implausible first premiss, (J). On the other, it fails because Roache hasn't provided reasons to think cohabitants know their survival is guaranteed. I conclude that her argument does not show cohabitants are unlike the rest of us in their concern for survival.[5]

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5 Many thanks to the editor for helpful comments.

Mathematical induction and explanation

ALAN BAKER

Marc Lange (2009) sets out to offer a 'neat argument that proofs by mathematical induction are generally not explanatory', and to do so without appealing to any 'controversial premisses' (2009: 203). The issue of the explanatory status of inductive proofs is an interesting one, and one about which – as Lange points out – there are sharply diverging views in the philosophy of mathematics literature. It may be that Lange is correct in his

Analysis Vol 70 | Number 4 | October 2010 | pp. 681–689 doi:10.1093/analys/anq074

verdict that proofs by mathematical induction lack explanatory power. However, I think that his argument to this conclusion is too quick.

Lange's core argument may be reconstructed as follows:

(1) Assume that a particular proof by mathematical induction of some general result, $\forall n P(n)$, is explanatory.
(2) Thus the basis step, P(1), of the inductive proof partly explains $\forall n P(n)$.
(3) Typically a proof by mathematical induction that starts from $n = 1$ can also be proved by going 'upwards and downwards' from any other arbitrarily selected $n = k$. For example, an inductive proof of $\forall n P(n)$ can usually be formulated whose basis step is P(5).
(4) There is no difference between the explanatoriness of a P(1)-based inductive proof of $\forall n P(n)$ and a P(5)-based inductive proof of $\forall n P(n)$. So P(5) also partly explains $\forall n P(n)$.
(5) Since P(1) partly explains $\forall n P(n)$, in particular it partly explains $P(m)$, for any $m \neq 1$. Similarly, since P(5) partly explains $\forall n P(n)$, in particular it partly explains $P(m)$, for any $m \neq 5$.
(6) Hence P(1) partly explains P(5), and P(5) partly explains P(1).
(7) This explanatory circularity refutes the initial assumption, (1), that the inductive proof of $\forall n P(n)$ is explanatory.
(8) Hence no proofs by mathematical induction are explanatory.

1. *Assessing the argument*

Lange's argument, which has the form of a *reductio*, depends on the condition (appealed to in step (7)) that mathematical explanations cannot run in a circle. In other words it cannot be the case, for two mathematical facts A and B, both that A explains B and that B explains A. I have some doubts about the inevitability of this condition, especially since Lange's argument is formulated in terms of *partial* explanation, but I shall not pursue them here. Instead I shall focus on what I take to be both the most central and the most problematic step in the above argument, step (4), which claims that a standard inductive proof of $\forall n P(n)$ (with basis case $n = 1$) and an alternative inductive proof that proceeds 'upwards and downwards' from some other base case (such as $n = 5$) must be regarded as equally explanatory. Schematically, the two kinds of proof run as follows:

P1P P(1)-based proof
For any property P:
if P(1), and
for any natural number k, if $P(k)$, then $P(k + 1)$,
then for any natural number, n, $P(n)$.

P5P P(5)-based proof
For any property P:
if P(5), and
for any natural number k, if P(k), then P(k+1), and
for any natural number $k > 1$, if P(k), then P(k – 1),
then for any natural number, n, P(n).

In comparing these two examples, Lange makes the following assertion:

> If the proofs by mathematical induction are explanatory, then the very similar proofs by the 'upwards and downwards from 5' rule are equally explanatory. There is nothing to distinguish them, except where they start. (2009: 209)

Clearly Lange does not think that there are literally *no* differences between the two kinds of proof. One obvious difference is that P1P involves verifying the base case P(1), while P5P involves verifying P(5). A second, equally obvious difference is that P5P includes a 'downward' induction step that P1P lacks. The real issue is whether either of these differences has any implications for the relative *explanatory power* of these proofs. In other words, do these differences make a difference? In the next section, I shall survey some potential candidates for such a difference-maker.

2. *Inductive proofs compared*

(i) P5P is longer than P1P

The combined effect of the two differences mentioned above is to make the P5P proof longer than its P1P counterpart. (This is certainly true for the example that Lange focuses on, involving proofs of the theorem that the sum of the first n natural numbers is equal to $n(n+1)/2$, and will be true in general except for occasional situations in which verifying P(5) turns out to be much shorter than verifying P(1).)

