

Revisiting Friedman's F53: Popper, Knight, and Weber

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Abstract:

Neither Karl Popper, nor Frank Knight, nor Max Weber are cited or mentioned in Friedman's famous 1953 essay "On the methodology of positive economics" (F53). However, they play a crucial role in F53. Making their contribution explicit suggests that F53 has been seriously misread in the past. I will first show that there are several irritating statements in F53 that are, taken together, not compatible with any of the usual readings of F53. Second, I show that an alternative reading of F53 can be achieved if one takes seriously Friedman's reference to ideal types; "ideal type" is a technical term introduced by Max Weber. Friedman was familiar with Max Weber's work through Frank Knight, who was his teacher in Chicago. Given that in F53's view ideal types are fundamental building blocks of economic theory, it becomes clear why both instrumentalist and realist readings of F53 are inadequate. Third, the reading of F53 in terms of ideal types gives the role of elements from Popper's falsificationist methodology in F53 a somewhat different twist. Finally, I show that the irritating passages of F53 make good sense under the new reading, including the infamous "the more significant the theory, the more unrealistic the assumptions".

1. Introduction

Friedman's famous 1953 essay "On the methodology of positive economics" (Friedman (1953) or F53) is very likely "*the most cited, the most influential and the most controversial* piece of methodological writing in twentieth-century economics" (Mäki (2009b), p. 47). Regarding its echo in the literature, there were more than 5,500 Google citations in January 2017 (out of more than 123,000 Google citations of all of Friedman's works). Regarding the controversies surrounding F53, there are, on the one hand, those who "uniformly condemned" Friedman's methodology.¹ The reason is less the substantial content of F53, but the impression that F53's theses are "simply muddled and confused" (Helm (1984) p. 121). Or, in somewhat more polite terms:

"F53 is inherently very hard to understand. This difficulty is not only due to its richness, but also due to its obscurities, ambiguities, and inconsistencies" (Mäki (2009b), p. 49).

On the other hand, F53 has been defended against its critics as articulating a completely coherent instrumentalist position:

"Every critic of Friedman's essay has been wrong. The fundamental reason why all of the critics are wrong is that their criticisms are not based on a clear, correct, or even fair understanding of his essay. Friedman simply does not make the mistakes he is accused of making. His methodological position is both logically sound and unambiguously based on a coherent philosophy of science—Instrumentalism." (Boland (1979), p. 503)

The instrumentalist interpretation of F53 is, in fact, its standard reading. The influential philosopher of economics Mark Blaug explained why F53's supposed instrumentalism has been accepted by many economists with such ease:

¹ Hausman (1992), p. 163 fn. 17; Hausman supports his claim by a list of more than 30 references.

“The idea that unrealistic “assumptions” are nothing to worry about, provided that the theory deduced from them culminates in falsifiable predictions, carried conviction to economists long inclined by habit and tradition to take a purely instrumentalist view of their subject.” (Blaug (1975), p. 399)

However, also this purely instrumentalist reading of F53 is certainly not uncontroversial.² Alternatives found in the literature comprise, among others, forms of instrumentalism deviating from “standard instrumentalism” (e.g., Hausman (2008 [1992]), and it has even been tried to “reread and rewrite F53 as a realist statement” (Mäki (2009c)). Of course, all authors find passages in F53 supporting their specific interpretation and dismiss others.

It did not make life easier that Friedman himself never intervened in all these controversies and confusions. He decided early on to adopt “a policy of not replying to critiques of the article” (Friedman (2009), p. 355). What may be additionally irritating is the fact that upon rereading his article in 2002 or 2003, Friedman “found its claims basically right” (Mäki (2009b), p. 60 fn. 1). Is it plausible that an author like Friedman does not realize how “muddled and confused” his article is, or that he realizes it but does not admit it? Or could it be that F53 has not been properly understood, including its infamous “the more significant the theory, the more unrealistic the assumptions” (14)?³

In this paper, I claim that many of the problems of F53 are due to the substantial influence of three authors on the paper who are neither cited nor mentioned in it: Karl Popper, Frank Knight, and Max Weber. Of course, the influence Popper’s falsificationism on F53 has been seen and appreciated in economics:

“The methodology of positive economics’ established falsificationism as the dominant methodology in economic research, as far as the self-perception of academic economists was concerned.”⁴

However, Popper’s influence on F53 is problematic insofar as Popper is rightly known as an enemy of instrumentalism, and F53’s methodological position is mostly seen as instrumentalist.⁵ Can this tension be resolved? Max Weber’s influence on F53 is indicated by several occurrences of his technical term “ideal type” in F53. However, the connection to Weber has almost never been discussed in the literature. Frank Knight emerges as an indirect contributor to F53 only when one asks how Friedman may have known of Max Weber. As I shall show below, Knight was an ardent admirer of Weber, and in the 1930s, Friedman studied Weber in Knight’s class in Chicago.

The effort to rediscover the role and influence of Popper, Weber, and Knight on F53 is motivated by the goal to get the methodological position of one of the most important economists of the 20th century right.⁶ Once the influence of those authors on F53 is explicit, the idea suggests itself that this paper has been seriously misread in the past. Furthermore, it will turn out that one may get some fresh insights into the methodology of economics beyond the

² See, e.g., Caldwell (1980) in response to Boland (1979).

³ Page numbers refer to the original pagination of F53, as in the facsimile reprint in Mäki (2009a).

⁴ Keppler (1998), p. 261. The influence of Popper on Friedman has been seen already in the 1970s, see, e.g., Latsis (1976).

