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Methodological Frames: Paul Bernays, Mathematical Structuralism, and Proof Theory

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Mathematical structuralism is deeply connected with Hilbert's and Bernays's proof theory and its programmatic aim to ensure the consistency of all of mathematics. That goal was to be reached on the sole basis of finitist mathematics, a distinguished, elementary part of mathematics. Gödel's second incompleteness theorem forced a step from *absolute finitist* to *relative constructivist* proof-theoretic reductions. The mathematical step was accompanied by philosophical arguments for the special nature of the grounding constructivist frameworks.

Against this background, I examine Bernays's reflections on proof-theoretic reductions of *mathematical structures* to *methodological frames* via *projections*. However, these reflections—from the mid-1930s to the late 1950s and beyond—are focused on narrowly arithmetic features of frames. Drawing on our broadened metamathematical experience, I propose a more general characterization of frames that has ontological and epistemological significance; it is rooted in the internal structure of mathematical objects that are uniquely generated by inductive (and always deterministic) processes.

The characterization is given in terms of *accessibility*: domains of objects are accessible if their elements are inductively generated, and principles for such domains are accessible if they are grounded in our understanding of the generating processes. The accessible principles of inductive proof and recursive definition determine the generated domains uniquely up to a canonical isomorphism. The determinism of the inductive generation allows us to refer to the mathematical objects of an accessible domain, and the canonicity of the isomorphism justifies at the same time an "indifference to identification." Thus is ensured the intersubjective meaning of mathematical claims concerning accessible domains.

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1. Describing the Context

Paul Bernays viewed mathematics as *the science of idealized structures.*[1] His perspective highlights the methodological changes that expanded, indeed *transformed* the subject during the 19th century. In his (1930), Bernays pointed to three related features characterizing this transformation: (1) the advancement of the *concept of set*, (2) the emergence of *existential* or *structural axiomatics*, and (3) the evolution of a close *connection between mathematics and logic*. He saw these developments as confronting the philosophy of mathematics with novel insights and new problems. In this early essay, Bernays took on the task of situating proof theory within the philosophy of mathematics and, in particular, clarifying the character of *mathematical cognition* (*mathematische Erkenntnis*).

More than 50 years later, Howard Stein observed in his (1988) that the 19th-century transformation of mathematics revealed a capacity of the human mind. He also asserted that this capacity had been discovered already in ancient Greece between the 6th and 4th centuries B.C. Stein emphasized that its *re*discovery teaches us something new about its nature and claimed that what has been learned "constitutes one of the greatest advances in philosophy." However, he did not explicitly formulate the "something new that has been learned" and, thus, did not clarify the dramatic philosophical advance. If we want to grasp this advance, we must deepen our understanding of the mind's mathematical capacity or, even more broadly, its capacities as they come to light in mathematics and its uses.

Taking a step toward deepening our understanding, section 2 begins by discussing the character of the 19th-century transformation as it is revealed in *existential axiomatics* and various foundational frames for it. I prefer to call existential axiomatics *structural* since it is the form of mathematical structuralism that evolved from Dedekind's work and is fully expressed in Bourbaki's *Éléments de mathématique*. Section 3 introduces Bernays's restricted *methodological frames* and his idea of viewing the formalization of axiomatic systems as a means of uniformly *projecting* them into such restricted frames.[2] This builds on

[1] Without taking on the task of interpreting "idealized," I consider for the purpose of this chapter "idealized" only to mean that structural definitions are obtained by a special kind of abstraction emphasized by Lotze; see (Sieg and Morris 2018, 32–34) and the remark by Bernays quoted in note 3. This "Begriffsbildung" is for me the core of the 19th-century *transformation* of mathematics; it is exemplified in Dedekind's *Was sind und was sollen die Zahlen?* My (2016) indicates the more philosophical side of the transition from Kant through Dedekind to Hilbert and beyond.

[2] Charles Parsons (2008) analyzes Bernays' "anti-foundationalism" and "structuralism." He focuses on Bernays's "later philosophy" and compares his structuralism to the *philosophical* structuralism of modern analytic philosophy, whereas I emphasize the continuity in his foundational reflections and connect his structuralism to the *mathematical* structuralism that originated in the 19th-century transformation of mathematics; see also notes 1 and 3. An informative survey of different forms of structuralism is found in (Reck and Price 2000). Finally, contemporary *scientific structuralism* as advocated by Suppes and many others is rooted in the mathematical structuralism as it emerged in the second half of the 19th century with deep connections, in particular, to Gauss,

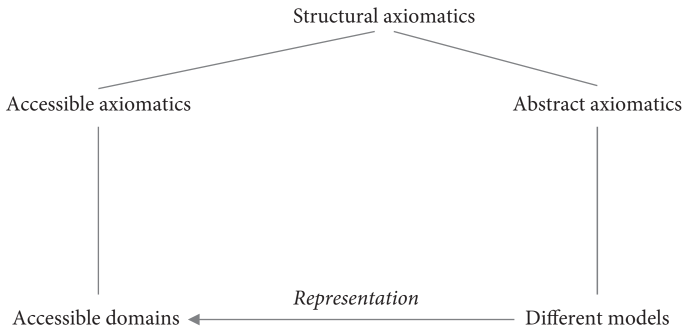

Figure 1 Accessible and abstract axiomatics

a philosophically significant distinction between two kinds of models for structural axiomatic theories, namely, those whose domains just satisfy broad structural conditions and those whose domains are in addition inductively generated. Bernays made this distinction in an elementary form for extensions of Hilbert's consistency program. Given our broader proof-theoretic experience, I generalize in section 4 "inductive generation" and introduce "accessible domains." These considerations lead to an informative and principled distinction between "abstract" and "accessible" axiomatics, both kinds falling under structural axiomatics. The diagram of Figure 1 reflects that distinction.[3]

The preceding incorporates, however, also Hilbert's perspective that analysis and geometry, for example, can be *represented* in set theory. That means that the different models of the axiomatic theories can be viewed as defined in subdomains of Zermelo's set theory, when the latter are viewed as accessible domains. Hilbert's perspective is discussed in section 2.

The elements of *accessible domains* have an internal structure grounding the principles of the structural axioms, but also ensuring that the domains are *canonically* isomorphic. I highlight cognitive aspects that make accessible domains

Riemann, Dedekind, and Hilbert. These connections, evident also in the work of Hertz, deserve a separate investigation. A first step was taken in a talk Aeyaz Kayani and I gave at the 2016 HOPOS meeting in Minneapolis; the talk was entitled *Roots of Suppes' Scientific Structuralism*.

[3] The diagram respects the distinctions made in (Bernays 1970) under the influence of Gonseth. Bernays asserts there, "Mathematical idealization is especially accentuated by the axiomatic treatment of theories." In German: "Die mathematische Idealisierung kommt insbesondere zur Geltung durch die axiomatische Behandlung von Theorien" (181). He continues, "As one knows, one has to distinguish two different kinds of axiomatics." Bernays follows Gonseth in calling the one "axiomatisation schématisante" and the other "axiomatisation structurante." That distinction is "parallel" to the one I am making between "generative structural definitions" (*accessible axiomatics*) and "abstract structural definitions" (*abstract axiomatics*). It would be of real interest to examine the philosophical aspects of their "axiomatisation schématisante" and compare them to those of accessible axiomatics.

suitable to serve as the core of *methodological frames* and as the basis for important relative consistency proofs. Finally, I formulate in section 5 a particular way in which we can investigate, I hope very fruitfully, "the mind's capacities as they come to light in mathematics and its uses."

