ON FORMALLY MEASURING AND ELIMINATING EXTRANEOUS NOTIONS IN PROOFS

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ABSTRACT. Many mathematicians and philosophers of mathematics believe some proofs contain elements extraneous to what is being proved. In this paper I discuss extraneousness generally, and then consider a specific proposal for measuring extraneousness syntactically. This specific proposal uses Gentzen's cut-elimination theorem. I argue that the proposal fails, and that we should be skeptical about the usefulness of syntactic extraneousness measures.

Many mathematicians and philosophers of mathematics think that it's somehow valuable for a proof to be "pure", that is, for it not to use notions extraneous to what is being proved. Not worrying about why that is for now, we would like to make better sense of the suggestion. What does it mean for a notion used in a proof to be extraneous to the theorem being proved? One way of making this sharper would be to develop a syntactic way of evaluating extraneousness. I want to consider such a proposal, using Gerhard Gentzen's cut-elimination theorem. I will argue that there are serious obstacles to making this proposal work.

1. FOUR CLAIMS CONCERNING EXTRANEOUSNESS

Bertrand's postulate states that for every natural number $n \geq 1$, there is a prime number between n and $2n$. It is so named because it was verified by computation for

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$n < 3,000,000$ by Joseph Bertrand in 1845. That mathematicians continue to call it a postulate is an artifact of Bertrand's original approach, for it was proved by Pafnuty Chebyshev in 1850. It was reproved by Srinivasa Ramanujan in 1919, and again by Paul Erdős in 1932.[1]

In a recent paper, J. Alan Robinson has written about Erdős's proof of Bertrand's postulate as follows:

> It is an example of how sometimes a proof draws on facts and ideas which seem wholly extraneous to the proposition being proved. (Gentzen's Hauptsatz states essentially that if a proposition is [formally] provable at all, then it is formally provable using only notions which are present in the statement of the proposition itself. It is hard to believe, is it not, that this assertion holds for informal "real" proofs?)[2]

Here, Robinson begins by noting the widespread belief that the facts and ideas used in a proof can be extraneous to what's being proved. He then notes that work in proof theory might be thought to bear on this belief in two ways: firstly by making clear what it might be for a fact or idea to be extraneous to a statement; and secondly by identifying the conditions under which a proof of that statement can be found that avoids all extraneous facts and ideas. Robinson likely has in mind suggestions made by Gentzen and Takeuti that I'll address specifically a little later. Lastly, he suggests skepticism about this proof-theoretic proposal, at least in its applicability to informal, rather than formal, proof.

In this paper, I want to amplify and extend Robinson's points, particularly regarding skepticism about the prospects of syntactic work for measuring extraneousness. Toward this, I want to state four claims about syntax and extraneousness that Robinson has addressed:

(1) The facts and ideas used in a proof can be extraneous to what's being proved.

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[1] Cf. Aigner and Ziegler [2004], p. 7.

[2] Cf. Robinson [2000], p. 283.

(2) The extraneousness of a notion in a proof to the proof's conclusion can be measured by whether or not the notion is present in the conclusion.

(3) Every formally provable statement is formally provable without using extraneous notions (as a result of Gentzen's Hauptsatz).

(4) There's no good reason to think that every statement can be informally proved without using anything extraneous.

Like Robinson, I think claim (4) is right. A full explanation would take us too far astray, but in short, I believe that at present we have no good theory of informal proof, nor do we have a systematic survey of all informal proofs that shows that extraneousness can always be avoided. Instead, in this paper I'll focus on claims (1)–(3). I want to extend Robinson's skepticism about the effectiveness of syntactic work for measuring extraneousness, as follows. I'll first discuss extraneousness generally, and extraneousness in the sense of (2) specifically. Next, I'll explain how proof theorists have proposed treating extraneousness formally, and discuss the value of this formal treatment. I'll then refute a straightforward reading of (3), and propose and evaluate three other readings of (3) suggested by ongoing work in proof theory. My conclusion will be that we should concentrate our attention on developing semantic, rather than syntactic, measures of extraneousness.

2. EXTRANEOUSNESS

Claim (1) states that proofs can contain elements extraneous to what's being proved. Declarations like this are common in mathematics. Mathematicians regularly aspire to eliminate extraneous notions from proofs, and are explicit about this. In a paper on a theorem of Tannaka and Krein in functional analysis, for instance, Salomon Bochner remarks that:

> The proof of Krein is based on ideas of N. Wiener and I. Gelfand which are extraneous to the problem, and we are going to give a new proof which stays wholly within the technique of uniform approximation…"[3]

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[3] Bochner [1942], p. 56.

Bochner does not say how he has determined what is extraneous to the problem. Even so, his judgment was evidently within the bounds of normal practice at the time, as measured by the reception of Bochner's paper by peers.[4]

Another example of a proof using notions extraneous to what's being proved is Harry Furstenberg's proof of the infinitude of primes.[5] Topology is *prima facie* irrelevant to this theorem. Open sets and topological bases have nothing obvious to do with the successor operation or primality. One could perfectly well understand what the infinitude of primes states without knowing what an topological space is. Indeed, the whole appeal of Furstenberg's proof is in its surprising use of an evidently irrelevant subject matter to reprove a classic theorem. Furstenberg isn't claiming that the topological proof gives new insight into the nature of primes or their distribution. His aspiration, I take it, was to give a proof that used obviously extraneous elements.[6]

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[4] No less an expert than Robert Cameron, in a review of Bochner's paper, confirms Bochner's view that Krein's proof involves "difficult concepts apparently extraneous to the problem", while Bochner's proof is "direct and relatively simple". Cf. *Mathematical Reviews*, MR0005788.

[5] Cf. Furstenberg [1955], p. 353. The infinitude of primes was proved by Euclid in *Elements* Book IX, Proposition 20. Furstenberg's proof goes as follows. He first puts a topology on the integers, by taking the arithmetic progressions $B_{a,b} = \{a + bn : n \in \mathbb{Z}\}$, for $a, b \in \mathbb{Z}, b > 0$, as the basic open sets. He then argues that each $B_{a,b}$ is also closed. Next, Furstenberg considers the set $A = \bigcup_p B_{0,p}$ for $p \geq 2$ prime. If there were only finitely many primes, then A would be the union of finitely many $B_{0,p}$. Since each $B_{0,p}$ is closed, A would be the union of finitely many closed sets, and hence would be closed. Since every integer besides ± 1 has a prime factor, every integer besides ± 1 is contained in some $B_{0,p}$. Thus, $A = \mathbb{Z} - \{-1, 1\}$. Then $\{-1, 1\}$, being the complement of a closed set, would be open. But this is impossible, since the basic open sets $B_{a,b}$ are all infinite, and every open set is a superset of some basic open set. Hence there are infinitely many primes.