⁵ See, e.g., Pheby (1991 [1988]), p. 88.

⁶ I will focus on F53 exclusively, not taking other writings of Friedman or any wider context into account. For writings that embed F53 into a larger theoretical context, see Pheby (1991 [1988]), pp. 84-85 and the articles of Part 4 of Mäki (2009a).

realism vs. instrumentalism divide, which may also be relevant for the discussion of the role of models in economics (see, e.g., Sugden (2000)).

This paper is structured as follows. I will first list some irritating statements of, and strange facts about, F53. Any reading of F53 must come to terms with these statements and facts. In Section 3 I shall discuss the question why F53 is so difficult to read and admits of so many different interpretations. I then turn to more substantive questions, first in Section 4 to the connection of F53 to the philosophy of Karl Popper. Section 5 considers the instrumentalist and the realist readings of F53, concluding that both of them are inadequate. In Section 6 I explain the systematic role of Weber's ideal types in F53, and sketch their historical journey from Weber to Friedman via Knight in Section 7. Section 8 then develops the consequences of the given ideal types view upon F53's as-if-methodology. Finally, I revisit the list of irritating statements and strange facts about F53 and show how the puzzlement dissolves under the new reading.

2. Irritating statements of and strange facts about F53

2.1 Economics as an "objective" science

F53 begins by setting a fairly ambitious goal for economics:

"In short, positive economics is, or can be, an "objective" science, in precisely the same sense as any of the physical sciences." (4, similarly 25 and 30)

There are two problems with this statement. First of all, the statement is very strong and one may wonder how it could be argued that economics may be as "objective" as the physical sciences. Second, the claim that economics is objective "in precisely the same sense" as any of the physical sciences is not really helpful because the word "objective" is put in quotes (linguists call them "scare quotes", see below Section 3). In other words, F53 does not exactly mean "objective" in the usual sense – but in what sense does it then mean "objective"?

2.2 Economic theory as "a set of tautologies"

About theory in economics, F53 states:

"Viewed as a language, theory has no substantive content; it is a set of tautologies. Its function is to serve as a filing system." (7)

According to the statement, a theory can be viewed as a set of tautologies. Tautologies are sentences like "All white things are white" or "All black things are black". In pursuing F53's parallel of economics to physics: can a theory like electrodynamics be viewed as a set of such tautologies? Or does the statement only apply to economic theories? It would still be strange. And if a theory is a filing system, it would probably be some sort of classification or taxonomy. How can tautologies produce a taxonomy?

2.3 "Assumptions" in quotes

A strange fact about F53 is that the term "assumption(s)" that occurs 73 times in F53, is used in quotes in roughly 50% of the occurrences (37 times, twice in section titles). Thus, when F53 refers to "assumptions" (in quotes), it does not really mean assumptions in some standard sense, but something else. What exactly are "assumptions" (in quotes)?

2.4 “[T]he more significant the theory, the more unrealistic the assumptions”(14)

In this most discussed sentence of F53, “assumptions” does not seem to appear in quotes. However, the full sentence is this:

“Truly important and significant hypotheses will be found to have “assumptions” that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense).” (14)

The “(in this sense)” refers back to the “assumptions” two lines above, thus to “assumptions” *in quotes*. The sentence is very hard to understand. First, it is difficult to see that the “assumptions” (whatever they are) of “truly important and significant hypotheses” shall be “wildly inaccurate descriptive representations of reality”. Second, it is even harder to understand how the unrealisticness of “assumptions” could be a particular *virtue* of significant theories. In his analysis of F53, Musgrave (1981) reads this sentence in three different ways by trying out three different meanings of “assumptions”. However, the result is equally negative because under all interpretations, the sentence comes out as mistaken.

2.5 Descriptive accuracy vs. analytic relevance

The following sentence continues the topic of the unrealisticness of assumptions and connects it with a presumed “analytical relevance” of economic theory:

“The basic confusion between descriptive accuracy and analytical relevance that underlies most criticisms of economic theory on the grounds that its assumptions are unrealistic ...” (33)

According to this statement, critics of the unrealisticness of assumptions of economic theory confuse descriptive accuracy and analytical relevance. Whatever analytical relevance is precisely, how can the assumptions of significant economic theory be analytically relevant *because of* their unrealisticness, i.e., *because of* their not being descriptively accurate? This is hard to understand.

2.6 “Appearances are deceptive” vs. “a more fundamental and relatively simple structure”

The following statement seems to contradict the common reading of F53:

“A fundamental hypothesis of science is that appearances are deceptive and that there is a way of looking at or interpreting or organizing the evidence that will reveal superficially disconnected and diverse phenomena to be manifestations of a more fundamental and relatively simple structure.” (33)

To be sure, this statement is not strange in itself, but it is certainly not easily reconciled with any position that can be legitimately called instrumentalist. Note that the statement claims a simple structure underlying the diversity of phenomena for *all* sciences, including economics. This sounds very much like a scientific realist’s credo, who believes that an unobservable theoretical “more fundamental and relatively simple structure” (whatever that is exactly) can be discovered by science, which unites apparently diverse and disconnected phenomena.

2.7 The extensive, but unreferenced use of Popper

There are many statements in F53 about hypothesis testing, prediction, falsification, etc., that seem to be more less directly taken out of Popper’s *Logic of Scientific Discovery* (Popper

(1959 [1934]). What is the exact strategic role of these Popperian elements in F53? Furthermore, how did these elements find their way into F53, given that the English translation of Popper's book appeared only in 1959, six years after the appearance of F53?

