Mathematical Induction and Explanatory Value in Mathematics.

The aim of proof is not merely to place the truth of a proposition beyond all doubt, but also to afford us insight into the dependence of truths upon one another
—Frege, Foundations of Arithmetic, *§2.*

Introduction

Marc Lange (2009) argues that (almost) all proofs by mathematical induction fail to provide explanations of their conclusions. If Mathematical Inductions did explain their conclusions, Lange argues, certain conceptual requirements on explanation would be violated. Lange does not himself *presuppose* a particular account of mathematical explanation, or even that *there is* a useful notion of mathematical explanatory value that valid arguments may have or lack. Moreover, he sees himself as adjudicating whether mathematical inductions are explanatory without appealing to intuitive judgments about particular proofs—either those of philosophers or those of practicing mathematicians.

In this paper, I contend that, if we grant the barest outlines of a notion of mathematical explanation, Lange's argument fails in an instructive way.[1] Appreciating the reason why it fails guides us to a deflationary position, according to which there is no *uniform* answer to the question whether mathematical inductions are explanatory. For an analogy, notice that it does not make sense to ask whether *proof by cases* is *in general* explanatory: some arguments involving proof by cases are,

[1]Some of my criticisms are similar to the criticisms voiced by Baker 2010. Since my thinking on the matter has been largely independent of Baker's paper, I should warn the reader that there is similarity but not perfect overlap between the negative parts of our paper. I differ from Baker in one more respect: whereas he simply undercuts Lange's argument, I offer here a new, deflationary, answer to the general question I take Lange to be asking.

some are not, depending, at least in part, on their component arguments. I call this view *Transmission-based* and provide a sketch of it in the last section. According to it, schematic patterns of deductive inference that involve sub-arguments, inherit their explanatory value from their component arguments (and possibly the global theoretical context).

Before discussing Lange's argument, it is useful to introduce the notion of mathematical explanation. Here is Mancosu from a recent survey of the field:

> Much mathematical activity is driven by factors other than justificatory aims such as establishing the truth of a mathematical fact. In many cases knowledge that something is the case will be considered unsatisfactory and this will lead mathematicians to probe the situation further to look for better explanations of the facts. This might take the form of [...], providing alternative proofs for known results, giving an account for surprising analogies, or recasting an entire area of mathematics on a new basis in the pursuit of a more satisfactory 'explanatory' account of the area.

In short, if you thought that mathematicians are driven exclusively by the aim of establishing truths, you should expect mathematical practice to look very different from how it actually does.[2] By contrast, the hypothesis that mathematicians sometimes seek explanations of mathematical facts can make sense of why mathematical practice is richer than just the accumulation of established truths.[3]

I will be concerned with two kinds of judgments about explanation. On the one hand, *absolute* ascriptions of explanatory value—such as 'proof X is explanatory'. On the other, *comparisons* of explanatory value—as in 'proof X is more explanatory than proof Y' (this makes the most sense, of course, if X and Y are arguments for the same conclusions). We should refrain from accepting any simple reductions between these two. In particular, we may want to reject (or at least not assume) links like:

(i) 'if X is more explanatory than all of the existing alternative proofs, then X is explanatory' (*reason:* there may not be an explanatory proof of a given result)

[2] On this note, see also Tappenden's (2008b) discussion of the multiple proofs of the theorem of quadratic reciprocity.

[3] Of course, other (broadly speaking) epistemic values (besides explanation) guide mathematical practice: there are internal standards like *rigor* as well as external standards like *suitability to scientific applications*. But even acknowledging all of these epistemic values, understanding mathematical practice seems to require an account of the drive towards explanatory proofs.

(ii) 'if X is explanatory, then is it more explanatory than all of the alternative proofs (*reason:* we want to leave open that there might be two proofs of a given result that are equally explanatory).

1 Lange's Argument

I reconstruct Lange's argument as proceeding from two general assumptions about explanatory value.

- *Non-Circularity*: if $p_1, ..., p_n$ explain q, then q cannot be part of an explanation of any of the p_i's.
- *Minimal Closure*: if $p_1, ..., p_n$ explain $\ulcorner \forall x \Phi x \urcorner$, then $p_1, ..., p_n$ explain $\ulcorner \Phi(t) \urcorner$, for any (referring) singular term t in the language (provided, of course, that $\ulcorner \Phi(t) \urcorner$ is not one of the p_i's).