[6] Neil Tennant has suggested the following response to this point. The extraneousness of the topology used in Furstenberg's proof could turn out to be only superficial or merely apparent, if its use of topology, when formalized in set theory, turns out to be sufficiently weak. Suppose for instance that the topology it uses can be formalized in a weak fragment of set theory, e.g. one that uses just boolean operations on simple sets of natural numbers. Then the allegedly extraneous topological elements are really just reconceptualizations of what was already the concern, namely sets of natural numbers, and hence are in fact relevant to the infinitude of primes. (Along these lines, in Cass and Wildenberg [2003] D. Cass and G. Wildenberg have shown that Furstenberg's proof can be reformulated in the language of periodic functions on integers, without reference to topology.)
I think this response is misguided, for three main reasons. Firstly, not every reconceptualization of a subject matter is necessarily relevant to that subject matter. Robinson seems to think that binomial coefficients are irrelevant to Bertrand's postulate, but both concern just the natural numbers. Secondly, the infinitude of primes doesn't seem to concern sets at all, so I don't see why sets, even simple ones, are relevant to the problem. Thirdly, if Furstenberg's proof were formalized in a different way, say in a theory in which the notion of "open set" was taken as primitive (as in Hausdorff [1962], pp. 258–261), the response would not work. Why should set-theoretic formalization take precedence over these alternatives?

I now want to turn back to what Robinson said. Robinson is responding to Erdős's proof of Bertrand's postulate, in which facts concerning binomial coefficients are used to establish upper and lower bounds on various (real and integral) quantities.[7] Evidently, Robinson thinks that these facts concerning binomial coefficients are extraneous to Bertrand's postulate. He doesn't say *why* he thinks this, but claim (2) provides one explanation. The "facts and ideas" concerning binomial coefficients and real approximations aren't "present in the statement of" Bertrand's postulate, at least not in any obvious sense, and so they are extraneous to it.

This raises the question of what it could mean for a fact or idea to be "present in" a theorem. One proposal is that what is present in a theorem is whatever must be understood or accepted in order to understand that theorem. These concepts and truths are the conditions for understanding the theorem, and as such are part of its content. That is why we can rightly call these concepts and truths "present in" the theorem.[8]

This semantic reading of "present in" is suggested by H.S.M. Coxeter's remarks concerning Sylvester's problem.[9] Coxeter says of Sylvester's problem that "This matter of collinearity clearly belongs to ordered geometry", and solves it in ordered geometry, in which betweenness is the basic notion.[10] He believes that betweenness is "present in" Sylvester's problem because straight lines are rightly defined in terms of betweenness: a line segment is, by definition, the set of points between two points.[11] Since the definition of straight line that Coxeter favors does not involve distance, he comments on on L.M.

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[7] Erdős's proof is explained and simplified (based on another observation of Erdős concerning binomial coefficients) in Chapter 2 of Aigner and Ziegler [2004].

[8] I am assuming here that understanding a theorem is not the same as grasping its truth conditions.

[9] Sylvester's problem says: let n given points have the property that the straight line joining any two of them passes through a third of the given points. Show that the n points lie on a straight line. It is so named because it was originally posed by J.J. Sylvester in Sylvester [1893]. This formulation is due to Erdős, in Erdős [1943], p. 65.

[10] Coxeter presents the axioms of ordered geometry, adapted from Pasch and Veblen's earlier treatments, in Coxeter [1989], pp. 177–8.

[11] Coxeter writes (Coxeter [1989], p. 176), "The essential idea [for problems like Sylvester's] is *intermediacy* (or 'betweenness'), which Euclid used in his famous definition: *A line (segment) is that which lies evenly between its ends.* This suggests the possibility of regarding intermediacy as a primitive concept and using it to define a line segment as the set of all points between two given points." Coxeter then defines rays in terms of line segments and betweenness, and lines in terms of rays and line segments. The definitions can be found on Coxeter [1989], p. 181.

Kelly's metric solution of Sylvester's problem that it "involves the extraneous concept of distance".[12] On this semantic reading of "present in", Coxeter's view is that metric notions are not present in Sylvester's problem.[13]

A difficulty with this semantic account of extraneousness is that it's hard to say what's necessary to understand and accept to understand a given theorem. The difficulty is clear in the case of Sylvester's problem: there are many other ways of defining straight line, some of which are significantly different from Coxeter's. In differential geometry, there are metric definitions of straight line as the shortest line between two given points. If we adopt such a definition, then betweenness becomes extraneous to Sylvester's problem, contra Coxeter. To support Coxeter's contention, we need some principled reason for maintaining the necessity of understanding and accepting Coxeter's "Euclidean" definition of straight line for understanding Sylvester's problem, and for rejecting the necessity of understanding and accepting a metric definition. This example illustrates the limits of the proposed semantic approach to clarifying what it is to be semantically present in a theorem. We currently lack a definitive, uncontroversial way of determining what concepts and truths must be understood or accepted in order to understand a given theorem.

I think claim (3) suggests a syntactic alternative to semantic readings of "present in" that attempts to bypass the semantic difficulties just discussed. Before considering this syntactic alternative, I want to introduce some terminology. Rather than speaking of something being not extraneous to a theorem, I'll say that it is "relevant" to that theorem. When a proof contains only elements relevant to what's being proved, I'll call such a proof "pure".

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[12] Cf. Coxeter [1989], p. 181.

[13] As noted, Coxeter's solution did not use distance. N. Steenrod had earlier solved Sylvester's problem in projective geometry (cf. Erdős [1944], p. 171). Both of these results show that distance is unnecessary for solving Sylvester's problem. Notice that Coxeter did not conclude that distance is extraneous to Sylvester's problem from these facts about what is or is not necessary to solve Sylvester's problem, but from a semantic fact about the proper definition of straight line. That is because he correctly recognized that what is extraneous to a theorem is distinct from what can be used to prove that theorem.

