Foundations of Mathematics: Reliability AND Clarity: the explanatory role of mathematical induction

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Abstract. While studies in the philosophy of mathematics often emphasize reliability over clarity, much study of the explanatory power of proof errs in the other direction. We argue that Hanna's distinction between 'formal' and 'acceptable' proof misunderstands the role of proof in Hilbert's program. That program explicitly seeks the existence of a justification; the notion of proof is not intended to represent the notion of a 'good' proof. In particular, the studies reviewed here of mathematical induction miss the explanatory heart of such a proof; how to proceed from suggestive example to universal rule. We discuss the role of algebra in attaining the goal of generalizability and abstractness often taken as keys to being explanatory. In examining several proofs of the closed form for the sum of the first n natural numbers, we expose the hidden inductive definitions in the 'immediate arguments' such as Gauss's proof. This connection with inductive definition leads to applications far beyond verifying numerical identities. We discuss some objections, which we find more basic than those in the literature, to Lange's general argument that proofs by mathematical induction are not explanatory. We conclude by arguing that whether a proof is explanatory depends on a context of clear hypothesis and understanding what is supposedly explained to who.

Lange [Lan09] describes a striking disagreement among philosophers concerning the explanatory power of mathematical induction, 'Some philosophers are quite confident that these arguments are generally explanatory, even in the face of other philosophers who appear equally confident of their intuition to the contrary. Very little in the way of argument is offered for either view.' We argue that the failure to see the explanatory nature of the usual inductive proofs is in fact a misunderstanding of what it is that is explained. We isolate the explanatory feature of specific inductive proofs and indicate why apparently simpler 'proofs' are incomplete.

The contrast raised by many philosophers, e.g, [Han90,Lan09,Man08,RK87] between explanatory and non-explanatory proofs can be phrased as 'A non-explanatory proof merely shows the result is 'true' while an explanatory proof provokes understanding of why it is 'true'. I put scare quotes around 'true' as in fact proofs do not show truth. They show that a result is a consequence of the hypotheses. A major difficulty with this literature concerning induction is that much of the analysis ignores a crucial criterion that Hanna [Han90] makes explicit (and then ignores), 'the proof must proceed from specific and accepted hypotheses'. In particular cases, making the implicit hypotheses explicit illuminates what the proof is supposed to explain.

In Section 1 we observe that the traditional emphasis on *the foundation* of mathematics leads to a misunderstanding of proof as simply a matter of 'reliable inference' and misses explanatory motivations in mathematics. In Section 2, we show how this distortion of the role of formal proof is reflected in a misapprehension of the goals of even basic proofs involving mathematical induction. Section 3 underlines the symbiosis between inductive definition and inductive proof that is implicit in many if not most proofs by induction. Complementing this discussion of how inductive proofs are explanatory, in Section 4 we expound some fundamental objections to Lange's assertion that inductive proofs are inherently non-explanatory. We return in Section 5 to our underlying theme: analyzing proofs solely as a way to verify truth obscures their explanatory nature. This tendency is amplified by a misunderstanding of Hilbert's proof theory; it aims to analyze not *proof*, but *provability*.

We critique several attempts to give a general characterization of mathematical explanation by examining how they fare in studying mathematical practice. We don't attempt to produce a positive theory but obey the injunction that concludes the Hafner and Mancosu article [HM05], 'It is our hope that this kind of testing of theories of mathematical explanation against mathematical practice will pave the way for future studies in the same vein. This seems to us the most promising approach for making progress in this treacherous area.'

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1 Reliability vrs Clarity

Much study of the foundations of mathematics focuses on the issue of finding a ground for all of mathematics and on issues of ontology and reliability. But, in fact, many of the 19th century foundational studies were concerned with the clarification of concepts and the relations between them. In [Bal14,Bal15] I expound the role of formal theories in clarifying notions and making mathematical progress. Here we focus on the relation between clarity and reliability in the context of mathematical induction and (generalized) inductive definition. As Coffa describes below, many histories of foundational studies have emphasized reliability over clarity. Perhaps in reaction, many discussions of mathematical induction have sacrificed the reliability aspect in favor of an explanation of one phenomena: why is this particular closed form of some expression hypothesized? But this analysis ignores the phenomena that inductive proof is designed to explain: the passage from example to universal. Regarding foundations, Coffa reported the history,

> [We consider] the sense and purpose of foundationalist or reductionist projects such as the reduction of mathematics to arithmetic or arithmetic to logic. It is widely thought that the principles inspiring such reconstructive efforts were basically a search for certainty. This is a serious error. It is true, of course, that most of those engaging in these projects believed in the possibility of achieving something in the neighborhood of Cartesian certainty for principles of logic or arithmetic on which a priori knowledge was to based. But it would be a gross misunderstanding to see in this belief the basic aim of the enterprise. A no less important purpose was the clarification of what was being said. . . .

