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## Austrian Philosophy

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# Foreword

This special issue of the *Hungarian Philosophical Review* presents papers resulting from current research on Austrian philosophy. The topics are mainly historical, however they provide an in-depth analytical reconstruction and interpretation of the views discussed. Part of the papers focus on lesser known aspects of and connections within the diverse strands of the Austrian philosophical tradition, others address some important influences of Austrian philosophy (including also philosophical aspects of psychology, linguistics and mathematics) on Hungarian intellectual life and academia.

The topics discussed are the following: Guillaume Fréchette provides an analysis of Brentano's views on perception. Denis Fiset writes on the reception of Mach by Brentano and his students. Christoph Limbeck-Lilienau investigates the influence of Meinongians on the First Vienna Circle's views on logic. Thomas Uebel presents a new understanding, and a possible defence of Carnap's methodological solipsism he advocated in *Der logische Aufbau der Welt*. Christian Damböck investigates the "plagiarism" or "Ideendiebstahl" charge according to which Carnap in the *Aufbau* allegedly had taken over views of Husserl formulated in the *Ideen II*. Gergely Ambrus discusses Schlick's Austrian psychophysical identity theory and its similarity to certain views of Russell and to contemporary Russellian monists, David Chalmers in particular. Friedrich Stadler provides a general context and background to these particular issues with an overview of "Austrian philosophy" at the University of Vienna from the 19<sup>th</sup> to the end of the 20<sup>th</sup> century. In addition, we also present papers about the diverse influences Austrian philosophy as broadly conceived exerted on Hungarian thinkers. Csaba Pléh discusses the influence of Karl Bühler and his school on Hungarian psychology and linguistics; Miklós Rédei analyses the connections between Gödel's and von Neumann's views on the foundations of mathematics. Péter András Varga discusses the *peregrinatio* of the Hungarian philosopher Bernhard (Bernát) Alexander, a noted Kant scholar and a major figure in Hungarian intellectual life at the turn of the 19<sup>th</sup> century.

In some more detail: Guillaume Fréchette's paper *Brentano on Perception* discusses what may be taken as the "standard view" of Brentano's theory of perception, according to which perceptual experiences constitute a subclass of intentional experiences. Fréchette argues that the standard view cannot be supported either by sense-datum theory, or adverbialist or representationalist theories of perception. Further, he suggests, Brentano's understanding of optical illusions presupposes the distinction between the subjectively and objectively given, which challenges the standard view, and fits better with an account of perception as openness to and awareness of the world.

Denis Fiset in his *The Reception of Ernst Mach in the School of Brentano* outlines the most important elements of this reception. First he discusses Brentano's lectures on positivism in which he evaluates Mach's theory of sensations. This is followed by a presentation of the early reception of Mach in Prague by Brentano's students; then the relation between Mach's descriptivism and phenomenology is established, showing that Mach's phenomenism was indeed a source of Husserl's phenomenology. Further, Mach's contribution to the controversy on Gestalt qualities is also examined as well as Mach's debate with Stumpf on psychophysical relations and Husserl's criticism of Mach's alleged logical psychologism.

Christoph Limbeck-Lilienau focuses on some less familiar aspects of the history of the precursors of the later Vienna Circle. First, he puts forth the historical thesis that, due to the lack of archival sources, it may be questioned whether the so-called "First Vienna Circle" existed at all, at least as a regular discussion group. Second, he uncovers hitherto unknown or neglected connections between the First Circle (Neurath, Frank, Hahn) and a group of philosophers strongly influenced by Meinong (as e.g. Alois Höfler). Limbeck-Lilienau argues that – besides the well-known influences of Mach and the French conventionalists – the interaction with the Meinongians paved the way for the reception of the new symbolic logic and especially of Russell's philosophy of logic and mathematics. Further, he claims that Neurath, and probably also Hahn, endorsed a logical realism similar to that of Russell and Meinong, which they renounced only after the reception of Wittgenstein's *Tractatus*.

Christian Damböck addresses a charge against Carnap that was formulated already in the 1990s by Verena Mayer and then by Guillermo Rosado Haddock, and was further radicalized in a recent article of Mayer, according to which Carnap in his *Aufbau* took over substantial parts from Husserl's (then unpublished) *Ideen II* without acknowledging his sources. Damböck refutes these claims, differentiating between several senses of plagiarism and „Ideendiebstahl“, and arguing that Carnap – though he might have been acquainted with Husserl's manuscript – cannot be accused of plagiarism even in the weakest sense.

Thomas Uebel in his *Overcoming Carnap's Methodological Solipsism: Not as Easy as it Seems* presents a novel understanding and a possible defense of Carnap's

methodological solipsism advocated in *Der logische Aufbau der Welt*. He brackets Quine's "is-at" objection against the constructional system of the *Aufbau* (published in *Two Dogmas* in 1951), and concentrates on Neider's objection, according to which the intersubjectivity of the meaning of the concepts constructed by the *Aufbau* methods is not achieved. Uebel suggests that there are remarkable resources to resist this charge, drawing on the distinction between re-creating and simulating intersubjectivity, if one takes Carnap's descriptions of the aim of the constructional programme literally. Uebel has extensively investigated Carnap's physicalist turn in previous publications, this paper however approaches this development from a new angle, and provides further insights to Carnap's goals in the *Aufbau* as well as to his reason for – finally – abandoning methodological solipsism that has been the epistemological fundament of the *Aufbau* programme.

Gergely Ambrus presents Moritz Schlick's "Austrian" psychophysical identity theory, presented in the *Allgemeine Erkenntnislehre*, and compares it with the Russellian monist views of Russell (formulated in *The Analysis of Matter* and *Human Knowledge*, for example) and also to David Chalmers' position, a representative of contemporary Russellian monism. A close similarity of Russell's and Schlick's views was already stated by Herbert Feigl long ago; so the goal of the comparison is to see in detail to what extent Russell's and Schlick's views are really akin, and further to determine the relation of some contemporary Russellian monist views to these historical ancestors. As a result, Ambrus argues that all three accounts share some fundamental tenets, namely linguistic physicalism, an ontology which may be described as physicalist dualist property pluralism, and a sort of dual-language account of the psychophysical identity thesis, which is an alternative to the reductionist materialism of e.g. Smart, Armstrong and Lewis. Further, he claims that Schlick, Russell and Chalmers all ground these tenets on a structuralist account of the meaning of physical terms, which, however, they lay out in importantly different ways.

Friedrich Stadler provides an overview of "Austrian philosophy" during the "long 20<sup>th</sup> century" through an institutional history of the Department of Philosophy with the main figures teaching philosophy at the University of Vienna. After a short review of philosophy as a key discipline within the Faculty of Philosophy, the development is described mainly from 1848 onwards with a focus on the last century. The personal and institutional breaks and continuities are characterized by a thematic analysis of the philosophical research and teaching in historical context. This is done with a focus on the typical Austrian "scientific philosophy" in its relation to alternative dominant currents. This specific dynamics becomes manifest on the one hand with the significance of philosophy within the Faculty of Philosophy and, on the other, with its role and function *vis à vis* the other classical faculties. The process of a gradual dissolution and diversification of the Faculty of Philosophy up to the present indicates this changing

role of a long-term, dominant “royal discipline”. Nevertheless, the restructuring and renewal of philosophy as a discipline and research field since the University reform after 2000 appears as a successful and promising turn with an increasing international visibility and appreciation.

The other papers discuss diverse influences of Austrian philosophy and related subjects on Hungarian philosophy and science. Csaba Pléh reviews the influence of Karl Bühler and his school. First he surveys the influence of Bühler’s works on *Denkpsychologie* on Valéria Dienes, Ferenc Lehnert/Lénárd, Antal Schütz and Imre Molnár, and then provides a detailed analysis of the influences of the mature Bühler of the Vienna years both on Hungarian psychology and linguistics. He displays the work of two Hungarian experimental psychologists, Paul (Pál) Schiller von Harkai, who did postdoctoral research in Vienna, and Ludwig (Lajos) Kardos, who was a PhD student of Bühler in Vienna. Schiller von Harkai developed a functionalist theoretical psychology combined with the Gestalt ideas of Lewin and Bühler. Kardos extended the sign-based perceptual theory of Bühler into a successful mathematical theory of light constancy that interpreted contextual influences on a general model. Besides Bühler’s reception in psychology Pléh also deals with the impact of Bühler’s theory of language on Hungarian linguistics: his reception by Gyula Laziczius, and his influence on Laziczius’ student, the linguist and psychoanalyst Iván Fónagy.

Miklós Rédei’s paper investigates the parallels and divergencies of Kurt Gödel’s and John von Neumann’s life and career. They were both born in the Austro-Hungarian Monarchy, had similar social background and education, and their careers had many parallels and partly overlapping research topics. Rédei presents these overlaps and personal encounters, beginning with the first major intersection of their interests, Gödel’s incompleteness theorems. Rédei first reconstructs the initially different but later converging interpretations of the second incompleteness theorem (which von Neumann also independently proved), and then, widening the scope of investigations, turns to Gödel’s and von Neumann’s general views on the nature mathematics. Rédei convincingly shows that although Gödel was a Platonist while von Neumann emphasized the empirical element in mathematics, the relation of their views is more complex; Gödel also acknowledged the role of empirical scientific theories for inventing new mathematical ideas. Their inspiration and attitude however was still significantly different, as von Neumann’s mathematical innovations were initiated in most cases by empirical sciences from quantum mechanics to economics (game theory), while Gödel’s interest and inspiration came mainly from pure mathematics and philosophy.

Péter András Varga’s reconstructs the early influences on Bernhard (Bernát) Alexander at the University of Vienna in 1868–1871. Alexander was an eminent scholar, later to become a major figure in Hungarian intellectual life: by the turn century he became a respected university professor, public writer and art critic,



a member of the Hungarian Academy of Sciences, serving also as the President of the Hungarian Philosophical Society. The paper investigates the influences Alexander received at the first station on his peregrination at the University of Vienna. This is interesting for it informs the reader both about the early formation of Alexander's thought as well as it provides insights into the philosophical scene in Vienna around 1870, before Brentano's arrival – hence presenting one of the rare intersections between the history of Austrian and Hungarian philosophy. The paper is supplemented with a document, an excerpt from Alexander's intellectual diary from the Vienna period, edited and introduced by Barnabás Szabados, Bettina Szekér and Péter András Varga.

*Gergely Ambrus – Friedrich Stadler*



UNITY AND TENSIONS  
IN AUSTRIAN PHILOSOPHY



## Brentano on Perception

### I. THE STANDARD VIEW

Thanks to his account of mental acts, Brentano is usually acknowledged as the philosopher of intentionality. What characterizes mental acts is their intentionality, that is, their directedness towards an object (Brentano 1874–1973. 68–124). Another important contribution of Brentano to contemporary philosophy lies in his conception of consciousness. In his view, mental acts are not only characterized by their intentionality with regard to their objects, but are also concomitantly self-directed (ibid. 180/98). This self-directedness is what makes them conscious.

Since intentionality and consciousness are two central marks of the mental, they also apply to perceptual acts as well. An act of sensory perception, insofar as it is mental, is intentional and conscious. It is worth noting, however, that while many philosophers have acknowledged in recent years the intentionality mark for the mental, the consciousness mark is rarely challenged.<sup>1</sup> This perhaps explains in part why Brentano's account of perception has received so little attention in the secondary literature. If, following his view, perception has to be intentional *and* conscious, then it seems that the conditions for any mental state to be a perception are very strict, perhaps too strict: we may want to say that there is *always* an (intentional) object in every perceptual act, but we may want to dispute that every perceptual act is *therefore* also conscious. Or conversely, we may want to say that every perceptual act is *conscious*, but we may want to dispute that every perceptual act *therefore* has an (intentional) object.

Another possible explanation for the recent lack of interest in Brentano's philosophy of perception may be found in one common interpretation of his conception of intentionality, according to which the objects of intentional acts are

<sup>1</sup> See Textor 2017 on disputing the intentionality mark.

immanent objects, that is objects that have “some kind of reality in the mind”.<sup>2</sup> Following this interpretation, if intentionality is the mark of the mental, then perception is nothing but a special case of intentionality, understood as a relation between a mental act and an immanent object. In other words, following the common interpretation of Brentano’s conception of intentionality, what one perceives is merely an intentional object that is an object in the mind; it is not an ordinary spatiotemporal object. On this interpretation, it seems as if Brentano would defend a view of perception along the lines of the argument from illusion.<sup>3</sup>

Following this common interpretation, it seems at first glance that Brentano’s account of perception would fall somewhere between phenomenalism and idealism, not only concerning perception, but thought as well. It remains disputable, however, whether what Brentano calls the intentional relation really is nothing more than a relation to a sense-datum (or, in the case of thought, to an idea), and whether perception, in his account, has to be understood as a special case of intentionality. Concerning the first point, we should bear in mind that in his later writings, he insisted on calling intentionality something “relation-like” (*etwas Relativliches*), abandoning the idea that it is a relation in the proper sense. Concerning the second point, even in the *Psychology from an Empirical Standpoint*, he stresses many times the point that there are external objects causing our so-called “physical phenomena” (the seen blue, the heard sound, the seen landscape, etc.). Since standard phenomenalist or idealist theories would not require this further premise, they would not likely bring it in. If there is an external world producing or causing our physical phenomena, as Brentano suggests, how is this suggestion understandable if the external world is given to us exclusively in a perceptual relation, understood as an intentional relation between a mind and its immanent object?

One way of understanding this suggestion in the framework of a conception of perception as a particular case of intentionality – understood as a relation be-

<sup>2</sup> At least following one common reading of “intentional inexistence” propounded most notably by Chisholm 1967 and Smith 1994.

<sup>3</sup> Hume 1748 had a first version of the argument. Based on Smith 2002, Crane and French 2016 propose the following reconstruction:

- (i) In an illusory experience, it seems to one that something has a quality  $F$ , which the ordinary object supposedly being perceived does not actually have.
- (ii) When it seems to one that something has a quality  $F$ , then there is something of which one is aware which does have this quality.
- (iii) Since the ordinary object in question is, by hypothesis, not- $F$ , then it follows that in cases of illusory experience, one is not aware of the object after all.
- (iv) The same account of experience must apply to both veridical and illusory experiences.
- (v) Therefore, in cases of veridical experience, one is not aware of the object after all.
- (vi) If one is perceptually aware of an ordinary object at all, it is in either a veridical or illusory experience.
- (vii) Therefore, one is never perceptually aware of ordinary objects.

tween a mind and its immanent object – is to consider it highly improbable that our sensory contents are not produced by anything in the physical world. Rather, it is highly probable that our sensory contents *are* produced by something physical (atoms, particles, energy fields, or forces). But probability, even very high or infinite probability, is not evidence. Since evident perception, that is, inner perception, is apparently for him the concept of perception *par excellence*, then there is no proper perception of the external world. Brentano often make this point or similar points.<sup>4</sup>

Understanding perception exclusively in the strong sense of evident perception, and as a particular case of intentionality (understood as a relation to an immanent object) seems to lead to a reading of Brentano in which the objects perceived are mere perceived contents or possibilities of sensations, a reading close to Mill's (or even Berkeley's) phenomenalism.<sup>5</sup> This too may help explain why Brentano's account of perception, given the common interpretation of his conception of intentionality, has received so little attention: if Brentano's account of perception is understood as it has usually been interpreted, then it is not meaningfully different from the account already offered by phenomenalist and idealist theories. In this case, it would be entirely understandable why Brentano's account has been neglected.

But even if we accept this reading of Brentano's account of perception, there is an important difference between Brentano's account of perception and Berkeley's, Mill's, or Mach's. As we have already emphasized, while Berkeley considers physical objects in terms of sense data, the existence of which depends upon their being perceived, and invokes God's perception for filling the gaps for cases where we do not actually perceive anything, Brentano does acknowledge that the world exists independently of our perception of it. He simply raises serious doubts about the idea that we perceive it exactly as it is. He thus avoids a position such as Mill's, where the permanent possibility of sensations accounts for the fact that physical objects are not always perceived. He also avoids Mach's phenomenalism by stressing the ontological distinction between the mental and the physical, which Mach rejects.

Thus, the common interpretation of Brentano's account of perception as a form of phenomenalism is not particularly plausible, even on the standard reading. His position, according to the standard reading, therefore contrasts with

<sup>4</sup> See for instance Brentano 1874/1973. 11/9; 128 ff.

<sup>5</sup> Jacquette 1996. 138 and 1990. 179 ff. suggests that it was the immanent intentionality thesis that led Meinong, Höfler, and Twardowski to introduce the content–object distinction and, by it, to abandon the “self-enclosed idealism implied by Berkeley's empiricism” (1996. 138), which was characteristic, in Jacquette's view, of Brentano's conception of intentionality. I explain why this historical reconstruction provided by Jacquette is problematic and why Brentano did not defend the immanent intentionality thesis in the way suggested by Jacquette and many others in Fréchette 2017.

idealism and phenomenalism. It seems to suggest that he would defend a view similar to critical realism. But again, this is not the case: against Locke, Brentano doesn't distinguish between primary and secondary qualities concerning their relation to the observer: for him, extension and colour are given on the same basis in perception. If Brentano sides neither with Locke nor Berkeley, neither with Mach nor Mill, how should we understand his position? Here, defenders of the standard reading have divergent opinions, but since perception seems to be the *enfant pauvre* of Brentano's theory of intentionality in the standard reading, scholarly discussion has been relatively sparse.<sup>6</sup>

However, the standard reading of Brentano – according to which he believes that intentionality is a relation to an immanent object, and perception is a special case of intentionality – has a grain of truth, at least insofar as there are many passages from the *Psychology from an Empirical Standpoint* that seem to support this reading. But as mentioned above, there are obvious problems with this reading when it comes to Brentano's supposition of an external world directly responsible for what we see, hear, etc. Furthermore, Brentano's criticism of phenomenalism<sup>7</sup> makes it difficult to champion a reading on which he appears to defend a variety of this same phenomenalism.

In short, the common reading of Brentano's thesis on intentionality attributes to him a suboptimal account of perception which does not fit with his critique of phenomenalism. Furthermore, it suggests that Brentano should be seen as a defender of the argument from illusion. But if causality is a relation that, according to him, operates between the external world and physical phenomena, and if the external world is not a simple theoretical posit but something of which perceiving agents are parts, then there must be a way in which, as perceiving agents, we are after all related with the external world.

## II. TENETS OF THE STANDARD VIEW

In order to address this issue, let us summarize in a few general theses the gist of Brentano's conception of perception according to the standard interpretation.

### *T1: Perception is a special case of intentionality*

T1 is simply a repetition of the common interpretation of Brentano's theory of intentionality, according to which intentionality is a relation to an immanent ob-

<sup>6</sup> Brentano's account of perception has been directly or indirectly discussed recently in Mulligan 2004, Textor 2007, Fisette 2011, Seron 2017, 2017a and Massin 2017.

<sup>7</sup> See for example Brentano against Mach (Brentano 1988), but also Brentano's lectures on positivism from 1894–95 (Brentano 1894–95), where he defends the view of a correlation between the seeing and the seen (against the identification proposed by Mach), advocating at the same time for the irreducibility of causality.



ject. Since all mental phenomena are intentional in Brentano's view, and since perceptual experiences (hearing a sound, seeing a colour, etc.) are mental phenomena, it follows that all perceptual experiences are intentional.

*T2: Perception is of something that truly exists*

T2 is a foundationalist thesis insofar as it restricts the use of "perception" to the perception of things that *truly* exist. If only mental phenomena *truly* exist (this thesis is expressed in the basic idea that physical phenomena exist only intentionally (or better: inexistent) in the mind, while mental phenomena truly exist), and if perception (*Wahr-nehmung*) is, by definition, perception of something that *truly* exists, then only *inner* perception (that is perception of mental phenomena) is perception in the relevant sense of the term.

T2 imposes obvious epistemological restrictions on the application of the term "perception": if there is a strong sense of perception in which what we perceive is what truly exists, then only inner perception is perception in the true sense (Brentano 1874/1973. 119–170). Following the standard account, this thesis may explain Brentano's rejection of Berkeleyan idealism, Machian phenomenalism, and Lockean realism, since it acknowledges that there is a domain of what it innerly perceived, which is perceived *as it is*.

*T3: What we truly perceive is a mental-phenomenon-containing-something*

T3 addresses in part the issue that was left undetermined in T2, namely the actual contents of so-called sensory perception. Brentano comes to T3 from the following premises: (a) only mental phenomena truly exist (i.e. only mental phenomena are objects of *inner* perception); and (b) objects of mental phenomena are inexistent objects (colours, chairs, landscapes, etc. as "intentionally contained" in the mental phenomenon). Therefore, what we "truly" (or *innerly*) perceive is what one could call a mental-phenomenon-containing-something. The hyphens here are meant to stress, first, the fact that what is innerly or "truly" perceived is not simply the seeing, the hearing, etc., but the hearing as the hearing of some specific tone, the seeing as the seeing of a specific colour, etc.; and second, that sensory contents are perceived only to the extent that they are intentionally contained in a mental phenomenon, which is the actual object of perception. Sensory contents are only indirectly perceived, so to speak, that is, as part of a mental phenomenon.

### III. THE NAÏVE UNDERSTANDING OF PERCEPTION

On the face of it, these three theses leave no room for anything but a restricted concept of perception, namely, that of inner perception. It is easy, on the basis of T1–T3, to understand why most readers of Brentano take him literally when

he writes at numerous places that only inner perception is perception (*Wahrnehmung*) in the proper sense.<sup>8</sup> Characterizing inner perception as the only kind of perception (and characterizing outer perception as the mere reception of physical phenomena) seems to lead Brentano to reject the naïve understanding of perception (or perceptual experience) in terms of “openness to the world” (McDowell 1994: 112), according to which we are presented, in perceptual experiences, with ordinary mind-independent objects, and that in such experiences we are aware of such objects.<sup>9</sup> This would support an understanding of Brentano’s position as defending the argument from illusion. Following the account at the basis of the three theses, it seems that no mind-independent objects are directly involved in perceptual acts. Moreover, T3 in particular makes it clear that Brentano would reject the transparency intuition that is often shared by philosophers who believe that our experience gives us features of mind-independent objects. In short, it seems that Brentano’s account of perception, following the standard view, cannot account for the basic intuition that perception is *primarily* of something other than itself.

Is this a plausible reading? I doubt it. Taken literally, it would mean that what I truly perceive when I am seeing a barn is not the barn but the seeing. While this view may capture in some way the intuition that we are *aware* of something in perceptual experiences, it leaves out too much from our naïve understanding of perception in order to count as a plausible account of perception. After all, when I see the barn and when I see a church, there are some obvious differences in my perceptions. Cashing out these differences simply in terms of modulations in the seeing implausibly downplays the naïve intuition that these perceptions give me some information (erroneous or not) about the world, not merely indirectly as what is contained in a mental act, but perhaps even directly about the location and various features of certain objects. If Brentano does reject the positions of Berkeley, Locke, Mill, and Mach on perception, then he should have more to say about this naïve intuition than simply dismissing it. He ought to acknowledge some kind of perceptual process through which my sensory organs gather information (both correct and incorrect) about my environment. The existence of such process could hardly be denied if the hypothesis of an external world is to be justified at all.

Although T1–T3 plausibly explain the lack of interest in Brentano’s account of perception, they are neither a plausible rendering of Brentano’s view of perception, nor are they compatible with some important insights by Brentano on

<sup>8</sup> Ibid. This was already the case with Husserl in the *Logical Investigations* (Husserl 1901/2001), who set the tone for the interpretation of Brentano in the phenomenological tradition, in Heidegger 1992[1925], 46. for instance, and later on in Føllesdal 1969, 680–681 and Jacquette 2006, 107, among others. See Hickerson 2007, 42 ff. for a discussion of the problems raised by this reading.

<sup>9</sup> On awareness, see Crane and French 2016.

the nature of perception that are rarely discussed in the secondary literature. Although it is true that for Brentano, inner perception has a priority over outer perception in the order of investigation, this priority does not imply that there is no outer perception properly speaking, or that “perceiving your sensing” is the only case of perceiving. In the rest of this paper, I will argue that T1–T3 are meant to provide an account only of *inner* perception: that they are meant to provide instances of “good” perception, not of perception in general.

#### IV. TWO OPTIONS FOR THE STANDARD VIEW

For a defender of the standard view, there are two main options in interpreting Brentano’s theory of perception, both of which would account for the idea that truly perceiving the barn is actually perceiving the seeing (which contains, in some special way, the barn as its intentional object). The first option is a relational account, which can be spelled out in two different ways. (1) First, one could argue that we directly perceive mental images (or physical phenomena, in the Brentanian sense) which are dependent on the mind, and that these have the properties that perceptually appear to us. Such a view basically amounts to a sense-data theory. We have already seen that Brentano would not endorse such a view in the framework of phenomenism.<sup>10</sup> The problem with such an account is that it introduces a veil of perception which makes our relation to the world highly problematic. Here again, it would make Brentano a defender of the argument from illusion, which does not fit with his critique of similar positions.

(2) Second, one could also try to argue for the relational account in terms of some variety of representationalism or intentionalism, conceiving of perception as a special kind of relation between one’s mind and the intentional object, mediated by the representational content. Crane (2009, 2009a, 2013) defends a similar view, though he maintains that his view is *not* relational as such: I can represent a golden mountain although there is no such thing; However, he seems neutral as to whether it actually fits with Brentano’s. Following this view, in perception a given object seems to me in a particular way: the “seeming to me in a particular way” can be explained in different ways. It might be explained in terms of representational content alone; for example, I see the barn as an old and unoccupied brownish building in the middle of the field. It may also be cashed out, at least partly, in terms of the mode or attitude of a specific experience:

<sup>10</sup> Of course, there is another option that is at least technically open: one could also accept the sense-data theory without accepting phenomenism, as in causal theories of perception for instance (e.g., Price 1932). But such theories are usually designed as a justification of our belief in the external world. Brentano’s account, however, both in the standard view and in the view argued for here, takes our belief in the external world to be primitive and unjustifiable.

seeing the barn is in this respect a different experience from merely imagining or remembering it. Independently of the question whether or not the mode or attitude plays a role in determining the phenomenal character of an experience, a representationalist account of Brentano's position should lead one to consider perception as (at least partly) determined by the representational content, that is, by the physical phenomenon. There might be an object which is represented – there might actually be such a barn in the field – but the experience represents a barn not in virtue of the existence of such a barn, but rather in virtue of being more or less accurate: for instance, an experience such as seeing the barn as floating above the field is likely to be less accurate than an experience such as seeing the barn as standing on the field.

Whether Brentano would agree that representations (or rather, presentations, *Vorstellungen*) represent in virtue of being more or less accurate can remain an open question for now, but if intentionalism is an option for the standard view, then it seems that only judgements of inner perception (of the form "Seeing exists", for instance), and not presentations per se, have correctness conditions and can be assessed for accuracy. Intentionalism therefore seems (at least on the face of it) not to be a real option for the standard view.

Even if we put this concern aside, it is also questionable whether Brentano would agree that representations represent in virtue of being the bearer of some semantic information, which is an essential component of a representationalist or intentionalist account. In the best case, intentionalism would fit only loosely with the standard view: Brentanian physical phenomena, in the standard view, are not really bearers of semantic information: they are not representational, and they are not, properly speaking, *about* the world in the sense that my seeing is about the "green as perceived." Certainly, Brentano sometimes calls them "signs of something real" (Brentano 1874/1973. 24/14) in a way which evokes Helmholtz's theory of perception, but unlike Helmholtz he rejects the idea that these signs carry information about the actual localization of the external stimulus, information which according to Helmholtz is processed by unconscious inferences.<sup>11</sup> In short, Brentano's physical phenomena are signs of an outside reality, simply on the (highly probable) assumption of the existence of an external reality; however, if one sticks with the standard view, they do not seem as such assessable for accuracy, nor do they *represent* something else.

Finally, and most obviously, intentionalism cannot account for the non-distinction view between content and object which is presupposed by the standard view.<sup>12</sup> In the intentionalist account, intentional objects are *not* identical with the contents of mental acts, as presupposed by the standard view.

<sup>11</sup> See Brentano 1979. 69 for a critique of Helmholtz's position. More on this below.

<sup>12</sup> I discuss the non-distinction view and proposes an alternative based on Brentano's view in his lectures on descriptive psychology in Fréchette 2017.

For these reasons, a relational (in this case representationalist) reading of Brentano's views on perception seems not to be very helpful for the standard view. Against such a reading, one can favour a non-relational reading of perception along the lines of adverbialism. According to this account, intentionality is quasi-relational, that is, the intentional content of one's mental act should be understood as a property of the perceptual experience itself rather than as some kind of object with a particular kind of existence. According to adverbialism, I do not see coloured objects, since colours are strictly phenomenal properties (and such a view fits well with Brentano's own view of colours). On this view, there is a common core between my seeing a yellow truck and my hallucinating a pink elephant, for in both cases phenomenal properties appear in the same way. The main problem with the application of this account to Brentano's views on perception is that while it fits well with his reism, in which *irrealia* are banned from the ontology (and therefore we present things in this or that way), it cannot account for the idea that what is presented are *intentional objects* (and not merely modes of presenting), and that these are in some relation with the outer world (not as representations, but as signs). If we consider Brentano's reism as his final word, not only in ontology, but in perception as well, then adverbialism may have some potential, but it entails the rejection of T3; adverbialism therefore seems not to be a real option for the standard view.

Thus, it seems that the only way to make sense of the standard reading of Brentano's view of perception is the relational account. It involves either ways however serious reconstruction under theoretical presuppositions that are not always plausible; this suggests that the alleged three tenets on perception (T1–T3) are perhaps giving a wrong picture of Brentano's actual views on perception.

## V. THE BACKGROUND TO BRENTANO'S VIEWS ON PERCEPTION

To give a plausible reconstruction of Brentano's view, it might help to take a quick look at the background to his views on perception and his take on perceptual illusion. Let us start with the background. There are a few central ideas from the history of the philosophy of perception that played an important role for Brentano's views. First, the Augustinian view distinguishing between higher and lower (sensory) perception already plays a role in the account developed in *Die Psychologie des Aristoteles* (Brentano 1867). In this context, sensory perception has a limited role: it is possible only through the active act of the soul, and not through bodily sensation alone (*neque enim corpus sentit, sed anima per corpus*). There are representations (*similitudines*) on the basis of the information (*informatio*) sent by the organs to the soul. *Intentio*, based on *informatio*, is our identifi-

cation of the object perceived.<sup>13</sup> Like Augustine, Brentano believes that sensory perception as such is possible only on the basis of an active act of the soul, or in Brentano's conception, on the basis of inner perception. We find a similar idea in Descartes (*c'est l'âme qui sent, et non le corps*),<sup>14</sup> with whom Brentano agrees (even against Aristotle!) at many places.<sup>15</sup> In these cases, ideas (representations) are isomorphic figures, pictures, or (as Brentano calls them) signs produced by external stimulation.

In all these cases – the distinction between lower and higher perception (Augustine) or the distinction between the stimulation on the retina and the produced images in the soul (Descartes) – one finds the idea that visual sensations, for instance, which are produced by the stimulation on the retina, are not by themselves responsible for our seeing; in order to really *perceive* the blue patch of colour in front of me, an active act of the mind is necessary. In Brentano, this act is called a presenting (*Vorstellen*). Against Reid, Brentano would refuse to say that the presenting and the presented are only “grammatically” distinct.<sup>16</sup> A presenting really exists, while a presented is strictly phenomenal and merely “inexists” intentionally in the presenting. On this account, Brentano obviously advocates for the ontological priority of acts (like presentings) over their objects (the presented); in other words, it seems that he argues that the being of acts of presentation is a condition for the inexistence of physical phenomena. If “sensory perception” designates the reception of the nerve signal produced by the stimulation of the sense organ, which is experienced as “having a physical phenomenon”, then it seems that for something to count as sensory perception, there must be a conscious mental act which is intentionally directed at the physical phenomenon. This would also explain Brentano's rejection of external perception (sensory perception taken in isolation from the acts in which we are conscious of it) as *Falschnehmung*.

This reading of the relation between mental and physical phenomena in terms of the ontological priority of the former over the latter has the consequence that one would have to admit that there could still be sensory perception in a relevant sense even without any external stimulation of the sensory organs. This does not challenge the intentionality thesis, since Brentano accepts cases where we have physical phenomena that are not produced by external stimulation, as we will see below.<sup>17</sup> But even if one accepts the ontological priority of the

<sup>13</sup> On Augustine, see Caston 2001. 33 ff.

<sup>14</sup> Descartes, *Dioptrique*, Discours IV, A.T. VI, p. 109 (Descartes 1902. 109).

<sup>15</sup> For instance Brentano 1975[1916]. 13 where he agrees with Descartes on this point against Aristotle, and praises Reid for doing the same.

<sup>16</sup> Reid 1895 [1764]. 182 ff. Compare Brentano 1975. 4.

<sup>17</sup> The other consequence of the ontological priority reading is that organisms with no mental phenomena (if there are such things) would be deprived of perception. Brentano however accepts this consequence. In his view, animals have no general concepts, and hence no higher intellectual activities: they only have sensations, affects, memory, and associative

mental over the physical, Brentano's point seems rather to be that perception in the strong sense of T2 cannot be accounted for strictly in terms of physical phenomena produced by external stimulation of the sense organ, since we have no evidence that these phenomena accurately depict external reality.

This line of thought about sensory perception may seem anti-realist to a significant extent, and when we put it in the context of its times, it obviously follows some important insights on sensory perception developed by Helmholtz under the influence of Johannes Müller, who can be labelled as anti-realist as regards the nature of perception. Müller (1837) thought that his law of specific nerve energies, according to which every sensory nerve reacts specifically and differently (as a light nerve, a sound nerve, a smell nerve, etc.) to a stimulation  $s$ , had the consequence that sensory perception is not perception of a quality of an external body, but of a quality of our nerves. This suggests that sensations cannot be seen as copies of external objects, but rather that they have a representational nature. This idea was also followed by Helmholtz, who argued that contents or sensations are rather *signs* that “completely depend on our organization” (Helmholtz 1878. 225 f.). Consequently, Helmholtz argued, perception should be seen as the result of this interpretation, this result being sometimes obtained through unconscious inferences.

Brentano accepted Müller's conclusion in his account of perception: it is not the quality of the external stimulation that determines sensation, but the specificity of the stimulated sensory organ. But does Brentano accept this simply on the basis of T2? In order to answer this question, it might be helpful to recall the views of Helmholtz and Hering, which both influenced Brentano to different extents. According to Helmholtz, Müller's law also confirms that there is a distinction between sensation and perception. Sensations are produced by the stimulation of the nerves and are fully specified, following Müller's law, by the specific characteristics or modalities of the sensory organs; nevertheless, we do interpret our sensations as giving us information about the position and form of objects in space (1867. 427). This interpretation is what Helmholtz calls “perception”. Perceptions, and only perceptions, are mental acts: sensations merely provide the material upon which perception operates.

Hering, on the other hand, rejects the distinction between sensation and perception. For him, the spatiality of our sensations is not something superimposed by the “perceptions” of Helmholtz; rather, spatiality (or a sense for spatiality) is built into sensings themselves. Hence, sensations are not unorganized raw material, but sensing itself, as an activity, has access to spatiality as a primitive

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processes. Sensations being mental phenomena, even animals have perception in the strong sense of T2, although to a very limited extent in comparison with humans. In the manuscript “On the Soul of Animals” (*Von der Tierseele*, Ps 18), dated 1903, he even goes so far as to leave open the possibility of substances having mental activities (Brentano 1903. 50185–6).



quality of what is given in sensations. Hering has no need for a further concept of perception as does Helmholtz, and can accommodate Müller's law by simply adding that objectual space, the space of objects, is something that we think on the basis of our experience and of our inferences. We *see* the trees in a row of trees as being bigger from a short distance, and getting smaller at a greater distance, but we *think* them as being of relatively equal heights. Characterizing this "thinking" as a perception, as does Helmholtz, suggests that in vision itself, for instance, purely hypothetical thought-like processes are involved (e.g., Helmholtz's unconscious inferences), a consequence rejected by Hering.

Where does Brentano stand? Like Hering, Brentano seems to draw the conclusion that Müller's law shows that a distinction between perception (of external objects) and sensation is superfluous. Sensations are specifically and spatially determined, and so is outer perception. According to him and similarly to Hering, I *see* the Müller–Lyer lines as being of equal lengths, but I think (or judge) them as being of unequal lengths. As far as outer perception is concerned, Brentano follows Hering's reading of Müller and rejects the distinction between perception and sensation. But in contrast to Hering, Brentano still wants to argue for perception as a mental process different from sensation (sensory stimulations). This view is expressed in T2, in which perception (i.e. *inner* perception) is only of something that truly exists. This explains the restriction made that the only veridical perception is inner perception (i.e. the perception of one's own mental acts).

In other words, Brentano wants to stress the two following points. First, perception in the strong sense of T2 is not to be confused with the reception of sensory stimulation which we experience as physical phenomena. Second, the distinction between perception and sensation does not take place at the level of sensory stimulation and its processing (as Helmholtz would have it). Rather, sensings themselves already provide information about quality and localization; this information is not processed in a further step, called "perception" by Helmholtz. Therefore, in order to avoid misunderstanding, the term "perception" should be reserved for "inner perception".

At bottom, this second point seems more terminological than philosophical. Brentano made this exact point in 1889:

The term "perception" has degenerated in an almost similar way [to the term "pleasure"]. Only really appropriate in respect of knowledge, it came to be applied in the case of the so-called external perception – i.e. in cases of a belief, blind, and in its essential relations, erroneous – and consequently would require, in order to have scientific application as a *terminus technicus*, an important reform of the usual terminology, one which would essentially narrow the range of the term (Brentano 1902. 83).<sup>18</sup>

<sup>18</sup> The English translation here (and in many other places) uses "impression" instead of "perception" as a translation of *Wahrnehmung*. I have corrected the translation here.



What this terminological remark suggests is that “perception” as a technical term simply covers too much. While Brentano prefers the traditional, Cartesian use of “perception” to designate cases of self-evident knowledge, and only such cases (the German *Wahrnehmung* suggests it more clearly than its French or English equivalents), he does not deny that we have some kind of access to the external world. He simply points out a terminological confusion arising from the use of a single term to designate two different processes. This point should not be taken as denying any kind of access to the external world. What I have called above “sensory perception”, in the broader or naïve sense of openness to and/or awareness of the world is not challenged in any sense by this remark.

But even if taken strictly in the terminological sense, Brentano’s remark is not without problems. First, the use of the term “perception” in late-nineteenth and early-twentieth century philosophy and psychology goes almost unanimously against Brentano’s suggestion. Even Brentano’s own students rejected the suggestion and used “perception” or “external perception” in the broader or naïve sense in which we used it above.<sup>19</sup> Assertions like “strictly speaking, so-called external perception is not perception” (Brentano 1874/1973, 70), which soon became emblematic of Brentano’s conception of perception, should therefore be taken with a pinch of salt.<sup>20</sup> In fact, the terminological remark on the use of the term “perception” seems not to be principled; rather, Brentano seems to stress in his later published works the terminological point against the use of “perception” by Helmholtz, Helmholtz’s student Wundt, and those who were influenced by them.<sup>21</sup>

In fact, in many texts Brentano does account for “outer perception” in terms which are quite comparable to those used to qualify perception as we are considering it here, as openness to and/or awareness of the world.<sup>22</sup> He argues that by association we use the term “perception” both for cases of intuition (*Anschauung*) and for states which are characteristic of the occurrence of such intuitions.<sup>23</sup> At

<sup>19</sup> See for instance Stumpf 1939, 207 ff.; Bergmann 1908, 9 ff.; Marty 1908, 121; Twardowski 2016 [1895], 201 ff.

<sup>20</sup> One of the reasons why it received an emblematic character is certainly Husserl’s point in the *Logical Investigations*, in which he says, quoting this exact same phrase, that Brentano never should “have said of inner perception [...] that it is really the only sort of perception in the true sense of the word” (Husserl 1901a/2001a, 239/345).

<sup>21</sup> See for instance Brentano 2009 [1896], 131, 148; [1897], 51.

<sup>22</sup> See for instance Brentano 1956 [ca.1884], 144: “In outer perception, we are directed towards physical things, colours, sounds, smells, tactile qualities, etc. In short, towards something qualitative and sensory. Since it is something physical, it should be located (if it exists at all) in the external world. For this reason, we locate for instance a green colour, that we see, on a particular object of the external world, and we say that the tree is green” (my translation).

<sup>23</sup> See Brentano (forthcoming: 53046): “By habitude, the name [perception] is closely associated with both the intuition under which it should properly be conceived and the different states which are typical for the occurrence of this intuition (since these states come always or most of the time along with this intuition)” (my translation).

bottom, taking the background of his views in consideration, we can summarize Brentano's views on perception with these three general ideas: (i) there is a general meaning of "perception" according to which it characterizes our openness and/or awareness of the world; (ii) inner perception is the only case of perception in which all cases of perception are cases of self-evident knowledge (and all these cases are exclusively cases of awareness); and (iii) outer perception is typically a case of perceptual experience in which physical things of the external world appear to us. All these cases are exclusively cases of openness. I will argue for the third idea in more detail in the next section.

## VI. PERCEPTION AND ILLUSION

If we restrict the application of T2 to point (ii) mentioned above, it leaves open the possibility of accounting for perception in the naïve sense of openness and awareness of the world in Brentano's conception. One obvious way of doing so would be to look at is conception of the physiology of perception. As I suggested above, Brentano's position on psychology, physiology, and perception in general in the *Psychology from an Empirical Standpoint* is determined to a great extent by the natural sciences of his times. In Brentano's case, it was the positivism of Comte that played the most important role. Scientific philosophy, in its "positive explanations – even if they were the most perfect ones – never claims to expose the producing forces of phenomena [...] [it] simply seeks to analyse with exactness the conditions of their emergence and to connect these conditions through law-like relations of succession and similarity" (Brentano 1869. 23). Comte's positivism was for him the reference model for the natural sciences, but it was not in principle incompatible with the Kantian idea of a scientific explanation in the form expounded by Helmholtz (1847), as Brentano himself concedes.<sup>24</sup> In other words, what Brentano rejects in Kantian philosophy of science is its constructivism, especially as applied in Helmholtz's concept of perception. Space is not a form of our intuition; it is a quality given in sensory perception. He would definitely reject the idea that "we can never perceive matter in itself, but only through its forces" (Helmholtz 1847. 4).

In 1874, this attitude seemed to mean for Brentano that psychology as a science of the mental should restrict the talk of intentionality (the relation between the mind and the objects perceived) to the domain of inexistent objects; that it should restrict the talk of consciousness to the domain of mental phenomena; and that it should restrict the talk of perception (in the sense of evident knowledge) to the domain of mental phenomena. Restricting the talk of intentionality, consciousness, and perception (in the sense of T2) to the realm of the mental

<sup>24</sup> See Brentano's concession to Kant in Brentano 1874–1973. 128, fn2/76.

seems, however, to be more a terminological restriction guiding the application of the right concepts to the right processes than the radical thesis often attributed to him that psychological knowledge does not apply to any other processes. It should therefore not be confused with the Kantian restriction of knowledge to phenomena. The former is motivated by the limits of our *evident* knowledge, while the latter is motivated by the alleged limits of knowledge *tout court*.

That psychological knowledge applies to processes other than intentional acts, conscious acts, and perceptual acts in the sense of T2, is clear when Brentano talks about the “conditions of emergence” of mental and physical phenomena, which are also part of psychological investigation. In the 1880s, Brentano gave to the investigation of the conditions of emergence the label “genetic psychology”. That these investigations are not undertaken in the published version of the *Psychology from an Empirical Standpoint* has mainly to do with Brentano’s abandonment of the initial plan to publish a series of six books, which started with the *Psychology* as we know it, not with any principled concern about the object of psychology.

The nature of mental and physical phenomena and their “conditions of emergence” are two different standpoints on objects that, eventually, could turn out to be identical.<sup>25</sup> Sensations (or sensings, presentings, etc.) as mental phenomena are not necessarily objects that are distinct from those studied by physiology. They may even be the same objects studied from different perspectives. Nor does it necessarily follow, because sensory objects do not “truly exist” that they have no substantial role to play in understanding perception. As we pointed out earlier, it would also be wrong to think that Brentano would reject the distinction between subjective sensations (e.g., hallucinations) and objective sensations (externally stimulated sensations) simply because this distinction is not systematically accessible in inner perception (more on this below).<sup>26</sup> Although descriptive classification has priority over genetic investigations in psychological research, it is not meant to override any genetic classification. In fact, when it comes to investigating the nature of perception, Brentano’s descriptive classification takes a surprising but revealing turn. Think of the explanation of the Müller-Lyer illusion that Brentano championed: on the face of it, his explanation follows the thesis defended by Helmholtz (1867. 566) of the perceptual overestimation of wide angles and the underestimation of narrow angles. But

<sup>25</sup> Brentano was a dualist, but took great pains in developing a theory of the mind that could still be true if, by any chance, materialism turned out to be true. In his lectures on the immortality of the soul from 1875/76, he stresses the following point (Brentano 1875. 29586): “Therefore, one should always and in every case consider as a factual unity (*sachliche Einheit*) the totality of the mental activities that we innerly perceive. Thus the soul is not a collective, not a group of atoms of which we could apprehend the disintegration. Rather, from the standpoint of the hypothesis which we formulated, if the soul is material, then it is a unitary atom and thus, like all atoms, it is incorruptible” (my translation).

<sup>26</sup> On this distinction, see Brentano 2009. 155 ff.

this similarity is only superficial. Helmholtz's model of explanation belongs basically to the category of physiological theories of illusions. Such models provide an explanation of the illusion on the basis of a disturbance in the information channels: it is merely the result of a physiological disturbance, which we describe as the overestimation of wide angles and the underestimation of narrow angles.<sup>27</sup>

While Helmholtz's model is based on the supposition of physiological disturbances, Brentano's model seems to be based on the supposition of an inappropriate application of the signalled information. Indeed, Brentano considers the Müller–Lyer illusion a case of “illusion of judgement” (*Urteilstäuschung*). In his view, this illusion of judgement is not to be confused with illusions “in which our phenomena do not correspond to the objectively given” (2009, 25). The broken stick illusion is such an illusion in the latter sense, and it is *not* an illusion of judgement, while the Müller–Lyer illusion is based on “a false evaluation of relations given phenomenally” (ibid.).<sup>28</sup> The optical paradox emerges because the judgement that the lines are unequal conflicts with the initial phenomenon in which the lines are of equal lengths.

It is quite remarkable here that both sorts of illusion presuppose a distinction between the objectively and the subjectively (or phenomenally) given. Müller–Lyer cases are such that the subjectively given actually matches the objectively given (two lines of equal lengths), but the paradox comes from the wrong judgemental evaluation of the subjectively given. In other words, the paradox comes from our *rejection* of ( $a_{\text{subj. given}} = a_{\text{obj. given}}$ ), where  $a$  stands for the lines of equal lengths. Broken stick cases are such that the subjectively given simply does not match the objectively given; the paradox here comes from the *acceptance* of ( $b_{\text{subj. given}} \neq b_{\text{obj. given}}$ ), where  $b$  stands for the unbroken stick. In the first case, the paradox arises only at the level of the judgement, while in the second case, it seems to come from a conflict which is intrinsic to the given itself.

It is also quite remarkable that Brentano here uses the term “the given” (*das Gegebene*), which is quite unusual in his vocabulary. What he means by “objec-

<sup>27</sup> On these theories, see Gregory 1970, 142, who labels them “physiological confusion theories”.

<sup>28</sup> In the phenomenology lectures of 1888/89, Brentano is a little more explicit on this distinction: “[Optical illusions] are of two sorts: (1) of the sort like when a stick in water appears broken, or an object appears misplaced in a mirror. Here, we have a real modification of the phenomenon; but this modification is caused by light waves which make their way to me in an unusual manner from the body from which they are sent and make me conclude to the existence of the object. [In this case], habitude leads me to deceptive hypotheses on its position and form. If I contented myself in designating the phenomenon as a different one, I would make no mistake. (2) The cases are different when I deceive myself about the subjective phenomenon itself; when it appears to me for example modified in a certain way, while the phenomenon is unmodified. [...] [This is the case with] the Zöllner figures. The appearance is so powerful that the modification of the phenomenon could barely be said to be more powerful. Even knowledge doesn't suspend the appearance.” (Brentano-forthcoming-2, 59032.)

tively given” and “subjectively given” is sometimes also described in terms of objective and subjective sensations. In his phenomenology lectures from 1888/89, he lists under subjective sensations the presentations of fantasy, but also the sensory feelings, the muscular sensations, reflex sensations, sensations of darkness, after-images, simultaneous contrast, and concomitant sensations. These sensations have a common and complex cause: they are the result of the conjunction, according to his student Marty, between innate and acquired dispositions.<sup>29</sup> And most importantly, they are not caused by external stimulation. Only objective sensations are caused by external stimulation.

Given this distinction, the Müller–Lyer case would be a case in which (a) I have an objective sensation of the lines as of equal lengths; (b) the subjectively given is identical with the objectively given; (c) I incorrectly reject the identity in (b). The broken stick case would be a case where (a) I have an objective sensation of the stick as unbroken, which (b) is not identical with the subjective sensation of the stick as broken and (c) I correctly accept that (a) and (b) are not identical.

There are two obvious questions here. First, how do we know that objective sensations are always accurate (i.e. that the nerve signal which we experience as a physical phenomenon is produced by the appropriate external stimulation)? If objective sensations are always correct or appropriate signs of external reality, then we must admit that it is at least possible to directly perceive (in a relevant sense of “perception”) external reality (ordinary mind-independent objects), otherwise the distinction between objective and subjective sensations would be purely arbitrary.

The second question is the following: is the distinction between objective and subjective sensation accessible in inner perception? If it is accessible, then I do have access in inner perception to the source of the stimulation. This would make T1, T2 and T3 false. T1 would be false because the external stimulation cannot be the target object of the intentional relation, and T2 and T3 would be false because if the distinction is accessible in inner perception, then inner perception would not be only perception of what truly exists (mental phenomena), and not only the perception of a mental phenomenon containing something (the physical phenomenon), but it would also give the correctness conditions of outer perception: an outer perception is correct when the external stimulation corresponds to the physical phenomenon, and it is incorrect when it does not correspond.

If the distinction is not accessible to inner perception, then T1, T2, and T3 would be quite implausible or in need of serious improvements. T1 would be implausible if the possibility of perceiving external stimulation is granted. T2

<sup>29</sup> See Marty 1889. Stumpf 1886 also uses the same distinction in his lectures on psychology.

and T3 would not make much sense if one argues that there are correctness or accuracy conditions for outer perception which are not accessible to inner perception (correctness is, after all, something which one experiences in inner perception). If, in outer perception, we are able to discriminate between subjective and objective sensations, it would be implausible to hold that this ability to discriminate disappears in inner perception.

What Brentano's interpretation of the Müller–Lyer illusion suggests is that there are illusions, some of which (like the Müller–Lyer one) are illusions of judgement, but others emerge from a conflict between the subjectively and the objectively given. This distinction is incompatible with the central premise of the argument from illusion, which Robinson calls the phenomenal principle:

Phenomenal principle: “If there sensibly appears to a subject to be something which possesses a particular sensible quality then there is something of which the subject is aware which does possess that sensible quality” (Robinson 1994. 32).

In order for him to agree with the principle, he would have to abandon the thesis that in perceiving the stick in the water, the unbroken stick is objectively given to me. If his take on perceptual illusions gives us an important insight on his conception of the nature of perception, then T1–T3 are simply not a correct rendering of this conception and should be given up.

## VII. THE ALTERNATIVE TO THE STANDARD VIEW

I believe that the grain of truth in the standard view of Brentano's conception of perception consists in the two following claims:

- (1) Understanding perceptual processes in the right way presupposes knowing what is the nature of perception. Hence, descriptive psychology is prior to genetic psychology in the order of investigation.
- (2) In order to investigate the nature of perception, the best place to start is with inner perception, since all cases of inner perception are “good” cases: they show us things (i.e. mental phenomena) as they really are.

However, we have seen here that these claims must be supplemented in order to conform with what Brentano actually says on perception:

- (3) Cases of inner perception should not be taken as paradigmatic cases of perception, as T2 suggests; they are simply instances of “good” cases.
- (4) There might well be cases of outer perception which could be included under the “good” cases<sup>30</sup> – and there definitely need to be a few of them if

<sup>30</sup> Nothing rules out that outer perception could be of something as it truly is: but if there are such cases, then these will not be cases of evident knowledge.

the distinction between subjectively and objectively given is supposed to serve its purpose – but understanding cases of outer perception correctly is a far more complex task, since it requires an empirical investigation of their conditions of emergence.

- (5) Perception in the naïve sense of awareness, that is, in the sense that it sometimes gives us perceptual awareness of ordinary mind-independent objects, is not challenged by Brentano's views. When I veridically see a blue patch, I have an objective sensation which is identical with the subjective sensation; when I see the broken stick in the water, I have an objective sensation of the unbroken stick. Having such a sensation presupposes, by definition, a regularity of the relation between the external object and the sign (the content of the objective sensation), which can be explained only if the possibility of having perceptual awareness of external objects is granted.
- (6) Perception in the naïve sense of openness, that is, the idea that in perceptual experiences we are presented with ordinary mind-independent objects, is not challenged either. The most basic form of perception is presentation (*Vorstellung*), and its presenting mind-independent objects is granted on the same basis as perception in the sense of awareness (5).

If this alternative is correct, it seems that Brentano could agree with the intentionalist tenet that representations (or rather presentations, *Vorstellungen* in his terminology) present or represent at least partly in virtue of being more or less accurate. Such an interpretation, even if it means abandoning the standard view, would be welcome at least for a proper understanding of the motivations and details of Brentano's descriptive psychology, of his realist ontology of his middle period, of his conception of time perception, and of his conception of mental dispositions.<sup>31</sup>

<sup>31</sup> This is an expanded version of "Brentano on Perception and Illusion", to be published in the proceedings of the 40<sup>th</sup> Kirchberg Symposium (C. Limbeck and F. Stadler eds.), which was given in Kirchberg, then in Munich and Guarapuava in 2017. The idea of this paper came from discussions with Marcello Fiocco in Salzburg in 2015/16 (see Fiocco 2017). I thank the Kirchberg audience for its input, especially those who took part in the workshop on Brentano and the Myth of the Given: Marcello Fiocco, Uriah Kriegel, Michelle Montague, and Hamid Taieb for stimulating interactions on the first version, Johannes Brandl for the discussions in Kirchberg and Munich, and Mark Textor for his input in Munich. Thanks also to Evandro Brito, Ernesto Giusti, André Leclerc, Mario González Porta, Gleisson Schmidt, Jean Siqueira, Wojciech Starzyński, and the other participants at Universidade Estadual do Centro-Oeste do Paraná (Guarapuava) for stimulating discussions during the Brentano conference there. This paper was written as part of the research project "Brentano's Descriptive Psychology" funded by the Austrian Science Fund (FWF, P-27215).