3. DETERMINING EXTRANEOUSNESS FORMALLY

To describe the syntactic way of evaluating relevance suggested by (3), I'll first need to review some proof theory.[14] Let *LK* denote Gentzen's "sequent" formulation of the first-order predicate calculus (without equality).[15] Each line of a proof in *LK* consists of *sequents*, which have the form $A_1, \ldots, A_m \Rightarrow B_1, \ldots, B_n$, where the A_i and B_j are formulas and $\Rightarrow$ is a symbol called the "sequent arrow". The intended meaning of the sequent $A_1, \ldots, A_m \Rightarrow B_1, \ldots, B_n$ is the formula

$$(A_1 \wedge A_2 \wedge \cdots \wedge A_m) \rightarrow (B_1 \vee B_2 \vee \cdots \vee B_n)$$

where $\rightarrow$ is the material conditional. The sequences of formulas $A_1, \ldots, A_m$ and $B_1, \ldots, B_n$ are called *cedents*, and are abbreviated by Greek capital letters Γ, Δ, etc.

Proofs in *LK* consist of sequents inferred from other sequents by two types of inference rules, which Gentzen called "structural" and "operational". The operational inference rules are introduction and elimination rules for the logical connectives and quantifiers of first-order logic. The structural inference rules include rules for switching around the order of formulas inside sequents (known as exchange rules), for eliminating duplicates of a formula on the same side of a sequent arrow (known as contraction rules), and for adding formulas to the left or right of the sequent arrow (known as weakening or dilution rules).[16] They also include the rule known as "cut", which has the form

$$\frac{\Gamma \Rightarrow \Delta, A \qquad A, \Gamma \Rightarrow \Delta}{\Gamma \Rightarrow \Delta}$$

and where A is called the "cut formula".[17] The use of cuts is comparable to the use of lemmas, in that the cut formula is not involved in—that is, is not a subformula of—what is being inferred. To infer $\Gamma \Rightarrow \Delta$ (say, concerning circles and lines), a cut uses statements

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[14] This technical overview is mostly adopted from Sam Buss' overview in Buss [1998], with some modifications from Gentzen [1935].

[15] By "without equality", I mean without axioms or introduction / elimination rules for equality. The equality symbol can occur as a predicate in formulas in *LK*, but those formulas will have to be introduced by the axioms and rules of *LK*.

[16] More precisely, the left and right weakening rules are $\frac{\Gamma \Rightarrow \Delta}{A, \Gamma \Rightarrow \Delta}$ and $\frac{\Gamma \Rightarrow \Delta}{\Gamma \Rightarrow \Delta, A}$, respectively.

[17] For a complete listing of these rules, see Buss [1998], pp. 11, 32.

involving the formulas in Γ and Δ, as well as another formula A (say, concerning right angles, also).

Initial sequents of proofs in LK are restricted to sequents of the form $A \Rightarrow A$, with A atomic; in other words, instances of the law of excluded middle for atomic formulas.[18]

I can now state Gentzen's Hauptsatz. This result states that every proof in LK can be transformed into a cut-free proof in LK with the same endsequent (i.e. conclusion).[19] Since the Hauptsatz yields proofs without instances of the cut rule, it is also known as the "cut-elimination theorem".

Gentzen noted that the following "subformula property" is a corollary of his Hauptsatz: all the formulas occurring in cut-free LK proofs are "subformulas" of the conclusion. The subformulas of a given sequent are all the substrings of the formulas comprising that sequent that are formulas.[20] This is because cut is the only inference rule in LK in which a formula that is a subformula of one of the upper sequents of the inference is not a subformula of the lower sequent.

Gentzen describes the import of the subformula property as follows:

> The final result is, as it were, gradually built up from its constituent elements. The proof represented by the derivation is not roundabout in that it contains only concepts which recur in the final result.. . . No concepts enter into the proof other than those contained in its final result, and their use was therefore essential to the achievement of that result.[21]

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[18] In Gentzen [1935], p. 82, Gentzen calls this restriction a requirement for LK being a "logistic" deductive calculus. A "logistic" deductive calculus, he explains, is one "in which the derivations do not. . . contain assumption formulae", and uses introduction and elimination rules. The restriction to initial sequents of the form $A \Rightarrow A$ for A atomic restricts the premises of LK proofs to logical truths of a particularly constrained type. I will revisit this restriction and its implications for Claim (3) later.

[19] Cf. Gentzen [1935], p. 87. The transformation can be done constructively.

[20] I will follow Negri and von Plato's convention that $A(t/x)$ is a subformula of $\forall x A$ and $\exists x A$ for all terms t, where $A(t/x)$ denotes the substitution instance of t for variable x in formula A. The notions of substitution and subformula can be defined inductively; for more details, see e.g. Negri and von Plato [2001], p. 63, and Enderton [2001], p. 113.

[21] Here I have combined two passages from Gentzen [1935], p. 88, 69.

Similarly, Takeuti has written of the subformula property that it shows that "any theorem in the predicate calculus can be proved without detours, so to speak."[22] What Gentzen and Takeuti are observing is that every statement provable in *LK* has a proof every formula of which is a subformula of that statement.

These observations can be restated more succinctly using the following definition. We say that a formula D is *J-relevant* to the conclusion C of a proof P if and only if D is a subformula of C. In other words, whether a notion is present in a statement can be measured syntactically by whether or not the notion, formally expressed, is a subformula of that statement, formally expressed. I'll follow the nomenclature of Section 2 and say that a proof P of a conclusion C is *J-pure* if and only if every formula occurring in P is J-relevant to C. Gentzen and Takeuti's observations then read as follows: as a result of the Hauptsatz, every statement provable in *LK* has a J-pure proof—that is, a proof every formula of which is J-relevant to its conclusion.

I want next to discuss why J-relevance is an appealing way of measuring relevance. Toward this, I want to describe what features make for a good relevance measure.[23] The main purpose of a relevance measure should be to enable the sharp and accurate evaluation of relevance. That is, whether or not D is relevant to C according to the measure should be clearly determinate; and D should be measured as relevant to C just in case D would be judged relevant to C by the standards of ordinary mathematical practice. Of course, what counts as relevant by the standards of ordinary mathematical practice is sometimes unclear, as discussed with respect to Coxeter's definition of straight line earlier. So a secondary purpose of a relevance measure should be to shed more light on what ought to be counted as relevant in ordinary practice. One way to pursue this end

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[22] Cf. Takeuti [1987], p. 21–2, and again on p. 29.

