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# Why sets are not collections

PREPRINT

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## Abstract

Sets are often taken to be collections, or at least akin to them. This paper argues, against this, that although we cannot be sure what sets are (and the question, perhaps, does not even make sense), what we can be entirely sure of is that they are *not* collections of any kind. The central argument will be that elementhood in a set and membership in a collection satisfy quite different axioms, and a brief logical investigation into how they are related is offered.

The latter part of the paper concerns attempts to modify the ‘sets are collections’ credo by use of idealization and abstraction, as well as the Fregean notion of sets as the extensions of concepts. These are all shown to be either unmotivated or unable to provide the desired support. We finish on a more positive note, with some ideas on what *can* be said of sets. The main thesis is that (i) sets are points in a set structure, (ii) a set structure is a model of a set theory, and (iii) *set theory* is a family of formal and informal theories, loosely defined by their axioms.

## 1 Introduction

Nowadays there seems to be widespread agreement among philosophers of mathematics that sets, as Cantor said, are collections of objects. It was not always like this: in the beginning of the 20<sup>th</sup> century there was a lively debate on what they were, and in particular whether they were collections, as Cantor said, or extensions of concepts, as Frege said. We find Carnap, in the *Aufbau*, arguing against the Cantorian position as follows:

We say of a class and of a whole that they “correspond” to one another when the parts of the whole are elements of the class. Since a whole can be divided into parts in various ways, there are always many classes which correspond to one whole. On the other hand, to each class there corresponds at most one whole, for the elements are uniquely determined through the class, and two objects which consist of the same parts are identical. Now, if a class were to consist of its elements (i.e., if it were identical with the whole that corresponds to it), then all those many classes which correspond to

the same whole would be identical with one another. But, as we have seen, they are different from one another. *Thus, classes cannot consist of elements as a whole consists of its parts.*[Carnap, 1967, p. 63, emphasis in original]

This might still not seem like a denial of Cantor’s view, since Cantor talks about *sets* and Carnap, following Frege, about *classes*. But in the next paragraph he makes sure that he intends his criticism to apply to the set concept as well:

The same holds for the mathematical concept of a *set*, which corresponds to the logical concept of a class. A set, too, does not consist of its elements. This is important to notice, since the character of a whole or a collection (or of an “aggregate”) has erroneously been connected the concept of a set ever since its inception (i.e., ever since Cantor’s definition).[Carnap, 1967, p. 63]

Gradually during the 20<sup>th</sup> century—perhaps influenced by the inconsistency of Frege’s own formalization of set theory—the support for the Cantorian position increased. Halmos, in his classic *Naive Set Theory* [1960, p. 1] begins by listing “a pack of wolves, a bunch of grapes, or a flock of pigeons” as typical examples of sets. Back then, there were still contrary voices: Max Black [1971, p. 615] retorts that, in that case, “It would then make sense, at least sometimes, to speak of being pursued by a set, or eating a set, or putting a set to flight”. But such objections were rare, and by now they have become almost extinct. Even Hallett, in his *Cantorian Set Theory and Limitation of Size* [1984], which is sharply critical of the standard ways that ZFC are motivated (i.e. the limitation in size doctrine and the iterative conception), seems to take for granted that set theory is at least *intended* to be about collections.

I will here defend the claim that, as Hallett [1984, pp. 299–305] also says, it is not at all clear at all what sets are. But I will go further, and argue that if there is one thing we *can* be rather certain about, it is that they are *not* collections. I will also go into why they are probably not extensions of concepts either, and instead try argue that, indeed, the whole question of what sets “are” may be fundamentally misposed.

## 2 Sets are not collections in the “ordinary” sense

We focus on axiomatic versions of set theory, such as ZFC and set theories resembling it. Some philosophers or mathematicians (such as Mayberry [2001]) would say that this, by itself, makes the investigation useless: ZFC is a certain formalization of set theory, but set theory itself is impossible to capture completely formally. But we will, at least to start with, only need parts of ZFC that are common among *all* set theories that have been put forward so far, from finitary ZF to second-order Z to NFU, and even non-well-founded set theories. ZFC and other axiomatizations, when we use them here, are employed because

they are well known and rigorous, and not because any one of them is taken to be the “true” theory of sets.

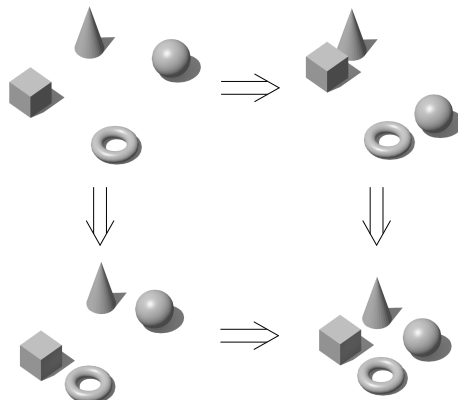
In arguing that sets are not collections, or at least not *just* collections we also need to have some kind of grip on what collections are. Some paradigmatic cases of these, which we must start with unless we want the statement ‘sets are collections’ to be as tautologous as ‘sets are sets’, are collections of ordinary physical objects. The relevant line for these in the OED reads “A number of objects collected or gathered together, viewed as a whole; a group of things collected and arranged.”

It is obvious that this notion of collection is dependent on the *action* of collecting: a collection is what you get when you collect things. In the set-theoretic case, this action is not supposed to involve anything above the action, if any, of bringing the elements of the set in question into existence.

