Extending, Expanding, and Laying Bare: A Unified Account of Generalization in Mathematics

Zachary Gabor

First Reader: Danielle Macbeth
Second Reader: Joel Yurdin

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Abstract

It is quite common for mathematicians to refer to theorems or definitions as generalizations of others. Although one gets a very good sense of what the term means by doing enough mathematics, it is not a term that mathematicians typically formally define. Indeed, with a little consideration, it can be seen that the task of giving a proper and comprehensive definition is highly non-trivial, because there are various different applications of the term in widely disparate contexts. Nonetheless, the use of the term is rarely, if ever controversial within the mathematical community. This suggests that there is something, albeit difficult to articulate, that mathematicians intuitively recognize the disparate cases to have in common. The primary goal of my thesis is to explain precisely what this commonality is, by giving a definition of generalization that is applicable to each of the various cases of the term’s use. In service of this goal, I lay out an ontological picture of mathematics that borrows both from the long-standing structuralist ontological view of mathematics, and from the work of Danielle Macbeth, who claims that mathematics is a study of objective concepts, the referents of mathematical definitions, which definitions give Fregean senses. Having given my account of generalization, I then elaborate on generalization’s roles in mathematical practice, using my account to shed some light on its utility in these roles.
The notion of generalization is one that mathematicians frequently use, but rarely, if ever, worry about defining precisely. In spite of the fact that, as we will see, mathematicians use “generalization” to refer to pairs of definitions or theorems\(^1\) that relate to one another in drastically different ways, it is rarely, if ever, a matter of controversy whether \(X\) is a generalization of \(Y\) in mathematics. Very roughly, a definition \(X\) of \(x\) is a generalization of a definition \(Y\) of \(y\), when the former definition is such that \(y\)'s may be conceived of as special cases of \(x\)'s. A first pass at formalizing this, natural to anyone versed in the language of contemporary mathematics, would be to claim that \(X\) is a generalization of \(Y\) if there is some bijective mapping from the \(y\)'s to a subset of the \(x\)'s which preserves some relevant mathematical properties of the \(y\)'s\(^2\). Indeed, such a mapping exists trivially in many cases, such as that of the correspondence between the real numbers and the complex numbers with imaginary part 0. However, there are some philosophical concerns that arise from this first attempt at defining mathematical generalization.

In particular, there seems to be a commitment to Platonism implicit in the phrasing of this characterization. The language seems to imply that there are some objects that are the \(x\)'s which correspond via a mapping to some subset of those objects called the \(y\)'s. Of course, this may just be a manner of speaking (and as we will see, this is more or less the case), but if it is only that, then more must be said about those subtleties that the manner of speaking glosses over.

Additionally, it is not obvious how the notion of a mapping applies at all to certain cases of mathematical generalization. For instance, we consider the notion of a commutative ring with

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\(^1\) Mathematicians commonly refer to both theorems and definitions as generalizations of other theorems and definitions. Intuitively, the two uses of the term are closely related, but the scope of this thesis will be confined to the use of the term with respect to definitions.

\(^2\) This rough characterization of generalization was first advanced to me by Phil Gressman, who instructed me in a course in Real Analysis at the University of Pennsylvania.
identity to be a generalization of the integers. But there is no way that we can map the integers onto some subset of a general abstract ring. In particular, there are infinitely many integers, whereas many rings only have finitely many elements. There are also operations that are considered to be generalizations of other operations, like the Lebesgue integral is of the Riemann integral. It is not on its face clear that when we are considering a single operation as a special case of another, rather than a set of entities as a special case of another, that we can talk about the relation of being a generalization in terms of a mapping of some set of entities onto another, since there is only one entity here, rather than a set. This suggests that there is further subtlety to the notion of generalization, that hasn’t been accounted for by the naïve characterization.

A characterization of mathematical generalization ought to accomplish the following aims: it ought to make clear what precisely, if anything, all of the seemingly disparate sorts of mathematical generalization share in common, and if there is no such commonality, and the term is instead being used with multiple meanings, make these meanings clear and make clear in precisely which cases which meaning is being used. What is more, an account of generalization ought to provide a framework in which it can naturally be explained how generalizations serve mathematical practice in the ways that they do.

My ambition is to give an account that accomplishes both of these aims. We will see that this account comprehends generalizations of all types, attending to the widely disparate examples of abstract algebra, concrete number fields, the Riemann and Lebesgue integrals, and distributions. Furthermore we will see how, in light of the account, generalizations can be used in mathematical practice to
a) *Explain* the truth of theorems, for which no properly explanatory proofs were previously known

b) Enable previously invalid mathematical inferences

c) Make rigorous the fuzzy mathematics used by physicists

I: The Apparent Heterogeneity of Mathematical Generalization

A primary reason to think that the task of giving a unified account of generalization in mathematics will be challenging is the apparent disunity in the term's use. Mathematicians refer to various definitions, fields of inquiry, or contexts, as generalizations of others, in spite of the fact that the relationships between these pairs of definitions, fields, or contexts seem quite disparate. First, there are instances of generalization in which one sort of entity is conceived of as an example of a broader class of entities. Abstract algebra provides many cases of such generalizations. For instance, prior to the development of modern algebra, we were aware of the integers, and the relations between triples set out by addition (that is, the description of the behavior of the integers with respect to the operation of addition by given by the collection of triples of integers, where \((a, b, c)\) is a member of the collection if and only if \(a + b = c\).)

However, with the toolbox of modern algebra at hand, we can see the integers under addition as an *example* of a group. Briefly, a group \(G\) is a set \(S\), and a binary operation \(*\), which associates pairs of elements in \(S\) with single elements\(^3\) of \(S\), wherein the following three properties are satisfied

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\(^3\) Again note that \(*\) can be thought of as a collection of triples of elements of \(S\), where \((a, b, c)\) is a member of the collection if and only if \(a * b = c\). I continue to emphasize this, because it will be important to recall at a certain juncture in this project (in section III) that operations can be thought of in this way.
(1) There is some element $e$ of $S$ such that for any element $x$ of $S$, $x \ast e = x = e \ast x$ (this is called the identity element)

(2) For each element $x$ of $S$, there is some element $x^{-1}$ of $S$ such that $x \ast x^{-1} = e = x^{-1} \ast x$

(3) For any elements $x$, $y$, and $z$ of $S$, $x \ast (y \ast z) = (x \ast y) \ast z$

If we let $S$ be the integers and * be addition, then these properties are clearly satisfied, so the integers under addition are an example of a group.

In contrast, when mathematicians speak about the generalization from the real numbers to the complex numbers, they typically speak of the more general and more specific entities as standing in an entirely different relation to one another. The complex numbers may be understood as the set of all sums of the form $a + bi$, where $a$ and $b$ are real numbers and $i$ is the square root of $-1$. The real numbers are just the sums on this form in which $b = 0$. Thus the real numbers are not conventionally understood as an example of “a set complex numbers” of which there are many, but instead are understood as a subset of the set complex numbers, of which there is only one.

Colyvan draws this same distinction between two different types of generalizations. He refers to the former type as the sort that “abstract[s] away from detail to lay bare the crucial features of a mathematical system in question” (2012, 88) and the latter as a sort that “extend[s] a system to go beyond what it was originally set up for” (2012, 87). In Colyvan’s terms, the crucial (at least in certain contexts) features of the integers along with the operation addition, are given in the axioms that define a group, and in recognizing that the integers under addition are an example of a group, we have lain this bare, and when we find that we can express the real
numbers as a subset of the rational numbers, we have extended the system of the real numbers to go beyond what it was originally set up for\(^4\).