[23] When I discuss relevance 'measures', I am not assuming that they are necessarily measures in the mathematical sense, e.g. in measure theory. I just mean ways of determining whether something is relevant to something else, and perhaps of determining the *degree* of relevance.

would be to settle on relevance measures that are amenable to mathematical investigation, for instance metamathematically[24] or topologically.[25] These types of investigations would clarify the conditions under which statements are relevant to others on the measure, which might help clarify the conditions under which statements ought to be judged relevant to others in ordinary practice.

I want to evaluate the extent to which J-relevance has these features. Firstly, it is clearly determinate whether a statement is J-relevant to another. Since the subformula property is a syntactic relation between finite strings, it is effectively decidable whether one string is J-relevant to another.

Secondly, as to whether a statement D is J-relevant to a statement C just in case D would be judged relevant to C by the standards of ordinary mathematical practice: no, as I'll explain. According to ordinary practice, definitions of the terms occurring in a statement are relevant to that statement. But those definitions may not be J-relevant to the statement, as I'll show by an example. Consider the infinitude of primes theorem, stated formally in the language of first-order Peano arithmetic:

$$\forall a \exists b [b > a \wedge \forall x [\exists y (x \cdot y = b) \rightarrow (x = 1 \vee x = b)]]. \tag{1}$$

Ordinarily, multiplication is defined in terms of successor. So the axioms defining successor, for instance, ought to be relevant to (1). But no formula involving successor is a *subformula* of (1). As a result, these axioms are not J-relevant to (1). I will return to this issue in Section 4.3.

However, it is true that every statement D that is J-relevant to a statement C would be judged relevant to C by the standards of ordinary mathematical practice, even if only in a syntactic sense.[26] That means that if we judge D to be relevant to C based on D's

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[24] E.g. by determining under what conditions a theorem has a proof consisting just of statements that are relevant according to the measure in question.

[25] E.g. by determining whether the relevance measure induces a metric, and if so, whether for every ϵ and for every statement C there is another statement D within ϵ of C under the metric.

[26] I want to stress the "even if only in a syntactic sense" part. Let Γ consist of formulas stating the premises of Furstenberg's topological proof, and let Δ consist of a formula stating the infinitude of primes. Then the

being J-relevant to *C*, we won't be mistaken (according to the standards of ordinary practice). While that is not all we hoped for regarding the accuracy of J-relevance, it is good nonetheless.

Finally, J-relevance is well-suited to mathematical investigation. In this paper I will investigate its metamathematical properties, in particular using methods and results from proof theory. It can be studied topologically as well, though I will not do so here. J-relevance is thus a good candidate for further investigation.

4. CLAIM (3)

Claim (3) states Gentzen and Takeuti's views of relevance. Gentzen and Takeuti observed that every statement φ provable in *LK* has a J-pure proof—that is, a proof every formula of which is J-relevant to its conclusion. Claim (3) says that every formally provable statement has a proof every notion of which is present in the conclusion, and that this follows from Gentzen's Hauptsatz. Since the subformula property follows from the Hauptsatz, and the subformula property suggests a way to understand when a notion is present in a statement—namely, J-relevance—I think it is reasonable to restate (3) as:

(3^*) Every formally provable statement has a J-pure proof, as a consequence of the Hauptsatz.

Next, I want to investigate how we should understand formally provability in Claim (3^*). Since this claim arises in the context of cut-elimination, presumably it should be understood as provability in a formal system with a cut rule. I will take it to refer to provability in a sequent calculus, such as *LK*.[27] If he means provability in *LK*, then while (3^*) is true (as observed by Gentzen and Takeuti), the problem is that only logical truths

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formulas in Γ are subformulas of the sequent $\Gamma \Rightarrow \Delta$, and hence are syntactically relevant to $\Gamma \Rightarrow \Delta$, but they are not ordinarily thought to be semantically relevant to $\Gamma \Rightarrow \Delta$, as discussed earlier.

[27] There are other sequent calculi for (classical) predicate logic besides Gentzen's *LK*, equivalent to *LK* in that they prove exactly the same sequents as *LK*. I will use one in Section 4.2, documenting its equivalence to *LK*.

are formally provable.[28] It's thus more plausible to understand formal provability in Claim (3^*) as provability in some extension of LK.

The problem now is that extending LK threatens the subformula property, and hence the truth of (3^*). Suppose we allow initial sequents from sets $\mathfrak{S}$ in addition to sequents of the form $A \Rightarrow A$.[29] These sets are not restricted to logical truths.[30] Let $LK_{\mathfrak{S}}$ denote such an extension of LK.

This understanding of formal provability yields the following straightforward reading of (3^*):

(3a) Every statement provable in an extension $LK_{\mathfrak{S}}$ of LK has a J-pure proof, as a consequence of the Hauptsatz.

However, (3a) is false. In general, systems $LK_{\mathfrak{S}}$ do not admit cut-elimination. As Jean-Yves Girard puts it, "the cut-elimination theorem holds for predicate calculus, but fails for first-order theories, as soon as they contain proper axioms."[31] To show this, he presents the following derivation:

$$\dfrac{\Rightarrow A \qquad \dfrac{\Rightarrow A \to B \qquad \dfrac{A \Rightarrow A \qquad B \Rightarrow B}{A \to B, A \Rightarrow B}\,(\mathrm{L}\!\to)}{A \Rightarrow B}\,(\mathrm{Cut})}{\Rightarrow B}\,(\mathrm{Cut})$$

This is a derivation of $\Rightarrow B$ from the set $\mathfrak{S}$ consisting of axioms $\Rightarrow A \to B$ and $\Rightarrow A$. By considering every way of proving $\Rightarrow B$ from these axioms, Girard shows that it can't be derived without using cut.[32]

For $LK_{\mathfrak{S}}$ in which cut-elimination fails, the subformula property does not necessarily hold. In such systems, there may be proofs with formulas that do not occur in the

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[28] By the soundness and completeness theorems for LK, though, a sentence φ is provable in LK if and only φ is a logical truth. Cf. Buss [1998], p. 33. This isn't surprising, since the only axioms allowed in LK are logical truths.