A set cannot be *this* kind of collection. Consider the pairing axiom:

$$\forall a \forall b \exists c \forall x (x \in c \leftrightarrow x = a \vee x = b)$$

Informally, this says that given two objects  $a, b$ , we can form a set which contains those objects *and nothing else*. But take four physical objects, which we will call *cube*, *cone*, *sphere* and *torus*, collect them into pairs in two different ways, and then collect the collections:



What you get is not a collection with two things in it, as the pairing axiom says, but a collection with four things. If you are *just* considering the sets as collections, there is no difference between  $\{\{a, b\}, \{c, d\}\}$  and  $\{a, b, c, d\}$ . In mathematical terms, collecting is an associative operation. It does not matter if you go by the upper-right or the lower-left path in the above figure; the result is the same anyway. So if collecting things was the same thing as putting them in a set, we would have had

$$\{\{cube, cone\}, \{torus, sphere\}\} = \{\{cone, sphere\}, \{cube, torus\}\}$$

which we do not.

But, the set theorist may protest, we have to take seriously the fact that collections are *themselves* objects, and that, by just collapsing everything to a single level, we make an ontological error. I will not dwell on whether this is the case, since the question is, indeed, ontological. In one sense, collections exist, since we can quantify over them, and they are not identical to the plurality of their members, since they have different properties (the collection is *one* while the plurality is *many*, as Russell would have said). In another sense, it seems at least sensible to hold that when I buy a six-pack of beer, I do not thereby buy an inaccessible cardinal number of objects (i.e. the whole set-theoretical hierarchy generated from them). But the concept of *object* is itself notoriously vague, so any interpretation may be as defensible as another.

What we can show, however, is that taking collections to be objects in their own right still does not make them sets. Suppose that we, again, collect the collections  $\{cube, cone\}$  and  $\{torus, sphere\}$ , as through the right-hand arrow of the figure. What objects will be in this total collection  $C$ ? We will have that  $\{cube, cone\} \in C$  and  $\{torus, sphere\} \in C$ , of course. But we *also* still have  $cube \in C$ ,  $cone \in C$ ,  $torus \in C$ , and  $sphere \in C$ ; you cannot have a collection of collections without also having the objects in said collections. Indeed, if we take collections to be objects, we will, in collecting  $\{cube, cone\}$  and  $\{torus, sphere\}$ , end up with a collection containing *all* subcollections of  $\{cube, cone, torus, sphere\}$ . So taking collections to be objects does not help. In fact, in one sense it makes the difference between sets and collections even greater, since the cardinalities of a set of sets and of the corresponding collection of collections will differ even more.

This argument can be made equally well using singletons, and in one sense it then pinpoints the unreasonableness of holding sets to be collections even better than. Suppose we have a thing  $x$ , which may also itself be a set. Why, in collecting it, would we end up with something *different* from what we collected?<sup>1</sup>

Now, it is not clear that the above argument would be applicable to Cantor's own understanding of sets. In fact, Cantor does not form sets of sets at all: he always associates sets with other objects, such as cardinals, functions or relations, which are different kinds of concepts for him, and then forms sets of these concepts (see, for example, [1962, pp. 279–280, 287–290]). He is not very careful to distinguish between set membership and subthood, and often uses 'enhalten' vaguely (as in [1962, p. 309]), relying on context to sort things out. Not even when introducing the finite cardinals does he avail himself of sets containing sets, as is usually done now. In this he is joined by another early set theorist, Dedekind, who (in)famously made an argument in terms of the possibility of forming certain thoughts in his attempt to prove the existence of simply infinite systems [1963, §66].

Sets of sets started to become more universally employed with the axiomatizations of set theory. The creators of these, like Zermelo, did not presuppose

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<sup>1</sup>Note that this has nothing to do with whether a single thing can really be called a "collection". I find no objection to letting the technical notion of 'set' admit these, as well as an empty set. The problem is not with existence, but with existence *over and above* what is in the set.

that sets “are” anything, however. It may therefore very well be that the current confusion regarding sets may stem from an inadmissible combination of the earlier Cantorian interpreted concept with the deductive power given by the later axiomatic version.

### 3 What *is* a pure collection, anyway?

We have so far given an informal argument that sets are not collections in the ordinary sense. To attain more clarity, and also to be able to better compare sets with collections, it is useful to formalize.

As a framework for building a theory of collections we will, following Boolos [1984], start with monadic second-order logic, with the second-order variables interpreted as ranging over pluralities. In particular, let  $\mathcal{L}^1$  be a first-order language with or without identity. Extend  $\mathcal{L}^1$  to a language  $\mathcal{L}$  by adding the following:

1. Monadic second-order variables  $X, Y, Z, W, \dots$  and second-order quantifiers  $\forall^2$  and  $\exists^2$ . We will generally suppress the exponents in these, and rely on the variables to separate first- from second-order quantification.
2. A lambda operator  $\lambda x$  for making predicates from formulae, governed by the axiom schema  $\vdash (\lambda x \varphi(x))(y) \leftrightarrow \varphi(y)$ , where  $\varphi(x)$  is any formula with  $x$  free.

For convenience, we introduce the following defined terms, where  $s, t$  are taken to be first-order terms, and  $S, T$  second-order.

1.  $s = t \equiv_{df}. \forall X (X(s) \rightarrow X(t))$
2.  $S \leq T \equiv_{df}. \forall x (S(x) \rightarrow T(x))$ .
3.  $S = T \equiv_{df}. (S \leq T \wedge T \leq S)$ .
4.  $S \vee T =_{df}. \lambda x (S(x) \vee T(x))$ .
5.  $S \wedge T =_{df}. \lambda x (S(x) \wedge T(x))$ .

