

Infinite Barbarians

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A recurring thought in the literature on infinite regresses is that they demonstrate that there is something defective about the form of an explanation that gives rise to that regress. While this might be what is vicious about *some* infinite regresses, in this paper I want to discuss a case of a vicious infinite regress where each explanatory step, especially the first, is not explanatorily objectionable, at least in principle. This case shows us that sometimes something else is at work in making an explanatory regress vicious: and while theorists will no doubt continue to disagree about the exact diagnosis, this case should contribute a useful data point and prevent us going astray due to a too limited diet of examples. Beyond the lesson it might teach us, I also think the case is interesting and entertaining enough to be worth considering in its own right.

Before presenting the case, let me make two terminological notes. I intend "infinite regress" to be fairly neutral: I mean something like an infinite sequence of cases, each in some sense following from or generated from the one before. (I will not assume all infinite regresses are *explanatory* regresses, though some theorists do.) And to say that one is "vicious" is to say that its presence in a theory makes that theory undesirable or unacceptable, as contrasted with a "benign" infinite regress which is either not a problem at all, or a feature worth having despite its being a regress.¹ It is possible to use these expressions differently: and if one used them much more narrowly, e.g. so that *by definition* a "vicious infinite regress" only had

¹ Here I employ the terminological choices in Nolan 2001.

certain specific explanatory structures, one could rule any apparent counterexamples out by fiat. But I think it is more fruitful to identify infinite regresses and viciousness in a comparatively neutral way, and then investigate the cases to see what commonalities or variety there is between them.

I will also be supposing that an explanation of an event need not cite *every* factor relevant to that event's occurrence. So when I say one historical event explains another, I should not be taken to imply that there are no other factors at work: and in this, I take myself to be following standard historical practice. Any standard historical explanation cites only some of the factors that play a role, and I take it historians aim to offer factors that play key roles, or particularly interesting roles, or salutary roles, or something of the sort.

Barbarians and the Fall of Rome

In the fourth and fifth centuries, the Roman Empire faced increasing threats from non-Roman peoples pressing on the Empire's borders. Eventually many of those groups broke through and overwhelmed the Romans, at least in Western Europe. Let me call these invading peoples "barbarians", following traditional usage, while disowning any suggestion that these people were inferiors, or savages, etc. A standing puzzle in the study of this period is to determine why these different barbarian groups pushed across the Roman borders, especially since they were often met by stiff military opposition from a powerful, resourceful, and much larger empire when they did so. Part of the explanation is the appeal of the rich lands, plentiful supplies of prestige goods, and disturbed military situation inside Rome's borders which made it a tempting place to raid or settle.

As well as explanations which appeal to the "pull" of Roman lands and opportunities, there are also explanations which appeal to "push" factors leading barbarian armies to move from territories they previously occupied. One "push" explanation, though not the only one, is that barbarian peoples were facing threats and competition from other barbarian groups: and moving into Roman lands may have seemed preferable to staying and fighting or losing their independence to dangerous neighbours. One relatively early example is an explanation offered of Sarmatian incursions across the Roman border at the Danube during the reign of the emperor Constantine I: according to one plausible theory, the Sarmatians were pushed south by pressure from tribes of Goths, Vandals and other Germanic groups. (This pressure was eventually checked temporarily by Constantine who defeated a Gothic army invading Sarmatian territory in 322). Crossing the Danube and taking their chances in the Empire was preferable, for the Sarmatians, to staying where they were and confronting the powerful Goth armies.

So far, so good. Appealing to this as an explanation, however, raises a new and interesting question: why were the Goths and other tribes on the move? Again, there have been a range of explanations offered, and the full story no doubt depends on more than one factor. But one traditional explanation for some of these movements, particularly on the part of the Goths, was because of pressure from other barbarian groups: the Huns, and their subordinates the Alans. Indeed, when one important branch of the Goths later crossed into the Roman Empire in 376, Roman historians recorded that it was because they were fleeing the Huns.