3. Why is F53 so difficult to read?

At first sight, it is truly amazing that an important and highly influential methodological article is still controversially discussed, more than six decades after its publication. There are several factors involved. In this section, I want to highlight one particular factor which is a special variant of a stylistic technique called "hedging". Hedging is a research topic in linguistics and is described as follows:

"Hedging is the expression of tentativeness and possibility and it is central to academic writing where the need to present unproven propositions with caution and precision is essential" (Hyland (1996), p. 433).

For example, a hedging phrase like "The data *seem to indicate* that ..." is found in many research papers as are other kinds, like various qualifiers or the use of passive voice.

F53 uses a special kind of hedging extensively: "apologetic quotation marks", or more commonly called "scare quotes". On its 41 pages, F53 uses scare quotes no less than 173 times (I have excluded ambiguous cases).⁷ In a practical guide to scientific writing, scare-quotes are explained as follows: "these marks are applied to tell the reader that an expression is not the author's and is not being used in the usual way."⁸ Note that this characterization is purely negative: scare quotes leave open what the expression is supposed to mean in the given context. Of course, the author may explain after having used scare quotes why they were used and what was meant. However, this is usually not what happens; authors thus leave readers in the dark about the precise meaning of the expression in scare quotes. The BioMedical Editor therefore recommends: "To avoid irritating your readers, use apologetic quotation marks sparingly or not at all." (*ibid.*) F53 certainly does not follow this advice.

F53's use of scare quotes is damaging because it concerns many of the central concepts of the paper. As I mentioned already, F53 very often uses "assumptions" in scare quotes.⁹ So, F53 *speaks* about assumptions, does not really *mean* assumptions, but *does not tell you what is meant* by "assumptions". Similarly, in F53 the term "objective" mainly occurs in scare quotes. Similarly, if you ask: what is the subject matter of economic models/theories, what are they about? F53 answers: "reality" (14, 25), the "real world" (31), "facts" (34), all in scare quotes.

Thus, F53 seems to distance itself from any straightforward form of realism, in particular from economics as gaining literally true knowledge about the world, but still claims some sort of "objectivity" for economics. So, it appears that the only position left is instrumentalism. At any rate, the large number of scare quotes that affect some of the most central epistemological concepts make the reading of F53 very difficult. Typically, for the already initiated reader the use of scare quotes may be illuminating: lacking a better expression, scare quotes

⁷ As far as I can see, only one other author explicitly noted "Friedman's repeated use of scare-quotes", illustrating it with four examples: Schliesser (2010), p. 179.—Sometimes, F53 emphasizes its distance to some standard meaning of a term even doubly by a "so-called" put in front of the expression in scare quotes, for instance when talking about "so-called «controlled experiments»" (10).

⁸ <http://www.biomedicaleditor.com/hedging.html>, accessed January 9, 2017.

⁹ See pp. 14, 15, 16 fn. 13, 18, 19, 20, 23, 24, 26, 27, 28, 29, 31, 32, 33 fn. 25, 40, 41, and 42.

signal the distance to the usual meaning of the term, without completely cutting the connection to it. The uninitiated reader, however, is left in the dark by the use of scare quotes because their message “the word is not to be taken in the usual sense” is purely negative. What *is* meant is not expressed nor even hinted at, and the uninitiated can only guess. In this sense the judgment that F53 is obviously the work of a philosophical amateur, is justified:¹⁰ philosophy should never extensively work with scare quotes, because it is its job to make things as explicit and as clear as possible.

Let us now turn to matters of concrete content of F53. I begin with its connections to Popper’s philosophy.

4. F53’s connection to the philosophy of Karl Popper

F53 has obvious, although unreferenced connections to Karl Popper’s philosophy called falsificationism: various elements of F53 appear to be directly taken out of Popper’s *Logic of Scientific Discovery*.¹¹ This provokes two questions. First, F53 appeared in 1953, and the English edition of Popper’s *Logic of Scientific Discovery* appeared only in 1959 (the original German edition appeared in 1934). So how did Friedman get access to Popper’s philosophy? The answer to this questions is given in interviews with Friedman in the 1990’s:

“One of the major benefits that I [Friedman] personally derived from the first meeting of the Mont Pelerin Society in 1947 was meeting Karl Popper and having an opportunity for some long discussions with him, not on economic policy at all, but on methodology in the social sciences and in the physical sciences. That conversation played a not negligible role in a later essay of mine, ‘The Methodology of Positive Economics’.”¹²

The second question is, why does F53 use Popper’s falsificationism at all? The obvious answer is that Friedman wanted to assimilate positive economics to the physical sciences, as we saw above. As Popper’s philosophy is aimed at explicating the methodology of science, and

¹⁰ Mayer (2009), pp. 122; “philosophically naïve and confused”, p. 139.

¹¹ Some examples from F53: “[T]he only relevant test of the validity of a hypothesis is comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted [...]; it is accepted if its predictions are not contradicted [...]. Factual evidence can never “prove” a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactly, that the hypothesis has been “confirmed” by experience” (8-9). “The evidence for a hypothesis always consists of its repeated failure to be contradicted, continues to accumulate so long as the hypothesis is used, and by its very nature is difficult to document at all comprehensively” (23). “The more general theory must have content and substance; it must have implications susceptible to empirical contradiction and of substantive interest and importance” (38). “Economics as a positive science is a body of tentatively accepted generalizations about economic phenomena that can be used to predict the consequences of changes in circumstances.” (39) “[T]he fundamental methodological principle that a hypothesis can be tested only by the conformity of its implications or predictions with observable phenomena” (40). “Any theory is necessarily provisional and subject to change with the advance of knowledge” (41). “The construction of hypotheses is a creative act of inspiration, intuition, invention; its essence is the vision of something new in familiar material. The process must be discussed in psychological, not logical, categories” (43).— Blaug stated already in 1975 “that Friedman is simply Popper with-a-twist applied to economics” (Blaug (1975) p. 399). While getting Popper’s influence right, Blaug misses another essential component of F53, as we shall see in Section 6.

