Mathematical Concepts and Definitions[1]

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> These are some of the rules of classification and definition. But although nothing is more important in science than classifying and defining well, we need say no more about it here, because it depends much more on our knowledge of the subject matter being discussed than on the rules of logic. (Arnauld and Nicole (1683/1996) p.128)

I Definition and Mathematical Practice

The basic observation structuring this survey is that mathematicians often set finding the "right" / "proper" / "correct" / "natural" definition as a research objective, and success - finding "the proper" definition - can be counted as a significant advance in knowledge. Remarks like these from a retrospective on twentieth century algebraic geometry are common:

> ...the thesis presented here [is that] the progress of algebraic geometry is reflected as much in its definitions as in its theorems Harris (1992 p.99)

Similarly, from a popular advanced undergraduate textbook:

> Stokes' theorem shares three important attributes with many fully evolved major theorems:
> a) It is trivial
> b) It is trivial because the terms appearing in it have been properly defined.
> c) It has significant consequences (Spivak (1965)(p.104))

Harris is speaking of the stipulative introduction of a new expression. Spivak's words are most naturally interpreted as speaking of an improved definition of an established expression. I will address both stipulative introduction and later refinement here, as similar issues matter to both.

What interest should epistemology take in the role of definition in mathematics? Taking the question broadly, of course, since the discovery of a proper definition is rightly regarded in practice as a significant contribution to mathematical knowledge, our epistemology should incorporate and address this fact,

[1] I am indebted to many people. Thanks to Paolo Mancosu both for comments on an unwieldy first draft and for bringing together the volume. Colin McClarty and Ian Proops gave detailed and illuminating comments. Colin also alerted me to a classic treatment by Emmy Noether ((1921) p.25 - 29) of the multiple meanings of "prime". An exciting conversation with Steven Menn about quadratic reciprocity set me on a path that led to some of the core examples in this paper. Early versions of this material were presented at Wayne State University and Berkeley. I'm grateful to both audiences, especially Eric Hiddleston, Susan Vineberg and Robert Bruner. Thanks to the members of my philosophy of mathematics seminar for discussing this material, especially Lina Jansson and Michael Docherty for conversation about the "bad company" objection. As usual, I would have been lost without friendly and patient guidance of the U. of M. mathematicians, especially (on this topic) Jim Milne (and his class notes on class field theory, algebraic number theory and Galois theory, posted at www.jmilne.org/math/) and Brian Conrad.

since epistemology is the (ahem) *theory* of (ahem) *knowledge*. A perfectly good question and answer, I think, but to persuade a general philosophical audience of the importance and interest of mathematical definitions it will be more effective, and an instructive intellectual exercise, to take "epistemology" and "metaphysics" to be fixed by presently salient issues: what connection can research on mathematical definition have to current debates?

II Mathematical Definition and Natural Properties

Both stipulative definitions of new expressions and redefinitions of established ones are sometimes described as "natural". This way of talking suggests a connection to metaphysical debates on distinctions between natural and artificial properties or kinds. Questions relevant to "naturalness" of mathematical functions and classifications overlap with the corresponding questions about properties generally in paradigm cases. We unreflectively distinguish "grue" from "green" on the grounds that one is artificial and the other isn't, and we distinguish "is divisible by 2" from "is π or a Riemann surface of genus 7 or the Stone-Čech compactification of ω" on the same ground. [2]

A particularly influential presentation of the issues appears in the writings of David Lewis.[3] It is useful here less for the positive account (on which Lewis is noncommittal) than for its catalogue of the work the natural/non-natural distinction does. Most entries on his list (underwriting the intuitive natural/non-natural distinction in clear cases, founding judgements of similarity and simplicity, supporting assignments of content, singling out "intended" interpretations in cases of underdeterminacy...) are not different for mathematical properties and others.[4] In at least one case (the "Kripkenstein" problem) naturalness will not help unless some mathematical functions are counted as natural. In another - the distinction between laws of nature and accidentally true generalizations - it is hard to imagine how an account of natural properties could help unless at least some mathematical properties, functions, and relations are included. The criteria in practice for lawlikeness and simplicity of laws often pertain to mathematical form: can the relation be formulated as a partial differential equation? Is it first or second order? Is it linear? The role of natural properties in inductive reasoning may mark a disanalogy, but as I indicate below this is not so clear. Of course, the use of "natural" properties to support analyses of causal relations is one point at which mathematics seems out of place, though again as we'll see the issue is complicated. In short, an account of the natural/nonnatural distinction is incomplete without a treatment of mathematical properties.

[2] For those unfamiliar with the philosophical background, "grue" is an intentionally artificial predicate coined by Nelson Goodman. "x is grue if x is observed before t and found to be green or x is observed after t and found to be blue." See the collection Stalker (1994) for discussion and an extensive annotated bibliography.

[3] See for example ((1986) esp p.59 - 69). A helpful critical overview of Lewis' articles on natural properties is Taylor (1993); Taylor proposes what he calls a "vegetarian" conception based on principles of classification rather than objective properties.

[4] Sometimes less grand distinctions than "natural-nonnatural" are at issue. In Lewis' treatment of intrinsic properties the only work the natural - nonnatural distinction seems to do is secure a distinction between disjunctive and non-disjunctive properties. Someone might regard the latter distinction as viable while rejecting the former. If so, the Legendre symbol example of section III illustrates the intricacy of even the more modest distinction.