(Ammianus Marcellinus 31.3.8-4.4)²

² There was a gap of about 40 years between Constantinian clashes with Sarmatians crossing into Roman territory under Gothic pressure and the most prominent case of Gothic groups themselves crossing into Roman territory fleeing the Huns. So it might be that the earlier Gothic movements against the Sarmatians were not caused by pressure from the Huns: or it might be that Hunnic movements far from Roman borders started to destabilise the Goths in the early fourth century, (either directly or indirectly), some time before the effects were felt by the Romans. Our sources make firm conclusions about this time period difficult. That *some* barbarian

We may also be interested in why the Huns were moving across the Eurasian steppes and putting pressure on the Goths. At this point, the attentive reader may be noticing the beginning of a pattern. At each stage, it seems open to us to explain the movement of one barbarian nation by appeal to pressure from other barbarians from further away from the Roman border: it would not be ridiculous to attempt to explain the movement westwards of the Huns by appeal to pressure from yet another group. (Indeed, if the Hsiung-nu, or Xiongnu, mentioned in Chinese sources are not the same people as the Huns, they are a likely suspect for a group who may have dislodged the Huns by moving west.)

Ambrose of Milan was one contemporary writer who claimed this was a cascade of barbarians each pushing the next: "The Huns threw themselves upon the Alans, the Alans upon the Goths, and the Goths upon the Taifali and Sarmatians; the Goths, exiled from their own country, made us exiles in Illyricum, and the end is not yet" (Ambrose *De Fide* II, cited in Maenchen-Helfen 1973 p 33). Ambrose may have been referring to pressures that happened around 376 (see Heather 1996 p 104), when another movement of Sarmatians towards Roman lands occurred. There is evidence of "pushes" of barbarians into Roman territory by the movement of other tribes at other times too: see Heather 1996 pp 32-38 for evidence that "pushes" by groups further from the frontier may have been responsible for many of the barbarian attacks on Roman territory during the second-century Marcomannic Wars (~162-180). This kind of push hypothesis, including the possibility of cascades of one tribe moving another, which moves another... is a pattern of historical explanation found not

movements were because of a cascade of pressure from one group pushing another, which pushed a third, and so on is plausible, however: one piece of evidence is Ambrose's report, discussed below.

just in ancient sources but generally accepted by today's historians of the barbarian attacks on Rome, particularly those of the late fourth and early fifth centuries.

While this sort of "push" hypothesis is one to consider when explaining the movement of any given barbarian people, it would be absurd to continue this explanatory pattern to infinity—postulating infinitely many barbarian groups, each pushing another to their west and south towards Rome, while being pushed by another group to their north and east. No serious historian would take this infinite extrapolation seriously for a minute. On the face of it, we must stop somewhere when giving this sort of explanation on pain of an infinite regress. Intuitively, though, this has little to do with a defect in this style of explaining the movements of a barbarian people per se: on the face of it, this "infinite barbarians" hypothesis is silly because we know that the Eurasian plain is not infinite, and there are not infinitely many human beings stretching Eastward from the Roman borders.

That each barbarian tribe might have another further away making it move is not even a metaphysically impossible hypothesis, if we consider alternatives to well-known facts about demographics and geography. Imagine an ill-informed ancient Roman proto-anthropologist, who suspects that the Asian steppes might continue North and East forever, and that maybe horde after horde of barbarians have been pushing South or West across it throughout an infinite past. (Or perhaps even were all dropped there in their starting positions some finite time ago by powerful though inscrutable gods.) That hypothesis might throw up some puzzling details—why are barbarians, on balance, always more worried by their eastern neighbours than their western?—but it does not look subtly contradictory or even a metaphysical impossibility, at least in any obvious way. It even has a certain degree of pleasing unity, compared to hypotheses according to which different barbarian tribes are on

the move for substantially different reasons. Still, even if we become convinced that hypothesis is a metaphysical possibility, it is still absurd when we bring what we know about the world back into play.