However, the form of a mathematical generalization can differ along other dimensions than the one we’ve already laid out. In our first two examples, the entity to be generalized was a set, and the class of entities to which we generalized was a set or a type of set. However, a generalization need not be of a set. For instance, mathematicians also take the idea of generalization to apply to operations. A good canonical example of this is the case of the generalization of the Riemann integral to the Lebesgue integral. The Riemann integral is an operation which takes functions as inputs, and outputs a real number corresponding to what is intuitively the “area under the curve” of the function’s graph. There are many functions however, (in which there is no intuitive value for “area under the curve”) on which the Riemann integral is not defined. The Lebesgue integral is defined entirely differently, but yields the same result for all functions for which the Riemann integral is defined, as well as for many more. In fact, the only functions on which the Lebesgue integral is not defined are only of mathematical interest as pathological examples for the integral and its surrounding theory. This is non-trivially a different sort of example of generalization than the ones we’ve already discussed for the following reason: we might say that the generalization from the integers to the notion of a group reveals that we can think of the integers as an example of a group, and that the generalization from the real numbers to the complex numbers reveals that we can think of the real numbers as a subset of the complex numbers, but it seems odd to say either of these things precisely in the case of the generalization from the Riemann to the Lebesgue integral. The Riemann integral isn’t an

\(^4\) We will attain some insight into what Colyvan means in referring to “what [domains are] set up for” when we discuss domain extensions in section (II).
example of a member of some broader class of entities known as the Lebesgue integrals; there is only one Lebesgue integral. Moreover, it is not obviously natural to think of the Riemann integral as anything like a “subset” of the Lebesgue integral; the Riemann integral is an operation, not a set. The most obvious natural account of the sense in which the Lebesgue integral is a generalization of the Riemann integral is that the Riemann integral gets at some property of functions, and that the Lebesgue integral gets at some more general notion of that same property; a property that for functions of which the first property obtains at all, is the same property, but also obtains of other functions. That is, in light of our generalization, we see that we can conceive of the property corresponding to the Riemann integral as just a special case of the property corresponding to the Lebesgue integral. If we are to give a unified account of generalization in mathematics, we must be able to explain how it is that these two cases, one generally thought to reveal properties as special cases of others, and the other generally considered to reveal objects as special cases of others, can be the same mathematical relationship.

There are also cases of generalization which seem somehow “imperfect” but are nonetheless referred to as generalizations. For instance, distributions are often referred to as “generalized functions” even though their relation to functions does not exhibit all of the same features as do the relations between the general and particular entities in each of the three previous cases I’ve laid out. Distributions were originally defined in order to rigorize mathematical notions used in physics which were previously not fully rigorous. “As far back as Heaviside” writes Folland, in his textbook on Real Analysis (1999, 281),

... engineers and physicists have found it convenient to consider mathematical objects which, roughly speaking, resemble functions but are more singular than functions... and one of the most important conceptual advances in modern analysis is the development of methods for dealing with them in a rigorous and systematic way.
Briefly, a distribution is an operator that takes a certain class of functions (smooth functions of compact support) as inputs, and gives real numbers as outputs, and has the property that input functions which are "close together" in a particular sense issue in outputs that are close to one another. Many of these operators correspond to functions, in that we can, by observing the limit of the sequence of outputs for particular sequences of input functions, "extract" the values that the corresponding function takes on at almost every point\(^5\). Thus, by this correspondence, we can think of most functions as special cases of distributions, a broader domain. However, we cannot simply reconceive of every function as an example of a distribution. Although the class of functions which correspond to distributions is very broad, there are many functions, even simple ones like \(f(x) = 1/x\)\(^6\) which do not correspond to any distributions. A unified account of generalization must account for cases such as these, and either explain the sense in which they are genuinely generalizations, or explain what tempts mathematicians to wrongly categorize them as the same sort of thing as other generalizations.

II: Generalization's Use in Mathematical Practice

The discussion above indicates a number of senses in which the things that mathematicians call generalizations differ from one another. However, having done merely this lays out a curiosity, rather than a problem. If there is a genuine philosophical problem here to solve, it is only in virtue of generalization being an important part of mathematical practice, worth explaining. In support of the claim that there is such a problem here, I will explain a few of the roles that mathematicians and philosophers have attributed to generalization within

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\(^5\) This phrasing, although it sounds rough, actually has a technical meaning in mathematics.

\(^6\) With, say, \(f(x) = 0\) at \(x = 0\).
An account of generalization in mathematics should be able to explain how generalization manages to fulfill these roles.

Among these roles, as Tappenden highlights in "Mathematical Concepts and Definitions," is their explanatory power. Tappenden discusses the case of a classical theorem in number theory called the Law of Quadratic Reciprocity, which gives a set of conditions for primes $p$ and $q$ about the solvability of quadratic equations in their modular arithmetic. Briefly, we say that $a \equiv b \mod c$, pronounced "a is congruent to b mod c" if dividing both a and b by c yield the same remainder. So, for example, 3 is congruent to 8 mod 5, as it is to 13, 18... as well as -2, -7.... The law of quadratic reciprocity states that "If $p$ and $q$ are odd primes, then $x^2 \equiv p \mod q$ is solvable exactly when $x^2 \equiv q \mod p$ is, except when $p \equiv q \equiv 3 \mod 4$. In that case, $x^2 \equiv p \mod q$ is solvable exactly when $x^2 \equiv q \mod p$ isn't" (Tappenden, 2008, 260). This is a theorem that "cries out for explanation," (2008, 260) as Tappenden puts it, because there is no obvious reason that information about the mod $q$ "universe" should give us any information about the mod $p$ "universe". Moreover, the disjunct provokes a request for explanation. There's a sleek statement of the law for all but a particular set of cases, so it is natural to demand an explanation for why the rule differs in just this case. Tappenden makes this point by quoting a review of a textbook on algebraic number theory, which states that "[t]he proofs [of quadratic reciprocity] in elementary textbooks don't help much. They prove the theorem all right, but they do not really tell us why the theorem is true." Instead, the review continues, "the quadratic reciprocity law should be understood in terms of algebraic number theory" (2008, 261). That is, we can understand why this theorem is true only in virtue of understanding it as a special case of more general theorems about more general domains in abstract algebra. The situation we are left with, according to Tappenden, can be described as follows (2008, 265-6):
Investigations of quadratic reciprocity and its generalizations reveal deep connections among... an astonishing range of fields... The upshot of the general investigations is a collection of general theories regarded by mathematicians (and hobbyists)... as explaining the astonishing connection between arbitrary odd primes.

It seems that the truth of the law of quadratic reciprocity is best explained in virtue of a generalization. In fact, according to Goldman (1998, 241), the establishment of the field of algebraic number theory as a whole was motivated by a search for such generalized laws, along with a solution to Fermat’s last theorem.

What is going on here? A preliminary explanation can be given through an analogy to an example of Colyvan’s (2013). Colyvan suggests that some physical phenomena are best explained by mathematical principles with the following example: Imagine that we are trying to fit a square peg into a round hole. The physical explanation for our inability to do so is that a particular bit of the boundary of the hole is coming into contact with a particular bit of the peg. But if we turn the peg slightly, we have a different explanation; that it is coming into contact with some other bit of the peg. But this seems wrong, because we want to say that the same thing, not something new, is going on in this case. That the explanation is the same in both cases is only apparent in light of a mathematical fact – that a square cannot be inscribed in a circle with diameter equal to its side length. So the real explanation here is mathematical. In our case the putative explanations for the truth of quadratic reciprocity – the elementary proofs – are mathematical as well, but the mathematical principle that gives a genuine explanation lies at a deeper level of abstraction. In light of an account of mathematical generalization, the explanation of what is going on here should become less opaque.