> The search for rigor might be, and often was, a search for certainty, for an unshakable "Grund". But it was also a search for a clear account of the basic notions of a discipline.[1]

We argue below that in the study of the explanatory power of mathematical induction there are several issues to be considered. And some of them might be dismissed as 'mere reliability'. But we insist that explanation is a fundamental goal of mathematics. We focus on mathematical induction as there is a substantial literature on its purported non-explanatory value.

Resnik and Kushner remark, 'Mathematicians rarely describe themselves as explaining' (page 151 of [RK87]). As a mathematician, I can only explain such a remark as a lack of exposure to mathematicians[2]. Perhaps the difficulty is that the notions being explained are abstruse. I give from a popular source (wikipedia) the first example that popped into my head.

> In mathematics, monstrous moonshine, or moonshine theory, is a term devised by John Conway and Simon P. Norton in 1979, used to describe the unexpected connection between the monster group M and modular functions, in particular, the j function. It is now known that lying behind monstrous moonshine is a certain conformal field theory having the monster group as symmetries. The conjectures made by Conway and Norton were proved by Richard Borcherds in 1992 using the no-ghost theorem from string theory and the theory of vertex operator algebras and generalized KacMoody algebras. [Ano16]

Many why-questions in mathematics arise from exploring unexpected connections across widely divergent areas. In this example, Conway and Norton observed certain strange sequences of numbers (1,196884, 21493760, ...) that arose in finite group theory also arose in complex function theory. Physics and functional analysis were involved in explaining this non-coincidence. One part of the solution involved collaboration between two finite group theorists (Paul Fong, Steve Smith) at University of Illinois in Chicago and our colleague A.O.L. Atkin. As a pioneer in using the computer for number theoretic calculations[3] and an expert in modular forms, Atkin instantly recognized these coefficients. The Langlands program is an even bigger example of an explanatory project that crosses many fields to explain certain analogies.

Of course, such major projects as described in the last paragraph can not be analyzed in a short paper or with my (lack of) expertise. In a series of vignettes, 'How to explain number theory at a dinner party', Harris ([Har15], page 51) presents a more concrete example. His foil says 'a number theorist sits at a desk and answers questions about numbers all day'. He replies,

> Actually, number theorists are not especially interested in answering questions about numbers. We really get excited when we notice that answers seem to

[1] page 26 of [Cof91]

[2] Section 3 of [HM05] give several specific examples of mathematicians using 'explain' in various senses.

[3] He and another U.I.C. mathematician, Neil Rickert, once held the record for the largest pair of twin primes.

be coming out in a certain way, and then we try to explain why that is. For example, the equation $x^4 14x^2 + 121 = 0$ – the question, what number solves that equation? – has not one but four answers: $\sqrt{2} + 3i$, $\sqrt{2} - 3i$, $-\sqrt{2} + 3i$, $-\sqrt{2} - 3i$. There's a pattern: you can permute $\sqrt{2}$ with $-\sqrt{2}$ and $3i$ with $-3i$. What does it mean? What does it tell us about solutions to other equation?
When our ideas about possible explanations are sufficiently claer, we set ourselves the goal of finding the correct explanation and then justifying it.

2 The explanatory function of Mathematical Induction

Hanna [Han90], distinguishes between formal and acceptable proof. Although, we discuss difficulties with her version of each notion in Section 5, this divide is useful for understanding the situation.

1. Formal proof: proof as a theoretical concept in formal logic (or metalogic), which may be thought of as the ideal which actual mathematical practice only approximates.