Lange acknowledges this contrast between the two proofs, but dismisses its significance, writing that 'the longer argument would be just as effective as the argument by mathematical induction in proving that P(n) for any natural number n' (2009: 207–8). This response seems to miss the point at hand, since 'effectiveness for proving' a given result turns on the soundness of a proof rather than on its explanatoriness. However, it seems plausible to think that length per se – in other words, sheer number of symbols – has no particular correlation with (lack of) explanatoriness either. Hence this is not a relevant difference between the two proofs.

(ii) P5P is more functionally complex than P1P

Another respect in which P5P is more complex than P1P, aside from just containing more symbols, is that it has more *parts*. Both proofs include a base case and an upward induction step, but in addition P5P has a downward induction step. What is the best way to describe this kind of complexity? One idea is to think of it as a form of *functional complexity*: P5P has more 'working parts' than P1P.

One way to try to link complexity to explanatoriness is via some version of Occam's Razor. Standard formulations of Occam's Razor give credit to theories that are qualitatively parsimonious, in other words theories that postulate fewer kinds of entities or mechanisms. In the present context, this suggests that a distinction should be made between how many different functional parts a given proof has and how many *kinds* of parts it has.

How might this play out in actual examples? Consider a third alternative inductive proof of the $n(n+1)/2$ result which proceeds by verifying the cases P(1), P(2) and P(3) separately, and then proving an upward induction step for all $n > 3$. This proof has four parts, one more than the P5P proof. However, a case can be made that it only has two kinds of parts, namely base cases and an upward induction step, and thus that this 'multi-base-case' proof is more qualitatively parsimonious than P5P.

Verdicts of the above sort concerning the numbers of kinds of parts of different proofs suffer from the problem that the key notions of 'kind' and 'part' are not clearly delineated for mathematics. Analogies can be made from science and from metaphysics, but it not always clear how to do this appropriately. Hence it is worthwhile to examine alternative ways to cash out the effects of the multi-part nature of proofs such as P5P.

(iii) P5P is more disjunctive than P1P

Perhaps the parts of P5P are better viewed not as functions but as disjuncts, in which case the salient difference between the two proofs is that P5P is more *disjunctive*. One advantage of adopting this perspective is that there is suggestive evidence from various sources that the degree of disjunctiveness of a proof does impact negatively on its perceived explanatoriness.

There is evidence from mathematical practice, for example when mathematicians voice dissatisfaction with certain aspects of computer-based proofs. Thus the original proof by Appel and Haken of the Four Colour Theorem was criticized by some mathematicians because the core of the proof involved going through several thousand cases (using a computer program), and it therefore did not provide a satisfying explanation for *why* the result is true.

There is also support from intuition. For example, going through the first 98 even numbers greater than 2 and verifying that each can be expressed as the sum of two primes clearly counts as a perfectly acceptable proof of the proposition 'All even numbers less than 200 satisfy Golbach's Conjecture.' Equally clearly, however, it does nothing to explain why this proposition is true.

Finally, there is support from the philosophical literature. This support comes from general models of explanation, such as the Kitcher/Friedman model that connects explanation with unification and therefore sees the disunifying aspect of disjunction as counting against explanatoriness. And support also comes from more specific analyses of explanation in mathematics. Indeed Lange himself, in Lange, forthcoming, develops a notion of 'mathematical coincidence' and defines such coincidences as true mathematical claims which lack explanation because they lack unified (i.e. non-disjunctive) proofs.[1]

3. *Minimality*

The best way to understand the disjunctiveness of a given proof is as relating to the number of different sub-cases into which the domain is divided by the proof. Note that even standard inductive proofs such as P1P are disjunctive to some degree, since the base case is always treated differently from all other cases in the domain. One potential problem, therefore, with highlighting disjunctiveness as the key explanation-related difference between the P1P and P5P proofs is that P1P is also disjunctive. This would seem to lend support to Lange's claim that even 'standard' inductive proofs are not explanatory.

It is worth recalling, however, that what we are responding to here in the first instance is Lange's specific assertion that there is nothing to choose explanation-wise between P1P and P5P; Lange's claim, as quoted earlier, is that there is *no* reasonable way to maintain that P1P is explanatory while P5P is not. Reflection on disjunctiveness suggests at least two ways to repudiate this claim, which rely on different background models of mathematical explanation.