It is also worth noting that both of the sorts of generalization mentioned by Colyvan (2012) are involved in the shift from the law of quadratic reciprocity to more general reciprocity laws. Gauss recognized that establishing reciprocity laws for higher powers involved doing
mathematics with the Gaussian integers, the integral analogue of the complex numbers. In addition to this, though, general reciprocity laws are stated about abstract number fields, the notion of is arrived at by the "laying bare" sort of generalization characterized by Colyvan. Again, an account of generalization in mathematics will explain whether both, or if not, which one, of these sorts of generalizations is essential for keying in on a new explanation, and also explain why this is the case.

There have been various instances in the history of modern mathematics and science in which the physicist’s reach has exceeded the mathematician’s grasp. Apart from the case of distribution theory, already discussed briefly above, the most famous case is probably that of the Calculus. The methods of Calculus, it is well known, were invented for use in solving problems of classical physics. Most importantly, these methods include differentiation, by which instantaneous rates of change are determined, and integration, by which total net change over an interval given a variable rate of change (representable as ‘signed area under a curve’) is determined. These methods were not, however, made mathematically rigorous until long after their adoption. It is easy to think that this rigorization has nothing to do with generalization. For instance, both of these operations depend on the notion of a limit, which was intuitively, but not formally understood at the time of the invention of the Calculus, but the formulation of a formal definition might be thought of simply as a matter of getting clear on what was already intuitively understood, in the same level of generality. But, if properly scrutinized, it is apparent that there is something that smacks of generalization going on here. Operations and properties that matter to Calculus, such as the limit, continuity, derivatives, and integrals, could all be perfectly well defined and understood in certain simple cases. For instance, on a constant function, these properties might all perfectly well be evaluated, and operations performed, using geometric and
algebraic methods. The need for more rigor arises when the horizon of these properties and
operations is expanded beyond the cases in which they may be rigorously defined according to
algebraic and geometric notions. Thus, the rigorization of these operations and properties may
well be understood as a generalization of the scope of their rigor, from a collection of simple
cases, to a broader array.

In addition to allowing for new explanations to come to light, Manders points out that
domain extensions can bring about a phenomenon which he refers to as “existential closure.”
“Existential Closure,” Manders writes, refers to [the condition achieved by] a class of processes
which attempt to round off a domain and simplify its theory by adjoining elements” (1989, 554).
The example Manders focuses on in his article “Domain Extension and the Philosophy of
Mathematics,” is that of the complex numbers. The basic idea is this: in the real numbers, some
polynomials can be fully factored into linear roots, whereas others can’t. For instance, working
in the real numbers, we may factor $x^2 + 2x + 1 = (x + 1)(x + 1)$, whereas $x^2 + 1$ cannot be
factored. But in the complex numbers, we may factor any polynomial into linear factors (even
when we allow the coefficients to be complex!). This means that, in Manders’ parlance, the
domain extension from the real numbers to the complex numbers achieves existential closure
under roots. Indeed, every domain extension, beginning with the natural numbers and ending
with the complex numbers, achieves some existential closure. This existential closure is often

\[\text{In extending the natural numbers to the integers we get additive inverses for every element. In extending the integers to the rational numbers, we get multiplicative inverses for every element. In extending the rational numbers to the real numbers, we get limits for every Cauchy sequence of elements (i.e. every sequence that "ought to have a limit").} \]
used to justify the extension itself, since in each case every element of the extended domain is necessary for existential closure\(^8\).

Existential closure is of more than just aesthetic value to mathematicians; it is of great instrumental value in mathematical practice, because it allows inferences to be made that could not have been made in the un-extended domain that fails to satisfy the relevant existential closure condition. For instance, if a mathematician is trying to prove something about all polynomials over the complex numbers, she may take an arbitrary complex polynomial, and break it up into linear factors. She could not have done this were her domain the real numbers, because the real numbers do not satisfy the necessary existential closure condition.

However, as Manders points out, there are instances when this existential closure comes at a price. "Moving," he reminds us, "from real to complex algebra eliminates the possibility of expressing assertions about the ordering of real numbers" (1989, 557). The standard notion of size, in the real numbers gives only two numbers of any given size, so along with the notion of sign, we may easily determine the order of any pair of real numbers. In the complex numbers, however, there are infinitely many numbers of any given size\(^9\), so there is no natural way to determine the order of any pair of the same size. For example, it doesn’t make sense to say that \(1 + 0i > -1 + 0i\), in the complex numbers, since \(|1 + 0i| = |-1 + 0i| = \left|\frac{\sqrt{2}}{2} + \frac{\sqrt{2}}{2} i\right| = \left|\frac{-1}{2} + \frac{\sqrt{3}}{2} i\right| = \cdots = 1\). This fact poses a worry for our first pass at characterizing what is going on

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\(^8\) Well, not quite. In going from the integers to the rationals, we need to stipulate that our domain is closed under addition to "necessitate the existence" of every rational number, but this is just another existential closure condition, and one that already obtained of the integers.

\(^9\) The complex numbers are canonically represented as the set of points in the plane with the real part represented by the coordinate on the horizontal axis and the imaginary part represented by the coordinate on the vertical axis. The magnitude of a complex number is given by the square of the distance, in this representation, of a point from the origin, \(|x + yi| = x^2 + y^2\), so all of the points in a circle in the complex plane have the same magnitude.
in domain extension. We said that, upon extending the domain of the real numbers to that of the complex numbers, we see that we can think of the real numbers as the subset of the complex numbers with imaginary part 0. But this set is not ordered, whereas the real numbers are, so strict identification seems too strong; we need more subtlety in our account.

III: Conceptual Structuralism

To properly express a definition of mathematical generalization, I will need to do some ontological groundwork. This groundwork will consist in motivating and developing a view that the entities studied by mathematicians are structures, and further, that these structures are not collections of objects, but rather concepts. More precisely structures are not collections of particular objects, nor even the concept of a collection of objects standing in a particular complex of relations with one another, but the rather the concept of a collection of objects standing in a complex of relations, which relations satisfy some collection of meta-relational properties; structures are meta-relationally individuated. I will motivate and defend this view by rehearsing an argument that has classically motivated the view that mathematics is a study of structures, and then by arguing that the problem raised by the traditional argument, which structuralism solves, can only be made sense of if the subject matter of mathematics is a collection of concepts, rather than objects. This ontological point of view is a framework that belongs here in service of my project, and is not the project itself. Thus, for reasons of conciseness, I will confine myself to motivating the positive view, and will not go so far as to rule out the various possible alternative views, or to defend my view thoroughly against objections (although I believe that both can be done.)
Structuralism is conventionally expressed as the view that mathematical objects have only those features which define their relationships to other mathematical objects. “Mathematics,” writes Hellman “is seen as the investigation... of ‘abstract structures,’ systems of objects fulfilling certain structural relations, without regard to the particular natures of the objects themselves” (2005, 536). An example of what it means to fulfill the structural relations of some mathematical structure will be helpfully expository here. Consider the example of $K_5$, the complete graph of order 5. This is a collection of five points, or “vertices” such that every pair of points is connected by a line segment “edge”. The participants in a 5 person round-robin Calvinball tournament are objects which satisfy the relational properties of the elements of $K_5$, where the vertices are players, and the relationship of being connected by an edge is the relationship of having played one another in the tournament. Statements about one setting or the other can be translated back and forth, and the truth values of these statements will always be preserved.