Obviously the prospects of a smooth fit between the background account of mathematical naturalness and the treatment of physical properties will depend on the broader metaphysical picture. If it takes as basic the shape of our classifying activity, as in Taylor (1993), or considerations of reflective equilibrium, as in Elgin (1999) there is no reason to expect a deep disanalogy. Accounts positing objective, causally active universals could present greater challenges to any effort to harmonize the mathematical and non-mathematical cases. However, though the issues are complicated, they principally boil down to two questions: first, what difference, if any, devolves from the fact that properties in the physical world interact through contingent causal relations and mathematical properties don't? Second: to what extent is it plausible to set aside the distinctions between natural and non-natural that arise in mathematical practice as somehow "merely pragmatic" questions of "mathematical convenience"?[5] Here too we can't evaluate the importance of mathematical practice for the metaphysical questions unless we get a better sense of just what theoretical choices are involved. To make progress we need illustrations with enough meat on them to make clear how rich and intricate judgements of naturalness can be in practice. The next two sections sketch two examples.

III Fruitfulness and Stipulative Definition: The Legendre Symbol

Spivak's remark suggests that one of the criteria identifying "properly defined" terms is that they are fruitful, in that they support "trivial" results with "significant consequences". It is an important part of the picture that the consequences are "significant". (Any theorem will have infinitely many consequences, from trivial inferences like $A \vdash A \& A$.) So what makes a consequence "significant"? I won't consider everything here, but one will be especially relevant in the sequel: a consequence is held in practice to be significant if it contributes to addressing salient "why?" questions. Evaluations of the explanatoriness of arguments (theories, principles, etc.) and evaluations of the fruitfulness of definitions (theories, principles, etc.) interact in ways that make them hard to surgically separate. I'm not suggesting that considerations about explanation *exhaust* the considerations relevant to assessing whether or not a consequence is significant or a concept fruitful because it has significant consequences. I'm just making the mild observation that explanation is easier to nail down and better explored than other contributors to assessments of significance, so it is helpful as a benchmark. As a contrast, it is also common for proofs and principles to be preferred because they are viewed as more natural.[6] However, the relevant idea

[5] This is a pivotal argumentative support in Sider (1986), to cite just one example. Discussion of the arguments would go beyond this survey, so I'll leave it for other work.

[6] For example, many number theorists count the cyclotomic proof as particularly natural. (Frölich and Taylor (1991 p.204) opine that this proof is "most natural" (or rather: " "most natural" ").) Similarly, in the expository account of Artin's life by Lenstra and Stevenhagen (2000) we read: "Artin's reciprocity law over $\mathbb{Q}$ generalizes the quadratic reciprocity law and it may be thought that its mysteries lie deeper. Quite the opposite is true: the added generality is the first step on the way to a natural proof. It depends on the study of cyclotomic extensions." (p. 48)). Gauss, on the other hand, though one of his proofs exploits cyclotomy, preferred a more direct argument using what is now called "Gauss' lemma". Of other proofs he wrote: "Although these proofs leave nothing to be desired as regards rigor, they are derived from sources much too remote...I do not hesitate to say that until now a *natural* proof has not been produced." (Gauss (1808)). Gauss may have revised his opinion were he to have seen subsequent research, given his often expressed view of the "fruitfulness" of the study of

of "natural proof" is uncharted and poorly understood; it would hardly clarify "mathematically natural property" to analyze it in terms of "mathematically natural proof". On the other hand, though the study of mathematical explanation is still in early adolescence, we have learned enough about it to use it for orientation.

An illustration of the quest for explanation in mathematics is the often reproved theorem of quadratic reciprocity:[7] If p and q are odd primes, then $x^2 \equiv p \pmod{q}$ is solveable exactly when $x^2 \equiv q \pmod{p}$ is, except when $p \equiv q \equiv 3 \pmod{4}$.[8] In that case, $x^2 \equiv p \pmod{q}$ is solveable exactly when $x^2 \equiv q \pmod{p}$ *isn't*. Gauss famously found eight proofs and many more have been devised[9]. One reason it attracts attention is that it cries out for explanation, even with several proofs already known. As Harold Edwards puts it:

> The reason that the law of quadratic reciprocity has held such fascination for so many great mathematicians should be apparent. On the face of it there is absolutely no relation between the questions "is p a square mod λ?" and "is λ a square mod p?" yet here is a theorem which shows that they are practically the same question. Surely the most fascinating theorems in mathematics are those in which the premises bear the least obvious relation to the conclusions, and the law of quadratic reciprocity is an example *par excellence*. . . . [Many] great mathematicians have taken up the challenge presented by this theorem to find a natural proof or to find a more comprehensive "reciprocity" phenomenon of which this theorem is a special case.(Edwards (1977), p.177)

A similar expression of amazement, and a demand for explanation and understanding appears in a review of a book on reciprocity laws:

> We typically learn (and teach!) the law of quadratic reciprocity in courses on Elementary Number Theory. In that context, it seems like something of a miracle. Why should the question of whether p is a square modulo q have any relation to the question of whether q is a square modulo p? After all, the modulo p world and the modulo q world seem completely independent of each other . . . The proofs in the elementary textbooks don't help much. They prove the theorem all right, but they do not really tell us why the theorem is true. So it all seems rather mysterious... and we are left with a feeling that we are missing something. What we are missing is what Franz

cyclotomic extensions. On Gauss on cyclotomy and reciprocity, see Weil (1974).