In a more recent paper (Lange, 2010), Lange denies relying on *Minimal Closure*. I am not completely convinced by the counterexamples that motivate him, but I do agree on one key point: any such counterexample would be relevantly unlike the use of *Minimal Closure* that his argument requires. In other words, there must be a plausible restriction of *Minimal Closure* that is both sufficient for Lange's purposes, counterexample-free and general enough to not be question-begging. In this paper, I concede both assumptions for the sake of argument, so I will not worry about appropriately restricting *Minimal Closure*.

Lange's first move is that the two assumptions imply:

> For every argument by mathematical induction X there is another argument Y (for the same conclusion) such that it's not the case that both X and Y explain the conclusion.

When X and Y are so related, I say that Y is an *evil twin* of X. Lange's second move is that there is no principled way of breaking the symmetry between a proof and its evil twin. More specifically, he thinks we can establish a biconditional of the form:

> X is explanatory iff Y is explanatory.

Together, these two moves imply that X is not explanatory.

Lange's justification for the first move is fairly simple. It is based on an example, but it is easy to see how to generalize it. Let $\mathbb{I}_1$ refer to the following argument for the basic number-theoretic fact:

> (*) the sum of the first n positive integers is $n(n+1)/2$.

Let $S(n)$ denote $1 + ... + n$, so that (*) can be captured by the equality $S(n) = n(n+1)/2$. (*) has a canonical proof by induction, based on these two remarks:

(i) $S(1) = 1(1+1)/2$
(ii) For all n, if $S(n) = n(n+1)/2$, then $S(n+1) = (n+1)(n+2)/2$
(the *upwards* induction step)

Each of (i) and (ii) can be verified to hold. However, we could have given a different argument by induction for (*), call this $\mathbb{I}_2$. The premises of $\mathbb{I}_2$ are (ii) and:

(iii) $S(5) = 5(5+1)/2$
(iv) For all n, if $S(n+1) = (n+1)(n+2)/2$, then $S(n) = n(n+1)/2$.
(the *downwards* induction step).

In simple terms, in order to prove (*), we don't need to start from 1 and go *upwards*. We might just as well start from 5 as long as we can go *upwards* and *downwards*.

There is no question that, if $\mathbb{I}_1$ is valid, then so is $\mathbb{I}_2$ (easy proof: use (iii) and (iv) to deduce (i) and then run $\mathbb{I}_1$). Yet, it cannot be the case that both $\mathbb{I}_1$ and $\mathbb{I}_2$ explain (*), for otherwise we'd violate *Non-Circularity*.[4] $\mathbb{I}_2$ is $\mathbb{I}_1$'s evil twin. This is a general fact: every proof by induction has an evil twin. In fact, it probably has infinitely many such evil twins. Most trivially, there is one such for each possible starting point (there may be others that break down the induction in different ways).

I also add, on Lange's behalf, that going *upwards* and *downwards* is not always a gerrymandered way of carrying out a mathematical induction. Suppose you wanted to prove that every integer is either even or odd. A convenient argumentative strategy would be to use a *upwards* and *downwards* induction (this is, of course, generally useful when trying to do induction along the integers).

Onto Lange's second move. When a proof has an evil twin, either there is an explanatory asymmetry or neither proof is explanatory. Lange believes that almost every argument by mathematical induction has an *explanatorily symmetrical* evil twin. If that is right, arguments by induction are non-explanatory. In his words: "it would be arbitrary for one of the arguments but not the others to be explanatory" (p.209). Lange does not quite give an explicit argument for this, but he does exclude a couple of possibilities. First, he points out, the greater *easiness* of the canonical induction can't be the source of the explanatory asymmetry. Easiness of computation need not have much to do with explanation. Second, the source for the asymmetry should not be found in a putative ontological priority of 1 (the base case of the canonical induction) over 5 (the base case of the evil twin). Whether or

[4] *Proof:* Suppose both proofs explain (*); since (iii) is an instance of (*), those premises also explain (iii) (by *Minimal Closure*). But, similarly, (i) is an instance of (*), so if (ii), (iii), (iv) explain (*), they also explain (i). So (i) is both explained by (ii),(iii),(iv) and part of an explanation for (iii)—violating *Non-Circularity*.

not there is a way to make sense of this talk of ontological priority of some numbers over others, it is unclear how it can relate to explanation. I agree on both counts, but that hardly justifies Lange's second move. The next section is devoted to resisting its conclusion.