A major motivation for the adoption of a structuralist picture of mathematics is the existence of problems like Benacerraf’s under-determination problem, laid out in his 1965 paper "What Numbers Could Not Be". The problem goes as follows: there are various set-theoretic reductions of the natural numbers. For instance, the Von Neumann ordinals express the natural numbers as $\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}, \{\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}\} \ldots$, whereas the Zermelo ordinals express the natural numbers as $\emptyset, \{\emptyset\}, \{\{\emptyset\}\}, \{\{\{\emptyset\}\}, \{\{\emptyset\}\}\} \ldots$. One may ask, within the context of either reduction, “is 1 an element of 3?” If we ask, though, our answer will depend on which reduction we are working with. $\emptyset$ is one of the three elements of $\{\emptyset, \{\emptyset\}, \{\emptyset, \{\emptyset\}\}\}$, but $\{\{\emptyset\}\}$ is a singleton set, containing only $\{\{\emptyset\}\}$, and not $\emptyset$. Thus, the question, which ought to be resolvable, appears
unsettled. The problem melts away if we acknowledge that the natural numbers are *neither one* of these sequences of sets, nor any other, but rather some abstraction from these sequences of sets, focusing on some properties of the relations in which the members of each sequence stand with one another.

If this is the case, and the natural numbers are just some structure instantiated by both the Zermelo and Von Neumann ordinals, then the answer is clear: as Shapiro puts it, “if one inquires whether 1 is an element of 4, there is no answer waiting to be discovered. It is similar to asking whether 1 is braver than 4 or funnier” (1997, 79). Although I have no objection to this claim, the way in which Shapiro elaborates on his structuralist view can be seen to be problematic if we keep this explanation in mind. Shapiro, (like Hellman, among others) refers to positions in structures as objects, which commits him to the view that structures themselves are collections of objects standing in certain relations to one another. The problem is that there is a disanalogy between the elaboration of Shapiro’s answer to Benacerraf’s problem, and the answer that we ought to give. Shapiro’s thought is that numbers are just objects (positions in structures) that, like many other objects, have the property that they have no set membership relation to other objects like themselves. My fork is not an element of my knife, just as the former is not braver than the latter. However, there is a disanalogy latent in the comparison that will come to the fore if we consider the fact that there was never any under-determination problem in the first place with respect to my fork and my knife, and that indeed, no such problem can be formulated, even for the sake of completing the analogy. This is because, in the case of the natural numbers there are two alternative pictures of the natural numbers, or if we accept the structuralist language, two systems instantiating the natural number structure, in one of which 1 is an element of 4, and in the other of which it is not. No pair of alternative instantiations of my fork and knife exist in
which one is an element of the other in one instantiation, and this is not so in the other. Indeed, it
does not seem that there are any instantiations of my fork and knife at all – objects of this sort are
not instantiated by anything non-identical to themselves. The answer, one suspects – with the
proviso for now that there is an obvious alternative that I have yet to rule out – is that the
structure that is the natural numbers is a concept – a concept of a certain type of system – rather
than a collection of objects, and that each of the positions are in turn concepts – concepts of
being an element of a system related to other elements in certain specified ways – rather than
objects. The view in general, is that **a structure is simply the concept of a system of a certain
kind, where kinds of systems are individuated by the complexes of meta-relational
properties that their relations satisfy, and that positions are concepts of the form “being a
system element standing in relations to other elements, which relations satisfy such and
such meta-relational properties.”** The traditional view (that Hellman and Shapiro accept
when they refer to structures as collections of objects) is that there are systems, which are
collections of concrete objects, and then there are structures, which are collections of abstract
objects, and then finally there are concepts of structures. I assert that, in distinguishing between
the latter two, proponents of the standard picture of structuralism have tried to discern two things
where there is only one. There are systems, and there are concepts of systems, which are
structures. There is no third thing. If we want to know something about cats, we may study some
individual cat, or some collection of individual cats, or we may consider what follows simply
from being a cat; we may analyze the concept “a cat”. There is no general abstract object “a
cat” that we may examine, either empirically, or in thought. Likewise, if we want to know

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10 I will at times, when I want to emphasize or to remind the reader that I take structures and positions to be
concepts, refer to structures as “structure-concepts” and positions as “position-concepts” but in each case, the
terms are interchangeable.

11 I will return shortly to the notion of an abstract object.
about some structure S, we may examine instances of this structure (i.e. systems), or we may consider the concept of S, but there is no general abstract collection of objects S that we may examine empirically or in thought; that which we examine in thought is the concept\(^{12}\). On this view, the disanalogy between the two cases can be easily accounted for: in the case of numbers, we are dealing with a concept that can be instantiated in various ways, whereas in the case of my cutlery, we are dealing with objects which are instantiated uniquely by themselves, if we even want to say that they are instantiated at all.

So, although Shapiro’s remark does in some sense get at the solution to the under-determination problem, we can see that it also fails to emphasize an important feature of the natural numbers that makes it possible for the problem to arise at all. A more perspicuous explanation might be phrased as follows: “Socrates was a man, and Wittgenstein was a man; they both instantiated the concept ‘a man.’ Socrates was poor, and Wittgenstein was rich, but ‘a man’ is, of course, neither poor nor rich. Likewise, we can say that both the Zermelo ordinals and the Von Neumann ordinals instantiate the natural number system (or, to convey the same content highly un-idiomatically, they are both ‘a natural numbers’) and although the Zermelo ordinal that instantiates 2 is a doubleton set, and the analogous Von Neumann ordinal is a singleton set, 2 is neither a singleton set, nor a doubleton set, nor even a set at all.”

The obvious alternative to this picture, to which I alluded just briefly above, is one in which we conceive of the natural numbers (as well as ‘a man,’ for instance) as abstract objects rather than concepts. The difference between my particular pieces of cutlery and the natural numbers on this picture would not be that the former were objects whereas the latter were not,

\(^{12}\) It should be noted that our investigation of mathematical concepts amounts to more than mere investigation thereof, but this fact alone is not reason to believe that the objects of this investigation are not concepts.
but merely that the former are concrete, whereas the latter are abstract. There are issues of ontological parsimony here, though. We have no trouble accepting that there are concepts, and that there are particular objects, but what specifically are abstract objects? How do we know that they exist? In what sense do they exist? And what are we to gain philosophically from supposing that such entities exist?

Fine raises a strong objection to the existence of abstract (or “arbitrary” as he calls them) objects, in the face of which he means to defend their existence. “Is it seriously to be supposed,” Fine asks, “that in addition to individual numbers, there are arbitrary numbers and that, in addition to individual men, there are arbitrary men? What strange sorts of objects are these? Can I count with arbitrary numbers or have tea with an arbitrary man?” In an effort to respond to this worry, Fine does something remarkable: he capitulates completely. Fine distinguishes between an ontologically significant and an ontologically insignificant sense of “there are,” and writes

If now I am asked whether there are arbitrary objects, I will answer according to the intended sense of ‘there are’. If it is the ontologically significant sense, then I am happy to agree with my opponent and say ‘no.’ ... But if the intended sense is ontologically neutral, then my answer is a decided ‘yes’. I have, it seems to me, as much reason to affirm that there are arbitrary objects in this sense as the nominalist has to affirm that there are numbers.