Since our purpose is to use this logic to frame theories, we have not explicitly attempted to minimize the number of primitives, but instead focused on including what we need in as natural a form as possible. Our theory of collections will follow the interpretation of collections as abstracted from the act of collecting. We therefore expand  $\mathcal{L}$  to a language  $\mathcal{L}_c$  by adding an operator  $\mathfrak{c}$ , taking pluralities to objects, with the intended interpretation  $c = \mathfrak{c}X$  iff  $c$  is the result of collecting the  $X$ 's. For simplicity, we will assume  $\mathfrak{c}$  to be defined for all pluralities, including the empty one, for which we call its value 0. It would be a fairly easy task to exclude the empty collection if we wanted to. For now, we assume it as an ideal element, much as zero is usually assumed as a natural number. We call 0 the *empty* collection, and any non-empty object

that contains nothing but itself and 0 a *trivial* collection, or a *thing*. We use the word *object* for anything in our domain, although, as it will turn out, this will also coincide with *collection*.<sup>2</sup>

Where  $t_1, \dots, t_n$  are first-order terms, write  $[t_1, \dots, t_n]$  for the plurality  $\lambda x (x = t_1 \vee \dots \vee x = t_n)$ . Write  $x \varepsilon y$  iff  $\mathfrak{c}[x, y] = y$ , and  $x \not\varepsilon y$  otherwise. When  $x \varepsilon y$ , we say that  $x$  is *in*  $y$ , or that  $y$  *contains*  $x$ . We write  $\downarrow b$  for the plurality of objects that are in  $b$ , i.e.  $\downarrow b =_{df} \lambda x x \varepsilon b$ . If  $x \varepsilon z$ , and for every  $y$  such that  $x \varepsilon y$  and  $y \varepsilon z$  we have either that  $y = x$  or that  $y = z$ , we say that  $z$  is a *minimal* proper container of  $y$ , and write this  $x \prec z$

As we highlighted in the previous section, what is characteristic for collections is that (i) collecting something with some of the objects in it makes no difference, and that (ii) the order one collects objects in makes no difference. Furthermore collections, like Cantor's sets, are collections of *well-individuated objects* rather than a chunks of *stuff*, which means that if  $a$  is in  $b$ , then we must be able to obtain  $b$  by adding (potentially infinitely many) individual objects, i.e. enclosing  $a$  in a sufficient number of minimal proper containers. Our three axioms for the theory of collections can therefore be taken to be

**Axiom 1.**  $\forall X \forall x (X \leq \downarrow x \rightarrow \mathfrak{c}(X \vee [x]) = x)$ .

**Axiom 2.**  $\forall X \forall Y \forall Z \forall W \mathfrak{c}[\mathfrak{c}(X \vee Y), \mathfrak{c}(Z \vee W)] = \mathfrak{c}[\mathfrak{c}(X \vee W), \mathfrak{c}(Z \vee Y)]$ .

**Axiom 3.**  $\forall x \forall z ((x \varepsilon z \wedge x \neq z) \rightarrow \exists y (x \prec y \wedge y \varepsilon z))$ .

From the first two a number of useful properties follow:

**Lemma 1.**  $\mathfrak{c}$  and  $\varepsilon$  satisfy the following:

1.  $\mathfrak{c}[x] = x$ .
2. If  $X \leq Y$  then  $\mathfrak{c}(X) \varepsilon \mathfrak{c}(Y)$
3. If  $x \varepsilon y$  and  $y \varepsilon x$  then  $x = y$
4. If  $x \varepsilon y$  and  $y \varepsilon z$  then  $x \varepsilon z$ .
5.  $\mathfrak{c}(X \vee Y) = \mathfrak{c}[\mathfrak{c}(X), \mathfrak{c}(Y)]$ .
6. If  $X(x)$ , then  $x \varepsilon \mathfrak{c}(X)$ .
7. If each  $x$  such that  $X(x)$  is in  $y$ , then  $\mathfrak{c}(X) \varepsilon y$ .

*Proof.* Let  $\Lambda = \lambda x x \neq x$ .

1. By taking  $X = \Lambda$  and applying axiom 1.
2. What we wish to show is that  $\mathfrak{c}[\mathfrak{c}(Y)] = \mathfrak{c}[\mathfrak{c}(X), \mathfrak{c}(Y)]$ . So:

$$\begin{aligned} \mathfrak{c}[\mathfrak{c}(Y)] &= \mathfrak{c}[\mathfrak{c}(Y), \mathfrak{c}(Y)] = \mathfrak{c}[\mathfrak{c}(X \vee Y), \mathfrak{c}(Y \vee \Lambda)] \\ &= \mathfrak{c}[\mathfrak{c}(X \vee \Lambda), \mathfrak{c}(Y \vee X)] = \mathfrak{c}[\mathfrak{c}(X), \mathfrak{c}(X \vee Y)] \end{aligned}$$

<sup>2</sup>Anyone balking at the notion of a one-object or no-object collection is welcome to interpret our use of the word *collection* as short for *collection or thing*.

3. Assume that  $\mathbf{c}[a] = \mathbf{c}[a, b]$  and  $\mathbf{c}[b] = \mathbf{c}[b, a]$ . Then  $a = \mathbf{c}[a] = \mathbf{c}[a, b] = \mathbf{c}[b, a] = \mathbf{c}[b] = b$ .

4. By 1., it is sufficient to show that  $\mathbf{c}(X) \varepsilon \mathbf{c}(Y) \wedge \mathbf{c}(Y) \varepsilon \mathbf{c}(Z) \rightarrow \mathbf{c}(X) \varepsilon \mathbf{c}(Z)$  for all pluralities  $X, Y, Z$ . Written out, the therefore we need to show that  $\mathbf{c}(Z) = \mathbf{c}(X \vee Z)$  from  $\mathbf{c}(Y) = \mathbf{c}(X \vee Y)$  and  $\mathbf{c}(Z) = \mathbf{c}(Y \vee Z)$ . The proof proceeds as follows.