Lessons from the Case

The absurdity of the infinite-barbarians hypothesis does not tend to show that the explanatory temptation that generated it is somehow always inappropriate or to be resisted. When we notice that the infinite hypothesis is crazy, that does not, and should not, lead us to think that there was something wrong with explaining Sarmatian movements on the basis of pressure from Goths and Vandals. The explanation of what is wrong with the infinite regress of barbarian groups appears to be that this is a "known finite domain" case (see Nolan 2001 pp 531-2).³ Sometimes it is obvious (or very plausible) that there are only finitely many of a certain kind of explanatory resource available, and that is why it is absurd to suppose that a sequence goes on infinitely.

As a subsidiary matter, I also think that the infinite barbarians case violates a parsimony desideratum on our theories. In general, we do not want to postulate more than is needed ("do not multiply entities beyond necessity"), and an infinitude of new postulations would normally need to be justified with some impressive theoretical motivations. See my Nolan

³ Another well-known regress that appears to be a known-finite-domain case is the regress of the chicken and the egg (discussed by Nolan 2001 pp 531-2): we are only under pressure to work out which came first if we think that the generations of chickens and eggs do not go back infinitely. If we think that because we believe there was a time before chickens and eggs, (and that chickens and eggs cannot form Zeno-sequences of shorter and shorter generations), the pressure, such as it is, to say which came first comes from our knowledge that the sequence had to have started *somewhere*. One advantage the barbarian case has over the chicken-and-egg case is that it is less obviously a temporal regress; and another is that it is more closely tied to a live explanatory challenge, though there was a time when the chicken-and-egg problem was a special case of the puzzle of the origin of the species, and so was also arguably a special case of a broadly scientific explanatory puzzle. A final advantage the barbarians case has over the chicken-and-egg regress and the regress associated with the cosmological argument is that both of those regresses can seem benign: an infinite past of chickens and eggs seems to have been Aristotle's position, and many think an infinite past of causes and effects has no absurdity in it, showing one of the ways traditional cosmological arguments fail to show that there is a first cause.

2001 pp 533-536 for discussion. I suspect that whenever some archaeological or historical evidence could be explained by postulating infinitely many interacting barbarians, a less extravagant postulate will handle the evidence just as well. However, while this egregious inflation of our posits to handle a few known barbarian movements would result in a sub-par historical theory, I think that this is not what immediately jumps out at us as ridiculous about the infinite-barbarians hypothesis. We already know that there were not that many barbarians, and there is not enough room for them on the steppes, so there is no need to rely on even slightly subtle, albeit implicit, judgements of theoretical virtue in this case. (Though for what it is worth I think that if we needed an extra reason to be suspicious of the infinite-barbarians hypothesis, its extravagance is an extra reason to dislike it.)

If it is reasonable to explain Sarmatian or Goth movements by reference to pressure from other barbarian tribes, but not reasonable to go all the way down the infinite regress of each barbarian group being pushed by another, this shows that some diagnoses of what is wrong with infinite regress arguments must be incorrect. One strand of thinking about what is wrong with infinite regresses is that they all display a certain kind of explanatory defectiveness: the explanans stands in need of just as much explaining as the previous explanandum, and the attempt to meet this demand leaves us with the same explanatory deficit. When an explanation gives rise to this pattern, it shows, according to this line of thought, that the first proffered explanation was not adequate after all. One example that displays this vividly is the "homuncular theory of vision" according to which what it is to see is to have a homunculus in the eye who looks out of it and reports to the brain: which is a theory that gives an inadequate explanation of sight in general, since it needs to postulate a sub-homunculus in the eye of the homunculus to account for its vision, and another sub-sub-homunculus in *its* eye, and so on. (See Nolan 2001 pp 530-531). But a number of more serious philosophical regresses are

supposed to be of this form: the regress of justification in epistemology, the instantiation regress in the theory of properties, and Lewis Carroll's tortoise regress, to name a handful.⁴