I will readily agree with Fine that we might rightly say that there are abstract objects, if “there are” is interpreted in an ontologically insignificant sense, the same as that in which we would interpret a nominalist’s affirmation that “there are” numbers. I merely wish to point out that this ontologically insignificant sort of being, the sort of being ascribed by the nominalist to numbers, is what, when speaking carefully, the philosopher and non-philosopher alike typically refer to as “not being.” The use of “there are” that Fine is focusing on is a mere manner of speaking; when the nominalist affirms that there are numbers, she does so with her fingers firmly crossed.
Ultimately, Fine’s theory of abstract objects is, as far as I can tell, dependent upon the presupposition that sets are objects, which contain objects as members. Indeed, it is telling that he takes for granted that there are individual numbers (even for the nominalist!) and questions only whether there are arbitrary numbers.

I believe that the tendency to postulate abstract objects is, in large part, grammatically motivated. The sentence “the integers are a group under addition” is thought to be true, and it is thought that this can only be so if there are some objects the integers to which our judgment conforms. But it may be instead that we are actually asserting, in a grammatically convenient, but metaphysically deceptive way, that the structure-concepts “integers under addition” and “group” are related in some particular way (as we will see, they will turn out to be related by both subsumption and subordination).

However, beyond the linguistic convention that tempts the mathematician to postulate mathematical objects, there may also be a deeper metaphysical issue at play, which leads the philosopher to think they must exist as well. This issue, Macbeth suggests, is that it is taken for granted, following Kant, that judgments may only be contentful (and thus, may only be objectively true) if they involve objects; concepts are just in our minds, and so if mathematics is about anything, other than the form of our own thought, it must be about objects. But, Macbeth argues, a proper grasp of Frege’s theory of sense and reference reveals that this entailment does not necessarily hold. The distinction between the thing corresponding to a linguistic term that is before the mind and the thing that is in the world may lie in the distinction between sense, the terms cognitive significance, as it matters to inference and judgment, and referent, the mind-independent thing that is grasped by the mind through the sense. According to Frege, both object words and concept words have senses and referents. “On Frege’s view,” Macbeth writes (2014,
...both concept words and object names express sense, and so are cognitively significant, and both concept words and object names designate something objective, namely, concepts conceived as laws of correlation and objects, respectively."

The sense/reference distinction certainly seems to be applicable to mathematics. For instance, "the singleton set \{2\}" and "the set of all even primes" have different cognitive significances, but the same referent. Indeed, we can certainly imagine the person who understands what each of the two phrases mean, but fails to recognize that they refer to the same set. Just as it would be quite strange not to suppose that the referents of our words for material objects pre-existed us, it would be quite strange not to suppose that the referent of "the singleton set \{2\}" and "the set of all even primes" pre-existed us. So, the proof that the singleton set \{2\} is the set of all even primes extends our knowledge in much the same way as does the discovery that Hesperus is Phosphorus. "But," the obvious objection goes, "this is simply because, in the mathematical case, the referent is an abstract object!" Even if we grant this objection for the sake of argument, our objector has not won, for if the set of all even primes is an object, and it is the same object as the singleton set \{2\}, and this object pre-existed us, then it certainly must have been true prior to our existence that the singleton set \{2\} is the set of all even primes. But, if this is the case, then it must also have been true prior to our existence that being the singleton set \{2\} is equivalent to being the set of all even primes. Then, since "being the singleton set \{2\}" and "being the set of all even primes" are surely both concepts, a contentful truth about concepts must have existed prior to us being around to grasp it. The idea is simply to

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13 A 'law of correlation' here means a truth function on all objects, wherein an object that falls under some specified concept is given the value true, whereas one that does not is given the value false.
14 With the obvious exception of those objects whose age we know or suspect to be less than ours.
collapse the supposed metaphysical distinction between "the set of all even primes" and "being the set of all even primes", and to take the two phrases to refer to the same thing.

The motivation to so collapse comes from the fact that there is no use that the mathematician can make of any supposed abstract object "the set of all even primes" beyond that which she can make of the concept "being the set of all even primes." In mathematician, in doing mathematics, does not examine the set of all even primes, hoping that its mathematical features impinge upon her intellect, as an anatomist hopes that an organism's features will impinge upon her vision. Instead, she considers what it means to be the set of all even primes—what follows from so being. Nothing follows inferentially from any object itself, but rather from its being such and such a sort of object; inferences are licensed only by concepts. Thus, even having granted to our objector that there are such things as mathematical objects, we still conclude that they are not the subject matter of mathematics. Postulating the existence of mathematical objects, then, is in no way metaphysically useful, and any theory that does so should be discarded as unparsimonious.

Structuralism is a picture of mathematics that works nicely for both sets and numbers, as is to be expected of a view motivated by a problem having to do with sets and numbers. But mathematics is not the study only of sets and numbers. Mathematics involves functions and operations, and properties, and relations as well. How do functions, for instance, fit into a picture of mathematics as about structures? Here, I think it is important to point out that the set theorist reduces a function $f$ to a set of ordered pairs wherein there may be only one pair with any given first entry, and an $n$-ary relation $R$ (where a unary relation is just a property) to the set of $n$-tuples to which $R$ applies. This is not to suggest that these set-theoretic reductions are what functions and relations just are; I think that it would be a mistake to do so. But, the fact that we can make
these reductions and still express the mathematical thoughts that we desire to express is reason to think that there is nothing essential here lost. That is, whatever essentially constitutes a function is captured in our characterization of a function as a set, just as it would be equally well captured in a characterization of a function as a rule, for instance. But, if the essence of a function can be exhausted by its description as a set, and a set is just a structure, then there is nothing lost in thinking of a function as a structure as well. A function is just a special sort of structure, in which a special sort or relation\textsuperscript{15} obtains between two antecedently given domains (the domains of which the domain and co-domain of the function are sub-collections)\textsuperscript{16}

Moreover, although it is virtually never the case that contemporary mathematicians think of their practice in set-theoretic terms, it is the case that sets play a peculiarly foundational role in most, if not all fields of mathematics. Analysis is done on measure spaces, which are sets, algebra on groups, rings, and fields, all sets, topology on topologies, which are once again sets; one could go on. Even category theory, which is often characterized as a foundational picture of mathematics in which mathematics is based on functions, requires a pre-mathematical notion of a set to get off of the ground.

**IV: An Account of Generalization**

With the ontological framework of conceptual structuralism in place, I can now lay out a definition of mathematical generalization. I will give two formulations of the definition, one that is more naturally applicable to the cases of generalization that Colyvan says “lay bare crucial

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\textsuperscript{15} In particular, a relation wherein every relatum in the domain is related to exactly one relatum in the co-domain.

\textsuperscript{16} The apparent redundancy here is due to the fact that the mathematician and the philosopher of mathematics appear to use the term ‘domain’ in two distinct senses. Mathematicians typically use the term in reference to a function, as the set on which the function is defined, but not every function’s domain is a ‘domain’ in the sense (to be laid out below) in which I think that the term must be understood as it used in referring to “domain extensions”.

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features" and another that is more naturally applicable to the case of domain extensions. However, I will also argue that the two definitions are equivalent; that any pair of mathematical definitions that satisfies the one definition of generalization also satisfies the other, thus vindicating the account as unified. To illustrate this fact, I will show that both particular cases of domain extension and of laying bare generalizations may be characterized not only by the definition naturally suited to them, but also (albeit un-idiomatically) by the definition that is not so suited. We will also see that generalizations such as that from the Riemann to the Lebesgue integral are distinct from both of the two aforementioned types, and require the formation of a new category in which to classify them, but we will also see that they satisfy the definition of generalization that I give.