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## The Reception of Ernst Mach in the School of Brentano

Franz Brentano is one of the most influential figures in the philosophy of the late nineteenth century. Brentano and his successors have established a philosophical program which had a decisive impact on the history of philosophy in Austria. This program stands out clearly in several lectures delivered by Brentano during his stay in Vienna, particularly in his inaugural address at the University of Vienna (Brentano 1929) in which Brentano outlines the program that he systematically develops in his *Psychology from an empirical Standpoint* (2009). This program was the result of Brentano's research in Würzburg (1866–1873) which has been partly inspired by Auguste Comte's positive philosophy and John Stuart Mill's empiricism (Münch 1989; Fisetite 2018). During his stay in Vienna, Brentano's interest in positivism remained intact as evidenced by his 1893–1894 lectures "Contemporary philosophical questions" in which he examines several versions of positivism, including Mach's version.

This paper is about the reception of Mach by Brentano and his students in Austria<sup>1</sup>. I shall outline the main elements of this reception, starting with Brentano's evaluation, in his lectures on positivism, of Mach's theory of sensations. Secondly, I shall comment the early reception of Mach by Brentano's pupils in Prague. The third part bears on the close relationship that Husserl established between his phenomenology and Mach's descriptivism. I will then briefly examine Mach's contribution to the controversy on gestalt qualities. The fifth part bears on Stumpf's debate with Mach on psychophysical relations and I shall conclude this study with some remarks on Husserl's criticism of Mach's alleged logical psychologism in his *Logical Investigations*.

<sup>1</sup> In a series of papers, I addressed Brentano's relationship with several versions of positivism, namely J. St. Mill (Fisetite, forthcoming), Auguste Comte (Fisetite 2018) and Ernst Mach (Fisetite 2012). In this paper, I shall summarize Brentano's stance *vis à vis* Mach and emphasize the reception of Mach by Brentano's students.

## I. BRENTANO'S LECTURES ON POSITIVISM (1893–1894)

In his lectures “Contemporary philosophical questions” which he held in Vienna one year before he left Austria, Brentano extensively discusses Mach’s positivism (LS 20. 29366–29475). He compares four versions of positivism, that of Auguste Comte, which he compares to Kirchhoff’s descriptivism, and Mach’s phenomenalism, which he compares to John Stuart Mill’s empiricism. Brentano claims that the two last versions of positivism mark a progress over the other two versions namely because they are more up-to-date with respect to the development of natural sciences at the time, and because, unlike Comte, for example, they recognize the philosophical value of the field of mental phenomena, i.e. psychology.

Brentano’s correspondence with Husserl and Mach in 1895 testifies that, despite his reservations regarding the metaphysical positions advocated by these different versions of positivism, there remains, however, a “consensus on the method of research”, namely with Brentano’s methodological phenomenalism (Brentano 1988. 203). Indeed, Brentano is an empiricist and he is also very much concerned with positivity. Brentano agrees with positivism that the given consists in phenomena which are also the objects of sciences (physical and psychological alike). The inquiry is limited to phenomena and relations between phenomena that one seeks to subsume under general laws. Brentano is also in agreement with this aspect of descriptivism which favours the “how” question over the why question in the sense that the description of phenomena is prior to, and a necessary condition to their explanation. However, Brentano does not endorse Mach’s thesis according to which the task of science is merely to describe and not to explain phenomena. In his lectures on positivism, Brentano also claims that “it is unfair to claim that advanced sciences renounces the search for causes” (LS 20. 29403).

But Brentano’s overall criticism of Mach rests on Mach’s phenomenalism with regard to a spatial external world which, according to Brentano, is grounded on the identity of the mental and the physical. In Brentano’s own words: Mach’s proof of the “absurdity of the assumption of a spatial outside world on the basis of the identity of the mental and the physical in sensations is a complete failure” (LS 20. 29443). Brentano’s criticism of positivism targets not only Mach’s theory of elements, but also Comte and especially Mill’s doctrine of the permanent possibilities of sensation, to which Brentano grants much importance in these lectures. Brentano maintains that most versions of phenomenalism that he considers in these lectures claim that they “do not allow anything real then their own mental phenomena” (LS 20. 29411), and the limitation to the description of phenomena presupposes that the objects of experience are reducible to our own mental phenomena, and to percepts in the case of sensory perception. For if phenomena are somehow related to experience, and then they are necessarily

related to mental states (sensory perception). In other words: *esse est percipii*. Moreover, Mach's doctrine of elements amounts to identifying two irreducible classes of phenomena and it therefore does not account satisfactorily for the duality in the percept or in one's state of mind such as an emotion between the feeling and what is felt, or between perceiving and what is perceived. According to Brentano, this duality correspond two classes of phenomena which are bearers of heterogeneous and irreducible proprieties.

Brentano advocates instead a form of critical realism according to which the only access one has to the external world is by means of phenomena through which they are given to experience, but these objects exist independently of being perceived. However, Brentano claims that with some modifications, it might be possible to preserve the core of Mach's doctrine of elements, provided that one replaces the identity relation between the two classes of phenomena by that of intentional correlativity (*Correlativität*), which Brentano has worked out in his lectures on descriptive psychology delivered in Vienna in the late 1880s and which I shall later examine<sup>2</sup>.

## II. THE EARLY RECEPTION OF MACH IN PRAGUE

Mach witnessed the very first moments in the establishment of a school of Brentano in Prague where he held a chair of physics from 1867 to 1895. It is also in Prague that the first contacts between Mach and Brentano's students took place. Several of Brentano's students held chairs in Prague at that time, the first being Carl Stumpf who began his teaching in Prague in the fall of 1879 and held that position until 1884. Thanks to Brentano's and Stumpf's efforts, Marty obtained a position in Prague and began his teaching in 1880. A few years later, Masaryk obtained a position in the newly created Czech University in 1882 and he will be joined later by Ehrenfels in 1896.

Beside Mach, the main leading scientist in Prague was Ewald Hering, with whom Stumpf maintained a close relationship (Stumpf 1930. 399).<sup>3</sup> With Hering and Mach, Stumpf and Marty were both members of a circle of scientific researchers in Prague whose official organ was the well-known journal *Lotus*.

<sup>2</sup> Let us recall Brentano's marked interest in Mach's positivism and his doctrine of elements, as evidenced by his numerous notes dictated in Florence during the winter of 1905–1906, when he was practically blind (Brentano, 1988). Brentano's interest in Mach (1914) is clear in the article "Von der psychologischen Analyse der Tonqualitäten in ihre eigentlich ersten Elemente" (Brentano 1979) which he had prepared for the Fifth International Congress of Psychology in Rome in 1905, and in which he discusses Stumpf's and Mach's doctrines.

<sup>3</sup> Notice that Stumpf was already acquainted with Hering's work in physiology, which he extensively discussed in his *Rambuch* in connection with the nativism-empiricism controversy on space perception (Stumpf 1873).

Hering and Mach were very much involved in the activities of this circle<sup>4</sup>. Due in part to the reputation of the researchers associated with the research group Lotos, Prague was considered at that time a leading research center in Europe and has attracted many researchers from abroad and many students. It was also during that period that began the formation of Brentano's students of the second generation such as Emil Arleth, who attended Stumpf's lectures as early as 1879 and received from Hering a solid training in the field of physiological psychology (see Marty 1916). Franz Hillebrand, a close friend of Stumpf, who, under the recommendation of Brentano, went to Prague in 1886 to study philosophy with Marty, has worked both with Mach and Hering and contributed significantly to Hering's research in physiology. He later published many works in this field, and in his intellectual biography on Hering, he acknowledged his debt to him (Hillebrand 1918; see Stumpf & Rupp 1927).

The scientific reputation of Prague partly explains why the American philosopher William James went to Prague, during his trip to Europe in 1882, in order to meet Hering, Mach, and Stumpf. The empiricism advocated by James at that time and which he later developed systematically in his book *The Principles of Psychology* (see Marty 1892) is in many respects akin with the positions advocated by Hering, Mach, and Stumpf on sense experience. Although Stumpf is very critical of James' sensualism as shown by Stumpf's works on emotions (Stumpf 1928b), and moreover of James' later conversion to pragmatism, he maintained a lasting correspondence with James that shows a close relationship between the two philosophers (Stumpf 1928a)<sup>5</sup>.

### III. HUSSERL'S PHENOMENOLOGY AND MACH

Brentano refers to his lectures on positivism in a letter to Mach dated May 1895 in which he responds to a letter from Mach (14-05-1895) in which he informs him of his appointment in Vienna to the chair of history and theory of inductive sciences, left vacant since the resignation of Brentano in 1880, and he thanks Brentano for supporting him despite the circumstances that precipitated his departure from Vienna in 1895. We know that most students from Brentano in Vienna enthusiastically supported Mach's appointment. Indeed, in September 1894, Mach was invited to the Congress of the Association of German physicists and naturalists held in Vienna and gave a talk entitled "The principle of com-

<sup>4</sup> The lists of lectures which are relevant for this period are published in the journal *Lotos* V. 1884. VI–VIII and VI. 1885. VIII–IX. Hering held many lectures during Stumpf's stay in Prague, mainly on the subject of colors, and on the law of specific nerve energies. Mach mainly lectured on the fundamental concepts of electrostatics.

<sup>5</sup> In a recent book, E. C. Banks (2014) compared Mach's and James' empiricism to that of B. Russell.

parison in Physics” (Mach 1997). Mach’s talk has generated so much interest from Brentano’s students, that Alois Höfler, a student of Brentano and Meinong, invited Mach to discuss his talk at a meeting of the Philosophical Society of the University of Vienna. This discussion aroused in turn so much interest that two further discussion sessions were organized by Josef C. Kreibitz, another student of Brentano. These discussions have convinced several members of the Philosophical Society, including Brentano’s students who were very much involved in this organisation (see Fissette, 2014), of the interest of Mach’s candidature to occupy Brentano’s chair in Vienna. Mach began his teaching at the University of Vienna in 1895 and we know the major influence he has had on the course of the history of philosophy in Austria.<sup>6</sup>

Worth mentioning in this regard is Husserl’s positive review of Mach’s talk three years before the publication of his *Logical Investigations* (Husserl 1897). We know that Mach (1897: 200) uses the term “phenomenology” (a “general physical phenomenology extending to all domains”) in his talk to name his own methodological stance based on the description and analysis of sensations as the main task he assigns to science. This phenomenology is in many respects similar to Husserl’s phenomenology in the *Logical Investigations*, which he defines as a descriptive psychology, but also to that of Stumpf understood as a neutral science whose task consists in the description and analysis of sense phenomena (Stumpf 1906a). Brentano himself explicitly establishes the connection between his descriptive psychology and Mach’s doctrine of elements in his lectures on descriptive psychology which he taught in Vienna between 1887 and 1891. Brentano also uses the term phenomenology to refer to this part of his psychology which deals with the description and analysis of conscious experiences and the subtitle of the second version of these lectures: “Psychognosie: the doctrine of the elements of human consciousness” unequivocally refers to Mach’s doctrine of elements<sup>7</sup> and thus confirms that there is some kinship between these different versions of phenomenology.

Let us now return to Husserl. In his Amsterdam lectures (1928), Husserl even characterizes his phenomenology as a radicalization of a phenomenological method previously used “by some scientific researchers and some psychologists” (Husserl 1997: 213) and he mentions the names of Mach, Hering, and Brentano. The first two names are the natural scientists who, according to Husserl, have extensively used this phenomenological method, while the psychologists he refers to in this passage are, of course, Franz Brentano and his pupils. This is confirmed in an appendix to § 1 of the 1925 lectures on phenomeno-

<sup>6</sup> See Haller & Stadler 1988. On several other aspects of the relationship between Mach and Höfler, see Blackmore 2001; on A. Meinong’s relationship with Mach, see Lindenfeld 1980.

<sup>7</sup> In the manuscript of Brentano’s lectures *Deskriptive Psychologie oder Beschreibende Phänomenologie. Vorlesungen 1888–1889* (59115–59116), he refers explicitly to *The Analysis of Sensations*.

logical psychology in which Husserl claims that one of the main sources of his phenomenology lies in Mach's work in the domain of sensations (Husserl 1962. 350) namely because his approach to psychology differs from that of traditional natural sciences thanks to its descriptive character. Referring this time to the famous empiricism-nativism debate between Helmholtz and Hering, Husserl writes about the meaning of the method in Mach and Hering:

The sense of this method in men like Mach and Hering lay in a reaction against the threatening groundlessness of theorizing in the exact natural sciences. It was a reaction against a mode of theorizing in mathematical speculations and concept-forming which is distant from intuition, a theorizing which accomplished neither clarity with insight, in any legitimate sense, nor the production of theories. (Husserl 1997. 211.)

This amounts to saying that in Mach and Hering, this phenomenological method imposes several constraints on one's descriptions, namely that which consists in admitting as *descriptum* only what is immediately and intuitively given in experience, which Husserl conceives of in *Logical Investigations* as sensory data and immanent contents of perception and experience as a whole.

Another quote, taken from his 1910 lectures "The Fundamental problems of phenomenology", corroborates what Husserl says in the Amsterdam lectures. He once again maintains that the origin of the phenomenological method lies in J. S. Mill and "in the sensation-monism of Mach, who likewise substitutes connecting groups of sensation for the thing" (Husserl 2006. 76).<sup>8</sup> *Prima facie*, these two remarks make it possible to establish a close link between Husserl's phenomenology and Mach's descriptivism which, as Husserl points out in this passage, beyond its strict methodological meaning of describing phenomena in the simplest and more economical possible way, is coupled with a metaphysical postulate which, as we have stressed several times, amounts to the reduction of physical objects and psychical functions to aggregates or complexes of sensations. Yet just like Brentano and most of his pupils, Husserl has always criticized this form of phenomenalism. The question is therefore how to reconcile the repeated criticisms of Mach's phenomenalism throughout his work with the leading role that Husserl clearly assigned to him in the genesis of his own phenomenology. Part of the response lies in Husserl's criticism of Mach in the *Logical Investigations* where he raises the objection of logical psychologism which I shall later discuss (see Lübke 1960; Sommer 1985).

<sup>8</sup> In the winter semester of 1903–1904, Husserl gave a lecture on the new publications in the domain of natural sciences, and Mach's book, *The Analysis of Sensations*, was on the program (see Schuhmann 1977. 76). Mach's book was also an important topic in Husserl's lectures entitled „Philosophische Übungen mit einigem Anschluß an E. Machs *Analyse der Empfindungen*“ in the summer semester of 1911 (see Husserl's letter to Vaihinger dated May 24, 1911, in Husserl 1994/V. 211–212).



## IV. MACH AND THE CONTROVERSY ON GESTALT QUALITIES

The name Mach is also associated with what has been called the controversy on Gestalt qualities to which gave rise the publication in 1890 of Ehrenfels' study "On Gestalt qualities" to which participated most of Brentano's students.<sup>9</sup> Ehrenfels' starting point is the first edition of Mach's book *Contributions to the Analysis of Sensations* in 1886, in which Mach points out that we have the ability to immediately "feel" spatial forms and even "sound forms", or melodies. The question that arises in connection with descriptive psychology pertains to the nature of these peculiar contents of presentation which are called spatial forms and melodies, for example. Ehrenfels wonders then if these phenomena are mere syntheses or sums of sensations or something entirely new and irreducible to such syntheses. Ehrenfels finally opts for Mach's position on that issue and claims that this species of phenomena constitutes something entirely new and autonomous with respect to mere bundles and aggregates or to mental chemistry and he relies on three short passages in Mach's book including the following:

If two series of tones be begun at two different points on the scale, but be made to maintain throughout the same ratios of vibration, we recognize in both the same melody, by a mere act of sensation, just as readily and immediately as we recognize in two geometrically similar figures, similarly situated, the same form (Mach 1914. 285).

Ehrenfels argues that Mach's analysis of sensations paved the way for his own solution to the problem of Gestalt qualities.

After reading Ehrenfels' paper, Mach wrote to him that he himself had developed, twenty years earlier, the ideas that are found in this study, and we can assume, with Mulligan and Smith (1988), that Mach (1865) here refers to his study "Bemerkungen zur Lehre vom räumlichen Sehen". In this original study, Mach wonders how it is possible to recognize two spatial configurations (*Gestalten*) as being one and the same figure, for example, how can we identify one and the same melody played in two different keys and by different instruments. This recognition and similarity cannot depend, Mach argues, on perceptual presentational qualities since they are different in both cases. Mach's remarks can be understood in the sense of a recourse, necessary in this case, to additional elementary sensations outside the sphere of presentations, namely to sensations that he calls muscular or kinesthetic sensations: "When we hear the same melody in two different keys, our apprehension of this 'sameness' rests on the fact that, for all the differences in tone-sensations, the same feeling-sensations are involved in both cases" (Mulligan & Smith 1988. 126). It is known that Husserl studied

<sup>9</sup> On the Gestalt controversy, see M. Ash (1995); on the relationship between Mach and von Ehrenfels, see Mulligan & Smith 1988.



similar phenomena that he calls “figurative moments” already in his *Philosophy of Arithmetic* in a quite different context, namely that of the explanation of indirect apprehensions of multiplicities<sup>10</sup>. In a footnote to chapter XI, Husserl in fact mentions Ehrenfels’ article, that he had not studied at that time, but he explicitly acknowledges his debt to Mach’s *The Analysis of Sensations*: “Since I read this work by the gifted physicist right after its appearance, it is quite possible that I too was partly influenced in the progress of my thought by reminiscences from that reading” (Husserl 1970. 211). That said, in Husserl’s later works, he preferably uses the notion of moments of unity in order to designate that kind of phenomena, and it is no longer to Mach’s name that he refers in this context, but to Ehrenfels and Meinong.

#### V. MACH AND STUMPF ON LAWS OF PHYSICS AND PSYCHOPHYSICAL RELATIONS

In 1896, a year after his arrival in Vienna, Mach was invited to attend the 3rd International Congress of Psychology held in Munich, of which Stumpf and Theodor Lipps were co-presidents. But Mach declined this invitation because of his precarious health, and Brentano replaced him (see Brentano 1897). Stumpf delivered the inaugural address published under the title “Body and Soul” (Stumpf 1910) in which Stumpf summarizes his main objections against this form of neutral monism in several of his writings, and in particular in the two Academy treatises (Stumpf 1906b. 1; 1906a. 10–14)<sup>11</sup>. Stumpf’s first criticism is directed against the phenomenalist conception of physics and the empiricist interpretation of its objects in terms of “permanent possibilities of sensation”. The objects of physics, like those of psychology, are not reducible to complexes of elements since sense phenomena, although they represent indeed the starting point and the term of the research in the natural sciences, are finally “the object of none of them” (Stumpf 1906a. 16). The second criticism is directed against his conception of the laws of physics: in spite of Mach’s profound understanding of the history of the development of thought in the natural sciences as evidenced notably by his work on the economic nature of the research in phys-

<sup>10</sup> Notice, however, that even before von Ehrenfels, Husserl already used the notion of Gestalt (rather than that of figural moment) in his 1889–1890 lectures on the concept of number (Husserl 2004. 298).

<sup>11</sup> Stumpf knew Mach’s work and discusses it many times in his lectures and publications. He reviewed the first and second edition of *Analysis of Sensations* (Stumpf 1886. 1900). In the first, he is critical of Mach’s phenomenism, but relatively laudatory about the work as a whole. In his review of the second edition of the book published in 1900, Stumpf is clearly more critical and denounces the unacceptable consequences of Mach’s positivism, and in particular the reduction of mental functions to sense impressions, the conception of the world as a sum of sensations, the dissolution of the subject, etc. See also Stumpf 1890. 55 ff.

ics, the thesis according to which laws of nature are nothing more than abridged reports on facts is logically unjustifiable. Stumpf does not dispute the value of the principle of economy of thought so important in classical positivism, but he considers that it leads to bankruptcy because, by confusing laws and simple facts, it has as a direct consequence logical psychologism as defined in Husserl's *Prolegomena* (Stumpf 1906a. 53n.), which I will discuss in the next section.

The third objection against Mach (cf. Stumpf 1910. 86) bears on psychophysical relations to which Stumpf attaches much importance in his writings. Unlike most of his contemporaries, including Brentano and Husserl, Stumpf unequivocally rejects the doctrine of parallelism according to which the physical and the psychological are aspects of one and the same reality and he advocates, following Lotze, a form of interactionism that rejects monism in favor of dualism. The position that Stumpf advocates in "Leib und Seele" is nicely summed up in the following quote taken from his posthumous book *Erkenntnislehre*:

The discredited dualism however, according to which everything in the world, including the mental and physical, stands in thoroughgoing interaction (directly or indirectly), now appears as the true monism. According to interactionism, the world is, despite the diversity of its parts, a unified organic whole. Thus the parallelistic view proves to be impractical and contradictory, and therefore the theory of interaction remains, for the time being, the best guide through the maze of this great problem. (Stumpf 1939–1940. 822.)

One of Stumpf's arguments in favor of interactionism and against parallelism is Darwin's theory of evolution (Stumpf 1910. 78–79) to which he attaches great importance since the Prague period, and notably in his studies on the origins of music and the psychology of sounds.

Mach awaited the publication of the second edition of Stumpf's talk in 1910 to respond to these objections<sup>12</sup>. In a short notice entitled "Sensory Elements and Scientific Concepts", Mach (1992. 121) summarizes Stumpf's objection in saying "that relations by means of scientific laws 'absolutely never' exist between immediately given sensory appearances; what scientists mean by lawfulness is always completely different" (Mach 1992. 121). In response to Stumpf's objections, Mach argues that the purely mathematical world to which Stumpf (1910. 84–85) refers is a metaphysical postulate foreign to a physicist who adheres to the descriptivist point of view and who refrains from crossing the threshold of appearances. Mach indeed argues that everything beyond the immediate data of experience is metaphysical, and any science that does not conform to pure

<sup>12</sup> The discussion with Mach was in fact introduced in the 1909 version of "Body and Soul" and repeated, with significant additions, in his 1910 collection of essays *Philosophische und Reden Vorträge*, in which he makes more explicit his criticism of Mach (Stumpf 1910. 83–87).

description has to deal with *Scheinprobleme*. Hence the monism of sensations according to which the world is made neither of matter nor of mind, but of a neutral material that can be treated according to the context, interest, and direction of research, as psychical or physical<sup>13</sup>.

#### VI. THE OBJECTION OF PSYCHOLOGISM IN HUSSERL'S PROLEGOMENA<sup>14</sup>

Let us finally examine Husserl's objection of logical psychologism that he imputes to Mach in chapter IX of his *Prolegomena to Pure Logic* entitled "The principle of economy of thought and logic", in which he denounces any attempt to base logic and the theory of knowledge on the principle of economy of thought. But let us bear in mind that Husserl's objection in the *Prolegomena* does not directly relate to the theories based on that principle as Husserl confirms in his correspondence with Mach. On the contrary, he recognizes the "extraordinarily successful" nature of Mach's research on the biological and psycho-cognitive aspect of science and the merits of a "genetico-psychological und biological" approach to science (Husserl 1994/VI. 255). These theories are perfectly legitimate and fruitful, Husserl says, "in their due limits" (Husserl 1982a. 123). The distinction in the *Prolegomena* between logic as a theoretical science (as a theory of science) and as a practical science (as *Kunstlehre*) is important to understand the meaning of this limitation. Indeed, in ignoring the difference between the actual content of logical propositions and their practical application, logical psychologism systematically confuses the use of a proposition for normative purposes with its theoretical content, and its main mistake consists precisely in claiming to provide logic as a whole with a foundation. Only then can an empiricist like Mach be called a psychologist. For whoever recognizes the merits of the division within logic between its theoretical and its practical aspect is quite justified to resort to physiological psychology, for example, to explain the mechanical use of methodological rules. In other words, the use of psychology in the theory of knowledge can only be considered psychologist insofar as these two aspects of logic are confused and the theory of knowledge be reduced to a *Kunstlehre* of knowledge.

<sup>13</sup> One of Stumpf's famous students who contributed significantly to the reception of Mach is the author of the novel *Man ohne Eigenschaften*, Robert Musil. Musil traveled to Berlin in 1903 to study philosophy, physics and mathematics, and in 1908, under the direction of Stumpf, he defended a doctoral thesis on Mach (Musil 1908). See R. Haller (2003) who summarizes some general aspects of the complex relationship between Mach, Stumpf and Musil's dissertation.

<sup>14</sup> On Husserl's criticism of Mach based on the objection of psychologism, see Fissette (2012).

This is confirmed by Husserl's analyzes in § 55 of the *Prolegomena*, which deals more specifically with this form of empiricist foundation of logic that uses the principle of economy of thought. In its most general sense, this principle is formulated as follows: "This tendency of obtaining a survey of a given province with the least expenditure of thought, and of representing all its facts by some one single mental process, may be justly termed an economical one" (Mach 1903b. 211). This principle can be understood either as a psychological principle, as Cornelius does, or as a biological principle. What Husserl calls the Avenarius–Mach principle is considered in this section a biological principle that is associated with the principles of evolution of species, their adaptation to the natural conditions of their environment, and their conservation. In addition to its recognized applications in the field of biology, the field in which this principle is the most fruitful is precisely that of the methods in mathematical logic that serve practical needs such as the system of decimal numbers and in general all the standard mechanical and algorithmic processes that are used in mathematics. For all these technical and mechanical processes, continues Husserl, are methodological artifices which serve essentially to the economy of thought, i.e., they are used in order to compensate for "the defects of our mental constitution" or the severe limitations of "men's intellectual powers" (Husserl 1982a. 126). In fact, all these methodological artifices are due to the very nature of our mental constitution and they are the result of a natural evolution or "certain natural processes of thought-economy" (Husserl 1982a. 126).

One can see that Husserl's interest in the *Prolegomena* for the theory of the economy of thought in explaining the methodology of scientific research is not incidental. However, Husserl considers that this interest depends on the role of this theory in the larger and much more ambitious program of a theory of science. Therefore, this is not the place where psychologism lies. For logical psychologism is only imputable to Mach in so far as it takes into account only one aspect of logic (practical and technological). Mach's main mistake, therefore, boils down to the limitation of knowledge to "the empirical aspect of science", especially to science as a biological phenomenon, and to the fact that he does not take into account the true "epistemological problem of science as ideally unified, objective truth" (Husserl 1982a. 133). For the theory of knowledge that Husserl advocates in the *Logical Investigations* "wishes to grasp perspicuously, from an objectively ideal standpoint, in what the possibility of perspicuous knowledge of the real consists, the possibility of science and of knowledge in general" (Husserl 1982a. 131). This task is an essential philosophical complement to the *mathesis* and the overall theory of science. In that respect, as a theory of knowledge,<sup>15</sup> phenomenology has nothing to expect philosophically from a

<sup>15</sup> In the introduction to the second *Investigation*, Husserl clearly indicates that his theory of knowledge differs from that of classical empiricism in that "it recognizes the 'ideal' as a con-

genetic explanation as Husserl points out in his discussion of the work of Külpe and Elsenhans regarding the meaning of his criticism of logical psychologism (Husserl 1982a. 319).

Mach responded to Husserl's criticism in the fourth edition of his book *The Science of Mechanics: A Critical and Historical Account of Its Development* in which he admits that his scientific approach is indeed "a psycho-cognitive sketch" (Mach 1919. 582), while denying of having confused "natural or blind thought and logical thinking" and much less logical and psychological issues (Mach 1919. 582). He conceives of his dispute with Husserl as a difference of method: Mach's method is inductive and proceeds from particular phenomena to the general laws (bottom-up) whereas, with his general theory of science, Husserl proceeds deductively from main principles and laws, which he defines as ideal entities, to particular cases. But Mach does not take into account in his response Husserl's phenomenological investigations in the second volume of his *Logical Investigations* and ignores, it seems, Husserl's phenomenology and the use of a descriptive approach in his analysis of conscious experiences. Mach further argues that even a theory of all possible theories in Husserl's program cannot do without research in the field of biology: "Even if the logical analysis of all the sciences were complete, the biologico-psychological investigation of their development would continue to remain a necessity to me (Mach 1919. 582).

In a letter dated June 18, 1901, Husserl (1994/VI. 255–256) acknowledges receipt of the new edition of Mach's work and reminds him that his criticism of psychologism in no way challenges the right of a "genetic-psychological and biological" approach to science, but he opposes, as we saw, "the subordination of the epistemological explanation of the purely logical in science under the points of view of psychological genesis and biological adaptation" (Husserl 1994/VI. 255). Husserl recalls, moreover, that the chapter on the economy of thought does not primarily target Mach's use of the principle of economy of thought, but rather Cornelius' use of this principle in a psychological sense (Husserl 1982b. 303; see Cornelius 1897). What Husserl more specifically criticizes in Mach is the one-sidedness of his empirical descriptions, and the fact that he does not take into account the ideal and purely logical content of science, as if the genetical point of view were enough for epistemological needs (Husserl 1994/V. 256). Now, we saw that Husserl's argument in the *Prolegomena* against logical psychologism was based precisely on the ideality of the laws of logic. That said, Husserl claims that there is no contradiction between these two approaches that are mutually compatible and complementary (Husserl 1994/VI. 257). Husserl's

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dition for the possibility of objective knowledge in general, and does not 'interpret it away' in psychologistic fashion" (Husserl 1982b. 238).

clarification seems to have dispelled Mach's concerns as evidenced by Mach's short letter of 23 June 1901 in which he says that he has nothing further to add to Husserl's clarification and he hopes that this dispute is past history.<sup>16</sup>

## VII. FINAL REMARKS

We know that Mach renounced his chair in Vienna in 1901 and that one of the candidates to fill this chair was none other than Husserl who even visited Mach thereupon during the Easter holidays of 1901.<sup>17</sup> Alois Riehl, a colleague of Husserl at Halle, seems to have been one of the serious candidates for the succession of Mach in Vienna. But since Riehl was not interested in that position, he strongly recommended Husserl's candidacy to Mach. According to Husserl, Mach would have positively received Riehl's recommendation and would have shown a preference for Husserl's candidacy for this position<sup>18</sup>. However, after numerous negotiations within the Faculty, the Commission took the opportunity to repatriate the physicist Ludwig Boltzmann to Vienna by offering him Mach's chair (see Blackmore 1995). Husserl's disappointment is manifest in a nostalgic letter to his compatriot T. Masaryk, in which he admits of having abandoned the long-cherished hope of obtaining a position in Austria:

Von der alten Heimat bleibe ich nun wohl dauernd getrennt, die in früheren Jahren gehegte Hoffnung, einmal nach Österreich berufen zu werden, habe ich längst aufgegeben – obschon ich mit Freude erst im letzten Jahre hörte, daß E. Mach, als er sich zurückzog neben Riehl auch mich als ihm erwünschten Nachfolger für Wien nannte (Husserl 1994/I. 107).

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<sup>16</sup> However, in a letter to W. Jerusalem from June 8, 1913, Mach wrote: "I became acquainted with Husserl through his *Logical Investigations*. I cannot discover in it anything other than psychological investigations. Nor can I understand how it could be regarded as anything else" (Mach in Blackmore 2001. 222).

<sup>17</sup> Husserl describes this meeting with Mach to his friend Albrecht in a letter dated from August of the same year (Husserl 1994/IX. 23–24).

<sup>18</sup> In Mach's own words: „Unter den von Ihnen Genannten möchte ich mir von Husserl das meiste versprechen." (Husserl 1994/IX. 23–24.)

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## The First Vienna Circle: Myth or Reality?

### I. INTRODUCTION<sup>1</sup>

Many philosophical traditions have been mentioned as predecessors of logical empiricism, from Mach's empiricism and Russell's new logic to neo-kantianism and Austrian philosophy in the tradition of Bolzano and Brentano. One group has been specifically singled out as a kind of predecessor of the Vienna Circle around Schlick, the so-called "First Vienna Circle" (Otto Neurath, Hans Hahn and Philipp Frank). Several views have been associated with the "First Vienna Circle": first, that it formed a kind of nucleus of the later Vienna Circle in which we can already find some ideas of the later logical empiricism; secondly, that the main influence on this group was the philosophy of science of Mach, Boltzmann and the French conventionalists (Poincaré, Duhem, Rey). I will claim in this paper, that for historical reasons there are severe doubts about the importance and even about the existence of the group called the "First Vienna Circle". I will also claim that due to the focus on the "First Vienna Circle", some other important philosophical influences on Neurath, Hahn and Frank in Vienna before 1914 have been ignored and have not been taken sufficiently into account in the genesis of logical empiricism. I will emphasize especially the influence of a Viennese group of Meinongians, who were in close contact with Neurath, Hahn and Frank and shaped some of their early views on the foundation of science. Naturally I do not contest the influence of Mach, Boltzmann and the French conventionalists, through whatever forum that may have happened, on the young Neurath, Hahn and Frank. But I will claim that the Meinongians are especially important for the philosophy in Vienna around 1910. They are an essential factor in the rise of analytic philosophy in Austria. And through their interaction with Russell, the Meinongians were critical for the reception of the analytic philosophy of Russell and Frege in Austria. Through their contact with

<sup>1</sup> The present paper is an extended version of a section on the First Vienna Circle in my paper *The First Vienna Circle and the Erlangen Conference* (2019). I thank Thomas Uebel, Elisabeth Nemeth, Denis Fisette and Bastian Stoppelkamp for discussions on earlier versions of the present paper.

the Meinongians, Neurath, Hahn and Frank were in contact with the early rise of analytic philosophy as strongly as they were influenced by the philosophy of science of Mach, Boltzmann and Poincaré.

I will first discuss the literature on the “First Vienna Circle” and evaluate the accuracy of the data about that group (1). Secondly, I will describe the philosophical scene in Vienna with a special focus on the Meinongians (2). Then, I will reconstruct the philosophical discussions into which Neurath, Hahn and Frank were involved in Vienna before 1914 from the available historical data we have (3). I will then show how the discussions among the Meinongians influenced especially the philosophy of logic and mathematics of Neurath and Hahn (4). As conclusion, I will evaluate the impact of the philosophical discussions before 1914 on the later Vienna Circle (5).

## II. RE-DISCOVERING THE “FIRST VIENNA CIRCLE”

The first Vienna Circle was absent from the standard histories of logical empiricism until the 1980s. Rudolf Haller (1982a, 1982b) and Friedrich Stadler (1982, 111–117) were the first to put an emphasis on an almost unnoticed group formed by Hans Hahn, Otto Neurath and Philipp Frank before WWI, and mentioned by Frank (1941 and 1949). Haller named that group the “First Vienna Circle” and suggested that it should receive special attention, particularly if one wanted to avoid the then dominant “received view” of logical empiricism. Against the view of a foundationalist empiricism and an ahistorical philosophy of science, Haller claimed that a focus on the forgotten “First Vienna Circle” would allow us to see a fallibilist, holistic, conventionalist and historically informed stream in the Vienna Circle which was overlooked, despite the fact that this stream re-emerged in the Vienna Circle in the 1930s around Carnap and former members of this “First Vienna Circle”. Subsequent historical reconstructions of the philosophy of the Vienna Circle followed Haller’s suggestion of a revisionary view of logical empiricism and emphasized the role of the forgotten First Vienna Circle and its decisive influence on later logical empiricism, especially on the left wing of the Vienna Circle (Uebel 1991 and 2000, Stadler 2015).

The only primary source about the “First Vienna Circle” comes from Frank (1941), who, in an English re-edition of his papers added an introduction called “Historical Background”. There, he intended to “clear up certain misunderstandings” about logical empiricism and its history (Frank 1941, 6). Frank emphasized the origin of logical empiricism in the thoughts of Ernst Mach, despite certain shortcomings of the latter’s philosophy of science, namely: Mach’s underestimation of the importance of logic and mathematics, his rejection of atomism and his belief that physics was actually about perceptual experiences. In this context, Frank mentions “a group of young men”, Hahn, Neurath and himself,

who tried to solve these shortcomings with on the one hand conventionalism (Poincaré, Duhem and Abel Rey) and on the other hand new developments in logic and the philosophy of mathematics (Couturat, Schröder and Hilbert are mentioned by Frank). In a much expanded version of his historical introduction (Frank 1949), Frank is more explicit. The group, so Frank, was actually a weekly discussion group which tried to rebuild a “new positivism”, based on the ideas of Mach and the French conventionalists.

According to Frank, the discussions of the group focused mainly on the way to overcome the mentioned shortcomings in Mach’s philosophy. Contrary to Mach, the group thought that the principles of science were “clear-cut mathematical relations among a small number of concepts” and not Mach’s abbreviated and economical descriptions of observations, which involved a great number of vague concepts. In order to allow for an independence of a mathematized theory from observation, one had to appeal to conventions. As was suggested by Poincaré, definitions and basic principles of science were chosen freely and this conventional part of a theory was then coordinated with empirical statements: „According to Mach the general principles of science are abbreviated economical descriptions of observed facts; according to Poincaré they are free creations of the human mind which do not tell anything about observed facts. The attempt to integrate the two concepts into one coherent system was the origin of what was later called logical empiricism.” (Frank 1949. 11–12.) Frank suggests that his early paper on conventions and laws of nature (Frank 1907) is an example of such an integration of Mach with conventionalism.

Unfortunately, Frank’s report is the only source on the discussion group.<sup>2</sup> Despite the lack of additional independent evidence for Frank’s narrative of a regular discussion group, it cannot be doubted that Hahn, Frank and Neurath knew each other at the time and did discuss, in some context or other, the problems mentioned by Frank. Although Neurath never speaks about a regular discussion group, he refers to his early intellectual friendship with Hahn and Frank. In a letter from 1934, Neurath writes: “I was acquainted with him [Hahn] for about 35 years, we discussed together Poincaré, Philipp Frank reported to us about Einstein’s very first publications.”<sup>3</sup> And he writes about Hahn: “35 years of similar endeavors in different domains. The joint youth with Poincaré, Duhem etc.”<sup>4</sup> And also: „Hahn and I have been friends for many years – since the Gymnasium time. (...) He, the older, taught me a lot of things. We, Frank and other[s] read Spinoza in the ’Rahnhof’ [a Viennese café].”<sup>5</sup>

<sup>2</sup> There is no source whatsoever from the time of the meetings (no programs, notes or incidental remarks in correspondence). Not even a document which may confirm that a group with Hahn, Neurath and Frank actually met for discussions.

<sup>3</sup> Letter of Neurath to Gerrit Mannoury, Sept. 22, 1934.

<sup>4</sup> Letter of Neurath to Hempel, August 16, 1934.

<sup>5</sup> Letter of Neurath to Carnap, 1945, quoted in Uebel 2000. 69.

Given the lack of sources on the meetings mentioned by Frank, Fisette (2011) suggested that the group was just part of the discussions at or after the meetings of the “Philosophical Society” in Vienna, of which Neurath, Hahn and Frank were members at this time.<sup>6</sup> For this reason, Fisette calls the First Vienna Circle a “pseudo-Circle”. Fisette’s doubts are certainly very helpful. They raise the question what entity the term “First Vienna Circle” is actually supposed to designate.

I will focus here on the philosophical discussions in which Neurath, Hahn and Frank were involved before Hahn’s departure from Vienna (1910), when Hahn was appointed at the University of Czernowitz (now in Ukraine).<sup>7</sup> These discussions at multiple institutional levels shaped the philosophical views of Neurath, Hahn and Frank. I will limit my reconstruction to actually documented discussions. Frank’s report on the First Vienna Circle can be seen as a synthetic and potentially misleading integration of diverse discussions into a more or less mythical regular discussion group.

### III. DISCUSSIONS BEFORE 1914

Neurath, Hahn and Frank were involved in several discussions on the philosophy of science between 1907 and 1910. These discussions took place in three different settings: informal meetings of Neurath, Hahn and Frank (perhaps with others), a reading group around Hahn and Höfler on the philosophy of mathematics and the discussions in the “Philosophical Society”.

Hahn had studied mathematics in Vienna. After research in Göttingen, he got his “Habilitation” in Vienna in 1905 and started to teach there. Neurath had studied economics in Berlin since 1903 and returned to Vienna in 1906 after getting his doctorate in Berlin. At the time, the scientific interests of Neurath and Hahn strongly converged as Neurath expressed his intention to focus more on mathematics, the natural sciences and the history of science.<sup>8</sup> In 1907, Neurath and Hahn met regularly in order to prepare a course on the foundations of mathematics and physics with a special focus on Poincaré, Mach and Russell. Neurath writes:

I am doing a lot of things. I am especially more and more interested in exact logic, but also general considerations on the foundations of the sciences, particularly the exact sciences are on my daily schedule. I try to revise and improve my knowledge in

<sup>6</sup> The evenings of the Philosophical Society were mostly followed by more informal discussions in a Viennese coffeeshouse.

<sup>7</sup> Frank’s suggestion, in Frank 1949, 3, that his discussion group met till 1912 is quite improbable, given that regular meetings would be reduced to Frank and Neurath after 1909. In 1912 Frank was appointed to the University of Prague.

<sup>8</sup> See Neurath to Tönnies, November or December 1906.

mathematics and physics etc. A local lecturer of mathematics plans to offer a course in the next winter term on the foundations of mathematics and mechanics (following the work of Poincaré). He asked me to teach the course together with him. [...] We meet twice a week and read Russell's *Principles of Mathematics*. Also Mach's *History of Mechanics* is sometimes our topic. (Letter of Neurath to Tönnies, Spring 1907.)

Apparently that course was never given,<sup>9</sup> but it may have triggered further discussions on the philosophy of science between Neurath and Hahn, and then also Frank.<sup>10</sup>

In the same year (1907), the philosopher Alois Höfler (1853–1922) was appointed at the University of Vienna as professor of philosophy, with a special focus on pedagogy. Höfler had studied physics with Boltzmann before turning to philosophy under the influence of Brentano and Meinong. He was one of the closest allies of Meinong<sup>11</sup> and after his appointment to Vienna gave a controversial inaugural lecture on Meinong's theory of objects.<sup>12</sup> For several reasons, Hahn, Neurath and Frank got into close contact and intellectual exchange with Höfler. First, the other professors of philosophy in Vienna did not seem very attractive to them.<sup>13</sup> Secondly, Höfler had a strong interest in the philosophy of physics and mathematics. Thirdly, Höfler was institutionally important, because of his role in the "Philosophical Society" in Vienna, an essential philosophical forum for Hahn, Neurath and Frank. Probably through that "Society", the three young scientists came into contact with Höfler.

Through Meinong's debate with Russell,<sup>14</sup> Höfler gained a strongly interest in the new symbolic logic. In 1908–1909, Höfler, Hahn and another Meinongian, Hans Pichler, met regularly in a reading group on logic and the foundations of mathematics.<sup>15</sup> Together, they read literature on the philosophy of mathematics, especially the German translation (1908) of Couturat's *Les Principes des Mathématiques*, which introduced Russell's logic and logicism to the German

<sup>9</sup> In 1907–1908, Hahn actually gave courses on the theory of functions and on the foundations of geometry.

<sup>10</sup> Though none of these meetings can actually be documented, see footnote 2.

<sup>11</sup> Meinong was Höfler's dissertation supervisor and co-author, see Höfler and Meinong 1890. Höfler's correspondence with Meinong includes more than 3000 letters.

<sup>12</sup> Höfler to Meinong, Oct. 3. and Dec. 31, 1907.

<sup>13</sup> Besides Höfler, two other professors taught philosophy in Vienna: Friedrich Jodl (1849–1914), a classical positivist strongly influenced by Feuerbach, and Laurenz Müllner (1848–1911), a priest and specialist in Christian philosophy. The successor of Mach and Boltzmann, the philosopher and psychologist Adolf Stöhr (1855–1921) becomes professor in Vienna only in 1910.

<sup>14</sup> Smith 1985 and Höfler's letter to Meinong from Feb. 21, 1908. Höfler's interest in Russell was also triggered by Cassirer's reaction to Russell and Couturat in Cassirer 1907.

<sup>15</sup> Höfler mentions this reading group in a report and Hahn mentions it in a letter to Höfler, reprinted in Limbeck-Lilienau 2015. 45.

public.<sup>16</sup> Couturat's book concluded with a long and ferocious attack on Kant's philosophy of mathematics. Neurath wrote a brief review of Couturat's book (Neurath 1909a). But it is not clear whether Neurath participated in Höfler's reading group, although some details may suggest so.<sup>17</sup> Pichler also mentioned discussions with Hahn on the foundations of arithmetics, especially on the problem of the completeness of arithmetics and the means to prove it (Pichler 1909, 75–76). At the same time Höfler gave two courses on Mach's and Boltzmann's philosophy of the natural sciences and organized discussions on the same topic in the "Philosophical Society", once together with Neurath.<sup>18</sup>

The "Philosophical Society at the University of Vienna" was an essential intellectual forum for Viennese philosophers and philosophically interested scientists like Neurath, Hahn and Frank, but also for other later members of the Vienna Circle (e.g. Viktor Kraft and Edgar Zilsel). In that forum, Hahn, Frank and Neurath gave their first philosophical talks at the University of Vienna.<sup>19</sup> Höfler, together with Twardowski and other students of Brentano had founded the "Society" in 1888/89. Leading scientists like Boltzmann, Wilhelm Ostwald, Felix Klein or the co-founder of psychoanalysis, Josef Breuer, had given talks there.<sup>20</sup> A central and innovative feature of the "Society" were discussion evenings. After his return to Vienna (1907), Höfler began to organize these discussions, though with an increasing resistance from the president of that society, Friedrich Jodl, who tried to suppress them. In 1909, this led to a major clash. Höfler, together with Meinong, protested against the suppression of these discussions and he met regularly with Hahn in order to re-establish them.<sup>21</sup> It is quite probable that Neurath and Frank were also in support of such discussions, as they were strongly involved in their relaunch at the end of 1909.<sup>22</sup> Due to the mentioned lack of data, we cannot reconstruct how often or regularly Hahn, Neurath and Frank met for discussions separately from the "Philosophical Society" and Höfler.

<sup>16</sup> On Russell's reception in Germany and Austria, see Pulkkinen 2005.

<sup>17</sup> At the time of the reading group, Neurath wrote Höfler about Schroeder's logic (letter from 1909). Pichler also mentions his discussions with Neurath on logic, Pichler 1909, 22. But these conversations with Pichler and Höfler may have taken place in the "Philosophical Society".

<sup>18</sup> Höfler's courses in the winter of 1908–1809 and 1909–1910 had the title "Lesung und Besprechung naturwissenschaftlich-philosophischer Schriften von Mach und Boltzmann". The discussion led by Neurath in connection to that course was called "Concept and Scope of the Apriori" (January 1910).

<sup>19</sup> Fisette 2011 describes the essential role of that Society for the formation of the Vienna Circle.

<sup>20</sup> For an overview of the talks and discussions at the "Philosophical Society", see Blackmore 1995 and Meister 1938.

<sup>21</sup> Letters from Höfler to Meinong from Spring 1909. Höfler and Meinong wrote a report, supported by Hahn, expressing their protest (Höfler Archive, Graz).

<sup>22</sup> With the discussions on "Does Absolute Motion Exist?" led by Frank (Nov. 1909) and the discussions on the Apriori led by Neurath (January and February 1910).

Let us focus now on the positions of the Meinongians around Höfler and let us show how some of these positions became attractive for some of the mentioned later members of the Vienna Circle.

#### IV. THE MEINONGIANS IN VIENNA

When Neurath, Hahn and Frank began to be actively involved into philosophical discussions, Mach had already retired and Boltzmann was dead. At the University of Vienna, it was Höfler who now centrally focused his interests on logic, the foundations of mathematics and the natural sciences. Despite Höfler's strong interest in the philosophy of science of Mach and Boltzmann, his philosophical positions were clearly Meinongian. He repeatedly emphasized, that he had, together with Meinong, introduced the central distinction between the content and the object of a mental act (Höfler/Meinong 1890), a distinction which was at the basis of Meinong's theory of objects (see Meinong 1899. § 2). In Vienna, Höfler continued to defend his Meinongian approach based on the new theory of objects.

Meinong had introduced his theory of objects of higher order as an extension of Brentano's conception of intentionality (Meinong 1899). Contrary to Brentano's "immanent object" of the intentional act, he distinguished between the psychological content of the act and the object, which may be any kind of object, psychological, physical or other. Meinong also distinguished between objects of lower order (psychological or physical objects) and objects of higher order, like relations or complexes, which do not exist in the same sense as psychological or physical objects. Based on this conception, Meinong had announced in 1904 a new philosophical discipline, the theory of objects (*Gegenstandstheorie*). The subject matter of the theory of objects is concerned with all objects, whether they exist or not, whether they are real, possible or impossible. As such, its topic goes beyond psychology (psychological objects), physics or metaphysics (real objects). Meinong claimed therefore that the subject matter of the theory of object is not covered by any of the existing scientific or philosophical disciplines.

Meinong tried also to explain the relation of the theory of objects to logic and mathematics. As mathematics dealt also with any kind of objects, Meinong took it to be a special sub-discipline of the theory of objects. It was one of the special sciences which forms part of the general discipline of the theory of objects (Meinong 1904. 508–509 and 511). Meinong was less clear about the status of logic in relation to the theory of objects, but he suggested that such foundational disciplines as "set theory", "metamathematics" or "mathematical logic" may be at the border of a special science of objects (like mathematics) and the general science of objects (Meinong 1904. 513). A further advantage of Meinong's theory of objects for mathematics and logic was the fact that it attempted to give a



foundation to these formal disciplines which was definitely not psychological or psychologistic. The objects of Meinong's theory are neither per se psychological, nor are they mind-dependent.

After Meinong's programmatic announcement of his new theory of objects, the position was immediately endorsed by Höfler (1905).<sup>23</sup> There, he defended the view that mathematics was part of the theory of objects in Meinong's sense, as well as logic, insofar as logic was concerned with the *Apriori*. Against the psychologists, he emphasized that logic and mathematics was not concerned with the psychological. He insisted that his own distinction between the content and the object of mental acts showed that contents are, but objects are not psychological. And as mathematics was also concerned with objects, it neither was based on psychology. He illustrated this with Hilbert's geometry, which had divorced the basic concepts of geometry from any rest of intuition ("Anschauung"). For Höfler, geometry dealt with objects of higher order in Meinong's sense, namely with relations, not with spatial intuitions. Against the Neo-Kantians, Höfler remarked that the categories and intuitions of transcendental philosophy were still psychological notions and that therefore the Neo-Kantians, contrary to Meinong, could not get rid of psychologism.<sup>24</sup> At the same time, Höfler also announced "Logical Studies" based on the theory of objects, as well as a paper on "Spatial and Spaceless Geometry".<sup>25</sup>

In Vienna, after his appointment in 1907, Höfler continued to promote the theory of objects and to work on the relation of logic and mathematics to the theory of objects. In 1908, Höfler became increasingly interested in Russell and his new logical foundations of mathematics. Since 1904, Russell had regularly reviewed and commented the newest publications of Meinong. In his correspondence with Meinong in 1908, Höfler repeatedly asked Meinong about Russell and the relation of Meinong's philosophy to Russell. Höfler was particularly interested in Russell's theory of relations and its possible connection to Meinong's theory of relations.<sup>26</sup>

Besides Höfler, a small group of philosophers strongly influenced by Meinong worked also on the theory of objects in Vienna. They were all part of the "Philosophical Society" and included Anton Oelzelt-Newin (1854–1925), a former student of Brentano and Meinong who had worked with Meinong in Graz; Josef Klemens Kreibitz (1863–1917), also a student of Brentano who did get his

<sup>23</sup> Höfler's defense had some international impact as it was given as a talk at the "5<sup>th</sup> International Congress of Psychology" in Rome.

<sup>24</sup> The correspondence between Meinong and Höfler shows an increasing clash between the Meinongians and the Neo-Kantians, with regular complaints by the correspondents that their papers are rejected by the *Kant-Studien*.

<sup>25</sup> None of these were published, although the "Nachlass" of Höfler contains a long manuscript on geometry with that title. Höfler's new ideas about logic were certainly incorporated in the huge second edition of his *Logik*, 1922.

<sup>26</sup> See letter of Höfler to Meinong, March 2, 1908 (Meinong Archive, Graz).

habilitation in Graz, before returning to Vienna in 1907 and Hans Pichler (1882–1958), who had originally studied with Windelband in Strassburg, but lived in Vienna from 1906 to 1912, before going to Graz in order to get his habilitation with Meinong.<sup>27</sup> All these philosophers published on the intricate question of the relation of the theory of objects to epistemology, psychology, ontology and logic.<sup>28</sup> The question of the status of logic was central to their writings.

Through their correspondence with Höfler, the interaction of Hahn, Neurath and Frank with Höfler is well established, but we can also reconstruct frequent meetings of Hahn with Oelzelt-Newin and Pichler, as well as discussions of Neurath with Pichler. An exact reconstruction of these philosophical contacts of Neurath, Hahn and Frank with the Meinongians would need further space. Through the close ties of the philosophies of Meinong and Russell, the philosophical scene around Höfler and the Viennese Meinongians was particularly favourable to a reception of the new theories in logic and the foundation of mathematics such as those of Russell, Couturat and Frege.

## V. LOGIC AND MATHEMATICS

Frank claimed that the main focus of discussion between Neurath, Hahn and himself was an empiricist or conventionalist view of scientific theories and “the primary role of mathematics and logic in the structure of science” (Frank 1949. 7). But he also claimed, that they wanted to connect Mach and the conventionalists “with the investigations in logic of authors such as Couturat, Schröder, Hilbert etc.” (Frank 1949. 7). For Frank, it was important to show how a complex axiomatized mathematical theory could be correlated with the empirical part of a scientific theory. But he did not say much about logic or the foundations of mathematics. This was different for Hahn and Neurath. Both showed a strong interest in logic and the foundations of mathematics.

The beginning of the century saw major shifts with the emergence of symbolic logic and the development of a non-psychologistic understanding of logic. At the same time the logicist program in the foundations of mathematics emerged. Already in the first decade of the century, Neurath and Hahn were well aware of these developments. Neurath clearly shared an anti-psychologist conception of logic and supported a logical realism similar to that of Russell and Meinong. Hahn not only was strongly interested in Russell, but probably was already then sympathetic to his logicist program. This positive reception of Russell and the sympathies for anti-psychologism and logical realism can be explained by

<sup>27</sup> On Oelzelt-Newin, see Dölling 1999. 81–82; on Kreibitz, see Binder 2001; on Pichler, see Sauer 2001.

<sup>28</sup> See especially Oelzelt-Newin 1907, Kreibitz 1909 and Pichler 1909 and 1912.

Hahn's and Neurath's close contacts to a group of Viennese Meinongians, with Höfler at its center, and the reception of modern logic among them.