¹² <http://hayekcenter.org/?p=5317>, accessed 8 Jan 2017. Unfortunately, this interview is not well documented. A better documented interview to the same effect is Hammond (1993), the relevant part of which is quoted in Backhouse (2012), p. 27. Friedman’s 1947 meeting with Popper is also reported in Frazer and Boland (1983), p. 135.

physics is his exemplary science, it seems only natural to exploit this philosophy for the methodology of economics. In this view, Popper's philosophy would be a welcome resource for F53. However, if one follows the instrumentalist reading of F53, obviously a substantial tension arises because Popper was an explicit opponent to instrumentalism.¹³ Are those elements of Popperian philosophy that look desirable such that one wants to import them into economics compatible with those elements of economic theorizing that are already in place, for instance instrumentalism? I shall not follow up this question because, as we shall see in the following, F53 should not be read as an instrumentalist position. Instead, according to F53, economic theorizing contains highly speculative elements that cannot be understood purely instrumentally, and these speculative elements are in need of strict empirical control (Sections 6 and 9). Popper's methodology informs economic theorizing about how this empirical control should be exerted.

5. Is F53's position instrumentalist or realist?

F53 indeed contains many passages that seem to support an instrumentalist position, and as I mentioned in the beginning, this is the standard reading of F53. However, there are some passages in F53 that do not fit well this reading, including the infamous sentence that I quoted already:¹⁴

“Truly important and significant hypotheses will be found to have “assumptions” that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense). [...] To be important, therefore, a hypothesis must be descriptively false in its assumptions.” (14)

The critical subject of this statement is “assumptions”—in scare quotes. This means that “truly important and significant hypotheses” (or theories) contain something

- that is only misleadingly expressed by the term “assumptions”, because
- it must be somehow similar to assumptions (in the usual sense)—thus the term “assumptions”, but
- it is significantly different from assumptions (in the usual sense)—thus the scare quotes.

Of these scare-quotes-assumptions, F53 states that they are “wildly inaccurate” or even “false”. This presumed property of “assumptions” is compatible with instrumentalism because the accuracy or truth-value of assumptions (or of “assumptions”, for that matter) is irrelevant for instrumentalism; it is only the accuracy of the empirical predictions, derived from the assumptions, that counts. However, what is furthermore said about the “assumptions” is not compatible with instrumentalism, namely, that “in general, *the more significant* the theory, *the more unrealistic* the assumptions (in this sense).” This statement does not fit instrumentalism because, as I said, instrumentalism passes no judgement whatsoever on the realistic or unrealistic character of “assumptions” (whatever they are).¹⁵ However, the statement is also not

¹³ This tension was discussed in detail in Frazer and Boland (1983).

¹⁴ This sentence has challenged many interpreters of F53, see, e.g., Blaug (1980), pp. 104-106; Musgrave (1981); Pheby (1991 [1988]), pp. 85-86; Mäki (2009c), pp. 94-95.

¹⁵ Compare the vigorous pronouncement of instrumentalism, there called “positivism”, and its implications by famous physicist Stephen Hawking. In his debate with fellow physicist and realist Roger Penrose, Hawking declares: “[Penrose] is worried that Schrödinger's cat is in a quantum state, where it is half alive and half dead. He feels that can't correspond to reality. But that doesn't bother me. I don't demand that a theory correspond to reality because I don't know what it is. Reality is not a quality you can test with a litmus paper. All I'm concerned with that the theory should predict the results of measurements”: Hawking and Penrose (1996), p. 121.

compatible with any form of realism because for realism, it cannot be *a virtue* of a theory to contain unrealistic assumptions. For the realist, unrealistic assumptions may be tolerable at best, but certainly not laudable. It may now become clear why F53 uses scare quotes around “assumptions”. In the usual sense of “assumption”, *not being realistic* is not a possible *positively* evaluated property. Therefore, F53 refers by the term “assumptions” to something different from what is usually understood by “assumptions”, and thus uses scare quotes. This is at least consistent. But what does F53 refer to by “assumptions”?

We may get a first clue what “assumptions” refer to in F53 by the statement that there is a “basic confusion between descriptive accuracy and analytical relevance that underlies most criticisms of economic theory on the grounds that its assumptions are unrealistic” (33). In other words, the virtue of highly valued “assumptions” is definitively not grounded in their descriptive accuracy, but in their “analytic relevance”, and analytic relevance seems to imply not being realistic.¹⁶ Unless one dismisses F53 at this point as “confused” or “untenable” (as most commentators do), it must be understood why it is that the “analytic relevance” of scare-quote-assumptions implies their not being descriptively accurate. Surely, in the toolboxes of both realists and of instrumentalists one doesn’t find assumptions whose high “analytic relevance” implies their not being realistic. So, whose toolbox do we have to inspect in order to understand what is going on? And, moreover, *what exactly is “analytic relevance”*?