John Passmore suggested that

Philosophical Regresses, on the contrary, demonstrate only that a supposed way of explaining something or 'making it intelligible', in fact fails to explain, not because the explanation is self-contradictory, but only because it is, in the crucial respect, of the same form as what it explains. (Passmore 1961 p 33)

More recently, Ricki Bliss has suggested that when a class of infinite regresses are genuinely vicious, it is because the explanans and explanandum are of the same form (Bliss 2013 p 412), and while an explanans and explanandum may be numerically distinct, she claims that in vicious infinite regresses "the phenomenon for which we are seeking an explanation reappears as its own explanation.", and "qua *explanans and explanandum*... they are identical." (Bliss 2013 p 410, 412) She quotes Passmore's remarks approvingly, claiming that this defect in an explanation is present even in the first step in the regress: these infinite regresses are "generated as a consequence of continually attempting to overcome an *explanatory failure* that arises at the first level of the analysis" (p 410). Bliss is explicit that this account "does not seem true of all genuinely vicious infinite regresses" (p 400), though she does argue against the major alternative accounts of what makes regresses vicious (pp 403-409), claiming that they fail even as an account of the viciousness of some regresses.

⁴ These regresses, as well as a number of other classic philosophical regresses, are listed and discussed in Wieland 2014.

In particular, Bliss criticises my claim in Nolan 2001 that some regresses are vicious because they concern domains which are known in advance to be finite. She says

One suggestion [for supposing a regress must terminate] is that it must terminate because we are dealing with a known finite domain. But this cannot possibly be a reason to think that the regress must terminate. After all, isn't what we are trying to do to establish whether or not the regress can be infinite? To understand what is problematic about this argument, we need first to consider the variety of reasons we could have for taking a domain to be finite: sometimes our reasons will be pragmatic or based on theory preference, for example. The appearance of an infinite regress in such a domain ought to force us to seriously question the reasons we have for supposing the domain to be finite in the first place. More to the point, this claim, as it stands, is question-begging. Exactly what we are seeking for is a reason to think that the regress cannot be infinite, to claim that it must be finite is patently circular. (Bliss 2013 pp 406-7)

I am not entirely sure how to unpack this argument, but I think Bliss is envisaging a situation where it is a live question whether a regress might be unacceptable. In that situation arguing, against someone who thinks there *is* such a regress, that there cannot be such a regress because the domain is finite, would be to make an argument that would be dialectically unsatisfactory. (If the proponent of a theory committed to an infinite regress believe the regress is actual, they will already believe there are more than finitely many cases.) That seems right as a dialectical diagnosis, but I do not yet see how it would rule out a regress being vicious due to a known finite domain. What we seem to have in the infinite barbarians case is a theoretical option we are sure is absurd as soon as we consider it: and then an

interesting question that arises is *why* the infinite-sequence-of-pushing-tribes option is so clearly and immediately unacceptable. In solving the puzzle of what is wrong with that theoretical option we are not constrained to only use considerations that might be common ground with a hypothetical person who endorses that infinite sequence of explanations. The focus of so much of the literature on infinite regress *arguments* might suggest the primary reason to evaluate an infinite regress is for the purposes of persuading someone an infinite regress is unsatisfactory, or convincing ourselves we have something to say for such a purpose, or something else dialectical.⁵ But citing vicious infinite regresses in arguments is only one thing we might want to do with infinite regresses we detect: and here is a case where many of us will be inclined to think a regress is vicious even if we never have occasion to use this fact against a realistic opponent. Without this requirement on a theory of vicious infinite regresses, Bliss's argument is no threat to a "known finite domain" diagnosis of what is wrong with regresses like the infinite barbarians one.