One model is the *winner takes all* model. According to this model, different proofs of a given result may vary along one or more explanation-related parameters, but only the proof that has the best combination of 'explanatory virtues' counts as genuinely explanatory. If disjunctiveness is one such parameter then, according to the winner takes all model, P1P is explanatory while P5P is not, since P1P is the less disjunctive of the two proofs and P5P has no compensating explanatory virtues. Although it does allow a distinction between the two proofs to be drawn, the winner takes all model is open to the criticism that it is not faithful to actual mathematical practice. Is it the case, for example, that mathematicians typically view only the most explanatory proof of a given theorem as being a genuine explanation?[2]

1 See also Baker 2009 for an account in the same spirit of notion of an 'accidental mathematical truth'.

2 Analogous criticisms have been raised concerning Kitcher's unification model of scientific explanation.

A second model is the *threshold* model. According to this model, a proof is explanatory if and only if it meets a certain threshold of explanatoriness. If this threshold invokes disjunctiveness, it might be that proofs with three or more disjuncts cannot be explanatory. P5P exceeds this threshold, while P1P does not, hence only P1P is explanatory. It is worth noting, however, that Lange might justifiably respond to this analysis by alleging that the threshold here is itself arbitrary and unmotivated, and hence that this method does not deserve to be counted as 'reasonable'. If one is going to take the threshold approach then why not, for example, treat two disjuncts as the threshold for unexplanatoriness (which is Lange's preferred approach in Lange (forthcoming))?

The underlying difficulty with both of the above models, in the current context at least, is that our goal is to draw an absolute conclusion (i.e. that P1P is explanatory while P5P is not) from a comparative difference (i.e. that P1P is less disjunctive than P5P). This suggests that the crucial feature of P1P is not that it is less disjunctive than P5P but that it is – in a certain sense, that needs to be made precise – *minimally* disjunctive. In order to flesh out this thought, it will be helpful to start with a general definition of minimality.

Definition: A proof, X, of a theorem, P, is **minimal**, relative to some larger family of proofs, F, if every part of X is present in every other proof (in F) of P.

Using this notion of minimality (which is an absolute, not comparative property), we can formulate a fourth difference between P1P and P5P, as follows:

(iv) P1P is minimal, among inductive proofs of $\forall n Pn$, and P5P is not minimal

We have already noted that P1P has only two basic parts: a base case and an upward induction step. This is significant because *every* proof by induction includes each of these kinds of part. In other words, every inductive proof requires at least one base case, and each inductive proof requires at least one upward induction step. P5P, by contrast, contains a part – the downward induction step – which is not common to all proofs by induction. Thus P1P is minimal, while P5P is not.

One objection to distinguishing P1P from P5P on the basis of minimality is that while some kind of base case and upward induction step are common to all inductive proofs, the particular forms that these take in P1P are not. In other words, it is not the case that every inductive proof includes a base case of $n = 1$ (e.g. the P5P proof does not). And it is not the case that every inductive proof has an upward induction step that proceeds in steps of 1 (for example, the inductive proof mentioned at the end of Section 3). Doesn't this undermine the attempt to use minimality to defend, for example, the claim that the P1P proof of the $n = 1$ base case partially explains why P(n) is true for all n?

Interestingly, it turns out that there is a way to connect even these specific features of the P1P proof to the general property of minimality. The key point is that, although having a base case of $n = 1$, or an upward induction step of size 1, is not required of every inductive proof, each is required of every inductive proof that is minimal. Assume, for example, that an inductive proof of $\forall n$P(n) has a base case of $n = 2$, but no base case of $n = 1$. The proof must include an upward induction step, to cover the cases $n > 2$, but it also needs a downward induction step, to cover the case $n = 1$. Hence this proof is not minimal, since it contains two kinds of induction step. A similar point holds for the induction step. Assume that an inductive proof as a (single) upward induction step that proceeds in steps of 2. If it also has only a single base case, for $n = 1$, then the proof will only cover the odd numbers. In order to generalize to all natural numbers, the proof also needs a second base case, for $n = 2$. So the proof is not minimal. (In general, m base cases will be required for an m-sized induction step.)