2. Structural Axiomatics and Frames

There is a form of axiomatization in mathematics that is not tied to theories of modern mathematical logic with their formal languages and logical calculi; I am thinking of the axioms for *abstract* concepts like that of a *group, field*, or *topological space*. The axioms really are just characteristics (*Merkmale*) of *structural definitions*. These structural definitions stand in a venerable tradition that goes back, in particular, to Dedekind's work in algebraic number theory, but also to his essay *Was sind und was sollen die Zahlen?* (*WZ*). In this 1888 essay, Dedekind discards natural numbers as abstract objects and introduces instead the concept of a *simply infinite system* via a structural definition. If one reads from this perspective his 1872 essay *Stetigkeit und irrationale Zahlen* (*SZ*), then one can see that Dedekind defines there the structural notion of a *complete ordered field*.

The concept of a complete ordered field, with a different way of formulating topological completeness, is also defined in Hilbert's 1900 essay *Über den Zahlbegriff*. The axioms of his *Grundlagen der Geometrie* are yet another example of a structural definition, namely, that of a *Euclidean space*. As the final example of such a definition, consider Zermelo's 1908 axioms for set theory: they give a structural definition of the concept *Mengenbereich over a set of urelements*. Zermelo leads in three steps to the axioms (262-263): (1) "Set theory is concerned with a *domain* **B** of individuals which we shall call simply *objects* and among which are the *sets*." (2) "Certain *fundamental relations* of the form $a \in b$ obtain between the objects of the domain **B**." (3) "The fundamental relations of our domain **B**, now, are subject to the following *axioms*, or *postulates*." These steps are typical for the definition of structural notions and parallel almost verbatim Hilbert's steps in the papers just mentioned; they are then followed by the successive introduction and detailed discussion of the axioms. To re-emphasize, the above axiom systems are not formal theories, but structural definitions in the Dedekindian mold.

When commenting on *SZ* for the third volume of Dedekind's *Gesammelte Abhandlungen*, Emmy Noether attributed to Dedekind an *axiomatic conception* (*axiomatische Auffassung*). Three different points informed her judgment. Here are the first two: Dedekind structurally defined the concept of a complete ordered field and proved that the system of all cuts of rational numbers constitutes an instance of that definition. For the third point she referred to an 1876 letter to Lipschitz in which Dedekind expressed his view on the systematic and quite formal development of analysis or any other mathematical theory:

> All technical expressions [can be] replaced by arbitrary, newly invented (up to now meaningless) words; the edifice must not collapse, if it is correctly constructed, and I claim, for example, that my theory of real numbers withstands this test. (Dedekind 1932, 479)

These are indeed the *three crucial elements* of the modern axiomatic method as Noether and others practiced it in the 1920s. It is incisively described in Helmut Hasse's talk *Die moderne algebraische Methode* (1930); the talk addressed a general mathematical audience and suggested an expansion of the "algebraic method" to other parts of mathematics. In characterizing the algebraic method, Hasse emphasized the three aspects Noether pointed to—generalizing, of course, her first two points to other structural definitions.

The axiomatic method, when conceived of as structural, requires an intelligible and philosophically distinguished *methodological frame*, what Bernays calls "methodischer Rahmen." For Dedekind, as emphasized in the preface to the first edition of *WZ*, that was *logic* with a broad contemporaneous understanding; the same holds for the early Hilbert and Zermelo. This logical frame allowed novel *metamathematical* investigations. The central ones could be carried out due to the fact that a *form of semantics* was available: *model* is any system that "falls under" a structural concept or that "satisfies" its characteristic conditions.[4] Dedekind introduced *mappings* (*Abbildungen*) to relate different models in structure-preserving ways.[5] Within this frame, carefully exposed in *WZ*, he proved the concept of a simply infinite system to be *categorical* and argued for the *proof-theoretic equivalence* of any two models.[6]

Hilbert was a master in using models to give independence and relative consistency proofs. Among other things, his investigations show in the most striking way the irrelevance of the "nature" of the objects making up a system that falls under a structural definition.[7] Hilbert's beautiful geometric model of the

[4] This pre-Tarskian semantics was sustained from Dedekind through Hilbert and Ackermann to Gödel in his thesis (1929); it is still used in contemporary mathematical practice.

[5] For Dedekind, mappings form a distinct second category of mathematical entities; they are understood as being given by laws. Sieg and Schlimm (2014) analyze the evolution of the notion of mapping and its use for such metamathematical purposes.

[6] The concept of *proof-theoretic equivalence* was introduced in (Sieg and Morris 2018, section B.2) in order to illuminate section 134 of *WZ* and Dedekind's deeply connected description of the science of numbers in section 73.

[7] John Burgess coined the apt phrase "indifference to identification." In his letter to Frege, written on December 29, 1899, Hilbert asserted that "any theory is only a framework [*Fachwerk*] or a schema of concepts together with the necessary relations between them." The basic elements (*Grundelemente*), he continued, "can be thought in arbitrary ways."

arithmetic concept *Archimedean ordered field* makes that point quite directly and convincingly; see (Hilbert 1899, secs. 13 and 15). However, Dedekind had articulated in *WZ*, and even more explicitly in his letter to Keferstein (Dedekind 1890), a crucial foundational demand for his frame, namely, to give a *logical existence proof* (*logischer Existenzbeweis*) of a model of the concept of a simply infinite system.[8] Dedekind asserted that such a proof was needed to guarantee that the newly introduced concept did not contain an internal contradiction. Hilbert formulated this demand, from the very beginning of his axiomatic investigations, in a quasi-syntactic way and required that no contradiction can be obtained in finitely many logical steps. (It is only *quasi*-syntactic, as no logical steps were explicitly presented.)

The methodological frame was also seen as deeply significant for the representation of mathematical practice. In Dedekind's *WZ*, the representation of elementary number theory was at stake and was achieved through the justification of both the principle of proof by induction and that of definition by recursion.[9] Hilbert dealt with geometry and analysis around the turn of the century. In lectures from 1920, *Probleme der mathematischen Logik*, he expressed the representational strategy with respect to Zermelo's set theory:

> Set theory encompasses all mathematical theories (like number theory, analysis, geometry) in the following sense: the relations that hold between the objects of one of these mathematical disciplines are represented in a completely corresponding way by relations that obtain [between objects] in a subdomain of Zermelo's set theory. (330)

Only a short time later, this representation is refined proof-theoretically, shifting from semantic model to syntactic reduction; that is the core of my discussion in section 4. Coming back to Dedekind's logical frame, we can observe that the development of his theory of systems and of mappings is quite principled: the part concerning systems uses *full comprehension* and the *extensionality principle*, whereas the part concerning mappings uses, for example, closure under composition, and inversion (for bijections). This framework is used to introduce chains (of systems) as a central concept and to develop elementary set theory up

[8] The proof Dedekind gave is problematic, but not because of any "psychologistic" aspects. Frege viewed it as essentially correct; see (Frege 1969, 147–148). For Bernays the real reason for its being problematic is "the idea of a closed totality of all logical objects that can be thought at all" (Bernays 1930, 47).

[9] A similar remark can be made about Dedekind's *SZ*, where he sketches in section 6 the beginning steps of analysis. In section 7 he establishes, in a quite dramatic way, the equivalence of his continuity principle to a theorem of analysis, namely, that every bounded, monotonically increasing sequence has a limit.

to the Cantor-Bernstein theorem.[10] Zermelo's system Z can be understood as a reconceptualization of Dedekind's logical frame: the contradictory comprehension principle is replaced by the restricted separation principle and the latter is supplemented by suitable set existence principles, e.g., the power set and union axioms and the axiom of infinity. It should be noted that mappings are no longer considered as belonging to a separate category of mathematical entities but are rather defined as sets.