Anti-psychologism in logic claims that logic is not about judgements, beliefs or thoughts (in a psychological sense). The objects of logic are not psychological objects. Logical realism is in strong support of such an anti-psychologistic view. Russell (1904) gave a clear characterisation of logical realism, a view he shared with Meinong: For Russell, every belief has an object other than itself and these objects of belief are extra-mental (with the exception of beliefs about mental states). Truth and falsehood applies not to beliefs but to these extra-mental objects of belief (Russell 1904. 204).<sup>29</sup> Also Meinong posited such objects. He called the objects of a belief or a judgments an 'objective' (in German: "Objektiv"), a term Russell translated by "proposition".<sup>30</sup> Both for Russell at that time and for the Meinongians, logic was about propositions, conceived as the primary bearers of truth. There are several reasons to believe that Neurath and Hahn were attracted to this view, when they read Russell (1903) and Couturat (1908) between 1907 and 1909.

Neurath gained a strong interest in mathematical logic, already before his return to Vienna in 1906, probably through the philosopher Gregorius Itelson (1852–1926), a strong influence on the young Neurath.<sup>31</sup> In 1904, Itelson had given two talks: "The Reform of Logic" and "Logic and Mathematics". Though never published, the talks became quite well known through a summary by Couturat, the only source on Itelson's conception of logic (Couturat 1904). For Itelson, logic is not about the laws of thought and "psychologistic logic" is "absolutely sterile". For him, logic is not about thoughts, but about the objects of thought: "Logic is the *science of objects in general*" and "Logic is the science of *all* objects, real or not, possible or impossible, in abstraction of their existence" (Couturat 1904. 1038–1039). Besides his anti-psychologism and logical realism, Itelson emphasized a strong connection between logic and mathematics: "mathematics is a purely logical science" (Couturat 1904. 1037). As for mathematics, it is also a science of objects, namely of ordered objects (sets, groups). Itelson's conception is very close to the logical realism defended by Russell.<sup>32</sup> Meinong himself noticed the similarity of Itelson's position with his own theory of objects (Meinong 1907. 211–212).

<sup>29</sup> Schlick 1910 also quoted that passage where Russell defines logical realism, but rejected the position.

<sup>30</sup> Meinongians like Höfler also sometimes identified "objectives" with states of affairs.

<sup>31</sup> In a letter to Tönnies (Dec. 30, 1904), Neurath says that he is "captivated by mathematical logic". In another letter (June 25, 1906), he calls Itelson the "sharpest mind I know" and a "second Socrates". On Itelson, see Buck 1926 and Freudenthal and Karachentsev 2011.

<sup>32</sup> Russell says in a letter to Meinong: "I myself have been accustomed to use the name 'Logic' for that which you call 'Theory of Objects'", and in the same letter: "I am in complete agreement with the view that mathematics is theory of objects. That is in fact one of the main theses of my *Principles of Mathematics*." Letter from Dec. 15, 1904, see Smith 1985.

Back in Vienna, Neurath read Russell's *Principles of Mathematics* together with Hahn and continued to promote Itelson's conception of logic. He mentioned it in a paper on Schröder's logic (Neurath 1909b, 5) and defended it in a discussion on the Apriori he led at the "Philosophical Society".<sup>33</sup> Also Pichler (1909, 22), in a discussion of Itelson's theory, thanked especially Neurath for explaining Itelson's conception of logic to him.<sup>34</sup> Whether Hahn shared such a conception of logic is not known, although Hahn's sympathies for Russell inclined him perhaps to a similar view. At least, Hahn expressed his interest in Meinong's theory of objects in a letter to the latter.<sup>35</sup> And in his correspondence with Meinong, Hahn emphasizes frequently his high esteem for the philosophical judgment of Meinong.

In the 1929 and after the intense discussions of Wittgenstein's conception of logic based on tautologies, Hahn rejected Itelson's logical realism: „If logic were to be conceived – as it has actually been conceived – as a theory of the most general properties of objects, as a theory of objects as such, then empiricism would in fact be confronted with an insuperable difficulty. But in reality logic does not say anything at all about objects; logic is not something to be found in the world; rather, logic first comes into being when – using a symbolism – people *talk about the world*.”<sup>36</sup> It is implausible that Hahn had already such a view in the 1910s. The view that logic does not express anything about the world and that the rules of logic are actually conventionally fixed grammatical rules of a language is the result of the discussion of the *Tractatus* in the Vienna Circle.<sup>37</sup>

Hahn's early philosophy of logic and mathematics can only be reconstructed in a fragmentary way, as it is expressed only in some book reviews. Hahn rejected any kind of intuition (or Kantian "Anschauung") as a justification for mathematical axioms. In geometry, this led him to accept the kind of conventionalism defended by Poincaré (Hahn 1908). Hahn also supported the movement for the arithmetization of analysis and the attempt to give a rigorous and axiomatic basis to arithmetics, free from any intuitions or empiricist justification. For Hahn, any empirical justification of mathematics would endanger the certainty and rigor of mathematics (Hahn 1909a). Hahn also supported Peano's rigorous axiomatic foundation of arithmetics. Later, Hahn said explicitly that a *logical* foundation of arithmetics was highly desirable (Hahn 1919). Given his interest in the logicist

<sup>33</sup> Höfler took notes of Neurath's remarks, see Höfler Archive, Nr. V. 32. 104.

<sup>34</sup> Other Viennese Meinongians also discussed Itelson's conception, Kreibitz 1909, 309 and Höfler 1922.

<sup>35</sup> In a letter to Meinong (April 4, 1910), Hahn mentions Meinong's and Mally's theory of objects (Meinong 1904), Meinong Archive, Graz.

<sup>36</sup> Hahn 1929.

<sup>37</sup> See also Carnap 1928, §107 for an early formulation of such a conventionalist conception of logic, also in Carnap 1937.

programs of Russell (1903) and Couturat (1908), he was probably attracted to such a view much earlier.

Despite Hahn's support for Poincaré's conventionalist conception of geometry, nothing indicates that he was sympathetic to an extension of conventionalism to other areas of mathematics or even to logic. In a review of a book by the Dutch mathematician and philosopher Gerrit Mannoury (Hahn 1912), Hahn mentions Mannoury's extension of Poincaré's conventionalism to arithmetics and logic. He mentions also that Mannoury conceives logic as "only an analysis of the forms of language". Although Hahn's review of Mannoury is neutral, Hahn does not express any sympathies for such an extended conventionalism. The extension of conventionalism to laws of nature and to causality is also explicitly rejected by Hahn in a review of Hugo Dingler (Hahn 1909b).

Let us summarize Hahn's early views on logic and the foundation of mathematics: Hahn supported the elimination of intuition from mathematics and the arithmetisation of analysis. An attractive way to eliminate intuitions from arithmetics was the logicist program, therefore Hahn's interest in Russell and Couturat. Concerning the foundations of logic, it is quite probable that Hahn shared the logical realism adopted by Russell, Neurath and the Meinongians. Hahn's remark about logical realism from 1929 represents possibly Hahn's own evolution, from an early Russellian conception of logic through Wittgenstein's conception of logic based on tautologies to a view close to Carnap's logical conventionalism.

## VI. IMPACT ON THE VIENNA CIRCLE

The logical empiricists of the 1920s and 1930s did not share the logical realism of Russell and Meinong, nor did they accept Meinong's theory of objects. But the philosophy of the Meinongians was imbued with an analytic spirit which was very close to the analytic philosophy of Russell and Moore. This favoured an early reception of the new symbolic logic and the philosophy of Russell and Couturat. The same year Schlick was appointed at the University of Vienna, Höfler published a strongly revised edition of his *Logik* (1922), a book he had originally published with Meinong in 1890. The new edition, co-authored with Ernst Mally, contained an extensive exposition of the logic from Russell's *Principia Mathematica*.<sup>38</sup>

When Carnap, Neurath and Hahn published the manifesto of the Vienna Circle (1929/2012), the importance of the Viennese students of Brentano for a renewed understanding of logic and the foundations of science was explicitly emphasized there. In a passage on the pre-history of the "scientific world

<sup>38</sup> The parts on *Principia Mathematica* in Höfler's *Logik* have been written by Mally.

conception”, most probably written by Neurath, the manifesto acknowledged that the students of Brentano “were working toward a rigorous new foundation of logic” (1929/2012. 79). In that passage, Neurath mentions the endeavors of Höfler for a re-discovery of the work of Bolzano, but he mentions also the work of Meinong, Hans Pichler and Ernst Mally. Meinong’s “theory of objects (...) shows some affinity to modern theories of concepts”, so Neurath, and “the early writings of Hans Pichler (1909) also stem from this intellectual milieu” (1929/2012. 79).

It would be certainly a mistake to underestimate the central importance of Mach, Boltzmann and of the French conventionalists in the early formation of Hahn, Neurath and Frank, although this was not the object of the present paper.<sup>39</sup> The intention here was to analyse the importance of the early analytic philosophy and its logic in the formation of Hahn, Neurath and Frank and the intellectual contacts which were particularly favorable for such a reception. This philosophical context has hitherto not been sufficiently appreciated or evaluated in the research on the genesis of logical empiricism.<sup>40</sup> The more or less mythical “First Vienna Circle”, as described in Frank’s narrative prevented an adequate historical analysis of the philosophical milieu in which Hahn, Neurath and Frank received their first philosophical formation.

Whether the “First Vienna Circle” was more than some occasional meetings in Viennese coffeehouses between three young scientists remains an open question. No historical sources indicate that it was more, and no sources prevent us to think that it was even less. And even if the “First Vienna Circle” actually existed, we cannot say anything about it, due to the lack of historical sources about it. We can just repeat Frank’s statements, without the ability to evaluate their historical accuracy. The importance the “First Vienna Circle” has acquired in the genealogy of logical empiricism is certainly an exaggeration, which is in no way justified by the historical evidence we have. Furthermore, the mythical presence of that circle has prevented us to see the actual historical data about Neurath, Hahn and Frank and their relevance for an explanation of their early philosophical positions.

<sup>39</sup> I analyse the influence of Mach and the French conventionalists on Hahn, Neurath and Frank before 1918 in Limbeck-Lilienau 2019.

<sup>40</sup> Uebel 2000 is an exception, as he was the first to emphasize the central role of Höfler for our young trio.

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## Carnap's *Aufbau*: A Case of Plagiarism?

This paper addresses the topic of Austrian Philosophy as a potential influence on Carnap by means of a case study, namely, the alleged influence of Husserl on Carnap's first major book, *The Logical Structure of the world* (hereafter: *Aufbau*).<sup>1</sup> In a recent article, Verena Mayer formulates a very radical claim, specifically that in the *Aufbau*, Carnap somewhat plagiarized Husserl, stealing ideas from the then-unpublished manuscript of *Ideas for a Pure Phenomenology and Phenomenological Philosophy II* (hereafter: *Ideen II*). The aim of this article is to refute this claim. Though Carnap might have been acquainted with Husserl's manuscript, there is no indication that he took a significant amount of ideas from the latter.

In section I, I provide a survey of the various accounts of Husserlian influences on Carnap as developed over the years by Verena Mayer and Guillermo E. Rosado Haddock. None of these accounts involves plagiarism, literally speaking, but some involve varieties of *ideendiebstahl* (theft of ideas). These accounts of Husserlian influences on Carnap include: (1) a more neutral *initial account*, which does not involve any accusation of *ideendiebstahl* yet; (2) a *weak account*, which only involves the more general claim of Carnap's being influenced by Husserl but failing to acknowledge this influence; and (3) a *strong account*, which adds the more specific hypothesis that the *Aufbau* is basically a convoluted presentation of ideas that were stolen from *Ideen II*. In section II, the weak account is rejected for empirical reasons; there is no evidence at all that supports the weak account, whereas at the same time there is plenty of evidence that refutes it.

<sup>1</sup> Work on this paper was supported by the Austrian Science Fund (FWF research grant P31716). For helpful comments, I am grateful to Verena Mayer. The present paper is intended as an appendix to Damböck 2019, which provides an extensive account of the development of the *Aufbau*. I do not repeat here every detail that is already found in Damböck 2019. For matters of space, references to primary sources are generally not provided in the present paper as they are already to be found in Damböck 2019. Therefore, if the reader is interested in the details of the empirical evidence that supports my account, the present paper is not an independent source at all, but must be read against the background of a previous reading of Damböck 2019. The general argument, however, is easily grasped even for readers that are not familiar with Damböck 2019.

In section III, the strong account is identified to be: (a) empirically ill-founded because it directly hinges on the validity of the weak account; and (b) methodologically ill-founded for being based on similarities alone, many of which can be identified in the *Aufbau* and in several other books that belong to a certain philosophical genre.

#### I. MAYER'S AND ROSADO HADDOCK'S ACCOUNT OF HUSSERL'S INFLUENCES ON CARNAP

In this paper, I focus exclusively on the Husserlian influences that are relevant for the *Aufbau*. Therefore, I discuss here the influences that Husserl had on Carnap's dissertation *Der Raum* only insofar as they are also relevant for the *Aufbau*.<sup>2</sup> Moreover, I exclusively aim to review various theses on Husserlian influences on the *Aufbau* that were formulated by Mayer and Rosado Haddock, ignoring other important aspects of this relationship. In particular, I hardly discuss any of the systematic aspects of the relationship between Carnap and Husserl.<sup>3</sup> This is somewhat unfair, to be sure, because it implies that all these points where Mayer and Rosado Haddock correctly point out certain affinities and overlap between Carnap and Husserl are ignored. For the present purpose, though, it must suffice to say that I widely agree with the neutral aspects of the discussion in the respective texts by Mayer and Rosado Haddock. These neutral aspects – viz., what I call the initial account – are also in wide agreement with the recent state-of-the-art interpretations of the relationship between Carnap and Husserl by Carnap scholars, such as Thomas Ryckman and A.W. Carus. However, the present paper exclusively aims to evaluate those aspects of the writings of Mayer and Rosado Haddock that accuse Carnap of stealing Husserlian ideas and, therefore, I henceforth take the neutral aspects for granted here without any further discussion.

The accounts of the Husserlian influence on Carnap by Mayer and Rosado Haddock were formulated in three different contexts: (1) (Mayer 1991, 1992), two papers that mainly consist of what I will call here the *initial account*; (2) (Rosado Haddock 2008), a book that consists of key features of the *weak account*; and (3) (Mayer 2016), an article that consist of the *strong account*. Before I go on to discuss these accounts, I start with some general observations on the notion of plagiarism and ideendiebstahl involved here.

<sup>2</sup> For discussion of these influences, see Carus 2007. 127-135; Sarkar 2003; Stone 2009 as well as Rosado Haddock 2008. Chapter 1.

<sup>3</sup> For reviews of the philosophical similarities and differences between Carnap and Husserl, see Ryckman 2007; Carus 2016; Damböck 2017. 176–181; as well as the writings of Mayer and Rosado Haddock that are discussed here, together with Richardson 2010.

### 1. *Some observations on plagiarism and ideendiebstahl*

To set the stage for the present discussion, it is first necessary to identify what exactly “plagiarism” might mean here. This is particularly important because Mayer’s strong account is based on a notion of plagiarism that is by no means uncontroversial. Firstly, Mayer sets aside the now-common notion of plagiarism that involves the literal reproduction of passages of a text. Mayer, in turn, does not claim at all that Carnap might have plagiarized Husserl in this sense; rather, she restricts her understanding of plagiarism to cases where “not inconsiderable thought content stem from the work of another author, without this authorship being acknowledged” (Mayer 2016. 176). For Mayer, this also includes cases where the non-acknowledged source was not published yet. “This would even constitute a particularly perfidious case of ideendiebstahl (theft of ideas)” (ibid.).

This notion makes sense, insofar as it is certainly true that in most cases where certain big figures accuse each other of plagiarism, the reproached injustice is ideendiebstahl, rather than copying portions of a text. Note also that there already exists a very prominent example of accusation of ideendiebstahl with regard to Carnap, namely, Wittgenstein’s (in)famous reproach that Carnap had stolen his account of physicalism (see Stadler 2015. 224–228); however, this and other famous examples also show how extremely problematic all kinds of “plagiarism” accusations immediately become as soon as we no longer consider the copying of a text but mere ideendiebstahl. First, it is often not clear at all what exactly the respective idea in question is. Second, the idea in question is quite often something that simply was hanging in the air and then became formulated in various varieties by different authors almost simultaneously. Third, the idea in question is usually formulated somewhat differently by different authors and has subtly different functions in the respective contexts. Therefore, it seems groundless to talk about ideendiebstahl at all as long as it does not become considerably clear that (1) the idea in question is sufficiently clear and precisely expressible, (2) there is good evidence that a person first came across the idea only in the presumably-plagiarized text, and (3) both the original author and the plagiarizer use the idea in the same way. As soon as any one of these conditions does not hold, the entire matter becomes all too muddy.

Consider the following example. An author, B, might take certain ideas from a book or author A but essentially receives these ideas in a non-affirmative way, i.e., developing her own alternatives that might show similarities but diverge in such a significant way that it won’t be accurate at all to straightforwardly say “this idea was taken from A.” Rather, B would have to start here to distance herself, such as by saying things like “there is a somewhat similar idea to be found in A, but A does not get it right, my own version differs, I do not buy A for this and that reason, etc.” In a case like this, it may happen that B simply decides to leave the diverging account of A as it is, i.e., not mentioning it at all.

The reason might be simply a question of space and legitimate selection of focus. B is no longer all that interested in A. She wants to develop an alternative account, at a different level of argumentation, with different targets in mind. Getting involved in a lengthy discussion of A would simply make no sense in the present context. Do we have any good reason to accuse A of plagiarism, even though she was initially inspired by B but in the end does not acknowledge this source of inspiration in her writing? Probably not. It is certainly true, however, that plagiarism accusations often take exactly that form. Therefore, it seems to be a good idea to be suspicious of plagiarism accusations of that kind and to treat them very carefully. In the present example, it would be a massive exaggeration, if not a malicious imputation, to call A a plagiarist because she had scientifically valid reasons not to mention B.

To conclude, it tends to be rather silly to talk about *plagiarism* in cases like the one mentioned above; also, the alternative notion of *ideendiebstahl* is very problematic in all its varieties. However, for the present purpose, I will keep the notion on the table, having noted all necessary restrictions. I will *not* use the term “plagiarism” here any longer, keeping the latter for the straightforward case of copying portions of a text without acknowledgement. Rather, I distinguish three different cases of alleged and/or actual “ideendiebstahl”:

(1) Ideendiebstahl is *not* to be attributed in the following case. An idea, X, that is found in A is used in B without acknowledging A (although B read A) and it additionally holds (a) that B’s approach to X is significantly different from A’s and that (b) B also read and got inspired by several other sources where X or related ideas can be found. I will call this a case of *pseudo-ideendiebstahl*. Pseudo-ideendiebstahl, in turn, though being neither plagiarism nor ideendiebstahl at all, is typically found in cases of polemical sources that somewhat try to devalue B and to demonstrate B’s moral inferiority, in comparison with A. Such moral accusations are, at the end, more a matter of taste or non-cognitive emotional stance. In that sense, such accusations are legitimate, to be sure; however, it is not legitimate to use a twisted reality in support of emotional readings like that. The interpreter is legitimately uttering her feelings if she only points out that, in her view, B is morally inferior in comparison with A; however, she illegitimately uses the notion of ideendiebstahl (or even plagiarism) if only pseudo-ideendiebstahl is involved.

(2) Ideendiebstahl *is* to be attributed in cases where sufficient evidence shows that B directly took an idea X from A without acknowledgement as soon as X: (a) is sufficiently clear and precisely expressible, (b) was initially found by B in A and only in A, and (c) is used in B in the same way than in A. I will call this a *concrete ideendiebstahl*. A concrete ideendiebstahl is not necessary a fraud, but it is bad scientific practice at least, which can be justifiably criticized (even, and in particular, in emotional readings that argue for B’s moral inferiority in comparison with A).

(3) Ideendiebstahl is also to be attributed in cases where sufficient evidence shows that a person B studied with a person A, had discussions with A, read texts by A, took classes by A, and (a) was obviously significantly influenced by all these interactions and (b) is perfectly aware of this fact but still (c) fails to acknowledge or even denies these interactions, for example, in an autobiography or interview. I will call this a *general ideendiebstahl*. General ideendiebstahl is not so much a matter of scientific practice, at least in cases where it is not accompanied by concrete ideendiebstahl, because there is no typical way to acknowledge such general influences in scientific publications. The mistake involved here, rather than being a matter of bad scientific practice, is more a question of morality and personal character (and therefore, of course, also legitimately can be invoked in the context of emotional readings of B).

None of the accusations of Mayer or Rosado Haddock involve plagiarism, literally speaking, i.e., the copying of passages of a text; however, some involve varieties of ideendiebstahl. The initial account of Mayer does not involve any accusation of ideendiebstahl or plagiarism at all, whereas the weak account of Rosado Haddock involves an accusation of general ideendiebstahl and the strong account of Mayer involves an accusation of concrete ideendiebstahl. Let us now review these three different accounts.

## 2. *The initial account (mainly Mayer 1991–1992)*

In (Mayer 1991, 1992), a neutral account of the parallels between the *Aufbau* and several writings by Husserl is formulated, which was later elaborated in several respects in (Rosado Haddock 2008) and is still present in (Mayer 2016). I describe this neutral account here by means of some idealizations because the relevant writings already contain traces of the weak and strong accounts (this holds, in particular, for Rosado Haddock's book). However, the neutral account is important because it contains several qualifications with whom not only Husserl scholars, such as Rosado Haddock and Mayer, would agree, but also all appear to be acceptable for recent Carnap scholars, such as Ryckman and Carus. The neutral account consists of the following points:

- (1) Carnap's *Aufbau* is influenced by his reading of Husserl, which includes the main published writings, such as the *Logical Investigations* and *Ideen I*, but possibly also the manuscript of *Ideen II*;
- (2) This influence involves significant parallels between the *Aufbau* and *Ideen II* and other writings by Husserl;
- (3) In particular, Carnap possibly took the term "constitution" directly from Husserl and tried to somewhat reimplement an approach similar to the "constitutional theory" of *Ideen II* at the level of formal logic;

- (4) On the other hand, there are also significant differences between Husserl and Carnap, including the absence of any “transcendental ego” in the *Aufbau*;
- (5) Carnap studied with Husserl in the winter term of 1923/24 and tried to receive support from Husserl for his plan to habilitate at the University of Freiburg; however, Husserl did not support Carnap’s plan, and Carnap finally submitted his habilitation thesis in December 1925 in Vienna (the thesis was the initial manuscript of the *Aufbau*, which was published in 1928);
- (6) The influences of Husserl on Carnap have been widely overlooked by Carnap scholars for a long time; we should appreciate them in order to develop more accurate accounts of the relationship between Logical Empiricism and continental and 20<sup>th</sup> century continental European philosophy in the last decade.

This account does not involve any accusation of ideendiebstahl or bad scientific practice because Carnap very well did refer to Husserl in the *Aufbau*. Because the extent to which Carnap was influenced by *Ideen II*, specifically, remains unclear, one may not necessarily expect any reference to this then-unpublished manuscript. Also, there is no conclusive evidence at all that Carnap ever read *Ideen II* or even heard of the manuscript. Still, it is not *impossible* that Carnap read the manuscript because during the winter term of 1923–1924, Carnap interacted with Husserl’s assistant, Ludwig Landgrebe, who prepared the manuscript. Why shouldn’t Landgrebe hand over the manuscript to Carnap for some time or at least report to him about certain aspects of the latter? There is no evidence, to be sure, that supports this, but it is fair to say that there is also no counter-evidence that refutes it.

### 3. *The weak account of ideendiebstahl (Rosado Haddock 2008)*

The weak account of ideendiebstahl is basically a product of Rosado Haddock’s book and certain speculations to be found there, which (Mayer 2016) picked up again and somewhat radicalized even further. Unlike the strong account, the weak account is not based on any strong claims about the influences of *Ideen II* on the *Aufbau*. Rather, the weak account claims that (1) Carnap interacted with Husserl much more than the initial account claims and, as a consequence of this, he was (2) influenced much more strongly than the initial account may suggest; in spite of this, Carnap (3) systematically (and intentionally) ignored these influences in his autobiographical writings. More specifically, the weak account is based on the following claims:

- (1) Carnap not only studied with Husserl in the winter term of 1923–1924, but he firstly went to Freiburg after WWI in order to study with Husserl; Carnap presumably took classes with Husserl already before 1923 and he definitely attended several of Husserl’s seminars and lectures during the three semesters that followed winter term of 1923–1924, i.e., during the entire period where he worked on the manuscript of the *Aufbau*.
- (2) Thus, between the fall of 1923 and winter of 1925–1926, Carnap was in close contact with Husserl and some of his students, including Ludwig Landgrebe; he frequently interacted with all of them and wrote the *Aufbau* against the background of these intense interactions that, therefore, became the main matter of inspiration for the *Aufbau* manuscript.
- (3) However, because Carnap finally submitted his habilitation thesis in Vienna, supported by Moritz Schlick, who was a strong critique of Husserl, he decided not to hang a lantern on the interactions with Husserl any longer; later on, he simply ignored them and almost entirely ignored Husserl in his autobiography, although he knew perfectly well that Husserl was a major influence, at least during the years 1924 and 1925 when Carnap wrote the *Aufbau*.

The weak account, therefore, is based on a perfectly empirical claim, i.e., that Carnap had much more interactions with Husserl than initially expected – and, of course, it stands and falls with this empirical claim. If those interactions occurred and deeply influenced and framed the period when Carnap wrote the *Aufbau*, we have a clear case of general ideendiebstahl here, because neither in the *Aufbau* nor in Carnap’s autobiography did Carnap sufficiently appreciate these deep influences. Therefore, if the weak account holds, we are justified in accusing Carnap of somewhat immoral social behavior.

#### 4. *The strong account of ideendiebstahl (Mayer 2016)*

The strong account assumes both the initial and the weak account, but also adds another very strong and ingenious element that explicitly can be found only in (Mayer 2016), i.e., the claim that Carnap basically used *Ideen II* as the primary source of the *Aufbau*. He took almost everything from Husserl, both the elements of constitution theory and the way in which they are connected. The reason for this theft of ideas was that Carnap, in 1925, got under serious time pressure – he only began writing the manuscript of the *Aufbau* in spring and Schlick was continuously insisting on getting the manuscript submitted in the summer. Carnap took the option of stealing material from Husserl’s unpublished work as an easy way to deal with this dilemma. He exploited Husserl’s manuscript intentionally and with explicit fraudulent intent. Therefore, he tried



to somewhat change certain aspects of Husserl's initial account in order to mask his actual source. The *Aufbau* is nothing but a somewhat twisted and degenerated copy of *Ideen II*, and Carnap, during his entire lifetime, successfully managed to keep this fraud unrecognized, intentionally never mentioning Husserl as the real (and only) source of the *Aufbau*. The strong account is thus clearly accusing Carnap of concrete ideendiebstahl.

## 2. THE WEAK ACCOUNT IS NOT SUPPORTED BY EMPIRICAL EVIDENCE AT ALL

The crucial claim of the weak account, namely (1), is entirely empirical. If Carnap did not just interact with Husserl during the winter term of 1923–1924, but also during the following three semesters, this would entirely change our picture of the development of the *Aufbau* because the received view is rather that Carnap distanced himself from Husserl in 1924 and started to work on the manuscript of the *Aufbau* in the fall of 1924 only against the background of this, say, de-Husserlization of his philosophy. This view, which is defended extensively in (Carus 2016) as well as (Damböck 2019), would become, at the least, very problematic if it turned out that Carnap studied with Husserl and interacted with him and his students during the entire period where he actually wrote the *Aufbau*. It would be untenable, then, to claim that Carnap moved away from Husserl in 1924, no longer taking him as an important source when writing the *Aufbau* in 1925. The weak account, in turn, stands and falls with the empirical soundness of (I), because the speculations in (2) and (3) become plausible only if (I) appears to be true. Therefore, first and foremost, we need to get clear about the empirical facts here. *What did happen in 1924–1925?*

First, it must be noted that Rosado Haddock, Mayer, Carus, and Damböck all assume that Carnap did start to write the manuscript of what later became the *Aufbau* only in fall 1924 and that the manuscript was finished in December 1925. This general picture is well-supported by evidence from Carnap's diaries and his correspondence.<sup>4</sup> Therefore, we can take this part of the story to be uncontroversial. Disagreement, in turn, only concerns the question of *how long* Carnap interacted with Husserl. Let us review the respective arguments and sources.

I focus here on the most recent formulation in (Mayer 2016. 186–189, 193–194), although at least parts of what is said there can already be found in (Rosado Haddock 2008. 47–48). Both Mayer and Rosado Haddock also suggest that Carnap *might* have already taken courses with Husserl before 1923. Because they do not

<sup>4</sup> For a very detailed examination, see Damböck 2019. The following description of the intellectual development of Carnap in the early 1920s is developed in this paper in much more detail. For the respective empirical sources, see this paper.

have any evidence that supports their claim and, more importantly, this side of the empirical picture is only of minor relevance for the weak account, we henceforth ignore this here. More relevant is the general question of why Carnap moved to Freiburg in 1919. Mayer suggests that he moved to Freiburg in order to study with Husserl (Mayer 2016. 186); however, this is certainly not the case. Carnap moved to Freiburg because the family of his wife owed an estate there, called Wiesneck (in Buchenbach, near Freiburg). Carnap and his family moved to Wiesneck because they found optimal living conditions there and for no other reason. Firstly, in 1919, Carnap did not intend to study philosophy any further, but rather finished his studies with a *Staatsexamen* and intended to become a high school teacher. It was only in the fall of 1920 that Carnap decided to strive for a career as a philosopher and only then did Carnap start to read Husserl in the first place.<sup>5</sup> Then, Husserl played an important role as a source of Carnap's dissertation, which was finished in January 1921. However, as far as the diaries indicate, there were no personal interactions between Carnap and Husserl until the fall of 1923.<sup>6</sup>

Until 1923, though considering himself a philosopher of science, Carnap made no significant effort towards the establishment of an academic career. He was financially independent then – because his stepfather was a rich farmer – and therefore did not feel the need to go to academia as long as he could easily survive as a private scholar. However, in 1923, Carnap and his family visited Mexico for some months, where Carnap's stepfather owed an estate. Because of this somewhat conflictual visit, together with huge financial losses during the hyperinflation that took place in Germany in the very same year, Carnap realized that he no longer could manage working solely as a private scholar. Therefore, the natural move for him in 1923 was to try to find a place to habilitate in order to finally start building a career as an academic philosopher. He firstly contacted Bruno Bauch in Jena and Heinrich Scholz in Kiel. After getting back to Freiburg from his Mexico trip in fall 1923, he also immediately got in touch with Husserl. The reason for the latter was certainly that Carnap preferred not to leave Freiburg, mainly for private reasons, and not necessarily because of any particular affinities with the philosophy of Husserl. The Husserl episode of the winter term of 23/24 is well-documented in Carnap's diaries;<sup>7</sup> however, the diaries also indicate that the Husserl episode of the winter term of 1923/24 was the first time that Carnap actually met Husserl in person;<sup>8</sup> and they also indi-

<sup>5</sup> See Carnap forthcoming. The first Husserl text that Carnap read was *Ideen I*, at some point in September or October of 1920.

<sup>6</sup> However, see also footnote 8, below.

<sup>7</sup> I do not repeat the respective quotations here, because this episode is extensively covered by both Carus 2016. 138–145 and Mayer 2016. 186–189.

<sup>8</sup> This is not to be conclusively drawn from the diaries because there are a couple of months between 1919 and 1923 where Carnap apparently wrote no diary at all. At any rate, Husserl is *not* mentioned in the diaries until October 1923. Also, there is hardly any mentioning of Husserl in Carnap's correspondence before that time.

cate that the Husserl episode ended in February 1924. After having extensively reported on his interactions with Husserl in the diary between October 1923 and February 1924, in the remaining parts of the diaries that Carnap wrote until his emigration to the US in December 1935 (that cover more than 500 pages in print), Husserl is mentioned only eight times, and no personal interaction is ever mentioned.<sup>9</sup> The picture we receive from the correspondence is similar. There is some mentioning of Husserl, but no indication of any personal interaction, such as attending courses and the like. However, Mayer, following Rosado Haddock and somewhat counteracting the evidence, claims that Carnap attended Husserl's "Oberseminar", and presumably also his lectures, during the three semesters that follow the winter term of 23/24. Why does Mayer commit herself to this far-reaching claim?

Mayer mentions, as evidence, Karl Schumann's Husserl chronicle (Schumann 1977. 281), indeed indicating that Carnap participated in Husserl's Oberseminar between the summer term of 1924 and the summer term of 1925. However, Schumann's claim is based on a letter that Ludwig Landgrebe wrote to the author in 1976,<sup>10</sup> namely, more than five decades after the period in question. Given that there is no indication at all that Landgrebe's reminiscence might have been based on any reliable source, such as diaries or signature lists, it must be assumed that Landgrebe simply was wrong here and did not recollect correctly the period when Carnap went to Husserl's Oberseminar and also met Landgrebe, namely, the winter term of 1923/24. This becomes even more likely as Landgrebe, in turn, fails to mention Carnap's presence at the Husserl seminar in the winter term of 23/24; therefore, Landgrebe most likely simply mixed up his reminiscences a bit more than 50 years later. It seems very likely that Landgrebe, in his letters, simply wanted to say that he met Carnap in Husserl's Oberseminar *at some stage in the 1920s, possibly around 1924–1925*. This is *almost* true, to be sure. Also, we may note here that certainly neither Landgrebe nor Schumann might have been aware of the huge importance we now put on the question of *when exactly* Carnap went to Husserl's seminar. At any rate, I conclude that Landgrebe most likely incorrectly remembered exactly when he met Carnap in Husserl's seminar. Evidence from the diary tells us that this took place not between the summer term of 1924 and the summer term of 1925, but already in the winter term of 1923–1924.

<sup>9</sup> Until 1930, Husserl is mentioned five times, but only in cases where Carnap discusses Husserl's writings and philosophy with Schlick, Kraft, and other members of the Vienna Circle. Then, on June 19, 1933, Carnap mentions a meeting with Landgrebe that is also reported by Mayer and Rosado Haddock. Finally, in 1935, Carnap mentions Husserl's lectures in Prague (which he did not attend).

<sup>10</sup> Unfortunately, the letter by Landgrebe seems to be lost; it is not available, at least in the Schumann Nachlass at Leuven. I got this information from Verena Mayer via personal communication (e-mail from November 23, 2018).

Mayer is perfectly aware of the fact that the only real evidence we have here, namely, Carnap's diaries, by no means suggests that Carnap attended Husserl's courses after February 1924. However, Mayer qualifies the diaries in a very negative way here, claiming:

Carnap's diaries for its most parts contain notes on private events (shopping, walks, feelings, visits of friends etc.), who possibly were intended to serve as memory aids. Beside of this, there are sometimes also some notes on important professional events *being by no means reliable though*. For example, there is no hint to be found on the work on the habilitation thesis and its submission. The diaries, therefore, are not such documents that may allow us to conclude the existence or non-existence of events being not noted. (Mayer 2016. 186, my emphasis.)

This suggests that Carnap only somewhat occasionally wrote certain superficial things in the diaries, hardly covering professional events and, in particular, not covering the work on his habilitation thesis at all. However, this is anything but true. Firstly, note Carnap's habilitation thesis. Work on the latter is not just mentioned by Carnap in the diary, but reported extensively and meticulously. The term "Konstitutionstheorie" shows up 30 times, only during the year 1925, always indicating work on the habilitation thesis. In an additional document complementing the diary, Carnap even protocolized each single paragraph he wrote on graph paper. Additionally, work on the habilitation thesis is extensively documented in the correspondence. It is also not true that Carnap's notes are not reliable. There is no indication that Carnap did not take the task of writing the diary seriously. On the contrary, from 1923–1924 onward, Carnap obviously got the impression that he had become part of an important philosophical movement and therefore tried to document all kinds of professional events as precisely as possible. Though it is true, on the other hand, that Carnap hardly ever reports personal feelings – he almost never adds emotional statements to his diary – he always tries to be as accurate as possible concerning mere matters of fact. This does not imply, to be sure, that *everything* we do not find in the diary necessarily never happened. However, it does imply at least for the period in question here – viz., the years from 1924 onwards – that Carnap obviously *tried* to write down everything that happened during the day if it had at least minimal relevance. The diaries of 1924 and 1925 cover about 90 pages in print, although Carnap always tries to be as brief as possible. During a 726-day period, between January 6, 1924 and December 31, 1925, there are only 108 days that are not covered by an entry and there are never more than 4 consecutive days without an entry. If at a certain day there is no or only a very brief entry to be found in the diary, this occurred due to vacation, illness, or a very intense period of work. The entries, on the other hand, cover all kinds of *external* events, such as traveling, meeting people, or having a party. The entries almost never cover *internal*

events, such as personal feelings or thoughts. Therefore, the period in question is covered extensively and with every desirable detail, in particular, concerning all kinds of professional interactions, for example, with Schlick and the Vienna Circle, with Franz Roh and Sigfried Giedion, and with Wilhelm Flitner and people in Leipzig, including Hans Freyer and Hans Driesch. Note also that travelling to Freiburg was not particularly easy for Carnap. He firstly had to walk two kilometers to the train station at Himmelreich, then go to Freiburg by train, and walk to the University again. Such a trip took at least half of a day and sometimes Carnap even stayed overnight with friends at Freiburg in order to avoid all-too-frequent commutes. Carnap usually reports on his visits to Freiburg in the diary, also indicating where he went and which people he met. Therefore, if there would have been any meeting with Husserl between March 1924 and December 1925, we definitely would find a report in the diary. Though it might be true that Carnap was no longer a big fan of Husserl in 1924–1925, he still was certainly aware of the fact of Husserl being the world's most famous philosopher at that time. Why should he refuse to note visiting such a celebrity in the diary?<sup>11</sup>

To conclude, there is no evidence whatsoever that Carnap met Husserl (or Landgrebe) during the period in question (*viz.*, March 1924 until January 1926). Rather, evidence from the diary makes it almost evident that there were no interactions with Husserl or Landgrebe at all. Therefore, the weak account clearly must be considered empirically falsified.

<sup>11</sup> There is one option to be mentioned here that is at least implicitly suggested by Mayer, namely, that Carnap somewhat intentionally did not add his meetings with Husserl in 1924 and 1925 to the diary because he intended to plagiarize Husserl from the start. However, apart from the fact that this version is dangerously close to becoming a conspiracy theory of some kind, it is by no means plausible or even consistent. If the strong account holds, this would imply that Carnap, only somewhere in 1925, decided to plagiarize Husserl, after getting pressed by Schlick to finish his habilitation thesis as soon as possible. So, why did Carnap fail to mention his meetings with Husserl during the entire year before that alleged event? Note also that, for philological reasons, it almost certainly can be ruled out that Carnap might have removed the Husserl-related portions from his diary only later. The diary is a huge corpus of material, indeed, covering six decades at several thousand pages of shorthand material. Almost all these sources appear to be the original sources, rather than copies or transcripts. This is true, in particular, for the entire period in question. In order to fake these original sources, Carnap would have had to work for weeks. More importantly, one would certainly be able to figure out that the version of the diary that covers the period in question is a transcript simply because original sources always contain various traces of daily use that transcriptions fail to contain. Therefore, speculations suggesting that Carnap intentionally hid his meetings with Husserl in the diaries, either from the start or by means of a later act of forgery, are clearly pointless.

### III. THE STRONG ACCOUNT IS EMPIRICALLY AND METHODOLOGICALLY ILL-FOUNDED

The strong account is also based on an empirical claim, though one being less easily verified or falsified, namely, the claim that Carnap read the manuscript of *Ideen II* at some stage. The claim is less easily falsified because it is significantly vague. Possibly, Landgrebe had told Carnap at some stage in the winter term of 1923/24 about the manuscript or had even shown portions to him. If this had happened, it is not likely that Carnap would have taken this to be so important as to describe it in the diary. On the other hand, the strong account implies an acquaintance with *Ideen II* that goes very far beyond a casual glance of a manuscript. In this way, even the strong account somewhat hinges on the weak account. In order to become plausible, the claim that the *Aufbau* is mainly a result of copying ideas from *Ideen II* necessarily involves the idea that Carnap must have been in the closest touch with Husserl and other people, such as Landgrebe, during the entire period when he was preparing the manuscript of the *Aufbau*. Therefore, as soon as it turns out that Carnap was not in touch with Husserl and people from his group during that period at all, the entire strong account becomes similarly unlikely and ill-founded. Therefore, we are justified in calling the strong account empirically ill-founded (viz., falsified).

That said, the strong account is also ill-founded at a methodological level. In order to demonstrate this, let us assume for the sake of the argument that Carnap had access to the manuscript of *Ideen II* at any time while writing the *Aufbau*. This would imply that *Ideen II* might have been present to Carnap in the same sense as other books, such as the monographs of Hans Driesch, Hans Freyer, Günther Jacoby, Oswald Külpe, Ernst Mach, Wilhelm Ostwald, Josef Petzold, Johannes Rehmke, Heinrich Rickert, Bertrand Russell, Moritz Schlick, Wilhelm Schuppe, Hans Vaihinger, Theodor Ziehen, and others that were mentioned in the *Aufbau*. All these monographs show significant parallels with the *Aufbau* and *Ideen II* in that they offer similar construction procedures in one or another way. Therefore, in order to evaluate the similarities between the *Aufbau* and *Ideen II* that Mayer highlights, these similarities firstly would have to be compared with those similarities that we can find between the *Aufbau* and the other books in the list. Due to space considerations, we cannot provide such a comparison here at all. Note, however, that exactly a comparison of that kind is what Mayer's account is lacking, for in order to justify the claim of concrete ideendiebstahl, it is by no means enough to point out certain similarities between the *Aufbau* and *Ideen II*. One also would have to demonstrate that these similarities *only* hold between the *Aufbau* and *Ideen II*. Only if Carnap could have found the ideas in question *only* in *Ideen II* and in *no other* book that he was citing in the *Aufbau* the accusation of concrete ideendiebstahl would cease to be entirely implausible. Even then, however, one additionally would have to provide certain rock-solid

arguments for Carnap having *Ideen II* on the shelf, while writing the *Aufbau*, in order to, say, obtain a somewhat viable proof of guilt. Arguments of the former kind clearly do not exist. It is neither to be expected that any further account of the plagiarism story might be able to provide evidence of the latter kind, for it seems rather obvious – though it cannot be proven here in detail – that all kinds of similarities as well as dissimilarities between the *Aufbau* and *Ideen II* also show up in one or another way in any book of the aforementioned list. Most importantly, some of these books were read by Carnap before 1923 and even before he first started to read Husserl. Moreover, the first sketches to the *Aufbau*, which already contain large parts of the later layout of spheres of reality and their mutual relationships, was already formulated in August 1920, viz., at a time where Carnap had not yet read Husserl at all. In this early manuscript, the major influences are Wilhelm Ostwald, Bertrand Russell, and some representatives of the Dilthey School and Neo-Kantianism. To conclude, Mayer's strong account is not only empirically but also methodologically ill-founded. Carnap could not only find the same ideas contained in *Ideen II* in a huge number of books who were at least partly mentioned by him in the *Aufbau*, but it also appears that the main parts of the philosophical layout of the *Aufbau* were already developed long before Carnap allegedly got acquainted with *Ideen II*, some of them even before he firstly started to read Husserl. This implies that there is no reasonable way at all to talk about ideendiebstahl here.

The bottom line is that Carnap neither plagiarized Husserl nor did he take ideas from Husserl without enough acknowledgement. Having noted this, we may now move on in order to investigate all these interesting aspects of the relationship between Carnap and Husserl that remain on the table after having rejected the “plagiarism” hypothesis of Mayer and Rosado Haddock.

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## Overcoming Carnap's Methodological Solipsism: Not As Easy As It Seems

Methodological solipsism is the position adopted by Rudolf Carnap in his *Der logische Aufbau der Welt* (*The Logical Structure of the World*, 1928, hereafter: *Aufbau*). My concern here is to investigate whether, and if so, how, it can be effectively argued against – under certain conditions. That is, I will not take account of perhaps the most famous criticism Carnap received and pursue a question of principle. I will assume that Carnap's *Aufbau* “does what it says on the tin” and ask on what grounds one can then take issue with it. I will argue that Carnap had remarkable resources to resist the criticisms he did receive.<sup>1</sup>

### I.

According to the methodological solipsism of the *Aufbau*, it is possible to develop by logical construction a conceptual system encompassing all of empirical science on a so-called autopsychological basis. “Within the autopsychological basis, the available basic elements are restricted to those psychological objects which belong only to one subject” (§ 63, 100); in other words, the construction starts exclusively from what is “given” to an individual consciousness (§ 64, 102).<sup>2</sup> This basis was chosen so as to reflect the “epistemic order”: the construction was to begin with objects that are “epistemically primary”, namely objects whose recognition is “presupposed” for the recognition of “epistemically secondary” objects (§54, 88–89), which in turn are presupposed by epistemically tertiary objects, and so on as long as required. From a basic type of object, after

<sup>1</sup> This is admittedly not the first time that I have been considering these matters, but ongoing discussions with neo-Carnapians lead me to think that revisiting the matter from a fresh angle may help the understanding of certain subtleties that previous discussions neglected. Note that my discussion is limited to methodological solipsism in the context of the *Aufbau* project.

<sup>2</sup> All references with paragraph and page numbers are to the English translation of Carnap's *Aufbau*.

a certain number of steps, another type of object can be constructed, and so on, up to four kinds in total. In ascending order of complexity, “the sequence with respect to epistemic primacy of the four most important object domains is: the autopsychological, the physical, the heteropsychological, the cultural” (§58, 94). Notably, the methodologically solipsist system was but one several possible construction systems of concepts; Carnap also envisaged, but did not develop, systems with a physical base (§§ 59–60).<sup>3</sup>

While the technical aspects of Carnap’s construction project do not concern us here, we must briefly reflect on its radical nature. The basic elements are a person’s “elementary experiences”, that is, experiences “in their totality and undivided unity” (§ 67, 108). This means, to be precise, that the basic elements of the system are “conscious experiences (in the widest sense): all experiences belong to it, whether or not we presently or afterwards reflect on them. Thus, we prefer”, Carnap wrote, “to speak of the ‘stream of experience’” (§64, 102). This bare “given” is unanalyzable as such; all that can be done with it is that “statements can be made about certain places in the stream of experience, to the effect that one such place stands in a certain relation to another place” (§67, 109). In this way, even sense data are to be constructed by a method of so-called quasi-analysis so as serve as building blocks for further constructions.

Even more daring is Carnap’s choice of basic relations according to which the basic elements are to be ordered: only a single one was to be used, namely “recollection of similarity” (§ 78, 127). (From this basic relation that of “part similarity” can be derived for use in quasi-analysis, such that elementary experiences are recollected as similar in part, and it was from classes of such similarities that basic sense data are constructed.) The aim was that all scientific statements were to be shown translatable into statements employing only iterations and logical permutations of these elementary elements and the elementary relation. To be sure, predicate logic and classical mathematics are presupposed (§107), but few if any constructive projects in philosophy have shown such ambition.

Now importantly, it must be stressed right away, as it was by Carnap, that “since the choice of an autopsychological basis amounts merely to an application of the form and method of solipsism, but not to an acknowledgement of its central thesis, we may describe our position as *methodological solipsism*” (§ 64, 102, orig. emphasis). Methodological solipsism made no ontological claims; it was one possible stance of construction theory. Let’s see what this comes to.

<sup>3</sup> Carnap offered no discussion of the epistemic primacy of the autopsychological over the physical, which indeed was a very widely shared assumption at the time. Among representatives of the Austrian tradition it was shared by theorists as different as Franz Brentano and Ernst Mach (see Crane 2006).

## II.

The *Aufbau*'s combination of reach of ambition and instrumentalist minimalism was not pursued for logical sport only. One of the main points of the *Aufbau* was to pursue "the formalization of scientific statements", namely their translation into sentences which replace each term with its constructional definition, i.e. their definition in terms of the elementary elements and relation alone. The ultimate aim was "to complete this formalization by eliminating from the statements of science these basic relations as the last nonlogical objects" (§ 153, 235) – to achieve the complete structuralization of knowledge.<sup>4</sup>

This aim, in turn, was to complete Carnap's theory and afford it reflexive blessing. According to the *Aufbau*, the objectivity of sciences rested on what he claimed to be a fundamental fact, that "scientific statements relate only to structural properties", that is, "they speak only of forms without stating what the elements and the relations of these forms are" (§12, 23). By furnishing a strictly scientific redescription of human knowledge, one that by complete structuralization stripped it of its "intuitive" features and represented it in terms of its purely structural features, the *Aufbau* was to provide constructive proof of the claim about objectivity. What Carnap set out to do, then, were two things: first, the provision of the conceptual skeleton of possible human empirical knowledge, and, second, the provision of a theory of how to go about producing such conceptual systems. In Carnap's terms, he provided both a "rational reconstruction" and a theory of such rational reconstructions.<sup>5</sup>

Now importantly, that Carnap's construction of objects proceeds according to their epistemic order "does not mean that the syntheses or formations of cognition, as they occur in the actual process of cognition, are to be represented in the constructional system in all their concrete characteristics" (§ 54, 89). The point was philosophical. Now the *Aufbau* itself shows little concern with "justifying" knowledge claims as such, but only interest in developing "constructional systems", in the logical construction of systems of concepts. That said, the point of these constructions, Carnap himself conceded, was the "*rational justification of intuition*". He elaborated: "*The constructional system is a rational reconstruction of the entire formation of reality*, which, in cognition, is carried out for the most part intuitively" (§ 100, 158, orig. emphasis; cf. § 179, 289).

Needless to say, this epistemological engagement of the *Aufbau* remained pretty minimal. It provided only the logical-conceptual foundations for justifications of knowledge claims. Yet precisely by showing all concepts to be struc-

<sup>4</sup> For various forms of non-foundationalist interpretations of the *Aufbau* that are drawn upon in this section see, e.g. Friedman 1987, 1992, Richardson 1998, Pincock 2005 and the discussions in Carus 2007. ch. 6 and Pincock 2009.

<sup>5</sup> This metatheory, the theory of rational reconstruction provided what nowadays we can call a toolbox of formal epistemology.

turally reconstructible, it was the objectivity of science that was to be explained and substantiated, so it was an epistemological engagement all the same. The *Aufbau* was not altogether epistemologically innocent – as the Index of Subjects of the *Aufbau* reveals. There we read under “Justification”: “see Rational reconstruction”, and then find “Rational reconstruction [*rationale Nachkonstruktion*] (rational justification)” (360 and 363).<sup>6</sup>

It is therefore not at all irrelevant to note that while Carnap abjured the claim to paint a psychologically realistic picture, he did claim that the relations of epistemological justification that obtain for our cognitions are *correctly* portrayed in the way they were portrayed in the *Aufbau*. It is true that Carnap stressed that the system with an autopsychological base was but one possible way of providing a construction system (one with a physical base was also possible), but likewise is it true that his choice of which one to develop in the *Aufbau* was not arbitrary. “From an epistemological viewpoint (in contradistinction to the viewpoint of empirical science), we are led to... a constructional system with autopsychological basis” (§ 59, 95). In other words, what is epistemological about the construction system of the *Aufbau* that was developed in it, is precisely its methodological solipsism.

But, and this also is extremely important, while this methodological solipsism was long regarded as entailing a form of reductionist foundationalism, its epistemological interest lay elsewhere for Carnap. There is, for instance, the (already mentioned) structuralist agenda which it facilitates, and with it the distinctive idea of how to sustain science’s claim to objectivity: “science wants to speak about what is objective, and whatever does not belong to the structure but to the material (i.e. anything that can be pointed out in a concrete ostensive definition) is, in the final analysis, subjective” (§16, 29). Another central concern, also facilitated, is the exemplification of the unification of concept formation as something postulated by the concept of unified science (§ 2, 7).

Some of these epistemological interests may, of course, also be served by constructions with other kinds of bases or other approaches to epistemology altogether. (It is an interesting question, not pursued here, which can survive the overcoming of methodological solipsism.) For now, however, it should be clear that methodological solipsism manages to combine these interests in the construction of its conceptual system. And one more thing: some of these interests are wholly independent of epistemological foundationalism, the grounding human knowledge claims in non-inferentially justified beliefs (let alone indubitable ones) and the desire to secure human knowledge against philosophical skepticism (beyond establishing objectivity for science), and therefore remain viable motivations for the *Aufbau* even if the foundationalist one is discounted.

<sup>6</sup> Originally, “Rechtfertigung, s. rationale Nachkonstruktion” and “*rationale Nachkonstruktion* (rat. Rechtfertigung)”, with §§ 100 and 143 in bold as indicating special importance among the nine sections mentioned. No such differentiation was made in the Index of the English version.

## III.

Now, turning finally to criticisms of methodological solipsism, I will bracket the most famous criticism of it, Quine's. According to Quine, Carnap's *Aufbau* project breaks down because the predicate "is at" (placing a perceived quality in physical space) does not receive an eliminative definition (at § 126).<sup>7</sup> (Call this the "physicalist charge".) This criticism is widely, but not universally, accepted, even by some authors who oppose Quine's interpretation of the *Aufbau* as foundationalist empiricist epistemology. For present purposes I disregard it, since its acceptance would pretty much render my inquiry void. Failure to provide eliminative reductions would certainly show that the slim base Carnap chose to provide an adequate basis for methodological solipsism in the *Aufbau* was inadequate, whichever of the aims mentioned is pursued. (Whether a more Machian strategy, starting from a small number of types of sense data, would do better, is anybody's guess.)<sup>8</sup> The conditions under which I wish to investigate whether methodological solipsism can be effectively argued against, advertised in my introduction, are precisely those that obtain when Carnap's construction is not yet viewed as having its reconstructive proficiency challenged.

But while I here bracket Quine's criticism, I must stress that we should grant him with considerable more insight and subtlety than your average critic of logical positivism displays in at least one respect – one in which the present investigation must emulate him. Quine accepted that Carnap's strategy of constructing a genealogy of all non-formal concepts on the sole basis of the relation of remembered similarity with unanalyzed whole first-person experiences as *relata* was to be of only reconstructive import. Certain shortcomings simply would not count. Two misunderstandings in particular must be guarded against. First, as Carnap himself stated (§ 50), rational reconstruction was *not* meant to be *descriptively adequate to knowledge acquisition* as it actually took place. The second misunderstanding is more subtle and can be illustrated with reference to Quine's "is-at" objection.

Any failure of reduction that constitutes a legitimate complaint about the rational reconstruction must show that this strategy betrayed its promise to reconstruct our ordinary and scientific discourse *even in its own terms*. Therein lay the *Aufbau's* failure on Quine's reading. His complaint of failure was *not* that with methodological solipsism reference to anything but phenomenal objects

<sup>7</sup> See Quine 1951/1953. 39–40; 1969. 74–75. There is, of course, also Goodman's criticism, in Goodman 1951 and Goodman 1963, of earlier stages of the reconstruction which is still more controversial (see Carnap 1961. ix–x; Proust 1984, Mormann 1994) and which does not seem to turn on assumptions peculiar to methodological solipsism as such but on specific aspects of Carnap's way of formalizing its realization.