2 Against Explanatory Symmetry.

The challenge I take up is twofold: first, I argue that *some* inductions are themselves explanatory in the absolute sense. I claim, then, to provide a counterexample to Lange's conclusion that (almost) all inductions are non-explanatory. $\mathbb{I}_1$ may not be such a counterexample, so I might agree with Lange that it's not explanatory. This potential point of agreement only sets up my second point: canonical inductions generally have explanatory virtues that their evil twins lack. So, even if $\mathbb{I}_1$ turns out not to be explanatory, Lange has not supplied the right diagnosis for it.

The example I put forward (let's call it $\mathbb{I}_3$) is a very simple instance of the so-called argument by *induction on the complexity*—familiar to students of the metatheory of any interesting logic. Let $\mathcal{L}$ be a propositional language with connectives interpreted classically. Consider the claim:

> (#) For every sentence p of $\mathcal{L}$, an assignment of truth values $\mathcal{I}$ to the atomic sentences of $\mathcal{L}$ settles the truth value of p.

First, define recursively a function *compl* that assigns to each sentence of $\mathcal{L}$ a natural number: atomic sentences get 0, for sentences of the form $\ulcorner{\sim}q\urcorner$, the clause is $compl(\ulcorner{\sim}q\urcorner) = 1 + compl(q)$, while for sentences of the form $\ulcorner p \vee q\urcorner$, $\ulcorner p \,\&\, q\urcorner$, etc., the clause is $compl(\ulcorner p * q\urcorner) = 1 + max\{compl(q), compl(q)\}$. Given *compl*, we can provide an induction in support of (#): for sentences of complexity 0, (#) is trivial. Next, we show that the inductive step:

> if (#) holds for sentences of complexity up to n, then it holds for sentences of complexity $n + 1$

must hold, because of the truth-functionality of the connectives. This completes the proof.

Most inductions on complexity are more interesting than this example, but the key observation is that these arguments are just standard mathematical inductions. Their only special feature is that they are typically carried out in a language that is slightly more expressive than the language of arithmetic (i.e. the language of our meta-theory).[5] Do they have “evil twins”? Of course, they do. Consider the induction $\mathbb{I}_4$ whose base step is:

($\#_B$) (#) holds for sentences of complexity up to 5

and whose inductive clauses are:

($\#_1$) if (#) holds for sentences of complexity up to n, it holds for sentences of complexity $n+1$

($\#_2$) if (#) holds for sentences of complexity up to $n+1$, then it holds for sentences of complexity n.

($\#_1$) is also a premise in $\mathbb{I}_3$ and its proof is obvious given the recursive definition of truth in $\mathcal{L}$. ($\#_2$) is immediate.[6] How does one prove ($\#_B$)? Well, and this is only partly tongue-in-cheek, prove that (#) holds for complexity 1; next prove that, because of this, (#) holds for complexity 2; next prove it for complexity 3,..., and finally, given that it holds for complexity 4, prove that it holds for sentences of complexity 5. $\mathbb{I}_4$ is an evil twin for $\mathbb{I}_3$—a proof of the same result by a mathematical induction that starts with a different base case.

What should we say about the relation between $\mathbb{I}_4$ and $\mathbb{I}_3$? It would be absurd to argue that the explanatory value of $\mathbb{I}_3$ is undermined by $\mathbb{I}_4$: $\mathbb{I}_4$ includes $\mathbb{I}_3$ as a *component*. In fact, I think $\mathbb{I}_3$ is a clear counterexample both to Lange's comparative premise (i.e. that $\mathbb{I}_3$ and $\mathbb{I}_4$ are on a par) and to the parallel ‘absolute’ conclusion (i.e. that $\mathbb{I}_3$ is not explanatory). The interesting point is that some of the reasons why $\mathbb{I}_3$ is better than $\mathbb{I}_4$, also apply to $\mathbb{I}_1$ and $\mathbb{I}_2$. Let me first go through some reason that support taking $\mathbb{I}_3$ as more explanatory.