Zermelo's system Z developed into ZF during the next 20 years and was adopted as the framework for structural axiomatics. This way of looking at mathematics from a conceptual point of view was clearly articulated by Bourbaki. In their programmatic *The Architecture of Mathematics* from 1950, the role of *principal structures* (*structures mères*) is brought out, and their role in making mathematics intelligible is emphasized. Bourbaki clarifies (1950, 225–226) "what is to be understood, in general, by a mathematical structure":

> The common character of the *different concepts* [my emphasis] designated by this generic name [mathematical structure], is that they can be applied to sets of elements whose nature has not been specified; to define a structure, one takes as given one or several relations, into which these elements enter (in the case of groups, this was the relation $z = x \tau y$ between three arbitrary elements); then one postulates that the given relation or relations, satisfy certain conditions (which are explicitly stated and which are the axioms of the structure under consideration).

The striking parallelism of this description with Hilbert's and Zermelo's formulations should be obvious. Indeed, Hilbert had expressed that perspective in his letter to Frege as follows: "Well, it is surely obvious that every theory is only scaffolding of concepts or a schema of concepts together with their necessary relations to each other, and the basic elements can be thought in arbitrary ways" (Frege 1980, 13). For Bourbaki the expression "this system of mathematical objects has the structure of . . ." is synonymous with "this system of mathematical objects falls under the concept of . . . " Bourbaki concludes this passage on structures-in-general as follows:[11]

[10] The Cantor-Bernstein theorem is not actually formulated in *WZ*. However, Theorem 63—a theorem that is neither proved nor needed for the further development in *WZ*—is used in a contemporaneous manuscript to prove the Cantor-Bernstein theorem; see (Sieg and Walsh 2017).

[11] For a detailed understanding of Bourbaki's notion of "structure" one has, of course, to consult their mathematical exposition in their "Théorie des ensembles" and, additionally, study the very informative papers (Dieudonné 1939), (Cartan 1942), and (Bourbaki 1949): they lay bare their methodological considerations and sympathies.

> To set up the axiomatic theory of a given structure . . . amounts to the deduction of the logical consequences of the axioms of the structure, excluding every other hypothesis on the elements under consideration (in particular every hypothesis as to their own nature).

Again, one should notice the parallelism to Dedekind, Hilbert, and Zermelo. The pure structuralism exemplified by Bourbaki is also formulated in Bernays (1955, 109):

> Not only did Euclidean geometry lose its distinguished position and thus its role as the evident theory of space, but now also the arithmetic theory of magnitudes appears just as the theory of one structure among others. The dominant viewpoint is now one of a general formal theory of structures.[12]

The papers mentioned in note 11 that precede the programmatic (1950) show Bourbaki as being in the direct and deeply "formalist" tradition of Hilbert but refusing to take on the methodological challenge of his foundational program. And what a challenge it was, or turned out to be.

The consistency problem was for Hilbert, as I mentioned already, a quasi-syntactic one. However, all the proof ideas concerning the consistency of the arithmetic of real numbers—indicated in lectures or publications from this early period—are of a semantic kind. In his Heidelberg talk of 1904 Hilbert gave for the first time a "direct" syntactic consistency proof, but it was given for a woefully weak system, a purely equational theory for natural numbers without any logical principles. Impressed by Poincaré's well-known criticism of his proof, Hilbert gave up on the syntactic approach until around 1920, when he returned to it after he had taken, what prima facie seems to be a very roundabout path or a genuine detour.

In 1913, the group around Hilbert started a systematic study of *Principia Mathematica* (*PM*) that ultimately resulted in the lectures *Prinzipien der Mathematik*. These lectures were given by Hilbert in the winter term of 1917–18 and written up by Bernays; they are the real, exquisite beginning of *mathematical logic* and literally provide most of the content in Hilbert and Ackermann's influential book (1928). The possibility of formally developing parts of mathematics, in particular number theory and analysis, made it reasonable to reconsider the syntactic approach to consistency. Such formalizations are indeed the

[12] Here is the German text: "Nicht nur, daß die Euklidische Geometrie ihre ausgezeichnete Stellung und damit ihre Rolle als evidente Raumlehre verlor, auch die arithmetische Größenlehre erscheint jetzt mehr nur als die Lehre von einer Struktur unter anderen. Der beherrschende Gesichtspunkt ist jetzt der einer allgemeinen formalen Strukturlehre."

basis for the *uniform projection* of (the mathematical development of) structural definitions into domains of special mathematical objects. The suggested connection to consistency and the special character of these objects must be clarified. Before doing so in the next section, I will let Hilbert speak one more time about his conception of mathematics at this point.[13]

In lectures from the winter term of 1919 (*Natur und mathematisches Erkennen*), Hilbert wanted to support the claim that "the formation of concepts in mathematics is constantly guided by intuition and experience, so that on the whole mathematics is a non-arbitrary, unified structure." Having presented a construction of the continuum and an investigation of non-Archimedean extensions of the rational numbers, he formulated this general point:

> The different mathematical disciplines are consequently necessary parts in the construction of a systematic development of thought; this development begins with simple, natural questions and proceeds on a path that is essentially traced out by compelling internal reasons. There is no question of arbitrariness. Mathematics is not like a game that determines the tasks by arbitrary invented rules, but rather a conceptual system of internal necessity that can only be thus and not otherwise. (Hilbert 1919, 19)

I quoted this passage to make it crystal clear that formalization is a *tool* for Hilbert; this tool allowed him to reconsider the consistency problem in a truly syntactic way. However, it took a while before features of this tool would inspire the particular methodological distinctions of proof theory and would be used in the pursuit of its reductive aims.

3. Formalizability and Reductive Projections

In the 1917–18 lectures, Hilbert and Bernays transformed a part of the system of *PM* with the axiom of reducibility into a *tool for formalizing analysis*. Having proved the least-upper-bound principle in this system of second-order logic, their final comment was,

[13] The development to strict formalization of mathematical practice and the emergence of formal axiomatics is discussed in my other contribution to this volume, namely, "The Ways of Hilbert's Axiomatics: Structural and Formal." The appendix contains additional information about Hilbert's and Bernays' "formalism."

> Thus it is clear that the introduction of the axiom of reducibility is the appropriate means to turn the ramified calculus into a system out of which the foundations for higher mathematics can be developed. (Hilbert 1917–18, 214)

The core methodological question was, Does this system provide a *logicist foundation* for mathematics? If it did, a philosophically satisfying reduction of mathematics to logic would have been obtained. In his talk of September 1917 at the Zurich meeting of the Swiss Mathematical Society, Hilbert reiterated Dedekind's view that mathematics is part of logic. The fundamental work of Frege and Russell bolstered that view, and Hilbert remarked:

> But since the examination of consistency is a task that cannot be avoided, it appears necessary to axiomatize logic itself and prove that number theory and set theory are only parts of logic.
>
> This method was prepared long ago (not least by Frege's profound investigations); it has been most successfully explained by the acute mathematician and logician Russell. One could regard the completion of this magnificent Russellian enterprise of the *axiomatization of logic* as the crowning achievement of the work of axiomatization as a whole. (Hilbert 1918, 1113)

To help him reach this crowning achievement, Hilbert asked Bernays to become his assistant for the foundations of mathematics—at this very meeting in Zurich. Bernays accepted Hilbert's offer and returned to Göttingen, his alma mater, for the following winter semester. From the very beginning, there was a productive collaboration between Hilbert and Bernays that led to an immediate and significant outcome, namely, the 1917–18 lectures *Prinzipien der Mathematik* I just discussed.

Addressing the methodological question of section 3, Hilbert and Bernays analyzed *PM* in subsequent lectures and examined the nature of the axiom of reducibility. They concluded that its acceptance amounted to using structural axiomatics with its existential presupposition in a different guise, applied to the system of predicates concerning individuals. Thus, Russell's approach did not resolve the foundational problem.[14] Bernays articulated in his (1922b) the issue of assuming the existence of a model for any structural notion as follows:

> In the assumption of such a system with particular structural properties lies something transcendental, so to speak, for mathematics, and the question

[14] Their quite compelling arguments were exposed in the lectures (Hilbert 1920, 361–362) and are quite carefully reviewed in (Bernays 1930, 49–50). The evolution of Hilbert's thought in the period from 1917 to 1922 is discussed in my (1999); see also (Ferreirós 2009).