2. Acceptable proof: proof as a normative concept that defines what is acceptable to qualified mathematicians.

Although this distinction is irrelevant to much of her analysis, our fifth proof below uses the notion of formal proof. Her argument rather depends on a dichotomy between a *proof that proves* (which is an alias for acceptable) versus *a proof that explains*. She writes (page 10 of [Han90]), 'I prefer to use the term *explain* only when the proof reveals and uses the mathematical ideas that motivate it.' A difficulty in her analysis is that while she defines 'acceptable proof' in terms of 'qualified mathematician', she omits discussion of the 'qualified' mathematician's understanding of the hypotheses of the theorem – this understanding crucially impacts the explanatory nature of the proof.

I take formal/acceptable to be the same distinction I make in [Bal13] between *Hilbert-Gödel-Tarski* and *Euclid-Hilbert* proof. The first requires the definition of a formal syntax and rules of inference and Tarski's name is adjoined to consider semantics. The second takes place in natural language. While there are specified definitions and axioms, the rules of inference may be implicit. With this background we consider some examples.

Hanna's examples are from [Ste78]; I address Steiner's more sophisticated approach to explanation later. Hanna writes[4]:

> The following example illustrates the difference between a proof that proves (acceptable) and proof that explains (explanatory):
> Prove that the sum of the first n positive integers, $S(n)$, is equal to $\frac{n(n+1)}{2}$.
> (page 10 of [Han90])

There are two issues that have to be explained here.

1. Why do we choose the formula $\frac{n(n+1)}{2}$?

[4] I added the parenthetical descriptions. And, I modified the question because as originally phrased, the first question is not asked. But the explanations proffered by Hanna all deal with it.

2. Why does our observation that this formula works on some examples extend to all natural numbers?

Modern attempts to answer the second question date from Maurolycus in 1575 ([Bus17]), followed quickly by Pascal. But Maurolycus simply argues for a few cases that the property extends from n to $n + 1$. Cajori [Caj18] attributes the explicit step of proving for each k, $P(k) \to P(k+1)$ to Jakob Bernoulli in 1713. Dedekind announces in his introduction (page 32 of [Ded63]) that a major result of his essay on number is 'a complete proof that the form of argument known as complete induction (or the inference from n to $n + 1$) is really conclusive.' The modern formal version appears in Peano. Dedekind and Peano thus address an even deeper why question, why does this 'rule of complete induction' gradually clarified until the late 19th century actually justify the passage from example to universal.

We consider five alternative 'proofs' and see how they answer each of these questions. We try to extract from the argument what the writer of the proof is actually taking as the hypotheses.

Gauss 1. Hanna rates the standard proof by induction as acceptable but not explanatory and the Gauss argument as both explanatory and acceptable. The Gauss argument is the following.

$$\begin{array}{cccc} 1 & + \;\; 2 & + \ldots & n \\ n & + \text{(n-1)} & + \ldots & 1 \\ \hline \text{n+1} & + \text{ n+1} & + \ldots & \text{n+1} \end{array}$$

Now there are n addition problems that each add to $n + 1$ and each number up to n occurs twice as a summand so the sum of the numbers up to n is $\frac{n(n+1)}{2}$.

I argue below that by Hanna's criteria the standard inductive proof is explanatory of 2) but not 1) and is acceptable in terms of verifying truth. While the hypotheses are not stated, when they are made clear, the argument can clearly be carried out in arithmetic with induction.

And I say that 'Gauss's' proof as given is explanatory of 1) but not 2). It is not acceptable because the expression $1 + 2 \ldots n$ is not defined (and is not easy to define). Remember, addition is a binary function. But $1 + 2 \ldots + n$ is what is variously called *anadic*, *variadic* addition or an example of plural quantification. It is not part of basic mathematical notation because no arity is prescribed. The function must be introduced by some form of inductive definition[5]. Such functions are implemented in many (e.g. list processing) programming languages.

To make Gauss' argument into a proof we have to clarify our estimate of what the 'qualified mathematician' is assuming. Here is one approach[6]. Work in informal set theory and for fixed n and $k < n$, define $f(k) = (n + 1) - k$. Then for each k with $0 \leq k \leq n$, $k + f(k) = n + 1$. So the sum of the first n numbers is

$$\frac{(1 + f(1)) + (2 + f(2)) + \ldots (n + f(n))}{2}$$

[5] There are various logics for studying this topic [Lin14]; but they are not considered in the papers under discussion.