First off, if mathematical explanation is to be anything like scientific explanation, simplicity must function as a tie-breaker of sorts.[7] I now set up a case that canonical inductions are (to varying degrees) simpler than their evil twins. The sense of simplicity that's relevant here is not to be confused with ‘easiness’: canonical inductions are simpler in that invoke fewer general premises, allow detour-free and direct pieces of argument.

[5] In particular, in addition to numbers and elementary operations on them, the language needs to the ability to talk naturally (i.e. not via coding) about the syntax and the semantics of the underlying propositional language $\mathcal{L}$.

[6] This is always be the case for instances of so-called ‘strong induction’: the validity of the downwards premise is immediate. We will question, however, that these downwards principles support or can be supported by explanatory arguments.

[7] I leave it open whether the most explanatory proof of a given result is also the simplest. Perhaps this is the case in Lange's canonical example of mathematical induction, our $\mathbb{I}_1$. As we saw, this theorem is often proved *via* an induction, but it can also be proven in a few alternative ways, including a rather intuitive visual proof. As Lange documents, philosophers' intuitions on the explanatory value of $\mathbb{I}_1$, compared to these other proofs, are wildly divergent.See Mancosu (1999), for a discussion of this case.

The most immediate observation is that canonical inductions employ only a proper subset of the general principles involved in their evil twins. This is certainly true even of $\mathbb{I}_1$-$\mathbb{I}_2$, and it does seem that minimizing the number of general principles is a guiding (though not indefeasible) heuristic for choice of induction steps among practicing mathematicians.

This is a slight, though noticeable, point in favor of canonical inductions. There is more: The *upwards* and the *downwards* induction steps cannot, by Lange's own lights be *both* justified by explanatory arguments. Given Non-Circularity, it cannot be the case that both of these arguments are explanatory:

- the argument from $P(x)$ to $P(x+1)$.
- the argument from $P(x+1)$ to $P(x)$.

It seems plausible to regard this as a strike against inductions that go upwards and downwards, unless necessary. If so, there is an *a-priori* reason of sorts to disregard inductions with both types of inductive steps. To circumvent this problem, the evil twins must be characterized in slightly different terms. The *upwards* step should require P to be preserved upwards starting from 5; the downwards step should require P to be preserved downwards starting from 5. Not only does the canonical induction make do with fewer general premises, but if we use both upwards and downwards principles, we must further complicate the premises involved.

The third reason to prefer $\mathbb{I}_3$ is that the *upwards* induction step appears to be naturally justifiable by an explanatory argument, while the *downwards* step does not. Sentences of complexity $n+1$ are built out of sentences of complexity up to n are settled by $\mathcal{I}$, and that, together with the truth-functionality of the connectives, does the explaining. By contrast, the fact that sentences of complexity up to $n+1$ are settled by $\mathcal{I}$ does not explain why sentences of complexity up to n are settled by $\mathcal{I}$: the latter is just a special case of the former.[8] [For the purposes of this short paper, I submit this as an intuitive datapoint, to keep the discussion tight, but it should be backed it with general reasons, and I think it is possible to provide them.]

I conclude that there is an asymmetry between canonical inductions and their evil twins. The asymmetry is quite general, though it is more pronounced in cases (like $\mathbb{I}_3 - \mathbb{I}_4$) in which the upwards induction step seems tied to some kind of dependence, while the downwards induction step seems not to be tied in this way. Inductions of this kind need not be "inductions on the complexity": much of what I said here would apply to, say, the theorem that the n^{th} term of the Fibonacci sequence is less than or equal 2^n.

[8] Sometimes you can explain why *all F's are G's* by pointing out that *all F's are H's* and G is a special case of H, but this does not appear to be one of those cases: you can't explain why there are at least three chairs in the house merely by pointing out that there are at least four.

The first two points of asymmetry, however, also apply to $\mathbb{I}_1$ and $\mathbb{I}_2$ as well. By contrast, we would be hard pressed to find any reasons to prefer $\mathbb{I}_2$. If so, $\mathbb{I}_1$ and $\mathbb{I}_2$ are not, after all, explanatorily symmetrical. So even on the assumption that $\mathbb{I}_1$ is not explanatory, Lange does not seem to have given the right diagnosis.