[29] We suppose these sets $\mathfrak{S}$ to be closed under substitution, meaning that for formulas Γ and Δ and free variable a, if $\Gamma(a) \Rightarrow \Delta(a)$ is in $\mathfrak{S}$ and t is a term, then $\Gamma(t) \Rightarrow \Delta(t)$ is also in $\mathfrak{S}$.

[30] Examples of such sets $\mathfrak{S}$ include axioms for equality, first-order arithmetic, and Tarski's geometry.

[31] The quote is from Girard [1990], p. 104. I will follow Girard and henceforth mean by "axioms", unless otherwise noted, statements that are not logical truths. The reason for special terminology is that in LK there are axioms available, but they are all of the form $A \Rightarrow A$, and hence logical truths. These are still available in extensions of LK.

[32] Cf. Girard [1990], pp. 125–6; cf. also Negri and von Plato [1998], pp. 418–9, and Buss [1998], p. 33.

conclusion—these formulas vanish as a result of cuts—which cannot be transformed into cut-free proofs. If the subformula property fails for a proof P and conclusion C, then some formula D occurring in P is not a subformula of C. Hence D is not J-relevant to C, and so P is not J-pure. Thus, in systems $LK_{\mathfrak{S}}$ where cut-elimination fails, there may be formally provably statements that are not J-purely provable. Consequently, (3a) is false.

Girard's claim that cut-elimination fails whenever axioms are added to sequent calculi bodes badly for (3^*), but what we've seen so far is not enough reason to give up on it. That's for two reasons: firstly, that while cut-elimination may fail, there may be weaker cut-elimination theorems that yield acceptable versions of (3^*); and secondly, there may be other ways of adding axioms to sequent calculi than the approach just considered, and those may yield acceptable versions of (3^*). I want to turn next to considering these possibilities.

4.1. **Free-cut elimination.** While not every cut can be eliminated in general from proofs in systems $LK_{\mathfrak{S}}$, some, the so-called "free cuts" can. Free cuts are, roughly, cuts whose cut formulas don't occur in an axiom from $\mathfrak{S}$ used in the proof. So a free-cut free proof is, roughly, one in which all remaining cut formulas occur in an axiom from $\mathfrak{S}$ used in the proof. The free-cut elimination theorem says that if a statement is provable in $LK_{\mathfrak{S}}$, then a free-cut free proof of that statement can be found. I now want to discuss this result, and whether it can be used to salvage (3a), or a reasonable substitute for (3a).

To explain the free-cut elimination theorem more precisely, I will need some more proof-theoretic background.[33] The *side formulas* of an inference are the formulas occurring in the cedents Γ, Δ. The *principal formula* of an inference is the formula in the lower sequent that isn't a side formula. All inferences except for cuts have principal formulas. An *auxiliary formula* of an inference is a formula in the upper sequent that isn't a side formula. In the inference

$$\frac{C \Rightarrow D, A, B}{C \Rightarrow D, A \vee B}$$

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[33] Cf. Buss [1998], pp. 12, 16, 43.

which uses the (directionally) right inference rule for $\vee$, C and D are the side formulas, $A \vee B$ is the principal formula, and A and B are auxiliary formulas. All inferences except for weakenings have at least one auxiliary formula. Usually the principal formula is the formula in the lower sequent containing the logical symbol introduced in that inference, as in the example just given, and the auxiliary formulas are the formulas in the upper sequent to which that inference affixes the logical symbol.

The *immediate descendent* of an auxiliary formula in an inference is the principal formula of that inference, with two exceptions. Cut formulas do not have immediate descendants, and the immediate descendants of auxiliary formulas A, B in exchange inferences are A, B, respectively. If C is a side formula in an upper sequent of an inference, say the i^{th} subformula of a cedent, then its immediate descendent is the same formula occurring in the lower sequent in the corresponding i^{th} position of the same cedent. A formula C is the *immediate ancestor* of a formula D if and only if D is the immediate descendent of C. C is an *ancestor* of D if and only if there is a chain of immediate ancestors from D to C. C is a *direct ancestor* of D if and only if C is an ancestor of D and C and D are the same formula.

We are now nearly ready to define free cuts. A cut formula is *directly descended* from a set of sequents $\mathfrak{S}$ if and only if it has at least one direct ancestor occurring in a sequent belonging to $\mathfrak{S}$. Put more loosely, a cut formula A is directly descended from $\mathfrak{S}$ if it occurs in an initial sequent belonging to $\mathfrak{S}$, and there is a chain of inferences from that initial sequent to A in which A occurs at each step. A cut formula is *anchored* if and only if it directly descends from $\mathfrak{S}$. A cut inference is *anchored* if and only if either the cut formula is atomic and both occurrences of the cut formula are anchored, or the cut formula is not atomic and at least one occurrence of the cut formula is anchored. Finally, a cut inference is *free* if and only if it is not anchored.

I can now state the free-cut elimination theorem. Let $\mathfrak{S}$ be a set of sequents closed under substitution. If $\Gamma \Rightarrow \Delta$ is provable in $LK_{\mathfrak{S}}$, then $\Gamma \Rightarrow \Delta$ is provable in $LK_{\mathfrak{S}}$ without using any free cuts.[34] There still may be cuts, so the subformula property fails to hold in

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[34] For a proof, see Buss [1998], p. 44.

general for free-cut free proofs. But there are no free cuts, so all the remaining cuts are anchored. That means that the cut formula of each remaining cut is (a direct descendent of) a formula occurring in an initial sequent of the proof belonging to $\mathfrak{S}$.[35] Hence the cut formula of each remaining cut occurs in an initial sequent of the proof belonging to $\mathfrak{S}$. So while the subformula property fails, it only fails for formulas already occurring in axioms belonging to $\mathfrak{S}$. Thus, the formulas that occur in the proof but not in the conclusion don't come out of nowhere, but rather come from $\mathfrak{S}$.

This might be thought to salvage something like (3a). True, there is no guarantee that J-pure proofs can always be found for theorems of $LK_{\mathfrak{S}}$. But free-cut free proofs can always be found for theorems of $LK_{\mathfrak{S}}$. For free-cut free proofs that are J-impure, the J-irrelevant formulas are subformulas of the axioms. This suggests the following modification of J-relevance. We say that a formula D is *JF-relevant* to the conclusion C of a proof P in an extension $LK_{\mathfrak{S}}$ of LK by a set of axioms $\mathfrak{S}$ if and only if either D is a subformula of C or of an axiom belonging to $\mathfrak{S}$. We say that a proof P of a conclusion C in a system $LK_{\mathfrak{S}}$ is *JF-pure* if and only if every formula occurring in P is JF-relevant to C.