[7]The basic facts are available in many textbooks. A particularly appealing, historically minded one is Goldman (1998). Cox (1989) is an engagingly written, historically minded essay on a range of topics in the area. Chapter 1 is a clear presentation of the basic number theory and history accessible to anyone with one or two university math courses. The presuppositions jump up significantly for chapter 2 (covering class field theory and Artin reciprocity). Jeremy Avigad is doing penetrating work exploring philosophical ramifications of algebraic number theory. See Avigad (2006) and elsewhere.

[8]$a \equiv b \pmod{c}$ means $(\exists n)\, a = nc + b$, or as we put it in school arithmetic "a divided by c has remainder b". When $(\exists x)\, x^2 \equiv p \pmod{q}$ we say p is a *quadratic residue* mod q.

[9]221 proofs using significantly different techniques are listed at http://www.rzuser.uni-heidelberg.de/ hb3/fchrono.html. A hardcopy is in Lemmermeyer ((2000) p. 413 - 417).

> Lemmermeyer's book is about. . . . he makes the point that even the quadratic reciprocity law should be understood in terms of algebraic number theory, and from then on he leads us on a wild ride through some very deep mathematics indeed as he surveys the attempts to understand and to extend the reciprocity law.[10]

The search for more proofs aims at more than just explaining a striking curiosity. Gauss regarded what he called "the fundamental theorem" as exemplifying the fruitfulness of seeking "natural" proofs for known theorems.[11] His instinct was astonishingly accurate. The pursuit of general reciprocity proved to be among the richest veins mined in the last two centuries. Nearly one hundred years after Gauss perceived the richness of quadratic reciprocity, Hilbert ratified the judgement by setting the "proof of the most general law of reciprocity in any number field" as ninth on his list of central problems. The solution (the Artin reciprocity law) is viewed as a major landmark.[12]

Gauss recognized another key point: the quest for mathematically natural (or, indeed, *any*) proofs of higher reciprocity laws forces extensions of the original domain of numbers.[13] (Once quadratic reciprocity is recognized, it is natural to explore higher degree equations. Are there cubic reciprocity laws? Seventeen-th?) To crack biquadratic reciprocity, Gauss extended the integers to the Gaussian integers $\mathbb{Z}[i] = \{a + bi \,|\, a, b \,\epsilon\, \mathbb{Z}\}$. Definitions that are interchangeable in the original context can come apart in the expanded one. This can prompt an analysis that reshapes our view of the definitions in the original environment, when we use the extended context to tell us which of the original definitions was really doing the heavy lifting.[14] This pressure to understand

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[10] Review of Lemmermeyer (2000) by F. Gouvêa at: www.maa.org/ reviews/brief_jun00.html

[11] A typical expression of his attitude is:

> It is characteristic of higher arithmetic that many of its most beautiful theorems can be discovered by induction with the greatest of ease but have proofs that lie anywhere but near at hand and are often found only after many fruitless investigations with the aid of deep analysis and lucky combinations. This significant phenomenon arises from the wonderful concatenation of different teachings of this branch of mathematics, and from this it often happens that many theorems, whose proof for years was sought in vain, are later proved in many different ways. As a new result is discovered by induction, one must consider as the first requirement the finding of a proof by *any possible* means. But after such good fortune, one must not in higher arithmetic consider the investigation closed or view the search for other proofs as a superfluous luxury. For sometimes one does not at first come upon the most beautiful and simplest proof, and then it is just the insight into the wonderful concatenation of truth in higher arithmetic that is the chief attraction for study and often leads to the discovery of new truths. For these reasons the finding of new proofs for known truths is often at least as important as the discovery itself. ((1817) p. 159 - 60 Translation by May [1972] p.299 emphasis in original)

[12] See Tate (1976). As Tate notes, the richness of the facts incorporated in quadratic reciprocity has not run out even after two centuries of intense exploration. A 2002 Fields medal was awarded for work on the Langlands program, an even more ambitious generalization.

[13] See Gauss (1825); Weil (1974a p.105) observes that for Gauss, even conjecturing the right laws wasn't possible without extending the domain.

[14] For another example, the definition of "integer" requires serious thought. Say we begin with the normal $\mathbb{Z}$ (a ring) in $\mathbb{Q}$ (a field). What ring is going to count as "the integers" if we extend the field to $\mathbb{Q}[\alpha]$? (That is: when we toss in α and close under $+$, x and inverses.) The

the original phenomena generated "class field theory": the study of (Abelian) extension fields. A central 20th century figure put it this way:

> ...by its form, [quadratic reciprocity] belongs to the theory of rational numbers...however its contents point beyond the domain of rational numbers...Gauss...recognized that Legendre's reciprocity law represents a special case of a more general and much more encompassing law. For this reason he and many other mathematicians have looked again and again for new proofs whose essential ideas carry over to other number domains in the hope of coming closer to a general law... The development of algebraic number theory has now actually shown that the content of the quadratic reciprocity law only becomes understandable if one passes to general algebraic numbers and that a proof appropriate to the nature of the problem can best be carried out with these higher methods.[15] (Hecke (1981) p.52-53)