> arises which principled position with respect to it should be taken. (Bernays 1922b, 10)

An intuitive grasp of the completed sequence of natural numbers or even of the manifold of real numbers should not be excluded outright, Bernays asserted. Alluding to contemporaneous tendencies in the exact sciences, he suggested a different strategy, namely, to see "whether it is not possible to give a foundation of these transcendental assumptions in such a way that only primitive intuitive knowledge is used" (Bernays 1922b, 11).

Bernays's programmatic suggestion is brought to life through the idea of *projecting* structural definitions into a constructive domain and examining the image from a constructivist standpoint: the *formalization* of the structural notion was seen as the means of projecting. In Bernays's still pre-Gödel essay of 1930 one finds the remark:

> At this point, the investigation of mathematical proofs by means of the logical calculus is brought to bear in a decisive way. This [investigation] has shown that the concept formations and the inference patterns used in the theories of analysis and set theory are reducible to a limited number of processes and rules; in that way we succeed in totally formalizing these theories within the frame of a precisely delimited symbolism. (Bernays 1930, 57)[15]

Note that the *total* formalization with restricted processes and rules is at stake, not the syntactic completeness of the formal theory used to capture the structural concept. At this point, normative considerations as to the effectiveness of formal theories entered; after all, it *should be decidable* by a finite procedure whether a given syntactic configuration constitutes a formal proof or not. The total and effective formalizability underlies Hilbert's view that the consistency problem for formal theories is a constructive one. Hilbert and Bernays saw the evolving *formal axiomatics* as applying in identical ways to different parts of mathematics. The significance of this fact is expressed even in *Grundlagen der Mathematik I*:

> Formal axiomatics, too, requires for the checking of deductions and the proof of consistency certain evidences, but with the crucial difference (when compared to contentual axiomatics) that this evidence does not rest on a special epistemological relation to the particular domain, but rather is one and the same for any

[15] Here is the German text: "Hier kommt nun die Untersuchung der mathematischen Beweise mit Hilfe des logischen Kalküls entscheidend zur Geltung. Diese hat gezeigt, daß die Begriffsbildungen und Schlußweisen, die in den Theorien der Analysis und der Mengenlehre angewandt werden, auf eine begrenzte Anzahl von Prozessen und Regeln zurückführbar sind, so daß es gelingt, diese Theorien im Rahmen einer genau abgegrenzten Symbolik restlos zu formalisieren."

> axiomatics; this evidence is the primitive manner of recognizing truths that is a prerequisite for any theoretical investigation whatsoever. (Hilbert and Bernays 1934, 2)

This remark provides the reason for the *uniform* character of the projections' images in a single finitist frame.

Bernays explicitly introduced the image of *projection* in the early 1920s. The appendix to this chapter, "Transition to Hilbert's Proof Theory in 1922," describes the related pre-finitist considerations in (Bernays 1922a) and the use of projections there. As late as 1970, Bernays wrote:

> Taking the deductive structure of a formalized theory as an object of investigation, the (structural axiomatic) theory is *projected* as it were into the number-theoretic domain. (Bernays 1970, 186)

The result of this projection will usually be different from the structure intended by the theory. Nevertheless, the projection has an important point:

> The number-theoretic structure can serve to recognize the consistency of the theory from a standpoint that is more elementary than the assumption of the intended structure. (Bernays, 1970, 186)

The emphasis on *number-theoretic* structures is an artifact of the developments in the wake of Gödel's (1931), namely, the arithmetization of metamathematics. Initially, Hilbert and Bernays viewed the exclusive focus on natural numbers in the foundational discussion as a "methodological prejudice."[16] In their proof-theoretic studies during the 1920s, they operated with what they thought of as broader classes of mathematical objects, namely, finite syntactic configurations like formulae and derivations, and accepted induction and recursion principles for them. The methodological situation is diagrammatically depicted in Figure 2, making clear the reductive role of the projection: it avoids the role of models and their representation, creating, rather, an image in the finitist domain. Hilbert and Bernays never precisely characterized the "finitist domain" and did not offer a rigorous delimitation of finitist mathematics, though the image was to be investigated from the *finitist standpoint*.[17]

[16] The remark is quoted in full and its context is analyzed in (Sieg 1999, 117–118). In his (1970, 188), Bernays calls "the arithmetizing monism in mathematics an arbitrary thesis." In his (1937, 81) he emphasizes that the "total elimination of geometric intuition" might be viewed as "unsatisfactory and artificial." He claims there, "The reduction of the continuous to the discrete succeeds indeed only in an approximate sense."

[17] The term "finite Mathematik" was seemingly a familiar one at this point in early 1922; it had been used in (Bernstein 1919) as covering any "constructive" tendency whatsoever.

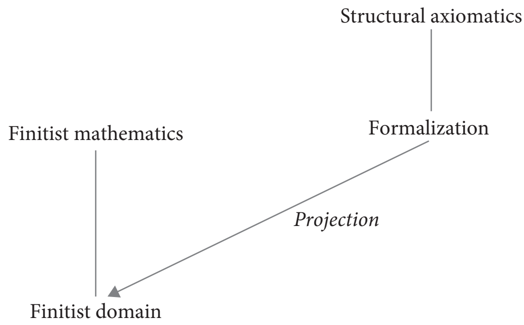

Figure 2. Projection into the finitist domain

As an exemplification of this methodological schema, consider the structural concept of a *complete ordered field* as formalized in second-order number theory; in the finitist domain one has to represent only the elementary formalism, not the infinite objects of its models.

In line with the inspiration from science for the proof-theoretic enterprise, Bernays emphasized in (1922b) the significance of what we now call the *reflection principle*. That principle is equivalent to the consistency of a formal theory T and states that the provability in T of a finitistically meaningful statement implies its finitist correctness; see section 4.[18] This refined metamathematical approach to the consistency problem was successfully realized in early 1922 for the quantifier-free system of *primitive recursive arithmetic*, which is a theory of definite mathematical interest. In the lectures (Hilbert 1921–22), one finds explicitly the beginning of Hilbert's proof theory and his finitist program. Attempting to extend this approach to theories with quantifiers, Hilbert's *Ansatz* from late 1922 replaced quantifiers by epsilon terms and investigated the resulting proofs by the substitution method; that approach was successfully taken up by Ackermann in his thesis (1924), though not in as sweeping a way as it was at first believed. Hilbert's address to the Bologna Congress in 1928 was a bold political act expressing his deep commitment to the international mathematical community, but it was also a remarkable scientific statement: the evolution of mathematical logic is described with great lucidity; the state of proof theory is presented, albeit mistakenly, as including consistency proofs for full elementary number theory by Ackermann and von Neumann; important metamathematical problems are formulated, in particular, the consistency problem for analysis, the

[18] See also (Hilbert 1928, 474) and (Bernays 1930, 55; 1937, 80; 1938, 153).

syntactic completeness problem for number theory, and the semantic completeness problem for first-order logic.

Gödel gave in his thesis (1929) a positive solution of the last problem; in his attempt to address the first problem, he discovered in August 1930 the syntactic incompleteness of familiar theories like *PM, ZF*, and von Neumann's set theory. A few months later he proved his second incompleteness theorem, which was viewed by some as radically undermining Hilbert's finitist consistency program.[19] That program, Gödel noted, had been attractive to mathematicians and to philosophers alike; in his 1938 lecture at Zilsel's, he wrote:

> If the original Hilbert program could have been carried out, that would have been without any doubt of enormous epistemological value. The following requirements would both have been satisfied: (A) Mathematics would have been reduced to a very small part of itself. . . . (B) Everything would really have been reduced to a concrete basis, on which everyone must be able to agree. (Gödel 1938, 113)

Gödel explored in this lecture a variety of extensions of finitist mathematics: from transfinite induction used in Gentzen's 1936 proof of the consistency of arithmetic to his own system of computable functionals of finite type that led eventually to the *Dialectica interpretation*; see (Sieg and Parsons 1995).