Using this relevance measure, we obtain the following modification of (3a):

(3b) Every statement provable in an extension $LK_{\mathfrak{S}}$ of LK has a JF-pure proof, as a consequence of the free-cut elimination theorem.

I have already argued that (3b) is true. I now want to argue that (3b) is an unacceptable substitute for (3a). The problem is that JF-relevance is a poor relevance measure. In Section 3, I gave some conditions for being an acceptable relevance measure. One of these was that everything a good relevance measure measures as relevant would be judged relevant according to the standards of ordinary mathematical practice. This is what was appealing about J-relevance: if A is a subformula of B, then A is clearly relevant to B. JF-relevance does not have this feature, though. As we discussed in Section 2, there are

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[35] To be precise, we should say that at least one occurrence of the cut formula in each remaining cut is a direct descendent of a formula occurring in an initial sequent of the proof belonging to $\mathfrak{S}$. I've suppressed the "at least one occurrence" part here because our interest is in seeing how badly the subformula property fails in free-cut free proofs. So we don't need to distinguish between occurrences of the cut formula in each cut.

proofs, like Furstenberg's topological proof of the infinitude of primes, whose axioms are widely agreed to be irrelevant to the conclusion—indeed, that is ordinarily the appeal of those proofs. Yet, merely by being axioms used in the proof, they would count as JF-relevant to the conclusion. Since JF-relevance is a poor relevance measure, I conclude that (3b) is a poor substitute for (3a).

In response, one might suggest that there is a difference between a genuine set of axioms for a given theorem, and an arbitrary set of premises for it. A genuine set of axioms for a theorem is one like the axioms of Euclidean geometry for theorems in elementary geometry, or Peano arithmetic for theorems in elementary arithmetic; as Frege put it, a genuine set of axioms is not "a simple list of characteristics; [instead] every element is intimately, I might almost say organically, connected with the others".[36] So, the response continues, as long as a set of axioms $\mathfrak{S}$ is a genuine, organically unified set of axioms for a given theorem, rather than just an arbitrary set of assumptions, JF-relevance is a good relevance measure. The axioms belonging to such sets $\mathfrak{S}$ ought to be measured as relevant to the theorems they are used to prove. It is a striking and welcome consequence of the free-cut elimination theorem that JF-pure proofs are always available for $LK_{\mathfrak{S}}$-provable theorems for these genuine sets of axioms $\mathfrak{S}$.

This response begs the question, however, because the problem of what makes a set of axioms a genuine organic unity is the same as the problem of relevance. It may be that the topological assumptions used in Furstenberg's proof of the infinitude of primes are arbitrary while the axioms of Peano arithmetic are an appropriately organic and genuine set of axioms for proving the infinitude of primes. But whatever reason there is for thinking this will be a reason for thinking that PA is relevant to the infinitude of primes and for thinking topology irrelevant. This problem is what moving to a formal measure of relevance was meant to avoid. Thus, this response is unhelpful for salvaging JF-relevance as a good syntactic relevance measure.

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[36] Cf. Frege [1980] §88, p. 100.

Another argument that JF-relevance is a poor relevance measure, and that (3b) is an uninteresting proposal, is the following. Suppose q is a JF-impure proof of a statement C from axioms $\mathfrak{S}$, with α_1 the first formula in q that isn't JF-relevant to C. That is, α_1 isn't a subformula of C or of any axiom in $\mathfrak{S}$. Now we can trivially modify $\mathfrak{S}$ by adding a tautology to $\mathfrak{S}$ involving α_1, such as $\alpha_1 \to \alpha_1$, for instance. Call this trivial modified axiom set $\mathfrak{S}'$. Now q is a proof of C from $\mathfrak{S}'$, but now α_1 is a subformula of an axiom belonging to $\mathfrak{S}'$. By this trivial move, we have made α_1 JF-relevant to C. Since we can do this for all of the finitely many formulas in q, this shows that we can trivially modify *any* proof into a JF-pure proof, just by adding tautologies to the axioms that proof uses. The free-cut elimination theorem is unnecessary. For these reasons, (3b) is an uninteresting proposal.

4.2. **Adding axioms as inference rules.** The first new strategy I want to consider for adding axioms to sequent calculi while preserving a subformula property, and hence salvaging a version of (3^*), is due to Sara Negri and Jan von Plato.[37] They work over the sequent calculus *G3c* rather than *LK*; since *LK* and *G3c* are equivalent (i.e. they prove the same sequents), we can follow Negri and von Plato in working over *G3c* in this section without disrupting the continuity with the rest of this paper.[38] Their strategy is to add axioms not as initial sequents of proofs, but rather as new inference rules, in such a way that cut-elimination is more or less preserved.

I'll clarify this proposal first by an example. Suppose we wanted to add axioms for $\neg Q$ and $Q \vee R$ to *G3c*, where Q and R are atomic formulas. For $\neg Q$, Negri and von Plato propose the rule

$$\frac{}{Q \Rightarrow \Delta}$$

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[37] Cf. Negri and von Plato [1998], and Chapter 6 of Negri and von Plato [2001], which is an improved version of the earlier paper.

[38] *G3c* differs from *LK* in having no structural rules; instead, the operational rules are formulated so that whatever can be derived by structural rules in *LK* can be derived without them in *G3c*. In Troelstra and Schwichtenberg [2000], Troelstra and Schwichtenberg prove the equivalence of *LK* and *G3c* in two steps. Firstly, they reformulate *LK* without rules for negation but with rules for $\perp$, and call the resulting system *G1c* (pp. 61–62), and then prove the equivalence of *LK* and *G1c* (p. 87). Secondly, they formulate *Gc*3 (p. 77) and prove the equivalence of *G1c* and *G3c* (p. 82).

where Δ is an arbitrary cedent. Let's call this rule 'Neg'. The reason for picking Neg is that we want a rule expressing that Q is false. Since Δ is an arbitrary cedent, $Q \Rightarrow \Delta$ means that Q implies anything whatsoever, including $\neg Q$, and hence Q is (in our classical setting) necessarily false. By Neg, $Q \Rightarrow \Delta$ may always be inferred (hence the empty upper sequent), expressing the falsity of Q.