$$\begin{aligned} \mathbf{c}[\mathbf{c}(Z)] &= \mathbf{c}[\mathbf{c}(Z), \mathbf{c}(Z)] = \mathbf{c}[\mathbf{c}(Z \vee Y), \mathbf{c}(Z \vee Y)] \\ &= \mathbf{c}[\mathbf{c}(Z \vee Z), \mathbf{c}(Y \vee Y)] = \mathbf{c}[\mathbf{c}(Z \vee Z), \mathbf{c}(Y \vee Y)] = \mathbf{c}[\mathbf{c}(Z \vee Z), \mathbf{c}(Y)] \\ &= \mathbf{c}[\mathbf{c}(Z \vee Z), \mathbf{c}(Y \vee X)] = \mathbf{c}[\mathbf{c}(X \vee Z), \mathbf{c}(Y \vee Z)] = \mathbf{c}[\mathbf{c}(X \vee Z), \mathbf{c}(Z)] \end{aligned}$$

This is, by definition, equivalent to  $\mathbf{c}(X \vee Z) \varepsilon \mathbf{c}(X)$ . From 2. we have that  $\mathbf{c}(X) \varepsilon (X \vee Z)$ , and  $\mathbf{c}(X) = \mathbf{c}(X \vee Z)$  follows by 3.

5.  $\mathbf{c}(X \vee Y) = \mathbf{c}[\mathbf{c}(X \vee Y)] = \mathbf{c}[\mathbf{c}(X \vee Y), \mathbf{c}(X \vee Y)] = \mathbf{c}[\mathbf{c}(X \vee X), \mathbf{c}(Y \vee Y)] = \mathbf{c}[\mathbf{c}(X), \mathbf{c}(Y)]$ .

6. We need to show that  $\mathbf{c}[\mathbf{c}(X)] = \mathbf{c}[\mathbf{c}(X), x]$ . But  $X = X \vee [x]$ , so  $\mathbf{c}[\mathbf{c}(X)] = \mathbf{c}(X) = \mathbf{c}(X \vee [x]) = \mathbf{c}(\mathbf{c}(X), \mathbf{c}[x])$ , by axiom 2.

7. Written out, this says that  $X \leq \downarrow y \rightarrow \mathbf{c}(X) \varepsilon y$ . But this follows directly from axiom 1 together with  $y \varepsilon y$ . □

These together entail that  $\varepsilon$  is a partial order on the domain  $D$  and that  $\mathbf{c}(X)$  is the supremum of the objects in the plurality  $X$ . Furthermore, adding axiom 3 entails that  $D, \varepsilon$  is fully characterized. Since the proof is fairly lengthy, we have moved it to an appendix.

**Theorem 1.**  *$D, \varepsilon$  is a complete atomic Boolean algebra.*

This theorem completely describes what the theory of collections is: it is the theory of complete atomic Boolean algebras. One example of such an algebra is classical mereology, at least if we add a zero object. Thus collections are intimately related to the kind of *wholes* studied in this theory. Theorem 1 gives an axiomatic, structural view of what a theory of collections is, rather than a substantial one, which holds such theories to be individuated by what they are “about” rather than their axioms. It is useful to draw a parallel to Hilbert’s characterization of geometry as consisting of the study of axiomatic systems similar to the Euclidean one, rather than intrinsically being about spatial intuition or physical space.

The structure of collections implies at once that being in a collection is not like being an element of a set. The first is transitive, while the second is not. A collection of one object is equal to its content, while a singleton set is distinct from its element. And collections form a Boolean algebra, while sets do not, at least in ZFC.



## 4 Abstracts, ideals and concepts

We have investigated the relation between set theory and collection theory. Collection theory is interpreted as akin to mereology, and it is well known that mereology is not set theory—if anything, it was this realization in Peano and others, against the algebraic tradition, that made most of our current applications of set theory possible (cf. Grattan-Guinness [2000, chs. 3 & 5]). Thus I do not, in any way, mean to suggest that the theory of collections would be a better foundation for mathematics than set theory, or even a possible one. My point is just that as long as we want set theory to be powerful enough to base substantial portions of mathematics on, we cannot take it to be about collections.

There are several possible rejoinders to this argument. One could claim, for instance, that what holds of collections of concrete, physical objects does not hold for collections of *abstract* objects, and that, since set theory is supposed to be about *all* collections, we can rely only on properties that hold for both abstract and concrete collections. But this cuts two ways: it is not clear, for instance, that there is an infinite collection of physical objects, so perhaps we should not have the axiom of infinity? One could, of course, take the position that set theory is about collections of abstract objects *only*, but that would mean that we make set theory inapplicable to concrete collections. It would also mean that the word ‘collection’, as used in “sets are collections of objects”, would be separated from any part of its usual meaning.

A slightly different approach is to apply ‘abstract’ to the word ‘collection’, rather than the word ‘object’. This may be motivated by the fact that, in order to form a set, we do not need to actually collect anything in the physical sense. But why would abstracting away the actual, physical act of collecting give rise to something that has *stronger* identity conditions than what we started with? Indeed, how could it? Having a domain of entities and abstracting away some of their qualities makes *more* entities count as identical rather than less. But the reason why a set is not a collection is exactly that sets have stronger identity conditions than collections: a collection is determined just by what *urelemente* are in it, while the determination of a set also requires us to know how these *urelemente* have been collected. As Goodman [1958] would have put it, collections are *hyperextensional*, while sets are merely extensional.

Similar objections can be raised against the claim that a set is an *idealization* of a collection. The universe of sets has much more structure than the universe of collections, which is really just a complete atomic Boolean algebra. But we idealize to *disregard* complications, and not to introduce them. Holding sets to be idealized collections would be a bit like holding quantum mechanics to be an idealization of Newtonian mechanics. Even if we just hold *some* sets to be idealizations, and the rest to be collections, the idealizations will outnumber the basis of collections that we are supposed to have idealized from momentarily.