At this point, when thinking from our contemporary perspective about *extensions* of the constructive basis for Hilbert's program, it is important to examine which structural notions need to be reduced and to reflect on the domains to which they are to be reduced. After all, the simplicity of the universal finitist basis has been lost, but there may be other bases, "on which everyone must be able to agree." In the same year in which Gödel made his remarks at Zilsel's, Bernays contributed a paper to *Les Entretiens de Zürich*, entitled *Über die aktuelle Methodenfrage der Hilbertschen Beweistheorie*; the paper was published in French three years later (Bernays 1941). Bernays addressed the same question Gödel had asked at Zilsel's: How can one extend the finitist standpoint? Both examined Gentzen's 1936 consistency proof of elementary number theory via transfinite induction up to the first epsilon number, and both asserted that this principle went beyond finitist mathematics.

Gödel referred to the French publication of this paper in a letter to Bernays of January 16, 1942. He writes with obvious surprise:

[19] For the developments that arose out of Gödel's Königsberg remarks and his (1931), see (Sieg 2011); von Neumann had independently discovered the second incompleteness theorem already in November 1930 as we know from his correspondence with Gödel that was published in volume 5 of Gödel's *Collected Works* (2003b).

> I read your article in the *Entretiens de Zürich* from the year 1938 with great interest; only what you say on p. 152, lines 8–11 is not comprehensible to me. Wouldn't that be tantamount to giving up the formalist standpoint? (Gödel 2003a, 133)

Gödel points to the last sentence of a paragraph in which Bernays answered his own question: What is the methodological restriction of proof theory, if it is not the restriction to the elementary evidence of the finitist standpoint? Bernays wrote (and I translate from the German original of his *Entretiens* contribution (1938, 16)):

> One can respond [to this question] that the general nature of the methodological restriction remains in principle exactly the same. However, if we want to keep open the possibility of extending the methodological frame, then we must avoid using the concepts of evidence and certainty in a sense that is too absolute.[20]

The paragraph ends with the sentence Gödel had pointed to: "In this way we gain, on the other hand, the fundamental advantage of not being forced to view the usual methods of analysis as unjustified or dubious" (Bernays 1938, 16).[21] Bernays agrees in his response to Gödel's letter that this perspective is not that of strict formalism, but he also emphasizes that he has never taken a formalist position.[22] Positively, he argues:

> It does not seem appropriate to posit in an absolute sense one methodological standpoint per se as evident and the standpoints differing from it as dubious or as only technically justified. That sort of opposition is also not at all necessary . . . as long as one decides to distinguish between different layers and kinds of evidence. (In Gödel 2003a, 139)

Bernays then points out that the certainty of a *thought system* (*Gedankensystem*) is not given from the beginning but is acquired through a kind of *intellectual*

[20] Here is the German text: "Hierauf ist zu erwidern, dass die Tendenz der methodischen Beschränkung grundsätzlich dieselbe bleibt, nur dass wir—wenn wir uns die Möglichkeit von Erweiterungen des methodischen Rahmens offen halten wollen—vermeiden müssen, die Begriffe der Evidenz und der Sicherheit in einem zu absoluten Sinne zu gebrauchen."

[21] Here is the German text: "Damit gewinnen wir andrerseits den grundsätzlichen Vorteil, dass wir nicht genötigt sind, die üblichen Methoden der Analysis als ungerechtfertigt oder bedenklich zu problematisieren."

[22] Bernays points to his (1930) and his essay "Sur le platonisme dans les mathématiques" as exemplary essays in which he took exception from such a perspective. Clearly, he had taken already in his 1922 papers such a "non-formalist" position.

experience (*geistige Erfahrung*). That observation pertains also to analysis. Nevertheless, he emphasizes, "that does not prevent one from contrasting the methods of analysis with an approach of more elementary evidence and of a more specifically arithmetic character" (In Gödel 2003a, 139).

Having articulated an open perspective that allows distinguishing between different layers and kinds of evidence, Bernays insists on the methodological significance of syntactic consistency proofs:

> The task of establishing the inner harmony of analysis from such a standpoint of more elementary evidence as a syntactic necessity by formalizing the inferences of analysis, that task gains in this way its methodological significance.[23] (In Gödel 2003a, 138)

What standpoint of "more elementary" evidence can be taken? How is an approach based on "a more specifically arithmetic character" to be understood? In subsequent papers Bernays made some general suggestions, which point in a direction that can be given more weight and significance by exploiting our more extended experience with proof-theoretic investigations.

4. Accessible Objects and Principles

To indicate the core metamathematical and methodological issues, I will discuss three examples of relevant proof-theoretic work. However, before giving these examples, I briefly recall the context as described at the beginning of the previous section: structural definitions are to be projected, via their associated formal development, into a "constructive" domain; their images are to be investigated from a "constructive" standpoint with the goal of establishing the consistency of the structural definition. Indeed, the methods for consistency proofs in the pursuit of variants of Hilbert's Program have been required to be "constructive," i.e., processes should be effective, mathematical objects should be inductively generated, and proofs should shun the law of the excluded middle. Bernays highlighted these features in his (1954), as the metamathematical investigations must be embedded in a suitable methodological frame. To be suitable for the programmatic proof-theoretic aims, such a frame must satisfy the constructivity requirements just listed, in particular, the crucial condition on mathematical

[23] Here is the German text: "Die Aufgabe, die innere Einstimmigkeit der Analysis von einem solchen Standpunkt elementarerer Evidenz an Hand der Formalisierung der Schlussweisen der Analysis als eine syntaktische Notwendigkeit zu erweisen, erhält damit ihre methodische Bedeutsamkeit."

objects: "The objects (making up the intended model of the theory) are not taken from the domain as being already given but are rather constituted by generative processes" (1954, 12). The nature of the objects is as irrelevant for Bernays as it was for Dedekind, but the generative processes give them a unique *internal structure*. This internal structure is independent of the completed totality of all the generated objects. Keep this observation in mind when I discuss now three paradigmatic proof-theoretic studies.

The first proof-theoretic study is important for two reasons: (1) it showed that Hilbert's program could be pursued from an extended constructive standpoint, and (2) it exemplified an important shift, as the formalization of the broader constructive principles could be used to prove rigorously formulated *relative* consistency results. As to (1), John von Neumann and Jacques Herbrand believed that Gödel's results spelled the definite impossibility of the program for strong formal theories like analysis or even full number theory. When writing his (1931), Herbrand knew Gödel's results well and proved finitistically the consistency of fragments of first-order number theory (PA), when the induction principle is restricted to quantifier-free formulae. Gödel viewed Herbrand's theorem, even in December 1933, as the most far-reaching result in the pursuit of Hilbert's finitist program (Gödel 1933a, 52). What changed the general approach to the consistency problem was the metamathematical fact proved in this first study: Gödel and Gentzen independently established in 1932 the consistency of full elementary number theory (PA) *relative* to its intuitionist variant (HA).[24] According to Bernays (1967) and the historical record, finitist and intuitionist mathematics had been viewed as co-extensional up to the discovery of the reduction of (PA) to (HA). This result showed that intuitionist mathematics is a proper constructive extension of finitist mathematics.

One can view this result as having been obtained by a projection of the concept *simply infinite system* through (PA) into a subdomain of intuitionist mathematics. The arithmetic principles governing the relevant subdomain are those of (PA) and are joined with intuitionist logic; the resulting formal theory is Heyting Arithmetic (HA). Notice, first of all, that (PA) is adequate for the formalization of ordinary number theory. Observe, second, that derivations in (PA) are syntactically translated into proofs in (HA). Indeed, the translation and the metamathematical argument, showing that the translation yields HA-proofs, can be carried out in (HA). The resulting HA-proofs are, finally, recognized from an intuitionist standpoint as being "correct."[25]

[24] As to the sequence of these discoveries see (Sieg 2011, 178).