For $Q \vee R$, Negri and von Plato propose the rule

$$\frac{Q \Rightarrow \Delta \qquad R \Rightarrow \Delta}{\Rightarrow \Delta}$$

again with Δ an arbitrary cedent. Let's call this rule 'Dis'. Dis expresses that if both Q and R by themselves imply Δ, then Δ holds.

We can derive $\Rightarrow R$ in $G3c$ + Neg + Dis as follows:

$$\dfrac{\dfrac{}{Q \Rightarrow R}\,\text{(Neg)} \qquad R \Rightarrow R}{\Rightarrow R}\,\text{(Dis)}$$

Thus, by adding these two rules, we can derive $\Rightarrow R$ from $\neg Q$ and $Q \vee R$ without taking $\neg Q$ and $Q \vee R$ as initial sequents. Note that the proof we have given is cut-free.[39]

Negri and von Plato give a general scheme for converting axioms into inference rules. Firstly, they present the following general schema:

$$\frac{R_1, \Gamma \Rightarrow \Delta \qquad \ldots \qquad R_n, \Gamma \Rightarrow \Delta}{Q_1, \ldots, Q_m, \Gamma \Rightarrow \Delta}$$

where $Q_1, \ldots, Q_m, R_1, \ldots, R_n$ are atomic formulas, and Γ, Δ are arbitrary cedents. Secondly, they prove that every (classical) *quantifier-free* theory can be converted to an extension $G3c^*$ of $G3c$ where the theory's axioms have been replaced with inference rules following this general schema.[40] Hence, Negri and von Plato say, "All classical systems permitting quantifier-elimination...can be converted into systems of cut-free nonlogical rules of inference."[41] Using their general schema, they obtain inference rule extensions of

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[39] Negri and von Plato give these inference rule formulations of the axioms $\neg Q$ and $Q \vee R$ on p. 127 of Negri and von Plato [2001].

[40] This is Proposition 6.1.6, Negri and von Plato [2001], p. 129. The notation $Gc3^*$ denotes any extension of $G3c$ by rules following this general schema and appropriately closed under substitution (cf. Negri and von Plato [2001], pp. 130–1).

[41] Cf. Negri and von Plato [2001], p. 141.

G3c for the first-order predicate calculus with equality, partial orders, and plane affine geometry, among others.

Negri and von Plato then prove a cut-elimination theorem for extensions $G3c^*$[42], and then obtain a version of the subformula property for $G3c^*$. They show that all the formulas occurring in proofs in $Gc3^*$ are either subformulas of the conclusion, or atomic formulas.[43] The reason that they cannot obtain a full subformula property is that in non-logical rules, formulas in the upper sequent can disappear in the lower sequent.[44] Since all the formulas occurring in the non-logical rules considered by Negri and von Plato are atomic, the subformula property they have found for $Gc3^*$ must incorporate atomic formulas in addition to subformulas of the conclusion.

With the failure of the subformula property for $G3c^*$, not every theorem provable in $G3c^*$ necessarily has a J-pure proof. Thus, the inference-rule approach does not yield a version of (3^*) for $G3c^*$ / *LK*. However, the alternative subformula property Negri and von Plato propose suggests the following modification of J-relevance. We say that a formula D is *JIN-relevant* to the conclusion C of a proof P in an inference-rule extension $G3c^*$ of *G3c* if and only if either D is a subformula of C, or D is an atomic formula. We say that a proof P of a conclusion C in $G3c^*$ is *JIN-pure* if and only if every formula occurring in P is JIN-relevant to C.

Using this relevance measure, we obtain the following version of (3^*):

(3c) Every statement provable in an inference-rule extension $G3c^*$ of *G3c* has a JIN-pure proof, as a consequence of the Hauptsatz.

Negri and von Plato's alternative subformula property for $G3c^*$ implies that (3c) is true for quantifier-free $G3c^*$. I want to argue that (3c) is not a good reading of (3^*). Firstly, JIN-relevance counts as relevant to a statement C not just subformulas of C, but also all atomic formulas. But there is no reason to expect that every atomic formula is intuitively

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[42] This is Theorem 6.2.4, Negri and von Plato [2001], p. 134.

[43] This is Theorem 6.4.1, Negri and von Plato [2001], p. 136.

[44] For example, the formulas Q and R disappear in the lower sequent of the inference rule Dis considered above.

relevant to the conclusion. For instance, atomic formulas concerning topology are JIN-relevant to the statement asserting the infinitude of primes, but as discussed in Section 2, topology is uncontroversially irrelevant to the infinitude of primes. Secondly, Negri and von Plato's methods at present only apply to quantifier-free theories. But (3^*) did not involve any such limitation, but extended to *all* formally provable statements. Hence, I conclude that (3c) isn't a good reading of (3^*).

4.3. **Adding axioms as antecedents.** I want to consider a second new strategy for adding axioms while preserving a subformula property and hence a version of (3^*). Suppose there is a proof P of $\Gamma \Rightarrow \Delta$ in a system $LK_{\mathfrak{S}}$. P uses only finitely many axioms from $\mathfrak{S}$, enumerable as $S_1, S_2, \ldots, S_n$. A *conditionalization* of the sequent $\Gamma \Rightarrow \Delta$ is the sequent $S_1, \ldots, S_n, \Gamma \Rightarrow \Delta$ resulting from adding the axioms used in P to the antecedent of $\Gamma \Rightarrow \Delta$.

This illustrates the last strategy for adding axioms to LK that I want to consider. For each sequent $\Gamma \Rightarrow \Delta$ provable in $LK_{\mathfrak{S}}$, form a conditionalization $S_1, \ldots, S_n, \Gamma \Rightarrow \Delta$ of $\Gamma \Rightarrow \Delta$. It is easy to see that each conditionalization $S_1, \ldots, S_n, \Gamma \Rightarrow \Delta$ is provable in LK.[45]

Now consider the following reading of (3^*):

(3d) Every statement provable in an extension $LK_{\mathfrak{S}}$ of LK has a conditionalization with a J-pure proof, as a consequence of the Hauptsatz.

Since each conditionalization $S_1, \ldots, S_n, \Gamma \Rightarrow \Delta$ is provable in LK, and proofs in LK have the subformula property, each conditionalization has a J-pure proof.[46] Thus (3d) is true.