Some definition X is a generalization of a definition Y when the structure-concept given by Y is subordinate to some sub-structure-concept given by X. Alternately, X is a generalization of Y when each position of the structure given by Y is subordinate to a unique position of the structure given by X. I claim that these two definitions are equivalent; that the former condition is satisfied if and only if the latter is. If any system that instantiates Y also instantiates either X or a substructure of X, then the occupier of each position in the system occupies the analogous position in a sub-system of a system instantiating X. So, for instance, because any system that instantiates the structure of the integers with addition also instantiates the structure of a group, the occupier of a position in a system instantiating the integers with addition occupies a (conceptually subordinating) position in a system that instantiates a (conceptually subordinating) structure, namely, the domain of all group elements. Likewise, if any definition Y gives a structure-concept such that in any system instantiating Y, the occupier of each position is an instance of an occupier of a position in a system instantiating X, then any system instantiating
the structure-concept given by Y must also instantiate some sub-structure of the structure given by X. So, for instance, because any occupier of a position within the real numbers structure also occupies the position within the complex numbers structure to which it is conceptually subordinate, any system instantiating all such positions (i.e. instantiating the real numbers structure) also instantiates a substructure of the complex numbers structure, (the “horizontal axis in the complex plane.”)

As I mentioned at the beginning of the section, I take note of these two equivalent definitions because, depending on the case of generalization, it may be more natural to think of the generalization relation along either set of lines. For instance, in the generalization from the definition of the integers under addition to that of a group, it is natural to think of the relationship between the two definitions along the lines of the first definition of generalization. Any system that instantiates the additive natural numbers structure must also instantiate the group structure. However, we may also observe that it satisfies the second definition as well – any instance of an integer is an instance of a group element. Nonetheless, the generalizing definition in such a “laying bare generalization” is typically given from the top down; it gives the structure as a whole (e.g., a group) directly, and thus the positions (e.g., group elements) implicitly.

Conversely, it is more natural to think of the sense in which the complex numbers are a generalization of the real numbers in terms of positions. Each instance of a real number is an instance of a complex number (with imaginary part 0). However, we might also say that any system instantiating the real number structure instantiates the “x axis of the complex plane,” a substructure of the complex number structure. This distinction of emphasis serves to explain the distinction between domain extensions and “laying bare” generalizations. In the case of a domain extension, both the generalizing and generalized definitions give structures only implicitly.
These structures are domains, the collection of all position concepts satisfying some features specified by giving an arbitrary element (i.e. position). The definition of the real numbers gives the concept of a position within the real numbers, and the concepts of relations between them, and gives the structure implicitly as the domain of all such position concepts related by the relevant relations. So, domain extensions involve bottom-up definitions; we begin with a position concept and relation concepts, which implicitly give a domain as a structure, and generalize by giving broader, subordinating position and relation concepts, which thus implicitly give a broader domain.

However, not every generalization in which the definition is of a position in the structure, rather than the structure as a whole, is a domain extension, properly speaking. Consider, for instance, the definition of the Lebesgue integral, which generalizes the definition of the Riemann integral: both of these definitions define structures in which an operation-relation obtains between two antecedently given domains, and both definitions do not give the structure as a whole, but rather, give an arbitrary pair of relata in the relation that constitutes the operation. But, in both cases, the pair of domains being related is the same: the real functions, and the real numbers. The ambient domain in which the two operations exist hasn’t changed. What has changed is the domain, in the mathematical sense of the term, of the operation. In the Lebesgue integral, more positions in the domain of real functions count as relata in the relation between the real functions and the real numbers that constitutes the operation. In general, operations are defined by giving an arbitrary pair of relata from the two antecedently given structures that the operation relates, and a generalizing definition of an operation gives a subordinating concept of what it is to be a position in the domain (in the mathematical sense) of the operation, thus meaning that a larger subcollection of positions in the structures related by the operation count as
relata. I will call the generalizations of operations *domain expansions*, with the intention of invoking the imagery of the domain (in the mathematical sense) of the operation expanding within the structures it relates, which remain fixed.

In the case of the sort of generalization that Colyvan thinks of as a “laying bare essential properties,” the generalizing definition gives a structure, rather than giving positions and relations, and so the relevant structure concept is not a domain, not a totality of positions instantiating some position concept. So, for instance, in the case of a group, the definition explicitly gives the structure of a group, not a group element and the relations that obtain among the positions. If the definition were given this way, and the structure given only implicitly, then the structure defined would not be a group, but rather the domain of all group elements. An individual group would relate to this definition in the same way that the structure “a line in the complex plane” relates to the definition of the complex numbers. This domain of all group elements, of course, is also given implicitly by the standard definition of a group, and is of mathematical import (for instance, when considering homomorphisms between groups), but, importantly the definition explicitly gives a structure circumscribed within this domain. In the case of a domain extension, the generalized structure will always be ‘smaller’ than the generalizing structure. In the case of a “laying bare” generalization, this is not so. The integers structure, conceived of as a domain, is smaller than the domain of all group elements. But, the integers structure is an instance of the group structure, and so is in no way smaller than it.

It is important to note that I have just made a claim that is distinct from any that I have made up until this point. I’ve claimed not just that every instance of a system instantiating the integers structure also instantiates the group structure, but that the integers structure *itself* is an instantiation of the group structure. This is the case because, in addition to the relation of
subordination, the structures given in a generalized and generalizing definition stand in a relation of subsumption to one another. That is, it is more than a manner of speaking – in precisely the same way that it is no more than a manner of speaking when we speak about mathematical entities as objects – when we say that the integers are an instance of a group. The positions of the integers relate to one another in such a way that these concepts themselves instantiate the group structure. The integers are an instance of a group, in a way that “a dog” is not an instance of a mammal. That is, they are an instance of a group in the sense in which Fido is an instance of a mammal. This is the case because structures, such as that of a group, are peculiarly broad concepts; they may be instantiated by collections either of objects or of concepts. This is in turn is the case because the properties that define a structure are meta-relational: properties that particular relations – whether they be among objects or among concepts – may instantiate. So, for instance, a mathematical definition may stipulate that some collection of pairs of positions must be related to one another by a transitive relation. But both relations between objects (like being the same size), and relations between concepts (like subordination) may be transitive. In fact, this alleviates the need to be careful about whether the relations given in a definition obtain between the positions of a structure themselves, or between the occupiers of those positions within a system instantiating structure; analogous relations, satisfying the relevant meta-relational properties obtain among both!

The subsumption relation that obtains between generalizing and generalized definitions also may be thought of as motivating the tendency to speak about certain mathematical structures in objectual language. One might think that if the integers are an instance of a group, then they must be objects, since we most typically think of concepts as being instantiated by objects. However, because our concepts of structures are constituted by meta-relational properties, which
may be instantiated by relations among objects or among concepts, we may maintain that the integers are a structure(-concept) while also granting that they instantiate the group structure.

V: Perspective on the Utility of Generalization in Mathematical Practice

If my account of generalization is to be thought at all plausible, it ought to be compatible with the understanding of the use that mathematicians make of generalizations in mathematical practice, which I detailed in (II). What is more, if I can demonstrate this compatibility by using my account to shed some light on the nature of this use in mathematical practice, this should count in the account's favor. I plan to do so in this section, developing the view that each of the three uses of generalization detailed in (II) is associated with a particular one of the three kinds of generalization that I identified in (IV), thus making manifest the natural fit of my account of generalization to its use in mathematical practice. “Laying bare” generalizations, I claim, are associated with the development of explanatory proofs, domain extensions with the enabling of mathematicians to make new kinds of inferences, and domain expansions, with the rigorization of fuzzy mathematics used in natural science.