<sup>8</sup> When Carnap envisaged one such in the "Preface to the Second Edition" (1961–1967. vii), he did not motivate his preference for it on these grounds.

became impossible. That much is taken for granted when we accept Carnap's strategy. It is rather that methodological solipsism *fails in its aim to simulate* ordinary cognition. That is, it fails to reconstruct physical object discourse in its own reconstructive terms, namely by not providing indicators necessary and sufficient for the recognition of the basic states of affairs in which physical objects figure.<sup>9</sup> (Even if the *Aufbau* had succeeded with its reductions contrary to what Quine claimed, it would only have been make-believe physical objects that he ended up with, but that would not have mattered then.) Quine's criticism, in other words, was immanent to Carnap's project.<sup>10</sup>

The criticism I want to consider here is likewise immanent to Carnap's project, but differs from Quine's in its focus. Note that, however new-fangled Carnap's logic and however radical his structuralism may be, the ground plan of the *Aufbau*, the order of epistemic primacy which is followed in the process of ever more complex constructions of concepts and objects of cognition, is very traditional indeed. It is so, to be sure, not in virtue of appealing to atomistic foundations – the conceptual system is so deeply holistic that the atomism charge largely misfires – but it is so traditional in virtue of the austere individualism of its base. The criticism I want to consider is that it is this individualism that brings the *Aufbau* to its fall, in other words, that Carnap's methodological solipsism is responsible for a highly significant and non-negotiable failure of the reconstructive project, namely, the failure to do justice to its own aim of reconstructing intersubjectivity.<sup>11</sup> (Call this the “social charge”). Again it may be helpful to illustrate what criticism would *not* fit the bill before proceeding.

An example of non-immanent criticism would be that Carnap's reconstruction of intersubjectivity in the *Aufbau* in its later stages fails on account of its inability to sustain a certain conception of it that is endorsed earlier in the book. Consider that the kind of objectivity that was in fact reconstructed in the *Aufbau* consisted of “intersubjective correspondences” that allowed the construction of an intersubjective world (§ 146). These intersubjective correspondences consisted in the far-reaching structural agreement between a constructional system as a whole (which holds for me and represents my experience of the world, call it “CS<sub>self</sub>”) and the constructional systems which are ascribed to others within this all-embracing constructional system (call them “CS<sub>other</sub>”). It was on the basis of this agreement that intersubjective objects and properties can be construct-

<sup>9</sup> For these conditions, see *Aufbau* §§ 2 and 49.

<sup>10</sup> It is criticism that in principle should sway Carnap to take it on board (as, without change of agenda, one is not likely to do in the face of non-immanent criticism).

<sup>11</sup> To be painfully explicit it's the austere epistemic individualism of methodological solipsism that is being attacked here – which is not related to the position of methodological individualism in the philosophy of social explanation (even though that attracted much heated criticism on account of being misunderstood as a sociological analogue of methodological solipsism).

ed, i.e. objects and properties constructed in an analogical fashion in  $CS_{self}$  and  $CS_{other}$ . This intersubjective world then allows for the construction of physics. All along, what this process of “intersubjectivizing” provides, however, are constructions that “do not consist in a hypothetical inference or fictitious postulation of something that is not given, but they consist *merely in the reorganisation of the given*” (§ 148, orig. emphasis).<sup>12</sup>

Compare now how Carnap answered the question of “how science can arrive at intersubjectively valid assertions if all its objects are to be constructed from the standpoint of the individual subject, that is, if in the final analysis all statements of science have as their objects only relations between my experiences”. His answer was that “[t]he solution to this problem lies in the fact that, even though the *material* of individual streams of experience is... altogether incomparable... all streams of experience agree in respect of certain *structural properties*” (§ 66, 107, trans. amended, orig. emphasis). Note that this defense of the intersubjective validity or objectivity of science depends on having taken a standpoint external to the epistemic subjects in question by postulating all of their “streams of experience” to “agree in respect of certain structural properties”: no such objective agreement was reconstructed later in the book, nor could it even be stated from the perspective of an individual with the autopsychological language as in *Aufbau*.

Now why would this not qualify as criticism immanent enough to satisfy our desiderata? To begin with, there's a delightful ambiguity in the phrase “all streams of experience agree in respect of certain structural properties”. On an ordinary understanding, this phrase speaks of different streams of experience (mine, yours, his and hers) and so provokes the charge of inconsistency: what is reconstructed later is not what was talked about earlier.<sup>13</sup> But a committed Carnapian is likely to interpret the phrase in question as already speaking from within the perspective of the *Aufbau*: what accounts for objectivity in the *Aufbau* is precisely that each subject is able, by the process of intersubjectivization, to build up an intersubjective world shared with (reconstructed) others. For present purposes, there is no need to disambiguate, for we may ask whether Carnap has any need, in the first place, to invoke whatever may be the objective nature of objectivity, let alone to reconstruct it. Once it is noted that the *Aufbau*'s point is to simulate, not recreate, human cognition, then it becomes readily apparent

<sup>12</sup> Likewise, the *Aufbau*'s reconstruction of the intentional relation and its directedness never reaches real world objects (§ 164).

<sup>13</sup> This was the criticism voiced first in Uebel 1992. 94, where it was also attributed to Heinrich Neider (see §4 below), and then more or less repeated in his 2007. 133. That Carnap employed two different conceptions of intersubjectivity in the *Aufbau* is also noted in Richardson 1998. 89-91 and made the basis of a fundamental criticism of the *Aufbau*. How Richardson's criticism relates to those considered and developed here must be considered on another occasion.



that the committed Carnapian is right to reject the criticism of inconsistency as, at a minimum, irrelevant, for reconstructing an intersubjectivity involving two independent subjects was never the point.

#### IV.

What this brings out clearly, I think, is that it is not at all easy to come up with an immanent criticism of Carnap's methodological solipsist construction system in the *Aufbau* – as long, that is, as one grants him technical success. Not only does this mean that any demonstration that experience-transcendent objects remain out of reconstructive reach is bound to miss the dialectical mark, but that any demonstration that the process of intersubjectivization does not deliver a social space involving independent others is bound to miss the dialectical mark as well. What has to be shown, rather, is that Carnap's methodological solipsist construction system in the *Aufbau* did not manage to realize its legitimate aim: that it failed to show that there obtained no epistemologically significant discrepancy between the reconstruction and what it was a reconstruction of, in particular, that their subjective origin was no bar to the objectivity of scientific knowledge claims as here conceived.

Readers will have noted that the dialectical situation just outlined is pretty much like the one in which critics of Carnap's methodological solipsism found themselves before Quine's physicalist charge. So how did opponent of methodological solipsism react? The first concrete objection Carnap himself was confronted with (more diffuse ones had been voiced in Neurath's review of the *Aufbau*)<sup>14</sup> focused on the fact that, according to the model provided, different subjects could not test and confirm or disconfirm the same knowledge claims about that world, but only very different claims about their own experience. If everyone only ever tests (and understands) a sentence by translating it into their autopsychological language, then there are no sentences that are ever tested in common by different people. It is clear why this is an instance of the social charge: whatever was reconstructed here, it was not the intersubjectivity of science, for methodologically solipsist simulacra for that were not provided.

This criticism was put to him first by Heinrich Neider in December 1929.

I said to Carnap: "You will have to drop the auto-psychological basis, because sentences concerning the auto-psychological domain are not intersubjectively verifiable, and sentences which are not intersubjectively verifiable cannot belong to science. Philosophy can consider them in explanations of how these sentences came to be formed and once they are formulated then they must be intersubjectively verifiable."

<sup>14</sup> For discussion and references, see Uebel 2007. 103–123.



ble.” We talked for a long time and then Carnap asked me: “That is indeed a correct observation ...” Neurath was delighted. Neurath had not been with us then, but the following evening we were at his house and told him about it. He said: “Of course, finally!”, for with this a bridge was built towards materialism, which he valued so much as the philosophy of the workers’ movement. (Neider in Haller & Rutte 1977, 29–30.)

In light of the foregoing remarks, we must consider the logic of Neider’s criticism and see whether it really does apply, whether, for instance, the difference between simulation and recreation was observed.<sup>15</sup>

V.

The difficulty of making the social charge stick can be brought out by considering disagreement about an observational claim.<sup>16</sup> In the intersubjective language  $L_{IS}$ , differences about observational claims concerning a physical object, say “ $P_{o_{IS}}$ ”, are unproblematical: subject  $A$  affirms “ $P_{o_{IS}}$ ” and subject  $B$  denies “ $P_{o_{IS}}$ ”. There are two subjects which take conflicting attitudes towards the same proposition about a physical object. But how would things look according to the *Aufbau*?

If we imagine — from the vantage point of the objective perspective that also can observe the structural similarity of different streams of experience — different subjects operating with autopsychological protocol languages, we get the following result. Agent  $A$  has to translate the observational claim “ $P_{o_{IS}}$ ” into her protocol-language  $L_A$ , “ $P_{o_A}$ ”, and affirm it, while agent  $B$  has to translate the observational claim into her protocol-language  $L_B$ , “ $P_{o_B}$ ”, and deny it. Clearly, what  $A$  affirms is not what  $B$  denies – contrary to the situation we wanted to reconstruct. Intersubjective disagreement stays unreconstructed.

Now it might be thought that this misdescribes the situation. Are the statements in the protocol languages of the two subjects not translations of a statement of the intersubjective language (or its denial) and is it not in virtue of this relation of translatability that the two statements of the different protocol languages stand in the logical relation of contradiction? The objection is not without merit, but it holds only as long as it is the case not only that intersubjective languages are translatable into autopsychological protocol languages but also that the latter are translatable into the former. But precisely this they are *not* according to the *Aufbau* – it was only in 1932 with “The Physical Language

<sup>15</sup> For a dating of Neider’s argument and discussion of its context, see Uebel 2007, 130–136; for the subsequent campaign by Neurath, with numerous references, see Uebel 2007, Chs. 6–8.

<sup>16</sup> The argument could also be put in terms of agreement, but disagreement makes it more vivid.

as the Universal Language of Science” that Carnap granted this (in effect: re-) translatability of the autopsychological language into the physical language.<sup>17</sup>

Prior to 1932, Carnap’s autopsychological protocol languages possessed a *je-ne-sais-quoi* that prevented their translation into the intersubjective language (and thereby presumably also the re-translation of a formerly intersubjective content which, once mixed with the autopsychological *je-ne-sais-quoi* could not be distilled out again).<sup>18</sup> We must conclude, given the state of reconstructive play defined by the *Aufbau* (i.e., no re-translatability from the autopsychological into the physical language), that if there ever are two subjects operating according to methodological solipsism, they could neither agree nor disagree with each other, in fact, they could not communicate at all. They would be caught in their autopsychological protocol language – a state clearly at variance with science as it is conducted: intersubjectively.

So much the for what methodological solipsism (under the *Aufbau* conditions outlined) comes to from an objective or outside vantage point which recognizes there to be two different subjects. This is not a conclusive argument against methodological solipsism (as deployed in the *Aufbau*), however, for this only shows that intersubjectivity cannot be *recreated* by intersubjectivization, but not that it cannot be simulated by it. To investigate this we must try to model the situation subjectively, from the inside, as the agents involved envisage it from the perspective of a subject in the methodologically solipsist condition. Can A

<sup>17</sup> See Carnap 1932a. The *Aufbau* itself keeps quiet about this untranslatability, but Edgar Zilsel 1932. 145–146 also noted and remarked upon this asymmetry. It might be thought that I make too much of what is but an oversight in the *Aufbau*. To think so is to forget, however, that according to the model of the *Aufbau* testing and understanding happen only at the level of the autopsychological language: the physical language is understood mediately only. Given the direction of reduction, understanding depends on the autopsychological base. Likewise it is no good to point to Carnap’s claim elsewhere in the *Aufbau* that, given psycho-physical parallelism, not only are “all physical objects reducible to psychological ones” but also “every statement about a psychological object is translatable into a statement about physical objects”, that between them obtains “mutual reducibility” (§§ 57–58, 92–93). Surely then, the counter would go, autopsychological statements  $Po_A$  and *not- $Po_B$*  are translatable back into the physical language so as to disagree there (as to whether  $Po_{IS}$  obtains). To this it must be responded that in §§ 57–60 Carnap addressed the general theory of constitution systems, as he did when he stated, in § 62, that a constitutional system of concepts could also be erected on a physical basis. Such statements say nothing about the specific constitution system developed in the *Aufbau*, but outline the possibilities opened up for constitution systems generally by the assumption of psycho-physical parallelism. It was from this array of possibilities that Carnap then chose the particular reduction relations of the *Aufbau*, namely the methodologically solipsist ones that mirrored the order of epistemic primacy that he took to obtain: cultural objects to heteropsychological objects, heteropsychological objects to physical objects, and physical objects to autopsychological objects (§§ 59 and 64, at 95 and 101). The distinctive feature of Carnap’s actual *Aufbau* among the many possible *Aufbaus* he could have constructed is that the autopsychological language translates the physical language but is not translatable back into it. For further discussion see Uebel 2014.

<sup>18</sup> Carnap never specified the *je-ne-sais-quoi* element beyond suggesting it to be peculiar to the meaning of the autopsychological language; see again Uebel 2014.

represent  $B$ 's disagreement with  $A$ 's observational claim? Let's put aside the question of how  $B$ 's body (to which psychological states are to be attributed) is to be constructed from  $A$ 's evidence in the absence of explicit definitions of physical objects in phenomenal terms (Quine's complaint): suppose (*per impossibile*) it had been done. What's relevant now is the question how  $B$ 's disagreement with  $A$ 's observational claim is rendered by  $A$ .

We begin by observing, as before, that  $B$ 's own description, " $not-Po_B$ ", would not be available for  $A$ , for that employs  $B$ 's autopsychological protocol-language  $L_B$ : evidently,  $A$  cannot use that. But " $not-Po_A$ " will not do either, it would appear, for, equally evidently,  $B$  cannot use  $A$ 's autopsychological language  $L_A$ .  $Po_A$  and  $Po_B$  do not speak about the same thing or have the same content, they are not identical protocol sentences. (One talks about  $A$ 's experiences, the other about  $B$ 's experiences.) However, if it is not the recreation of intersubjectivity that we are after, but merely its simulation, why isn't it good enough for  $A$  to represent  $B$  as holding " $not-Po_A$ " etc.? Why will the solipsist fiction not do? For purposes of simulation (unlike for those of recreation) no pre-established harmony is needed, only assurance that the merely fictitious intersubjectivity entertained by *methodologically* solipsist subjects is harmless and does not impede the maintenance of a belief system that is functionally equivalent to the belief system their non-methodologically solipsist counterpart possesses.

It may be wondered whether a thin reconstruction of intersubjectivity, one that abstracts from the give and take of real intersubjectivity (like the simulations under consideration), could do the job. In particular it may be thought that there is an additional layer of complexity that emerges when we turn from the question whether physical objects have been successfully reconstructed to the question whether other subjects have been successfully reconstructed – and that this additional layer remains out of reach for methodological solipsism, so that attempts to simulate another with the resources of an autopsychological language inevitably compare unfavourably with simulations of physical object discourse in autopsychological language. Attempts at simulating intersubjectivity, the suspicion goes, are much more complex. For the other is not just a body but also a mind and that means that the task is to simulate that mind's representational activity – which includes the reflexive representation of its own and others' representational activity.

Again the critic stumbles over the difference between simulation and recreation. Of course, from a methodologically solipsist base I cannot recreate a point of view truly independent from mine: in that sense it can and must be questioned whether an autopsychological language can sustain the conception of another mind. But if it is granted that from a methodologically solipsist position I can simulate cognition of another body (as it is, prior to the physicalist charge, by default) and that a description of a mental event can be attached, under certain conditions, to another body whose cognition is simulated, then what should

stop a methodologically solipsist subject from attributing the ability to represent representing, reflexive mental states, to another body? It is hard to see what should make the simulation of complex, i.e. reflexive, mental states of another so difficult, if the simulation of first-order mental states of another is granted.<sup>19</sup>

It must be admitted that it is difficult to establish that methodologically solipsist reconstructions like those envisaged by Carnap are functionally inferior – given what we have to grant (we may add: counterfactually) to avoid anachronism. Granted that another body (by definition a transcendent object) can be simulated, there seems to be nothing to stop Carnap attributing mental states to that body so as to make sense of the “expressive events” observed to be happening there (§ 140, 216). But does this mean that methodological solipsism carries the day?

## VI.

Let us return to Neider’s charge that Carnap’s methodological solipsism fails to reconstruct the intersubjectivity of science as required. In light of our discussion we must distinguish: required for what? It is clear that Neider’s charge is correct in this respect: Carnap’s methodological solipsism fails to represent the intersubjectivity of scientific discourse correctly. Scientists do agree or disagree about statements in the intersubjective physicalist language. But what this criticism amounts to is that true intersubjectivity is not being recreated. What Neider’s criticism does not establish is that Carnap was unable to simulate intersubjectivity for his own epistemological purposes (which do not include, as we saw, humdrum justification).

Precisely this is the conclusion that Carnap came to. In *The Old and the New Logic* he therefore drew a new picture of the relation between the intersubjective physicalist language and the methodologically solipsist protocol language.

The analysis of the concept of science has shown that [...] they can be reduced to root concepts which apply to the ‘given’, to the content of immediate experience. [...] Thus, a genealogical tree of concepts results in which every concept must in principle find its place according to the way it is derived from other concepts and ultimately from the given [...] (‘methodological positivism’) [...] A second constitution system, which likewise includes all concepts, has physical concepts for its basis, i.e., concepts which apply to space and time [...] (‘methodological materialism’) [...] the positiv-

<sup>19</sup> *Nota bene*: it is Carnap’s *Aufbau* that is immunized from typical criticisms by the distinction between simulation and recreation. Once a more traditional epistemological agenda is pursued, as in *Scheinprobleme der Philosophie* (“Pseudoproblems of Philosophy”, 1928b), the dialectical situation changes significantly, but this cannot be dealt with here beyond some brief hints below.

ist and the materialist constitution systems do not contradict one another. Both are correct and indispensable. The positivist system corresponds to the epistemological viewpoint because it proves the validity of knowledge by reduction to the given. The materialist system corresponds to the viewpoint of the empirical sciences, for in this system all concepts are reduced to the physical, to the only domain which exhibits the complete rule of law and makes intersubjective knowledge possible. (Carnap 1930/1959. 143–144.)

Note that here both constructional systems — and so also the physical one — are “indispensable”. This marks a change from the *Aufbau* where the physical system was merely recognized as possible and where the rational reconstruction of scientific knowledge was assumed to be able to get by with just the constructional system on an autopsychological basis. So Carnap now operated with a two-language model, according to which the business of intersubjective science was conducted in the physicalistic language, but for which a translation into a phenomenalist protocol language was still required to provide its claims with epistemological justification.<sup>20</sup> (At this point, the physical language was held to be a universal language into which all and only other intersubjective languages were translatable.)<sup>21</sup>

Needless to say, this only ameliorated but did not solve the problem that critics of methodological solipsism perceive. It remained the case that, as in the *Aufbau*, first-person reports about psychological states were not translatable into physical statements. The asymmetry between autopsychological and heteropsychological stayed in place: only the latter were translatable into the physical language. Epistemologically nothing much had changed.<sup>22</sup> Predictably, this was the point subsequently pressed by Neurath, who had started a campaign for radical physicalism, i.e. the sole employment of the physical language, and to this end devised a succinct private language argument. This story I have told elsewhere.<sup>23</sup> Here I can only summarise the brisk development and note three questions arising.

In order to accord fully with the intersubjective nature of science, one's evidence sentences required full translatability into the intersubjective physical language, for without it they fall outside of science while science itself remains

<sup>20</sup> Another change, vis-a-vis *Aufbau*, is this emphasis on reduction for *validation* purposes.

<sup>21</sup> This was spelt out in manuscripts from 1930 – early drafts, later revised, of “Die physikalische Sprache als Universalsprache der Wissenschaft” and “Psychologie in physikalischer Sprache” – in which Carnap spoke of two universal languages, the physical and the phenomenal language of which the former was limited to the domain of intersubjective languages; see Uebel 2007. ch. 6.

<sup>22</sup> Except, as noted in a previous footnote, the switch to the project of now validating individual knowledge claims.

<sup>23</sup> For a detailed analysis of this stage of the Carnap–Neurath debate, see Uebel 2007. chs. 7–8.

epistemologically incomplete. The consequences are evident. Originally Carnap's protocol sentences were meant to formulate what was directly given and to provide the basic sentences for methodological solipsism. But once they are treated like physical sentences they fall under the same epistemological regime as the rest of the physicalist language. This means that the primacy of the intersubjective language has been established and that the first person has lost its unconditional privilege. This, of course, was the position Carnap endorsed by the end of 1932. Accordingly, Carnap excluded methodological solipsism from playing any further role in "the logic of science", which he soon declared take the place of (traditional) epistemology.<sup>24</sup>

One question which arises is how Neurath's argumentation dealt with the distinction which helped Carnap to block Neider's argument from having consequences for his epistemology, the distinction between recreation and simulation. The short answer is that Neurath's private language argument called into question whether it was possible for a solitary individual to sustain and ensure the consistent use of her language and thereby undercut a fundamental assumption of the simulation project pursued by methodological solipsism. The second question is closely related. What prompted Carnap, who, after all, found Neurath's argumentation difficult, to drop his simulationist resistance? The all-too-short answer is that Carnap changed his meta-philosophical perspective from rational reconstruction to adopt the stance of "logical tolerance".<sup>25</sup> Thus arises the third question. Given this change of perspective, does it still make sense to speak of Carnap's "overcoming" of methodological solipsism? The again too short answer is that logical tolerance does not prevent the adoption of non-essentialist, pragmatic first-order philosophical positions as superior to the competition. Needless to say, longer answers are needed but must be deferred.

What I tried to show here was only that and how Carnap's methodology of rational reconstruction in the *Aufbau* appeared to render impervious to criticism a conception of epistemic order that, given his revolutionary philosophical fervor elsewhere, was remarkably traditionalist.

<sup>24</sup> See Carnap 1934/1937. Part V and 1936; for discussion, see Uebel 2018.

<sup>25</sup> While logical tolerance introduced as such only in Carnap 1934/1937, § 17, it was already operative in his (1932b) which renounced the unconditional privilege of the first-person perspective and the demand for methodological solipsism.

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# Austrian Identity Theory and Russellian Monism: Schlick, Russell and Chalmers\*

## I. INTRODUCTION: THE COMMON FEATURES OF ALL RUSSELLIAN MONIST VIEWS

In this paper I present Moritz Schlick's views on the mind-body problem in some detail, which, beyond being an original contribution to the topic, may also be seen as a representative of a wider "Austrian" approach to the psychophysical relation, sometimes dubbed as the "Austrian Identity Theory". Further, I will investigate Schlick's connections with certain views of Russell (which they developed independently),<sup>1</sup> and to a representative of kindred contemporary views, namely David Chalmers' "Russellian monist" views.

The motivation for investigating these authors in particular are varied. As for the reasons of scrutinizing Chalmers' present views in particular: Russellian monism about the consciousness-brain relation became rather popular in the last two decades,<sup>2</sup> the main motivation for this development being Russellian monism's promise to solve certain problems which other contemporary naturalist theories, including reductionist, non-reductionist and eliminativist materialism and naturalist property-dualist theories, are notoriously unable to solve – and a major protagonist in this development has been David Chalmers.<sup>3</sup> As for putting Russell on the list: all contemporary Russellian monists consider Russell's (1927, 1948, 1956) views as their common ancestor. As for Schlick: his "Austrian view" on the mind-body problem, propounded in the *Allgemeine Erkenntnislehre* (1918, 1925) is rather similar to the views of Russell, and may also be considered as an alternative to the later-day materialist identity theories of Smart, Armstrong and Lewis – as Herbert Feigl emphasized long ago.<sup>4</sup>

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<sup>1</sup> See Feigl's comment on the independence in Feigl 1975.

<sup>2</sup> See e.g. Stoljar 2001; 2006, Strawson 2006, Chalmers 2013, and some earlier proponents as e.g. Lockwood 1992 and Maxwell 1979.

<sup>3</sup> See in particular his „Panpsychism and Panprotopsychism" (2013).

<sup>4</sup> See Feigl 1975. Russell and Schlick: a Remarkable Agreement on a Monistic Solution to the Mind-Body Problem.

So, by examining Schlick's and Russell's ideas together with Chalmers' Russellian monism in some detail I hope to lay out their similarities and differences, which, besides being of historical interest, may contribute to the evaluation of their respective merits and failings.

\* \* \*

According to Alter–Nagasawa 2015, the common features of Russellian monist views are the following:

*Structuralism about physics:* physics describes its basic properties in only structural/dispositional terms.

*Realism about the relevant intrinsic properties:* there are intrinsic properties that both constitute consciousness and serve as non-structural/categorical grounds for the structural/dispositional properties described in physics.

*Phenomenal or protophenomenal foundationalism:* at least some of those intrinsic properties are either phenomenal properties or protophenomenal properties (nonphenomenal properties that perhaps also in combination with structural/dispositional properties, constitute consciousness).

The virtues of Russellian monism over all contemporary naturalist theories of consciousness (reductionist, non-reductionist and eliminativist materialism and naturalist property-dualist theories alike) are, according to Chalmers 2013, the following. Russellian monism solves the problem of mental causation: it accounts for the causal efficacy of qualia, in a way that evades the mental epiphenomenalism *versus* overdetermination dilemma which threatens all other naturalist theories. Further, it answers the conceivability argument: it provides an explanation of why zombies are conceivable which does not imply that qualia are non-physical properties.

Besides these common features, we may find some further fundamental assumptions shared by Schlick, Russell and Chalmers, namely: (1) linguistic physicalism; (2) physicalist dualist property-pluralism; (3) Russellian or Austrian identity theory; and that (4) physical-concept-structuralism grounds all (1), (2) and (3). Somewhat more detailed:

According to (1) linguistic physicalism, *the linguistic-conceptual thesis*, all real entities can be identified by physical (or: microphysical or theoretical physical) terms, i.e. we can refer to any real entity by a physical concept (as well). According to (2), *the ontological thesis*, all Russellian monists are pluralist: they assume that many different kinds of qualities constitute the world. Further, they are dualist in holding that this set of variagated qualities divides into two large groups: phenomenal ("mental") and non-phenomenal ("physical"/non-mental) qualities, and *both* are taken to be real *in the same sense*. Their ontology is also

*physicalist* in holding that phenomenal qualities reside on the same ontological level as the non-phenomenal, “merely physical” qualities.

As for (3), *the thesis about the psychophysical relation*: they advocate a *Russellian* or *Austrian identity theory*. This is a dual-language view, asserting that since physical concepts determine only the structural properties of qualities (in different senses, see below) hence it is *possible* that the denotatum of some physical concept is a quale; and hence that some physical concepts like “c-fibre firing” *refer to a mental event*, and *not* to an ontologically distinct but co-instantiated *brain event*. Further, there are arguments to the the point that it is in fact so.

For (1), (2) and (3) the following similar arguments may be reconstructed from Schlick’s, Russell’s and Chalmers’ texts.

As for premise (1): Schlick, Russell and Chalmers all hold what Schlick calls epistemic parallelism, namely the view that synchronically with the perception of any mental event, a physical event (a brain event) is also perceptible. This is a very widely accepted view since the late 19<sup>th</sup> century, considered as empirically well-confirmed. Further, they all reject metaphysical parallelism, i.e. that the parallelly perceived mental and brain events are ontologically distinct. From these two tenets (1) follows, for in case (1) were not true, then the two epistemically parallel perceptions ought to be about ontologically distinct events, since the perceived mental event could not be referred to by a physical concept, hence it were not possible that the perceived mental and the physical events are identical, since necessarily, an event referred to by a mental concept could not be identical with an event referred to by a physical concept.

As for the arguments in favour of (2): pluralism about qualities follow from external world realism, which was extensively argued for by Schlick and also by Russell (in his realist periods), and taken for granted by Chalmers; and from the claim that different structural properties are associated with different qualities – a view, I take it, is also shared by all three authors. As for dualism: on the one hand, we have direct knowledge of the existence of phenomenal qualities, on the other we also know that there are non-experienceable extra-mental qualities, since this is implied by external world realism. (In the *Allgemeine Erkenntnislehre* Schlick argues in detail for the existence of extra-mental qualities, e.g. by arguing against “the philosophies of immanence”, neo-Kantian and phenomenalist views, and also against reductionist materialism about phenomenal qualities. Russell also accepts both the existence of percepts and non-experienceable “external to the mind” qualities. Chalmers all the same: he is a realist about both about physical properties and qualia.) As for physicalistic dualism: epistemic parallelism in itself would allow metaphysical parallelism (or natural supervenience), i.e. non-physicalistic property-dualism, but these views apparently cannot account for the causal efficacy of conscious events (or phenomenal properties). Schlick and Russell takes the causal efficacy of the mental for granted, and Chalmers also accepts it in his later Russellian monist views, unlike earlier,

e.g. in Chalmers 1996, where he seemed to lean more towards mental epiphenomenalism. According to physicalistic property-dualism, qualia (or micro-qualia or proto-qualia) are *on the same ontological level* as physical (or microphysical) qualities, hence my label “physicalistic dualism” as opposed to non-physicalistic dualism, according to which qualia naturally supervene on physical properties, hence they form a kind distinct from the kind of physical properties.

In favor of (3), the Schlickian and Russellian consciousness-brain state identity theory: contrary to supervenient physicalism and parallelism, such identity theories can explain the causal efficacy of the consciousness easily: phenomenal property instantiations are *on the same ontological level* as the non-phenomenal property instantiations, and they are *not* adjoined by parallelly instantiated non-phenomenal properties; hence their causal efficacy is not called into question.

(1), (2) and (3) are all supported by (4) structuralism about physical concepts. Structuralism, however, is laid out in various ways by Schlick, Russell and Chalmers. In the next section I will discuss these different accounts of structuralism in more detail.

## II. SCHLICK'S VIEWS ON THE NOTION OF THE „PHYSICAL” IN THE *ALLGEMEINE ERKENNTNISLEHRE*

Schlick's views on the “physical” are quite complex, hence I find it enlightening to present it from diverse angles, i.e. by presenting Schlick's views on the meaning of scientific physical terms, his views on the concept of the “physical” and his account of the methods of constructing scientific physical terms.

According to Schlick, *the meaning of scientific physical concepts is the conceptual role implicitly defined by the axioms of the relevant physical theories*. For example, the meaning of electric field “E”: the conceptual role “E” plays in the Maxwell-equations. Schlick's model was Hilbert's conception of the meaning of *geometrical concepts*: the implicit definition by the axioms of geometry. An important characteristic of such an account, which is underlined by Schlick, that *no appeal is made to any intuitive element* in the definition. Schlick applied this idea to interpreting the meaning of *theoretical physical terms* (e.g. of physical space, time, mass, charge etc.), also emphasizing the essentially non-intuitive character of the content of these concepts.

As for *the nature of the “physical”*: on Schlick's understanding, the “physical” is *a system of concepts, not a metaphysical category*. “Reality is called ‘physical’ *in so far as it is designated by means of the spatio-temporal quantitative conceptual system of natural science*” (Schlick 1918/1925/1985. 294). Hence, Schlick contends, a physical entity is *not* an extended and *quality-less* entity (as according to Democritus or

Descartes).<sup>5</sup> *The natural world* consists of variegated *qualities*, among them subjective, experiential qualities, accessible to consciousness and non-subjective, non-experiential qualities, not accessible to consciousness, which depend on each other in law-like ways.

*The method of constructing scientific physical concepts* is laid out by and large as follows. We obtain scientific physical conceptual systems in several steps.

*Step 1.* Determining intersubjective qualitative concepts, from the subjective sensory experiences directed at the same (*Ding an sich*) entity.

*Step 2.* Determining quantitative relations between the properties identified by the intersubjective qualitative concepts.

*Step 3.* Introducing a theory that explains the quantitative relations (identified in step 2.), such that its theoretical terms are characterized exclusively by non-intuitive/non-experiential quantitative features.

We may illuminate these steps by two examples, by the construction of *the concept of physical space* and of *thermodynamical concepts*. The notion of objective physical space is of fundamental importance for Schlick, since physical spatial location plays a role in the construction of all scientific physical concepts. The steps are the following:

*Step 1.* Obtaining the concepts of objective, *Ding an sich* space-points from the points of subjective sensory spaces, e.g. the visual field, by the method of coincidences: i.e. by correlating an objective (*Ding an sich*) point to the singularities of the sensory intuitive fields (e.g. the visual experience of a finger pointing to a location on a blackboard) of different subjects observing the same (*Ding an sich*) objects (viz. the finger and the blackboard).<sup>6</sup>

*Step 2.* Determining quantitative relations between the points of objective space (e.g. the notions of distance, interval).

As for thermodynamical concepts:

*Step 1.* Determining intersubjective qualitative concepts of thermodynamics: pressure, volume, temperature. (As for temperature: correlating the subjective thermal sensations of observers with thermometer readings – the length of the mercury rod; as for pressure: correlating the subjective pressure sensations of observers with pressure-meter readings.)

<sup>5</sup> Schlick 1918/1925/1985. 293.

<sup>6</sup> Cf. Schlick 1918/1925/1985. 272 ff.

*Step 2.* Determining quantitative relations between such intersubjective qualitative concepts, i.e. between pressure, temperature and volume (for example, the gas law:  $PV/T = \text{const.}$ )

*Step 3.* Introducing the *non-qualitative quantitative* concepts of microscopic particles with mass, velocity, location, number, kinetic energy, and explaining the quantitative relations between the intersubjective qualitative concepts in terms of these quantitative concepts, e.g. *the number of particles hitting the wall in a time unit and mean kinetic energy.*

As a consequence of the general features of such method of construction, the resulting (*scientific*) *physical concepts* are *purely quantitative*. And, Schlick contends, *by these quantitative concepts* we can identify *all qualities of the natural world*; both the experiential/ phenomenal qualities with which we are acquainted, and the non-phenomenal ones with which we are not.

As for the concepts of microphysical entities: atoms or electrons are accounted for as bundles of interconnected (microphysical) qualities, like mass, charge etc. Thus, *we are not acquainted with* the qualities of such *theoretical physical entities* (and hence with microphysical entities), but we *can be identify them by the quantitative physical concepts* the meanings of which are determined by *implicit definitions*, i.e. by their „role“ in the relevant physical laws. Hence the *theoretical physical concepts involve no reference to the qualities* of the natural entities – but this does not imply that natural entities have no qualities. As Schlick formulates: *qualities are absent from the physical description of Nature, not from Nature itself.*

Such a view may be dubbed as structuralist in the following respects: scientific physical concepts do not appeal to the intrinsic qualities of physical properties, and their meaning is the conceptual role they play in certain physical law statements, which themselves express relations between physical entities, not their intrinsic qualities.

### III. SCHLICK'S MOTIVATIONS AND PHILOSOPHICAL CONTEXT

After briefly canvassing the diverse aspects of Schlick's notion of the “physical”, I shall address the philosophical context in which Schlick views emerged, and the question of what motivated his account.

In general, it seems fair to characterize Schlick's project as aiming at a reconciliation of his complex empiricist epistemological theory with his external world realism. The main features of Schlick's epistemological theory proposed in the *Allgemeine Erkenntnislehre*, may be characterized briefly by the following features:

- (1) According to Schlick's *general analysis* of the concept of knowledge:
  - i. Knowledge is *never intuitive*, it cannot be merely an act of intuition or „living through” (erleben), nor some sort of unification between the object and subject of knowledge – not even in the case of our knowledge about the qualities of phenomenal experiences.
  - ii. Knowledge is always *mediated by concepts*; it is always a matter of comparing, fitting into a system.
  - iii. Knowledge is a *re-identification* of an already known object as something else.
- (2) Knowledge about *the external world* must be “anchored” in sensory experience.
- (3) *Physical* knowledge is knowledge gained through the application of physical theories and methods.
  - i. The characterization of *physical* knowledge must be based on the investigation of physical science, on the reconstruction of the creation/construction of scientific physical concepts.
  - ii. The meaning of *theoretical physical* concepts: the *conceptual role* implicitly defined by the axioms of physical theories.
  - iii. The advancement of physical knowledge progresses from the subjective/“perspectival”/qualitative perceptual experiences towards the – more and more – objective/“perspectiveless”/quantitative theoretical descriptions of the phenomena.<sup>7</sup>

Schlick's external world realism was in important respects close to a version of critical realism, propounded earlier by Alois Riehl.<sup>8</sup> Accordingly, the *Ding an sich* world outside consciousness exists and certain aspects of it *can* be known; genuine scientific knowledge is about the nature of the external, *Ding an sich* world.

Schlick's theory of knowledge aims at integrating his external world realism with his empiricist epistemology the following way. It is admitted that we have no *direct* knowledge of the external world; but this is not a problem, for we have no direct knowledge about anything else either (there is no intuitive knowledge whatsoever). But we do know that there is an external world (based philosophical arguments directed against immanence philosophies), and we also have knowledge about (certain aspects of) it, along the way Schlick's general theory of knowledge and his account of physical concepts describe it.

<sup>7</sup> “Perspectival” and “perspectiveless” in the sense of Nagel's use of these terms in Nagel 1986.

<sup>8</sup> See Riehl 1887, Heidelberger 2006.

#### IV. RUSSELL'S UNDERSTANDING OF PHYSICAL- CONCEPT-STRUCTURALISM

Russell advocated structuralism about physical concepts mainly in his Russelian monist period, e.g. in *The Analysis of Matter* (1927) and in *Human Knowledge* (1948) but he formulated structuralist views already earlier, in the *Introduction to Mathematical Philosophy* (1919), and even in *The Problems of Philosophy* (1912). His brand of structuralism was markedly different from Schlick's as the following quotes attest.

There has been a great deal of speculation in traditional philosophy which might have been avoided if the importance of structure, and the difficulty of getting behind it, had been realised. For example, *it is often said that space and time are subjective, but they have objective counterparts; or that phenomena are subjective, but are caused by things in themselves, which must have differences inter se corresponding with the differences in the phenomena to which they give rise.* Where such hypotheses are made, it is generally supposed that we can know very little about the objective counterparts. In actual fact, however, *if the hypotheses as stated were correct, the objective counterparts would form a world having the same structure as the phenomenal world, and allowing us to infer from phenomena the truth of all propositions that can be stated in abstract terms and are known to be true of phenomena.* If the phenomenal world has three dimensions, so must the world behind phenomena; if the phenomenal world is Euclidean, so must the other be; and so on. In short, every proposition having a communicable significance must be true of both worlds or of neither: the only difference must lie in just that essence of individuality which always eludes words and baffles description, but which, for that very reason, is irrelevant to science. (Russell 1919. 61; my emphasis.)

Thus it would seem that, wherever we infer from perceptions, it is only structure that we can validly infer; and structure is what can be expressed by mathematical logic, which includes mathematics (Russell 1927. 254).

The only legitimate attitude about the physical world seems to be one of complete agnosticism as regards all *but its mathematical properties* (Russell 1927. 270).

In order to illuminate Russell's conception we have to clarify some of his fundamental notions, namely: intrinsic properties are first-order properties of entities, both monadic and relational. Structural properties are second- or higher-order formal-mathematical properties of intrinsic properties. Physical concepts refer to structural properties of physical (i.e. external worldly) objects, that is to second- or higher-order formal-mathematical properties of them. Some examples of intrinsic properties may be: the location of perceptual events in phenomenal space and time; colour qualities; relations of colours as e.g. colour distance, col-



our temperature; location in physical space, relations between spatial points, e.g. distance.

Structural properties are the abstract, mathematico-logical properties of these intrinsic properties such as reflexivity, symmetry or a transitivity (for example, the similarity of colour qualities is symmetrical and intransitive). These abstract structural properties, Russell emphasizes, say nothing about the intrinsic nature of the properties they are properties of; hence a colour-space and a sound-space may have the same abstract structural properties.

Very briefly, Russell's argues for his structuralist understanding of physical concepts as follows. According to Votsis' (2004) reconstruction, Russell's arguments are based on the Helmholtz-Weyl principle and the Mirroring Relations principle. According to the Helmholtz-Weyl Principle "we assume that differing percepts have differing stimuli" (*The Analysis of Matter*. 255). In short, *different effects (i.e. percepts) imply different causes (i.e. stimuli/physical objects)*. The Mirroring Relations Principle asserts that "(...) the relations which physics assumes (...) are not identical with those which we perceive (...) but merely correspond with them in a manner which preserves their logical (mathematical) properties" (*The Analysis of Matter*. 252). In short, relations between percepts mirror (i.e. have the same mathematical properties as) relations between their non-perceptual causes.

From these principles Russell's thesis apparently follows, according to which the structural properties of the external world are knowable, and the scientific physical concepts grasp these structural properties. As it is well-known, Newman (1928) formulated an objection asserting that Russell's structuralism is near-vacuous,<sup>9</sup> but I will not address this topic here, since my aim is not the evaluation of Russell's view but its reconstruction and comparison with other structuralist views.

## V. RUSSELL'S CONTEXT AND MOTIVATION

Russell's motivation for advocating structuralism about physical concepts was similar to Schlick's: his goal may also be seen as to reconcile external world realism with an epistemology with strong empiricist leanings.<sup>10</sup> Russell was an external world realist from 1898 (since his break with idealism) – though, of

<sup>9</sup> According to objection, in case the cardinality of the physical objects and the percepts representing them is the same, then the existence of a concrete structure of the physical entities isomorphic with the concrete structure of the percepts (which represent the physical entities) follows simply from set theory, hence it provides no empirical information about the properties of the physical objects (except for their cardinality).

<sup>10</sup> Russell's attitude towards empiricism was not so unambiguous as Schlick's. Until around 1912 Russell was a Platonist concerning logic and mathematics, further he accepted the existence of universals and also held that some universals are known directly by acquaintance. These are, of course, no empiricist views.

course, his ontological views changed greatly from his early extreme ontological pluralism towards his later more modest realism. In 1914 he abandoned external world realism in favor of phenomenalism – in e.g. *Our Knowledge of the External World* (1914); *The Relation of Sense Data to Physics* (1915) etc. –, but then again he switched back to external world realism (*Introduction to Mathematical Philosophy* [1919]; *The Analysis of Matter* [1927]; *Human Knowledge* [1948]). On the other hand, Russell advocated *the principle of acquaintance* since 1905, according to which „whenever a relation of supposing or judging occurs, the terms to which the supposing or judging mind is related by the relation of supposing or judging must be terms with which the mind in question is acquainted” (Knowledge by Acquaintance and Knowledge by Description [1910]). The principle can be viewed as a linguistic grounding of the Cartesian demand for certain knowledge by a meaningfulness criterion: only such statements are meaningful which can be known, in principle, with certainty. Further, after returning to realism from phenomenalism Russell advocated a causal-representational theory of perception: perceptual experiences are caused by the external objects, which they represent.

These views, however, seem to be *prima facie* in conflict. For on the one hand, the content of perceptual sentences are external objects and properties, but on the other, according to the principle of acquaintance, for a sentence to count as possibly expressing knowledge, its terms must refer to objects with which we are acquainted; but we are not acquainted with external objects, only with sense data or percepts (plus universals). Russell’s solution to this problem is the following. We *do not know* the intrinsic qualities of the objective world, since our perceptual experiences of the external world screen them off (the veil of experience). But *we can know* the structural properties of the external world. For we can know the structural properties of our percepts, since they are abstract, second-order properties of the intrinsic properties of our percepts with which we are acquainted. And the structural properties of our percepts are *isomorphic* with structural properties of those objective, external (extra-mental) events that are spatio-temporally continuous with our percepts and cause them. Further, knowledge claims about the structural properties of the external objects *can be formulated meaningfully* because *we know these structural properties*, since they are isomorphic with the structural properties of our percepts, and we know the latter by relying on our acquaintance knowledge about the intrinsic properties of our percepts.

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As it is well-known, Russell then abandoned his Platonism due to the influence of Wittgenstein. However, his later views concerning universals were still ambiguous. Nonetheless it is evident that his interest turned towards empirical sciences and the nature of empirical knowledge. Further, that his analysis of the meaning of sentences about the external world may be seen as expressing an empiricist attitude, due to its being constrained by the principle of acquaintance, which may be seen as (a partly) empiricist criterion of meaningfulness.

## VI. PHYSICAL-CONCEPT-STRUCTURALISM BY CHALMERS

Among the contemporary advocates of physical-concept-structuralism the dominant view is that microphysical terms are causal-role concepts.<sup>11</sup> A characteristic representative of such views is Chalmers' account: according to him, physical concepts – among them microphysical concepts – describe *the functional/causal role* their referents play. For example, the meaning of “mass” is to be understood as: the property *that plays the “mass-role”*, i.e. an entity having the property of mass causes other entities also having mass to move in certain ways, and other entities having mass cause it to move in certain ways. (Somewhat more precisely: an entity having mass  $m_1$  in the neighbourhood of another entity having mass  $m_2$  behaves (moves, exerts force) in accordance with the equation  $F = km_1m_2/r^2$ ).

Such an account of microphysical concepts can be accommodated both with Chalmers current Russellian monist views, and his earlier non-physicalist naturalist property-dualist view (proposed in *The Conscious Mind*, 1996). According to the former some microphysical concepts refer to micro-phenomenal qualities or proto-phenomenal qualities identifying them by their causal role (not by their micro-phenomenal qualities). According to the latter, they refer only to non-phenomenal microphysical qualities.

Chalmers' motivation for structuralism about physical concepts, in contrast with Schlick and Russell (and Maxwell), was *not* the goal of *reconciling external world realism and empiricist epistemology*. Such a reconciliation was a real task for Schlick or Russell, partly because external world realism was a real issue for them; phenomenalism and different versions of neo-Kantianism were serious

<sup>11</sup> Among the contemporaries or near contemporaries, Grover Maxwell's view is historically very significant as it represents an important link in the story leading from Russell's physical-concept-structuralism towards the contemporary Russellian views, which all take physical concepts to be causal-role concepts. Maxwell held, similarly to Russell, that *all physical concepts* are *theoretical concepts* (all entities *not given* to the mind are theoretical entities). But, contrary to Russell, he interpreted the meaning of theoretical terms in the framework of the Ramsey-sentence account of the meaning of theoretical terms. Accordingly, *theoretical terms have reference*, and their reference is determined indirectly, by their role in the network of the *causal (and logical) relations* expressed by the physical theory, i.e. “by description”. Thus theoretical terms, hence *all physical terms*, refer to *external objects*, which are identified by their structural properties. But at the same time, the identifying descriptions of physical terms contain only terms with the reference of which *we are acquainted* (viz. only terms referring to the phenomenal qualities and to logical relations which appear in the Ramsey-sentence), in line with what Russell's *principle of acquaintance* demands.

Accordingly, we cannot know any intrinsic properties of physical events by direct observation, i.e. by acquaintance, *the whole physical world is unobservable*. But by description, i.e. with our physical theories we can obtain knowledge about certain properties of the physical world, namely the structural (higher-order) properties of physical events. These structural properties are *the causal roles* of the physical events. In general, *all theoretical physical concepts* are causal role concepts, according to Maxwell. More specifically, brain event concepts like “c-fibre firing” are also causal-role concepts, which refer to a causal structure which a certain event-complex of the c-fibre regions of the brain possesses (see Maxwell 1979).

contenders in the early 20<sup>th</sup> century when they formulated their structuralist views; and an epistemology according to which knowledge must be anchored in perceptually observable facts was not readily reconcilable with it.

However, the later developments in philosophy of science and analytic metaphysics in the 20<sup>th</sup> century reshaped the theoretical context in such a way that the original formulation of problem became obsolete. With the idea, advocated by the logical empiricists, that the best available knowledge about the physical world is provided by physical science, which need not and cannot be justified from without, by a special *philosophical* epistemology, and with realism about the content of physical theories, the original question was overcome. So what, then, were the sources and motivations of Chalmers' structuralism?

One motivation may have been that structuralism about microphysical concepts underlies Russellian monism, which, according to Chalmers, facilitates a more adequate account of the consciousness-brain relation than the alternatives (namely that it solves the problem of mental causation and answers the conceivability argument). It is worth remarking that while other proponents of physical-concept-structuralism also recognized this implication, it was not their main motivation for accepting it; it was rather taken as a further bonus for those wishing for a naturalistic account of the consciousness-brain relation.

However, some further motivations may be unearthed from Chalmers' works. It seems Chalmers' microphysical-concept-structuralism is based on what we may call as *the functional analysis thesis*:

(FA) The meaning of *all* physical concepts, viz. micro- and macrophysical, chemical, biological and cognitive psychological concepts is provided by a *functional analysis* which identifies the causal role of the denotatum of the terms.

So if the general (FA) thesis is justified, so is microphysical-concept-structuralism. But what are the sources and the support for the functional analysis thesis? In my view, these may be the following:

- A. (FA) may be based on Chalmers' account of the meaning of natural kind terms.
- B. (FA) may help to explain why the ontology of the physical has a layered structure. And perhaps also
- C. (FA) may be based on the „Canberra Plan”.

Let's see these in turn.

A. *The meaning of natural kind terms*. In my view, one source of Chalmers' understanding of the meaning of microphysical concepts is his *theory of meaning of natural kind terms*. *Prima facie*, there is a plausible connection here; after all, if there are natural kinds *at all*, *electron* or *charge* seem obviously candidates for being natural kinds.

Chalmers' semantic theory about natural kind terms may be seen as a synthesis of Kripkean and Fregean insights. The gist of these views may be illustrated roughly as follows.<sup>12</sup> According to Kripke, the meaning of "water" is: the substance that has those *essential properties* (in this case: chemical constitution) which that substance has which plays the "water-role" *in our world*. According to the Fregean view: "water" is the substance which plays the "water-role" whatever it(s) constitution) may be. Chalmers embraces both these aspects of meaning in his two-dimensional semantics, expressing it by the concepts of primary and secondary intension. According to the primary intension, "water" is the substance that which plays the "water-role" in world  $w$ , considering  $w$  as actual. According to the secondary intension, "water" is the substance that which plays the "water-role" in world  $w$ , considering  $w$  as counterfactual. Now, if electron is a natural kind, then accordingly the secondary intension of "electron" is: the entity that which actually plays the "electron-role", in all worlds. (As for the primary intension, the issue is more controversial: for it may be argued that in the case of fundamental microphysical types such as electron, the "role" and the intrinsic property which it identifies are necessarily connected so that it is not possible that the "electron-role" is played by some property different from that which is the realizer of the role in our world – unlike in the case of "water" and other higher-level types.)

Some doubts, however, may be raised about such an extension of the theory of meaning. For the theory of natural kind terms of Kripke, and also of Chalmers, relies on *the ordinary language use* of such terms, and some related *metaphysical* and *semantic intuitions* and arguments: i.e. on *a piece of analytic metaphysics*. But it is questionable whether the meaning microphysical terms can be adequately based on such grounds.

In other words, the theory of meaning Chalmers extends to microphysical terms (like "electron", "proton", "charge", "spin") is originally about macrophysical or chemical kind terms ("water", "gold") or macro-biological kind terms ("tiger"). But is such an extension readily acceptable? Kripke himself did not indicate much how his theory should be applied to theoretical terms. There are certain problems with the application of Kripke's account of the meaning of natural kind terms to the meaning of theoretical terms like microphysical terms (cf. e.g. with Papineau 1996) and these problems may be inherited by Chalmers' account of microphysical terms. However, I shall not pursue this issue here any further.

*B. (FA) helps to explain why the ontology of the physical has a layered structure.* Another source of support for the (FA) thesis may be the supposition that if we accept (FA) then we have an explanation of the layered ontological structure of physi-

<sup>12</sup> Chalmers 1996, Chalmers 2006a.

cal phenomena, which Chalmers accepts (in line with the widespread view). For Chalmers hold that

- (1) All higher-order physical properties (i.e. chemical, biochemical, biological, cognitive psychological) are metaphysically determined by the microphysical properties. And
- (2) Every physical property is metaphysically determined by properties one level below; these determining properties are also metaphysically determined by other properties one level below them; and *a fortiori*, until the lowest (micro)physical level which is *not* determined by anything under it, these are the ultimate fundamental properties.<sup>13</sup>

This layered structure of metaphysical determination is explicated by Chalmers, relying on the (FA) functional analysis thesis as follows. Of any property  $P_n$  on level  $n$  a *reductive explanation* can be given, namely: there exists another property  $P_{n-1}$  (or a set of properties  $P_{n-1}^1, \dots, P_{n-1}^m$ ), on level  $n-1$  which *satisfies the functional description* of  $P_n$ , viz. it *realizes the causal role* associated with  $P_n$ . This is so down until the lowest level; but of  $P_0$ , the property realizing the causal role of  $P_1$ , a functional analysis cannot be given,  $P_0$  is an irreducible ultimate quality.

Now, relying on this account we may obtain a justification of (FA), in the following way: *if* (FA) is true, *then* the layeredness of the ontology of the physical *can be well explicated* relying on (FA). That is, we can explicate *how* microphysical properties metaphysically determine higher-level physical properties. The explanation is provided by the level-by-level reductive explanations of  $P_n$ -s by  $P_{n-1}$ -s, (i.e.  $P_{n-1}$ -s realizing the causal roles of  $P_n$ -s) which is enabled by the assumption that all  $P_n$ -s (except for  $P_0$ ) have a functional analysis. Although this is *not a conclusive argument* in favour of (FA), nonetheless it provides *strong support* for it (especially if there is no alternative explicatory conception).

There is, however, a problem with such a justification. This argument in support of the (FA) thesis may work only if (1), the claim that higher-level physical properties *metaphysically supervene* of on microphysical ones, is *independently justified*. But this seems not so. For Chalmers argues for (1) by an (in)conceivability argument. Briefly: if we set all microphysical facts, then it is *inconceivable* that some higher-level physical fact would be different from what it actually is. (An example of such a scenario may be: if we set all microphysical facts of the world then it is not conceivable that a particular wombat having in actual fact two offsprings, could have only one, or three; see Chalmers 1996. 73).

But *why* would it be *inconceivable* that there may be higher-level physical properties which are not fixed by the microphysical properties? Because Chalmers *denies* the existence of *emergent physical properties* which are not determined

<sup>13</sup> Cf. e.g. Chalmers 1996. 43–46, Chalmers–Jackson 2001.

metaphysically (i.e. not entailed a priori) by the microphysical properties. The problem is, however, that Chalmers seems to ground the exclusion of such higher-order emergent physical properties *on the (FA) thesis*, together with the further idea based on (FA), according to which *all properties can be reductively explained in terms of one level lower properties*. For if a property is reductively explainable in terms of properties one level below, then this is tantamount to the claim that the lower-level property implies, *logically determines* the higher-level one. Hence there is no such logical possibility that the lower-level property is instantiated while the higher-level property does not; hence such a scenario is *inconceivable*.