This provides the background for our central example of a mathematically natural stipulative definition: the Legendre symbol. The statement of the law of quadratic reciprocity given above was broken into cases, and it's good intellectual hygiene to find a uniform statement. For this purpose we define the Legendre symbol $\left(\frac{n}{p}\right)$ (p an odd prime):

$$\left(\frac{n}{p}\right) =_{def} \begin{cases} 1, & \text{if } x^2 \equiv n \pmod p \text{ has a solution and } n \not\equiv 0 \pmod p \\ -1, & \text{if } x^2 \equiv n \pmod p \text{ has no solution and } n \not\equiv 0 \pmod p \\ 0 & \text{if } \; n \equiv 0 \pmod p \end{cases}$$

The Legendre symbol supports a single-case statement of quadratic reciprocity:

For odd prime p, q: $\left(\frac{p}{q}\right)\left(\frac{q}{p}\right) = (-1)^{\frac{(p-1)(q-1)}{4}}$

The Legendre symbol doesn't just give this one-off compression of a statement. It also streamlines proofs, as in Dirichlet's (1858) reformulation of Gauss' first proof of quadratic reciprocity. Proof 1 used nothing fancier than mathematical induction. Dirichlet pointed out that economy of machinery was traded for fragmentation: Gauss proves eight separate cases. Using (a minor generalization of) the Legendre symbol Dirichlet trims the cases to two.

The value of this kind of unification has been discussed in the philosophical literature on explanation.[16] On first pass, it might seem that this is a textbook

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obvious answer is $\mathbb{Z}[\alpha]$; that's what Gauss and Kummer used as "the integers", and in the specific cases they addressed it happened to do the job. But in general this won't work, and it becomes a genuine problem to identify the natural ring of integers for a general algebraic number field. The question was analyzed and answered in what remains the accepted way by Dedekind in 1871. The basic details and core references are in Goldman [1998] (p.250 - 252).

[15] Jim Milne has drawn my attention to a remark of Chevalley, echoing Hecke's point about "higher methods" with a puckish parody of Hegel: "The object of class field theory is to show how the Abelian extensions of an algebraic number field K can be determined by objects drawn from our knowledge of K itself; or, if one wants to present things in dialectical terms, how a field contains within itself the elements of its own transcending (and this without any internal contradictions!)." Chevalley (1940) p.394 my translation

[16] For more, see the chapters by Paolo Mancosu in this volume and Tappenden (2005).

case of a definition effecting a valuable unification: The new statement of the theorem is brief and seemingly uniform, and a proof the definition supports reduces many cases to just a couple. However, a recognized problem is that only some unifications are genuine advances. When the unification is effected by appeal to predicates that are "gerrymandered" the unification may be unilluminating and spurious.[17] We gain nothing if we produce a "unified theory" of milkshakes and Saturn by rigging concepts like "x is a milkshake or Saturn".

So: Does the Legendre symbol reflect a natural mathematical function or an artifice? To appreciate the force of the question, note that one's first impression is likely to be that the definition is an *obvious* gerrymander. We apparently artificially simplify the statement of the theorem by packing all the intricacy and case distinctions into the multi-case Legendre symbol definition. The question is especially pressing because discussions of the metaphysics of properties often view "disjunctive" predicates as *prima facie* artificial, or unnatural, or "gruesome", or "Cambridge", or [insert favorite label for 'metaphysically fishy'].[18] In short, our problem is to find principled reasons to go beyond this impasse:

Thesis: The Legendre symbol is a useful stipulation that contributes to mathematical knowledge. It allows for one-line statements of theorems that had required several clauses, and it supports streamlined proofs by unifying a variety of cases into two. This supports the verdict that it is mathematically natural.

Antithesis: The symbol is paradigmatic as a definition that is valuable only for limited reasons pertaining to accidental facts of human psychology. That it is a hack, not a conceptual advance is displayed right in the syntax of its definition.

The foothold allowing us to progress is that the function corresponding to the Legendre symbol is itself the object of mathematical investigation.[19] It is a mathematical question whether the Legendre symbol carves mathematical reality at the joints and the verdict is unequivocally yes. There is so much mathematical detail that I don't have nearly enough space to recount it all. To convey the point, it will have to suffice here to gesture at some of the mathematics, with the assurance that there is much more where this came from and a few references to get curious readers started learning more for themselves.

The Legendre symbol (restricted to p relatively prime to the argument on top) is a function from numbers to $\{1, -1\}$. For fixed p, the function is completely multiplicative (for any m and n, $\left(\frac{mn}{p}\right) = \left(\frac{m}{p}\right)\left(\frac{n}{p}\right)$). It is periodic mod p ($\left(\frac{m}{p}\right) = \left(\frac{m+p}{p}\right)$). Under multiplication, $\{1, -1\}$ and $\{1, 2, , \ldots, p-1\}$

[17] Kitcher (1989) recognizes this as a limitation on his account of explanation as unification and he accordingly considers only patterns based on projectible predicates. I suggest in Tappenden (2005) that this limits the account to applications where the concept of "projectibility" makes sense, and mathematics seems to be a place where it doesn't. But perhaps mathematical practices of conjecture and verification afford more of a basis for a distinction like that between inductively projectible and inductively nonprojectible predicates than I allowed for.