With a conception of generalization as a relation between structures in hand, it is not especially complicated to explain how proofs given in a generalized setting may be more explanatory than those given in a particularized setting. A mathematical proof is achieved by a series of inferences, to which the mathematician is entitled in virtue of the hypotheses of the statement to be proved, which lay out the structure or structures about which something is to be proved. Particular inferences made in the course of a proof are made in virtue of particular structural properties of the structure in question. So, for instance, in the proof (by contradiction) that there are infinitely many primes, we may assume that there is a number one greater than the
product of a supposed list of all primes, due to the Archimedean property, the structural property
of the natural numbers that every number has a successor. However, because a generalized
structure subordinates a particularized structure, it may be that the structural properties necessary
to license the inferences that go into some proof are weaker than the properties of the structure
given in the hypotheses; the structural properties at work may in fact be broader properties that
apply equally to some subordinating structure. The additional structural properties of the
particularized structure may serve only to obfuscate what is going on.

So, for instance, if we show a student of elementary algebra that the function \( f(x) = x^2 + 2x - 3 \) has roots at 1 and -3, using the method of completing the square, but without
naming it, she will most likely see nothing more than a string of seemingly arbitrary
manipulations that appear to issue miraculously in a solution. But, if we instead show her the
method “completing the square” as applied to an arbitrary quadratic polynomial, she will see that
the properties exploited in finding the solution are those that exist of any quadratic function. She
will not just see that it works in some particular case; she will, (provided that she has a pretty
good knack for algebra) understand why it works in any case, and thus, a fortiori, in that
particular case. Likewise with the law of quadratic reciprocity, the inferences drawn in the course
of a proof of the law may appear similarly arbitrarily organized in the particularized setting.
However, in the setting of an arbitrary number field, we see the structural properties that are
essential to the proof highlighted, because they are the only structural properties of the proof in
question.

It is important to take note of a minor complication here: that is, a general reciprocity law
is not the same as the law of quadratic reciprocity. In fact, it must be altered to be even coherent
in the more general setting. However, just as in the case of the solution to a quadratic, a general
proof elucidates a feature of a general structure (specifically of a function \( ax^2 + bx + c \), that its roots are given by \( \frac{-b \pm \sqrt{b^2 - 4ac}}{2a} \)), that, in a particularization of this structure, may be elaborated in different terms, (e.g. that \( x^2 + 2x - 3 \) has roots at 1 and -3), in the case of the law of quadratic reciprocity, understood within the context of the general law, is just an elaboration of the general law in a particularized context, which may be put in different terms due to the particularities of the particularized context.

Although those generalizations that abstract from structural wholes, like the definitions of abstract algebraic structures, may provide settings in which proofs are more explanatory, they are not the sort of generalization that opens up new moves to a mathematician in the course of her proofs. Only domain extensions have this characteristic. The property attained through a generalization that allows new moves to be made in mathematical proofs is the property that Manders names existential closure. This property is one of existence; in general that, given any position in a structure, or collection of positions standing in some (meta-relationally) specified relations with one another, there exists some position or collection of positions that stand in a (meta-relationally) specified relation with the given position or collection. For instance, that any element has an additive inverse, that any subset has a supremum, or that any polynomial has a factorization into linear factors. The corresponding inferences that may be made are, for instance taking the additive inverse of an arbitrary element, taking the supremum of an arbitrary set, or taking the linear factorization of an arbitrary polynomial. It is certainly possible to give a top-down definition of a structure with such an existential closure condition (for instance, a structure with the third exemplary condition can be described structurally as an algebraically closed field). However, these definitions, at least insofar as they are mathematically significant, always follow the bottom-up definitions of such structures as domains, for the simple reason that it cannot be
shown from the top-down definition alone that the structure-concept given by the definition is not self-contradictory. So for instance, although we may define a group, which has the existential closure property that every element has an inverse, we may not prove from the definition of a group that any group exists, i.e. that the structure given may coherently be instantiated. Instead, to prove that this is the case, we must give a bottom-up definition of a domain, and prove that this domain is an instance of such a structure. So, as long as we accept that there is such a thing as a permutation of the integers 1 and 2, and that there is a coherent operation for composing a pair of any two such permutations, then we may perfectly well postulate the domain of all such permutations under this operation, and show quite easily that an arbitrary element of this domain has an inverse, thus proving that a group (in particular, the symmetric group $S_2$) exists. Conversely, we may easily stipulate the definition of a shmoup, which has definitional axioms that are identical to those of a group, with the exception that a shmoup has two distinct identity elements. Although the definition of a shmoup can be given just as simply and clearly as can that of a group, there are no shmoups. It can be proven relatively easily that the axioms defining a shmoup are self-contradictory. However, our inability to find a contradiction in the definitional axioms of a group, or of any other structure defined top-down, may never serve as grounds for the conclusion that those axioms are self-consistent; we may not yet have been clever enough. Only through realizing the structure defined may we ground such a conclusion.

This is precisely why it is of mathematical interest to give a construction of a domain, even if it is already freely used, and taken to be well understood. For instance, giving a construction of the real numbers as the collection of all Dedekind cuts, or as the collection of all
equivalence classes of Cauchy sequences of rational numbers\textsuperscript{17}, and then showing that that either of these domains has the desired properties demonstrates that, so long as we accept that it makes sense to talk about Dedekind cuts, or about Cauchy sequences of rational numbers, it also makes sense to talk about real numbers. In light of this perspective on the usefulness of such constructions, we are equipped to see the value of the constructions given by Von Neumann and Zermelo, which give rise to Benacerraf’s under-determination problem. Such constructions show that, given the coherence of a few basic principles of set theory, the natural numbers structure is self-consistent. Although this fact is unlikely to ever have been in serious doubt, the thought is that the basic notions of set theory are ones on which we have a yet firmer grasp, and the coherence of our notion of the natural numbers is made manifest in such a construction. The constructions of the real numbers are not ontologically significant, in that the real numbers are not identically either the Dedekind cuts or the equivalence classes of Cauchy sequences of rational numbers, but are rather conceptually significant, in that they demonstrate that (given the self-consistency of certain structures on which the constructions depend) the real number structure is self-consistent. The set-theoretic reductions of the natural numbers are conceptually, rather than ontologically significant, in a perfectly analogous sense.

When a definition that gives a generalization makes mathematically rigorous the fuzzy math used in scientific practice, the generalization given is, at least most typically, a domain expansion of an operation. A cursory examination of the nature of scientific practice reveals why this is the case: theories of empirical science are necessarily predictive, and a predictive theory is

\textsuperscript{17} A Dedekind cut is a subset of the rational numbers satisfying a few relatively straightforward properties. A Cauchy sequence of rational numbers is, roughly speaking, a sequence of rational numbers in the tail of which any pair of sequence elements are close together. We define equivalence classes of such sequences by stipulating that two such sequences are equivalent if the tails of the two sequences become close together.
one that correlates circumstances of a certain form with observable phenomena of a certain form according to some rule, thus taking on the structure of an operation. For instance, Kepler’s laws associate the circumstances of a planet’s position relative to its star with quantitative predictions about the pattern of its motion, according to rules given by algebraic operations. When Calculus was invented, the operations that involved taking the slope of a curve at a point, and the area under a curve, which could both easily and rigorously be performed on a special class of curves, were applied to a more general class of curves. Although the computational methods for these operations clearly yielded the “correct” answers, they were not mathematically rigorous; there was no clear, precise, definition of what an integral was, and thus no way to rigorously prove that the methods involved in the computation of an integral actually yielded the correct result in every case. The computational methods that underlie quantum physics involve “functions” that aren’t really functions, most famously, the delta function, which is 0 everywhere, except for an infinite “spike” at $x = 0$. But, thought of as just a function, the delta function is not rigorously or coherently defined (“infinity” cannot be a value taken on by a real function), so in turn, no properties of the function may be rigorously established, and the physicist’s computational methods cannot be certified as mathematically coherent. However, once we define the operation in question such that it makes rigorous mathematical sense in every case we care about, we can confirm the validity of the scientific computations.