3 Transmission and Proof Ideas.

Lange's general argument is ineffective, but the question he raises is a good one: can we say anything principled and general about whether particular arguments by induction are explanatory? I do not have a characterization of the class of explanatory mathematical inductions, but I do not think the question requires a characterization.[9] We can instead exploit important connections between the explanatory value of a certain proof by induction and the explanatory value of its sub-arguments.

Let me reiterate an earlier point. Ask: is *proof by cases* explanatory? There is no uniform answer to this question, and we should not expect one. It simply depends on further issues: is the division in cases natural or gerrymandered? Are the sub-arguments within each case explanatory? Does breaking down in cases miss some case-independent explanation?

We should not expect mathematical induction to be very different. That is, we should not expect mathematical inductions to be uniformly all explanatory or all non-explanatory. Furthermore, we should expect the explanatory value of mathematical inductions to depend, at least in part, on the explanatory values of their components. In this spirit, I propose the following requirement.

> **Transmission Requirement:** a proof by mathematical induction is explanatory only if the arguments for all of its components are themselves explanatory.

The 'components' of a mathematical induction are the arguments for its base case(s) and the argument(s) for its induction step(s). Sometimes one or more of these arguments will be completely trivial: that's enough to pass this requirement. Completely trivial facts do not demand explanation. In addition to this, just as in *proof by cases*, there may be further requirements, but we'll see what the Transmission Requirement can do on its own.

As formulated, the Transmission Requirement only applies to absolute judgments of explanation. However, it can naturally be extended so as to have a comparative upshot. In general, just stipulate that comparisons of expanatory value

[9]In fact, Steiner (1978) suggests, in my view plausibly, that most natural attempts at a characterization of *mathematical explanation* are bound to fail.

among proofs depend on multiple features and that the explanatory value of the sub-arguments is one such feature. Part of our ground for preferring $\mathbb{I}_3$ over $\mathbb{I}_4$ was an argument to the effect that the two proofs could not both have explanatory sub-arguments, and that the downwards proof did not track dependence relations among the appropriate facts.

The implicit suggestion here is that the reason why $\mathbb{I}_1$ is not explanatory (if indeed it isn't) has to do with the algebraic manipulation involved in the inductive step. That manipulation does not explain why we can transition from $P(n)$ to $P(n+1)$. Moreover, the Transmission Principle implies that, in general, if we are going to look for explanatory inductions, we must look for proofs whose every sub-argument is itself explanatory. If we claim, as I do, that the inductions on the complexity that track the structure of naturally recursive sets are explanatory, we must also be committed to the view that their component arguments are explanatory. This strikes me as exactly right in the example I provided and in many similar example.

§3.1 Transmission and Extant Models of Explanation.

Incidentally, that some kind of Transmission Requirement applies to mathematical explanation is implicitly entrenched in the literature on mathematical explanation. In this section, I briefly consider Steiner's model and Kitcher's and explain how they can be understood as implying Transmission.

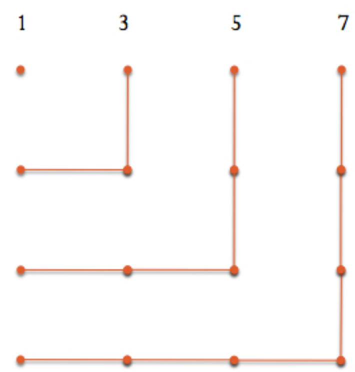
§3.2 Formalization-Independence.

There is one last piece of argument in favor of a Transmission Requirement. If the Tranmission requirement is correct, we should expect explanatory value to be invariant among formalizations of the same proof-idea. Consider a mathematical fact that is closely related to the theorem about the sum of the first n positive integers.

> The sum of the first n odd positive integers (denote this by $O(n)$) is n^2.

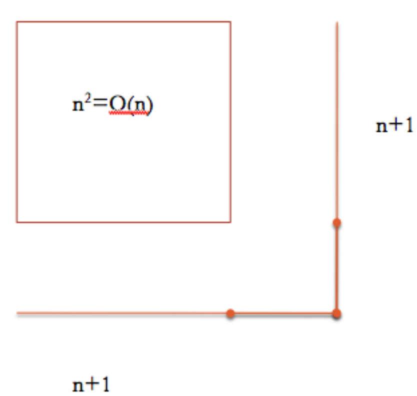
As usual you can prove this by induction, but, once again, an intriguing visual proof is available.