Let us then turn to Bliss's positive proposal for what makes for viciousness of an infinite regress: the proposal that when such a regress is vicious, in some sense the form of the phenomenon to be explained reappears as its own explanation. Bliss does not offer a general account of what it is for an explanans and explanandum to share a form, and perhaps she would not want to say that explaining the movement of one barbarian group by citing pressure from the movement of another we are engaged in explaining something by reference to something else "of the same form". But I think that an attempt to apply Bliss's proposal about viciousness to this case faces a dilemma. If we say that the explanans and explanandum in these cases are not of the same form, then we have an example of a vicious infinite regress

⁵ For examples in the recent infinite regress literature focused on infinite regress *arguments* see e.g. Grattan 2010, Taşdelen 2014 or Wieland 2014.

in which each step is not the same form as the one before, so it would be a case of a counterexample to Bliss's theory of viciousness of regresses. If, on the other hand, we *do* say they are of the same form, then someone applying Bliss's account would be committed to saying that the problem with the explanatory structure arises "at the first step", and so there is something explanatorily defective even e.g. with explaining the movement of the Sarmatians by reference to the movement of the Goths.⁶ But while the fantasy of infinitely many tribes is absurd, it is clear that there is nothing wrong, in principle, with the first step, or even the first few steps, of this regress: it is clear that suggesting the Sarmatians moved because of pressure from the Goths is a sensible, legitimate, and perhaps correct, case of historical explanation.⁷

Perhaps this just shows that the barbarian regress is an explanatory regress that Bliss's account does not apply to, in which case it serves as a useful reminder that Bliss's positive

⁶ One option open to Bliss at this point would be to distinguish explanatory projects: the project of explaining particular movements of barbarian groups, one by one, and the project of explaining why barbarian groups move like this at all. Then she could claim that there is a problem, even at the first step, if the second explanatory project is what we are concerned about: explaining barbarian movements by appeal to other barbarian movements may not advance the goal of explaining why there are any barbarian movements at all. (That Bliss might consider this move is suggested by her distinguishing explanatory projects this way in Bliss 2013, p 411, and her application of the distinction between explaining individual stages versus explaining why there is anything of the sort when she discusses explanations of why individual things exist, versus explanations of why anything exists at all, on p 414.) But I do not think this will help appreciably in this case. For one thing, it is not plausible that historians are primarily concerned to explain why barbarian groups move at all when they focus on these particular movements in the fourth and fifth centuries AD. Even if it were, we could still ask about the project of explaining each particular movement (rather than explaining barbarian movements in general): and when we focus on this project of explaining each movement in turn, the infinite series of explanations still looks absurd in the barbarians case. So I do not think the Blissian should try to explain our reaction that the infinite case is absurd or out of bounds by suggesting that we have somehow shifted to thinking about how to explain why there are ever movements of barbarian groups.

A more general point to be made in this connection is that the infinite barbarians case is an example of an infinite regress that can arise even when we do not have a *general* explanatory goal in mind: explaining barbarian movements in general, or vision in general (as in the homuncular theory of vision case) or of relations holding in general (as in the regress of instantiation or Bradley's regress of attaching relations to relata). So diagnoses of infinite regresses in terms of the failure of a general explanation cannot cover all the cases.

⁷ Black 1996 claims that a case where we explain a person's having blue eyes by reference to that person's parents having blue eyes is a case where the explanans is the same form as the explanandum, but where the explanation is unproblematic, and offers this as an objection to Passmore's account of viciousness. Presumably Black would be happy to see it as an objection to Bliss's account as well. What would be helpful to add is a defence of the claim that Black's explanation of an individual having blue eyes in terms of parents having blue eyes is a good explanation. My appeal to historical practice is intended to help establish that explaining a barbarian movement in terms of the movement of other barbarians is *not* a defective explanation *per se*.

account must be partial, even when we are considering explanatory regresses. Passmore claims his account is partial in another way, restricting it to infinite regresses from consistent premises: though since there seems to be nothing inconsistent even about the infinite barbarian strategy, it is a counterexample to even the letter of his view. I think it is best to reject all such general "explanatory defectiveness" accounts in favour of a pluralistic account of what is wrong with different vicious regresses, but whatever over-arching theory of vicious infinite regresses we adopt, it is important to recognise that an explanatory pattern can be in good standing, even if chaining infinitely many cases of it together gives us a vicious regress.⁸