In the case of the integral, more than the mathematical structural relation must be preserved for the generalized operation to count as a faithful operation. The integral, on its pre-technical conception, is an intuitive operation that may be computed geometrically. To count as a faithful generalization, the Riemann integral, (and eventually, the Lebesgue integral as well) must be an operation that, in cases where the integral is intuitively computable, proceeds
according to principles that subordinate the intuitive principles of pre-technical operation. The Riemann integral does in fact satisfy this condition. If the area under a curve is intuitively computable\(^{18}\), then the notion of a partition of that curve’s domain corresponds to our intuitive notion of a partition, and our notion of upper and lower sums correspond to intuitive definitions, and their use in the definition of the Riemann integral can easily be seen to correspond to a valid geometric method for computing the area under the curve.\(^{19}\)

**VI: Conclusion**

There remains but one of the primary questions I raised in the early sections of this paper to be answered: the question of whether distributions may properly be called generalized functions\(^{20}\). A first pass at an answer has to do with the immediately preceding material (see n.19); because each individual distribution that corresponds to a function may be regarded as a

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\(^{18}\) There is surely room for disagreement on the question of for which functions precisely this is the case, but I suggest that it is plausible to characterize these functions as the simple functions for which the pre-image of any singleton is either empty or a finite collection of intervals; that is functions that consist of finitely many ‘steps,’ where each step is an interval of the number line.

\(^{19}\) In the case of distributions, the generalizing definition is not itself a domain expansion at all. The definition gives what it is to be an arbitrary distribution, thus implicitly defining the structure to be the domain of all distributions, and so is a domain extension. However, implicitly given in the definition are a collection of domain expansions which are, in an important sense, the ones that matter to scientific calculation. In the course of experimental science, specific calculations must be made yielding specific results. Thus, to utilize a theory that employs distributions to make predictions, scientists must use not an arbitrary distribution, but particular distributions. And each particular distribution that corresponds to a function is a domain expansion of that function. Of course, the sense in which it counts as a domain expansion is slightly subtler than the sense in which the Riemann integral counts as a domain expansion of the intuitive integral (or the Lebesgue of the Riemann). In the case of the integral, the domain on which the intuitive integral is defined is a subset of the functions, and the domain on which the Riemann integral is defined is a broader subset of functions. In the case of a distribution, the antecedently given ambient domain, on within which the domain on which the operation is defined is expanded is conceived anew as a different domain. Instead of being an operation defined on real numbers, as it is when thought of as a function, a distribution is thought of as an operation on smooth functions of compact support, with each real number in the domain of the particularized operation corresponding to a certain equivalence class of sequences of such functions in the reconceived domain. Now, after the domain expansion, our function (or more precisely an equivalence class of distributions corresponding to an equivalence class of functions) is defined not just on a special subset of the smooth functions of compact support, but on all of them.

\(^{20}\) As a matter of personal history, this question is the one that first turned my attention to philosophical questions about generalization in mathematics.
domain expansion of a function, each such distribution is in fact a generalization of some particular function. But this explanation is insufficient, for there are many distributions that correspond to not function at all. If we are to assess the legitimacy of the term ‘generalized function’ as it applies to all distributions, we must then consider whether the relationship between the domain of all distributions and that of all functions is a domain extension. It certainly seems to get close. Every position in the particularized domain of functions, with the exception of those that are, in a certain sense, pathological, corresponds to some position in the generalized domain of distributions. On the other hand, we are doing philosophy, not playing horseshoes, so it appears that close is not good enough.

But, I think that the inexactness of the applicability of my definition of generalization to the case of distribution theory does not entail that the name ‘generalized function’ is totally inappropriate for distributions. The name is not totally inappropriate, because of a key difference between mathematical definitions and definitions in natural language. Frege reminds us that mathematical concepts have sharp boundaries (Macbeth, 2014, 299); for every argument, they assign a truth value. Concepts of natural language do not share this feature. Someone may be decidedly bald, or decidedly not bald, but there is no hard and fast answer as to whether a man who has lost most, but not all of his hair can be correctly described as bald. The definition of generalization I have given here is not a mathematical definition of a mathematical concept. It is a philosophical definition of a meta-mathematical concept. Thus, it seems that distribution theory, with respect to the predicate ‘generalization’ is just one of those cases in which there is not a hard and fast answer. In philosophy, unlike in mathematics, we need not go as far as Gödel to recognize that there are some questions on which there is simply no deciding.

Epilogue

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Macbeth, following Grosholz, distinguishes between three forms of unity which parts of a whole may exhibit, and claims that one of the three plays a distinctive role in mathematical practice. The sort of unity that is "neither an essential unity, as a living body is, the parts intelligible only in relation to the whole, nor an accidental unity, the whole reducible to the parts in relation" (2014, 72 n.24) Macbeth labels "an intelligible unity of parts within the whole, neither of which is [conceptually] prior to the other." It is this emergence of substantive new intelligible features, not proper to the whole or the parts of such unities, that allows mathematicians to extend their knowledge, by recognizing them.

In those generalizations that are fruitful in mathematical practice, the pair of definitions exhibits an intelligible unity. When we see that the integers with addition form a group, we may think of them in group theoretic terms, but we need not always do so; we may still perfectly well refer to "negative two," which, strictly speaking, has no group theoretic meaning, rather than "two inverse" which does. But, as Tappenden is right to point out, when we place our understanding of the theory of the integers within the context of an understanding of more general structures, the former understanding is augmented over and beyond what can merely be said about the integers as such in the language of groups. To see why, rather than merely that the Law of Quadratic Reciprocity is true, we need to understand it as a special case of Artinian Reciprocity, which requires the framework of abstract algebraic number theory to be stated. This theory's being brought to bear on integral number theory reveals something new about the particularized domain.

Of course, the truth of Artinian reciprocity, as considered in the particular context of integral number theory and quadratics, is logically equivalent to the truth of the law of quadratic reciprocity. And yet, from the mathematician's point of view, we genuinely know more about the
latter, in virtue of knowing about the former. This is a peculiar feature of intelligible unities; that there is something about at least one of such a unity’s parts that comes to be known only in virtue of recognizing that it can stand in this unity. We understand quadratic reciprocity and the integers in and of themselves without reference to any more general notions, but we also understand more about them - we come to know emergent truths about them - in virtue of understanding them as a part of the greater wholes of Artinian reciprocity, and the domain of abstract number fields. It is through coming to know these emergent truths that mathematicians extend their knowledge of reality. Not every generalization exhibits this feature; generalizations may exhibit only accidental unity, in virtue of being arbitrary or uninteresting. Mathematicians must search for generalizations which extend our knowledge. Thus, the group structure, and group theoretic facts about antecedently known domains, are not merely invented or stipulated; they are discovered. This element of mathematical practice, and the fact that we can succeed and fail in finding fruitful generalizations – get a generalized definition wrong in such a way that it is not fruitful, and then correct ourselves – is that which grounds mathematics in reality.

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**Works Cited**


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