But then, Chalmers' *inconceivability argument for (1)* the metaphysical supervenience thesis, *relies on (FA)*, the functional analysis thesis. Hence the justification of (FA) cannot be that (1) the metaphysical supervenience thesis can be well explicated by the (FA), since such justification would require that the truth of (1) does not depend on (FA) – but it does, it seems.

There is also another formulation of the argument for the metaphysical supervenience of all higher-level physical facts on microphysical facts provided in Chalmers-Jackson 2001. Accordingly, PQTI, the conjunction of all microphysical (P), phenomenal (Q), that's all (T) and indexical (I) truths implies M, where M is any arbitrary macrophysical truth, like e.g. „Water is H<sub>2</sub>O”, or „Water is to be found in lakes on Earth” etc.<sup>14</sup>

PQTI implies M, because

(1) PQTI implies complete information (in the language of physics) about the structure, dynamics, composition and distribution of macroscopic systems.

(2) This information about the structure, dynamics, composition and distribution of macroscopic systems, and appearance implies ordinary macroscopic truth, such as M.

So, for example „Water is H<sub>2</sub>O” is implied by PQTI (in particular by P, the complete set of microphysical facts).

According to Chalmers, such a justification for the thesis that microphysical facts (P) imply all macrophysical facts is that the thesis is „extremely plausible”. So, we may ask, *why* is this thesis *extremely plausible*?

i. One support for the „extreme plausibility” claim is that Chalmers rejects that there is downward causation from higher-level physical states to microphysical states. If we allowed for a downward causal capacity of some higher-level physical properties then the entailment thesis would fail. In „Strong and Weak

<sup>14</sup> Here I will not address the particular issues which were debated between Chalmers and Jackson with Block and Stalnaker, i.e. whether the explicit definability of the concepts of higher-level properties in terms of microphysical concepts are required for the entailment thesis to hold (Block–Stalnaker 1999, Chalmers–Jackson 2001). I am not concerned with this issue here, because the worries I discuss seem to be grounded even if we accept Chalmers' position in this debate.



Supervenience” (2006b) Chalmers contends that downward causation is not logically impossible, however, there is no empirical evidence for it, so he is sceptical about it. So, accordingly, Chalmers’ view is that the metaphysical supervenience thesis has a strong empirical support.

ii. Second, Chalmers also argues as follows:

(the information in P) includes complete information about the structure and dynamics of the world at the microphysical level: in particular it includes or implies the complete truth about the spatio-temporal position, velocity and mass of microphysical entities. This information suffices in turn to imply information about the structure and dynamics of the world at the macroscopic level, at least insofar as this structure and dynamics can be captured in terms of spatiotemporal structure (position, velocity, shape, etc.) and mass distribution. For example, for any given region of space and time, the information in P implies information about the mass density in the region, the mass density in various subregions, the causal connections among various complex configurations of matter in the region, and the extent to which the matter in the region behaves or is disposed to behave as a coherent system. (...) The central point here is that a macroscopic description of the world in the language of physics is implied by a microscopic description of the world in the language of physics. Such a thesis is extremely plausible: it is not subject to any worries about the translation between vocabularies, and involves only a change in scale. (Chalmers–Jackson 2001. 330.)

So the extreme plausibility is based on the idea the microscopic and macroscopic objects and states of affairs are characterized by the same kinds of properties (with the concepts of “spacetime position”, “velocity”, “mass” having the same meaning both in the micro and macro descriptions). However, even if we accept this account of the relation between the macroscopic and the microscopic descriptions of the world in the language of physics (which may be questioned cf. e.g. Block–Stalnaker), it is clear that biological properties of macroscopic biological systems (i.e. complex macrophysical systems) are not described in the language of physics; so that all biological facts are implied by the microscopic description of the world is not obvious. Here again, the (FA) thesis may come to the rescue. For provided there is a functional description of the biological property, then according to Chalmers’ assumption it is in principle possible to find some biochemical properties satisfying this causal/functional description, and then some lower-level chemical properties satisfying the causal roles of the biochemical properties, *a fortiori* until we get to the level where the realizer properties are described in the language of *physics*. But then, it seems, in order to support the general metaphysical supervenience thesis about physical phenomena, according to which all higher-level physical facts are metaphysically determined by the microphysical facts, we again relied on the (FA) thesis, so the metaphysical supervenience thesis is not independently justified.



*C. The Justification of (FA) in line with the „Canberra Plan”?*

Just to mention very briefly a further possible support for the (FA) thesis: Chalmers and Jackson also advocated a general metaphysical programme, the so-called „Canberra Plan”, according to which *all* concepts, not only scientific physical concepts but also folk concepts, ought to be constructed in the way theoretical physical concepts are. Accordingly not only „charge” is what actually plays the „charge-role”, but also „free will” is what actually plays the „free will role”, and „Gödel” who actually plays the „Gödel-role”. So if it were true that for *all terms* a corresponding causal-functional concept can be provided, according to the methods of the Canberra Plan, then this may provide support for the (FA) thesis such that it does not rely implicitly on the thesis according to which microphysical truths (or the PQTI) logically entail all higher-level physical truths.

I think, however, that no further support is available for the (FA) thesis from this direction, since the Canberra Plan is an extension of the (FA) thesis to *other kinds of concepts* beyond the theoretical physical concepts. Further, it seems, such extension leaves untouched the objections against (FA) formulated above.

To sum up: we have seen that Chalmers’ view about the meaning of physical concepts *comes from a very different background, theoretical framework* than Schlick’s and Russell’s. Further, that Chalmers’ arguments for his version of physical-concept-structuralism have their own problems, namely:

- (1) It is not unambiguous that the theory of meaning about ordinary (macrophysical or macrobiological) natural kind terms (“water”, “gold”, “tiger”) is readily applicable to microphysical terms.
- (2) The argument for (FA) based on the layered ontology of the physical seems question-begging.
- (3) The Canberra Plan does not provide a further justification of (FA), since it is an extension of it to other (not physical) concepts.

### III. CONCLUDING REMARKS: THE VIRTUES OF SCHLICK OVER RUSSELL AND CHALMERS

Since the content, background and motivation of Schlick’s, Russell’s and Chalmers’ structuralism about physical concepts are rather different, it is difficult to give an evaluative comparison of them. Therefore I focus instead on some virtues Schlick’s account may have over the others’.

*The general method of determining the meaning of physical concepts.* Schlick grounds his theory about the meaning of theoretical physical concepts *on the reconstruction of the actual methods* of concept formation in physical sciences. Russell, in con-

trast, bases his account on a specific philosophical theory of perception, and also on a strong philosophical epistemological constraint, expressed by the principle of acquaintance. This seems too restrictive, and further, it is in opposition with Schlick's general approach, according to which *scientific knowledge need not and cannot be justified from without, by philosophy*. As for Chalmers: he bases his account on a conceptual analysis grounded in ordinary and philosophical (metaphysical, linguistic and epistemological) intuitions, in the vein of contemporary analytic metaphysics.

*The explanation of why physics does not grasp the intrinsic qualities of natural phenomena.* Schlick explains more plausibly *why scientific physical concepts do not involve the qualities* of their referents. This is a consequence of the general features of scientific physical concept formation; i.e. it simply follows from the proposition that theoretical physical descriptions are purely quantitative, they leave out qualities from the description of nature, but not from nature itself. For Russell this is a consequence of his quasi-Cartesian account of the perception-world relation, according to which perceptual experiences screen off the intrinsic properties external objects. This seems too a restrictive. (Note also, that this formulation of "screening off" would be nonsensical according Schlick, a sort of category-mistake.) According to Chalmers, *all* physical concepts can be functionally analysed, and functional descriptions *eo ipso* leave out the qualities of their referents. But the functional analysis thesis seems not sufficiently supported.

*Explaining (away) dualistic intuitions.* The meaning of scientific physical concepts involve no appeal to qualia; hence the intuitiveness of qualia not being physical. But scientific physical concepts may nonetheless refer to qualia – hence the explaining away of the intuition. (Note that this way of explaining away dualistic intuitions is *not a refutation of property-dualism, rather its acceptance*, as it accepts the reality of qualia *on a par with* non-phenomenal physical qualities. So it is rather the *refutation of non-physicalistic property-dualism*, according to which qualia naturally supervene on non-phenomenal physical properties, and the *vindication of physicalistic property-dualism*, according to which qualia are on the same ontological level as non-phenomenal physical qualities.)

So we may conclude that Feigl was right, the views of Schlick and Russell (and we may add: Chalmers) are in fact *in a remarkable agreement*: they all accept (1) linguistic physicalism; (2) physicalist dualist property-pluralism; (3) a dual-language account of the consciousness-brainstate identity thesis, i.e. Austrian or Russellian identity theory; and that (4) structuralism about physical concepts play a substantive role in the grounding of (1)-(3). However, they also *importantly differ* in how they lay out the structuralist idea, both in content, context and motivation.

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## Austrian Philosophy: Outlines of a Discipline at the University of Vienna in the 20<sup>th</sup> Century\*

### I. BACKGROUND

Since the foundation of the University of Vienna, the Faculty of Philosophy, as a “faculty of the arts”, had played a subordinate role – also as a field of study – in relation to the faculties of Medicine, Law and Theology.<sup>1</sup> The Faculty continued to be instrumentalized as an *ancilla* by the respective prevailing powers until 1848. In predominantly Catholic Austria, Immanuel Kant’s philosophy was marginalized for being enlightening or revolutionary, and the position on the philosophy of German idealism was regarded as crucial to the development of a specifically “Austrian philosophy” after 1848. This happened in the spirit of an alleged Austrian *Sonderweg* (distinct course in Austria-Hungary) against each form of dialectic, transcendental and aprioristic philosophy of Prussian-German origin.<sup>2</sup> But given the specific development in the monarchy, this very claim should be examined with a critical eye to increase the focus on Kant’s polarizing or identity-defining role in proportion to the Austrian line of tradition extending from Bernard Bolzano through Robert Zimmermann and up to Franz Brentano and his influential school.<sup>3</sup> There is no question that the Königsberg philosopher remained a reference until the heyday of the Vienna Circle and also thereafter. It was about answering the question of the autonomy and scientific nature of philosophy given the growing importance of individual disciplines of the cultural, social and natural sciences (including mathematics) in the maelstrom of the second scientific revolution. From the beginning, the propaedeutic nature of the Faculty of Philosophy and the subordinate role of philosophy had in any case simultaneously prompted Kant’s appeal to revalue the “lower” faculty against the three “higher” ones due to its intrinsic formative aspect and the

\* This article is an abridged English version of Stadler 2015.

<sup>1</sup> A descriptive overview is the unpublished dissertation of Wieser 1950.

<sup>2</sup> Regarding the thesis of a typical “Austrian philosophy”: Lehrer et al. 1997.

<sup>3</sup> On the relation of scientific philosophy and the Vienna Circle to (Neo-)Kantianism see Stadler 2015 and 2018.

reflective reason versus the profession-oriented faculties for physicians, government officials and theologians.<sup>4</sup>

If we characterize the philosophical scene at the University of Vienna during the Austro-Hungarian Empire from the mid-19<sup>th</sup> century to the end of the First World War, we can identify the following main lines:

- A marginal Hegelianism and a moderate Kantian tradition.
- A predominantly anti-idealistic philosophy concerned with linguistic criticism, which was oriented towards empirical individual disciplines and spanned ideologically from the Catholic to the social-liberal (late-) Enlightenment. The direction of empirical philosophy and psychology claiming exact methodology that took its origin from Brentano dominated towards the end of the 19<sup>th</sup> century; in the latter phase, up until the First World War, Ernst Mach prevailed with his teaching, which had an impact up until the inter-war period.
- At the same time, a diverse philosophical “counter world” of a metaphysical supreme discipline existed apart from actual research work.

If – despite all differentiation – we want to mention essential characteristics of “Austrian philosophy” in Vienna during the monarchy, we can certainly agree with Carl Siegel in noting a trend towards objectivism and realism from an epistemological and logical perspective (Siegel 1930). Schools, institutions and social movements make the contents and outlines of all these trends more understandable, which document a stronger presence of philosophy within the faculty and university.<sup>5</sup>

## II. NEW BEGINNINGS IN THE FIRST AUSTRIAN REPUBLIC

Following the vacancies during the First World War (with the exception Adolf Stöhr), three chairs were filled at the same time in 1922. This represented an upswing, which was to secure Viennese philosophy and psychology being recognized worldwide up until the time of the Austro-fascist *Ständestaat* (corporative state).

With this unique initiative, Moritz Schlick who was to become the centre of the world-famous Vienna Circle until his assassination at the university in June 1936 and also served as head of the Ernst Mach Society (1928–1934) continued the direction pursued since Mach and Boltzmann. On the other hand, the emerging developmental, experimental and cognitive psychology became established till 1938 with Karl Bühler and his wife Charlotte Bühler.

<sup>4</sup> Kant 1798/1986; for a discussion of Kant’s late writing: Gerhardt 2005.

<sup>5</sup> As general references: Meister 1927 and 1937; Bauer 1966; Benedikt 1992; Acham 1999.

The history of philosophy and the tradition of transcendental philosophy continued to be systematically backed, also by Robert Reininger, within the Vienna Philosophical Society, later on covering the Austrian Kant-Gesellschaft. As of 1924, the history of ancient philosophy was further strengthened through Heinrich Gomperz's appointment to the fourth chair before he had to leave his position early due to his opposition to the Schuschnigg regime. In 1935, his full professorship was converted into a tenure track assistant professorship to which the Catholic philosopher Dietrich von Hildebrand who came from Munich was appointed for two years until the "annexation".

Alois Dempf who held this professorship from 1937 to 1938 until his dismissal by the national socialists was called to succeed Schlick. This represented a markedly radical change of the tradition and denomination of Schlick's chair for natural philosophy, which now became Catholic-oriented metaphysics and Christian *Weltanschauung* – a direction that again displayed continuity in terms of personnel and content after 1945 with Dempf's return and the appointment of Leo Gabriel.

Among the external lecturers who attained their *habilitation* (*venia docendi*) in the inter-war period, Sigmund Kornfeld, Hans Eibl, Karl von Roretz and Rudolf Carnap, who along with Schlick was to be found at the very core of the Vienna Circle before moving to Prague in 1931, are worth mentioning here. Friedrich Waismann, a student and long-term assistant of Schlick's was able to work at the institute as a librarian until 1936 without being officially employed. He gave lectures on a regular basis and was a key member of the Vienna Circle, also as a dialogue partner of Ludwig Wittgenstein (McGuinness 1984).

Looking at the subject of philosophy at the Vienna University in concrete terms, we can see that the teaching faculty (altogether 22) during the period from 1918 to 1938 included Moritz Schlick who taught scientific and analytical philosophy as a full professor from 1922 to 1936, Rudolf Carnap as an extraordinary lecturer and titular professor from 1926 to 1931 and Viktor Kraft as an extraordinary lecturer and titular professor from 1914 to 1938. The most widely taught subject was history of philosophy, which along with ethics also drew the largest number of listeners. The already mentioned *Philosophical Society of the University of Vienna* was a crucial affiliated institution and simultaneously the local group of the Kant-Gesellschaft (*Kant Society*) from 1927. Scientific philosophy accounted for about one seventh of the lectures held at the Philosophical Society as was the case in Vienna International University Courses. If we focus on the discipline philosophy at the University of Vienna between 1918 and 1938 (Wieser 1950, 158, 231 and 235 ff.), we can identify as proponents of scientific philosophy Moritz Schlick, Rudolf Carnap, and Viktor Kraft out of 22 teachers in total. The most frequent topics were history of philosophy together with ethics, both of which had also the largest attendance of students. In parallel, the *Philosophische Gesellschaft an der Universität Wien* (*Philosophical Society at the University of Vienna*), since 1927 also acting as the

Austrian branch of the German Kant-Gesellschaft (Kant Society), was an important and influential society covering a broad spectrum of philosophical research (Reininger 1938. 21–43). As in the “Wiener Internationale Hochschulkurse” (Vienna International University Lectures) only one seventh is to be judged as part of the scientific philosophy paradigm (Gabriel 1972. 8 and 14).

### III. ON PHILOSOPHY DURING AUSTROFASCISM AND NATIONAL SOCIALISM

Schlick’s assassination on 22 June 1936<sup>6</sup> marked a de facto end to the then already world-famous Vienna Circle and to analytic and scientific philosophy in Austria before National Socialism caused a violent and definitive close to this philosophical movement.<sup>7</sup> The consequences of the destruction and expulsion of this scientific culture by the anti-Semitic forces at the University of Vienna continued to have an impact for a long time well into the Second Republic (Stadler 2005; Pasteur et.al. 2003–2004). After the Nazis seized power in March 1938, dismissals and expulsions in the spirit of the racist dictatorial state occurred, with support also coming from members of the University of Vienna.

Prior to the “annexation”, three chairs existed at the Institute of Philosophy. (As an overview: Benetka 1995. 338 ff.) These chairs were held by Alois Dempf, Robert Reininger and Karl Bühler.<sup>8</sup> The changes due to the takeover of power by the National Socialists after March 1938 signaled the attempt of political and ideological “standardizing” but were also an expression of a polycratic science policy of National Socialists between the poles of Berlin and the “Ostmark”.<sup>9</sup>

Professor of Christian philosophy Alois Dempf’s *venia docendi* was withdrawn and he was forced to retire for political and ideological reasons. Due to his activities in Red Vienna and his Jewish wife Charlotte Bühler, Karl Bühler, founder of the Vienna school of cognitive and Gestalt psychology was also dismissed and forced to emigrate to the U.S.<sup>10</sup> His long-standing coworker Egon Brunswik had emigrated to the US to Berkeley one year earlier, too, and was followed by his later wife Else Frenkel-Brunswik. We can thus speak of a total break at the University of Vienna in the case of the Bühler school. This break also meant the end

<sup>6</sup> On the background of this murder: Lotz 2009.

<sup>7</sup> As a characterization of the preceding “conservative revolution”: Mohler 1972; on the expulsion of scientific philosophers and philosophers of science: Stadler 2010; on the intellectual migration in general: Stadler et al. 1995.

<sup>8</sup> It has to be mentioned that psychology and pedagogy were linked together within a research and teaching field. Cf. Brezinka 2000; Olechowski 2015.

<sup>9</sup> Heiss 2003; Huber et al. 2011; Huber 2012; Pfefferle 2014. Memory book for the victims of NS at the University of Vienna: < <http://gedenkbuch.univie.ac.at> > Last access 30-11-2018.

<sup>10</sup> On the dismissal and forced migration of Karl and Charlotte Bühler: Ash 2004; Eschbach et al. 2004. On the consequences of the NS for psychology: Benetka et al. 1988. 147–167.



of an innovative cooperation between philosophy and psychology (Karl Bühler and Moritz Schlick) as well psychoanalysis, which failed to gain a foothold as an academic discipline, and social research (as part of the Research Unit for Economic Psychology around the Büblers).<sup>11</sup> It was during the National Socialist era that the psychologist Hubert Rohrer launched his career. Similar to Richard Meister in pedagogy, Rohrer worked for psychology and university politics well into the Second Republic. After the “annexation”, psycholinguist Friedrich Kainz was appointed provisional head of the Institute of Psychology and – as a successor to Dietrich von Hildebrand who was dismissed in March and emigrated to the U.S. later – received the vacant tenure track position as an associate professor for philosophy with a particular focus on aesthetics and the psychology of language. (On Kainz in more detail: Heiss 1993.) Kainz is a typical example of an opportunist and wryneck who moved up the career ladder in the Second Republic (Tilitzky 2002. 778 ff.) and worked as a full professor for psychology of language, aesthetics, art philosophy and history of philosophy. (On the life and work of Kainz: Gelbmann 2004; Levelt 2014.)

Finally, reconstruction in the Nazi spirit was to be put into practice by electing Gunter Ipsen and Arnold Gehlen to the two chairs in philosophy: Karl Bühler’s chair was filled by SA and NSDAP member Ipsen who came from Königsberg on 22 May 1939 as professor for *philosophy and ethnology* (*Philosophie und Volkslehre*) and was appointed director of the Institute of Psychology in September 1943. Expectations of a new philosophy for the purposes of the National Socialist expansion policy in the East and South East seem to have played a role just as the interdisciplinary perspective for National Socialism’s demographic policy ambitions. Since Ipsen was recruited to the military after the outbreak of the war, his field of action with regard to the expected philosophical and sociological support of *Ostforschung* (*research on the East*) remained limited until 1945 when he was dismissed from the University of Vienna like all *Reichsdeutschen* (*Germans of the Reich*). From November 1940, Robert Reininger who retired in 1939 was succeeded by philosopher and sociologist Gehlen who had held the *Kant chair* in Königsberg since 1938 and was subsequently commissioned after the “annexation” by the Reich Ministry of Education to reorganize philosophy and the institutes of the Faculty of Philosophy in Vienna. Simultaneously, he took up the directorship of the Institute of Psychology in April 1940 and that of the Institute of Philosophy in 1942. He supported philosophical anthropology and the sociological approach in the context of the new *Volksforschung* (*Volk research*). Hans Eibl’s extraordinary professorship was maintained even though he strongly urged that it be converted into a full professorship with the help of

<sup>11</sup> Regarding the Pedagogical Institute of the City of Vienna headed by Karl Bühler, and the linked research unit with Marie Jahoda and Paul Lazarsfeld see Benetka 1990.

Dean Viktor Christian. Due to his strong involvement in National Socialism, he was obliged to retire early after 1945.

At the beginning of the Second Republic, the philosophers active prior to 1938 who had not emigrated were recalled, and – after a delayed denazification which was terminated as of 1948 – the members of the institute active during the national socialist era were rehabilitated and put back into service. This is evident in the emerging careers of Erich Heintel and Friedrich Kainz. Between clerical restoration and a failed de-nazification, the way was paved for a conservative restoration of philosophy after 1945.

#### IV. PHILOSOPHY IN THE SECOND AUSTRIAN REPUBLIC BETWEEN PROVINCIALIZATION AND INTERNATIONALIZATION

Along the lines of the general political and cultural development at the beginning of the Second Republic, a characterization of the *supreme discipline* of philosophy along with psychology and pedagogy at the University of Vienna in the first decade after the Second World War can be described as a phenomenon of both continuity and disruption (Stadler 2004a). For a long time, the Faculty of Philosophy was an important organizational unit of the university in faculty dynamics and also had more than just subject-specific significance with the mandatory *Philosophicum* and the general standards in teacher training for secondary schools. (On doctoral studies and the *Philosophicum*: Meister 1958.) As programmatically signaled by the headline of a relevant journal, that's where *science and world view* (*Wissenschaft und Weltbild*) was taught and transmitted. With the *Wiener Zeitschrift für Philosophie, Psychologie und Pädagogik* (*Vienna Journal of Philosophy, Psychology and Pedagogy*), the *Fächerbündel* (individual combinations of courses taken from different subjects) which also denoted the corresponding teacher training for the subject at secondary schools, the so-called *Gymnasien* (*Philosophischer Einführungsunterricht; philosophical introduction lessons*) was featured in another periodical. The biographies of the most important editors of these two journals – Alois Dempf and Leo Gabriel at the one hand and Richard Meister and Hubert Rohrer on the other – allow us to reconstruct the development of these disciplines from the First to the Second Republic on the basis of various university appointments.

Here, we are confronted with considerable elite continuity, which is linked to the phenomenon of forced emigration and non-existent remigration in the context of half-hearted denazification followed by the Cold War period. A critical examination of the individual disciplines was carried out relatively late (Fischer et al. 1993) after a deepening had been provoked in connection with exile and emigration research (Stadler 2004b).

As far as the correlated break is concerned, relevant research has already brought substantial findings to light: In the context of *Vertriebene Vernunft* (*exiled*

*reason*), innovative movements such as the Vienna Circle or the school of Gestalt and cognitive psychology, which have an international recognition up to this day, were expelled and destroyed (Ash 1995; Ash et al. 1996; Benetka 1995; Stadler 1997/2001). An adequate description of philosophy, psychology and pedagogy at the University of Vienna during the period of “reconstruction” since the so-called *zero hour* (*Stunde Null*) is only possible against the backdrop of this dual history of science. The hardly practiced remigration is to be included in an overall assessment just as the related “second wave” of emigration of a younger generation of philosophers from Vienna as a result of the dominance of a clerical-conservative culture. (Pasteur et al. 2003–2004; *Österreich – Geistige Provinz?* 1965.)

What needs to be noted from a gender perspective is that – unlike exiled philosophy – the proportion of women in home-grown philosophical activity was virtually zero after 1938 and 1945. This is also related to the fact that the proportion of male and female philosophers of Jewish origin in philosophers forced to emigrate was relatively high prior to the ‘annexation’ (Frauen im Exil 2005; Stadler 1998; Korotin 1997; Ingrisch 2015).

In more recent studies, the social framework has been described as restoration under the sway of the founding myth (Hanisch 1994). To be sure, the university-wide context as well as the general situation of philosophy, pedagogy and psychology in Austria represents a specific general frame of reference (Preglau-Hämmerle 1986. 197 ff.; Gabriel et al. 1968; Haller 2004; Acham 2004, Vol. 6.1; Benedikt et al. 2005, Vol. 5).

Only the most important developments can be discussed here (Korotin 1993–1994; Leaman 1993–1994): Alois Dempf, who had published for a while even after his forced retirement, was able to resume his activities after the war in Vienna (Heiss 1993. 138 ff.). He was called to the University of Munich in the year 1948 but continued to work as a visiting professor in Vienna for several more years. Leo Gabriel who had already been active as a cultural official, teacher and instructor in adult education during Austro-fascism and had attained his *habilitation* in 1947, became Dempf’s successor to the chair, ensuring the continuity of political Catholicism at the university. With his holistic philosophy inspired by Othmar Spann – e.g., his *Führertum und Gefolgschaft* (1937) (leadership and followership) – as well as the all-embracing *integral logic*, he would also shape philosophy in Vienna for some decades to come. (As to the autobiographical description of Gabriel: Lotz-Rimbach 2004.) As mentioned earlier, Friedrich Kainz’s career continued to evolve steadily after 1945 until the crisis year of 1968 (Heiss 1993. 145 f.; Rathkolb et al. 2010). After 1945, Viktor Kraft was the only member of the Vienna Circle who managed to resume his teaching and research activities after having been dismissed by the Nazis. In 1945 the university library was reactivated and he retired as national librarian two years later. In 1947, when Kraft was 67, he was appointed associate professor and finally full professor for philosophy from 1950 to 1952 – for almost two years until his retirement.

During this time, Erich Heintel pursued his career. He attained his *habilitation* following the “annexation”, becoming a member of the NSDAP and “lecturer of the new system” (Dozent neuer Ordnung) for philosophy (metaphysics, epistemology, theory of value and ethics). Following an interruption for political reasons, he was able to lecture again in the 1949–1950 winter semester after having successfully applied for his *venia docendi* (authorization to teach) to be reissued. He was appointed associate professor in 1952 and full professor in 1960. With Gabriel and Heintel, both Christian existentialism and German idealism based on Protestant theology took root at the University of Vienna. Here, we have some sort of continuation of the polarization of the culture war during the inter-war period: From Gabriel’s and Heintel’s perspective, both Marxism and “positivism” were “labyrinths of philosophy”, which also reflected the view of the then minister of education Heinrich Drimmel (Knoll 1986: 278; Vienna University Archives: UAW, PhF, FSP, 16.10.48; Weiss 2009). The Vienna Circle, pure theory of law and psychoanalysis continued to be regarded as manifestations of a Jew-ridden liberalism and socialism (Topitsch 1967; König 2013; Nemeth 1993).

Ten years of “reconstruction” had resulted in a quantitative development of the classical philosophical teaching activities while continuation and stabilization were simultaneously observed in the conflicted area between “repressed humanism and delayed Enlightenment” (Benedikt et al. 2005, Vol. 5). The attempt to take up the scientific philosophy of the First Republic around Viktor Kraft who was reactivated at short notice remained episodic for a variety of reasons (Fischer et al. 2006). In the 1953–1954 academic years, he brought the young American philosopher Arthur Pap to Vienna as a visiting professor with the support of the Fulbright program. The latter was a pioneer in post-war analytic philosophy and had unsuccessfully attempted to pick up the earlier links of Viennese philosophy to what was the “golden age of Austrian philosophy” (Fischer 1995) on a global scale. For this purpose, he hired the highly talented Viennese philosopher Paul Feyerabend who assisted him in publishing his book *Analytische Philosophie. Kritische Übersicht über die neueste Entwicklung in den USA und England (Analytic Philosophy. A Critical Overview of the Most Recent Development in the U.S. and England)*, published by Viennese Springer Verlag in 1955 – “in memory of and for the revival of the Vienna Circle”. For Feyerabend, the Kraft Circle of the Austrian College 1949–1953 – which included a personal meeting with Ludwig Wittgenstein – and the working group at the Vienna Institute of Science and Art (Institut für Wissenschaft und Kunst) meant a break with the Austrian province and the beginning of his international career (Keupink–Shie 2006; Topitsch 1960; Feyerabend et al. 1966. 3; Stadler 2010).

This brief renewal attempt is typical for the decade of “reconstruction” – a situation Ernst Topitsch, another former member of the Institute of Philosophy, characterized as *Österreichs Philosophie – Zwischen totalitär und konservativ* (1967)

(*Austria's Philosophy – Between Totalitarian and Conservative*).<sup>12</sup> In this work, the author, an admirer of Heinrich Gomperz, criticized the ideological philosophy of the political Catholicism and the natural law variant of Christian philosophy in keeping with his book *Vom Ursprung und Ende der Metaphysik* (1958) (*On the Origin and the End of Metaphysics*). Troubled by the philosophy of Weltanschauung, Topitsch himself accepted a call to a chair in Heidelberg in the year 1962, before he went to the University of Graz in 1969, where he worked till the end of his life (2003).<sup>13</sup> Another member of the Kraft circle was Béla Juhos who – despite his international reputation – only got as far as becoming an external lecturer for theoretical philosophy with the title of an associate professor and represents another example of the marginalization of science-driven philosophy. In November 1965, the “Juhos case” triggered by Béla Juhos’ article “Gibt es in Österreich eine wissenschaftliche Philosophie?” (Is there a thing as a scientific philosophy in Austria, 1965) even led to a parliamentary question being directed to the then minister of education (Theodor Piffl-Percevic), which prompted former Austrian President Heinz Fischer to publish a piece of writing on the issue of “freedom of science in Austria”. In Vienna, Juhos remained a “thinker without any impact” even though he had made significant contributions to epistemology and the philosophy of science (Schleichert 1971).

Following Gottlob Frege, Bertrand Russell, Alfred North Whitehead and especially world-famous Kurt Gödel, the establishment of modern (symbolic) logic in Vienna – besides the traditional fields of philosophy, metaphysics, epistemology, ethics and logic, came late with a separate institute of logistics headed by Curt Christian. In the 1980s, a separate (meanwhile closed down) institute of philosophy of science and science studies was added (Stadler 2012). Against this backdrop, the “autochthonous provincialization” (Fleck 1996) appears to be a targeted strategy of immunization of the political and scientific elites. This is all the more the case given the fact that a promising younger generation of philosophers went abroad or left Vienna due to these structural deficiencies: Besides the already mentioned Feyerabend and Topitsch, this was, for instance, also true for Heinz von Foerster, Werner Leinfellner, Hubert Schleichert, Heinrich Kleiner – and not least for Austrian Wolfgang Stegmüller who was most influential in Germany (Stadler 2010). In Vienna, it is only since the 1970s that returned emigrant Kurt Rudolf Fischer, a fellow student of Feyerabend in Berkeley, contributed to a slow internationalization and pluralization process in his many years of working as a visiting and honorary professor at the Institute of

<sup>12</sup> E.g., there was an attractive list of candidates for an associate professor (1. Friedrich Waismann and Carl Friedrich von Weizsäcker, 2. Béla Juhos, 3. Erich Heintel), to which Heintel was appointed. Cf. Reiter 2011. 77–84.

<sup>13</sup> *Aufklärung und Kritik*, 2004.

Philosophy and, above all, thanks to his contacts with Anglo-American analytical philosophy (Diem-Wille et al. 2002; Stadler 2017).

Curiously, Leo Gabriel began his career in the Second Republic by taking over Schlick's former chair in the year 1951. "Integral logic and universalism" as "all-encompassing truth" was the motto now. The fact that this philosophical program did not remain just a personal opinion is demonstrated by the symptomatic historical influence in subsequent decades: at the 1968 XIV. *International Congress of Philosophy* in Vienna, Gabriel succeeded in having integral philosophy declared as a state philosophy, so to speak.

As a result, we have a continuity, which followed on political Catholicism and universalism of the corporative state with "integral logic" and at the same time prevented the remigration of philosophers who had been forced into exile. Add to this Erich Heintel's post-war career as an advocate of transcendental philosophy, neither the international remoteness of Viennese philosophy in the first decades after 1945 nor the continued exiling of philosophers are surprising. This bipolarity was still passed on students of the two mentioned full professors before the Institute of Philosophy began opening up and assumed a more pluralist orientation since the 1970s.

The development after the large 1968 International Congress of Philosophy in Vienna with the subsequent retirements of Gabriel (1972) and Heintel (1982) put an end to the dual dominance with two separate institutes of philosophy since the UOG 1975 (university act 1975). It is characterized by the establishment of the second generation of the two full professorships and additional appointments and calls, which can, first and foremost, be outlined by way of calls here. Karl Ulmer from Germany, who succeeded Kainz, was active as a hermeneutical philosopher focusing on immanent text interpretation and rational argumentation for about ten years from 1970. In 1982, he was followed by Hans Dieter Klein who had attained his *habilitation* at the institute and continued to nurture German idealism and transcendental philosophy towards systematic philosophy. Herta Nagl who developed the classical canon towards philosophy of history and feminist philosophy also came from this generation. Hans-Dieter Bahr was called from Germany to succeed Heintel who mainly advocated a postmodern philosophy of technology apart from the philosophical tradition. Norbert Leser was called to a newly created chair in social philosophy and hermeneutics. He dealt with the Catholic social teaching in the context of Austrian intellectual history besides his specialization in Austro-Marxism. By focusing on (applied) ethics, French existential philosophy and Austrian philosophy, Peter Kampits who came from Gabriel has changed his mentor.

From 1976 until his retirement, Michael Benedikt supported the Kantian tradition as a full professor and linked phenomenology to critical anthropology. He earned special merit from the publication of the voluminous six-volume book series *Verdrängter Humanismus – verzögerte Aufklärung* (*Repressed Humanism – De-*



layed *Enlightenment*) on philosophy in Austria from 1400 until the present day (Benedikt et.al. 2010).

Johann Mader worked as a professor from 1971 to 1996 in line with German transcendental philosophy and classical history of philosophy. During this period, Günther Pöltner and Helmuth Vetter, in particular, also developed and established phenomenology, which is still strongly represented today. Here, attention should be paid not least to Franz Martin Wimmer who was able to develop the focus on intercultural philosophy as it exists today in a sustainable manner. At the same time, mention should be made of the many representatives of the *Mittelbau* (academic teaching and research staff who are assistant or associate with *habilitation*).<sup>14</sup>

In the field of philosophy of science and analytical philosophy, a reconnection to and further development of the great tradition of the interwar period can be linked with Wittgenstein and Vienna Circle research, which had started already before at the remaining Austrian universities (Stadler 2012). In 1972, Erhard Oeser who had come from Heintel took over the newly created chair in philosophy and philosophy of science. As part of the large 1968 International Congress, philosophy of science was still represented marginally, this is confirmed by the analysis of the situation of *Philosophie in Österreich (Philosophy in Austria)* in an international comparison carried out at that time (Gabriel et al. 1968; Fischer et al. 1993; Stadler 2005; Generally on philosophy in Austria since the Monarchy: Benedikt et al. 2005, 2010; Acham 2004, 2006). Thus, it was no coincidence that the modern analytical direction has only manifested itself as part of an informal working group for linguistic analytical philosophy since 1983 at the Vienna institute, which was mainly enriched by visiting and honorary professor Kurt Rudolf Fischer (Diem-Wille et al. 2002). The institute was extended through the foundation of the no longer existing institute of philosophy of science and science studies in the year 1986.

Since the beginning of the 21<sup>st</sup> century, the Institute of Philosophy has developed into one of the largest ones in the German-speaking area as a result of several calls mainly from abroad.<sup>15</sup> In the year 2011, the Institute Vienna Circle founded as an association in 1991 was established within the organizational unit of the Faculty of Philosophy and Education – as a kind of belated token of restitution and recognition of the Viennese heritage.<sup>16</sup>

The past and future calls open up specializations and pluralization between “continental” and “analytical”, practical and theoretical philosophy as well as an interdisciplinary and/or transnational networking with an increasing presence of

<sup>14</sup> Already in 2013 ca. 50 members of the department incl. project researchers (third party financed investigators) were listed in the website.

<sup>15</sup> See: < <https://philosophie.univie.ac.at> > Last access 30-11-2018.

<sup>16</sup> See < <https://wienerkreis.univie.ac.at/das-institut/> > and < <https://univie.ac.at/ivc> > Last access 30-11-2018.

female philosophers. Given the institute's development from the First to the Second Republic, this has ushered in a new phase with growing international recognition. But that is already a different story.

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# THE INFLUENCE OF AUSTRIAN PHILOSOPHY IN HUNGARY



## The Impact of Karl Bühler on Hungarian Psychology and Linguistics\*

Due to his extremely varied and rich professional profile, it is very hard to classify the work of Karl Bühler (1879–1963). He was a pioneer of experimental psychology investigating thought processes, an early synthesizer of child psychology, and a theoretician, who tried to renew the psychology of language, and place the renewal of psychology into a complex vision of language. Further, with his analysis of the regulatory aspects of animal behavior and the role of selection in evolution he has become a mentor and first proponent of multilevel theories of selection in cognition. Thus, in a way, Bühler was also a mentor of the later evolutionary epistemology and evolutionary psychology (Pléh 2014). (See about his life the volumes edited by Eschbach 1984, 1988, and Musolff 1997.)

This paper is partly conceptual, partly historical/philological. My aim is to show how the different aspects of the rich oeuvre of Karl Bühler have become part of Hungarian linguistics, psychology, and philosophy in mid-20<sup>th</sup> century. That is the conceptual part. In some cases (that is going to be the historical aspects) I shall try to show the underlying factual aspect of the intellectual connections. I shall not try to give a thorough analysis of the work of Bühler, only relate to the issues of his work that have become relevant in the Hungarian context.

As Bolgar emphasized in Bühler's necrology, Bühler was a man with much varied interests, who always concentrated on the issues of *how*.

A catalogue of his concerns would include the psychologies of thinking, perception, language, and child development, as well as theories and systems. He did not look for a single operating principle, but in all his work he asked the question *how* [...] How does man think? How does he perceive? How does he communicate? [...] Rarely did he ask the question *what*. (Bolgar 1964. 677.)

\* Much of this paper is based on a larger manuscript from a time I was working at Collegium de Lyon. The fruitful discussions with Elisabetta Basso on the philosophy of psychology helped to shape my vision.

## I. THE IMPACT OF THE EARLY DENKPSYCHOLOGIE OF KARL BÜHLER IN HUNGARY

Bühler had a medical education as well as a philosophy degree, but he was drowning to psychology early on. As one of the leading researchers of the Würzburg school of thought processes at the turn of century, working there and later in Bonn and Munich as well with Külpe, Bühler had become a proponent for the psychological reality of abstract thoughts (Bühler 1907, Bühler 1908). The three basic features of the Würzburg research attitude were:

- Mental activities are guided by various *non-image-like (unanschauliches)* factors, such as attitudes.
- There are characteristic *rules of individual cognition* (thus logics is given a psychological interpretation).
- All these factors should be interpreted by implying that mental activity is always directed outwards, it is characterized by intentionality.

The attitude of the school is well characterized by Ogden (1951), by Humphrey (1951), and by the readers of Rapaport (1951), and Mandler and Mandler (1964), and Nyíri (1974) showed how it might be related to the general anti-psychologism born at the end of 19<sup>th</sup>-century philosophy. Regarding the substantial message, this school has challenged the elementaristic and sensualistic metatheory of mental life. Solving of problems is goal oriented and structurally organized, and (some) of thought content is structured, not merely an associative chain. There is a consciousness of rules, relations, and intentions (Bühler 1907, Rapaport 1951, Mandler and Mandler 1964, Mandler 2007). Another consequence of this attitude was questioning of the sensualistic bias of most empiricist philosophical tradition and pointing towards a more propositional organization of human thought processes. A modern version of this attitude is shown by Fodor (1996). All of these pointed towards a more systematic vision of thought and language processes, with a concentration on the notions of fields and tasks (Mandler and Mandler 1964, Pléh 1984) and towards a more objectivistic, supra individualistic interpretation of thought and meaning following on the steps of the anti-psychologist semantics of Husserl (1900), as interpreted by his mentor Oswald Külpe (1912) (see about these influences Krug 1929, Münch 1997, Kusch 1999).

This seemingly rather abstract endeavor had many challenging aspects and provocative consequences for modern psychology. One was methodical, that concerned the extended use of introspection and detailed report of the inner workings while subjects were interpreting for example the meaning of maxims or proverbs like *Not all that shines is gold*. This aspect created many controversies, the founding father of German experimental psychology Wundt (1907) questioning the entire method and classifying the studies as pseudoexperiments.



## II. THE IMPACT OF THE DENKPSYCHOLOGIE OF BÜHLER IN EARLY HUNGARIAN THEORETICAL MONOGRAPHS

The early works of Bühler concentrating on the psychology of thought processes basically had two impacts in Hungary. First, they have become part of the intellectual discussion of the *organization of thought*, and the relations of logical and psychological models. Early on Valéria Dienes (1879–1978) a young follower of Bergson, the first woman to obtain a PhD in Hungary, and a critical analyzer of all of the modern psychology published a short synthesis where she analyzed the importance of the Würzburg tradition. This was an original synthesis that presented both Ivan Pavlov, Vladimir Bekhterev and the Würzburg school of the psychology of thought processes as the reformers of modern psychology. For Dienes, the key feature was the emphasis on *hidden factors* and functions. By hidden factors she meant that our mental life shows a number of organizational aspects that are not directly apparent, they are not transparent to the self-studying conscious mind. Thinking is governed by hidden rules – as claimed by Bühler (1908) and the Würzburg School – that we cannot get to know directly, only through their products, their mental outcomes. But the real winner for Dienes was Bergson who transformed the issue of introspection into the issue of intuition (Pléh 2005).

Hildebrand-Dezső Várkonyi (1920) a young Benedictine psychologist, later a leader of the new psychology movement at the university of Szeged (see about his life and impact Szokolszky 2016) has written a relatively detailed review of the debates around non sensory thought. His conclusion, on the basis of a contextual analysis of the Würzburg studies and the studies of Binet on his own daughters was that while there is a phenomenological non-sensory thought, in its origin and context, thinking always has a sensory backing. “there always is an ideational background to thought: images follow thought as a shadow. Imageless, ‘pure’ thought we cannot recognize in ourselves” (Várkonyi 1920. 79).

A generation later, Ferenc Lehner (Lénárd) (1911–1988) in the same leading philosophy journal in Hungary at the time, analyzed in detail the *Denkpsychologie* work of Karl Bühler, Otto Selz and others. Lehner (1939) has mainly summarized the debates about the validity of the Würzburg findings. Lénárd has preserved this heritage of Karl Bühler in his later professional life, as well. He has referred to the Denkpsychologie of Bühler both in his short history of psychology (Lénárd 1946, Lénárd 1989) and in his monography on problem solving published five times (Lénárd 1984). In his history book, he in fact presented the Würzburg tradition as the first new psychology of the modern times. He detailed the methods and the basic non sensory content oriented research of Bühler in great detail (Lénárd 1946).

The presence of the thought psychology of Bühler, and its later combination with newer approaches of Gestalt psychology, and psychoanalysis, especially re-

garding the task aspects of thought were further exemplified by the exhaustive reader compiled in America by David (Dezsó) Rapaport (1911–1960), a graduate both of Hungarian psychoanalysis and of the psychology seminar at Budapest University. The book brings back richly annotated translations of Bühler, Ach and other Würzburg people along with Lewin, the psychoanalysts, as well as Claparède, and Piaget. This classic edited book shows as the editor himself acknowledged, his Hungarian university education.

I had hoped that this volume would be published simultaneously with that of my teacher, Paul von Schiller, which was to rescue from oblivion some little-known European studies in the instinctual behavior of animals. But Paul von Schiller is dead, and I can only acknowledge again my indebtedness to him. (Rapaport 1951. ix.)

In his own theory of thinking proposed as the concluding chapter of his reader, Rapaport in his psychoanalytic attempts to find ways for adaptive thought processes combined with drive forces, relies in two notions coming from Bühler. The first is the differentiation between reproductive and productive thought, and the second is the central importance of the task consciousness as an organizing factor in higher level thought processes.

The other influences of the early Würzburg work of Bühler were mainly indirect, but resulted in published German works.<sup>1</sup> *Two PhD dissertations were defended in Würzburg by Hungarian students after Bühler has left*, but along the lines set up by him, both directed by Karl Marbe. Though, as he recalled in his autobiography, Marbe (1930) had many controversies with his mentor, Külpe, he still followed the line of using introspection to study thought processes, and to reveal the contentful flow of thought. His first Hungarian PhD student was Antal (Anton) Schütz (1880–1953), a Piarist priest, who obtained a doctoral degree in psychology in 1916 in Würzburg, with a research that followed the attitude of contemporary cognitive experimental psychology, that of the Würzburg School (see Pléh 2005). His dissertation was entitled *Zur Psychologie der bevorzugten Assoziation und des Denkens* (Schütz 1916a, 1916b, 1916c) (see about it in his autobiography as well, Schütz 1942). He was investigating the hidden tendencies determining associative recall. In his actual studies he was using mass verbal associations first done in Hungarian over a large number of subjects and stimuli. In the book version he analyzed the possible personal determinants of association, such as age, emotional status and psychopathology of the experimental subjects. What makes his studies related to the Würzburg School and specifically even Bühler that in his Hungarian survey paper he emphasized that among

<sup>1</sup> I would like to thank the help of Prof. Dr. Armin Stock University of Würzburg Adolf-Würth-Center for the History of Psychology. He helped me with references for the early works of Antal Schütz and Imre Molnár.

the different determinants of association a most important one is the task. The anti-mechanistic Würzburg scholars had shown to him that besides strictly associative factors thought related, emotional and volitional factors also play a role in verbal associations (Schütz 1916c).

Schütz has later found his place at the University of Budapest not as a psychologist, but as a professor of Catholic dogmatics, in line with his first degree. He has tried later on to forge a unique alliance between Catholic dogma and a critical appraisal of contemporary psychology (Schütz 1944). In his first academic inauguration talk about the relevance of the psychology of Aristotle today, he pointed out the importance of the objectivistic trend represented by Bühler to support the idea that “in our mental life there are atemporal elements beside the temporal ones, as emphasized clearly by Bühler and his school” (Schütz 1927. 63).

Schütz has gone beyond merely criticizing experimental psychology for its simplemindedness. The main point of the psychological ideas of Schütz was that scientific psychology has to be treated with great criticism (Schütz 1941, Schütz 1944). This point of view has some messages for professional psychology as well. The main idea of Schütz was that the processes of thought – in accordance with the theory of the Würzburg School – cannot be regarded as mere sensory accumulation processes: the essential moment of thought comes from the subjects’ particular computations or acts. This dynamics of acts was the key for him to avoid reductionism, to avoid reducing the mind to its elementary processes. Schütz (1944) considered positivism and evolutionary theory as barren and factually untenable ideas.

At the same time, he feels a curious attraction towards contemporary characterological movements. In one of his works, in his [second] academic inauguration talk (Schütz 1941), he tried to elaborate connections between schools or streams in logics and personality types of the representative researchers. In the same way as one can distinguish different types of thinking in people, one can distinguish different types of thought among scientific trends as well. Logical atomism, for example, is connected to a typical analytic personality, while holism in logics is similar to an integrative or unit-forming personality. In fact, it is a personal world view that appears in the disguise of logical schools, through the filtering effects of personality. [...] For Schütz, the[se ideas] supported his campaign against reductionistic psychology. In his view, only these synthetic ideas based on the integrity of personality will be able to create harmony between mind-guided Catholic ideas and modern psychology. (Pléh 2008. 175.)

The other student who obtained a PhD in Würzburg under Marbe was Imre Molnár (1909–1996) who has later on become the director of the Child Psychology Institute in Budapest between 1948 and 1962, later becoming the research institute of psychology of the Hungarian Academy of Sciences. As he recalled in

his sometimes not too factual autobiography, Molnár (1990), a Hungarian Jewish youngster from Nagyvárad (Grosswardein) in the Partium of Hungary then becoming part of Romania first wanted to become an actor or a movie director in Berlin, and enlisted to the university under fatherly request. He fell in love with psychology under the impact of Köhler, Lewin and Spranger. Moving to Würzburg, he obtained his PhD with Marbe studying the relationship between set phenomena and the issue of esthetic value. Marbe was earlier involved in the psychology of art as well, thus the interest of the disillusioned would be actor and the mentor has probably ell meet. The mostly conceptual-theoretical dissertation was related to the Bühler heritage in one regard. It revolved around the issue of the objective value in esthetics, with a conclusion that one cannot abstract from the person regarding the value of artwork. His German pen name was Emerich Molnár, and he published his dissertation in a high profile journal of the time, and even as a separate monograph (Molnár 1933a, Molnár 1933b). Marbe, who was by that time also an acknowledged industrial and marketing psychologist, certainly had an influence on the later career of Molnár who has become a leading figure in the stabilization of Hungarian industrial psychology with his textbooks and with his detailed studies of the psychophysiological stress reactions of weavers (Molnár 1982, Molnár and Stadler 1966).

The impact of the early work of Bühler also showed up in the school curriculum and in everyday talk about the mind, where textbooks like the one for high schools by Lénárd (1960) on may editions were crucial. His entire outlook followed the later Bühler. Youngsters were introduced to psychology as the study of (internal) experience, (external) behavior, and (World III) work (of art). But he also introduced early Bühler buzz words such as the concept of *Aha experience* (*Aha Erlebnis*), the sudden recognition of new insights and connections between ideas. Similarly, as one of his other 'brand words', in fact criticizing Sigmund Freud's supposed wish fulfilment image of man, another concept proposed by Bühler was also a shining start of Hungarian educational psychology. The notion of functional pleasure (*Funktionslust*): the recognition that functions are practiced because their practice itself is a source of pleasure. Bühler (1921, 1922, 1927) described it to be very crucial in child development but also in several aspects of human culture. "In humans the functional pleasure has become a central factor of development" (Bühler 1921. 150). In his elaborate system,

Bühler proposes a triad of fundamental 'drives' or motivation systems, stemming from three variations of the experience of pleasure: (a) pleasure coming from the satisfaction of need; (b) pleasure coming from activity, from functioning; and (c) pleasure coming from creative work (Bugental et al. 1966. 198).

These two expressions, *aha experience* and *functional pleasure* have become popular in Hungarian psychological terminology, without too much awareness as to their origins.

### III. THE HUNGARIAN IMPACT OF THE VIENNA SCHOOL OF BÜHLER

After serving in the war as a medical doctor, and following Külpe to Bonn and Munnich, Bühler had become a professor at the Dresden Technical University, and then from 1922 to 1938 at the Institute of Psychology at Vienna University. Working together with his wife Charlotte Bühler, he turned this institute into one of the main centers of psychology in the German speaking world (Ash 1987, 1988). Bühler and his wife were sort of leaders of the Austrian pedagogy movement. Karl Bühler fulfilled two functions, one as a university professor and another as an adjunct leader at the Pedagogy Institute of the City of Vienna. The university life was the scene of the more theoretical and experimental works, together with people like Egon Brunswik (1934) and Lajos Kardos (1934), while the Pedagogy Institute was responsible for fostering a socialist inspired educational reform, both in teaching and in test development. Nyíri (1986, 1992) the Hungarian historian of philosophy provided a good survey of the Vienna intellectual scene to which the work of Bühler was integrated, and Bartley (2004) the historian of Wittgenstein and Popper, showed in particular the historical and social setting of these educational reforms, and that they mainly represented a move towards a less authority-based and more child oriented education.

Karl Bühler (1922) himself had a crucial role in working out the theoretical framework for child development studies in Vienna, with 5 German and 3 English editions of his developmental psychology textbook. His book, besides its general Darwinian outlook, was a basic textbook mainly about the preschool years. Compared to similar textbooks it had a number of interesting peculiarities: the constant use of comparative psychology examples and analogies in interpreting the instinct, habit, and intellect triad of children, the important role attributed to language and drawing, and an excellent portrayal of infant social behavior. The German edition of this book together with the test work of his wife, Charlotte Bühler (Bühler and Hetzer 1932) and her diary studies of youth have become standard references in Hungarian educational psychology for decades.

The institute lead by Karl Bühler had an excellent collection of students and assistants, and made contacts with many circles outside psychology as well, including the Vienna Circle of philosophers. The atmosphere of the institute is well described by the modern decision theorist, Gigerenzer:

The sparkling intellectual atmosphere of early twentieth-century Vienna produced Wittgenstein, Popper, Neurath, and Gödel – in addition to a string of other great thinkers. Among them was Karl Bühler, who, when he founded the Vienna Psychological Institute in 1922, was one of the foremost psychologists in the world. Egon Brunswik began to study psychology in Vienna in 1923 and soon became an active participant in Bühler's famous Wednesday evening discussion group; on Thursdays he went to Moritz Schlick's Thursday evening discussion group [...] In 1927, Brunswik submitted his doctoral thesis to Bühler and Schlick, the same two advisors to whom Karl Popper submitted his thesis a year later. (Gigerenzer 2000. 45.)

#### IV. THE BASIC THEORETICAL COMMITMENTS OF KARL BÜHLER IN VIENNA

As a professor in Dresden, and later for almost two decades in Vienna, Karl Bühler (1927) elaborated a sign-based theory of mental organization, a communication-based, semiotic theory of the mind. The features of his rich oeuvre can be summarized as a series of foundational theses, all colored with a strong evolutionary commitment. The evolutionary aspects are highlighted with italic letter type.