[6] In the paper Hanna draws on, Steiner [Ste78] refers to Quine's set theory book [Qui69].

since the numerator of this expression contains each number up to n twice. Now there are certainly n such k since the domain of f is n and by the (generalized) distributive law the sum is $\frac{n(n+1)}{2}$.

Note that the function $f(n)$ is usually taken as a definition of $\Sigma_{k=1}^{n} k$. So we see a way to fill the gap and produce an acceptable proof is to assume the generalized algebraic laws (i.e. distribution, associativity and commutativity over arbitrary finite sums) AND that $\Sigma_{k=1}^{n} k$ is well-defined. This is a hidden induction. But as Sally's proof (below) shows, these additional assumptions represent a 'qualified mathematician's hypotheses'. Of course the generalized algebraic laws work as well for any ring (a proof by induction on the number of addends); but the definition of $\Sigma_{k=1}^{n} k$ does *not*. The ring might not be ordered, let alone well-ordered.

Gauss 2. We now rephrase Gauss's proof in an even more concrete form[7]. A standard problem for introducing the problem of finding a closed form for $\Sigma_{i=1}^{n} i$ is the 'handshake problem'. There are $n+1$ people at a party and each shakes hands with each other person. How many handshakes are there. One analysis is, 'Each of the $n+1$ people shakes hands with n others so there are $n(n+1)$ handshakes. Whoops! I counted each handshake twice. So there are $\frac{n(n+1)}{2}$ handshakes'. But another way to count says the first person shakes hands with n others, avoiding repetitions the second shakes hand with $n-1$, the third with $n-2$. So the sum is

$$\Sigma_{i=1}^{n}(n-i) = \Sigma_{i=1}^{n} i.$$

Since both calculations give us the number of handshakes, the values are equal.

Here the two steps are actually quite separate. The calculation of the number of handshakes does not depend on induction. But then another method of calculation is introduced. The first calculation has no trace of induction[8], although it certainly relies on the connections between the natural numbers and actual counting. The second certainly uses the inductive definition of the Σ notation. There is an apparent use of generalized associativity and commutativity, but not distributivity.

The standard proof. We notice that $1 + 2 = \frac{2(3)}{2}$ and check this for a few more small numbers. Now the mantra says, show $(\forall n)P(n) \rightarrow P(n+1)$. So one asks what is $\frac{n(n+1)}{2} + (n+1)$? With a common denominator of 2, we have[9]

$$\frac{n(n+1)+2n+2}{2} = \frac{n^2+3n+2}{2} = \frac{(n+2)(n+1)}{2}.$$

But why is this explanatory of the formula holding for *all* n? *This* proof translates the basic intuition; I can transform my calculation for 1 into a calculation for 2 into a calculation for 3 . . . into a single step by the power of algebra. That is, the calculation with

[7] While this statement is appealing to students, a more formal version with the same proof is: How many edges are there in the complete symmetric graph on n vertices?

[8] This assertion of course depends on where I begin my arithmetic. There is no trace of arithmetic, if my assumption is that $(N, +, \times)$ is a semi-ring (ring without additive inverse). But there is if I go back one step further and define multiplication inductively from addition.

[9] There is also a geometrical picture to understand the algebra in the numerator of this calculation. Represent $n(n+1)$ by an n high by $n+1$ wide rectangle. Then to add $2(n+1)$, place two 1 by n strips on top of the rectangle.

the variable n which can interpreted as any number. So stepping through the numbers will establish the result for all natural numbers. The procedure is not meaningless. The proof scheme is a distilled hint of how to understand why the proposition is true for all n.

The choice of the particular formula $\frac{n(n+1)}{2}$ may remain a mystery; but adding any of the many geometric pictures for motivating this step can complete the explanation of the result.

The general aim of inductive proofs is to move from observing $P(n)$ for a few n to the statement $\forall n P(n)$. The key contribution of the axiom/rule of mathematical induction is to reduce the intuitive iterative calculation to a finite statement $\forall k[P(k) \rightarrow P(k+1)$. This step is amenable to 'algebraic proof'.

