hempel got it just backwards: it is not that history can offer only explanation sketches, but that nomological-deductive accounts [...] provide only narrative sketches; the covering law model yields sound explanations only insofar as that skeleton can be fleshed out imaginatively with the sinew and muscle of the corresponding narrative. (Richards 1992: 23)

According to Richards, the problem with Hempel's nomological model as well as other equally *nomologically eager* models is that they assume that currently valid law-like generalizations, first, lay ready at hand and, second, simply match as objective patterns the (pre-determined as) relevant facts of the *explananda* they allegedly cover. However, only in very limited, artificial, textbook-like situations (insofar as the isolation of the phenomenon is ascertained) this seems to be the case. Whenever we want to explain a *real event in time* the explanatory work will not really be done by any prearranged formal relations between selected antecedent conditions and matching laws, but precisely by the detailed investigation of the case that would, among other things, justify their use. And for that, according to Richards, we need narratives, narrative principles and narrative cognition.

López Beltrán's claimed continuity between scientific explanatory methodologies, striving at different ranges of applicability, as based on their common ultimate narrative nature, finally becomes a plea for a reasonable and healthy combination of approaches (1998: 277–278) that is rather in line with Currie and Sterelny's (2017: 20) integrative proposal. Such self-assumed *explanatory pluralism* (cf. Mantzavinos 2016) would avoid the downright dismissal of scientific explanations solely based on their form or mode of presentation and thus be more than compatible with a more nuanced and specifically *argumentative* approach to explanation discussion and assessment.

21.5 Conclusion

The *just-so charge* is a derogatory label, a negative assessment judgement that has been often misinterpreted and hastily attributed to explanatory attempts of a *narrative* nature on account of their form or discursive presentation through the catch-phrase *just-so story*. This is misleading and rather at odds with

S. J. Gould's original introduction of the concept within epistemological discussions.

However, the academic effectiveness of the label, working as a global flaw charge and preventing, in many cases, a more careful analysis of significant epistemological suggestions, has, in many cases, forced defenders of the relevance of narrative science to meet the challenge and try to respond to it.

In this chapter, I have analysed those responses that range from assuming the methodological benefits of narrative formats (or at least of the integration of narratives with other epistemological approaches) whenever the phenomena under scrutiny meet certain conditions to the bold postulation of the ultimate narrative nature of all explanatory endeavour.

These qualified defences of narrative science constitute a contribution to contemporary discussions on *explanatory pluralism* and, together with other suggestions, establish the possibility of analysing scientific reason-giving practices as primarily subject to the *dialectical requirement* of openness to collective survey and discussion rather than to aprioristic predetermined formulae precisely aiming at circumventing it. Nothing could be more *just-so*.¹⁸

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