[25] *Correct* is to be understood in this context in two different ways. In the formal metamathematical argument, one establishes the partial reflection principle for (PA) within the constructive theory (HA) for a certain class of arithmetic statements. In the overall methodological considerations, one recognizes the proofs in (HA) as "fully correct" from the intuitionist standpoint. For a more detailed discussion, see my (1984) or its republication in (Sieg 2013, 250–252).

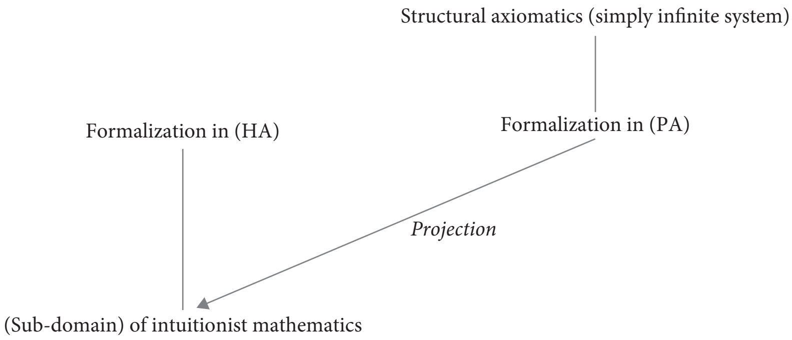

Figure 3. Projection into a sub-domain of intuitionist mathematics

Here we have a direct and essentially logical reduction. Figure 3 points again to formalization as the means of projection, but it incorporates the formalized principles needed for the *relative* consistency proof. Thus, the diagram has three central, distinct components: (i) an articulation of the abstract axiomatic theory as a formal one, (ii) the identification of the syntactic objects of the formal theory with elements of a suitable domain, and (iii) the precise formulation of constructive principles concerning that domain. The Gödel-Gentzen result is special in that both the classical and constructive theory have the same mathematical principles; it is "only" the underlying logic that is different.

Significantly later, results were obtained for the notion of a *complete ordered field*. That concept is also categorical, but I should emphasize that categoricity does not guarantee accessibility: Cauchy sequences, Dedekind cuts, and Hilbert's "Strecken" (of his geometric model) all constitute complete ordered fields that are isomorphic, but not canonically so. In the second study, the classical theory is a subsystem of analysis (i.e., of second-order arithmetic) with the comprehension principle for arithmetic formulae only; the system is denoted by $(ACA)_0$. It can be shown to be conservative over (PA) and, as (PA) is relative consistent to (HA), it is consistent relative to (HA).[26] Despite the fact that this subsystem of analysis is proof-theoretically not stronger than (HA), it is adequate for a significant part of mathematical practice: Weyl's development of classical analysis in *Das Kontinuum* can be formalized in $(ACA)_0$; see (Feferman 1988) and also (Takeuiti 1978). All of this is reflected through an easy modification of the

[26] A proof-theoretic argument for the conservative extension result is given in (Feferman and Sieg 1981, 112). It can be established in (HA); that fact is important for the proof of the partial reflection principle. Many reductions of classical to constructive theories are found in that paper. Significant reductive results are presented in (Rathjen and Sieg 2018) for a much-extended range of theories.

diagram in Figure 3: "simply infinite system" is replaced by "complete ordered field" and (PA) by $(ACA)_0$.

The third study is even more illuminating as to the broad methodological issues, but it is also mathematically more complex. We aim again for a projection of the notion of a *complete ordered field*, but this time through the classical and impredicative subsystem of analysis $(\Pi^1_1\text{-CA})_0$ into the domain of the finite constructive number classes whose principles are formalized in the intuitionist theory $\mathrm{ID}(\mathbf{O})_{<\omega}$. $(\Pi^1_1\text{-CA})_0$ has the comprehension principle for Π^1_1-formulae, whereas $\mathrm{ID}(\mathbf{O})_{<\omega}$ expands (HA) by closure and minimality principles for the $\mathbf{O}_n$; these principles are formulated subsequently, once we have stated the generative clauses for the number classes. We first notice that $(\Pi^1_1\text{-CA})_0$ is adequate for the formalization of mathematical analysis. In Supplement IV of Hilbert and Bernays (1939), analysis is developed in second-order arithmetic. A careful examination of their development shows that the comprehension principle is used only for Π^1_1-formulae. Second, the reduction is obtained in two steps. In Feferman (1970), $(\Pi^1_1\text{-CA})_0$ is shown to be proof-theoretically equivalent to the classical theory of finitely iterated inductive definitions $\mathrm{ID}_{<\omega}$. The first step is then followed by the reduction of the classical theory $\mathrm{ID}_{<\omega}$ to intuitionist $\mathrm{ID}(\mathbf{O})_{<\omega}$; this second step was taken in my 1977 thesis and involves crucially transformations of infinitary proof figures that are identified with elements of the constructive number classes. The transformed infinitary proofs of a subclass of arithmetic statements are recognized in $\mathrm{ID}(\mathbf{O})_{<\omega}$ as correct. These considerations are reflected in a modification of the diagram in Figure 3: "simply infinite system" is again replaced by "complete ordered field," (PA) by $(\Pi^1_1\text{-CA})_0$, and (HA) by $\mathrm{ID}(\mathbf{O})_{<\omega}$. The published presentation of this second step is Sieg (1981), but a sketch is given in (Sieg 2013, 254–256).

A summary discussion of the crucial aspects of these investigations can be given with the help of the three diagrams in Figures 1–3. The first diagram simply reflects my distinction between accessible and abstract axiomatics. The second diagram indicates the perspective for the finitist investigation of the images of structural axiomatic theories; the images have been obtained through the formalization of the theories. The third diagram adds two significant new components. The image of the projected abstract notion is no longer found in the finitist domain, but rather in that of intuitionist mathematics; that is the first new component. The second new component is the formal articulation of the theory in which the metamathematical investigations proceed, here (HA) and $\mathrm{ID}(\mathbf{O})_{<\omega}$. An appropriately generalized diagram is found in Figure 4.

What general features should be required of methodological frames, so that they are suitable for extensions of Hilbert's constructivist program? Bernays reflected already in his (1938) on constraints for frames and took an *arithmetical perspective in the strict sense* as central:

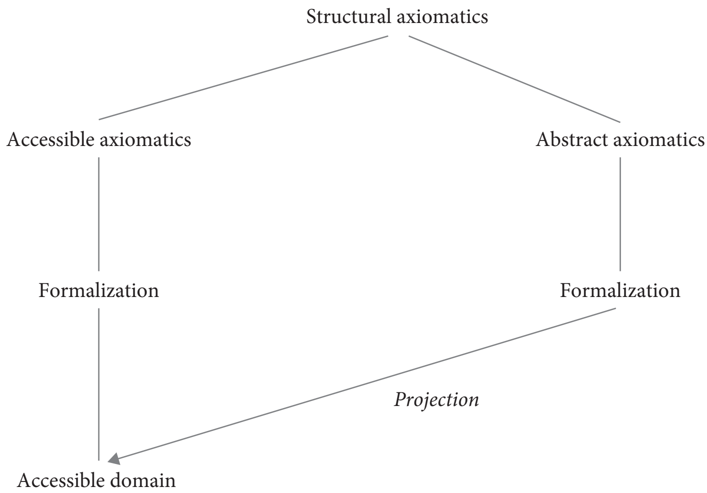

Figure 4. Reductions to accessible domains

> [Accordingly,] arithmetical is the representation [*Vorstellung*] of a figure that is composed of discrete parts, in which the parts themselves are considered *either only* in their relation to the whole figure *or* according to certain coarser distinctive features that have been specially singled out; arithmetical is also the representation of a formal process that is performed with such a figure and that is considered only with regard to the change that it causes. (Bernays 1983)

These considerations underlie the requirement that methodological frames must have domains of objects that are constituted through generative *arithmetical* processes that are then captured through the adopted principles. Bernays called this special form of structural axiomatics *sharpened axiomatics* (*verschärfte Axiomatik*).