Sally's proof: systematic generalization. Paul Sally's sequence of proofs[10], which follow using both the generalized algebraic laws and the Σ notation, show the generalizing power of algebraic methods. Consider the equation:

$$(n+1)^2 - 1 = \Sigma_{k=1}^{n}((k+1)^2 - k^2). \tag{1}$$

Notice that the right hand side telescopes. (The subtracted term in one summand is the positive term in the previous summand.) So the right hand side simplifies to the left and the equality is true. We can easily give a geometric motivation for this formula. Write a square that is $n+1$ units on a side as a union 1×1 square in the lower left hand corner, then a 2×2 with the same lower left corner, etc. Then note the difference between the successive small squares gives a disjoint cover of the large square.

Note that the kth summand on the right side of Equation 1 simplifies to $2k+1$ so the right hand side *en toto* simplifies to $n + 2\Sigma_{k=1}^{n} k$. So we have

$$(n+1)^2 - 1 = n + 2\Sigma_{k=1}^{n} k$$

and a little algebra gives

$$\Sigma_{k=1}^{n} k = \frac{n(n+1)}{2}.$$

But this approach yields more. The same telescoping argument shows

$$(n+1)^3 - 1 = \Sigma_{k=1}^{n}((k+1)^3 - k^3).$$

Again we analyze the right hand side. Each summand is $k^3 + 3k^2 + 3k + 1 - k^3$ which equals $3k^2 + 3k + 1$. So we have

$$(n+1)^3 - 1 = 3(\Sigma_{k=1}^{n} k^2 + \Sigma_{k=1}^{n} k) + n.$$

Using the formula for the sum of the first n positive integers and moving all but the first term on the right to the left, $3(\Sigma_{k=1}^{n} k^2)$ equals $n^3 + 3n^2 + 3n + 1 - 1 - n - 3(\frac{n(n+1)}{2})$. Now again a little, very basic, algebra gives

[10] Paul Sally presented this argument at a University of Chicago class for high school teachers on Aug. 3/4, 2012. Doubtless, the approach is old; the use of telescoping series dates at least to the Bernoulli's, Euler and Goldbach [BVP06]. Sally was not only a distinguished researcher in p-adic analysis and representation theory but a national leader in Mathematics Education.

$$\Sigma_{k=1}^{n} k^2 = \frac{n(n+1)(2n+1)}{6}.$$

Let us emphasize what is explanatory about this proof. If we think of the formulas for the first n squares, cubes, 4th powers etc. as a sequence of distinct problems that require a new inspiration for the formulas for each larger power, they are a great mystery. But this proof provides a unified strategy for attacking all the problems. There isn't (here) a geometric picture for the formulas after the first couple of dimensions but we have (implicitly) a procedure for generating each formula. The telescoping procedure can be visualized intuitively in exactly the same way as the Gauss argument.

What are the hypotheses of this proof? The algebra of polynomials is assumed (including generalized associativity and generalized distributivity which must be established by a separate induction) and the whole argument is an induction on the power of k in the sequence being summed.

Assertions of the non-explanatory nature of the standard inductive proof seem to be based on the idea that the algebraic manipulations in going from $P(n)$ to $P(n+1)$ are inherently non-explanatory. The glory of algebra is that one does not need (and possibly cannot) keep track of the meaning of each term in a derivation; nevertheless, the variables have the same interpretation at the end of the derivation as the beginning. The thought seems to be that losing track of the explicit reference of each term means the argument is non-explanatory but mere calculation. We have just seen the fallacy of this assertion in Sally's methods of generalization.

Steiner [Ste78] quotes Kreisel and Feferman as suggesting that the more explanatory proof is the more general and/or abstract. He gives three more specific versions of this assertion; I quote only the third.

> (c) Of two proofs of the same theorem the more explanatory is the more abstract (or general).
> Kreisel explicitly adopts (c) – in a private communication –writing that "familiar axiomatic analysis in terms of the greater generality of (the theorems occurring in) one proof than (in) the other" is 'sufficient' to distinguish between proofs' explanatory value. (page 136 of [Ste78])

Steiner rejects this view arguing (page 144 of [Ste78]) ' It is not, then, the general proof which explains; it is the generalizable proof.' This description fits Sally's proof well. Steiner refines this notions further and writes 'an explanatory proof depends on a characterizing property of something mentioned in the theorem: if we 'deform' the proof, substituting the characterizing property of a related entity, we get a related theorem.' Unfortunately this notion of characterizing property is elusive as demonstrated by the examples in [HM05]. But in this particular case, it seems we can identify the characterizing property as the representation of $(n+1)^2 - 1$ as a telescoping series. The tool of telescoping series is exploited.