- (1) All behavior is regulated by signs. *There is no meaningless behavior.*
- (2) Human behavior is oriented to supraindividual meanings. *All human behavior has three aspects: experience, behavior, and reference.*
- (3) All behavior is characterized by holistic organization aimed at species-specific signals. *Structure, meaning, and goals characterize all behaviors.*

On the one hand, Bühler was in the uneasy position of defending the reality of abstractions in directing human life, and, on the other hand, he was at the same time defending naturalism with a strong Darwinian flavor. As part of incorporating selectionist explanations to different domains, Bühler (1922) also extended Mach's (1905) idea of seeing hypotheses, and trial and error everywhere. He proposed continuity between instinct, trial and error learning, and intellect, and the domain of selection is, respectively, the organism, behavior, and ideas. Already Thorndike (1896) interpreted trial and error learning using a selectionist terminology. The third level also appears in the famous experiments of Köhler (1921, 1925) on chimpanzees, where insight comes as a selection of 'ideas', as an entirely internal process, with no visible solution attempts.

The three systems of instinct, habit and intelligence always strive to construct a model of their environment. In this modeling activity, the role of Darwinian selection and its broader interpretation are pivotal for Bühler (1936a). He was the first to formulate two principles that dominate today's philosophy of mind

(Dennett 1994) and philosophy of neural processes (Changeux 1983; Edelman 1987): all behavioral organization is characterized by an early stage where a rich and redundant inventory of behavior is formed, with an excessive number of elements and associations, and a later, selective stage, where certain patterns are chosen on the basis of environmental feedback.

The main point about the relationship between the three levels – as expressed rather definitively by Karl Popper (1972), a disciple of Bühler – that instead of risking survival as in Darwinian evolution, we are only risking our ideas in intellectual selection. The three levels also differ in their flexibility, but the organizing principle is the same for all of them: initially, there is an attempt to develop a variety of responses to an environmental challenge, which is later reduced based on the feedback from the environment.

There is no demarcation line between human mentality and animal mental life. Intention-based, teleological, and holistic organization is true of all behaviors, and it creates unity between the work of biology and that of the psychology. Table 1 shows how Bühler distinguished between the different levels of behavioral selection.

*Table 1. Three levels and pools of selection according to Bühler (after Pléh 2014).*

| <i>Features</i>       | <i>Instinct</i>           | <i>Habit</i>                   | <i>Intellect</i> |
|-----------------------|---------------------------|--------------------------------|------------------|
| Pool of selection     | Individuals               | Behaviors                      | Thoughts         |
| Roads to selection    | Darwinian selection       | Reinforcement                  | Insight          |
| Proofs                | Species-specific behavior | Associations, new combinations | Detour           |
| Representative author | Volkelt, Driesch          | Thorndike                      | Köhler           |
| Organization          | “Naturplan”               | Associative net                | Mental order     |

Karl Bühler made these principles and levels central to his idea about child development as well. The three levels appear in children in a gradual manner. As his interpreters underlined it:

The process of humanization is structured into three ‘stages’ (a) that of the dominance of instinct during the first weeks after birth; (b) that of ‘training’ [*Dressur*]; and (c) that of the beginning of an intellectual life, which is distinguished by the use of tools. It is hypothesized by Bühler that these different stages are determined by the maturation of different brain areas, especially that of the brain stem and the cortex with its various functional units. (Bugental et al. 1966. 197.)

On the technical side, however Bühler had doubts about the ‘intellectual’ nature of the chimpanzee achievements shown by Köhler, since he believed that real intellect needs reorganization, perspectives, and doubts. Most interestingly, he connected his theory of motivation as well to the three proposed levels. Trial



and error is made possible by functional pleasure, and human intelligence is made possible by creative drives.

These main visions appeared already in his work on the interpretation of perception (1922b), which is in a way a continuation of the communication based theory of perception proposed by Helmholtz a generation earlier (Pléh 2008). In his view, all perceptual processes should be interpreted from the point of signing. Basically he claimed that the stimuli have a subjective appearance, and an object reference must be 'computed' by the mind based on it. The stimulus is always ambiguous regarding its referent object. For the computation of the real world, the entire signal context should be considered. This attitude allowed for the experimental and mathematical verification by his students, as shown by the constancy experiments of Brunswik (1934) for size, and Kardos (1934) for lightness.

Perceptions furthermore always have a signal function that goes beyond the mere stimulus. As signals, they should be related both to the objects evoking the percepts, i.e., the causal agents responsible for them, and to the evoked behavior of others. Linguistic signs are special since they also relate to the supraindividual rule-systems that are responsible for them. The sign based (semiotic) interpretation of perception is crucial for Bühler to show that all kinds of behaviors (including on lower levels) necessarily have different aspects, not unlike the linguistic signs he studied later in detail.

The issue of the proper place of Gestalt principles was central for Bühler's interpretation of perception. Bühler was among the first systematic proponents of a Gestalt-based organization in perception and mental life (Bühler 1913), although at that time he was mainly interested in Gestalt organization in artistic forms. Interestingly enough, the book of Bühler was reviewed early on in Hungary by Gyula Kornis (1915), his later professional friend. Kornis emphasized that Bühler saw the independence of Gestalt organization, but at the same time a unity of analytic and synthetic processes in it.

The Berlin *Gestalt* School did not acknowledge Bühler sufficiently, because the experimental attitude of Bühler has been too analytic for them. Bühler followed classical psychophysical methodology, when trying to reveal *Gestalt* organizing principles in visual displays. According to the Berlin school, Bühler did not recognize the primacy of Gestalt organizing principles, such as pregnancy. While Bühler and his students (Brunswik 1934 and Kardos 1934, 1935) studied Gestalt organization and particularly constancy phenomena, they did not follow the Berlin school in all regards. They did not believe in 'direct perception', and they allowed for much more computations, based on the stimulus array, and a comparison of different fields or domains to arrive at an object representation.

Bühler returned to the *Gestalt* issue at the end of his life (Bühler 1960, 1961). For Bühler, even when he returned to the issue at the end of his life, Gestalt or-



ganization was a biological function. As Cattaruzza (2015) analyzed his approach in detail, Bühler compared and treated psychological and biological functions together. In his examples of animal behavioral coordination, he always interpreted animal behavioral regulations as complex, *Gestalt*-based sign functions. As a matter of fact, much later he even tried to relate his *Gestalt* convictions to the new cybernetic organizing principles (see about this Garvin 1966).

## V. THE HUNGARIAN IMPACT OF BÜHLER IN VIENNA

Table 2 summarizes the impact of Bühler on a next generation, setting the Hungarians with bold.

*Table .2 Students and followers of the Vienna Karl Bühler and some of their ideas (after Pléh 2008)*

| <i>Topic</i>                               | <i>Student, follower</i>                                       | <i>Continued topic</i>                                   |
|--|--|--|
| Sign functions in perception and Gestalten | Ludwig Kardos, Egon Brunswik                                   | Constancies, sign theory of perception                   |
| Teleology of animal behavior               | Konrad Lorenz, Paul (Harkai) Schiller, Kardos                  | Releasers, behavioral evolution                          |
| Language functions                         | Popper, Lorenz, Jakobson, Gyula Laziczius, Iván Fónagy, Kardos | Anthropogenesis, language functions, culture, World III. |
| Selection in development                   | Lorenz, Karl Popper, F. Hayek, Harkai                          | Selectionist theory of knowledge, competition of ideas   |

During the Vienna years, Bühler had different types of contacts with Hungarian science.

- formal, official contacts
- mentoring Hungarian students and “postdocs”
- influencing the intellectual outlook of Hungarian psychology
- the impact of Bühler’s theory of language on Hungarian linguistics

As for the official contact between and Hungarian intellectuals, he certainly had some contacts with influential figures of the time. The society section of the Hungarian Philosophical Association published in their journal, *Athenaeum* in 1937 we learn that Bühler had a talk in 1936 in Budapest, on *The Future of Psychology*, that he was probably talking about his multilevel theory of behavior published in German the same year (Bühler 1936a).

In 1937 he was elected to be an external member of the Hungarian Academy of Sciences, on the promotion of Gyula Kornis, a most influential conservative philosopher, Anton Schütz, and Gyula Moór, a Neokantian legal philosopher.

The promotion emphasized his role in developing the psychology of higher mental processes, in relation to language and mental development. Specifically, as a follower of Külpe he proposed a new vision of non-sensory mental acts, relying on systematic introspection of the subjects. The promotion also emphasized his stage theory of mental development (instinct, habit, intellect). They also referred to his Gestalt studies and theory of color vision (Magyar Tudományos Akadémia, 1937).

As the archival material of Gyula Kornis kept at the Hungarian Academy Manuscript collection shows, they were in friendly, though not intimate terms, visiting each other's seminars in the 1930s. Kornis has referred to Bühler in many of his writings, mainly to Bühler's studies of thought processes. Bühler remained in close contact with his Hungarian psychologist colleagues later on as well. After he was imprisoned by the new authorities following the Anschluss of Austria, and he had to leave Vienna, he arranged for his reprint collection to be taken over to Budapest. As Kardos, his doctoral student recalled to me, as a Jew Kardos did not dare to go back to the Vienna of the III<sup>rd</sup> Reich, thus Ferenc Lénárd, who was an *Ungardeutsch* safe to travel was actually responsible for the transfer (Kardos 1984, Pléh 1985b). The collection, organized in the early 1950s by Ilona Barkóczi and Zsolt Tánzos is still in the possession of Loránd Eötvös University Institute of Psychology that has taken over the old library of the Psychology Seminar of Pázmány Péter University in the 1950s. Incidentally, the exile library of the Bühler couple – that has a few entries from the Vienna years – was repatriated to Austria, and is being professionally catalogued (Felsner et al. 2016). The same still awaits for the reprint collection, referred to in Budapest as the *Bühler separatum* collection.

## VI. PERCEPTION RESEARCH OF LAJOS (LUDWIG) KARDOS

The most direct influence of Karl Bühler in Vienna towards Hungarian psychology came from his perception research. Although Brunswik the other Vienna perception pupil of Bühler would also be considered to be Hungarian, he was in fact coming from the upper Hungarian (today Slovakia) branch of the Brunswik family famous in Hungarian intellectual history, and was raised in Vienna as son of a government employee, and was never part of Hungarian psychological and intellectual life. The real Hungarian influence was on Lajos (Ludwig) Kardos. Kardos (1899–1985) has been both the mentor and the savior of Hungarian experimental psychology in the 1950s–1960s acting as a chair of psychology at Budapest Eötvös University between 1947 and 1972, at a time when psychology was less then welcome as a discipline (Pléh 2008, 2013), and there he developed a locomotion based theory of animal memory. However, in his early years in Vienna, he has become a leading follower of Karl Bühler in perception research. Kardos was a liberal

left wing youngster, who has become part of the Jewish exodus in the 1920s due to *numerus clausus*, and he started and finished his university studies in Vienna. *Numerus clausus* was the first practically anti-Semitic law in Hungary that was trying to limit the proportion of Jewish students to 6%, which was the proportion of Jews in the general population of Hungary at that time, while the actual rate of Jewish students was 25–40% in the 1910s in different faculties (Kovács 1994). Kardos studied both medicine and mathematics at the University of Vienna, obtaining his medical degree in 1925. But the real turning event of his life was that in the 1920s he became a student of Karl Bühler (Kardos 1984a, Pléh 1985b, Murányi 1985). After defending his thesis he published it in Nazi Germany (Kardos 1934), with some benevolent lying from Bühler as Kardos mentioned in an interview (Pléh 1985). As Dejan Todorović pointed out to me,

The book was dedicated to Karl Bühler, and in the preface Kardos extends thanks to various people including Bühler, Spearman, Woodworth, Brunswik, Heider, MacLeod, and especially Koffka, for “long and deep discussions”. This shows that he was in communication with leading researchers in the field at that time. (Todorović 2010.)

Starting as a student of Karl Bühler Kardos worked on constancy phenomena (Brunswik and Kardos 1929, Kardos 1930), and he became well known through his monograph on the role of shadows and lightness constancy in object perception (Kardos 1934). He was among the first perceptual psychologists to combine the attitudes of careful experimentation with courageous mathematical modeling, basically claiming that constancy can be rendered with a mathematical model comparing the light input from a surface with that coming from the neighborhood (Kardos 1934, 1935).

According to Kardos, color and lightness constancy phenomena are a key to object perception. He has taken over his interest towards color and lightness constancy as well as the theoretical attitude of treating perception as a signal issue from Bühler (1922b). Constancy itself can be rendered with a mathematical model comparing the light input from a surface with the average light coming from the neighborhood. As Alan Gilchrist (2010) pointed out to me in personal correspondence

The idea that lightness depends on a comparison of target and surrounding luminance was, I think, widely accepted among at least Gelb, Koffka, and others. The central idea of Kardos was that “neighborhood” was much more concrete. It was not a matter of distance from a target surface, but rather a frame of reference. He used the terms relevant and foreign “field” as in field of illumination. Furthermore it was not the kind of vague idea others had. He defined how a field is segregated within a complex image – two factors: penumbra, and depth boundaries (corners and occlusion boundaries). (Gilchrist 2010.)

The treatment of constancies by Kardos is a rather striking combination of phenomenological analysis, careful experimentation about contextual effects, and an innovative application of higher mathematics. In his phenomenological analysis there is careful consideration of notions like object, field, sign, and the like. Phenomenology for Kardos was by far not a license for loose talk. Rather, it was rather a combination of conceptual analysis and presentation of primary experiences. “In the natural, lay attitude directed towards ‘object properties’ vision provides a phenomenal field in which there is no real articulation between shadows and parts without a shadow similar to a figure–ground organization” (Kardos 1934. 23).

This attitude is the reason his treatment continues to be central even in contemporary theories of lightness constancy. The modern synthesis of Alan Gilchrist (2006) about black and white perception is basically centered on the interpretation of the Kardos experiment and his models.

Kardos proposed that the lightness of a surface is co-determined by both its relevant field of illumination and the foreign field of illumination, although the main influence is that of the relevant field. The relevant field is the field to which a target surface belongs; the foreign field is the adjacent field of illumination. Perhaps his most important insight was that failures of constancy are the expression of the influence of the foreign field. He studied the competing influences of these fields where they are most equal in strength: in perceptually segmented but weak frameworks. (Gilchrist 2006. 65.)

In a way, Kardos belongs to the rare type of historical heritage who’s work (at least his work on perception) is not merely of historical interest today, but forms part of contemporary mainstream perceptual psychology.

In a peculiar manner, Kardos has later on fell for information theory, and was among the first psychologists in Hungary to use the cybernetic idiom to characterize the mind (Kardos 1964), and elaborated a complex neuro-cybernetic proposal for the origin of mental life (Kardos 1980). Kardos has felt the continuity of the signaling theories of his mentor in his own perceptual research. In the 1984 Hungarian translation of his 1934 monograph, he felt pity for his missing of cybernetic notions that would turn the phenomenological language into a more mathematically neutral idiom.

How much easier would have been my task (in 1934) had I available the conceptual apparatus of present day information theory and cybernetics! [...] How easier would it have been to state that our color experiences are informations about some optical aspects of objects, and to state that stimuli work as information channels characterized by noise. (Kardos 1984. 13.)

## VII. BEHAVIORAL TELEOLOGY NOTIONS OF BÜHLER IN HUNGARIAN COMPARATIVE PSYCHOLOGY

Bühler outlined his mature theoretical position in an influential book on the supposed crisis of psychology, which was published in 1927. Bühler proposed a vision of mental life that was modeled on language. He considered three entities, out of which two were agents. The Subject, with his first person experience, the Partner, with his second person experience, whose behavior the Subject tries to modify, and the Object of the situation, which the behavior reacts to and which the behavior is coordinated with. From the point of view of the history of ideas, in the 1920s and 1930s Bühler tried to overcome in a sometimes eclectic, but certainly in a liberal way the controversies among the internalist, the behavioral, and the culturalist approaches to the human mind, and the task of psychology (Bühler 1922a, 1927, 1934, 1936a, 1990). He belonged to the class of those Central European scholars who were looking for a meaningful unity in their science, while being aware of the divisive naturalistic and spiritualistic trends. The much cited quote below shows how relevant his attitude is even for contemporary debates of the study of the human mind.

When someone raises a new topic, why does he have to look down scientifically on his neighbor? In the large house of psychology there is room for *everyone*; one could direct his spectacles on the skyline of values from the attic, others could at least claim for themselves the basement of psychophysics, while the walls are intended to out the entire enterprise into the causal chain of events.” (Bühler 1927. 142.)

Bühler started from the idea that the foundations of traditional psychology had been challenged due to the severe criticism of associations. The structured principle of the Gestaltists, the search for an underlying, non-conscious order by the psychoanalysts and the Würzburg *Denkpsychologie*, the idea of elementary behavioral organizations proclaimed by the behaviorists, and a search for spiritual organization in Spranger all challenged elementarism and association as an explanatory principle. In the market of ideas all of these novelties presented themselves as exclusive. In reality, however, they were supplementary to each other.

Karl Bühler postulated three ‘concentric’ levels of selection:

For me, in Darwinism the concept of play field seems to be productive. Darwin has basically known only one such play field, while I point to three of them [...] These three play fields are: instinct, habit and intellect. (Bühler 1922. VIII.)

Early ethologists, with whom Bühler was familiar, such as Heinroth, Uexkühl and Konrad Lorenz, clearly described three factors in the unraveling of animal behavior (see Lorenz, 1965, for a review). The first is the postulation of species

specific behavioral patterns. The development of these patterns is predetermined, and is a characteristic of the species in evolutionary terms, but it also requires critical, environment-dependent experiences. In addition, exactly due to the existence of innate/internal biological programs, the animal can never be described as a passive, merely reacting creature: its entire behavioral range is an expression of internal behavior program-patterns.

Karl Bühler tried to unify psychology by relying on these early ethological principles. The key element in this unification account was the idea that all behavior – from the simplest animal behavior to human culture-creating behaviors – is assumed to be meaningful. As a matter of fact, Bühler on the evidence of his unpublished manuscripts, went back to the crisis issue several times, but less with a negative, and rather with a positive message. He hoped to prove both on factual and historical material that psychology has a double commitment, it does belong to the biological sciences, but at the same time to the mental sciences. In this later aspect it does define the attributes and modes of human existence. For example it gives a categorial analysis of language, and for the modes, the issue of gender or age differences (Bühler 1969. 180).

#### VIII. THE WORK OF HARKAI SCHILLER

This meaning and intention or teleology centered vision of animal and human behavior had a decisive influence on Hungarian theoretical and comparative psychology. Paul von Harkai Schiller (1908–1949), or under his English pen name, Paul von Schiller was the most important theoretical and experimental psychologist in Hungary to take up this message of the heritage of Karl Bühler. As the careful analysis of both Magda Marton (1996) and Dewsbury (1994, 1996) clearly showed Harkai was a groundbreaking researcher of an international status in his attempts to connect comparative psychological thought with a semiotic and Gestalt based notion of behavioral organization.

His one and a half decade long work in Hungary in a historical sense was crucial in establishing experimental psychology at the Faculty of Arts at Pázmány University in Budapest, being responsible for the organization of a psychology Seminar, with many doctoral student, including David Rapaport. (See about this broader framework Lénárd 1946, Pléh 1997). His attitude tried to combine *epistemological philosophical issues with the experimental methodology of natural sciences*. He formed his research attitude during his postdoctoral travels to Köhler in Berlin, and Bühler in Vienna. His entire attitude of trying to reveal holistic and at the same time teleological organization in behavior showed the impacts of the Berlin Gestalt, the meaning based proposals of Bühler, the comparative ideas of early ethology, especially Lorenz, and the action based developmental theory of Piaget (Pléh 2005).

The first peculiar feature of his work is an empirically and theoretically motivated renewal of Aristotelian functionalism. For Harkai *The task of psychology* (1940, modified German version Schiller 1947) is to overturn traditional Cartesian dualism, the postulation of a “multi-level man”, a vision that supposes the reality of a mental world on the same abstraction level as physiological processes. Harkai juxtaposes with this image a view of biological man, which is in fact the renewal of an Aristotelian thought by proposing that body and soul, physiology and psychology are not two different levels. Mental phenomena are a particular organization of human bodily or physiological processes. This peculiar biological functionalism links him in the history of Catholic psychology to the works of Mercier (1897/1925), a Belgian neo-Thomist “modernizer”, who – well aware of the facts of experimental psychology of the time – advocated the unity of body and mind. Mercier contrasted this view with that of Wundt who basically defended Cartesian dualism in a modern setting. For Harkai it was also pivotal that there is continuity between Cartesian dualism and the ideas of Wundt (1903).

As far as the impact of Bühler is concerned in his work, Harkai analyzed the early Bühler and the Würzburg school and related efforts (Meinong, Marbe, Watt, Messer, Bühler, Ach, Külpe, Selz, Höningwald) already in his doctoral thesis as bringing the victory of the introspective methods and the non-sensory elements. In his vision that lead to a “clear differentiation in our mental life between acts and contents. [With the advent of non-sensory elements] teleological, active moments had to be postulated that permeate the mental world with their directionality. This moments were first outlined as central by Brentano.” (Harkai 1930. 51.)

In his continuing theoretical work that first appeared as a series of papers (Harkai 1937, 1939) he constantly used Bühler as one of the foundations for his idea of an intentional motivational behavior theory, in a way as a biologized Brentano. He even claimed that while Bühler was a good organizer of action research, at the same time he was too loose regarding teleology (Harkai 1939).

In his later, finalized synthesis the organization of behavior was interpreted as the interaction of the environment and a unified biological organismic entity (Harkai 1940, 1944). The motivational system of organisms only makes sense in an evolutionary background and cannot be interpreted merely as an interaction of experience and physiological processes. This gives a curious flavor to the view of Harkai on the *unity of psychology*. For him the key to unity is that one has to consider the actions in animal behavior, their motivational aspects, the direction of mental processes (their intentionality) and their unified organization. According to Harkai the inspiration for this psychology should come from the followers of the intentionality tradition initiated by Brentano (1874), and of course Karl Bühler (1927) talking about the semiotic unity of psychology, claiming that all human or animal action is characterized by a goal and at the same time it is guided by certain signals. At the same time, behavior has an objective reference

and is organized as a whole. This is what Bühler and Harkai thought to be the right attempt to overcome the contradictions of the fragments of contemporary psychology – motivation-centered psychoanalysis, cognition-centered experimenters and overt action centered behaviorists should unite in a goal centered holistic experimental psychology.

Harkai was expressly a biological functionalist, taking the contemporary early German ethology seriously, parallel with the attitude of Bühler. He has taken up an idea popular in German zoology especially in the writings of Jakob von Uexküll (1864–1944) that the animal lives in a world articulated by its body and by its nervous system, attributing certain meanings to certain elements in the environment. Animals live in a partly constructed *Umwelt* (Uexküll 1909, 1925). “Uexküll thus starts off not from the idea of an objective environment but from a ‘subjective external world’ given to the living being, selected by its sensory and effector apparatus” (Harkai Schiller 1940. 113–114).

What he called “psychological biologism” was an empirically and theoretically motivated renewal of Aristotelian functionalism.

He compared the inspiration of Bühler and Piaget in their ideas about the genesis of consciousness.

In his expression theory Bühler sees in signs a saving on actions; consciousness arises when the operations of physiological regulation do not assure our vital values. Action accommodates the occasions of our life field the needs of our organism. According to Piaget reflex, habits, intention and thought are all instruments of adaptation. They develop in cyclic circles, from restless search towards theories, towards action organizing schemata. In all of these conceptions, joint by the best representatives of our science, it becomes more and more clear that physiological and conscious events are in their higher organization aspects of action organization. (Harkai 1944. 33.)

His theory also appeared in actual experimental work. His numerous (partly posthumous) publications concentrated on what we would call today representational phenomena in animals. Detour behavior (Schiller 1948, 1950), figural preferences and drawings by apes (Schiller 1951, 1952, Schiller and Hartmann 1951).

#### IX. THE IMPACT OF BÜHLER ON THE LATER COMPARATIVE WORK OF KARDOS

The approach Bühler was taking towards a critic of early non intentional visions of animal behavior promoted by behaviorists lead to the other aspect of his criticism of naive behaviorism: the issue of regulation (Garvin, 1966). Animal behavior is regulated not in a mechanistic manner but in a complex cybernetic way. In a late paper of his, Bühler (1954) outlined directional and object-based naviga-



tion both in aviation and in bird flight; he analyzed the mechanical conception of Loeb (1900, 1912) on animal tropism. Interestingly, Bühler claimed that the mechanical vision of Loeb is mistaken because it did not consider cybernetic regulatory factors such as the ones already highlighted by Claude Bernard regarding the regulation of the inner milieu. It is remarkable that the last manuscript of Bühler (1969) that was only published posthumously also presented a theory and a series of experiments on animal navigation, mainly concentrating on bees and birds. The issues of animal teleology figured in earlier works of Bühler, especially in his child development book. As analyzed by Ter Hark (2007) the 1927 book was rather crucial in presenting the intention based synthetic theory of animal behavior, in contrast to the entirely mechanistic vision of Loeb. Friedrich (2018) also shows very clearly how crucial was for Bühler to contrast the mechanistic vision of Loeb with the trial and error vision of intentional animal behavior promoted by Thorndike and Jennings.

Interestingly enough, the former student of Bühler, who started with constancy phenomena, Lajos Kardos, two generations later in a way returned to the Bühler inspiration as a comparative psychologist. Kardos started his theory on the genesis of mental life with an analysis of the Loeb–Jennings debate, and of the coordinative, cybernetic attitude: the genesis of prediction is necessary for the genesis of the mind (Kardos 1980). This theoretical book of Kardos compared to all his other work reads as surprisingly speculative. He is not doing experiments, neither is he doing too much reading. On the basis of some elementary biological background Kardos set out to analyze the postulated behavior of theoretical monocellular organisms. This excursion is used to shed light on the origin of mental life. In this regard it is remarkable that his teacher half a century earlier used the same attitude when proposing a unified sign based framework for psychology, and also started from the Loeb–Jennings debates (Pléh 2013). Unity of biological and meaningful elements in human life on all levels of mental organization was the key notion for Bühler.

The distance between the integrated behavior of the amoeba and human scientific thought is certainly impossible to grasp. Still, on the basis of the most modern observations both can come under two common concepts: they are holistically organized and are characterized by meaningful events. (Bühler 1927. 392.)

In his analysis of the origin of mind the starting point for Kardos was avoidance behavior. Warning signs are crucial in the development of mental life. Starting from the etymology of prevention ('prevent' → 'praevenio') he claimed that organisms use information that precedes harmful events: "harmful impacts are consistently preceded by biologically irrelevant impacts" (Kardos 1980. 24). Signals precede the harmful event. The animal avoids the harmful space, and "the adiphore space is a secure starting place; from here, by well-controlled action it

can avoid any dangerous contact or can achieve contact when desirable” (Kardos 1980. 94).

Kardos initiated a long series of experimental studies on animal learning and memory in rodents from the 1950-as on. On the theoretical level he started from an analysis of the relationships between the “animal way of life” and mental organization. In this regard he is a Gestaltist who was sensitized in the circle of Bühler (1934) to the ideas of early ethology emphasizing species specific behavior and the different *Umwelts* of animals. For Kardos, the essential difference in the way of life between other mammals and apes is the *opposition between locomotion and manipulation*. The actual animal learning experiments of Kardos were run through 30 years, using maze and discrimination learning technologies. His starting point was the idea that behavioral equivalence is crucial to learning. That is an idea again that goes back to the concept of behavioral equivalence claimed by his teacher Bühler (1927) in the framework of early continental ethology. The underlying sign based equivalences in animal learning for Kardos are One place – one sign – one behavior.

The first studies along this line were his experiments on “aequiterminal routes”. (Kardos and Barkóczy 1953). Rats had to learn two slightly different types of mazes, where in one version they had to learn that the same goal has different values depending on the route taken. That was impossible to learn. Rats are not able to learn the distinction that if you came from left than you have food, and if you come from the right, you have no food.

The interpretation of the experiment was that memory representation in animals with a locomotory way of life is place tied, rats being unable to learn different targets being on the same place if the place was reached by different routes. These behavioral results are to be explained according to Kardos by postulating a *mnemonic field* (Kardos 1988). Using some more complex spatial learning situations such as star shaped mazes, Kardos proposed a mnemonic theory slightly different from the cognitive maps of Tolman (1948). Kardos (1988) was claiming that rodents basically are maintaining memory images as vivid as their percepts, rather than cognitive maps as Tolman (1948) claimed.

## X. THE IMPACT OF BÜHLER’S THEORY OF LANGUAGE ON HUNGARIAN LINGUISTICS

Bühler has started to use his general model of language already in his arguments for a three aspect psychology in 1927. His theory is a self-proclaimed *Organon model*, referring to the logical theory of Aristotle. It is a conceptual framework, starting from ‘axioms’ that treat language not in an abstract way, but as an instrument of communication. Persyn-Vialard (2005, 2011) analyzes the functional nature of his model. Human language has by necessity three functions: a) it has an

experiential, inner, first-person reference, it is an *expression (Ausdruck)*, b) it has a relation to other people's behavior, i.e., it has a *directive function (Appel)*, and c) most specifically, it represents something from the external world; it is a symbol (*Darstellung*). For Bühler the proposal is not merely about dimensions of linguistic signs. According to his "crisis-book" (Bühler 1927), this tri-partiality is not a characteristic of language exclusively. It is also a story about the triple aspects of the human condition: the inner world, behavior, and reference to something external and objective are all crucial to mental life. Bühler in this regard speaks about the general semiotics of behavior.

Signs of human language obtain their object reference through a supra-individual logical intentionality. In human language, there is a hierarchy among the three functions. The descriptive and intellectual function is always the leading one. We can express emotions mainly by naming things, and the same holds for the directive functions. At the same time, Bühler was not insensitive to what we would call today the "expressive aspects of speech". He claimed that while the referential function is the basic and defining function of human language, tone of speech, interjections, and other elementary features of our speech channel are also used to express emotions (Bühler 1936b).

The supra-individual semantics shall be the foundation of the existence of a human sphere of thoughts. Through his "objective semantics" Bühler's early interest towards the reality of thought obtained a new anchorage. It will be echoed a generation later by a follower of Bühler in the philosophy of science, Karl Popper (1972, 1976, 1994).

Bühler put rather clearly the connections between his psychological ideas and the communicative specificities of sign based coordination. Social life needs coordination, and in this regard semantics is always social. By developing the descriptive function, animal signal systems increase their efficiency.

1. When where there is real social life, there is a need to coordinate meaningful behaviors of the members of the community. Since the reference points of this coordination are not given in a common perception, they have to be provided with a higher order contact, specifically with semantic dispositions.

Individual needs or dispositions have to be manifested somehow and these manifestations have to be noticed in order for them to be validated in the joint enterprise.

By coordinating signs with objects and states of affairs, they do obtain a new semantic dimension. And due to this process, their communicative efficiency increase importantly. (Bühler 1927. 50–51.)

For Bühler the central issue in the study of language use was the role of grammar, or linguistic organization at large. He relied on the proposal of Saussure (1922), the founding father of modern structure-based linguistics, to start from a differentiation between *langue and parole* (language and speech), and empha-

sized repeatedly that the study of speech as an activity presupposes the study of grammatical linguistic structures. At the same time, regarding relationships between language and speech, under a Humboldtian inspiration, Bühler has turned the Saussure-ian system from a single-instance system differentiating social and individual as *langue* and *parole*, into a four-instance system. The entire system is shown in Table 3.

Table 3. The full system of aspects of language in Bühler

| <i>Level/objectivity</i> | <i>Subjective I.</i> | <i>Objective II.</i>    |
|--------------------------|----------------------|-------------------------|
| Lower level 1.           | speech activity      | language product        |
| Higher level 2           | speech act           | linguistic construction |

#### XI. EARLY HUNGARIAN STRUCTURALIST INTERPRETATIONS OF THE MESSAGE OF SPRACHTHEORIE

The *Ausdruckstheorie* of expressive movements analyzed by Bühler (1933a) was early on absorbed by Hungarian philosophers and philologists. László Bóka (1934) the later literary theorist criticized Bühler in his early review to be too far from language, and too much following Klages. In Bóka's view Bühler was mainly treating in this book language as lacking expressive powers. As a matter of fact, the language theory of Bühler a few years later (1933b) proved the opposite, the same year.

They discussed in many aspects his vision of expressive power in a presentation of a paper by László Gáldi (1940), a later influential lexicographer and style theorist on the expressive power of language as it is related to the lexical choices, mood and the likes. The debate centered around "language character-ology", the issue for expressive differences among languages, and the individual use of language for emotion expressions.

Bühler's theory of language was also discussed in many details from a philosophical point of view by Gáldi (1943). The linguistic reactions around the same time were more consequential. In a way, Bühler figured as a central author in the Saussure inspired first wave of structural linguistics in Hungary.

Gyula Laziczius (1896–1957) the founding father of a Saussure and Prague school inspired structural linguistics in Hungary (see about his impact Kiefer 2008). Laziczius criticized this extension at the time. In general he was very receptive of the general frame of the theory of language promoted by Bühler in his textbook (Laziczius 1942, 1966). As Fónagy (1984), his student reminded us, in the textbook of Laziczius, there are over 100 (!) pages presenting the *Sprachtheorie* of Bühler. At the same time, he was very critical of the combination of Saussure and Humboldt.

The principle that Bühler treats as his third axiom is questionable to the first sight. There is a fourfold distinction involved here, as of *speech actin* [*Sprechhandlung*], *speech work* [*Sprachwerk*], a *speech act* [*Sprechakt*] and *linguistic construction* [*Sprachgebilde*]. [...] Bühler did not realize that the distinction of action and act, work and construction cannot be fit into the distinction of “langue” – “parole” since they do cross classify. [The crucial issue is that in this combination] We abstract in a mixed manner the individual and social aspects, and the „language” nature of the extracted set is unquestionable. With the procedure of Bühler we thus arrive to the undifferentiating of “language”, to the undifferentiating that was to be resolved by Saussure exactly through distinguishing “langue” and “parole” [...] This faulty thesis should necessarily be deleted from the axioms of linguistics, and replaced by the correctly interpreted distinction by Saussure between “langue” and parole. (Laziczius 1940. 42–43.)

## XII. INTERPRETING BÜHLER'S FIELDS AND PRAGMATIC MESSAGE

Two generations later, I tried to treat the fourfold distinction of Bühler as a positive program, where the social–Individual dimension is combined with a speech act like theoretical frame. On the subjective side, Bühler stresses the processes rather than ‘knowledge’ in the classical sense, and in a sense similar to another Aristotelian rejuvenation theory proposed by Gilbert Ryle (1949) the British philosopher, differentiating between *knowing how*, rather than *knowing what*. Bühler supposed that individual actions are accommodated to the system, therefore the analysis of the system (linguistics) was always prior to its usage, to the psychology of language, and also supposed that the social system manifests itself through individual acts (Pléh 1984).

Bühler emphasized two crucial aspects when he talked about the structure of language. The first is the *structure dependence* of the value of individual items. Along with his commitment to *Gestalts* in the organization of all of perceptual psychology, Bühler believed that each linguistic sign obtains its function only with reference to the entire system of signs. On the other hand, signs in combinations form new unities, often by rounding up meanings.

Regarding the structural elements of language, he postulated, in line with structuralist principles, three levels: sounds, words, and sentences. It was especially important for Bühler to show that human language has a double articulation: words and sentences. The other logical possibility – and he has lengthy thought experiments about this – would be to have unstructured long distinct strings to correspond to each individual state of affairs. Unlike this logical possibility, this is the sense in which human language has a double articulation.

Human language is based at least on two classes of institutions (conventions) and accordingly, has two classes of linguistic structures. [...] this corresponds to the choice of words and the construction of sentences. There is a type of linguistic structure that cuts the world into pieces, decomposes it into objects, events etc., in order to reduce it to *abstract elements*, and apply a sign to each of these elements; while the other provides semiotic tools for an integral construction of the same world along *relations*. [...] At the same time these two articulations can move from one to the other, what was syntactic may become part of the vocabulary, and what was lexical may become syntactic. (Bühler 1934. 160.)

This kind of structuralist credo was much appreciated by Laziczius (1940, 1942) at the time. Two generations later I have also pointed out the importance of the early pragmatic vision of the psychology of language concentrating on the notion of fields and deixis.

The notion of field taken over from *Gestalt* psychology was a main tool for Bühler to connect Gestalt considerations with structural linguistics (Garvin, 1966). Bühler spelled out clearly this relationship between fields and Gestalts:

I am convinced that the concept of field in the future should be as central [in linguistics] as it is for us psychologists. As for the notion of “form” [...] let its use be constrained to cases where two things are alternating, such as content and form. In this constrained sense, the most elaborate form can always become content, and the most substantial content can become form for the specialist of Gestalts and for the theoretician. (Bühler 1936b. 61.)

Language in the vision of Bühler functions in two fields. The first field is the *deictic field*, or *demonstrative field*, which is the world of perception “out there”. Language has an entire class of signs, deictic elements (in the terminology of Peirce (1883) indexical signs) that have their meaning filled from the perceptual field. In this regard the notion of the *origo* of the *here*, *now*, and *me* was very important for Bühler (Marthelot 2012). In his vision, deictic signs are not some remnants of an ancient status of language. Communication merely via “naming signs” would make it rather clumsy. The logical criticism of deictic elements in scientific language should not be extended to natural language use.

Where is it written that intersubjective understanding of things [...] is only possible one way with the use of naming signs [*Nennwörter*], with conceptual signs, linguistic symbols? (Bühler 1934. 105.)

The other field in language is the *symbol field*, of which the specific components are concept–word–symbols. These are not tied to a situation, but create an internal linguistic context, and form a continuum; on one extreme of the scale

they function in sentences that are entirely devoid of context, like mathematical propositions. For Bühler such a duality represented the unity and the duality of sensual and abstract moments in language. On the other end of the continuum, the internal, linguistic field provides an interpretation for the signs – as signs are interpreted in relation to each other.

The particular sign obtains its anchorage and the filling of its meaning in the syntagma with other signs of its kind. In this situation the physical environment falls in the background and becomes irrelevant, as the surface of the paper becomes irrelevant as we read books. [...] What is conserved and becomes an object of most careful work is the *synsemantic* anchorage of the sign; it requires be interpreting and understanding in a deeper manner from the linguistic context. *In extremis*, it is merely the intralinguistic, *synsemantic* field that gives its relevance. (Bühler 1936b. 60.)

The expressive and directive functions are related mainly to the deictic field, while the descriptive function to the symbolic field.

Karl Bühler's theory of language was rather modern both in its combination of a philosophical tradition with his intimate knowledge of modern linguistics, and in its argument for an active theory of language. His concentration on the 'descriptive function of language' was accompanied by a hypothesis of constant joint social work and coordination between speakers and hearers. While the representational function was conceived as crucial by him, it does not passively determine our vision of the world through language. Bühler has combined a Kantian inspiration of a priori determination by categories, with a more dynamic activity theory he has taken from Husserl (Persyn-Vialard 2005). As his French editors, Bouveresse (2009a, 2009b), Bühler treated the representational function in a mediating way. Symbolic language drives representation in a dynamic way, much like an instruction system for the hearer to look for things in the real world. This idea is spelled out in detail in his theories of deixis, anaphora, and the relation between the two fields (Marthelot 2012).

There is a cognitive division of labor that corresponds to these linguistic structural levels. First, articulation into words and sentences makes for human memory economy. We do not have to memorize a different sign for each situation. Second, both the constitutive signs and the entire sentence are anchored in the perceptual field. Words look for their referents in the actually perceived world, as well as sentences look for situations corresponding to them.

In my interpretation a generation ago, I contrasted Bühler with the decontextualized modern experimental psycholinguistics of the 1970s (Pléh 1984). Bühler proposed or represented a more complex foundation both for linguistics and for the psychology of language. He has treated language as both a biological and as a social system, where the biological and social are not in contrast or opposition. This was supplemented with the idea of communication having varied functions,

and being embedded in the field of perception and signs as well. One point was missing from the theory, however, and it is very interesting regarding the history of psycholinguistics. Bühler, just like the other great synthesizer of language and psychology a generation earlier, Wilhelm Wundt, did not intend to connect and confirm his axiomatic and theoretical approach to language with his experimental inspiration. Most likely there were two aspects missing to turn psycholinguistics into an experimental chapter. The lack of technical means to easily manipulate and register language stimuli, which has come with magnetic sound recording and analysis systems. Bühler and his generation were also missing language statistics and information theory that later allowed to characterize linguistic stimuli, words and sounds, and even sentences as independent variables, with numbers.

### XIII. LANGUAGE FUNCTIONS AND THE DOUBLE CODING THEORY OF IVÁN FÓNAGY

Regarding the nature of sign-relations, Bühler (1933a, 1933b, 1933c, 1934) had very clear ideas about the communication of emotions and iconicity. He believed that even though the arbitrariness, the lack of motivation between sign and signified is crucial for human languages, human signs are still treated by the users as iconically and emotionally expressive, hence language has an emotionally important iconic basis as well (Bühler 1933c). This suggestion is related to Bühler's general biological commitment: signs are originally biologically relevant movements, and they always functions as expressions of emotions beside their cognitive functions. The Hungarian linguist and psychoanalyst Iván Fónagy (1920–2005) had a very creative extension of the ideas of Bühler regarding the types of linguistic signs and language functions. Regarding the signs themselves, Fónagy claimed that language signs are conventional in the sense of Saussure, but they are not arbitrary. There is an iconic relation between signs and the emotional status of the sender. Regarding the actual communicative situations, Fónagy (1966, 1971) claimed that all speech events, while they serve a descriptive function, they also have a second layer of coding: they code the inner status of the speaker as well. There are coding mechanisms that are responsible for descriptive function of language, but its output is always supplemented by the work of a “Distorter” that tries to use linguistic variations and possibilities to express emotional meanings directly.

From a phylogenetic point of view we might consider the Distorter as a residue of a *pre-linguistic* communication-system, integrated with the linguistic code, and distortion as continuous synchronic motion, a permanent *recreation* of language. The coexistence of Grammar and the Distorter, the double coding of messages is a successful means of *self-programming*. (Fónagy 1971. 219.)



An interesting aspect of this theory was that it treated the emotional/expressive functions as secondary ones. Like the *Sprachtheorie* of Bühler (1933a, 1934), Fónagy also accepted the human specificity of the descriptive, cognitive function of language. The phylogenetically archaic expressive/emotional function is secondary in relation to this. The interpretation of this issue of functions is proposed, however, by Fónagy (1984) in the framework of a more extended multifunctional model of Roman Jakobson (1970) where expressive functions relate to sender and poetic function to the message itself. An innovation of Fónagy is the connection between the poetic function and double coding. In his view, a key to analyze art is to realize that artistic form is also a realization of emotional double coding in language. Form that is unmotivated regarding the referential objects is in an iconic relation with emotions. (Fónagy 2001). Later working together with his psychonaltic son, Peter Fónagy he argued that double condign is based on a „internalized oral mimicry” (Fónagy and Fónagy 1995).

\* \* \*

Bühler was a general inspiration for Hungarian psychologists in the mid Century by proposing unification inspired overcoming of the assumed crisis of psychology. Bühler abstracted three basic parameters of the assumed crisis: (1) the problem of mechanistic explanation, (2) the indirect study of hidden processes, and (3) the subjectivity–objectivity issue. Contrary to the postulation of a split within psychology between natural science and human science, proposed by the followers of Dilthey, according to Bühler, meaningful organization is a characteristic of all behavior, and is not a specificity of the human mind. At the same time, however, behavior should also be interpreted in new ways. It is always a self-initiated activity, never simply reactive as most behaviorists would like it to be. Not even animals – and certainly not humans – can be regarded as merely reactive creatures, as mere automata. Organisms always attempt to construct a model of their environment. In this modeling activity the role of Darwinian selection and its broader interpretation is pivotal for Bühler (1921, 1922a, 1936a). This modeling and motivational inspirations were his most lasting impacts both in Hungarian linguistics and philosophy and psychology.

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## Parallels and Divergencies: Gödel and von Neumann\*

### I. A WEAK CLAIM

John von Neumann (1903–1957) and Kurt Gödel (1906–1974) are two towering figures of 20<sup>th</sup> century science, contributing in particular to mathematics in exceptionally significant ways that had a lasting impact on modern mathematics. Their life and scientific careers had many parallels and their research interests overlapped. But their philosophical views about sciences, especially about the nature and foundations of mathematics were very different. The aim of this paper is to highlight some parallels and what appears to be a correlation between the divergences of their philosophical positions and differences in their scientific research and career. Correlation is not causation in general. So no simplistic claim is formulated here about either scientific research determining the nature of a philosophical position or about their philosophical views setting their research agenda. Rather, on their example it should become clear that scientific research and philosophical views are intertwined and they both enfold conditioned both by personal traits and by a broader social context, some of which the paper indicates.

### II. BEFORE THE 1930 KÖNIGSBERG ENCOUNTER

Both von Neumann and Gödel were born in the Austro-Hungarian Empire: Gödel in Brünn/Brno, Bohemia; von Neumann in Budapest, Hungary. Their life and careers have been reviewed by several biographers (see Feferman 1986, Buldt et al. 2006. Chap. B), especially Köhler 2006a and Köhler 2006b for Gödel, and Macrae 1992, Aspray 1990, Rédei 2005 for von Neumann). Below I rely on these sources when it comes to recalling some episodes from their life and career.

The families they were born into were both well-to-do, headed by a father working successfully in textile industry (Gödel's father) and in banking (von Neumann's father). The financial security provided by the family environments

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made it possible to realize talents, first by receiving solid elementary education, and, subsequently, allowing to benefit from the higher education provided by universities in the German speaking segment of the European university system: Gödel studied in Vienna, von Neumann in Berlin and in Zürich.

Gödel, after considering physics as a field of study, finally chose to study mathematics in Vienna University. His teacher was Hans Hahn, a major figure in functional analysis (“Hahn-Banach Theorem”). Although von Neumann was already a reasonably trained mathematician at the time of graduating from high school, which was due to private tutoring he had received from a university professor, he enrolled in the chemical engineering program in the *Eidgenössische Technische Hochschule* in Zürich. Simultaneously, he registered as a PhD student in mathematics in Budapest. Gödel’s PhD (1930) was in logic, proving completeness of first order logic, von Neumann’s PhD (1926) presented a new axiomatization of set theory. Both PhD’s were major contributions to logic and mathematics, respectively – a very similar start of their academic careers.

In Vienna Gödel was in touch with the philosophers in the Vienna Circle from 1926 but he distanced himself intellectually from this circle because “he had developed strong philosophical views of his own which were, in large part almost diametrically opposed to the views of the logical positivists” (Feferman 1986. 4). According to Gödel’s reply to a questionnaire, he had embraced a realist philosophy of mathematics by 1925 (Gödel 1986. 37). Such a philosophy of mathematics was in sharp contrast to the logicist understanding of the nature of mathematics adopted by the Vienna Circle. So, from 1931 Gödel started abandoning the Vienna Circle meetings and from 1933 he stopped attending completely (Köhler 2006a).

Von Neumann did not have contacts to the Vienna Circle – or to any significant philosophical school – during this time. His interest in philosophy was weak at best, and at that time was restricted to the rather internal, technical issues of the Hilbert program. He hoped to be able to help to show that the Hilbert program can succeed. Accordingly, von Neumann was regarded as the major representative of the formalist understanding of mathematics. But to the extent this classification of von Neumann’s view is correct, it is only so by qualification: he was a moderate formalist, emphasizing the importance of the intuitive content behind the concepts in formal axiomatization. This is expressed already in his axiomatization of set theory (von Neumann 1928):

We begin with describing the system to be axiomatized and with giving the axioms. This will be followed by a brief clarification of the meaning of the symbols and axioms [...]. It goes without saying that in axiomatic investigations as ours, expressions such as “meaning of a symbol” or “meaning of an axiom” should not be taken literally: these symbols and axioms do not have a meaning at all (in principle at least), they only represent (in more or less complete manner) certain concepts of the untenable “naive set theory”. Speaking of “meaning” we always intend the meaning of the concepts taken from “naive set theory”. (Taub 1961. 344, translation from Rédei and Stöltzner 2006.)

After receiving his PhD von Neumann went to Göttingen to work as Hilbert's assistant; apparently with the intention of continuing his work on the Hilbert problem (von Neumann 1927d); however, in Göttingen his interest turned to the mathematical foundations of quantum mechanics.

The publication of the three foundational papers on quantum mechanics (von Neumann 1927a, von Neumann 1927c, von Neumann 1927b) marks a significant deviation of von Neumann's scientific interest from that of Gödel. Not just in the sense that von Neumann's attention gets diverted from the problems of mathematical logic and foundations of mathematics to the foundations of physics – while Gödel was working on his dissertation on completeness –; but, more importantly, the pure mathematical problems von Neumann solves in these papers (first and foremost the spectral theory of unbounded selfadjoint operators defined on an abstract Hilbert space) are obviously directly motivated by the problem situation in the sciences (physics). This type of mathematical work, which is growing out from the empirical sciences, is uncharacteristic of Gödel – a divergence between von Neumann and Gödel about which more will be said below, and which is already present at this early stage of their career. This is due to some extent to the contingent fact that Göttingen was a major center of theoretical physics where the newest results of the emerging quantum mechanics were followed and Hilbert happened to be lecturing on the foundations of quantum theory in 1926.

Working on foundations of physics in Göttingen von Neumann also had to deal with a problem which, to the best of my knowledge, Gödel did not address systematically: the problem of the nature of the axiomatic approach in the context of *empirical* sciences. The problem of how to carry out an axiomatization of an empirical science, which goes back to Hilbert's 6<sup>th</sup> problem *Mathematical Problems. Lecture delivered before the International Congress of Mathematicians at Paris in 1900* (Hilbert 1976; see also Wightman 1976 and Corry 1997), and which is very different from the problem of axiomatization within mathematics and logic. Von Neumann addresses this problem explicitly first in his joint publication with Hilbert and Nordheim (Hilbert et al. 1927). The position they work out is a characteristic mixture of formal axiomatics and informal but explicit stipulations linking mathematics to empirical postulates. This position was dubbed “opportunistic soft axiomatics” in the papers Stöltzner 2001, Stöltzner 2004, Rédei and Stöltzner 2006, Rédei 2005, where the details of this concept can be found, together with an illustration of this sort of axiomatization by the example of (non-relativistic) quantum mechanics as systematized by von Neumann in his book von Neumann 1932. What is relevant from the perspective of the comparison of Gödel's and von Neumann's views is that for von Neumann this sort of “soft” axiomatization is, again, directly motivated by the problem situation in empirical science (physics). Furthermore, this concept takes into account the actual practice of creating mathematical models of physical phenomena.

## III. THE 1930 KÖNIGSBERG ENCOUNTER

The world lines of Gödel and von Neumann crossed the first time at the Königsberg conference in 1930, and their meeting coincided with the well-known substantial turn in the history of logic and hence philosophy of mathematics: it was during this conference that Gödel announced his first incompleteness theorem the first time in public. The main events at (and right after) the conference are described in Sieg's introductory comments (Gödel 2003. 329–335) to the von Neumann–Gödel correspondence (see also Köhler 2006a). The essential points are the following: von Neumann, after learning from Gödel the existence of undecidable propositions, proved the second incompleteness theorem independently and reported on this to Gödel in a letter dated November 20, 1930. But by then Gödel had also arrived at this result and had in fact submitted his paper containing this result on November 17. Von Neumann, acknowledging Gödel's priority, did not wish to publish on the matter (von Neumann's letter to Gödel, November 29, 1930. Gödel 2003. 339–340).

From the perspective of parallels and divergences between von Neumann and Gödel the remarkable aspect of the von Neumann–Gödel exchange right after the Königsberg conference is that they sharply disagreed on the philosophical significance of the second incompleteness theorem: von Neumann declared:

Thus, I think that your result has solved negatively the foundational question: there is no rigorous justification for classical mathematics. What sense to attribute to our hope, according to which it is de facto consistent, I do not know – but in my view that does not change the completed fact. (Von Neumann to Gödel, November 29, 1930. Gödel 2003. 339–340.)

Von Neumann held this position consistently from the moment of discovery of the second incompleteness theorem and he expressed it unambiguously several times: in a letter to Carnap in which he discusses the publication of the Königsberg talks and also in a letter to his Hungarian friend, the physics professor in Budapest, Rudolf Ortway. The relevant passages from these letters are as follows:

To Ortway:

Gödel's results mean that there is no “complete” axiomatic system, not even in mathematics, and I believe that there is actually no other consistent interpretation of this complex of questions. (Von Neumann to Ortway, July 18, 1939. Rédei 2005.)

To Carnap:

(1) Gödel has shown the unrealizability of Hilbert's program.<sup>1</sup>

(a) There is no more reason to reject intuitionism (if one disregards the aesthetic issue, which in practice will also for me be the decisive factor).

Therefore I consider the state of the foundational discussion in Königsberg to be outdated, for Gödel's fundamental discoveries have brought the question to a completely different level. (I know that Gödel is much more careful in the evaluation of his results, but in my opinion on *this* point he does not see the connections correctly). (Von Neumann to Carnap June 7, 1931. Rédei 2005; also see Mancosu 1999.)

Gödel disagreed with this interpretation; at least initially, in 1931:

I wish to note expressly that Theorem XI [the second incompleteness theorem] does not contradict Hilbert's formalistic viewpoint. For this viewpoint presupposes only the existence of a consistency proof in which nothing but finitary means of proof is used, and it is conceivable that there exists finitary proofs that *cannot* be expressed in the formalism of P [Russell's Principia plus the Peano axioms]. (Gödel 1931, Gödel 1986. 195.)

The disagreement between Gödel and von Neumann is explained by their different interpretations of intuitionism and finitism: for von Neumann these were essentially the same from the start whereas Gödel regarded finitism a narrower concept. Identifying finitism with the Hilbert program Gödel thus came into agreement with von Neumann's evaluation of the significance of the second incompleteness theorem in 1933 (see Sieg's description for more details about Gödel's changing position and eventual agreement with von Neumann's interpretation of the second incompleteness theorem; Gödel 2003. 332).

Although Gödel's and von Neumann's views on the interpretation of the second incompleteness theorem converged eventually, they diverged in the more informal philosophical conclusions they had drawn from the failure of the Hilbert program. The divergence was both explicit and tacit: it got formulated explicitly as a Platonist philosophy of mathematics in the philosophical works of Gödel and it led to an empiricist concept of mathematics in the philosophical reflections by von Neumann; furthermore, it manifested in a tacit manner in the different types of mathematical research they carried out.

<sup>1</sup> Von Neumann's footnote: "I would like to emphasize: nothing in Hilbert's aims is false. Could they be carried out then it would follow from them absolutely what he claims. But they cannot be carried out, this I know only since September 1930."