> Represent the n^{th} positive odd integer as an L-shaped array of $2n-1$ dots (with 1 being represented by a single dot). The next odd positive integer (i.e. $2n+1$) can be represented by pushing the representation of $2n-1$ down the diagonal and adding two more dots. Hence, you can always arrange the representations of the first n positive odd integers so as to form an n by n array.



Some maintain that the proof counts as a proof even without accompanying text (Brown 1999). Others hold that some text is required. Be that as it may, let us suppose that the text is present and is roughly on the lines I provided.

Compare this proof with a different proof by mathematical induction. First, observe that $O(1) = 1$. At the inductive step, however, instead of invoking algebraic manipulations, invoke the visual proof:



The induction assumption provides us with the equality $n^2 = O(n)$. Let the square represent an array of n^2 dots. From this point on, reason in exactly the same way: the new odd integer (i.e. 2n+1) can be represented by pushing 'out' the sides of the L-shaped representation of 2n-1 and adding one new dot. The new array of dots is a square of side $n + 1$.

Bracket possible concerns about the rigor of these two proofs or of visual proofs in general. They are orthogonal to the point I want to illustrate. According to Lange's view the explanatory value of the second, visual, induction stands and falls with the explanatory value of the non-visual induction. According to the Transmission-based picture, the explanatory value of the visual induction is much more directly tied to the explanatory value of the non-inductive visual proof, and is *wholly independent* of the explanatory value of the non-visual induction.

I do not see how the verdict of the Transmission-based account could fail to be wrong. Our two visual proofs embody the same *proof idea*. Two proofs that rely on the same proof ideas but differ in formalization should not differ in explanatory value.[10] The explanatory value of proofs really depends on the substantive pieces of argument employed, rather than on the formalization that frames them. Formalizations only matter insofar as they allow or disallow certain patterns of argument.[11]

§3.3 Explanatory Ties.

It is perhaps natural to think that mathematical inductions are not explanatory if there is a direct (i.e. non-inductive) argument for their conclusion. If the picture I have been sketching is correct, this is false. There are perfectly explanatory proofs by induction that can be replaced by equally explanatory direct proofs.

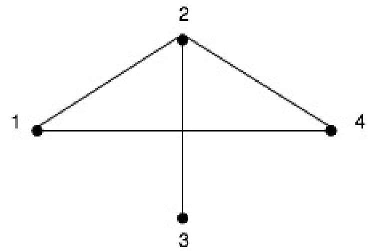
My example in this connection comes from graph theory. Graph theory is an extraordinary source of intriguing inductions, but here we will focus, once again, on an extremely simple case. Let me review some basic definitions first: a *graph* is a triple $\langle V, E, R\rangle$, in which V is a set of *vertices*, E is a set of *edges* and R is a relation that associates with each edge e, two vertices v and w, called e's *endpoints*. A graph is *simple* if (i) no edge e and vertex v are such that vR_ev and (ii) no two distinct edges e_1 and e_2 have the same endpoints. A *path* p is a simple graph such that there is an assignment f of positive integers to the vertices of p such that for all e, v, w, vR_ew iff $f(v) < f(w)$.

A *walk* is a sequence of vertices and edges $\langle v_0, e_1, v_1, ..., e_k, v_k\rangle$ such that for $1 \leqslant i \leqslant k$, e_i's endpoints are v_{i-1} and v_i. In the context of simple graphs, you can also think of a walk as a sequence of vertices. To simplify the presentation, we will adopt this characterization (but the result I will mention in the next paragraph holds beyond simple graphs).

Every path coincides with a walk, but not every walk coincides with a path, since walks can (and paths cannot) go through the same vertex more than once.

[10] The visual induction does more by way of formalizing the proof idea; the non-inductive visual proof instead relies on the reader to infer to the general case from the individual picture.

[11] For example, a proof by cases might isolate the case for separate treatment and miss an important common feature.



A simple graph in 4 vertices.

A path.