The generality of a formal proof. Recall the hypotheses (Gauss 1) of my first explanation of writing a correct proof in naive set theory. Equally well it could be thought of as a proof in second order arithmetic. The definition of n-adic addition is in fact made by a recursive definition (in the technical sense). Both that defininion and the generalized algebraic laws can formalized in first order Peano arithmetic. So, writing PA^1 for first order Peano arithmetic, we have a theorem:

$$PA^1 \vdash \Sigma_{k=1}^{n} k = \frac{n(n+1)}{2}.$$

Now from the standpoint of Hanna this seems like a hair-splitting analysis of the hypotheses. What it actually shows is that this formula holds not just for the Natural Numbers but for any model of first order Peano arithmetic. It is a vast generalization. Even more the same analysis applies to Sally's argument.

We have presented five 'proofs' of the formula for the sum of the first n numbers. We see that in order to assess the explanatory value of each we must carefully stipulate the hypotheses. We see that one can avoid an actual induction, if one assumes the generalized algebraic rules and the definition of the Σ symbol. But this just hides the explanatory core of the solution.

3 Inductive Definition and Inductive Proof

A mathematical induction almost always is a response to an inductive definition. The motivating definition is often hidden from view as in the case of the examples from arithmetic analyzed above, where the generalized algebraic sum and anadic addition are often taken as just part of 'general mathematical knowledge'. But it is much more evident in proofs that involve algebraic constructions.

Generalized inductive definition[11] is required to construct such objects as the closure of a set to a subgroup or in a logic, the set of formulas in logic or theorems of a theory.

Hafner and Mancosu [HM05] criticize the Resnik and Kushner [RK87] assertion that Henkin's proof of the completeness theorem is explanatory, asking 'what the explanatory features of this proof are supposed to consist of'. Here is an answer. The relevant form of the completeness theorem asserts, 'every syntactically consistent first order theory has a model.' We sketch the Henkin proof[12]: T has the *witness property* if for every formula $\phi(x)$, there is a *witness constant* c_ϕ such that

$$T \vdash (\exists x)\phi(x) \rightarrow \phi(c_\phi).$$

Now the proof has two steps: 1) every syntactically consistent theory can be extended to a complete theory with the witness property 2) every complete syntactically consistent theory with the witness property has a model.

Both steps are explanatory. The extension of an arbitrary consistent T to one satisfying the witnessing property depends precisely on the axioms and rules of inference of

[11] A set X is defined by generalized inductive definition if there is rule assigning to each finite subset X_0 of X some larger set X_0' (the closure of X_0) and for each $X_0 \subset X$, $X_0' \subset X$. This notion is given a more inductive format if one starts with a set Y and successively closes it to obtain X. [Sho67]

[12] We take the version form [Mar02] but similar accounts can be found in any modern logic text.

the logic. Extending to a complete theory is often done by Zorn's lemma. If one finds this method unexplanatory, it can be done by an inductive construction adding ϕ_α or $\neg\phi_\alpha$ at stage α; the goal is to ensure that each sentence is decided. To construct the model, consider the set of witnesses, M and show that after modding out by the equivalence relation cEd if and only $T \vdash c = d$, the structure $M' = M/E$ satisfies T. More precisely, show by induction on formulas that for any formula $\psi(\mathbf{c})$,

$$T \vdash \phi(\mathbf{c}) \text{ if and only if } M' \models \phi(\mathbf{c}).$$

Note that here the formulas and theorems of T arise by a generalized inductive definition; so we must induct on the structure of the formulas to complete the proof.

This induction shows exactly how the new structure arises from the syntactic data; in contrast, Gödel reduces the first order case to propositional logic. Further, Steiner's deformation is realized in the many variants of this argument. In particular, the first step of the proof will have minor variants depending on which deductive system is chosen. But the second stage will be the same. Moreover, completeness for many other logics (type theory, infinitary, modal, 2nd order, etc.), the omitting types theorem, and a long list of more technical results in model theory all derive from this method of model construction. A later adaptation of the idea of constructing an actual algebraic object from the syntactic description is applied in a proof of the Hilbert Nullstellensatz (page 89 of [Mar02]). Thus, here we can exhibit Steiner's characterizing property as this uniformly defined transfer from a collection of sentences in a formal language to a mathematical structure.