6. F53’s connection to the sociology of Max Weber

We can answer these questions when we take seriously F53’s talk about the connection of relations between descriptive accuracy, analytical relevance, and ideal types. It should be noted that F53 uses the expression “ideal types” six times.¹⁷ Here is what Friedman says about the role of ideal types in economic theory, first about a widespread misunderstanding:

“The confusion between descriptive accuracy and analytical relevance has led ... to misunderstanding of economic theory ... “Ideal types” in the abstract model developed by economic theorists have been regarded as strictly *descriptive* categories intended to *correspond directly and fully* to entities in the real world *independently of the purpose* for which the model is being used.” (my italics, 34)

Thus, ideal types

- are not strictly descriptive categories (also 36),
- do not directly and fully correspond to entities in the real world,
- are not chosen independently of the purpose of the model.

Instead, the function of ideal types is “to isolate the features that are crucial for a particular problem.” (36)

¹⁶ In the cautionary footnote 12 on p. 12, Friedman warns the reader: “The converse of the proposition does not of course hold: assumptions that are unrealistic (in this sense) do not guarantee a significant theory.” Unfortunately, this warning has not been taken to heart by all readers: “F53’s examples of excellent scientific theories assume zero air pressure and profit maximization. The strong version [that “unrealisticness is a virtue”, p. 94] suggests that there might be even theories that assume that air pressure is infinitely large and that businessmen aim at maximizing their losses – these assumptions would be more unrealistic than the ordinary ones. But obviously, such unrealistic assumptions would not be epistemologically virtuous, thus the strong version is questioned” (Mäki (2009c), p. 95).

¹⁷ 34, 35 (three times), 36 (twice). In addition, F53 speaks on p. 36 of “ideal and real entities in a particular problem”, and it presumably refers to ideal and real types.

Ideal types are, of course, part and parcel of Max Weber's (1864 – 1920) sociology. Weber describes them as follows:

"An ideal type is formed by the one-sided *accentuation* of one or more points of view and by the synthesis of a great many diffuse, discrete, more or less present and occasionally absent *concrete individual* phenomena, which are arranged according to those one-sidedly emphasized viewpoints into a unified *thought* construct. In its conceptual purity, this mental construct cannot be found empirically anywhere in reality." (Weber (1949 [1905]), partially reprinted in Weber (2008 [1949, 1905]), p. 71)

The example with which Weber (1949) begins his important discussion of ideal types is

"an ideal picture of events on the commodity-market under conditions of a society organized on the principles of an exchange economy, free competition and rigorously rational conduct. This conceptual development brings together certain relationships and events of historical life into a complex, which is conceived as an internally consistent system." (*ibid.*)

And according to Weber, this is the function of this ideal type in research:

"Its relationship to the empirical data consists solely in the fact that where market-conditioned relationships of the type referred to by the abstract construct are discovered or suspected to exist in reality to some extent, we can make the characteristic features of this relationship pragmatically clear and understandable by reference to an ideal-type. This procedure can be indispensable for heuristic as well as expository purposes. The ideal typical concept will help to develop our skill in interpretation in research: it is no "hypothesis" but it offers guidance to the construction of hypotheses. It is not a description of reality but it aims to give unambiguous means of expression to such a description." (*ibid.*)

Before coming to F53's main example, let us look at an illustration of ideal types by Weber himself in which three ideal types are used; it is taken from the posthumously published work Weber (1958 [1922]). Its subject are all forms of "legitimate rule" (*legitime Herrschaft*, also translated as "legitimate authority") in which the respective rule/authority is stabilized through some kind of legitimation. Weber claims that there are exactly three *pure* types of legitimate rule/authority, and each of them is connected with fundamentally different sociological administrative structures. First, there is "legal authority," connected with purely conventional rules and bureaucracy; second, there is "traditional authority," connected with patriarchy; third, there is "charismatic authority," connected with a leader. The fundamental properties of these three ideal types are first that these three types exhaust all *pure* types of legitimate rule, second that the three types are mutually exclusive, and third that real cases of legitimate rule, the "real types", are mixtures of the ideal types.

The main example of ideal types in F53 is taken from Alfred Marshall who, according to F53, constructed two ideal types of firms (without using the expression "ideal type"): "atomistically competitive firms" (with "perfect competition") and "monopolistic firms" (with "perfect monopoly") (34-35).¹⁸ Clearly, these two ideal types fulfill Weber's abovementioned three fundamental properties.

¹⁸ The model of perfect competition also plays an important role in Frank Knight's theorizing: it marks Knight's transition from the "method of "successive approximation", common to economic analysis since at least the time of J.S. Mill" to a Weberian ideal type analysis; see Emmett (2009), p. 118. – In the following section, I shall come back to Knight's mediating role for F53.

To better understand the functions of ideal types, I suggest to conceive of them as *the basic vectors in a vector space of the pertinent phenomena*. This conception of ideal types suggests itself by the three fundamental properties that Weber attributed to them. First, the ideal types used in a particular situation exhaust all pure types, i.e., they span the complete space of real phenomena. The ideal types are mutually exclusive, i.e., the basic vectors are orthogonal. Third, real types (real phenomena) are mixtures of ideal types, i.e., linear combinations of the basic vectors.

In the case of phenomena that can be analyzed in two ideal types, like Marshall's firms, the situation looks like this (see figure 1).

[Figure 1 here]

In our case, any real type r can be analyzed in terms of the two ideal types (the generalization to more ideal types is obvious):

$$\overrightarrow{\text{Real type } r} = \alpha \cdot \overrightarrow{\text{ideal type 1}} + \beta \cdot \overrightarrow{\text{ideal type 2}}$$

In other words: the real type is a linear combination of ideal types.