Thus, the crucial question is, which procedures can be viewed as generative (arithmetical) ones? Elementary inductive definitions of syntactic notions, like formula or proof, were clearly viewed in that light from the very beginning. Due to Aczel's (1977) we have an extremely general way of generating mathematical objects that goes far beyond the arithmetical generation of Bernays. Aczel's ways allow, of course, the generation of natural numbers, elementary syntactic objects, but they also yield constructive ordinals and even the elements in segments of the cumulative hierarchy of sets.[27] I focus on just natural numbers **N** and

[27] The latter case has its roots in Zermelo's investigation of *Mengenbereiche* in his (1930); their quasi-categoricity ensures that Zermelo's work on *Mengenbereiche* is for sets what Dedekind's work on *einfach unendliche Systeme* is for natural numbers.

constructive ordinals **O**, the second constructive number class. **N** is generated from some element *0* using an injective successor operation *s* and two rules: *0* is in **N** and if *n* is in **N**, then *s(n)* is also in **N**. The second constructive number class is also generated with the help of two rules, namely, *0* is in **O** and if *e* is (the Gödel number of) a recursive function enumerating elements of **O**, then *e* is also in **O**.[28] The closure and minimality principles for domains are standard for **N** and can be articulated for **O** as follows:

$$(\forall x)\ (\mathrm{A}(\mathbf{O},x) \rightarrow \mathbf{O}(x))$$

and

$$(\forall x)\ (\mathrm{A}(F,x) \rightarrow F(x)) \rightarrow (\forall x)\ (\mathbf{O}(x) \rightarrow F(x))$$

The first formula expresses that **O** is *closed* under the generating clauses, whereas the second formula schema says (*F* being any formula in the language of HA expanded by the unary predicate **O**) that **O** is *minimal* among all predicates that are closed under the generating clauses. The latter is the principle of *proof by induction* for **O**. The intuitionist theory ID(**O**) is the extension of (HA) by the two preceding principles.[29]

N and **O** are examples of *i.d. classes* that obey not only the *principle of proof by induction* but also the *principle of definition by recursion*, because they are deterministic.[30] The deterministic i.d. classes are the *accessible domains*, and the associated accessible principles support canonical isomorphisms between any two such classes. They are centrally positioned in the final diagram of Figure 4 that combines and generalizes the diagrams from Figures 1 and 3.

The methodological point of projections and the resulting structural reductions is to coordinate and bring into harmony two crucial aspects of mathematical experience: the *conceptual* one involving abstract notions that have many different models, and the *constructive* one concerning accessible domains that are characterized uniquely up to a canonical isomorphism. The first aspect provides mathematical explanations that rest on conceptual understanding, whereas the second aspect facilitates thinking about mathematical objects and fundamental principles that are grounded in the inductive generation of those

[28] The antecedents of these generating clauses can be expressed by a formula that is arithmetic in **O**. Their disjunction is abbreviated by A(**O**, *e*).

[29] The intuitionist theory $\mathrm{ID}(\mathbf{O})_{<\omega}$ is a similar expansion (HA) with principles for the finite constructive number classes $\mathbf{O}_n$; the latter are obtained by iterating the definition of **O**, but allowing in the second generating clause also branching over already obtained number classes $\mathbf{O}_k$ with *k* less than *n*.

[30] An i.d. class is deterministic if the generating operations are injective. Consequently, all of its elements have an associated unique construction tree that is of course well-founded.

objects. *Reductive projections* are the crucial means for joining those aspects guaranteeing the coherence of abstract concepts. The philosophical significance of consistency proofs is to be assessed in terms of the objective underpinnings of the frames to which reductions are achieved. It is precisely here that the various accessible domains play a distinctive role and offer, through a comparison of their generating operations, a scale for assessing relative consistency proofs. This remains an open field for penetrating philosophical investigation and concrete mathematical work.[31]

In this open field, questions are being pursued that transcend traditional issues in the philosophy of mathematics and that are based on one common insight: mathematics *systematically* investigates *concepts* that are structurally defined. Which concepts are to be considered, which logical means are to be used for the development of their theories, and which methodological frames should be considered—these questions have been controversial. From this perspective, the controversy between "classical" and "constructive" mathematics can be transformed into two probing questions, (1) *what is characteristic of and possibly problematic in* classical mathematics and (2) *what is characteristic of and taken for granted as convincing in* constructive mathematics. Answers to these questions have hardly been advanced by "ideological" discussions. Some argue as if an exclusive alternative between *Platonism* (taken to be required for classical mathematics) and *intuitionism* (taken to be required for constructive mathematics) had emerged from sustained foundational work over the last 150 years or so; others argue as if that work were deeply misguided and had no bearing on our understanding of mathematics. Both attitudes prevent us from turning attention to two crucial and more specific tasks, namely, on the one hand, to understand the role of abstract structural concepts in mathematical practice and, on the other hand, to clarify the function of accessibility notions in philosophical analysis. These tasks have fundamentally to do with mathematical cognition; some fruitful directions for explorations are discussed in the next section, which also happens to be the last one.

[31] As to more up-to-date work in proof theory concerning proof-theoretic reductions, see the contribution to the *Stanford Encyclopedia of Philosophy* Rathjen and I wrote (Rathjen and Sieg 2018). The volumes (Kahle and Rathjen 2015) and (Jäger and Sieg 2017) are also rich sources for contemporary work in proof theory. To obtain an "abstract" grasp of accessible domains, I have been interested in their category-theoretic characterization for quite a while see (Sieg 2002, 372–373). Patrick Walsh worked on this very issue in his dissertation (Walsh 2019).

5. Exploring Cognitive Capacities

Reconnecting with Stein's remarks on capacities of the mathematical mind, I am led back to the 19th century and to Dedekind. In his *Habilitationsrede* from 1854 Dedekind remarks on different ways of conceiving the object of a science and asserts that this difference "finds its expression in the different forms, the different systems in which one seeks to frame its conception" (429).[32] The need to frame the conception of a science arises from the fact that our intellectual powers are imperfect. "Their limitation leads us to frame the object of a science in different forms, and introducing a concept means formulating a hypothesis on the inner nature of the science." How well the concept captures this inner nature is determined by its usefulness for the development of the science; in mathematics that mainly means its usefulness for constructing proofs. Dedekind put the theories from his foundational essays to this test by showing that they allow the direct, *stepwise* development of number theory and analysis by means of our *Treppenverstand* using exclusively the *characteristic conditions* (*Merkmale*) of the structural definition of the relevant notion as starting points. *Creating concepts* and *deriving theorems* are consequently the tools to overcome, at least partially, the limitations of our intellectual powers.[33]

The theme of such *specifically human* understanding is sounded also in a remark from Bernays (1954, 18): "Though for differently built beings there might be a different kind of evidence, it is nevertheless our concern to find out what evidence is for us." Bernays emphasized, as mentioned already, that evidence is acquired through intellectual experience and experimentation in an almost Dedekindian spirit. In 1946, he wrote, for example:

> In this way we recognize the necessity of something like intelligence or reason that should not be regarded as a container of [items of] a priori knowledge, but as a mental activity that reacts to given situations with the formation of experimentally applied categories. (Bernays 1946, 91)

Intellectual experimentation of this kind *in part* supports the creation of concepts that define abstract structures or characterize accessible domains; *in part* it is supported through the illuminating use of these concepts in proofs of significant theorems of mathematical practice. These aspects of the mind are central, if we

[32] Here is the German original: "Diese Verschiedenheit der Auffassung des Gegenstandes einer Wissenschaft findet ihren Ausdruck in den verschiedenen Formen, den verschiedenen Systemen, in welche man sie einzurahmen sucht."