[18] On "disjunctiveness" of properties see Sanford (1970), (1981) and (1994) and Kim (1992).

[19] Some years ago, in a early effort to articulate some of these ideas, I called this the "self-consciousness" of mathematical investigation. (See Tappenden (1995) and (1995a))

(aka $(\mathbb{Z}/p\mathbb{Z})^*$)) are groups, so the function restricted to $(\mathbb{Z}/p\mathbb{Z})^*$) is a surjective homomorphism. These are all mathematically good things, in that they are experience-tested indicators that a function is well-behaved. Another is that multiplication on $\{1, -1\}$ is commutative. (That is: the group is *Abelian*) This is usually handy; a century of research revealed it be especially pivotal here.[20] Many more such indicators could be listed, but this list says enough to clarify some of the simpler criteria for "mathematical naturalness" at issue.

What of higher-powered considerations? It would require too much technical background to do more than glance, but some of the flavor of the mathematics is needed to convey what reasons can be given. To crack Hilbert's ninth problem we need to properly generalize in many directions, and to do this we need to reformulate the question. (In contrast to the picture one might get from meditating too long on first-order logic, generalizing is much more than just picking constants and replacing them with variables. Articulating the right structures, which then can be generalized, is an incredibly involved process and it is hard to get it right.)[21] The issue in quadratic reciprocity for primes p and q can be rethought as the circumstances under which we can split (i.e. factor into linear factors) an equation in a field extending $\mathbb{Q}$. This can in turn be seen as a question about the relations between $\mathbb{Q}$ and the field $\mathbb{Q}[\sqrt{q*}]$ ($q* = q$ or $-q$ depending on q). Field extensions K/F are sorted by the smallest degree of a polynomial from F that splits in K. In the case of $\mathbb{Q}[\sqrt{q*}]/\mathbb{Q}$ the degree is 2. The basic fact of Galois theory is: If K is the splitting field of a polynomial $\theta(x)$ over a field F, we can associate with K/F a group Gal(K/F) (the *Galois group*) encoding key information about K/F and $\theta(x)$.

These statements have been fruitfully generalized in many ways: we can consider not just degree 2 polynomials but other degrees and their Galois groups. We can consider not just prime numbers but more general structures sharing the basic property of primes. Considering other fields besides $\mathbb{Q}$ induces the need to generalize "integers (of the field)". The Galois group is a group of functions, and we can define other useful functions in terms of those functions . . ., and more. After this and more reformulating and generalizing, lasting nearly 200 years (if the clock starts with Euler) we arrive at the Artin reciprocity law. It has quadratic reciprocity, cubic reciprocity,. . ., seventeen-ic, reciprocity . . . as special cases. The core is a function called the Artin symbol fixed by several parameters (Base and extension field with induced general integers, given generalized prime,. . .). The punch line for us is that when you plug in the values for quadratic reciprocity, (fields: $\mathbb{Q}$ and $\mathbb{Q}[\sqrt{q*}]$, generalized integers: the ordinary $\mathbb{Z}$, generalized prime: $p \in \mathbb{Z}$, . . .) the Legendre symbol appears![22]

Now of course given any function in any theorem, we can always jigger up

[20] Class field theory and the Langlands programme differ precisely on whether the Galois group is Abelian. The fact that seems trivial for $\{\{1, -1\}, \mathrm{x}\}$ casts a long shadow.

[21] Wyman (1972) is a superb exposition of a few of the reformulations and re-reformulations needed to generalize quadratic reciprocity.

[22] Cox ((1989) p.97 - 108) describes how the Legendre symbol falls immediately out of the Artin symbol as a special case. Samuel (1970) (esp. p.92 - 93) conveys with beautiful economy the basic idea in a less general form with fewer mathematical prerequisites. (Think "Artin symbol" when Samuel refers to the "Frobenius automorphism".)

many more general versions with the given function and theorem as special cases. Most will be completely uninteresting. The generalizations we've been discussing are mathematically natural, as measured in many ways, some subtle and some obvious. The core fact is the one Gauss noted: Investigations of quadratic reciprocity and its generalizations reveal deep connections among (and support proofs in) an astonishing range of fields that initially seem to have little in common. (Not just elementary arithmetic, but circle-division, quadratic forms, general algebra of several kinds, complex analysis, elliptic functions ...) The upshot of the general investigation is a collection of general theories regarded by mathematicians (and hobbyists) of a range of different backgrounds and subspecialties as *explaining* the astonishing connection between arbitrary odd primes. The judgement that the Legendre symbol carves at a joint interacts with a delicate range of mathematical facts and judgements: verified conjectures, the practice of seeking explanations and understanding, efforts to resolve more general versions of known truths (and the evaluations of "proper" generalizations that support this practice), judgements of similarity and difference, judgements about what facts would be antecedently expected (quadratic reciprocity not among them), and more.