If this reconstruction of the theory of ideal types is correct, economic models have according to F53 two heterogeneous elements. First, there must be a set of ideal types that span the vector space of all phenomena in question—F53 calls them the “abstract model” (35). Second, there must be rules how to analyze real types in terms of the given ideal types (35–36). In the given reconstruction in terms of a vector space of the pertinent phenomena, these are rules how to determine the coefficients α and β .

Before we come to the application of ideal types in F53 to the famous as-if methodology, we should first ask whether F53's presumed reference to Weber has any basic plausibility. After all, Max Weber is nowhere mentioned nor cited in F53. Did Friedman really know Max Weber's work in 1953?

7. How did Friedman know of Max Weber?

First, we should note that the absence of any reference to Max Weber's work in F53 is not an indicator that Weber does not play any role in F53. We saw already that, by Friedman's own admission, Popper plays a vital role in F53 without being quoted nor mentioned. So in principle, the same could apply to Weber. But is this plausible?

According to American sociologist Edward Shils, Friedman attended a seminar on the work of Max Weber at the University of Chicago in 1935 or 1936, given by economist Frank Knight, one of the founders of the Chicago school in economics. Other attendees were, among others, Edward Shils¹⁹ himself and later Nobel laureate in economics George Stigler.²⁰ As Shils reports, “the procedure was a line-by-line reading of the first three chapters of Weber's *Wirtschaft und Gesellschaft*, with comments by Knight” (Shils (1981), p. 184). For Knight, Weber was an extremely important figure.²¹ Knight was not only the first translator of one of

¹⁹ Edward Shils became a well-known sociologist and also a translator of Max Weber: see Weber (1949).

²⁰ Shils (1981), p. 184; Shils and Grosby (editor) (2006), pp. 3, 50.

²¹ According to Noppeney (1997), p. 327, it is “widely unknown” that “Frank Knight played a crucial role in the spread of Weberian ideas in the American social sciences.”

Weber's works into English, namely the *General Economic History* Weber (1927) (Emmett (2009), p. 77). As Emmett notes, Knight also "defended economic theory using an "ideal type" methodology." This was due to the fact that

"Weber drew [Knight's] attention both because Weber saw the problems of modern social science in much the same way that Knight did, [...] and because Weber offered Knight a different way out of the intellectual morass of American social thought than that followed by many of his contemporaries." (Emmett (2009), p. 112)

Methodologically, Knight therefore built upon Weber's ideas. As Emmett (2009), p. 118 puts it:

"[Knight's] most famous methodological essay [...] is also a forceful defense of "ideal type" analysis and Weber's notion of *Verstehen*".

The following autobiographical quote by Knight illustrates his "admiration and enthusiasm for Weber and his thought" (Noppenev (1997), p. 329):

"There has been the work of one man whom I have greatly admired. If I were to start out again, I would build upon his ideas. I am referring of course to Max Weber." (Schweitzer (1975), p. 279)

Following Weber, for Knight economic theory (concerning a specific domain of inquiry) must begin with a comprehensive list of the ideal types that, in my reconstruction, span the vector space of the real phenomena.

"Economic theory is not a descriptive, or an explanatory, science of reality. Within wide limits, it can be said that historical changes do not affect economic theory at all. It deals with ideal concepts which are probably as universal for rational thought as those of ordinary geometry." (Knight (1935), p. 277)

Note how well this corresponds to what F53 has to say about theories:

"Viewed as a language, theory has no substantive content; it is a set of tautologies. Its function is to serve as a filing system for organizing empirical material and facilitating our understanding of it; and the criteria by which it is to be judged are those appropriate to a filing system." (7)

The "set of tautologies" mentioned in the quote are the "stipulative" (or "synthetic") definitions of the ideal types. Stipulative definitions are definitions by which new terms are introduced (or "stipulated", in contrast to "analytic" definitions that concern terms already in use).²² Also Knight's comparison of the "ideal concepts" of economic theory with those of geometry is taken up in F53. After having stated that "[t]he model is abstract and complete; it is an "algebra" or "logic"" (24), F53 continues a little further down:

"A simple example may perhaps clarify this point. Euclidean geometry is an abstract model, logically complete and consistent. Its entities are precisely defined – a line is not a geometrical figure "much" longer than it is wide or deep; it is a figure whose width and depth are zero. It is also obviously "unrealistic." There are no such things in "reality" as Euclidean points or lines or surfaces." (25)

Given that F53 makes much of the opposition between "descriptive accuracy" and "analytical relevance" (see Section 5 above and Section 9 below), it is interesting to see that Knight similarly states that

²² On this type of definitions, see, e.g., Hoyningen-Huene (2004), pp. 68-69.

“a ‘science’ of human behavior, to be **relevant** to or practically significant, *must* describe *ideal* and not actual behavior.” (Knight (1935), p. 278, italics in the original, my boldface)

The substantive correspondence between Knight and F53 is remarkable enough. In addition, it is extremely likely that Friedman knew Knight’s respective paper very well, because he is one of the four editors of the collection of Knight’s essays that were, on the occasion of Knight’s forty-ninth birthday, published in 1935 (Knight et al. (1935), p. 8). The editors note that “[t]he entire responsibility for the choice of articles falls on us” (Knight et al. (1935), p. 7), thus also on Friedman. This collection contains Knight’s (previously unpublished) essay Knight (1935) from which I quoted above.

Surprisingly, very few authors have noticed the connection between F53 and Max Weber’s ideal types.²³ Hoover (2009) gets it exactly right when writing

“Friedman (F53, 36) himself refers to perfect competition and monopoly as ideal types, the application of which to concrete cases requires judgment about their suitability and about the objects of the analysis.” (p. 310)

However, Hoover does neither mention the mediating function of Frank Knight in the given case nor does he follow up the connection to Weber. This may be due to the fact that in his paper, Hoover is mainly focused on the causal realism component of F53.