The upshot is that not only is the P1P proof minimal, in the sense defined above, but that it is *uniquely* minimal among inductive proofs. The *only* way for an inductive proof of $\forall n$P(n) to have just two parts is for it to have a base case of $n = 1$ and an upward induction step of size 1. Every other variation on this basic inductive form will have some sort of base case and some sort of upward inductive step, but all of this alternatives will have other, extra parts.[3]

What about the method known as *strong induction* (or *complete induction*, or *course of values induction*)? Lange includes strong induction in his survey of variants, and sets it out as follows:

> For any property P:
> *if* P(1) and
> for any natural number k, if P(1) and P(2) and... P($k - 1$), then P(k),
> *then* for any natural number n, P(n).
> (2009: 208)

It is not necessary, however, to list the base case as a separate assumption in the strong induction schema. In other words, the above schema can be compressed to the following single step:

> For any property P:
> *if* for any natural number k, if P(1) and P(2) and... P($k - 1$), then P(k),
> *then* for any natural number n, P(n).

Moreover, it can be shown that any result provable using ordinary induction can also be proved using strong induction. Hence at first glance it would

3 For a useful discussion of various alternative forms of inductive proof, see Hafner and Mancosu 2005: 21–23.

seem that strong induction undermines the claim that ordinary induction using the P1P base case is minimal, for strong induction allows an alternative proof from *no* base case.

I think that it is still possible to defend the minimality of the P1P proof. To see why, note that even though the P(1) base case need not be listed independently, it must still be proved separately within the context of the induction step. This is because for $n = 1$, the antecedent of the initial conditional of the induction step is vacuously true: P(m) holds for all $m < 1$ because there are no such cases. Hence for this conditional to be established, one needs to check that P(1) holds. Having done this, one can go on to prove the conditional for all other values of k. Thus, despite initial appearances, strong induction does not eliminate the need to prove a base case: instead it incorporates this proof as a separate component of proving the induction step. Hence the minimality of the ordinary induction schema, P1P, can be maintained.

How might minimality be linked to explanatoriness? One idea is to view it as analogous to an oft-cited condition on scientific explanations, namely that such explanations cite only factors that are *relevant* to the given explanandum. For example, appealing to the fact that a given sample of salt was placed in hexed water fails to count as a genuine explanation for why the salt dissolved. Why not? Because the fact that the water had been hexed is irrelevant to the behaviour of the salt. One point about an irrelevant factor is that it is superfluous: the given line of (putative) explanation can be reformulated without it. So too with certain features of non-minimal proofs. The downward induction step in P5P, for example, is not essential to an inductive proof of $\forall n$P(n). This seems like a good reason to conclude that P5P is not explanatory.

4. *Conclusion*

Lange's thesis is that no proof by induction can be explanatory, on pain of explanatory circularity. Any 'standard' inductive proof of a given result can be mirrored by various modified inductive proofs which proceed in upward and downward induction steps from different base cases. Lange's thesis is based on the claim that the standard inductive proof and its rivals are all explanatorily on a par.

My response to Lange has been to indicate some features on the basis of which the purported explanatory symmetry between a standard inductive proof and other rival proofs might be broken. The two most promising such features are, firstly, that the standard proof is *less disjunctive* than its rivals, and, secondly, that the standard proof is *minimal* (in a precisely definable sense) whereas its rivals are not. As was pointed out earlier, both non-disjunctiveness and minimality have links to aspects of scientific explanation that have been discussed in the literature. Non-disjunctiveness fits well with unificationist models of explanation, such as those of Kitcher and

Friedman. And minimality echoes the spirit of causal models of explanation, where such models are used to rule out derivations involving irrelevant factors (such as the hexed salt example) that are deemed explanatory by the deductive-nomological model.

As I mentioned at the outset, my aim has not been to attack Lange's general thesis that proofs by mathematical induction are not explanatory, but rather to undercut the argument that Lange provides for this thesis. To defend the contrary claim that certain inductive proofs are explanatory would require providing a worked-out theory of mathematical explanation, something that I have not tried to do here. What I have tried to do is to show that there are plausible ways of thinking about mathematical explanation which are in tension with key steps in Lange's argument, and to this extent his premises are more 'controversial' than he admits.[4]

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4 A version of this paper was delivered to the Work in Progress Seminar, Department of Philosophy, University of Oxford, in July 2009 and benefited from useful feedback from audience members. Thanks also to Marc Lange and Arlyss Gease for helpful comments on earlier drafts.

Bangu's random thoughts on Bertrand's paradox

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Bangu (2010) claims that Bertrand's paradox rests on a hitherto unrecognized assumption, which assumption is sufficiently dubious to throw the

Analysis Vol 70 | Number 4 | October 2010 | pp. 689–692 doi:10.1093/analys/anq080