This case causes trouble for some other related diagnoses of what goes wrong with infinite regresses. Peter Geach, for example, famously claimed that "often when philosophers think the trouble is a vicious regress, the real trouble arises already at the first step: if it is rightly diagnosed there, we can forget about the regress". (Geach 1979 p 100) Were one to generalise Geach's claim from "often" to "all", the case of the barbarians stands in obvious contrast: there is no trouble at the first step, or even in the first few, though if we extended the explanatory scheme to infinity we would get an absurd result. Graham Priest 2016 p 186 says "a regress is vicious if, after every step, what is to be accounted for is the very same thing as

⁸ There are a number of writers who defend a pluralist account of what unacceptable things may be shown by an infinite regress argument: see Nolan 2001, Grattan 2010, Wieland 2014, or Maurin 2013, though Maurin argues that there is a core of cases which are the philosophically "interesting" ones (p 18) which all involve regresses of ontological dependence, and so would presumably exclude this one from philosophical interest.

The infinite barbarians case may cause trouble even for some pluralists' classifications of infinite regresses, should they intend those classifications to be exhaustive. Wieland, for example, at one point suggested that every infinite regress argument fell either into his "Paradox Schema" or "Failure Schema" (Wieland 2013 p 107 fnt 16), and as I have construed the argument it does not obviously fall into either schema. (My guess is Wieland would disagree and construe it as matching one of his schema, though there is not space to pursue this hypothetical disagreement here.) On the other hand, the pluralism I defend in Nolan 2001 already naturally accommodates this case, treating it as a "known finite domain" case. And in any case I made no claim to exhaustiveness in characterising what might be wrong with particular kinds of regresses: pluralists who offer an open-ended list need not be concerned about adding new categories.

was to be accounted for before" in a context that suggests he is giving an account of viciousness rather than just stating a sufficient condition.⁹ That must also not be right, given the infinite barbarians case, since the infinite regress is vicious, but not for Priest's reason.

Let me finally mention two general accounts that have been offered of the viciousness of vicious infinite regresses that are less closely related to the Geach-Passmore-Bliss family of diagnoses. Löwenstein 2017 offers a general theory of infinite regress arguments that covers this case, but since on his reconstruction an argument against the infinite case contains, as an implicit premise, the claim of the form, called (FINITY), that "No P is a P in virtue of an infinite chain of further Ps" (p 337)—in this case, that there is no barbarian movement that is a barbarian movement because of an infinite chain of barbarian movements—he seems committed to reconstructing the infinite regress "argument" against every barbarian movement being caused by another such movement as being a *petitio principii*.¹⁰ (Wieland's "premise 5" in his reconstruction of regress arguments in Wieland 2014 functions in a very similar way.) This does not seem adequate to account for its force to me, though it is hard to see how to further adjudicate this question.

As I said above, of course, we might be interested in the viciousness of the barbarian regress without any ambitions to use this fact in an argument against a hypothesis of infinite barbarian movements. When it comes to which regresses are vicious, Löwenstein says

An infinite regress is benign if the preceding elements are sufficient for

⁹ Thanks to Ross Cameron for bringing Priest's statement to my attention.

¹⁰ Note that if we interpreted the infinite regress argument only as being against the conclusion that there were infinitely many barbarian tribes, Löwenstein's reconstruction would not technically beg the question, since denying "there are infinitely many tribes arranged thus-and-so" is weaker than denying that there are infinitely many tribes *simpliciter*. But I take it the target of the regress is more specific: it is against the hypothesis that each barbarian movement is to be explained by the movement of a prior one. And the conclusion that there is no such sequence is very close indeed to the relevant instance of Löwenstein's (Finiteness). Given its required target, then, I think Löwenstein's reconstruction in effect builds the conclusion in to the argument as a premise. Thanks to an anonymous referee for pushing me on this point.