The history of quadratic reciprocity also illustrates the importance of inductive reasoning in mathematics. Euler conjectured the law decades before Gauss proved it, on the basis of enumerative induction from cases.[23] This issue will be revisited in the research article, so I'll just note the key point.[24] In many cases, the natural/artificial distinction is linked to projectibility: Natural properties ("green") support correct predictions and artificial ones ("grue") don't. It is common, as in the influential work of Sydney Shoemaker ((1980) and (1980a)), to connect this with a thesis about causality. Simplifying Shoemaker's picture drastically: natural properties are those that enter into causal relations, and it is because of this that only natural properties support induction properly. Euler and quadratic reciprocity reveal a limit to this analysis: induction, as a pattern of reasoning, does not depend for its correctness on physical causation. The properties supporting Euler's correct inductive reasoning have the same claim to naturalness deriving from projectibility that "green" has. This is consistent with the observation that mathematical properties don't participate in causation as Shoemaker understands it. Even though there is much more about inductive reasoning in mathematics that we need to better understand, and we should have due regard for the differences between mathematical and empirical judgements, we shouldn't underestimate the affinities.

Delicate issues of identity and difference of content also arise, as in Dedekind's proof of quadratic reciprocity in *Theory of Algebraic Integers*. Dedekind describes himself as presenting "essentially the same as the celebrated sixth proof of Gauss" (from Gauss (1817)). The derivation recasts the treatment of cyclotomic extensions in *Disq. Ari.* 356. Dedekind is plausibly described as presenting the same argument in a conceptual form avoiding most of the cal-

[23] Edwards (1983), Cox (1989 p. 9 - 20), and Weil (1984) are excellent treatments of the inductive reasoning that led Euler to his conjecture. There is a particularly beginner-friendly discussion at the online Euler archive http://www.maa.org/news/howeulerdidit.html.

[24] For more on plausible reasoning in mathematics see Jeremy Avigad's first article.

culations. The challenge for the analyst of reasoning is to find some way of characterizing what kind of success it was that Dedekind achieved.

Returning to the main line, we can draw a moral about the division of mathematical concepts into artificial and non-artificial. Whether or not a concept is "disjunctive" in a way that gives it a grue-like artificiality is not something we can just read off the syntax of the definition. This example (or rather, this example plus the rest of the mathematics whose surface I've only scratched) illustrates how inextricably judgements as to the naturalness and proper definition of basic concepts are embedded in mathematical method.

Let's remind ourselves of the orienting question: i) is the Legendre symbol artificial or natural and ii) how can we tell? The answer is i) natural, and ii) that is a mathematical question, to which a substantial answer can be given. The richness and depth of the mathematical rationale provides the basic answer to the suggestion that the mathematical naturalness of a property, function, or relation can be shrugged off as "pragmatic/mere mathematical convenience". It is hard to see how this assessment of one category as better than others is different than cognate assessments in the natural sciences or philosophy. This response won't work against someone who maintains that *all* such distinctions of natural and non-natural are "merely pragmatic" matters of "philosophical/physical/chemical/biological/etc. convenience" but that is a separate topic. Our point is that mathematics is on an equal footing.

IV Prime Numbers: Real and nominal definition revisited

We've observed that a common pattern in mathematics is the discovery of the proper definition of a word with an already established meaning. The Legendre symbol can be redefined in light of later knowledge, to reflect the best explanations of the facts. This may appear to be in tension with a long-standing philosophical presumption that the definition introducing an expression is somehow privileged as a matter of meaning. But the unfamiliarity of the Legendre symbol may make the point seem abstruse. It will be worthwhile to look at a comfortingly familiar example: "prime number". We learn the original definition in elementary school: $n \neq 1$ is prime if it is evenly divided by only 1 and n. Over $\mathbb{N}$ the familiar definition is equivalent to: $a \neq 1$ is prime if, whenever a divides a product bc (written $a \mid bc$) then $a \mid b$ or $a \mid c$. In extended contexts, the equivalence can break down. For example, in $\mathbb{Z}[\sqrt{5}i] = \{a + b\sqrt{5}i \mid a, b \,\epsilon\, \mathbb{Z}\}$, 2 is prime in the original sense, but not in the second, since $6 = (1 - \sqrt{5}i)(1 + \sqrt{5}i)$; 2 divides 6 but not $(1 - \sqrt{5}i)$ or $(1 + \sqrt{5}i)$.

Once the two options are recognized, we need to say which will be canonical. The choice will be context-sensitive, so I'll take algebraic number theory as background. The word "*prime*" is given the *second* definition.[25] ("*Irreducible*" is used for the first.) The reason for counting the second definition as the proper one is straightforward: The most significant facts about prime numbers turn out to depend on it. As with quadratic reciprocity, this is complicated, so I'll just observe that ongoing efforts to explain facts about the structure of natu-

[25] This point is addressed in many places; one is Stewart and Tall (2002) p. 73 - 76.

ral numbers justify choosing the novel definition of prime.[26] Say we describe the situation this way: investigations into the structure of numbers discovered what prime numbers really are. The familiar school definition only captures an accidental property; the essential property is: $a \mid bc \rightarrow (a \mid b \text{ or } a \mid c)$. These descriptions of the situation might seem philosophically off. First, it might be maintained that it is *obviously* wrong to say that in $\mathbb{Z}[\sqrt{5}i]$, 2 is only divisible by itself and 1, but isn't a prime number, since that is *analytically* false, given what "2 is a prime number" *means*. Such objections would be unnecessary distractions, so I'll reformulate the point in extensional terms. The original definition of prime number fixes a set $\{2, 3, 5, 7, ...\}$ with interesting properties. In the original domain $\mathbb{N}$ it can be picked out by either definition. The new definition is the more important, explanatory property, so it is the natural one.