Against this, one could perhaps claim that while set theory is not an idealization in the sense that empirical theories are, the sets themselves may be taken to be *ideal elements*. This is consistent with there being many more of them than the objects we idealized from; the same holds for Dedekind’s introduction

of real numbers, for instance. Indeed, in modern terminology, his procedure just consists in going from an ordered set to the ordered set of *ideals* of that set. But given that we take the rational (or, equivalently, natural) numbers to be urelemente, as Dedekind certainly did, the resulting reals are still hyperextensional, unlike sets: every real number is uniquely determined by which rationals are elements of them. Real numbers *can* thus be viewed as collections of rational numbers, which means that this kind of introduction of ideal objects does not support the introduction of a full set-theoretic hierarchy.

Summing up, this all points to the lesson that, strictly speaking, a set cannot be *just* a collection, but has to be more like a collection with a certain structure to it. As Boolos [1971] mentions, Kripke used the picture of throwing a lasso around a collection of objects to make a set out of them. A picture I have sometimes found helpful is to say that making a set out of  $X$ 's is like a throwing them in a bag.<sup>3</sup> In the same vein, Lawvere and Rosebrugh [2003, p. 1] write “It has been said that a set is like a mental ‘bag of dots’, except of course that the bag has no shape [...]”. Enderton [1977, p. 3] writes “The fact that  $\{\emptyset\} \neq \emptyset$  is reflected in the fact that a man with an empty container is better off than a man with nothing—at least he has the container”, thereby implicitly adding the container to his description, two pages earlier, of a set as “a collection of things (called its *members* or *elements*), the collection being regarded as a single object”. He also provides no explanation of how someone might be in possession of nothing at all, without also having the container which necessarily contains this nothing.

These images are, however, just that—images. No mathematician believes that all things *actually* have lassos around them, or are inside an infinity of bags. Likewise, the “mental” part of it, as found in Lawvere, should probably not be taken literally either. Sets are very unlikely to be mental objects: ZFC simply postulates too many of them for that to be plausible. So, no matter if these pictures help or not, they are not *accurate*. Perhaps they can be taken to give intuitive support to our belief that ZFC is consistent, but they give us no reason at all to believe it to be true.<sup>4</sup>

Similar objections pertain to the so-called iterative conception of sets [Boolos, 1971, Schoenfield, 1977], which is sometimes seen as providing a rationale for the axioms of ZFC. The fundamental problem is that the iterative conception does treat sets as collections, which, as we have seen, they are not. Perhaps we could hold that the iterative process itself makes sets *more* than collection (e.g. by individuating them by how they are made, and not just by what is in them), but this means that sets are not, after all, just collections, but collections with a genealogy. In that sense, they would be more like *species* in the evolutionary sense, but I know of no philosopher of mathematics who has advocated such a view.

This still does not even touch on the iterative conception's main problem:

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<sup>3</sup>Emmy Noether described Dedekind as also seeing a set as a “bag full of objects” [Hallett, 1984, p. 158]. The picture seems hard to avoid.

<sup>4</sup>Of course, intuition does not have a good track record when it comes to judging the consistency of set theories either, so the value of any intuitive support may be questionable.

its lack of any independent reason for us to believe in it. Again, we are dealing with images that are designed to make it *intelligible* what sets are, at the cost of making belief in them almost impossible. If sets truly were bags, we could of perhaps picture what it means for every collection of objects to be put in a bag of its own (at least if bags were allowed to intersect). But we can see that not everything is in a bag of its own, so why believe that everything is in a set of its own, which is, after all, a kind of abstract bag? Likewise, we can convince ourselves that we understand what it means to carry out the operations that are used to create the iterative hierarchy (hint: it will take a lot of abstract bags and a lot of time). But we have no evidence that anyone has actually done this, so there is no reason to think that the iterative hierarchy describes anything existing.

The iterative hierarchy and the Cantorian interpretation of set are intertwined. Thinking of sets as abstract bags makes them a bit more intelligible, and the iterative hierarchy then seems like it would help provide a picture of the bag universe. Conversely, understanding the iterative conception helps one understand what a set is: it is one of the abstract bags into which we put objects while carrying out the transfinite induction of that conception. But our gain in understanding is offset by the increase in implausibility of the view: why believe in all those bags? Or is set theory purely a game of make-believe, as Field [1980] held mathematics as a whole to be?<sup>5</sup>

Given the problems with interpreting sets as collections, or as something like collections (such as abstract bags), the Fregean interpretation may actually seem more attractive. In contrast to actual collections, it introduces a hierarchy of concepts. It does, of course, depend on a Platonist ontology in order to avoid psychologism, but since the objects in question are logically determined rather than given through an iterative process, their existence comes with a kind of built-in motivation. As Frege saw it when writing the *Grundgesetze*, the theory of sets is part of logic, and to deny it is to contradict yourself.

The problem, of course, is that at least for the *Grundgesetze* version, affirming the theory of sets *also* means contradicting yourself. Indeed, one of the more common reasons given for why the Fregean interpretation is wrong is Russell's paradox, but that paradox does not by itself exclude the possibility that sets are the extensions of concepts. It is perfectly possible to hold that the predicates  $x \notin x$  or  $x = x$  do not express concepts, just as it is possible to hold that they do not determine sets. We may specifically want to do so, at least in the first case, if we wish to avoid the version of Russell's paradox engendered by the concept of non-self-instantiation.