However, (3d) doesn't imply that the original statement $\Gamma \Rightarrow \Delta$ has a J-pure proof. In general, a conditionalization of a statement does not have the same content as the original statement. But (3^*) is a claim about $\Gamma \Rightarrow \Delta$ having a J-pure proof. Since (3d) does not obtain a J-pure of $\Gamma \Rightarrow \Delta$, but only of the contentually different conditionalization $S_1, \ldots, S_n, \Gamma \Rightarrow \Delta$, (3d), though true, fails to be a good reading of (3^*).

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[45] This is an instance of what is ordinarily called the 'deduction theorem'. Cf. Troelstra and Schwichtenberg [2000], p. 106.

[46] This method of adding axioms to *LK* is discussed in Negri and von Plato [1998], p. 419.

In response, one could observe that there are cases when statements and their conditionalizations arguably have the same content. These are cases when the statements $S_1, \dots S_n$ added to the antecedent of the original statement C are definitions of the terms used in C. More generally, these are cases where the statements added to the antecedent must be understood or accepted to understand C.

If right, this means that J-pure proofs are always available for such statements. This is a vindication of (3^*), showing that syntactically pure proofs are always available. The problem is that this only holds for a semantically-restricted domain of statements. Since (3^*) was meant as a claim about syntactic measures of relevance, and to avoid semantic matters, this response is a failure.

5. CONCLUSIONS

I have just discussed the four ways of adding axioms to the first-order predicate calculus deemed most promising by proof theorists. Each yields a new understanding of formal provability and hence of (3^*). Each version of (3^*) has problems: (3a) is false; (3b) is a failed attempt to patch the problems of (3a); and (3c) and (3d) fail as good readings of (3^*). One of these approaches might be improved in future proof-theoretic work, and as a result the corresponding reading of (3^*) might then be more plausible. But at present, (3^*) is not tenable.

The original hope expressed in (3) was that syntactically pure proofs would always be available for every formally provable statement, as a result of the Hauptsatz. I've argued that this hope is unfounded, at least at present. The Hauptsatz only guarantees syntactic purity for logical truths, not for mathematical truths like Bertrand's postulate. While J-relevance is an appealing relevance measure for reasons documented earlier, (3^*) would have made it far more appealing. Nevertheless, J-relevance and J-purity are still interesting. J-purity provides a sharp formulation of purity, and furthermore there are results showing when it is and isn't possible.

While the syntactic strategy for showing the necessary existence of pure formal proofs fails, other syntactic strategies might be more successful. There may be other well-motivated formalized relevance measures for which pure proofs are more widely available than for J-relevance. We should continue to look for such measures, with the hope that the insights gleaned from such a search will shed light on the deeper problem of developing a semantic theory of relevance.

I want to close by discussing briefly what I take to be the chief problem with syntactic relevance measures. This is that they take as candidates for relevance only what is explicitly mentioned in the statement of the conclusion. But this isn't how relevance works in practice. For instance, when Euclid constructs an equilateral triangle in Proposition I.1 of the *Elements*, he uses circles, which aren't mentioned in the statement of the problem. Yet since antiquity the proofs of *Elements* I have been customarily thought to be pure. This is a fact of the practice that a full account of purity must be able to accommodate, refute, or explain away. But we wouldn't be able to address this fact were we restricted to measuring as relevant only what's explicitly mentioned in statements. For then Euclid's proof would of necessity be impure. I conclude that an account of relevance adequate to the data of mathematical practice must look beyond the crude types of relevance measures considered in this paper, and must instead take on the difficult questions of how mathematical statements get their meanings, and how these meanings are related to each other.

REFERENCES

Martin Aigner and Günter M. Ziegler. *Proofs from The Book*. Springer–Verlag, Berlin, third edition, 2004.

Salomon Bochner. On a theorem of Tannaka and Krein. *Annals of Mathematics (2)*, 43: 56–58, 1942.

Samuel R. Buss. An introduction to proof theory. In Samuel R. Buss, editor, *Handbook of proof theory*, volume 137 of *Stud. Logic Found. Math.*, pages 1–78. North-Holland, Amsterdam, 1998.

Daniel Cass and Gerald Wildenberg. A novel proof of the infinitude of primes, revisited. *Mathematics Magazine*, 76(3):203, June 2003.

H. S. M. Coxeter. *Introduction to Geometry*. Wiley, second edition, 1989.

Herbert B. Enderton. *A mathematical introduction to logic*. Harcourt/Academic Press, Burlington, MA, second edition, 2001.

Paul Erdős. Problem 4065. *American Mathematical Monthly*, 50(1):65, January 1943.

Paul Erdős. Solution to Problem 4065. *American Mathematical Monthly*, 51(3):169–171, March 1944.

Gottlob Frege. *The Foundations of Arithmetic*. Northwestern Univ. Press, 1980. Translated by J.L. Austin.

Harry Furstenberg. On the infinitude of primes. *American Mathematical Monthly*, 62(5): 353, May 1955.

Gerhard Gentzen. Investigations into logical deduction. In M. E. Szabo, editor, *The collected papers of Gerhard Gentzen*, pages 68–131. North-Holland Publishing Co., Amsterdam, 1935.

Jean-Yves Girard. *Proof Theory and Logical Complexity*, volume 1. Elsevier, 1990.

Felix Hausdorff. *Set Theory*. Translated from the German third edition by John R. Aumann et al. Chelsea, New York, second edition, 1962.

Sara Negri and Jan von Plato. Cut elimination in the presence of axioms. *Bulletin of Symbolic Logic*, 4(4):418–435, December 1998.

Sara Negri and Jan von Plato. *Structural proof theory*. Cambridge University Press, Cambridge, 2001.

John Alan Robinson. Proof = Guarantee + Explanation. In Steffen Hölldobler, editor, *Intellectics and Computational Logic*, volume 19 of *Applied Logic Series*, pages 277–294. Kluwer, 2000.

J. J. Sylvester. Mathematical Question 11851. *Educational Times*, 59:98, 1893.

Gaisi Takeuti. *Proof theory*, volume 81 of *Studies in Logic and the Foundations of Mathematics*. North-Holland Publishing Co., Amsterdam, second edition, 1987.

A. S. Troelstra and H. Schwichtenberg. *Basic proof theory*. Cambridge University Press, Cambridge, second edition, 2000.