The situation is the mathematical analogue of an example in Hilary Putnam's classic "The Analytic and the Synthetic". In the nineteenth century, one could hold "Kinetic energy is $\frac{1}{2}mv^2$" to be true by definition. With relativity, the new situation can be described in two ways. We could say "Kinetic energy is $\frac{1}{2}mv^2$" is not true by definition and also we have learned kinetic energy is not $\frac{1}{2}mv^2$, or that it *was* true by definition but Einstein changed the definition of "kinetic energy" for theoretical convenience. Neither of these options aptly captures what went on. Saying that we have just changed our mind about the properties of kinetic energy doesn't respect the definitional character of the equation, but saying we have embraced a more convenient definition while retaining the same words fails to do justice to the depth of the reasons for the change, and so fails to capture how the new definition was a genuine advance in knowledge. As with "kinetic energy", so with "prime": it is of less consequence whether we say that word meaning changed or opinions about objects changed. What matters is that the change was an advance in knowledge, and we need a philosophical niche with which to conceptualize such advances.

The idea that discovering the proper definition can be a significant advance in knowledge has overtones of a classical distinction between "real" and "nominal" definition. "Real definition" has fallen on hard times in recent decades. Enriques' *The Historic Development of Logic*, published in 1929, addresses the topic throughout, in a whiggish recounting of the emergence of the idea that "real definition" is empty and that all definitions are nominal. That seems to be where things stand now. We might need to rework too many entrenched presuppositions to revive the distinction in its traditional form, but it would help to reconstruct a minimal doctrine to support the distinctions we want to draw and connections we want to make. The core motivation is that in mathematics (and elsewhere) finding the proper principles of classification can be an advance in knowledge. We appear to have enough of a grip on the real/nominal distinction that it might be useful in this connection, and we don't have to accept

[26] There also also deep reasons for excluding 1. Widespread folklore sees the rationale to be a clean statement of the uniqueness of prime factorization; this makes excluding 1 appear to be a matter of minor convenience in the statement of theorems. As against this, an algebraic number theorist I know remarked in correspondence: "... with the advent of modern algebra and the recognition of the different concepts of unit and prime in a general commutative ring it became clear that units are not to be considered as prime." The statement of unique factorization, on the other hand, was seen as "not particularly compelling."

everything that comes bundled in the package. For instance, one concession to changing attitudes seems sensible. Since we are no longer sure what to make of defining a thing as opposed to a word let's stick with the contemporary view that definitions are stipulations of meaning for expressions.

One reason for carefully delineating what should be involved with a prospective concept of real definition is indicated by Richard Robinson in the classic *Definition*. The concept has been called on to play too many different roles: Robinson lists twelve importantly different ones. (1950 p.189-90) Robinson's recommendation is that talk of "real definition" be dropped, and that each of the components be treated separately. But this might be an overreaction, giving up the prospect of salvaging valuable insights from the tradition. The activities did not come to be associated by accident, and even if no single concept can do *everything*, we can identify a subset of roles that can usefully be welded together. From Robinson's list, these include: "Searching for a key that will explain a mass of facts", "improvement of concepts", plus (if these can be construed neutrally) "searching for essences" and "searching for causes".

Of course, to speak of "improvement of concepts" we'll need to specify a criterion for improvement. Putting together a useful, metaphysically uncontentious doctrine of real definition seems promising if we take the relevant improvement to partially involve finding "a key that will explain a mass of facts". That is, a concept is improved is if it is made to fit better into explanations (or into arguments that are good in some other way). The connection between singling out specific definitions as real and the practice of giving explanations is also indicated in the recent revival of "essence", if we understand "essence" in terms of real definition and role in argument. Since "essence" is seen as metaphysically charged we'll take "real definition" to be basic, and essences, to the extent we want to use that idea, will be what real definitions define.[27]

The idea that real definition fits with "cause" draws on the observation that, for Aristotle, some mathematical facts cause others. This is not how we use "cause" today, so it might be better to appeal to conceptual or metaphysical dependence. Once more this can be understood in terms of explanatory argument structure.[28] This need not make mathematical explanation fundamentally different from ordinary explanation. We could opt for an account of explanation and causation as sketched in Kim (1994), taking dependence as basic. (We'll need an account of dependence anyway, to spell out what it is for facts about primes to depend on the second definition, for example.) Causation, as one kind of dependence, supports explanation, but it is not the only kind that does.

[27]This is the approach of Fine (1994), though one would not call the view "metaphysically innocuous". Our search for a neutral account is not driven by any sense that a meaty view like Fine's isn't right, but only by the goal of avoiding extra debate while charting a framework to explore mathematical cases in parallel with cases in science and common life.