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<sup>5</sup>One could make the classical comparison with theology here. Consider a theory that assumes that every existing object, including not-too-large collections, is overseen by an individual guardian angel. But the angels themselves, of course, have to be overseen as well, as do the collections of them. We therefore get a hierarchy mirroring the set-theoretical one, with 'angel  $b$  oversees  $a$ ' corresponding exactly to ' $b$  is the singleton set of  $a$ '. This theory may or may not be intelligible, and since it mirrors set theory, it can be used as a foundation of mathematics. The problem is that we have no reason to take it to be true. It may be appropriate to quote one of David Lewis's exclamations here: "Must set theory rest on theology? — Cantor thought so!" Lewis [1993]

More serious for our purposes is the fact that the Fregean interpretation has trouble motivating the powerset axiom: why would every subset of a set have a concept it is the extension of? All explanations of why this would hold seem to be based on the combinatorics of collections, rather than on what concepts we can construct or define. Perhaps it can be worked around using some kind of haecceitistic concepts (e.g. a specific concept tied to every object), but it is hard to see how it could ever be made natural.<sup>6</sup>

The Fregean conception, when we try to work it out, also tends to make extensionality a bit more complicated than we want. Extensions arise as abstractions from concepts for which, being unsaturated, questions of identity do not arise at all. For the logic of the concepts themselves, which on the Fregean view are more primitive, classical set theory may very well not be the most fitting formalization. Instead, we will probably be better off looking at some version of intensional type theory.

These problems for the Fregean interpretation all arise in plain Zermelo set theory. When we go to ZFC they only get worse. Neither the axiom of foundation, nor that of separation, nor even choice, seem possible to motivate if we take sets to be extensions of concepts. It is also the case that even if these issues could be met, we would not really have gained much in the way of insight into what sets are. Although Frege, infamously, did write “I assume that it is known what the extension of a concept is” [1953, §68], it turned out that the notion was far more prone to contradictions than he had assumed at that time. And even if we are only asking for some kind of intuitive understanding, answering what an extension is without taking recourse to the notion of set (as in ‘the extension of  $\varphi$  is the set of objects  $x$  such that  $\varphi(x)$ ’) seems more or less impossible.<sup>7</sup>

So while the Fregean interpretation is perhaps not as definitely wrong as the Cantorian one, it has severe problems, and thus we have no plausible interpretation of what the word ‘set’, as it is used in mathematics, actually refers to. But how could mathematics work at all if we do not have that? It may seem almost paradoxical that mathematics could work so well, and mathematicians understand at least parts of it so well, without knowing anything about what the word ‘set’ refers to.

## 5 Sets as points in a set structure

The proper interpretation of this situation, I believe, is to accept that the whole project of trying to explain what sets are without presupposing a specific axiomatization is fundamentally misplaced. Set theory has evolved gradually, and only received its current form in the mid 20<sup>th</sup> century. It is sometimes treated as

<sup>6</sup>Parsons gives a similar argument in [1974]

<sup>7</sup>The suggestion that we, perhaps, could identify the extension of  $\varphi$  with the *collection* of things  $x$  such that  $\varphi(x)$  fails for the same reason as the one that we can identify sets with such collections: we can have that such collections have the same constituents, even though the concepts we have defined them through have different extensions.

having sprung fully-armed from Cantor’s mind, but of course that is not what happened. Cantor drew on earlier mathematics, and indeed much of what we nowadays call set theory does not come from Cantor at all but from Peano, Frege, Russell, Zermelo, Skolem, von Neumann, and even to some part from algebraists like Leibniz, Boole and Schröder. Especially in the 30’s and 40’s, the two main traditions—the Cantorian and the Fregean—began converging, with logicians like Gödel and von Neumann applying formal logic to set theory, and set theory to formal logic [Ferreirós, 2007, esp. ch. 11].

We have no reason to believe that these founders of ZFC in all cases relied on the same philosophical motivations, and in some cases a philosophical motivation was most likely lacking altogether. One of the main reasons for many mathematicians’ acceptance of the axiom of choice was arguably its *usefulness*, quite separate from its truth or falsity. Given all this, we should not be surprised that set theory more resembles the body of laws of a country, arrived at through debate and compromises, than an instrument engineered with a single purpose in mind.

Since set theory has developed independently of a specific conception of set, we should not expect any specific conception to be able to answer the question of what a set is. Instead of starting with the question “what is a set?”, we should start with “what is set theory?”. This question can, at least partially, be answered by pointing to actual texts of set theory: works of Cantor, Zermelo and Fraenkel, or some standard textbook such as Kunen [1980] or Jech [2006]. While this may not gain us insight into what the *essence* of set theory is, it is not clear what such insight would be. As we already mentioned, set theory has grown to its current form organically rather than guided by a single philosophical vision. Until the middle of the 20th century the subject was sometimes not even separated from topology, as witnessed in Hausdorff’s classic *Grundzüge* [1914]. Contemporary set theory has inseparable ties to category theory, logic, combinatorics, measure theory and model theory, and all of these influence what it is. The multitude of theories that can rightly be called *set theories* make up a Wittgensteinian family rather than a clear-cut logical class definable by sufficient and necessary conditions. This means that the most perspicuous way to specify what set theory is will be to give examples of set theories: ZFC, NBG, NF, ZFA, etc. A set theory is any theory that is similar enough to any of these. Just as we held about the theory of collections, this puts set theory in the same boat as Hilbertian geometry: a subject defined by a certain collection of axioms, rather than some kind of pre-theoretical notion of what it is about. This also ties in with one reason why set theory is not collection theory: the axioms for Boolean algebras, which define collections, differ radically from the kind of axioms that define a set theory.

Assuming some kind of loose grasp on what a set theory is, a *set* can be said to be whatever is taken as a value by the first-order variables in such a theory. In structuralist words, it is a *point in a set-structure*, and a set-structure is a kind of binary relation. It is therefore, as for example Voevodsky [2006] has recently noted, somewhat misleading to say that ZF is about a sort of things called *sets*. Although the statement may be interpreted in a way that makes it true, it gets

the logical order backwards since it is the sets that get the explanation of what they are from the axioms of the set theory, rather than the axioms of the set theory which get their explanation from intuitions about sets. Less misleading would be to say that set theory is about membership structures, or perhaps, as Voevodsky does, that it is about a kind of trees without symmetries. A particularly tautologous (but also particularly truthful) way to say this is that *set theory is about models of set theory*.