4 Objections to Lange's argument that proofs by mathematical induction are not explanatory

Lange [Lan09] presents an argument that proofs by induction are generally not explanatory. I have already described how these are arguments are explanatory. Here I try to identify some of the flaws in Lange's analysis. Various authors [Lan,Bak10,Car16,Wys] have raised a number of specific objections, which I found generally sound[13], to his argument. I now advance several further objections that I regard as more basic.

The most basic is that while his argument is against the explanatory ability of a *proof by induction*, Lange reduces the discussion to whether the premises of the argument explain the conclusion. Since it is a serious issue (e.g. [Han90,Ste78,HM05]) whether one proof is more explanatory than another, this reduction clearly misses a major problem.

I now sketch Lange's argument. Noncontroversially, he notes that a proof by mathematical induction proceeds by the following rule of inference[14]:

[13] Particularly relevant is the recognition by Wysocki [Wys] and Cariani [Car16] that the basic thrust of up versus down induction breaks down for argument based on generalized induction definition (such as induction of formulas or closing a subset of a group G to a subgroup of G).

[14] This ignores, of course, the many uses of mathematical induction to prove $P(n)$ and for each $k \geq n$, $P(k) \rightarrow P(k+1)$ then for all $k \geq n$, $P(k)$. His argument could be complicated to handle this case as well as it does the one it explicitly addresses, but he doesn't even consider such situations.

If $P(1)$ and for each natural number k, $P(k) \to P(k+1)$ then for all k, $P(k)$.

He then asserts, 'The *explanans* would be (for some particular property P) the fact that $P(1)$ and that (for any natural number k) if $P(k)$ then $P(k+1)$.'

He then proposes an alternative rule:

If $P(5)$ and both a) for each natural number k, $P(k) \to P(k+1)$ and b) for each natural number k, $P(k) \to P(k-1)$ then for all k, $P(k)$.

Lange argues that not both of these rules can be explanatory. He writes 'Relations of explanatory priority are asymmetric. Otherwise mathematical explanation would be nothing like scientific explanation.' What are the relations between? We have argued that the explanatory object is a proof and such is the title of his paper. But he concludes (where P is some property that may or may not hold of a natural number), 'It cannot be that $P(1)$ helps to explain why $P(5)$ holds *and* that $P(5)$ helps to explain why $P(1)$ holds, on pain of mathematical explanations running in a circle.' This leap exacerbates the reduction of the argument to considering only 'premises and consequence' by conflating the entire explanation with any component of it[15].

Lange's objection is not about induction. Lange's argument applies more generally to show that for any domain A and $a \in A$, $P(a)$ cannot be a partial explanation for the assertion $A \models (\forall x)P(x)$. We will show several examples of arguments of this form that contradict this assertion.

A standard mathematical technique is to show that all elements of some collection[16] Q satisfy $P(x)$ by first showing an *underlying fact* that restricted to Q, $(\exists x)P(x) \leftrightarrow (\forall x)P(x)$. (In Lange's case, Q is the set of natural numbers.) Now to show that all elements satisfy P, we need only find a convenient a such that $P(a)$ holds. There could be another a' almost as convenient and we could conclude the result for still another a'' that would be very hard to check. This theme shows up in such proof paradigms as proving a function is well-defined, showing that an equivalence relation is a congruence, and in many other situations that are more complicated than appropriate for discussion here. Perhaps Lange would argue that such proofs are not explanatory. But they are direct answers to, for example, 'Why is this function well-defined?'

Here are two more specific examples. Let G be a group with a subgroup K. Here Q is the set of cosets of K. Question: Does aK have an element of even order? Underlying fact: If one element of a coset aK has an element of even order then every element has even order. The key (easy) fact is that if a homomorphism f of groups maps x to y, and $y \neq 1$ then the order of x is divisible by the order of y. So if the order of a is even so is the order of $f(a)$ and any member of $f^{-1}(a)$. Now to determine if aK has an element of even order, we can check the order of any element of the coset (preferably one with low order).

Normal form arguments illustrate the same point. To take a high school example, all quadratic equations can be expressed in *each* of three normal forms: (by degree: $ax^2 + bx + c$, factored form: $a(x - r_1)(x - r_2)$, vertex form: $a(x - h)^2 + k$). Here Q is a collection of quadratic polynomials that are all equivalent by the usual algebraic oper-

[15] Baker [Bak10] notes this objection but does not develop it.