However, the following is true: *every walk between u and w contains a path between those two vertices*.[12] The textbook proof of this result is an induction on the length of walks: for walks of length 1 (i.e. single-vertex walks), the result is immediate. Let the induction hypothesis be that the statement of the theorem holds for walks of length up to n and consider a walk W of length $n+1$. If W does not contain repetitions, then it immediately determines the path. If it does contain repetitions, say it contains the vertex z twice (at positions i and j), then consider the walk that results by deleting the segment of the walk between i and j. This walk is of length up to n and hence must, by induction hypothesis, contain a path.

Needless to say, $\mathbb{I}_5$ has all kinds of evil twins.[13] But it is not so much the evil twins that I want to focus on, as much as what we might call its *virtuous* twins.

The result in question does not really require an inductive proof. It can be proven directly, with the appropriate definitions.

Say that w is a walk. Now define the function f recursively.

If for some v, $w = \langle v \rangle$, $f(w) = w$.
If for some walk w' and edge-vertex pair $\langle e_k, v_k \rangle$, $w = w' + \langle e_k, v_k \rangle$, then,

Case 1, v_k does not occur in w', in which case $f(w) = w$.

Case 2, v_k does occur in w', in which case we find its earliest occurrence and drop everything that comes after it, and let the result of this operation be $f(w)$.

[12]It's crucial fact in graph theory that there is no guarantee that the path in question will 'reach' all of the vertices reached by the walk, but this need not concern us here.

[13]But there is a solid case to be made that these twins are explanatorily inferior to the canonical proof–for much the same reasons. First, an evil twin of $\mathbb{I}_5$ would have to establish the result for walks of length 1 in the course of establishing it for walks of length 5. Second, the *upwards* induction steps can be justified by explanatory arguments (you can sketch an explanatory answer to why walks of complexity $n+1$ must contain paths, if walks of length n do), but you cannot sketch an explanatory answer to why walks of length up to n must contain paths, if walks of length up to $n+1$ do. As above, there is no explanatory reason for why the downward principle should go through.

We can now directly and without induction prove that $f(w)$ is/determines a path.

It is intuitively clear that no explanatory advantage is acquired by eliminating the induction in favor of a recursion + universal argument. These two proofs simply embody the same proof-idea.

4 Conclusion

On the transmission-based picture, the explanatory value of mathematical inductions is ultimately tied to the explanatory value of arguments that are not themselves mathematical inductions. If that is true, although we may lack a criterion that characterizes which inductions are explanatory, we can conclude that the question whether inductions are explanatory *in general*, can, in a sense, be dispensed with. There is no more of a 'global' issue concerning the explanatory value of mathematical inductions, than there is a global issue of the explanatory value of proof by cases.

References

Baker, A. 2010, "Mathematical Induction and Explanation", *Analysis*, 70(4), pp. 681-689. Brown, J. 1999, *Philosophy of Mathematics: an Introduction to the World of Proofs and Pictures*, London, Routledge.

Kim, J. 1994, "Explanatory Knowledge and Metaphysical Dependence", *Philosophical Issues*, 5, 1994, pp. 51-69.

Kitcher, P. 1975, "Bolzano's Ideal of Algebraic Analysis", *Studies in History and Philosophy of Science*, 6, 229-269.

Lange, M. 2009, "Why Proofs by Mathematical Inductions are not Explanatory", *Analysis*, 69(2), pp. 203-211.

Lange, M. 2010 "What are Mathematical Coincidences (and Why Does it Matter)?", forthcoming in *Mind*. Mancosu, P. 1999, "Mathematical Explanation: Problems and Prospects", in *Topoi*, 20, pp. 97-117.

Mancosu, P. 2008a, "Mathematical Explanation", in Ed Zalta (ed.), *The Stanford Encyclopedia of Philosophy*.

Mancosu, P. 2008b, "Mathematical Explanation: Why it Matters", in *The Philosophy of Mathematical Practice*, Mancosu P. (ed.), Oxford, OUP.

Steiner, M. 1978, "Mathematical Explanation", in *Philosophical Studies*, 34, pp. 135-151.

Tappenden, J. 2008a, "Proofstyle and Understanding in Mathematics I: Visualization, Unification and Axiom Choice", in Mancosu, P. and Jørensen, K. *Visualization, explanation and reasoning styles in mathematics*, Springer Verlag, pp.147-217.

Tappenden, J. 2008b, “Mathematical Concepts and Definitions”, in *The Philosophy of Mathematical Practice*, Mancosu P. (ed.), Oxford, OUP.