Such models come in many shapes and sizes. Some of them are elements in other models of set theory, although none of them are elements in themselves. But there are also models that are not sets at all, such as ones in toposes or proper classes. For first-order set theory, there are models which are also models of PA, and models which are relation algebras, such as [Tarski and Givant, 1987]. For higher-order set theory we presumably want only models in which  $\in$  is interpreted as *membership*, but what can that mean if we, as I have argued here, simply do not (and perhaps cannot) know what kind of relation the word ‘membership’ refers to, independently of the axioms we have taken to govern it?

Skolem spoke of the *relativity of set-theoretic notions*: that axiomatic set theory can never fully specify the references of its terms [1922, p. 296]. But axiomatic set theory, as Skolem correctly noted, is the only coherent set theory there is. For Skolem, the reason for this was the antinomies. I have indicated in this paper why, even without these, the informal notion is unworkable: there simply *is* no informal notion of set that describes anything close enough to the axiomatic version, which means that axiomatic set theory, with its inherent relativity, is all we have.

In the current literature it is often assumed that the sole reason for believing in Skolem-style relativity is the Löwenheim-Skolem theorem, and that for logics where this theorem does not go through this relativity does not arise. But there remains a form of relativity even in higher-order formalizations of set theory. Assume second-order ZF, with the standard semantics so that the second-order variables range over *all* subsets of the domain. Which these are will, however, depend on what kind of set theory we have in our metalanguage. Does the metalanguage’s  $\omega$  include any Cohen reals as subsets, for instance? Many set theorists likely do not use a metalanguage in which it does, but most probably do not even consider the question, or explicitly treat an answer as an extra assumption, as is shown in the habit of explicitly calling attention to uses of the continuum hypothesis in a proof.

Furthermore, even if we can give explicit characterizations about our use of mathematical language, as we do when we say ‘assume the continuum hypothesis’, an infinite amount of underdetermination will always remain. The verbal (or written) behavior of mathematicians is just not sufficient for us to be able to say which specific language they use, so we just have to accept that no matter what logic we use, there will always be relativity.

Axiomatization provides ways for us to somewhat lessen this relativity. To be sure, it is always done against a background: a logical calculus used in derivations, or, in our case, a metalanguage used for semantics. But, at least as long as we cannot unequivocally *point* to sets and non-sets, or otherwise non-

linguistically indicate them, axiomatization remains our best way to become clearer about what we refer to.

Similar views were held by several of the early 20<sup>th</sup> century axiomatists. For instance, we find von Neumann [1925] accepting classical logic as a background, but arguing for axiomatization as follows:

The methods of logic are not criticized to any extent, but are retained; only the (no doubt useless) naive notion of set is prohibited. To replace this notion the axiomatic method is employed; that is, one formulates a number of postulates in which, to be sure, the word “set” occurs but without any meaning. Here (in the spirit of the axiomatic method) one understands by “set” nothing but an object of which one knows no more and wants to know no more than what follows about it from the postulates.

It is easy to dismiss the views of von Neumann, and of Hilbert, for whom he worked as an assistant, as expressions of naive formalism. But, as has been pointed out several times in later years, it is equally reasonable to see them as advocating a form of structuralism; Hilbert, after all, replied to Frege’s insistence on proving existence theorems before making definitions that “if the arbitrarily posited axioms together with all their consequences do not contradict one another, then they are true and the things defined by the axioms exist” [Frege, 1971, p. 12]. Rather than being meaningless, or denotationless, as the strict formalist would have it, the terms in an axiomatic theory get their denotation and meaning from the axioms.

The difference may, perhaps, be one of perspective. Bourbaki, focusing somewhat more on the *texts* of mathematics, are closer to the formalist interpretation than Hilbert or von Neumann, although even they call their method *structuralist* rather than formalist. In the end, however, the approaches are interrelated: from consistent axioms we can define a non-empty class of structures satisfying those axioms. Conversely, any structure can be described, at least partially, by giving the axioms that hold in it. One could therefore say that *formalism is the axiomatic method as seen through the lens of syntax*, and *structuralism the axiomatic method as seen through the lens of semantics*. They are fundamentally equivalent.

Since our question (to what does the term ‘set’ refer?) is semantic, interpreting set theory in structuralist terms allows us to give the most appropriate answer, namely that a set is a point in a membership structure. *Set theory* is the theory of membership structures, and it is this theory that determines what sets are. But, as Skolem noted, it can never do this completely; there will always remain some relativity of the terms to the structure itself, rather than to the theory.

Characterizing set theory as being primarily about membership structures and only secondarily about sets also brings it more in line with the rest of mathematics. Group theory is about *groups*, and only secondarily about the elements of those groups. Or, if it is thought that the comparison with group

theory is inadmissible because this theory is not categorical, consider the theory of vector spaces. All vector spaces with the same dimension are isomorphic, so the theory is categorical up to a cardinal number parameter. This is exactly the same situation as in second-order ZFC, since in that theory all models having the same cardinality are isomorphic as well. For a vector space, the parameter is the dimension, while for ZFC<sup>2</sup> it is the cardinality itself, but the amount of determination or underdetermination is the same. Just as the points in a vector space are parasitic on the space itself—they get their identities only from the relations they stand in to other points—we should recognize that the sets in a set theory are secondary to the set-theoretic structure itself.