[33] This is discussed in detail in (Sieg and Morris 2018). The functional role of concepts or, in Bourbaki's terminology, of structures is emphasized by Heinzmann and Petitot in their contribution to this volume.

want to grasp the subtle connection between reasoning and understanding in mathematics, as well as the role of *leading ideas* in guiding the construction of proofs and of *concepts* in providing explanations.[34]

How can we explore these issues in a systematic and yet open way? The investigation of proofs and their conceptual contexts is central for such research. In a way, I am arguing for an *expansion of proof theory* to consider informal mathematical proofs as objects of theoretical study; formal representations of proofs and their metamathematical investigation are important, but in the end—for our purposes—subservient to the examination of what Hilbert called "the notion of the specifically mathematical proof" (1918). Even for Gentzen in (1936, 499), "The objects of proof theory shall be the *proofs* carried out in mathematics proper." Hilbert had made already an additional claim concerning the general philosophical significance of formalized mathematics that "is carried out according to certain definite rules, in which the *technique of our thinking* is expressed":

> These rules form a closed system that can be discovered and definitively stated. The fundamental idea of my proof theory is none other than to describe the activity of our understanding, to make a protocol of the rules according to which our thinking actually proceeds. . . . If any totality of observations and phenomena deserves to be made the object of a serious and thorough investigation, it is this one. (Hilbert 1927, 475)

A good start for such an investigation is a thorough *computer-based formal reconstruction* of parts of the rich body of mathematical knowledge that *is systematic*, but that is also *structured for human intelligibility and discovery*.[35] In order to expand formal methods by *heuristics* (leading ideas) and to carry out proof search experiments, we must isolate truly creative elements in proofs and implement them. Thus, we will come closer to an understanding of the technique of our thinking, be it mechanical or non-mechanical. In a radio broadcast of 1951, Turing remarked: "The whole thinking process is still rather mysterious to us, but I believe that the attempt to make a thinking machine will help us greatly in finding out how we think ourselves" (Turing [1951] 2004, 486). It is no less mysterious more than 75 years later, but we have now powerful computational

[34] In my paper *Gödel's Philosophical Challenge (to Turing)* (2013a) I explore the ways in which Gödel and Turing, in quite different ways, try to overcome the limitations of particular formal theories. Turing appeals to "initiative" and varied mathematical experience, whereas Gödel seeks a deeper understanding of abstract concepts, in particular, that of "set."

[35] See my paper with Patrick Walsh on *natural formalization* (2017), but also our discussion of Gowers's "human-centered automatic theorem-proving" in (Gowers 2016) and (Ganesalingam and Gowers 2013, 2017).

and sophisticated logical tools as well as a broad methodological perspective for exploring human mathematical cognition. I am convinced that such explorations will illuminate "one of the greatest advances in philosophy."

Appendix: Transition to Hilbert's Proof Theory in 1922

Hilbert's consistency issue had been raised in a "model theoretic" form already by Dedekind. To guarantee that the concept of a simply infinite system does not contain internal contradictions, Dedekind proved the "logical existence" of a system falling under this concept. In the Second Problem of his Paris talk of 1900, Hilbert formulated the goal of ensuring the "mathematical existence" of a structurally defined concept by giving a consistency proof. In (Hilbert 1905), a *direct* syntactic consistency proof was given for a purely equational system of arithmetic. It took the integration of mathematical and logical investigations (as described in sections 2 and 3) to be able to resume such "proof theoretic" investigations in the early 1920s.

Bernays's contribution (1922a) to the issue of *Die Naturwissenschaften* that celebrated Hilbert's 60th birthday was fully aligned with Hilbert's conception of structural axiomatics. His sketch of how to address the consistency problem is based on talks Hilbert had given in Copenhagen and Hamburg during the first half of 1921; they were published as (Hilbert 1922) .[36] The transitional features of Hilbert's paper are also reflected in Bernays's considerations.[37] For the axiomatic treatment of geometry, Bernays formulated matters as follows (1922a, 96):

> The spatial relationships are, so to speak, projected into the mathematical-abstract sphere; in this sphere, the structure of their connection presents itself as an object of pure mathematical thinking and is being investigated with the sole focus on logical relations.[38]

[36] The three aspects of 19th-century developments he pointed out in his (1930), and which I discussed at the very beginning of this paper, are already present here in (Bernays 1922a). As to the philosophical significance of this new kind of axiomatics, he emphasized that (1) it involves an "Abgehen vom Apriorismus" (95) and (2) mathematics, so understood and developed, is an "allgemeine Formenlehre" (99).

[37] The development of Hilbert's foundational investigations in this critical period between the 1917–18 lectures and the 1921–22 lectures is described in (Sieg 1999). All the relevant sources are, of course, available now in (Ewald and Sieg 2013).

[38] Here is the German text: "Die räumlichen Verhältnisse werden gleichsam in die Sphäre des Mathematisch-Abstrakten projiziert, in welcher die Struktur ihres Zusammenhanges sich als ein Objekt des rein mathematischen Denkens darstellt und einer Forschungsweise unterzogen wird, die nur auf die logischen Beziehungen gerichtet ist."

How is such an *investigation* to be realized? The structural axiomatic treatment provides the basis for the exclusive focus on logical relations. Any mathematical proof is taken to be "a concrete object all of whose parts can be surveyed; it must be possible, at least in principle, to communicate it [the proof] completely from beginning to end" (97). That a proof does or does not end in a contradiction is "a concretely checkable property." At exactly this point, the logical calculus of "Peano, Frege, and Russell" comes in: these three logicians expanded the calculus in such a way "that the thought-inferences of mathematical proofs can be completely reproduced by symbolic operations" (98). A joint formal development of mathematics and logic is thus ensured, but there is no sense yet of the theoretical means needed for metamathematical investigations. Bernays only writes that, in principle, it is possible to obtain consistency proofs for analysis through "elementary, ostensively certain considerations." Hilbert (1922) thought that one would not have to appeal to any principle of induction, thus sidestepping Poincaré's objection to his earlier syntactic consistency proof.

This brief appendix is simply to point out that Bernays, in his first paper on foundational matters, is fully aligned with Hilbert and uses the representation of mathematical proofs in formalisms as a tool for their investigation, not as a way for characterizing mathematics as a formal game.

Acknowledgments

The perspective on foundational problems I expressed in this chapter is deeply shaped by my intellectual experience as a student of mathematics in Berlin: I was fascinated by *structuralist mathematics* as taught by Karl Peter Grotemeyer, learned the elements of category theory, and read a lot of Bourbaki; at the same time, I was affected by Paul Lorenzen and his philosophically critical attitude toward the foundations of that very mathematics.

It was only later, after having studied mathematical logic in Münster under Dieter Rödding and worked in proof theory at Stanford with Solomon Feferman, that I started to appreciate the balanced perspective of Paul Bernays: his characterization of mathematics as *the science of idealized structures* and the philosophically significant role proof theory was assigned in his scheme of things.

That position was alluded to at the end of my first reflective essay (1984) and became topical and connected to Bernays in (1990). I formulated matters more pointedly in two seminars at the University of Bologna on April 11 and 12, 2007, under the title *Reductive Structuralism: Joining Aspects of Mathematical Experience*. In June 2015, I gave a talk at the University of Vienna under the title *Reductive Structuralism*; the present chapter is an elaboration of those talks.

The translations in this paper are mine, unless quoted from a particular source. I want to thank Erich Reck and Georg Schiemer, who read earlier drafts and made many helpful suggestions. Critical remarks of Patrick Walsh prompted me to rethink and rewrite the central section 4.

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