#### IV. DIVERGENT CONCLUSIONS FROM THE INCOMPLETENESS THEOREM

Von Neumann never tried to write philosophy systematically<sup>2</sup>, Gödel did. In fact, from about 1943, “[...] Gödel devoted himself almost entirely to the philosophy of mathematics and then to general philosophy and metaphysics” (Feferman 1986, 13). By that time Gödel was at the Institute of Advanced Study (IAS) in Princeton – just like von Neumann. Von Neumann got appointed in 1933, soon after the IAS had been established. Gödel visited IAS three times before settling there permanently in 1940. It was during those visits that Gödel found the proofs of relative independence in ZF of the axiom of choice (1935) and continuum hypothesis (1937) – Gödel’s other two major contributions to mathematics. Von Neumann was fully aware of these achievements and he played a crucial role in arranging Gödel’s permanent appointment to IAS, when Gödel desperately tried to leave Austria in 1939: he urged IAS to try to secure a special visa for Gödel. In a letter to Veblen von Neumann writes:

The claim may be made with perfect justification that Gödel is unreplaceable for our educational program. Indeed Gödel is absolutely irreplaceable; he is the only mathematician alive about whom I would dare to make this statement. He represents a very important branch of mathematics, formal logics, in which he outranks everybody else to a much higher degree than usually happens in any other branch of mathematics. Indeed, the entire modern development of formal logics concerning “undecidable questions”, the solution of the famous “continuum hypothesis”, and quite unexpected connections between this field and other parts of mathematics, are his entirely individual contribution. Besides, the oeuvre of his scientific achievements is obviously still in steep ascent, and more is to be expected from him in the future. I am convinced that salvaging him from the wreck of Europe is one of the great single contributions anyone could make to science at this moment. (Von Neumann to Veblen September 27, 1939. Rédei 2005.)

The expectation expressed in von Neumann’s letter about further major contributions to mathematics by Gödel were not really met. It has been found puzzling why from the early 1940s Gödel’s interest changed to philosophy from mathematics, where he proved so brilliant (Köhler 2006b). It sure is part of the answer that Gödel, by nature being an introverted person, needed congenial stimulus, discussions with colleagues, and it was unfortunate that the most suitable colleague to exchange ideas with, namely von Neumann, was mainly away

<sup>2</sup> His reservation to write philosophical papers came to the surface when he declined an invitation to a philosophy conference when the invitation was coupled with the expectation of writing up his contribution in form of a paper. (See von Neumann’s letter to Ernest Nagel December 9, 1953. Rédei 2005.)

from IAS doing war-work (Köhler 2006b). But this divergence between Gödel and von Neumann was not simply a contingent, unfortunate circumstance caused by war. It was already a consequence of a difference in attitudes towards mathematics – a difference in philosophy of mathematics.

Gödel embraced a realist-platonist concept of mathematics. Perhaps the most important (Köhler 2006a) articulation of his platonistic philosophy is his paper prepared for the Gibbs Lecture in 1951. Gödel intended to publish this paper; however, the paper remained a hand-written manuscript that only got published in 1995 (Gödel 1951). One of the main claims of this paper is that mathematics is *incompletable, inexhaustible*. The main argument in favor of this claim uses the second incompleteness theorem:

It is *this* theorem which makes the incompleteness of mathematics particularly evident. For *it makes it impossible that someone should set up a certain well-defined system of axioms and rules and consistently make the following assertion about it: All of these axioms and rules I perceive (with mathematical certitude) to be correct, and moreover I believe that they contain all of mathematics*. If someone makes such a statement he contradicts himself. For if he perceives the axioms under consideration to be correct, he also perceives (with the same certainty) that they are consistent. Hence he has a mathematical insight not derivable from his axioms. (Gödel 1951. 309; emphasis in original.)

From this incompleteness argument Gödel draws the following “disjunctive conclusion” (Gödel 1951. 310):

*Either mathematics is incompletable in this sense, that its evident axioms can never be comprised in a finite rule, that is to say, the human mind (even within the realm of pure mathematics) infinitely surpasses the power of any finite machine, or else there exist absolutely unsolvable diophantine problems...* (Gödel 1951. 310; emphasis in original.)

The further consequence of this (non-exclusive) disjunction is a non-mechanistic, non-materialistic concept of the human mind (if one takes the first component of the disjunction). The second component of the disjunction “...seems to disprove the view that mathematics is only our own creation...” (Gödel 1951. 311) because

So this alternative seems to imply that mathematical objects and facts (or at least *something* in them) exist objectively and independently of our mental acts and decisions, that is to say [...] some form or other of Platonism or “realism” as to the mathematical objects. (Gödel 1951. 211–312.)

On the basis of the position that mathematics is not a human creation Gödel also criticizes the logical positivists concept of mathematics (“logicism”, Gödel calls

it “conventionalism”), but his criticism is not an outright rejection. He acknowledges that the logicist position is right about claiming that mathematics does not state anything about the physical world because mathematical statements are true “...already owing to the meaning of the terms occurring in it, irrespectively of the world of real things” (Gödel 1951. 320).

What is wrong, however, is that the meaning of these terms (that is, the concepts they denote) is asserted to be something man-made and consisting merely in semantical conventions. The truth, I believe, is that these concepts form an objective reality of their own, which we cannot create or change, but only perceive and describe. (Gödel 1951. 320.)

Since von Neumann did not write papers on philosophy of mathematics proper, one has to interpret the nature of his mathematical research and rely on his semi-popular writings to get a picture of how he saw the features of mathematics. The major source in this connection is his 1947 paper (von Neumann 1961), in which he addresses philosophical questions about mathematics, in particular the consequences of the second incompleteness theorem.

Von Neumann’s first main conclusion from the second incompleteness theorem is that the concept of mathematical rigor is not something that one can establish once and for all. Rather, he regards it as historically changeable. There is no absolute, fixed notion of precision, clarity and exactness:

Whatever philosophical or epistemological preferences anyone may have in this respect, the mathematical fraternities’ actual experiences with its subject give little support to the assumption of the existence of an a priori concept of mathematical rigor (von Neumann 1961. 6).

From the changeability of the concept of mathematical rigor von Neumann draws another conclusion, which is very characteristic for his concept of mathematics: he thinks that “the variability of the concept of rigor shows that something else besides mathematical abstraction must enter into the makeup of mathematics” (von Neumann 1961. 4). What is this “something else”? von Neumann is very careful in his answer. He says that the case “in favor of the empirical nature of this extra content” is strong – without claiming that such a position is defensible without reasonable doubt. But the whole spirit and trust of his paper von Neumann 1961 – and his activity as a mathematician – clearly indicate that this is the position he thinks is the right one:

The most vitally characteristic fact about mathematics is, in my opinion, its quite peculiar relationship to the natural sciences, or, more generally, to any science which interprets experience on a higher than purely descriptive level (von Neumann 1961. 4).

A specific aspect of this relationship is that

It is undeniable that some of the best inspirations in mathematics – *in those parts of it which are as pure mathematics as one can imagine* – have come from the natural sciences (von Neumann 1961. 2; my emphasis).

Von Neumann gives geometry and calculus as “monumental” examples for mathematical theories that have empirical origins – but he could have mentioned many more. In fact, von Neumann’s own mathematical activity unfolded in a way that it is itself evidence for the truth of his claim: his early work on functional analysis mentioned earlier grew out of the problem of mathematical modeling of quantum phenomena; his work on ergodic theory originates in Boltzmann’s work on classical statistical mechanics; the theory of von Neumann algebras (“rings of operators”) emerged in the context of general quantum theory partly out of the need to decompose (factorise) quantum systems into subsystems; the theory of continuous geometry has its origins in quantum logic. These areas are part of pure mathematics; yet, they clearly originate in problem situations in physics. But von Neumann’s position is even broader in the sense that he regards fields other than physics as a potential source of mathematical concepts and knowledge. Economics is the prime example: for von Neumann it was the source and motivation to develop game theory. Paying close attention to sciences, formulating mathematical concepts, isolating structures, grasping the content of the scientific situation in terms of axioms and investigating their consequences, is very characteristic of von Neumann’s mathematical research, and it is in full harmony with his picture of what is essential about mathematics. In this he diverges from Gödel significantly. Gödel typically did not work on mathematical problems arising from the sciences, physics in particular. When he worked on problems related to physics, the motivation came not from physics proper but rather from philosophy: for instance in Gödel’s work on relativity theory, when he proved that the Einstein equations admit a solution in which closed time-like curves appear (Gödel 1949a), the motivation, according to his own account, was his desire to clarify the relation between relativity theory and Kant’s philosophy of time (Gödel 1949b. 274) (see also Stöltzner 2006. 289).

This is not to say that Gödel regarded mathematics and physics as completely separate. In his criticism of Carnap’s logicist position about meaninglessness of mathematical statements he writes:

If it is argued that mathematical propositions have no content because, by themselves, they imply nothing about experiences, the answer is that the same is true of laws of nature. For laws of nature without mathematics or logic imply as little about experiences as mathematics without laws of nature. (Gödel 1953. 360.)



In an example following the above quotation Gödel describes how mathematics actually does add genuine content to natural laws (see Stöltzner 2006. 293) for further, current examples elaborating this idea). Yet, it is fair to say that Gödel, unlike von Neumann, did not consider physics (empirical sciences more generally) as a crucial source of mathematics, without which the nature of mathematics cannot be understood.

In one respect the von Neumann and Gödel concepts of mathematics are parallel though. Both think that mathematics is not an arbitrary creation: von Neumann's position entails that mathematical content is coming to us from the natural and social world mediated through natural and social sciences, physics and economics in particular. Gödel would not say this, but to the extent mathematics is not (fully) our creation, he regards it as allowing a somewhat empiricist position much like in connection with physics:

This whole consideration incidentally shows that the philosophical implications of the mathematical facts explained do not lie entirely on the side of rationalistic or idealistic philosophy, but that in one respect they favor an empiricist viewpoint.<sup>3</sup> (Gödel 1951. 313.)

Gödel even goes as far as drawing the conclusion that, as a result

If mathematics describes an objective world just like physics, there is no reason why inductive methods should not be applied in mathematics just the same as in physics (Gödel 1951. 313).

It is perhaps too strong to characterize this parallel between von Neumann and Gödel's pictures of mathematics by saying that "Through the rejection of conventionalism (by Gödel) the strict limit between empirical and mathematical truths disappears" (Stöltzner 2006. 292) but it is clear that neither Gödel nor von Neumann regarded mathematics as subjective or arbitrary.

Another idea formulated by von Neumann underscores the importance of empirical origin of mathematics. Von Neumann acknowledges that once the mathematical concepts needed to form a mathematical model of an extra-mathematical phenomenon has been obtained, they take on their own life, they develop internally and after a while the resulted mathematics gets so far from its origin that those origins are hard to trace or recognize. Von Neumann mentions specifically the axiom of choice and the continuum hypothesis, precisely the two major problems to which Gödel made substantial contributions in the 1930s, as examples of issues to which mathematics has been led following its internal

<sup>3</sup> Gödel's footnote: "To be more precise, it suggests that the situation in mathematics is not so very different from that in the natural sciences" (Gödel 1951. 313).

development (von Neumann 1961). This von Neumann regards as a perfectly normal process that is part of the ordinary workings of mathematics. There is however, in von Neumann's view, also a danger lurking in this internal development:

As a mathematical discipline travels far from its empirical source, or still more, if it is a second and third generation only indirectly inspired by ideas coming from "reality" it is beset with very grave dangers. It becomes more and more purely aestheticizing, more and more purely *l'art pour l'art*. This need not be bad, if the field is surrounded by correlated subjects, which still have closer empirical connections [...] But there is a grave danger that the subject will develop along the line of least resistance, that the stream, so far from its source, will separate into a multitude of insignificant branches, and that the discipline will become a disorganized mass of details and complexities. In other words, at a great distance from its empirical source, or after much "abstract" inbreeding, a mathematical subject is in danger of degeneration. [...] whenever this stage is reached, the only remedy seems to me to be the rejuvenating return to the source: the re-injection of more or less directly empirical ideas. (Von Neumann 1961. 9.)

## V. CLOSING COMMENTS

The just described divergence in their views about the nature of mathematics would probably have had the consequence that Gödel and von Neumann would not have interacted too much at IAS even if von Neumann had not been increasingly involved in war work during the 1940s as the war was raging on. In this connection one could mention that von Neumann did not interact too much with the another prominent member of IAS, Einstein, either – in spite the fact that one would expect this, given their major roles and interest in foundations and philosophy of quantum mechanics. (See Rédei 2011 for mentioning an episode of interaction between them on foundations of quantum mechanics.) It seems that a more substantial cooperation between Einstein and von Neumann was hindered by Einstein being *less* mathematically minded than ideal for a fruitful exchange of ideas with von Neumann, whereas Gödel was *more* inspired by pure mathematics than ideal for a useful and more intensive intellectual contact between Gödel and von Neumann at that time.

After the war the careers of Gödel's and von Neumann diverged wildly: von Neumann got involved more and more in applied research (computer development) and government advising, which culminated in his appointment as Atomic Energy Commissioner (1954). In this change of von Neumann's career other philosophical considerations, unrelated to philosophy of mathematics, played a role: von Neumann thought that scientists should get involved in government-military advising for moral reasons (Rédei 2005. Introduction). Gödel

remained fully within the protected walls of academia and continued his work, mainly pursuing philosophy. What started in the Austro-Hungarian Empire as very similar academic careers and which developed subsequently through overlapping scientific interests thus ended in the U.S.A. as radically different. In the divergence philosophical differences about the abstract epistemological nature of mathematics and its relation to sciences played a role, and conversely: these views were shaped by the scientific research they carried out.

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# A Snapshot of Austrian Philosophy on the Eve of Franz Brentano's Arrival: The Young Bernhard Alexander in Vienna in 1868–1871

## I. INTRODUCTION

When Bernhard (Bernát) Alexander arrived in Vienna in autumn 1868, he was still far from being the widely respected university professor, public writer, and art critic who would later influence entire generations of Hungarian philosophers and the intellectual climate of his native country.<sup>1</sup> What separated him from this status was not merely his young age (he had just turned 18) and the corresponding lack of academic career milestones he reached in the coming decades (in the face of the rising tide of antisemitism in Hungary), but, first and foremost, the lack of his strong philosophical commitment to Neo-Kantianism he acquired throughout the later stages of his academic peregrination which he spent in Berlin (WS 1871–1872), Göttingen (SS 1872), and Leipzig (WS 1872–

<sup>1</sup> Alexander was appointed to the University of Budapest as a lecturer (*Privatdozent*) in 1878, i.e., shortly after his return to Hungary. Yet, he was passed over when the successor of Cyrill Horváth, Alexander's own teacher (see Section II. 2 below) was elected in 1886 and had to wait until 1895 to become an extraordinary professor. Even then, this appointment proposal received only a minimal majority in the faculty, and Alexander's promotion to the rank of ordinary professor, which, according to normal academic procedure, would have taken place after three years, was repeatedly rejected between 1898 and 1902 (as presented on the basis of archival documents: Gergely 1976. 15 ff). As we are going to see below, Alexander, in retrospect, attributed these obstacles to latent antisemitism. In 1904, Alexander, however, managed to secure his professorial appointment by virtue of governmental intervention. As a full professor, he gained considerable fame (e.g., his public lecture course was attended by more than thousand people from all over Budapest, cf. Gergely 1976. 19), became dean of the faculty (1914–1915), and, *inter alia*, president of the Hungarian Philosophical Society (1914–1919). His career suffered a second blow due to his alleged support for the Hungarian Soviet Republic of 1919 (cf. Turbucz 2017). In 1923, he returned from emigration and was partially compensated, but his career never fully recovered. (There is still no detailed intellectual biography of Alexander, pioneering preliminary work was done by Gábor 1986; for reliable biographical data directly based on primary sources, see: Kovács et al. 2012. 25–27.) Even though a complete bibliography of Alexander also remains a scholarly *desideratum*, it is indicative of the extent of his journalistic activity, that a collection of newspaper commentaries published by Alexander in German language in the *Pester Lloyd* during the last years of his life (i.e., in 1924–1927, including, literary, the last month of his life), which was preserved in his literary estate (Ms. MTAK 4110), consists of 24 newspapers cutouts alone (in addition to further 7 undated and one posthumously published items).

1873–SS 1873).<sup>2</sup> Given that it took decades for the mature Alexander's young students educated in Neo-Kantianism of their professor to discover phenomenology for themselves and introduce it to Hungary, it might be compelling to speculate as to which kind of different course the history of philosophy would have taken in Hungary, had the young Alexander arrived in Vienna just a few years later or had stayed just a few years longer in order for him to encounter the philosophical debut of Franz Brentano in Spring 1874 (see Brentano 1874b) who broke new grounds in Austrian philosophy. Yet, the present paper is not an exercise in counterfactual history-writing in philosophy. Rather, it is dedicated to reconstructing and exploring the historical perspective offered by Alexander's account of Austrian philosophy as it was actually practiced in Vienna on the eve of Franz Brentano's arrival who inaugurated Austrian Philosophy (with a capital 'P'), both according to the hagiographical narrative construed by the Vienna Circle and the more scientific, though still extrinsically motivated conception of Rudolf Haller.<sup>3</sup> Thus, the present paper is intended to augment the author's

<sup>2</sup> This historiographical judgement was already pronounced by Gyula Kornis (1885–1958), Alexander's colleague who became an ordinary professor shortly after Alexander's expulsion, see Kornis 1930. 196–197. (Technically speaking, Kornis inherited the chair of Alexander's teacher Horváth through Horváth's successor Pauer, while Alexander's chair, which was dedicated to the history of philosophy, had remained vacant for decades after Alexander's forced retirement in 1922, see Gergely 1976. 29.)

<sup>3</sup> Classical exposition: Haller 1979. 7 ff. (Haller notably employed an idiomatic capitalization: "*Österreichische Philosophie*", which I render by idiomatically capitalizing the English word "philosophy"). Already by that time, Haller was fighting against his critics who claimed that the historiographical idea of Austrian Philosophy is a "Procrustean bed" (Haller 1986), plainly "false" (40), or simply too "unclear and blurred" (42) to be useful as a historiographical category. His defense boiled down to asserting the exceptional nature of the "lineage of tradition [*Traditionslinie*]" that is constituted by Austrian Philosophy (41). Even as late as in the last year before his retirement (i.e., becoming a professor emeritus), Haller insisted on the idea of an Austrian *Sonderweg* in the history of modern philosophy (cf. Haller 1996. 153), supposedly characterized (see 14–155) by the shared rejection of Kantianism and subsequent German Idealism, aversion to existentialism, adoption of the methodology of the critique of language, and commitment to making philosophy scientific (even though Haller admitted that Brentano's philosophy exhibited "an impressive residual potential for metaphysics [*metaphysischen Restpotenzial*]" ; 155). In a telling passage of his late intellectual autobiography, however, Haller clearly stated that his introduction of the historiographical category was motivated by extrinsic consideration, namely "to pursue the tradition of Austrian philosophy from the vantage point of Russell, G. E. Moore and their followers", who, Haller confesses, embody the "*Weltgeist*" (Haller 2001. 583). The alleged exceptionalism of Austrian Philosophy, thus, seems to be rooted in Haller's admitted metaphilosophical preference for analytic philosophy. On the other hand, it is possible to make sense of Austrian philosophy not as an extrinsically motivated prescriptive notion, but rather as a descriptive notion pertaining to the local peculiarities of philosophy as it was practiced in the Habsburg Empire – which, however, obviously antedated the symbolical datum of 1874. Furthermore, as Katalin Neumer recently pointed out (notwithstanding her professional indebtedness to the research program initiated by Haller), Haller's thesis is not merely descriptively untrue (e.g., as she argued, Wittgenstein, who was probably the most prevalent Austrian philosopher, stood in the striking proximity of German *Lebensphilosophie* during the late stage of his thinking), but also disregards the historical fact that Austria (i.e., the Habsburg Empire) was far from being a homogenous

earlier research on overcoming the received view of the (pre)history of phenomenology, e.g. his attempts at reconstructing the biographical and conceptual links between Edmund Husserl and Herbartian psychology transmitted via Robert Zimmermann (see Varga 2015) or between the School of Brentano and the philosophical logic that was prevalent in German academic philosophy (*Universitätphilosophie*) prior to the slow reception of Boole-Jevons-style old English logic and the triumphant rise of Bertrand Russell's new English logic (see Varga 2016b). In any case, the story of Alexander's stay in Vienna in 1868–1871, no matter how brief it had been, represents one of the few authentic pieces of the history of Hungarian philosophy which are, at the same time, equally part of the history of Austrian philosophy proper.<sup>4</sup>

## II. THE HISTORICAL CIRCUMSTANCES OF ALEXANDER'S STAY IN VIENNA

### 1. Courses attended

In the unpublished *curriculum vitae* that Alexander attached to his doctoral application at the University of Leipzig in July 1873, he wrote the following about his studies in Vienna and the brief prelude he spent at the university of his native city:

When I entered university, I have already chosen philosophy as the special field of studies. I have visited classes relating to this and philology for a year in Budapest and went then to Vienna where I had the pleasure of winning Professor [Robert] Zimmermann as a dear friend and supporter of my studies. On his advice, I enthusiastically studied natural sciences, [...] have attended – with the exception of Prof Zimmermann's lectures – only classes in theoretical branches of medicine, like anatomy and physiology. Of course, I continued my philosophical studies in a private way and prepared for the final university examination, mainly in order to obtain a supporting grant from the Hungarian government, so that my time, which, until then, was divid-

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and exclusively German-speaking *Nationalstaat* (Neumer 2004. 126). In the end, as Neumer insightfully remarked (see esp. 129), the idea of an 'Austrian Philosophy' is rooted in the general methodological dilemmas of writing the history of philosophy, insofar as Haller's thesis represents a markedly universalistic conception of philosophy (hence Haller's emphasis on the supposedly universal values embodied by analytic philosophy), while philosophy, Neumer believed, simultaneously purports the claim of universal validity and is embedded in locally determined networks.

<sup>4</sup> Despite autobiographical documents made available already in the 1920s (see Section II. 2 below), in the research literature there are only cursory discussions of Alexander's academic peregrination (cf. note 1 above and, e.g., Kornis 1930. 196 ff.).



ed between jobs for earning my living and my studies, could be dedicated to the latter alone. I passed the final university examination in philosophy and German language and literature with excellent results and went then to Germany [...].<sup>5</sup>

It is worth comparing Alexander's autobiographical narrative with archival data pertaining to his university studies. The individual student records at the University of Budapest have, unfortunately, been destroyed during in the turmoil surrounding the uprising of 1956, but the University Archives of Vienna preserves detailed information on the classes Alexander registered for during the Austrian section of his academic peregrination (first partial publication in Varga 2016c. 262–263). According to this data, the young Alexander, aged 18 and specifying “Hungarian” as his native language and “Jewish” as his religion, enrolled on the basis of the record of his previous studies at the University of Budapest (until 1873: of Pest) in WS 1868–1869 and opted to attend the following classes:<sup>6</sup>

- *Practical philosophy* (Praktische Philosophie) by Robert Zimmermann;
- *Aesthetics* (Aesthetik) by Robert Zimmermann;
- *Advanced seminar* (Philosophisches Conversatorium) by Robert Zimmermann;
- *Sophocles (Sophocles)* [full title: *Philological Seminar: Ancient Greek (Interpretation of Sophocles' Ajax); Griechische Übungen im philologischen Seminar (Interpretation von Sophokles Aias)*] by Emanuel Hoffmann;<sup>7</sup>

<sup>5</sup> Ms. UA Leipzig, Phil. Fak. Prom. 03403 (1876). 10 (all translations and transcriptions are by the present author, unless indicated otherwise). Original (supplied only in case of quotation from unpublished sources): “Als ich die Universität bezug, hatte ich Philosophie schon als Fachstudium erwählt. Ich hörte ein Jahr lang diesbezügliche und philologische Vorlesungen an der Universität zu Pest und gieng dann nach Wien, wo ich so glücklich war, im H. Professor Zimmermann einen warmen Freund und Förderer meiner Studien zu gewinnen. Auf sein Anrathen studierte ich eifrig Naturwissenschaften, zu <?> hörte zwei Jahre lang, mit Ausnahme der Vorlesungen von H. Prof. Zimmermann<,> nur theoretische Zweige der Medicin, wie Anatomie und Physiologie. Ich setzte natürlich privatim meine philosophische Studien fort, und bereitete mich auf das Staatsexamen vor, hauptsächlich, um eine Unterstützung von Seiten der ungarischen Regierung zu erhalten und meine Zeit, die bis dahin zwischen den Beschäftigungen, mir meinen Lebensunterhalt zu sichern und meinen Studien getheilt war, nun ganz diesen letzteren widmen zu können. Ich bestand das Staatsexamen aus Philosophie, deutscher Sprache und Literatur mit ausgezeichnetem Erfolg, gieng dann nach Deutschland <...>.”

<sup>6</sup> I provide a full transcript of Alexander's entries Ms. UA Wien, Nationalen Phil. Fak. WS 1868/69 ff. (Med. Fak. since Alexander's second academic year), collated against the corresponding course catalogues (*Öffentliche Vorlesungen an der k. k. Universität zu Wien im Winter-Semester 1868/9 [...]*. Wien, Kaiserlich-königliche Hof- und Staatsdruckerei. 1868 ff.). I have previously published the list of the philosophical courses (see: Varga 2016c. 262–263). The dates of Alexander's registration and de-registration from the faculties of the University of Vienna, as well as the personal data from his registration form, was already published in Patyi et al. 2015. 69.

<sup>7</sup> Emanuel Hoffmann (1825–1900), professor of classical philology at the universities of Graz (1850) and Vienna (1856), editor, amongst others, of Augustine's *De civitate dei*.

- *Euripides*[, *Cyclops*] (*Euripides*[, *Cyclops*]) by Johannes Vahlen;<sup>8</sup>
- *Theory of organic and inorganic chemistry* (*Theorie der organischen und unorganischen Chemie*) by E[duard] Lippmann;<sup>9</sup>
- *Mathematics* (*Mathematik*) by Joseph Petzval.<sup>10</sup>

In the subsequent SS 1869, Alexander registered for the following classes:

- *History of philosophy, Third Part; From Kant down to the modern age* (*Geschichte der Philosophie [III. Coursus; V]on Kant bis auf die Neuzeit [official title: bis auf die Gegenwart]*) by Robert Zimmermann;
- *[On] the life and works of Fr. Schleiermacher* (*[Über] Fr. Schleiermachers Leben und Werke*) by Robert Zimmermann;
- *[Presentation of the] Sankhya philosophy* (*[Darstellung der] Sánkhya[-]Philosophie*) by Ludwig Poley;<sup>11</sup>
- *Philosophy of law* (*Rechtsphilosophie*) by Lorenz von Stein.<sup>12</sup>

In the following semester (WS 1869–1870) Alexander transferred to the Faculty of Medicine; yet, he remained faithful to Zimmermann:

- *Descriptive anatomy* (*Descriptive Anatomie*) by Joseph Hyrtl (1810–1894);
- *Anatomy training course* (*Secierübungen*) by Joseph Hyrtl;
- *[General and medical-pharmaceutical] chemistry* (*[Allgemeine und medizinisch-pharmaceutische] Chemie*) by Joseph Redtenbacher (1810–1870);
- *Advanced seminar* (*Philosophisches Conversatorium*) by Robert Zimmermann;

<sup>8</sup> Johann Vahlen (1830–1911), professor of classical philology at the universities of Breslau (1856), Freiburg (1858), Vienna (in the same year), and Berlin (1874), commentator, *inter alia*, of Aristotle's rhetorical writings.

<sup>9</sup> Eduard Lippmann (1838–1919) obtained his habilitation in 1869 at the University of Vienna, where he would become an extraordinary professor of chemistry in 1875. He specialized in the chemistry of aromatic organic compounds.

<sup>10</sup> The class attended by Alexander was probably one or two of the sub-classes of the *Introduction to Advanced Mathematics* (*Einleitung in die höhere Mathematik*): *Algebraic Analysis* (*Algebraische Analysis*) or *Theory of Higher-Order Equations* (*Theorie der Höheren Gleichungen*). Joseph Petzval (1807–1891) started his professorial career at the University of Budapest (1835) but soon moved to Vienna (1835), where he had been ordinary professor of mathematics (1837–1877). Today, Petzval is mostly remembered for his pioneering work in the 1840s and 1850s on designing photographic lens. Contrary to what is indicated in Alexander's registration form, in WS 1868–1869, Petzval only taught the advanced theoretical classes. The introductory classes were held, instead, by the astronomer Edmund Weiss (Weiß, 1837–1917) who would become an extraordinary professor only in 1869 (ordinary professor: 1875).

<sup>11</sup> The less-known Ludwig Poley (1812 [?] – 1885) obtained his *venia legendi* for indology at the University of Vienna in 1867 and started lecturing in the same year. His application in 1871 for the status of an extraordinary professor was, however, rejected. Poley continued lecturing until his death. For his biography, see Schroeder 1917 (which confirms Alexander's report, see below, of Poley's personal acquaintance of Hegel).

<sup>12</sup> After being forced to leave his native University of Kiel due to his involvement in the German Revolution of 1848–1849, Lorenz von Stein (1815–1890) became an ordinary political science at the University of Vienna in 1855 and launched a career as an influential, philosophically-inclined, and highly decorated professor.

- [crossed out: *History and critique of the*] *concept of God [in the modern world-views]* ([*Geschichte und Kritik des*] *Gottesbegriff[es in den modernen Weltanschauungen]*) by Carl Sigmund Barach-Rappaport;<sup>13</sup>

At the new faculty in the next semester (SS 1870), Alexander continued to take a combination of medical and philosophical courses:

- *Anatomy of the sense organ[, the brain and the nervous system]* (*Anatomie der Sinnesorgane[, des Gehirns und des Nervensystems]*) by Joseph Hyrtl;
- *Anatomy of the vessel system* (*Anatomie des Gefäßsystems*) by Anton Friedlowsky (?–?);
- *Organic chemistry [Part II: Carbon-rich compounds]* (*Organische Chemie[, Theil (Kohlenstoffreichere Verbindungen)]*) by Ernst Ludwig (1842–1915; different name provided by Alexander);
- *Psychology* (*Psychologie*) by Robert Zimmermann.

Alexander remained at the medical faculty in WS 1870–1871 as well:

- *Advanced seminar* (*Phil[osophisches] Conservatorium*) by Robert Zimmermann;
- *Topographical anatomy [of the neck and the trunk]* (*Topographische Anatomie [des Halses und Rumpfes]*) by Joseph Hyrtl;
- *Comparative osteology [after the completion of human osteology]* (*Vergleichende Osteologie [nach Abschluss der menschlichen Knochenlehre]*) by Joseph Hyrtl;
- *Anatomy Training Course* (*Secierübungen*) by Joseph Hyrtl;
- *Pharmacology[, general therapy and the theory of prescriptions]* (*Pharmakologie[, allgemeine Therapie und Receptirkunde]*) by Carl Ritter von Schroff (1844–1892);
- *Pharmacognosy* (*Pharmakognosie*) by Carl Ritter von Schroff;
- unidentifiable;
- *Physiology und advanced anatomy* (*Physiologie und höhere Anatomie*) by Ernst Brücke (1819–1892);
- *[History of the Old] German literature* ([*Geschichte der älteren d]eutsche[n] Literatur*) by Wilhelm Scherer.<sup>14</sup>

<sup>13</sup> The less-known Carl Sigmund Barach-Rappaport (1834–1885) was appointed as a lecturer (*Privatdozent*) at the University of Vienna after years-long faculty infighting in 1861. In 1870, he transferred to Lamberg and, in the subsequent year, to Innsbruck where he became an ordinary professor (see also: Wieser 1950. 13 ff., 85). He was regarded by his contemporaries as a representative of “ethical idealism” (Eisler 1912. 46).

<sup>14</sup> See note 21 below.

The present author was unable to locate Alexander's file for SS 1871, though Alexander reported to have attended classes in a pattern similar to that of SS 1870 (cf. Alexander 1928. 21–22). Let us now turn to other primary sources which permit a glance beyond the surface of archival data!

## 2. Intellectual relationships

There are a series of synchronous and diachronous autobiographical accounts pertaining to Alexander's stay in Vienna.<sup>15</sup> Shortly after Alexander's death, the incomplete set of the letters Alexander wrote to his professor Cyrill (József) Horváth Sch. P. (1804–1884), has been published (see Alexander 1928), together with the separate publication of the letters sent to Horváth by József Bánóczy (Weisz; 1849–1926), Alexander's fellow traveler (see: Bánóczy 1928). Horváth, who had been ordinary professor of philosophy at the University of Budapest since 1863, gained considerably fame in the historiography of Hungarian philosophy as the creator of an eclectic-Hegelian philosophical synthesis, termed the system of "concretism [*concretismus*]"<sup>16</sup> which, however, is supposed to have never been devised, let alone presented by Horváth in its entirety.<sup>17</sup> In his contribution to Bánóczy's *Festschrift*, Alexander described Horváth, the classes of whom they attended during their preparatory university years in Budapest, as somebody who "became aware of us, supplied us with books, but we have not learnt any philosophy from him, except from his books he supplied to us" (Alex-

<sup>15</sup> Separately published recollection of the Göttingen phase of his academic peregrination: Alexander 1919b. 164 ff.

<sup>16</sup> Horváth 1868. 15 ff.

<sup>17</sup> This historiographical scheme is already present in the obituary of Horváth delivered by his colleague Imre Pauer (1845–1930), successor to Horváth's chair in Budapest, at the general session of the Academy on November 23, 1885: Horváth's promised *opus magnum* "was never finished", the system the book intended to present "is far from being completed, the basement of it might not yet be stable, and its details not formulated even in the mind of its master" (Pauer 1885. 17, 18). Pauer reported to have gone through Horváth's literary estate but "failed to find the finished system anywhere" (*loc. cit.*). Pauer's evaluation is shared by the modern research literature (see, e.g., Mészáros 2000. 181), though Béla Mester has recently argued that the "topos" of Horváth's work on a system of philosophy is "construction" resulting from "external, non-philosophical requirements" (Mester 2011. 83), namely the requirement of reproducing the perceived role of Western system-making philosopher within his local Hungarian cultural context (cf. 82). At the same time, Mester believes, this requirement was "deeply interiorized" by Horváth himself as well (83). Without wishing to discount Mester's legitimate concern for external conditions of philosophical production, I believe that, from a purely philosophical point of view (i.e., without committing oneself to deliberately psychological or sociological approaches), it is very hard to distinguish between supposedly genuine intention of philosophical system-making and the one supposedly resulting merely from interiorized external requirements. In the end, all what the historian of philosophy could do is to point towards Horváth's manifest own intention of producing a philosophical system (cf., e.g., note 16 above).

ander 1919a. 8). At one occasion, the sexagenarian professor reportedly accused his young students “of trying to steal my philosophical system from me”.<sup>18</sup> As if that were not enough, one might conjecture whether there have been a latent confessional tension between the Piarist priest Horváth and his young Jewish students (or whether one of the parties have ever imputed such intentions to the another), even though there is no clear sign indicating that such thing has ever actualized. In any case, Alexander’s retrospective account recalls the latent and manifest antisemitism they faced during other stages of their early intellectual biography.<sup>19</sup> Last but not least, the content of their letters is obviously determined by the conscious or subliminal genre constraints of letters written by young students believing themselves to be at the mercy of their academic benefactors. From this point of view, it is hard to overestimate that we are now in the possession of a synchronous and relatively direct autobiographical source pertaining to the Viennese section of Alexander’s academic peregrination, namely the publication of his diary entries written in Vienna (see below in this special issue, 209 ff.).

Taken together, these sources permit a glance beyond the surface of archival data. Alexander, e.g., was attracted by Poley’s originality and first-hand experience of Hegel, though simultaneously alienated by Poley’s lack of systematic rigorousness.<sup>20</sup> On the other hand, Alexander had fond memories of Lorenz von Stern’s lecture course on the philosophy of law (cf. Alexander 1928. 12). With regard to classes attended by Alexander, it is intriguing to ask why he dedicated such amount of his precarious time to studies of natural sciences. Éva Gábor, whose merits in Alexander scholarship cannot be overestimated, believed that this was motivated by Alexander’s own “recognition that the profound study of philosophy indispensably presupposes the scientific, anatomical-physiological knowledge of the human body” (Gábor 1986. 12); though, few sentences earlier Gábor hinted at medicine being a typical career path for offspring of (lower) middle-class families like Alexander (see 10). In a similar vein, the Jewish scholar Lajos Blau (1861–1936) who published the letters sent by Bánóczi to Horváth conjectured that the equally conspicuous amount of interest Bánóczi dedicated to studies of natural sciences and medicines are indicative of his intention of “becoming a physician” (Bánóczi 1928. 109), pointing at Bánóczi’s passing remark according to which the study of anatomy “paints the uncertain future in a more comforting color” (120; quoted by 109). In stark contrast to these mundane

<sup>18</sup> Alexander 1919a. 8; Alexander’s anecdote logically implies that there was something to be stolen in the first place; though it could be colored by the retrospective historiographical assumptions of Alexander’s age and hence cannot be taken as a direct evidence that Horváth actually believed himself to be in possession of an elaborated system worth misappropriating.

<sup>19</sup> See Alexander 1919a. 6 ff. This assessment was shared by Alexander’s contemporaries, see, e.g., Sebastyén 1934. 37–38.

<sup>20</sup> Alexander 1928. 12–13; hitherto not identified due to the fact that Alexander did not explicitly use the name of Poley.

existential reasons, Alexander's doctoral *curriculum vitae* from Leipzig, quoted at the beginning of the current section, makes the intriguing claim that it was Robert Zimmermann, ordinary professor of philosophy at the University of Vienna between 1861 and 1896, who directed the attention of his young student towards natural sciences, more specifically towards theoretical medicine. In fact, Zimmermann's role is supported by a closer reading of Bánóczy's letter's to Horváth in which the need to study natural sciences first surfaces upon the explicit recommendation of Zimmermann ("he recommended", 113). Who was this strange professor of philosophy who dared to direct the attention of his students *away from philosophy*? Before attempting to address this question, let us take a closer look at the aforementioned *existential aspect* of Alexander's stay in Vienna.

Already in his heavily stylized letters to his former university professor, Alexander made a passing remark on the seclusion he suffered in the university city of Göttingen: "one lacks social circle [*literally: szociális köre*]" that would "counterbalance" the "tedious studies" (Alexander 1928. 39). His private diary entries, however, reveal the true nature of Alexander's longing for »social circle«. As a young man away from home, he was far from leading an ascetic life: e.g., he co-organized a "carousal [*Trinkelage*]" to celebrate the publication of his first feature (*Feuilleton*) in the Viennese daily *Die Presse*.<sup>21</sup> Yet, Alexander's "social circle" went beyond the confines of usual pass-time social activities by young adults. Alexander's diary entries witness a curious kind of informal "fraternity [*Bruderschaft*]",<sup>22</sup> which exhibits traits both of the more formal student fraternities (*Corps, Burschenschaften* etc.) which were prevalent in nineteenth-century universities in the German-speaking cultural area, as well as of the less specific, though recurring behavioral patterns and intellectual inclinations that characterize young students of philosophy. When Franz Brentano's students, who would later establish the official *Philosophical Society at the University of Vienna (Philos-*

<sup>21</sup> See orig. p. 6. As mentioned in the corresponding footnotes by the editors, the *Die Presse*, once the flagship of Vienna's journalistic landscape (and predecessor of today's eponymous daily), had already been on the decline when its actual owner, Carl Dreger issued a journalistic *carte blanche* for the newcomer Alexander. By the way, it is indicative of the *Die Presse*'s situation – and it, to a certain extent, also deducts from the value of Dreger's offer – that in 1870 the bulk of the cultural sections (*Feuilleton*) of the daily issues was filled with endless instalments of the same novels (in case of the three-volume novel *La Pucelle* by the schoolteacher-turned-writer Karl Fren(t)zel [1827–1914], more than hundred instalments were published; biographical data based on *Deutsches Literatur-Lexikon*). On the other hand, even though the non-fiction content in the cultural section was unsurprisingly tilted towards literature, history, and music, Alexander was not alone in reporting on novelties of the philosophical literature. E.g., the Germanist Wilhelm Scherer (1841–1886), professor at the university between 1868 and 1872, who was incidentally Dilthey's friend and, precisely in 1870, lobbied heavily for Dilthey's appointment to Vienna (cf. Dilthey 2011. 544 ff., esp. 545, n. 1), published a review of the first (and only) volume of Dilthey's biography of Schleiermacher (Scherer 1870).

<sup>22</sup> Orig. p. 5. Cf. also: „das wir zu Ehren eines neuen Bruders veranstaltet hatten” (orig. p. 4), „der neue Bruder” (orig. p. 5).



*ophische Gesellschaft an der Universität zu Wien*) convened in the Café Kaiserhof<sup>23</sup> in WS 1887/87, they were, as they recalled two decades later, „arguing over the existence of the external world and, for this purposes, questioning the existence of the stone tables, causing astonishment on the faces of uninvited listeners”.<sup>24</sup> In a similar vein, it is fascinating to read Alexander’s description of their debating about the “big secrets of mankind” (orig. p. 4) and the futility of “increasing the number of the many question marks already possessed by human race” (*ibid.*). In contrast to the more formally-epistemologically oriented problems discussed by Brentano’s disciples in the late 1880s (probably not devoid of the vague influence of Neo-Kantianism), the burning issues for Alexander and his »brothers« were unapologetically metaphysical: “attaining truth” (orig. p. 4) and the “luring idea” of “immortality (orig. p. 3). They were also not afraid of dedicating their fraternity to overcoming individualism towards the advancement of humanity. As if that were not enough, Alexander’s description seems to attest an emotional, almost mystical facet of their experience, even though this facet is articulated in pantheistic terms (see esp. the description at the bottom of orig. p. 5). It is probably not without reason, that the young Alexander was reportedly entertaining the idea of becoming a rabbi (cf. Gábor 1986. 11) and he retained his distinctly Jewish identity throughout his entire life.<sup>25</sup>

The “fraternity” of Alexander and his fellow students might constitute a promising subject matter for comparative studies of cultural forms among nineteenth-century students of humanities in German-speaking area; but what could turn it into a piece of genuine history of philosophy is, of course, the ability of its professional articulation in terms of the historical tradition of philosophy, i.e., Alexander’s ability to connect his vague ideas to what he was taught at the university. In this regard, the diary preserved an explicit declaration of Alexander’s philosophical preferences that is worth being quoted in its entirety:

<sup>23</sup> This is probably not identical with the bar of today’s hotel, but rather located in the *Josefstädterstrasse*.

<sup>24</sup> Anonymous 1913. 3. According to the historiography of the Society, the crystallization process that transformed the debating circle into an officially organized structure was initiated by the external example of the Philosophical Society at the University of Berlin, see: Meister 1938. 4.

<sup>25</sup> In one of his juvenilia published during his last Viennese winter term, Alexander revealed strong sympathy for regarding religion as a merely cultural phenomenon: “In religions, all old is new and all new is old, and all progress consists in a combination of basic elements. Everywhere, the chords of the soul are struck in a way that the resulting tone is religion, because the chords – the human soul – are of the same composition, because the sublime phenomena of nature, which elicit that tones from these chords, are essentially of the same nature everywhere.” (A[lexande]r 1871a. 9.) The reviewed author was Max Müller (1823–1900), an Indologist who, besides making a stellar career in Oxford academia, was a successful popularizer of his science not shy of drawing wide-ranging, and often far-fetched, conclusions on the basis of comparative linguistics and history of religions.

Therefore, I am not a Hegelian in metaphysics, since I feel that it is not the path on which the common sense should wander, if it is intent on providing viable results. I hope to arrive at a natural world view that is equally distant from a flat ethical and scientific naturalism, as well as from a blurred idealism. Being a child of my time, I am a realist and compelled towards the views of a Herbart and a Lotze. As to how I would diverge from them, I do not know yet; but I think I will do [so].<sup>26</sup>

The part of Alexander's above declaration which is, *nolens volens*, the most authentic is probably his confession of being under the influence of the philosophical *Zeitgeist*. This, of course, leaves open the question as to which extent the latter, culminating in Alexander's avowal of "being a realist" corresponds to his latent philosophical convictions and preferences expressed in other parts of his diary analyzed above. In order to resolve this tension, one has to take into account Alexander's professional philosophical activities in Vienna – which would bring us back to Zimmermann.

### 3. *Writing projects*

During his period spent in the Imperial city, Alexander pursued a cluster of philosophical writing projects. Already the sources published in 1928 univocally testify that Alexander's involvement in philosophy originated in Zimmermann's advanced seminar (*Philosophisches Conversatorium*), which, as indicated by our data in Section II. 1, Alexander consistently visited in each semester of his stay in Vienna. Even in the earliest report, the first one of Alexander's preserved letters to Horváth which was written shortly after the start of Alexander's second semester in Vienna, he characterized Zimmermann as "my teacher in philosophy" (Alexander 1928. 9), and in the diary entry from January 16, 1870 (i.e., almost one year later), Zimmermann is referred to as "my professor" (orig. p. 6), not to mention Zimmermann's description in the doctoral *curriculum vitae* as "dear friend and supporter of my studies." In contrast to Brentano, whose advanced seminars, regularly announced under the title *Together with the students: Reading, commentary and critical review of selected philosophical writings (In Gemeinschaft mit den Studierenden: Lesung, Erklärung und kritische Besprechung ausgewählter philosophischen Schriften)*,<sup>27</sup> was dedicated to one book per semester (often opting for classical authors of the history of philosophy); Zimmermann let each student present separate papers during his seminar sessions on a philosophical novelty. Alexander, as well as his companion Bánóczy excelled in this genre: Already in

<sup>26</sup> Orig. p. 2.

<sup>27</sup> For the most complete list of the classes announced by Brentano, see: Antonelli 2001, 496 ff.



the first semester, Zimmermann “approved” Alexander’s paper and invited him to a discussion in his flat (Alexander 1928. 11). Zimmermann “became very fond of us”, Alexander recalled, “and assigned us the most important papers” (Alexander 1919a. 10). “He told me several flattering things, I returned from his flat beaming with delight, in elevated spirits”, Alexander recorded in his diary with regard to a publication that originated from his presentations on Zimmermann’s seminars (orig. p. 6).

It is precisely Alexander’s writing projects originating from these seminar assignments which render Alexander’s student relationship to Zimmermann especially worth our attention. A detailed look permits us to reconstruct two major distinct writing projects which are outlined below. In none of them did Alexander’s main unpublished manuscript survive, but some of them Alexander managed to publish short pieces of writings representing the tips of the icebergs of his researches. These icebergs are surrounded by the floating debris of Alexander’s minor occasional articles in the *Die Presse* (often only in its local edition, the *Local-Anzeiger der “Presse”*), published pseudonymously under various monograms and hence hard to identify in unambiguous manner.<sup>28</sup>

(1) Alexander’s first distinct writing project originated in the first semester from his assignment to review a booklet written by Heinrich Adolf Rinne (1819–1868), an inconspicuous German physicist who died a premature death in the summer of the very year of the publication of his pamphlet.<sup>29</sup> Rinne’s work indeed lacked any reference to names of philosophers, let alone their works; and Alexander claimed to have demonstrated its dependence on Hermann Lotze’s early book, the *Medical Psychology* (Lotze 1852; see Alexander 1928. 10–11). Alexander was not alone in raising such concerns: The anonymous editorial reviewer of Rinne’s booklet in the influential *Literarisches Centralblatt für Deutschland* clas-

<sup>28</sup> The monogram „B. A–er.” could definitely be assigned to Alexander, as it is used for the article he explicitly mentioned in his diary (see note 21 above) and it seems specific enough. Probably the same applies to the monogram “B. A.”. On the other hand, the present author is reluctant as to whether articles signed simply as “B.” could unambiguously be ascribed to Alexander (especially articles which are not specifically philosophical), even though he demonstrably employed this monogram in case of an article he explicitly mentioned as his own in his letters ([Alexander] 1870; cf. Alexander 1928. 21; for a probably different author using the same monogram, see the issue of January 27, 1870, p. 13). Barring any supplemental information, anonymous reviews cannot be ascribed to Alexander, even if they content would suggest Alexander’s authorship. Furthermore, Alexander probably continued contributing to the *Die Presse* even after he left Vienna (cf. A[lexander?] 1871).

<sup>29</sup> Rinne 1868. In his letters, Alexander misreported its title as *On the Significance of Materialism in Psychological and Ethical Regard (Über die Bedeutung des Materialismus in psychologischer und ethischer Beziehung*; see Alexander 1928. 10), which was reproduced by Gábor 1986. 13–14 and Zóka 2012. 54, without providing any bibliographical reference to it. Heinrich Adolf Rinne studied in Munich and Göttingen and obtained his doctoral degree at the latter university. Subsequently, he embarked on a professional medical career and died in 1868, shortly after being appointed at a mental hospital in Hildesheim (biographical data based on his entry in *Biographisches Lexikon der hervorragenden Ärzte und aller Zeiten und Völker*).

sified the first three sections as being on the verge of “plagiarism” (Anonymous 1869). This issue of the *Literarisches Centralblatt* was published on April 17, 1869; i.e., two days before Alexander’s letter to Horváth in which Alexander reported his discovery to his former teacher. It is, thus, far from being surprising that Alexander’s acrimonious running commentary, which was allegedly 40 pages long (see Alexander 1928. 11), remained unpublished.<sup>30</sup> It is worth mentioning, though, that this preoccupation made him aware of Friedrich Albert Lange’s book on materialism which is a seminal, yet still underestimated contribution to the genesis of Neo-Kantianism (besides being named by Brentano as the forerunner of his own idea of psychology, cf. Brentano 1874a. 13).<sup>31</sup> The fruits of Alexander’s study of Lange’s *opus magnum* are already manifested in a short newspaper review article (A[lexande]r 1871c) he published in March 1871 about Ludwig Büchner (1824–1899) who had been one of the protagonists during the controversy on materialism that raged in the 1850s and, precisely through the voluminous pamphlet reviewed by Alexander (Büchner 1869), had just ignited another controversy centered around the philosophical implications of Darwinism that would preoccupy philosophers and philosophically-inclined scientists in the 1860s and 1870s. Alexander adopted from Lange the main thrust of the argumentation by virtue of which early Neo-Kantian professional philosophers attempted the recapture the ground they had been forced to cede to natural scientists after the demise of Hegelianism and other absolute systems of philosophy. Alexander instantiated this argumentative strategy claiming that, “[t]he materialism, which, at the beginning, had been a reaction against philosophy, wanted to become a philosophy” (A[lexande]r 1871c. 14). In trying to do so, however, scientific materialism “regularly becomes shipwrecked” (*ibid.*), he claimed. In other words, the new wave of philosophers not only attempted to legitimate their endeavor by pointing out that their scientific critics are also engaged in doing philosophy, but their true strategy was to lure their enemy into the foreign terrain of philosophy where professional philosophers would prevail. Alexander, too, claimed by relying on Lange that Büchner is “entangled in the bonds of the old *Naturphilosophie* that is so much ridiculed by him”.<sup>32</sup>

<sup>30</sup> It must be said in Alexander’s favor that the long benevolent review of Rinne’s booklet by Heinrich Ritter (1791-1869), the aging historian of philosophy, in the renowned *Göttingische Gelehrte Anzeigen* on August 5 of the previous year did not mention Rinne’s indebtedness to Lotze (besides suggesting the presence of a broadly-speaking Herbartian framework; cf. Ritter 1868. 1242).

<sup>31</sup> At that time, only the first, single-volume and less-discussed edition was available: Lange 1866. On the significance of Lange for the genesis of Neo-Kantianism, see Klaus Christian Köhnke’s seminal investigations (1986. 233 ff.).

<sup>32</sup> A[lexande]r 1871c. 14. The passages Alexander had in mind were probably Lange’s claim that Büchner is under the spell of the “after-effect [*Nachwirkung*] of the Schellingian-Hegelian philosophy” and occasionally even falls into “vague pantheism” (Lange 1866. 304). In accordance with the general line of his argumentation, Lange also attempted to prove that Büchner “propounds a completely new concept of philosophy without, however, exactly

Hence, the philosophical standpoint manifested by the young Alexander in Vienna might, from a level general enough, be described as being committed to scientific ideals and, thus, subsumed under the historiographical concept of Austrian Philosophy. What such historiographical classification overlooks, however, is specifically the early Neo-Kantian approach to sciences, as exemplified by the young Alexander's critique of Büchner. What else does the case study of Alexander's juvenilia teach us about Austrian philosophy as it was actually practiced on the eve of Brentano's arrival that inaugurated it in the historiographical sense?

(2) It is only the present critical edition of the Viennese entries from Alexander's diary which reveals Alexander's second distinct writing project (hitherto apparently conflated with the previous one),<sup>33</sup> namely the presentation and critique of a philosophical-cultural sensation *du jour*, receded into oblivion since then (even the author's name was overshadowed by an eponymous twentieth-century philosopher): Eduard von Hartmann's (1842–1906) *Philosophy of the Unconscious*, which was first published in November 1868 (Hartmann 1869) and went through eight editions within a decade. Already in February 1869, the Leipzig-based cultural weekly celebrated Hartmann's book as one of the „outstanding publications of recent philosophical literature” (Gottschall 1869. 113), and the book was reviewed at great length in the leading professional philosophical journal already during the second half of the same year (see: Reichlin-Meldegg 1869). Alexander himself prepared a paper that consisted of 53 densely written pages and presented it spanning three sessions of Zimmermann's advanced seminar in November 1869 (see: Alexander 1928. 13). Alexander also reported that Zimmermann recommended him to submit his paper to the *Zeitschrift für exacte Philosophie* (*ibid.*; full title of the journal: *Zeitschrift für exacte Philosophie im Sinne des neuern philosophischen Realismus*). This passage in Alexander's letter has hitherto been taken at its face value, without looking into the issue as to whether Hartmann's bestseller was actually reviewed in that journal or not. This question can be answered in affirmative mode; however, the author of the review was, unfortunately, not Alexander but rather Friedrich Bartolomäi,<sup>34</sup> a lesser-known German Herbartian pedagogue who belonged to the

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defining it” (298), because Büchner insisted on philosophy being generally intelligible and accessible to lay audience; though this argumentation of Lange verges on being a sophism.

<sup>33</sup> Cf. Gábor 1986. 14–15; Zóka 2012. 55; I was unable to locate any writing of Alexander published in the *Neue Freie Presse*, as claimed by Zóka. Szemere, the editor of Alexander's letters to Horváth, was careful enough not to try to guess what Alexander's writing project, described at lengths by Alexander in the letters without exactly telling its subject matter, was about (cf. Alexander 1928. 5).

<sup>34</sup> Friedrich Bartolomäi (1817–1878) studied mathematics and philosophy in Jena (since 1840), where he joined the Herbartian pedagogical circles and worked as a teacher at the flagship teacher training school. From 1866 he was employed as a statistician in Berlin (mental health problems since 1877). Biographical data from *Lebensskizzen ausgewählter Herbartianer* of the *Arbeitsstelle für Internationale Herbartianismusforschung* at the University of Düsseldorf,

inner circle of the self-avowedly Herbartian journal (Bartholomäi 1871). The review was published in issue no. 3 of vol. 9, which spanned 1869–1871, so its submission probably coincided with the incubation of Alexander’s own plans. In March 1870, Alexander reported to have submitted his paper “to the journal of the Herbartians”, even though the “journal appears to be discontinued, as only one issue was published in the previous year and no issue so far in the current year” (Alexander 1928. 17). It must have been a bitter disappointment for Alexander to find out that his juvenilia collided with a review originating from the inner circle of the journal.