[16] If one were to develop this argument in first order logic, Q would be a formula. However, in the spirit of the general discussion of induction we describe here informal mathematical arguments.

ations. Any of the infinitely many of the forms of the polynomial have the same vertex and the same roots. Factored normal form makes the roots evident; vertex normal form makes the vertex evident. Algebraic transformations are used to put the polynomial in a convenient form for the problem at hand. But one must compute the vertex or roots and the normal form makes this easier. So the choice of one (indeed any) particular equivalent to the original polynomial is partially explanatory of finding the roots or vertex of the polynomial. Normal form arguments are, in fact, a clear example of explanatory argument. The ability to reduce to a normal form is the key point of the explanation.

Tappenden (page 171 of [Tap05]) gives a slightly different example that illustrates the same point. The value of an integral over a plane area does not depend on the choice of coordinates[17]; but the ease of evaluation does. So one might find the evaluation by a particular choice of coordinates more explanatory than by another. Thus, one specific case can be a partial explanation of another.

We noted that Lange's argument did not really address inductive proof but any argument for a univesral proposition. These examples demonstrate the failure of Lange's contention that an argument for a universal statement $(\forall x)P(x)$ cannot be explanatory if the arguments appeals to any instance of P. And thus his claim that no proof by mathematical induction can be explanatory also fails.

5 Proof versus Provability

We began with Hanna's distinction between formal and acceptable proof. Neither of these is an appropriate analysis of proof. The second, as she interprets it, is ambiguous about what assumptions are intended. The first was never intended to be such an analysis. Hilbert's goal was to study the *existence* of a proof by providing certain minimal characteristics. He deliberately ignored in this focus on reliability such aspects of a 'good' proof as motivation, irredundancy, organization. While we hailed generalizability as a hallmark of an explanatory proof, one must also note that a proof can be too general. In saying this, we bring out still another aspect of 'explanatory proof'; the quality of an explanation depends on the intended audience[18] The importance of audience is emphasized by this description by Fields Medalist William Thurston of the reaction to his proof of the 'geometric Haken conjecture, a revolutionary result in low-dimensional topology.

> It became dramatically clear how much proofs depend on the audience. We prove things in a social context and address them to a certain audience. Parts of this proof I could communicate in two minutes to the topologists, but the analysts would need an hour lecture before they would begin to understand it. Similarly, there were some things that could be said in two minutes to the analysts that would take an hour before the topologists would begin to get it. [Thu94]

[17] Here, Q is the set of possible coordinatizations.

[18] In fact, Hanna's article in an educational journal reflects the common use of Gauss' proof for future American teachers of middle school mathematics. The goal of the activity is not understanding the step from example to universal but just some notion of justification.

None of these characteristics of 'good proof' are captured by 'a proof is a sequence of statements each of which is an axiom or deduced from prior statements by one of clearly stated list of rules of inference'. As Burgess [Bur10] puts it, 'For formal provability to be a good model of informal provability it is not necessary that formal proof should be a good model of informal proof.'

On the other hand, the proofs that contain only motivation for the inductive step, miss the real difficulty. How can a finite process of proof justify a statement about infinitely many objects? Thus, in constructing more explanatory proofs above, we have included the both inductive definition and proof that is essential for explaining this step but also a motivation (often geometric) for the induction step. Such proofs both verify and explain.

After much of this paper was written, we found our view summarised in the earlier work of Resnik and Kushner who, employing Van Fraassen's notion of a why-question, wrote,

> nothing is an explanation *simpliciter* but only relative to the context-dependent why-question(s) that it answers. . . . Whether or not a given proof counts as an explanation depends on the why-question with which it is approached. (page 153 of [RK87])

We return to our original theme of the interaction of reliability and clarity. As Tappenden explores with rich examples and from several perspectives in [Tap05], the notion of mathematical explanation needs to be treated in the general context of the development of an area of mathematics. Our examples, even while focusing on the proof of specific propositions, have demonstrated several aspects of context dependence: the exact choice of hypotheses, what precisely is to be explained, and to who.

The notion of good proof restores a proper balance between 'reliability' and 'clarity' that is lost by mistakenly identifying 'provable' with 'a proof'.

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