Schliesser (2011) is the only source that realizes connections between Weber, Knight, and Friedman (and Stigler and Parsons). However, Schliesser plays down Weber’s influence on Friedman. With respect to the passages in which Friedman refers to Marshall and his ideal types of firms (F53, 35), Schliesser (2011), p. 542 writes:

“Here one can see Friedman *casually employing* the very Weberian language of “ideal types” and explaining their function in Weberian terms.” (p. 542, my italics)

Neither Hoover nor Schliesser, however, apply ideal types, which occur only in part V “Some Implications for Economic Issues” of F53, to the earlier parts of the essay, thus missing out on the fundamental role they play in F53.

As to why nobody seriously followed up the connection between F53 and Weber, I can only speculate. One reason is certainly the strong disciplinary segregation between economics and sociology after WWII. Clearly, as Frank Knight demonstrates, the transition between these two disciplines was much more fluid before WWII, and even more fluid in Weber’s work itself. A second reason may be that Knight’s influence upon economics waned massively because “by the postwar period his work was relegated to the non-scientific realm of “social philosophy”.”²⁴ In the same way, also Weber’s might have disappeared from sight in economics.²⁵

²³ For instance, in Mäki (2009a) that contains 14 papers on various aspects of F53, Max Weber and ideal types are mentioned only once, in the paper that I am about to discuss in the main text. – I became aware of the other authors who also saw the connection between F53 and Max Weber only after it had sprung to my eyes by F53’s use of the term “ideal types”; searching the internet for “Friedman and Weber” disclosed the other authors.

²⁴ Emmett (2009), p. 111. For example, Frazer and Boland (1983) write: “Knight’s was an antiempirical view of economics. He held instead a complex philosophy of economics as an assumption oriented science [...], but Friedman was to depart dramatically, by 180 degrees, as it were.” (p. 134)

²⁵ Here is some utterly unrepresentative anecdotic evidence. None of the approximately 60 economics students in my graduate classes on the philosophy of economics at the University of Zurich (spring terms 2015 and 2016) could associate an author with the concept of “ideal types” – they had never heard the term –, and none knew anything about Frank Knight.

8. Consequences for F53's as-if methodology

Given my reconstruction of F53's use of ideal types as the basis vectors of the vector space of the real phenomena under consideration, a very clear understanding of F53's as-if methodology emerges. However, first one has to deal with the occurrences of "as-if" *before* the introduction of ideal types (on 16, 18, 19 twice, 20, 21 three times). They are all contained in Section III of F53 entitled "Can a Hypothesis be Tested by the Realism of its Assumptions?" Friedman proceeds in this section by presenting four cases of hypotheses that contain counterfactual assumptions, with only one case from economics. As he shows that it makes sense to use these hypotheses, it is obvious that they cannot meaningfully be tested by the realism of their assumptions, because they would trivially fail the test and would be dismissed. How does Friedman show that these counterfactual hypotheses make sense?

F53 starts with an example from elementary physics that can hardly be contradicted. Applying the Galilean law of free fall to real situations of falling balls on the surface of the Earth is "equivalent to saying that a ball so dropped behaves *as if* it were falling in a vacuum." (16, similarly 18) Clearly, this assumption is counterfactual. The next example is "a constructed one designed to be an analogue of many hypotheses in the social sciences." (19) In this example, an intentionally counter-factual hypothesis is constructed aimed at explaining the density of leaves around a tree. Roughly, the hypothesis is "that the leaves are positioned as if each leaf deliberately sought to maximize the amount of sunlight it receives" (19). The third example concerns the shots by an expert billiard player that are made as if they were analytically calculated (21). The fourth example contends that individual firms "behave as if they were seeking rationally to maximize their expected returns" (21). In all cases, the use of these hypotheses that contain counter-factual assumptions is justified by the fact that for a certain range of circumstances, these hypotheses are capable of generating valid predictions.

The spirit of these examples in Section III appears to be purely instrumentalist: one may use *any* as-if hypothesis for predictions, even if it is wildly counter-factual, as long as it is capable of doing its job, namely producing correct predictions. Clearly, the impression that F53 articulates a purely instrumentalist position is at least partly rooted in these examples. This is, however, not the main message of Section III. Its main message is the negative answer to its title question: "Can a hypothesis be tested by the realism of its assumptions?" This *negative* answer clears the ground for the *positive* treatment of assumptions to which Friedman proceeds in Section IV entitled "The Significance and Role of the "Assumptions" of a Theory".

After the introduction of ideal types in Section V of F53, entitled "Some Implications for Economic Issues", F53 returns to the question of as-if hypotheses. However, only *a very specific sub-class* of the as-if hypotheses discussed in Section III is further considered (36-38), namely, the specific subclass that is relevant for economics. An exemplary case of this subclass is extensively discussed in F53: it is the (real) firm that can be analyzed in terms of two ideal types. Any firm is a linear combination of "monopolistic firm" and "perfect competitor" with coefficients $\alpha \neq 0$ and $\beta \neq 0$, respectively. However, a real firm may be treated "as if it were a perfect competitor for one problem, and a monopolist for another" (36). In other words, for *some* problems a firm may be treated as if $\alpha = 0$ and $\beta = 1$, and for *other* problems as if $\alpha = 1$ and $\beta = 0$. Thus, according to the later parts of F53, an as-if hypothesis used in eco-

nomic analysis is not just *any* counterfactual assumption with predictive power, but the *temporary* counterfactual identification of a real type with one of the associated ideal types, *depending* on the problem in question.