I actually believe this comes rather close to how at least some set theorists seem to think about and work with their subject, although not always how they talk about it. Jech [2006, p. 3] starts out by noting that, “[i]ntuitively, a set is a collection of all elements that satisfy a certain given property”, thus paying his respects to both Frege and Cantor. But, because of Russell’s paradox, he continues, this naive interpretation cannot be taken to be literally true. The way out is to go axiomatic: “[t]he axioms of ZFC are generally accepted as a correct formalization of those principles that mathematicians apply when dealing with sets.” [2006, p. 4]. Nowhere is anything said about whether the axioms in question are correct for dealing with *sets themselves*. Instead, as an axiomatic theory, it gives a rigorous version of an informal mathematical practice. Likewise, while the authors of the popular logic textbook *Language Proof and Logic* give the usual naive conception of set as “a collection of things, like a set of chairs, a set of dominoes, or a set of numbers”, they also say that “[p]ersonally, we think of sets as being a lot like Tinkertoys or Lego blocks: basic kits out of which we can construct models of practically everything” [Barker-Plummer et al., 2012, p. 413–414]. The second of these is obviously a characterization of sets through the roles they play in mathematical or logical practice, rather than through some kind of metaphysical pseudodefinition like the one Cantor gave.

Practices are neither true nor false. While a formalization may capture a practice better or worse, the vagueness of the practice itself usually means that, in many cases, there is simply no fact of the matter as to whether it is *correct* or not. Perhaps we should rather see ZFC as a kind of *explication* of set theory, or of the concepts of set and membership as they are used by mathematicians. Such explications are, as Carnap [1950] famously held, not only to be judged according to their faithfulness to intuition, but also according to criteria like exactness and usefulness. It is undeniable that ZFC fulfills these admirably, and I believe that is all we should ask of it.

## 6 Appendix: derivation of theorem 1

Let the *infimum* of a plurality  $X$  be the object

$$\inf X =_{df.} \mathfrak{c}(\lambda y \forall x (X(x) \rightarrow x \varepsilon y)).$$

We will need the following lemma:



**Lemma 2.**  $D, \varepsilon$  is lower continuous, i.e. for every chain  $C \subseteq D$  and every element  $a$ ,  $a + \inf C = \inf \lambda x (x = a \vee C(x))$ .

*Proof.*

$$\begin{aligned}
a + \inf C &= \mathbf{c}([a] \vee \lambda y \forall x (C(x) \rightarrow x \varepsilon y)) \\
&= \mathbf{c}(\lambda y (y = a \vee \forall x (C(x) \rightarrow x \varepsilon y))) \\
&= \mathbf{c}(\lambda y (\forall x ((x = a \vee C(x)) \rightarrow x \varepsilon y))) \\
&= \inf \lambda x (x = a \vee C(x))
\end{aligned}$$

□

Let the *pseudocomplement* of an object  $x$  be defined as

$$x' =_{df.} \mathbf{c}(\lambda x \forall y \forall z ((z \varepsilon x \wedge z \varepsilon y) \rightarrow z = 0))$$

It is easily shown that the only object both in  $a$  and its pseudocomplement is the empty collection 0.

**Lemma 3.** The pseudocomplement  $a'$  of a trivial collection  $a$  is a complement, i.e.  $a + a' = 1$ .

*Proof.* Let  $a$  be a trivial collection. Since the only objects in a trivial collection are that collection itself and the empty collection, the definition of pseudocomplement reduces to

$$\begin{aligned}
a' &=_{df.} \mathbf{c}(\lambda x (x \neq a \wedge x \neq 0)) \\
&= \mathbf{c}(\lambda x x \neq a)
\end{aligned}$$

From this it follows that  $a + a' = \mathbf{c}(\lambda x x = a \vee x \neq a) = 1$ . □

An object  $a$  is *join-irreducible* iff  $\forall x \forall y (a = x + y \rightarrow (a = x \vee a = y))$ .

**Lemma 4.** An element  $a$  is join-irreducible iff it is a trivial collection or the empty collection.

We have omitted the proof since it is fairly trivial. Let  $T$  be the plurality of trivial collections in  $D$ , and let  $T_x =_{df.} \lambda y (y \varepsilon x \wedge T(y))$ . Together, the lemmas give us:

**Lemma 5.**  $D, \varepsilon$  is atomistic, i.e.  $x = \mathbf{c}(T_x)$  for all  $x$ .

*Proof.* A lattice is *strong* iff for each nonzero join-irreducible element  $b$  and each pair of elements  $a, c$ ,  $a < b \varepsilon a + c \rightarrow b \varepsilon c$ . This is trivially satisfied in our case since there are no non-zero objects in any trivial collection  $b$  that are not themselves not  $b$ . By axiom 3,  $D, \varepsilon$  is strongly atomic. Together with the existence of complements of trivial collections, a theorem of Walendziak Walendziak [1994] then entails that  $D, \varepsilon$  is atomistic. □

In an atomistic lattice, the pseudocomplement  $a'$  can be characterized in a much simpler way as

$$x' = \mathbf{c} \lambda y (T(y) \wedge y \not\leq x)$$

**Lemma 6.** *Every object has a unique complement, which coincides with its pseudocomplement.*

*Proof.* We first show that  $x + x' = 1$ :

$$\begin{aligned} x + x' &= \mathbf{c}(\lambda y (y = x \vee (T(y) \wedge y \not\leq x))) \\ &= \mathbf{c}(\lambda y (y = \mathbf{c}(T_x) \vee (T(y) \wedge \neg T_x(y)))) \\ &= \mathbf{c}(\lambda y (y = \mathbf{c}(T_x) \vee y = \mathbf{c}(T \wedge \neg T_x))) \\ &= \mathbf{c}(\lambda y (y = \mathbf{c}(T))) \\ &= 1 \end{aligned}$$

This complement is unique since it is the supremum of objects  $y$  that only have 0 in common with  $x$ , and suprema are unique.  $\square$

From lemmas 5 and 6 we can finally derive our theorem:

**Theorem 1.**  *$D, \varepsilon$  is a complete atomic Boolean algebra.*

*Proof.* From an application of the Birkhoff-Ward theorem Birkhoff and Ward [1939], which says that any complete atomistic lattice with unique complements is a complete Boolean algebra.  $\square$

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