In this case, however, Alexander managed to publish a condensed version of his paper as a feature article spanning the bottom parts of four pages in the cultural section (*Feuilleton*) of the declining Viennese daily *Die Presse*.<sup>35</sup> His diary provides the useful information that this article was definitely the first one he published in the cultural section of German-language newspapers (cf. orig p. 6). Collated with his letters to Horváth, it also becomes clear that he sent the printed text to his Hungarian mentor who benevolently compared it to the aforementioned review<sup>36</sup> that was published in the mainstream professional *Zeitschrift für Philosophie und philosophische Kritik* (cf. Alexander 1928. 17), and that Alexander was entertaining the idea of publishing a Hungarian version of the article.<sup>37</sup>

Unlike the lost longer manuscript which reportedly “sharply criticize[d] the inconsistencies and falsehoods” in Hartmann’s book (Alexander 1928. 13) and attempted to demonstrate that Hartmann’s “inductive method fails to attain the results on which he based his system” (17); Alexander’s published feature article is self-avowedly descriptive (e.g., he does not want to “impose a judgement on the reader”; A[lexande]r 1870. 2). At the same, time there is, to a certain extent, a *harmonia praestabilita* between Alexander’s more general remarks in the article and the views expounded by Brentano when he ascended to the lectern two years later (see: Brentano 1874b). Both were situated in what Frederick C. Beiser has recently so aptly termed as the post-Hegelian “obsolescence crisis”<sup>38</sup> of philosophy – even though the young Alexander was more sympathetic to the idea that

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Germany (<https://www.uni-due.de/herbartianismus-forschungsstelle/Skizze.shtml>; last visited: 10-12-2018).

<sup>35</sup> A[lexande]r 1870. First identified by: Varga 2016c. 261.

<sup>36</sup> Reichlin-Meldegg 1869; hitherto not identified.

<sup>37</sup> See: Alexander 1928. 17. I was unable to locate a possible realization of this plan by Alexander.

<sup>38</sup> Beiser 2014. 49 and passim. As I have argued elsewhere (see esp.: Varga 2016a), Early Phenomenology (i.e., the School of Brentano, Edmund Husserl and his Munich, Göttingen, and, to a certain extent, early Freiburg students and collaborators) constitute the blind spot of Beiser’s investigations, even though his framework, namely the rehabilitation of German theoretical academic philosophy (*Universitätsphilosophie*) of the post-Hegelian decades during the second half of the so-called long nineteenth-century as a legitimate object of scholarly history of philosophy (which, under a different label, was actually pioneered by Köhnke, see Köhnke 1986), is apparently promising for contextualized understanding of Early Phenome-

Hegel had been “dead long enough” to be regarded as “classical writer of philosophy” (A[lexander] 1870. 13) who is no more able to “engender new enthusiasm” (A[lexande]r 1871b. 14); while Brentano acrimoniously recalled Jakob Friedrich Fries’ (1773–1843) remark that Hegel’s philosophy would “belong to the history of policing the schools [*Schulpolizei*], rather than the history of philosophy” (Brentano 1987. 67; cf., e.g., Fries 1840. 671). Disregarding the differences in their magnanimity towards their historical predecessors, both regarded the current crisis of philosophy as a chance, rather than a malady. In the concluding part of his inaugural address, Brentano called the “rich burgeoning” of “natural science and its subspecies” the precondition for the arrival of “springtime for philosophy” (Brentano 1874b. 20). Alexander, too, declared that “[w]ithout accurate knowledge of natural sciences one could hardly dare to engage in speculations” (A[lexande]r 1870. 1), and he praised Hartmann for attempting to ground his “system” on an “empirically [...] unshakeable basis” (2). Vice versa, he chastised Hegel for failing to account for the source of “the creative efficacy of his ideas” which are “nothing else than pure concepts” (1). In sum, one is really compelled to construct a counterfactual narrative (as mentioned in Section I above): Had Alexander remained at the University of Vienna, he might have found his new philosophical hero in Brentano who ascended to the lectern in Spring 1874. Instead, Alexander wrote to Horváth at the end of SS 1871 that, despite his strong personal and professional ties to Zimmermann, he is bored by the Viennese menu in philosophy and his ship is bound to sail to Berlin (see: Alexander 1928. 21–22). There is, however, a factual (rather than a counterfactual) hero of Alexander’s Vienna period, namely his actual teacher Zimmermann. Even the plethora of Alexander’s occasional minor writings testify to Zimmermann’s influence – and to the influence of the ideas transmitted by him, most notably the philosophy of Herbart. E.g., Alexander’s relatively mature review of Zimmermann’s treatise on Samuel Clarke concludes in highlighting Clarke’s significance for the development of the ideas of Herbart (cf. A[lexande]r 1871d. 15), even though the original text of Zimmermann’s academy lecture – delivered on January 19, 1870, i.e., during the heydays of Alexander’s admiration for Zimmermann – barely announced Herbart’s name (cf. Zimmermann 1870b. 360) and even its expanded version published subsequently only mentions Herbart within the general context of overcoming the one-sidedly emotive grounding of ethics and aesthetics.<sup>39</sup> Hence, in the last section of the present paper I am going to look into the intellectual fruits of Alexander’s stay in Vienna, respectively what they could tell us about Austrian philosophy itself (regardless of the capitalization of ‘p’).

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nology. The present paper, together with other works by the present author, intends to serve as a small contribution towards realizing this approach.

<sup>39</sup> Zimmermann 1870a; the existence of this separate, expanded version of Zimmermann’s talk, published in the *Denkschriften* of the Academy, is often overlooked.

### III. ALEXANDER AND THE “ZIMMERMANN RIDDLE”

#### 1. *The contested relevance of Alexander’s philosophical master in Vienna*

The relevance of the young Alexander’s Viennese period for the historiography of philosophy is, to a certain extent, rooted in nothing else than his relationship to Robert Zimmermann. Zimmermann, not unlike Alexander, was a productive author but an elusive thinker. Since the vast body of Zimmermann’s writings is mostly dedicated to aesthetics (respectively, consists of occasional pieces of writing), the true extent of his philosophical views, which he must have expounded to his students in his lecturing activity that spanned almost all major historical periods and disciples of philosophy (cf. Wieser 1950. 78–83), remains in the shadow, especially during the later decades of his professorship. Alexander recalled that he and his travel companion Bánóczy “had been the only ones amongst [Zimmermann’s] students who made philosophy the main course of study of their lives” (Alexander 1919a. 10). This claim is historically untrue, as Zimmermann, who had been an ordinary professor of philosophy at the University of Vienna (the only one after Brentano’s demotion to the rank of *Privatdozent* in 1880),<sup>40</sup> oversaw entire generations of Brentano’s Viennese students, including, most notably, Edmund Husserl, who studied in Vienna in SS 1881 – WS 1881–1882 and WS 1884 – SS 1886 (see Schuhmann 1977. 9–17; full course list: Varga 2015. 99–101), not to mention the fact that one of the examiners during the philosophical part of Husserl’s mathematical doctoral examination was Zimmermann himself (see already: Schuhmann 1977. 11). Alexander’s claim is, however, historically true in the sense that he (and Bánóczy) had been the only famous philosophy students of Zimmermann whose *loyalty to their master was undivided*. It is, namely, not by chance that, decades later, Alexander and his audience were unaware of the fact that Husserl, who, by then, had already reached the zenith of his philosophical influence, was a counterexample to Alexander’s claim of being the only student of Zimmermann who made a career in philosophy. Quite the contrary, as I have argued elsewhere (see Varga 2018. 108 ff.), it was Husserl’s own deliberate decision to reduce his philosophical lineage to Brentano alone (which was cemented precisely around this time by Husserl’s hagiographical account of his study at Brentano in Vienna, completely ignoring the classes he took under Zimmermann; see Husserl 1919, 1989. 304–315). During his Halle period in 1896, in contrast, Husserl still mentioned in a letter *both* Brentano and Zimmermann as his most influential teachers in philosophy (see Purkert and Ilgauds 1987. 206).

<sup>40</sup> Concerning the historical circumstances of Brentano’s demotion from the point of view of the development of early phenomenology (based on a combination of printed and unpublished sources), see Varga 2014. 86 ff.



What is at stake philosophically is whether Zimmermann had been in the position to transmit ideas from the outside to Husserl and other fellow disciples of Brentano in Vienna. Furthermore, what renders this question especially pertinent is Zimmermann's exposure to the philosophy of Bernard Bolzano (1781–1848), the polymath thinker from Prague whom Husserl believed to have rediscovered (see, e.g., Husserl 1900. 224–227; 1975. 226–229) and who indeed anticipated many fundamental tenets of early phenomenology (as well as of contemporary analytical philosophy and logic). After all, both Robert Zimmermann and his father Johann August Zimmerman (1793–1869) had been member of the inner circle of the disciples of Bolzano, who explicitly called Zimmermann junior his “beloved son [*Herzensjunge*]” (Bolzano 2006. 229) and put “great trust” in him (Bolzano 2005. 521), namely in Robert Zimmermann's capacity as “an effective tool for the propagation of our ideas” (47). Indeed, the young Zimmermann verifiably advertised Bolzano's ideas when historical circumstances permitted (see, e.g., Zimmermann 1849). Especially Eduard Winter (1896–1982), the pioneering Bolzano scholar who had personal ties to the late phase of the Prague wing of the School of Brentano, was an ardent supporter of Zimmermann's role as a transmitter of Bolzano's ideas (already: Winter 1933. 252). In the face of the mounting evidence of the mature Zimmermann's public silence on Bolzano, Winter later developed the irrefutable hypothesis that Zimmermann conspiratorially denied Bolzano even if he remained attached to Bolzano's ideas at the bottom of his heart.<sup>41</sup>

In the specific context of the historiography of early phenomenology, it has been alleged that Zimmermann as a professor of philosophy in Vienna “was in a position to promote Bolzanian doctrines at least to some extent” (Rollinger 1999. 69); while Zimmermann's role as a transmitter has recently come under heavy criticism from the point of view of the general historiography of Austrian philosophy (see, e.g., Morscher 1997). The present author has argued (Varga 2016c) that Husserl had indeed been significantly exposed to Zimmermann, whose classes he regularly attended in *Vienna* in 1884–1886 (though not through the philosophy textbook he used in the secondary school). At the same time, the doctrines Zimmermann verifiably transmitted to Husserl were not of Bolzano, but rather general Herbartian ideas (which are relevant on their own right to the development of Husserl's specifically phenomenological idea of intentionality). From a philosophical standpoint, the crux of the issue is whether the concept (*Begriff*) is defined in a logical way that clearly anticipates the phenomenological idea of intentional content (as exemplified by the original, Bolzanoian edition of Zimmermann's logic textbook, see esp. Winter 1975. 41–42, 45) or rather by virtue of a pictorial theory of representation, e.g., as a “certain *picture* of a tree

<sup>41</sup> See Winter 1993. 34; it must be said in Winter's favor that Bolzano and his disciples indeed employed conspiratorial method to counter censorship and other repressive measures.

[*gewisses Bild eines Baumes*]” that is distilled from the series of the corresponding experiences, as Zimmermann wrote in the more widespread, Herbartian version of his logic textbook (already: Zimmermann 1860. 17; emphasis in original). Is Alexander’s acquaintance of Zimmermann, reconstructed in the above section of the present paper, is of any avail when reconstructing the influence Alexander’s philosophical master might have exerted a decade later on his students who would originate phenomenology?

## *2. Alexander’s juvenilia as a baseline of Austrian philosophy prior to Austrian Philosophy?*

The possibility of using Alexander’s Viennese period as a baseline for reconstructing philosophy, more specifically philosophical logic, as it was practiced in Austria prior to the birth of Austrian Philosophy – in other words, the possibility of obtaining an unfiltered picture of Zimmermann’s thinking that, in case of the 1880s, was mixed with that of Brentano – hinges on reconstructing the entirety of the corpus that is presented by Alexander’s juvenilia. His longer manuscripts reconstructed above, if we were in their possession, might provide detailed information on the logical doctrines Alexander acquired during his philosophical apprenticeship in Vienna, but his feature articles are apparently not specific enough. Is it possible to look beyond the surface of these occasional writings? It is less known that there exists a partial print (Alexander 1876) of Alexander’s original dissertation by virtue of which he obtained his doctoral degree at the University of Leipzig in August 1873, and it is even lesser known that the original dissertation itself has been preserved in the Hungarian Jewish Archives.<sup>42</sup> Unlike the dissertation, which discusses the contemporary critique and defense of Kant’s transcendental deduction of the categories in the *Critique of Pure Reason*, the original manuscript is simultaneously more broadly conceived and closer to Kant’s letter: it presents Kant’s general intellectual biography with a special focus on pre-critical works (furthermore, the partial print does not exactly correspond to any of the seven chapters of the original dissertation, which, in turns, differs from Alexander’s mature work on Kant [Alexander 1881]). The original dissertation manuscript unmistakably attests Alexander’s adherence to the Herbartian philosophy, as the opening sentence of its introduction is a direct quotation from Herbart on the long-lasting significance of Kant for the contemporary condition of German philosophy (Herbart 1850. 513, lines 5–10). Yet, this

<sup>42</sup> Ms. HJA, XIX-113. I am grateful to Péter Turbucz for directing my attention to this item. It contains both the handwritten fair copy of Alexander’s dissertation (with a few marginalia, presumably from the reviewers of the dissertation) and Alexander’s so-called half-way (*halbbrüchig*) manuscript of the former. I am not aware of any extensive discussion in the scholarly literature of the partial print of Alexander’s dissertation, let alone of its original manuscript.



source is, again, not suitable for providing a glimpse into Zimmermann's ideas. Alexander's dissertation exemplifies interesting early strata of Neo-Kantianism precisely before its institutional crystallization (see, e.g., Pollok 2010); yet, by the same token, Alexander's loyalty is here divided between what he might have learnt from Zimmermann and what he acquired during the later stages of his academic peregrination.

There is, however, another source pertaining to Alexander's Vienna period. "I must write anthropology as a prize essay for the Hungarian academy", he recorded in his diary on January 16, 1870 (orig. p. 6). The prize competition was announced during the General Assembly of the Hungarian Academy of Sciences in April 1869 (see the corresponding editorial footnote); but it was declared unsuccessful during the General Assembly in May 1871.<sup>43</sup> According to the rules, the applicants must have submitted code-named fair copies handwritten by somebody else and, in case the prize competition was deemed unsuccessful, the accompanying envelopes containing the authors' data must be destroyed without opening them. This is what happened in 1871, so it seems impossible to tell whether Alexander realized the plan he recorded in his diary entry and, if so, which one of the two submitted prize essay manuscripts was written by him. Fortunately, in a letter written to Horváth in October 1871, Alexander confessed that he indeed submitted a prize essay during the last year and, furthermore, his essay was the second one (see Alexander 1928. 24). The manuscript itself – as far as I know, hitherto unidentified – is preserved in the archives of the Hungarian Academy of Sciences (Ms. MTAK). Alexander's voluminous prize essay, arrived on September 30, 1870 and inventoried under the number 913/870 (volume XXXIX), provides a comprehensive survey of many fields of philosophy relevant to anthropology, including, fortunately, a specific discussion of philosophical logic and psychology. Already Alexander point of departure is unmistakably Herbartian (see 162 ff): our presentations (*képzetek*) of human are different (e.g., the presentation of an infant); how is it then possible to attain a concept (*fogalom*) which "encompasses [*át fogl*]" several presentations (163)? According to the renowned tradition of philosophical logic, Alexander clearly distinguishes between content (*tartalom*) and extension (*kör*) of a concept (see 164), but there are at least three specific and compelling aspect of his theory: He also takes account of the name (*név*); (2) he provides a Herbartian account of the fusion of the particular presentations like "the rays of a fan" (162); and, what especially anticipates the so-called genetic phenomenology that would be devised by another student of Zimmermann a few decades later, he raises the issue of how the genetically fused perceptual elements constitute a "network [*háló*]" (166) that make subsequent generalizing object recognition possible in the first place (this

<sup>43</sup> See: *A Magyar Tudományos Akadémia 1871. május 20-án tartott ünnepélyes közgyűlésének tárgyai* (*A Magyar Tudományos Akadémia Évkönyvei* XIII/VII). 1871. 73.

is why, Alexander says, a small child recognizes a dog even on a picture and, if confronted with a wolf, she would recognize it as a dog).

According to the evaluation report, Alexander's prize essay was "richer" than his competitor, but lacking an "independent, coherent, organized unity." It was deemed rather like a "collection of material [*anyaggyűjtemény*]," on the basis of which somebody could "write an essay worthy of the prize."<sup>44</sup> Indeed the text looks like a collection of dense summaries, lacking specific bibliographical references. At the beginning of his work, Alexander listed a series of authors he relied upon (including, e.g., Lotze, Lange, Hartmann, Voigt, I. H. Fichte; but notably missing Zimmermann himself). Given that Alexander's prize essay indeed represents an unfiltered, synchronous, and detailed imprint of the philosophical ideas he acquired in Vienna under the guidance of Zimmermann, I think it warrants a closer and more extensive study in the form of a critical edition that would identify the sources of the individual elements which coagulated in the prize essay (maybe even a partial translation of the text). The young Viennese student Alexander, after all, did not leave us with empty hands.

Rudolf Haller explicitly claimed that "the beginning of Austrian Philosophy could be identified with" Brentano's Vienna period, thus, "in a slightly exaggerated form", the year 1874 was "the year of birth of Austrian Philosophy" (Haller 1986. 36). There is a considerable discrepancy – both in terms of the *dramatis personae*, their writings, and the involved philosophical doctrines – between the content of Haller's Austrian Philosophy (with capital 'P') and what was reconstructed above based on the snapshot provided by the Viennese section of Alexander's academic peregrination. At the same time, the latter not only temporally preceded the former, but it must also be regarded as its precondition, though obviously not the single one, both historically and philosophically. Furthermore, I think it is possible to draw a more general lesson from these investigations that transcends the narrow confines of the history of late nineteenth-century philosophy in Vienna: The discrepancy between Austrian Philosophy (with capital 'P') and what seems to have been depicted on Alexander's snapshot could be said to epitomize the methodologically relevant difference between a historiographical construct and the actual richness and *materiality* of the history of philosophy (notwithstanding the necessity of devising such constructs). If Austrian Philosophy was born in 1874, then Alexander, his teachers, and his fellow philosophers might represent the philosophical equivalent of a premature birth – but they have equal right to live and capture our scholarly attention.

<sup>44</sup> *A Magyar Tudományos Akadémia 1871. május 20-án tartott ünnepélyes közgyűlésének tárgyai*. 73.

## UNPUBLISHED SOURCES

- Ms. HJA *Hungarian Jewish Archives* (Budapest, Hungary)  
 Ms. MTAK *Library and Information Centre of the Hungarian Academy of Sciences*  
 (Budapest, Hungary)  
 Ms. UA Leipzig *Universitätsarchiv Leipzig* (Leipzig, Germany)  
 Ms. UA Wien *Universitätsarchiv Wien* (Vienna, Austria)

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# Die Wiener Einträge 1869–1870 aus dem Tagebuch des ungarischen Philosophen Bernhard Alexander

HERAUSGEGEBEN VON BARNABÁS SZEKÉR, BETTINA SZABADOS  
UND PÉTER ANDRÁS VARGA

## EINLEITUNG DER HERAUSGEBER

Die nachfolgende Erstveröffentlichung gibt die 1869–1870 in Wien entstandenen Tagebucheinträge des jungen Bernhard (Bernát) Alexander (geb. 1850 in Budapest, gest. ebenda 1927) wieder. Zusammen mit seinem Reisekompagnon József Bánóczy (geb. Weisz, 1849–1926) studierte Alexander zwischen WS 1868–1869 und WS 1870–1871 in Wien, wo er Philosophie (u.a. bei Robert Zimmermann) und Medizin hörte. Während der weiteren Strecken seiner akademischen Peregrination, die ihn nach Berlin, Göttingen und Leipzig führten (er promovierte in Leipzig im August 1873),<sup>1</sup> hat sich Alexander dem Neukantianismus angeschlossen, den er als Mitübersetzer von Kants *Kritik der reinen Vernunft* und als einflussreicher Dozent in Budapest (ordentlicher Professor seit 1904) Generationen von ungarischen Philosophen vermittelte. Die frühen Einträge seines erst in Wien angefangenen und im Laufe seiner Peregrination unsystematisch weitergeführten Tagebuches bieten jedoch eine synchrone und relativ unvermittelte Aktorperspektive auf die noch vor-kantische Frühphase seiner geistigen Entwicklung, sowie auf das Studentenleben des späten 19ten Jahrhunderts. Darüber hinaus erhebt der Text offenbar auch literarische und philosophische Ansprüche.

Obwohl schon Alexander selbst autobiographische Beiträge zu seiner Peregrination verfasste<sup>2</sup> und kurz nach seinem Tod die Jugendbriefe von Alexander an seinen Budapester Philosophielehrer, den spätdidealistischen Eklektiker

<sup>1</sup> Die Dissertation wurde in seiner Heimatstadt gedruckt und gelang wohl deshalb nicht in den Kreislauf der internationalen Kant-Forschung: Alexander, Bernhard 1876. *Kant's Lehre vom Erkennen*. Budapest, Ph. Wodianer.

<sup>2</sup> Siehe z. B. Alexander Bernát 1919. Bánóczy József. In *Emlékkönyv Bánóczy József születése hetvenedik évfordulójára*. Budapest, Franklin-Társulat. 1–38, 9 ff.

Cyrrill Horváth SP (1804–1884), herausgegeben wurden,<sup>3</sup> blieb das Tagebuch unveröffentlicht und sein Inhalt ist nur aus sporadischen Referaten in der Alexander-Kurzmonographie von Éva Gábor bekannt.<sup>4</sup>

Auf dem inneren Titelblatt des Heftes befindet sich ein Vermerk von Alexander, der der Schriftart nach den Pariser Einträgen zuzuordnen ist: “Im Falle mir ein Unglücksfall zutritt, so sollen diese Blätter ungelesen verbrannt werden. Diese Bitte müssen meine Freunde erfüllen, und sie werden es auch thun.” Eine ähnlich lautende Aufforderung wurde in dem längeren und auf Ungarisch verfassten Vermächtnis ausgesprochen, das sich auf der vorherigen Titelseite befindet und explizit auf 18. VI. 1875 datiert ist. Keine dieser beiden Wünschen wurden von Alexanders Freunden und Familienmitgliedern erfüllt.

Zu dieser Edition wurden die Seiten 1–9 (nachträgliche Nummerierung) von den Herausgebern aus dem Original transkribiert, kollationiert und mit Anmerkungen versehen. Die Wiener Tagebucheinträge und die Einträge bis S. 56 verfasste Alexander in Kurrentschrift auf Deutsch. Die Wiener und die darauffolgenden Berliner Einträge unterscheiden sich erheblich in ihren Schriftbildern. Die Wiener Schriftweise stellt wegen des verwendeten Schreibmaterials besondere Herausforderungen dar. Der Rest des Heftes ist auf Ungarisch in moderner Schrift geschrieben. Der Gesamtumfang des Heftes beträgt 257 Seiten (ausschließlich der geschriebenen Titelblätter), von dem es nur 69 Textseiten gibt. Auf dem Titelblatt befindet sich neben den oben erwähnten Testamenten die Titelangabe “Tagebuch”, sowie Angaben zu seinem Namen und der Adresse unter welchem Alexander spätestens seit dem Anfang seines zweiten Wiener Semesters wohnte: “Bernhard Alexander. Gr<osse> Schiffgasse 10. 1. St<ock Nr.> 8.” (Diese Angaben sind auch auf Grund ihrer Tinte und Schreibweise den Wiener Einträgen zuzuordnen.)

In der nachfolgenden Edition wurden die vereinzelt gestrichenen oder eingefügten Textteile individuell vermerkt, sowie die von Alexander relativ selten verwendeten Abkürzungen (z. B. “u.”). Die Herausgeber haben es versucht, so wenig wie möglich in die originellen Satzstrukturen einzugreifen. Auch die Orthographie wurde nicht modernisiert, um dieses Textstück, die zu den wenigen ursprünglich auf Deutsch über die Österreichische Philosophie verfassten zeitgenössischen ungarischen Augenzeugenberichten gehört, authentisch vermitteln zu können.

Die Herausgeber möchten sich Herrn Dr. Antal Babus, Leiter der Handschriftenabteilung der Zentralbibliothek der Ungarischen Akademie der Wissenschaften, für die freundliche Genehmigung bedanken, Teile des Tagebuches (Signatur: Ms 4110/26) veröffentlichen zu dürfen.

<sup>3</sup> Alexander Bernát – Szemere Samu (Hrsg.) 1928. *Alexander Bernát ifjúkori levelei Horváth Cyrrillhez*. Budapest.

<sup>4</sup> Gábor Éva 1986. *Alexander Bernát*. (A múlt magyar tudósai.) Budapest, Akadémiai.

## EDITION

Wien 9 Dezember 1869.<sup>5</sup> Es ist schon sehr lange<, > daß ich keine Zeile in mein Tagebuch schrieb.<sup>6</sup> Mittlerweile sind böse Tage gegangen u<nd > gekommen, die Zeiten, in denen ich Noth litt<, > sind verschwunden, aber auch jetzt ist meine äußere Lage nicht günstig genug, um mir Zufriedenheit zu gewähren. Ich habe noch heute keinen Winterrock,<sup>7</sup> und bin in geplagtem Zustande, in welchem mir tausend kleinere und größere Nadelstiche das Leben sauer machen. Doch davon ein andermal.

Ich habe sonderbare Metamorphosen im Geiste durchlebt. Mehr als ein Jahr ist<'>s nun, daß ich in Wien lebe, aber ich fühle mich gehoben, wenn ich auf diese Zeit zurückblicke, denn nicht vergebens habe ich sie durchlebt. Was Erfahrung und Wissenschaft nur bieten konnte<, > habe ich reichlich genossen. Ich<sup>8</sup> kann sagen, ich bin immer vorwärts geschritten, nie rückwärts. Bei alledem war ich kein Bücherwurm, ich habe mehr Tage im<sup>9</sup> Nichtsthun vollbracht, mehr Tage herumgeschweift, als gerade neben den Büchern zugebracht, aber nicht einmal diese Zeit achte ich für verloren. Ich fühle, ich bin nicht hinter meinen Kräften zurückgeblieben, ich erreichte, was mir die Möglichkeit geboten <hat>. Vielleicht täusche ich mich auch, aber kein Gefühl ragender Reue beunruhigt mich, ich lebe in Frieden mit mir und der Welt, wenn mir diese auch nicht gerade die beste Seite gezeigt hat. Was ich studierte, habe ich meinem Wesen assimiliert, meine Ken<n>nisse stehen nicht zerstückt und zusammenhanglos, wie fremde Gäste, die die Zeit<sup>10</sup> auseinanderstäubt,<sup>11</sup> in meinem Kopfe, ich suchte sie meinem Wesen einzuverleiben, sie als innerstes Eigenthum mir zu erwerben. Ich strebte mehr nach Tiefe als nach Breite, da mir für letztere noch immer Zeit genug bleibt, wenn mir ein bleibender Grundstock lebt, an dem sich aller Stoff, alle Elemente ankrystallisiren. Durch ruhiges liegen lassen meiner Ansichten,

<sup>5</sup> Im Original unterstrichen.

<sup>6</sup> <Da der Eintrag auf der ersten Seite steht und das Heft selbst kein erkennbares Zeichen physischer Diskontinuität aufweist, bezieht sich diese Bemerkung wohl auf ein anderes, verschollenes Tagebuch von Alexander oder aber deutet auf eine bewusste Stilisierung durch Alexander hin.>

<sup>7</sup> <Laut Grimm-Wörterbuch (Bd. 30, Sp. 471): "rock für den winter; wärmender, dicker rock".>

<sup>8</sup> Nach "Ich" gestrichen: "bin".

<sup>9</sup> Im Original: "in".

<sup>10</sup> Im Original Komma nach dem Wort.

<sup>11</sup> Im Original Semikolon nach dem Wort. <Vgl. wohl Immanuel Hermann Fichtes Kritik der Herbart'schen atomistischen Psychologie: "so sehen wir nicht ein, was ihr [der Seele] auch nur für einen Augenblick ihre Dauer sichern könne, und warum nicht der ganze Mensch gleich den Wolken, die am Himmel *erscheinen*, und die sich auch nur aus mancherlei Dunsttheilchen ohne innere Einheit gebildet haben, wesenlos auseinanderstäubt und verfliegt nach allen Seiten!" (Fichte, I. H. 1832. Über Gegensatz, Wendepunkt und Ziel heutiger Philosophie. Erster kritischer Theil. Heidelberg, J. C. B. Mohr. 286–287). Um diese Zeit beschäftigte sich Alexander nachweislich mit den Werken I. H. Fichtes.>

verschwemmen<sup>12</sup> diese nicht, oder lösen sich auf, sie runden<?> und vervollkommenen sich, und wenn ich einen Gedanken recht innig durchlebt habe, so ist mir das mehr werth, als die Lecture von<sup>13</sup> einer ganzen Reihe wissenschaftlicher Compendien. Vor dem Unverstandenen habe ich nicht die heilige Scheu, wie vor einem Heil<i>gthum, ich lasse es, wenn ich keinen Weg zu demselben finde, ich bahne mir einen solchen, wenn ich mir seine Spuren finde. Und darin fühle ich, daß ich Recht habe, denn das rein Menschliche kehrt sich in mir hervor, das fähig ist<,> alles Große u<nd><sup>14</sup> Wahre und Schöne aufzufassen und zu begreifen. In der Metaphysik bin ich deshalb auch<sup>15</sup> kein Hegelianer<,> weil ich fühle, daß das nicht der Weg sei, auf welchem der gesunde Menschenverstand wandern soll, wenn er lebensfähige Resultate zu erzeugen<sup>16</sup> Willens ist. Ich hoffe zu einer natürlichen Weltanschauung zu gelangen, die ebenso weit entfernt sein soll von der eines platten ethischen und wissenschaftlichen Materialismus, als von einem verschwommenen Idealismus. Als Sohn meiner Zeit bin ich Realist, und neige mich den Ansichten eines Herbart<s> u<nd> Lotzes zu. Wie ich von ihnen abweichen werde<,> weiß ich noch nicht, aber ich glaube<,> ich werde es <thun>, denn mein Denken scheint mir nicht steril zu sein, und ist auf selbständigem Wege zu Vielem<sup>17</sup> gekommen, was jene Männer im Zusammenhange gedacht und geschrieben haben. Aber eines ist es, was ich für das Wichtigste halte, der große Welträthsel ist mir aufgegangen, nicht die Lösung des Problems, aber es selbst in seiner ganzen großartigen Herrlichkeit, und unergründlichen Tiefe. Wenn ich so die zahllosen Wasseratome sehe, oder den gestirnten Himmel, oder die Erde, in der finsternen Pracht der Nacht, so weiß ich nicht<,> wie mir ist. Ich fühle mich fremd, mir schwindelt<'>s, ich weiß nicht<,> wer ich bin, was das alles ist, ich staune über den leisesten<sup>18</sup> Luftzug, über das gewöhnlichste Menschenangesicht. O was wiegt mir, das eine Gefühl, das in mir oft in solcher Lebendigkeit ist, auf, was habe ich von all dem, was in hundert Büchern geschrieben steht, wenn ich mich so träumend in alle Geheimnisse des Seins versenke. Wenn<sup>19</sup> es mich bei meinen Büchern ergreift, dann lasse ich alles fahren, ich denke nicht mehr, worüber sie sich alle die Denken geplagt <haben>, ich sinne nur für mich hin, und lasse das Gefühl in mir walten. Das mir die größte Seligkeit<sup>20</sup> gewährt, ein Mensch zu sein, in vollem Sinne des Wortes mit

<sup>12</sup> <Laut Grimm-Wörterbuch (Bd. 25, Sp. 1205): "in eigentlicher bedeutung, von wasserfluten, die etwas fortreizen, dann etwas bedecken mit angeschwemmtem (sand, schlamm) [...]. die übertragene anwendung schlieszt sich oft eng an die bildliche an".>

<sup>13</sup> Nach "von" gestrichen: "hundert".

<sup>14</sup> Nach "u<nd>" Unlesbares gestrichen.

<sup>15</sup> Einfügung.

<sup>16</sup> Im Sinne von "erweisen" (vgl. Grimm-Wörterbuch, Bd. 3, Sp. 1081).

<sup>17</sup> Verbesserung für "vielen".

<sup>18</sup> Nach "leisesten" Unlesbares gestrichen.

<sup>19</sup> Vor "Wenn" gestrichen: "Ich".

<sup>20</sup> Nach "Seligkeit" gestrichen: "u<nd> die gr".

fühlender Brust dem All gegenüber. Ich könnte oft wahnsinnig werden von all den dunkeln<sup>21</sup> unaussprechlichen Ahnungen, die dann in mir auftauchen, ich weiß nicht<,> wohin ich mich vor mir selbst flüchten soll, und das Gewöhnlichste ist mir willkommen, das mich meinen Gedanken entreißt. Denn wenn es lang anhielte, ich könnte es nicht ertragen, ich müßte den Verstand verlieren. O Unsterblichkeit, welcher lockender Gedanke bist du für den Menschen -- sage ich dann vor mich hin, was würden sie opfern, wenn sie deiner gewiß wären.

Erst gestern Abend war es, als wir<,> ein trauriger<sup>22</sup> Zirkel, eben von einem Trinkgelage zurückkehrten, das wir zu Ehren eines neuen Bruders veranstaltet hatten. Ich hatte viel des Bieres genossen, und doch war meine Seele klar. Ich tobte und lärmte, und suchte die innere Stimme zu übertäuben, und brachte nichts heraus, als was sie mir eingab. Ich rief, das große Geheimniß der Menschheit hat mich angepackt, und sie fühlten sich verwandt, denn auch in ihnen lebt<'>s. Was thue ich auf der Welt, rief ich, die Zahl der vielen Fragezeichen vermehren, die unser Geschlecht schon besitzt? Und doch ist<'>s eine Seligkeit<,> versetzte mein Freund W<eiss>,<sup>23</sup> wenigstens die Fragen alle zu wissen.

“Herum wandern unter den vielen Problemen, die noch anstarren, ohne ein einziges lösen zu können?<'>“

Sp. meinte:<sup>24</sup> Was<sup>25</sup> plagt Ihr sich, wollt Ihr Euer Geschlecht weiter bringen, damit es schneller die Wahrheit erlange, und früher aufhöre zu leben?

Ja, rief ich, ich will nicht in der Menschheit Waagen sitzen, ich will das Roß sein, das es zieht, das Roß<,> rief ich, wie berauscht von diesen Gedanken.

Die andere lachten und wir giengen ruhig<sup>26</sup> weiter.

Zu Hause angelangt setzten wir uns um den Tisch, und durchlebten die wehevollste Stunde, die man sich denken kann. Jeder schrie, den Egoismus zu zerbrechen, und jeder in seiner Weise, für die Menschheit zu leben und zu wirken. Wie Feuer loderte die Begeisterung in uns, mit ihren heiligen Strahlen, erwärmte und erhob sie unsere Herzen. W<eiss> betonte, daß er Alles für die Freunde hingebte, daß wir zusammenhalten, und uns gegenseitig fördern wollen, daß das Eigenthum des Einzelnen nicht ihm, sondern der Bruderschaft angehören, sein Wirken nicht ihm, sondern der Menschheit zu Gute kommen sollte. K. schwur, er gebe sein Leben für Freunde und Verwandte hin, die er

<sup>21</sup> Historische Schreibweise.

<sup>22</sup> <Laut Grimm-Wörterbuch (Bd. 21, Sp. 1550): “vom freunde: alt besonders in der Verbindung *tr. geselle*”.>

<sup>23</sup> <József Bánóczy (bis 1874: Weiss/Weisz; 1849–1926), der Alexander während seiner ganzen Peregrination begleitete. Damals wohnten sie noch getrennt, in der geographischen Nähe voneinander: Kleine Schiffgasse 12 und Grosse Schiffgasse 10.>

<sup>24</sup> Im Original Komma.

<sup>25</sup> Verbesserung für Unlesbares.

<sup>26</sup> Im Original: “ruhige”.

mit ganzer Seele liebe, und dieser Mann des Gefühls, versicherte uns<sup>27</sup><,> uns zu folgen und sich uns anzuschließen, wohin wir immer giengen. Sp. saß da, mit dem melancholischen, pessimistischen Gesichte, und wir bekämpften lebhaft, seine Schwäche, sich in nichts zu vertrauen. Nach und nach löste sich die Spannung auf dem Gesichte und wir hatten ihn ganz überwunden. E<...><,> der neue Bruder, der schlasse<sup>28</sup> Kaufmann zitterte am ganzen Körper, als die edelsten Gefühle, als die reine Menschheit sich in uns allen zeigte, und tausend gute Entschlüsse keimten in seiner Seele. Wir sprachen von Liebe, in der wir schon alle einige Erfahrungen gesammelt <haben>, und ich gab meine innerste Sehnsucht kund, einem Wesen ganz anzugehören, in<sup>29</sup> ihm gänzlich „aufzugehen“. Dann saß ich still leidend <?> vor mir hin, und dachte an die Naturnothwendigkeit<,> mit der die Menschen die<jenige> sind, die sie sein müssen, an die mannigfaltigen Naturen, die hier versammelt sitzen, jeder andere, jeder aber gut und edel in seinem innersten Wesen mit Gefühlen, die die Welt einschauen.<sup>30</sup> Bis vier Uhr morgens, hielt mich die Aufregung, die ich körperlich und geistig empfand, wach, und schon um ½ 7 wachte ich auf, um in meine Wohnung<sup>31</sup> heimzukehren.

16/1 1870. Eben bin ich von einem Trinkgelage zurückgekehrt, daß ich zu großem Theile veranstaltet habe. Am 13. d<ieses Monats> ist mein erstes Feuilleton in der Presse<sup>32</sup> erscheinen, und ich habe dafür 20 Fl<orenos><sup>33</sup> erhalten. Es ist ein kleiner Anfang, das ist wahr, aber es ist doch einer. Mein Professor<sup>34</sup> gratulirte mir dazu, und sagte, ich habe Anlagen<,> einen guten Stil zu bekommen. Er forderte mich auf<,> das Referat<sup>35</sup> über das neue Werk I<manuel> H<ermann von> Fichtes zu übernehmen,<sup>36</sup> ich solle wenigstens darüber arbeiten, wenn ich nicht mit der Richtung übereinstimme. Er sagte mir noch manches Schmeichelhafte, ich kam mit freudestrahlendem Gesichte aus seiner Wohnung, ich fühlte mich gehoben. Auch der Eigenthümer der Pres-

<sup>27</sup> Aus Versehen gestrichen.

<sup>28</sup> <Laut Grimm-Wörterbuch (Bd. 15, Sp. 500): „schlaff, weich, [...] sonst auch nachlässig, unthätig“.>

<sup>29</sup> Im Original: „im“.

<sup>30</sup> <Im Sinne von „anschauen“, „durchschauen“, vgl. Grimm-Wörterbuch (Bd. 3, Sp. 267).>

<sup>31</sup> <Grosse Schiffgasse 10. 1. St. Nr. 8. >

<sup>32</sup> <Vgl.: Philosophie des Unbewußten. *Die Presse*. Bd. 23, Nr. 12 (13. Jan. 1870). 1–4, gekennzeichnet mit „B. A.-r.“>

<sup>33</sup> <Lateinische Abkürzung für Gulden, die offizielle Währung der Doppelmonarchie ab 1867.>

<sup>34</sup> <Robert Zimmermann (1824–1898), ordentlicher Professor für Philosophie an der Wiener Universität 1861–1896.>

<sup>35</sup> Nach „Referat“ gestrichen: „s“.

<sup>36</sup> <Fichte, I. H. 1869. *Vermischte Schriften zur Philosophie, Theologie und Ethik*. Bd. 1–2. Leipzig, F. A. Brockhaus.>

se Dr Dreger<sup>37</sup> sagte mir, ich solle ihm nur wieder verschicken was ich habe, kurz mein Feuilleton<sup>38</sup> scheint für mich vielversprechend zu werden. Aber mein Ziel muß ich erreichen, als Preisschrift für die ung<arische> Akademie muß ich eine Anthropologie anfertigen,<sup>39</sup> das soll für mich die große Leiter sein, auf der ich enger klimmen will zu äußerem Ansehen und Ruhm, zu innerer Befriedigung und Harmonie. Wird mir aber das je möglich sein? Jetzt halte ich in mir den ganzen Zwiespalt, den das große Räthsel der Welt mir aufdrängt. Ich habe den Muth verloren, je über diesselbe hinaus zu können, u<nd> glaube<,> kein Mensch, kein Wesen, selbst Gott ist dieß nicht im Stande. Es ist ein so ungeheurer Gedanke<,> innerhalb einer Natur zu stehen, sich als etwas zu fühlen<,> und doch nie über sich hinauszukönnen. Ich glaube<,> der Kopf zerspringt mir bei diesen Gedanken. Ich fühle mich<sup>40</sup> so unheimlich, mein Stuhl zittert unter mir, ich meine <?> es wenigstens zu fühlen, dann fällt mir ein:<sup>41</sup> mein Stuhl? Was ist mein Stuhl? Materie, was ist Materie? Was ist Gott? Woher Gott? Woher Materie, woher die ganze Welt? Woher Alles was man denken kann? Woher meine Fragen u<nd> Zweifeln? Ich habe in einer Abhandlung gesagt, die Frage <“>woher<“> hat keine<sup>42</sup> Ende, immer und immer kann man danach fragen. Das scheint <...> und kalt. Die Menschen beruhigen sich dabei, und essen und trinken weiter. Warum? Weil<sup>43</sup> sie diesen Gedanken nicht gefühlt haben. Hat ihn aber jemand gefühlt, dann ist es nicht mehr zum Aushalten, dann glaubt man wahnsinnig zu werden. Es ist etwas anderes, diesen Satz sich einfach herzusagen und dabei zu staunen, als ob man eine Marionette anstaunte, oder ein perpetuum mobile, daß ein erfinderische<r> oder wahnsinniger Kopf endlich gefunden zu haben glaubt.

Ich kann nicht weiter, ich fürchte mich vor diesen Gedanken, er ist mein Verderben. Ich weiß nicht<,> ob ihn schon je ein Mensch gefaßt <hat>; so tief und so <?> streckend, so sinnberäubend, wie ich ihn fasse. Er soll mein geheimer Leitstern sein, in allen was ich unternehme. Er soll meine Seligkeit und meine Qual werden. Man sagt, daß das Wissen dem Menschen Macht verleiht, wenigstens innere, daß er so geistiger Besitzer all dessen werde, was er begreift.

<sup>37</sup> <Carl (Karl) Dreger (1825–1896), Verleger der *Presse* aus ihrer Periode nach der Abspaltung der *Neuen Freien Presse* im Jahr 1864, die schließlich 1896 zum Abstellen des Blattes führte (die bis heute erscheinende namensgleiche Tageszeitung wurde erst 1945–1946 gegründet).>

<sup>38</sup> Nach “Feuilleton” gestrichen: “ist”.

<sup>39</sup> <An der Allgemeinen Sitzung am 17. April 1869 erließ die Ungarische Akademie der Wissenschaften u.a. ein Preisausschreiben zur Abfassung eines »Handbuches der Anthropologie« im Umfang von min. 12–14 Oktavbogen, das sein Gegenstand »mit dem gegenwärtigen Stand der jeweiligen Wissenschaft übereinstimmend« und zugleich »auf einer populären, hübschen und geregelten Sprache« darstellt.>

<sup>40</sup> Im Original: “mith”.

<sup>41</sup> Im Original Komma.

<sup>42</sup> Nach “keine” Unlesbares gestrichen.

<sup>43</sup> Im Original ohne Fragezeichen: “Warum weil”.

Dieser Gedanke aber, der höchste<,> den ein Mensch fühlen kann, hebt auch weithinaus über alles Gewöhnliche. Obwohl ich überzeugt bin, daß alle<,> die um mich leben<,> von diesen Gedanken keine Spur haben -- fühle ich doch keinen Stolz, nur ein Drängen meines Gehirns<,> als wollte es mir den Kopf zersprengen. Ich glaube<,> das ist die wahre Gedankengröße, daß sie so hoch hebt über alles Gewöhnliche, daß man keines der gewöhnlichen menschlichen Gefühle mehr empfiehlt, weder Mut, noch Stolz, nur eines bleibt, der ragende Zweifel, die schreckliche Verzweiflung. Vielleicht wird auch diese abgestumpft. Kann man innerlich durchdrungen von diesem Gedanken noch unter Menschen leben. Nein, ich glaube<,> man muß ihn verbannen aus dem gewöhnlichen Umkreis des Denkens, man muß ihn mit einer Decke verhüllen, man muß ihn tief versenken als Grundstein aller seines Denkens und Fühlens, er darf nur tragen das ganze geistige Menschenleben, er darf es aber nicht durchdringen, denn diesen Durchbruch hält kein Erdgeborener, kein sin<n>endes Wesen aus. Dieser Gedanke<sup>44</sup><,> daß man nicht weiß woher, innig durchgeföhlt, vollständig begriffen<,> ist das schrecklichste Räthsel, zu welchem der Menschengest gelangen kann.

Ich will für jetzt abbrechen, denn ich kann nicht weiter.

<sup>44</sup> Am Ende des Wortes gestrichen: "n".



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# Summaries

GERGELY AMBRUS

## **Austrian Identity Theory and Russelian Monism: Schlick, Russell and Chalmers**

This paper discusses Moritz Schlick's "Austrian" psychophysical identity theory, formulated in the *Allgemeine Erkenntnislehre*, and compares it to the similar views of Russell and to contemporary Russellian monism. A close similarity between Russell's and Schlick's views was already stated by Herbert Feigl long ago; beyond investigating this relation, my aim is also to identify features contemporary Russellian monists may have in common with their historical ancestors. I argue that they share some fundamental assumptions: linguistic physicalism, an ontology which may be characterized as physicalist dualist property pluralism, and a dual-language account of the psychophysical identity thesis which is an alternative to reductionist materialism. Further, Schlick, Russell and Chalmers ground these tenets on a structuralist account of the meaning of physical terms which, however, they lay out in importantly different ways.

CHRISTIAN DAMBÖCK

## **Carnap's *Aufbau*: A Case of Plagiarism?**

In a recent article, Verena Mayer formulates a very radical claim, specifically that in the *Aufbau*, Carnap somewhat plagiarized Husserl, stealing ideas from the then-unpublished manuscript of *Ideen II*. The aim of this article is to refute this claim. Though Carnap might have been acquainted with Husserl's manuscript, there is no indication that he took a significant amount of ideas from the latter.

DENIS FISETTE

## **The Reception of Ernst Mach in the School of Brentano**

This paper is about the reception of Ernst Mach by Brentano and his students in Austria. I shall outline the main elements of this reception, starting with Brentano's evaluation, in his lectures on positivism, of Mach's theory of sensations. Secondly, I shall comment the early reception of Mach by Brentano's pupils in Prague. The third part bears on the

close relationship that Husserl established between his phenomenology and Mach's descriptivism. I will then briefly examine Mach's contribution to the controversy on gestalt qualities. The fifth part bears on Stumpf's debate with Mach on psychophysical relations and I shall conclude on Husserl's criticism of Mach's alleged logical psychologism.

GUILLAUME FRÉCHETTE

### **Brentano on Perception**

Brentano's philosophy of perception has often been understood as a special chapter of his theory of intentionality. If all and only mental phenomena are constitutively intentional, and if perceptual experience is mental by definition, then all perceptual experiences are intentional experiences. I refer to this conception as the "standard view" of Brentano's account of perception. Different options are available to support the standard view: a sense-data theory of perception; an adverbialist account; representationalism. I argue that none of them are real options for the standard view. I suggest that Brentano's conception of optical illusions introduces a presupposition that not only challenges the standard view – the distinction between the subjectively and objectively given – but that also makes his account more palatable for a naïve understanding of perception as openness to and awareness of the world.

CHRISTOPH LIMBECK-LILIENAU

### **The First Vienna Circle: Myth or Reality?**

In the genealogy of logical empiricism, the so-called "First Vienna Circle" (Neurath, Frank, Hahn) has been considered an essential episode, connecting the philosophy of Mach and the French conventionalists with the later logical empiricism of the Vienna Circle around Schlick. The present paper makes three claims: (1) We make the historical claim that the lack of archival sources on the "First Vienna Circle" does not allow a reliable reconstruction of such a discussion group, and even allows some doubts about its existence, at least as a regular discussion group. (2) We emphasize the interaction of the young Neurath, Hahn and Frank, in Vienna around 1910, with a group of philosophers strongly influenced by Meinong and claim that this interaction was very advantageous for a reception of the new symbolic logic and especially of Russell's philosophy of logic and mathematics. New archival sources permit us to reconstruct such an interaction. (3) We claim that this Meinongian context in Vienna shaped some philosophical positions of Neurath and Hahn, especially their view of the nature of logic and mathematics. We claim that at least Neurath, but probably also Hahn, endorsed a logical realism similar to that of Russell and Meinong. It was only after the reception of the *Tractatus* in the Vienna Circle that such a logical realism was unanimously rejected by the logical empiricists. Besides the obvious influence of Mach and the French conventionalists on the young Neurath, Hahn and Frank, this heritage from the Meinong school should be taken into account in an evaluation of the early philosophies of our mentioned trio.

CSABA PLÉH

**The Impact of Karl Bühler on Hungarian Psychology and Linguistics**

This review paper analyses the influence of the theories of Karl Bühler on Hungarian twentieth century psychology. The Würzburg Denkpsychologie works of Bühler showed up in early theoretical works of Valéria Dienes in the 1910s, and later in the reviews and experimental studies of Ferenc Lehnert/Lénárd from the 1930s to the 1970s. Two Würzburg based PhD dissertations under the direction of Karl Marbe done by Anton Schütz on associative sets, and by Imre Molnár on the objective foundations of aesthetic experience show an important inspiration going back to Bühler: a commitment to the existence of supraindividual organizations.

The mature Bühler of the Vienna years had a central impact on two Hungarian experimental psychologists. Paul Schiller von Harkai who spent some postdoctoral months in Vienna developed a functionalist theoretical psychology combining it with ideas from the Gestalt theories of Lewin and Bühler. He extended the ideas of Bühler about the universality of meaningful holistic organization of behavior into a task-centered motivational psychology. The other follower was Ludwig Kardos, a PhD student of Bühler in Vienna. Kardos extended the sign-based perceptual theory of Bühler into a successful mathematical theory of light constancy that interpreted contextual influences in a general model. In his later work on animal memory and the origin of mental life in the 1950-1980s period Kardos has taken up the evolutionary interests of Bühler. He proposed a theory of the origin of mind where the information aspect plays a leading role. In a way this is a continuation of the sign-based semiotic theory of mind entertained by Bühler.

MIKLÓS RÉDEI

**Parallels and Divergencies: Gödel and von Neumann**

John von Neumann and Kurt Gödel are two towering figures of 20<sup>th</sup> century science. Their life and scientific careers had many parallels and their research interests overlapped. But their philosophical views about sciences, especially about the nature and foundations of mathematics were different. The paper highlights some parallels and correlations between divergences of their philosophical positions and differences in their scientific research and career.

FRIEDRICH STADLER

**Austrian Philosophy: Outlines of a Discipline at the University of Vienna in the 20<sup>th</sup> Century**

The article provides an overview of “Austrian philosophy” during the “long 20<sup>th</sup> century” through an institutional history of the Department of Philosophy with the main figures teaching philosophy at the University of Vienna. After a short review of philosophy as a key discipline within the Faculty of Philosophy, the development is described mainly from 1848 onwards with a focus on the last century. The personal and institutional breaks and continuities are characterized by a thematic analysis of the philosophical

research and teaching in historical context. This is done with a focus on the typical Austrian “scientific philosophy” in its relation to alternative dominant currents. This specific dynamics becomes manifest on the one hand with the significance of philosophy within the Faculty of Philosophy and, on the other, with its role and function *vis à vis* the other classical faculties. The process of a gradual dissolution and diversification of the Faculty of Philosophy up to the present indicates this changing role of a long-term, dominant “royal discipline”. Nevertheless, the restructuring and renewal of philosophy as a discipline and research field since the University reform after 2000 appears as a successful and promising turn with an increasing international visibility and appreciation covering also the typical Austrian tradition in philosophy.

THOMAS UEBEL

### **Overcoming Carnap’s Methodological Solipsism: Not As Easy As It Seems**

Methodological solipsism is the position adopted by Rudolf Carnap in his *Der logische Aufbau der Welt* (*The Logical Structure of the World*, 1928) according to which it is possible to develop, by logical construction, a conceptual system encompassing all of empirical science on the basis of concepts pertaining only to an individual’s phenomenal experience. In this paper I investigate whether, and if so, how, methodological solipsism can be effectively opposed when it is assumed – as it was until Quine’s criticism published only in 1951 – that the *Aufbau* succeeds in its reconstructive aim. I argue that Carnap had considerable resources to block several ways of pressing the criticism that it overlooks the social dimension of knowledge – but not all of them.

PÉTER ANDRÁS VARGA

### **A Snapshot of Austrian Philosophy on the Eve of Franz Brentano’s Arrival: The Young Bernhard Alexander in Vienna in 1868–1871**

When the young Bernhard Alexander arrived in Vienna in 1868 he was not yet the towering figure of late nineteenth – early twentieth century Hungarian philosophy. The philosophy he encountered in Vienna was, too, not yet the Austrian Philosophy (with a capital ‘P’) which Rudolf Haller believed to have been born few years later in 1874. Based on the combination of unpublished sources from four archives (including Alexander’s Viennese diary entries, co-published in this journal issue by B. Székér and B. Szabados) and Alexander’s early occasional writings, I reconstruct the historical circumstances of Alexander’s academic peregrination in Vienna (e.g., courses attended, intellectual relationships, and writing projects). There is a considerable discrepancy – both in terms of the *dramatis personae*, their writings, and the involved philosophical doctrines – between the content of Austrian Philosophy and the snapshot of Austrian philosophy that becomes visible to us through the lens of Alexander’s peregrination. Yet, the latter not only temporally preceded the former, but it also constitutes the conceptual and historical precondition of Haller’s Austrian Philosophy. In particular, the young Alexander could provide us with an unfiltered picture of what Robert Zimmermann, his Viennese philosophical master, could have transmitted to another generation of young Viennese students, including Edmund Husserl, who convened around Franz Brentano one decade later and inaugurated Austrian Philosophy with a capital ‘P’.