the succeeding ones, but the latter do not explain or ground the former...
 Conversely, an infinite regress is vicious if the succeeding elements do ground or explain the preceding ones. Thus, if an infinite regress is supposed to be vicious and create a regress problem in the first place, it must crucially include Ground. (Löwenstein 2017 p 353)

where, in turn, Ground is a principle of the form "Every P is a P in virtue of some E distinct from it." (p 337): in this case, presumably, the principle that every barbarian movement is a barbarian movement in virtue of (/because) of some barbarian movement distinct from it. We might well worry that Löwenstein's account overgenerates, since it is not obvious that causes all the way back in time, *ad infinitum*, is vicious, nor proper parts all the way down in division, *ad infinitum*, is vicious, even if we also think that effects occur because of their causes, or that wholes are they way they are because of their parts. But I also think the current case, or at least a variant, also causes some trouble for Löwenstein's account. It is true that the current case satisfies the relevant instance of "Ground", and is vicious. But in the (very strange, but plausibly metaphysically possible) scenario where there has been an infinite chain of barbarian movements, each triggering the next, ending with barbarians crossing the Roman frontier, Löwenstein's account predicts that the regress would still be vicious (and the case would still satisfy Ground), even though any recording angel aware of all the facts would see nothing vicious in their account of the barbarians. On the face of it, it is contingent that the infinite barbarian regress is absurd, depending on contingencies such as a finite Eurasian plain, finite Earthly resources, and other obvious but finite limitations of time and organisation. But there is not this kind of wriggle room in Löwenstein's analysis of vicious regresses once they satisfy Ground and meet some other criteria that appear to be met in the world where there really are barbarian groups all the way out.

Taşdelen 2014 focuses on the logical form of infinite regress arguments, arguing that in general there are crucial premises of such arguments can be understood as counterfactual

conditionals. The current case does not challenge that aspect of Taşdelen's reconstruction, but it does cause trouble for another aspect of his account. Taşdelen points out, correctly in my view, that it is often "at the heart of the matter" to say what the problem would be if there were an infinite sequence of the relevant sort (p 198). But Taşdelen's suggestion about the general form this "inadmissibility" premise takes is to say that it is *not possible* that there be an infinite sequence of a certain sort (pp 202-3), or that the relevant sequence is *impossible* (e.g. p 210): and it is clear from his formal treatment that the relevant possibility is the kind that interacts with counterfactuals in a roughly Lewis-Stalnaker system, so that when A is impossible in this sense, it happens at no world relevant for counterfactual evaluation (see pp 202-3), rather than, say, mere epistemic impossibility.¹¹ This requirement for an infinite regress to be inadmissible may well be too strong, if the relevant possibility is e.g. metaphysical possibility. This is for a similar reason that Löwenstein's diagnosis faced difficulties, since the barbarian regress is absurd even if it is metaphysically possible. In at least some cases where we know there is no such infinite sequence, the additional requirement for viciousness that there *cannot* be such a sequence is too much. As far as I can see, Taşdelen could allow for more kinds of ways to claim an infinite sequence was "inadmissible" besides claiming that such a sequence is impossible, without too much violence to the rest of his view. It would motivate a modification, rather than wholesale rejection, of Taşdelen's proposal.

Conclusion

Even good patterns of explanation can be taken too far: and explaining a few cases of barbarian movements by appeal to pressure from other groups can be a good thing, even if

¹¹ Strictly speaking, what Taşdelen employs is the claim that not every *step* of the sequence is possible, rather than a claim about the possibility of the sequence as a whole. (p 202). But this distinction will not matter for current purposes.

that sort of explanation has to stop somewhere. What we can learn from this case (and other "known finite domain" cases) is that not all infinite regresses "go wrong at the first step" whether because of an explanatory defect or for any other reason.

This case should also be a reminder that philosophers interested in methodological principles should not just restrict their attention to unpacking philosophical arguments, with perhaps an occasional sideways glance to physics or other natural sciences. History, too, is centrally concerned with explanations, and philosophers will benefit from considering history not just as a source of questions and challenges for the philosophy of history, but because of lessons that can be drawn for quite general questions in methodology and beyond.¹²

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