[28]More on this cluster of Aristotelean ideas can be found in Mic Detlefsen's contribution. The core idea, that there is a prospectively illuminating, broadly Aristotelean, way of understanding "essence" in terms of explanation has been noted for some time. Unpublished work by my colleague Boris Kment promises to be illuminating on this point. In talks, Fine (Eastern APA, December 2004) has sketched an account of essence as connected fundamentally to explanation; he tells me (correspondence May 2006) that though he has not yet published anything on the explanation-essence connection, he has explored it in unpublished work.

Our theme has been that theorizing has to be driven by the data, so we should look to see how the cases we've just considered appear in this light. Taking real definition and essence to be functions of explanation (and other, harder to pin down properties of argument) promises to give a plausible accounting of why the algebraic definition of "prime" counts as picking out the essential property of the class of primes in the context of algebraic number theory. Relative to an explanation of quadratic reciprocity via Artin reciprocity, the "real definition" of the Legendre symbol becomes its specification as a special case of the Artin symbol rather than the multicase initial definition. As with "prime number", the definition with the central role in the central arguments is not the one that introduced the symbol. Here too further investigation unearthed and justified something that it makes sense to call the "real definition".

V Prospective connections

a) "Fruitful concepts"

Frege, in various writings, addressed the issue of valuable concepts and definitions. Though he does not develop the idea systematically, there are enough tantalizing hints to spur efforts to reconstruct the doctrines behind them.[29] The importance of "fruitfulness" in theory choice has long been recognized. Kuhn (1977) lists it among the theoretical values in science, remarking that despite its importance the idea was little studied. This situation changed little in subsequent decades, though an essay by Daniel Nolan (1999) takes first steps toward analyzing what is virtuous about this theoretical virtue. A prerequisite for real progress is a more detailed analysis of cases that have unfolded in practice.

b) Burge on "Partial Grasp" of Senses; Peacocke on "Implicit Conceptions"

Euler, Gauss, and others had true thoughts about prime numbers before the concept was properly defined. It seems right to say that Dedekind's presentation of Gauss' third proof of quadratic reciprocity really does present "essentially" what is going on, while omitting the calculations that Gauss himself (for all we know) may have thought essential. Such cases are examples looking for a theory (specifically, a theory of partially grasped content and sameness of content). Burge (1990) has articulated a theory of meaning that builds upon the idea of "partial grasp". Following Burge, Peacocke (1998), (1998a) explores the relation of the nineteenth century $\delta - \epsilon$ definitions to the Newton/Leibniz presentations of calculus, plausibly suggesting that the later definitions are implicit in the earlier. As critics made clear, many complications remain to be sorted out.[30] In particular, Rey (1998) notes Newton and Leibniz may have had a conception explicitly in mind contradicting the $\delta - \epsilon$ treatment. On the other hand, the commitments of researchers are complex, and could involve methodology that, when consistently followed out, might undercut other aspects of the researchers' view of what they are doing. (Here one remembers the amusing episodes in the development of the axiom of choice, where opponents aggressively rejected the

[29] See Tappenden (1995)

[30] The volume containing Peacocke's papers has several more devoted to criticism.

axiom while finding themselves repeatedly applying it tacitly in proofs.) Peacocke has identified a deep problem that we have only begun to see clearly. Here too, philosophical analysis and study of rich cases from practice will have to go hand in hand.

c) Wright/Hale on the Caesar Problem

The insight inspiring neo-logicism is that arithmetic can be derived from an abstraction principle. This suggests that we revisit Frege's reservation: How can we conclude, from this definition, that Julius Caesar is not a number? One appealing strategy is to spell out and defend the naive reaction: Caesar isn't a number because he just isn't the *kind of thing* that *can* be. Pursuing this line, Hale and Wright (2001) argue that in sortal classification some categories are inherently disjoint. They concentrate their brief discussion on abstract reflections on individuation and criteria of identity; my impression is that most readers have been left wanting more. A good way to go beyond foot-stomping staredowns where each side just reaffirms favored intuitions is to assemble more information about a range of cases. The neo-logicist program may need clarification of the structure of natural classification in mathematics for a further reason: the so-called "bad company objection". Some abstraction principles have evidently false consequences, but might be excluded if a viable artificial/natural distinction ("Cambridge/"non-Cambridge" in Wright-Hale's parlance) could be articulated and defended. Not all the troublesome abstracts are plausibly ruled unnatural, so this is not a cure-all. But we shouldn't assume that all problematic abstracts are problematic for the same reason.

VI Summing Up: The *Port Royal* Principle

The examples we've seen are enough to conclude that mathematical defining is a more intricate activity, with deeper connections to explanation, fruitfulness of research, etc. than is sometimes realized. We need a philosophical framework to help us keep the books while we sort it out, and to guide us as to what consequences if any this can have for other areas of philosophy. The philosophical stance implicit in the above has affinities to the position Penelope Maddy has called "mathematical naturalism" ((1997) and elsewhere). Though I differ with Maddy on some details, we agree that rich analysis of mathematical practice is a *sine qua non* for judgements in the philosophy of mathematics. The lines that open this paper, from the *Port Royal Logic*'s treatment of real definition, prompt the name "*Port Royal* principle" for the kind of "naturalism" at issue: "nothing is more important in science than classifying and defining well... [though] it depends much more on our knowledge of the subject matter being discussed than on the rules of logic."

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