This is how F53 describes the general situation in science:

“A meaningful scientific hypothesis or theory typically asserts that certain forces are, and other forces are not, important in understanding a particular class of phenomena.” (40)

In the case of the falling stone near the surface of the Earth, the “important” force is gravity, and the (main) unimportant force is air friction. Although the real force acting on the stone is given by the vector addition of the two forces, for the given phenomenon we can neglect air friction. In the case of a firm, its real behavior is given by the linear combination of the two ideal types. For particular situations, however, we may neglect the contribution of one of the ideal types and treat the firm as if it was just the other ideal type. – F53 continues after the above quote:

“It is frequently convenient to present such a hypothesis by stating that the phenomena it is desired to predict behave in the world of observation *as if* they occurred in a hypothetical and highly simplified world containing only the forces that the hypothesis asserts to be important. In general, there is more than one way to formulate such a description – more than one set of “assumptions” in terms of which the theory can be presented.” (40)

Here it becomes clear that F53’s “assumptions” refer to the temporary, i.e. problem-specific identification of a real type with one of the pertinent ideal types. From this, it follows that

“[s]uch a theory cannot be tested by comparing its “assumptions” directly with “reality.” Indeed, there is no meaningful way in which this can be done.” (41)

Of course, there can be no direct test of the usefulness of the temporary, willfully counterfactual identification of the real type with one of the associated ideal types. The question regarding the quality of such a theory

“can be settled only by seeing whether it yields predictions that are good enough for the purpose in hand or that are better than predictions from alternative theories.” (41)

This can again be (mis-)read as an instrumentalist statement, which would miss the functions that ideal types play in Friedman’s (and Weber’s and Knight’s) methodology. Instead, ideal types isolate and idealize *real things*, for instance gravity in physics or “forces” working in an economy. They are thus far away from being just arbitrary fictions. Instead, by being the basic vectors of the pertinent vector space of real phenomena, they are the building blocks out of which descriptions of real phenomena can be constructed. Thus, despite its rhetoric, there is a substantive dose of realism in F53’s position.

9. Revisiting the puzzling statements of, and facts about, F53

Given our analysis of various aspects of F53, we can now revisit the most puzzling statements of, and facts about, F53 as outlined in Section 2. We will have reached the interpretive and re-constructive goal of this paper if most of the puzzlement disappears.

9.1 Economics as an “objective” science

“In short, positive economics is, or can be, an “objective” science, in precisely the same

sense as any of the physical sciences.” (4)

Note first that there are scare quotes around “objective”, so Friedman is not exactly sure what “objective” means.²⁶ However, he thinks that he does not have to address this problem as long as there is a sufficient similarity to physics whose status as “objective” is unchallenged (at least in the given context). Remember that Friedman discussed at length (3.5 pages, 16-19) the law of falling bodies, at the beginning of Section III entitled “Can a hypothesis be tested by the realism of its assumptions?” His answer to this question is in the negative, because, in the case of economics, the “assumptions” of hypotheses contain ideal types, as we have seen. In physics, here in the concrete case of the law of free fall, the assumptions also contain idealizations, among them the neglect of air friction, or, equivalently, the assumption of a vacuum. So there is a strong parallel between physics and economics regarding the nature of their “assumptions”. The parallel extends further, namely to the conditions under which a hypothesis of this kind is accepted. In the case of free fall, the “formula is accepted because it works” (18). However, this holds for stones and not for feathers. Similarly, an explanation of a real type by an ideal type works within certain limits.

The upshot for F53 is: physics works with idealizations in quite the same way as economics does with ideal types. Because physics counts as objective, so does economics.²⁷

9.2 *Economic theory as “a set of tautologies”*

Here is F53’s puzzling statement about economic theory again:

“Viewed as a language, theory has no substantive content; it is a set of tautologies. Its function is to serve as a filing system” (7)

I note first in passing that also this statement does not really fit an instrumentalist understanding of the function of theories. More importantly, in the positivist tradition definitions were often called “tautologies”. Given our analysis, it is clear that Friedman means ideal types that have to be introduced by stipulative definitions as the first step of theory building. Of course, the set of ideal types by itself “has no substantive content”. It is indeed a “filing system” if we think of the ideal types as the basic vectors in the vector space of the representations of the pertinent real phenomena. Every real type, i.e. every real phenomenon, can then be represented as a linear combination of ideal types (see Section 6, above).

9.3 *“Assumptions” in scare quotes*

As noted before, the term “assumption(s)” occurs 73 times in F53 of which 37 occurrences (50%) are in scare quotes. The reason for the scare quotes should now be clear. In the usual sense, an assumption is usually something that is (tentatively) presumed to be at least approximately correct, although this is not really provable.²⁸ All these connotations are inappropriate

²⁶ Oddly enough, also Max Weber uses scare quotes around “objectivity” in the same context, namely, in the German original title “Die “Objektivität” sozialwissenschaftlicher Erkenntnis”, translated as “Objectivity of Social Science and Social Policy” (in some instances printed without scare quotes): Weber (1949 [1905]).

²⁷ I am not endorsing this argument, I am only presenting it and am trying to make Friedman’s thinking plausible. For a fuller comparison of the objectivity of physics and economics, both disciplines should be embedded in a general framework that makes meaningful comparisons between disciplines possible. For such a possible framework see, e.g., Hoyningen-Huene (2013).

²⁸ According to the Oxford Living Dictionaries, an assumption is “a thing that is accepted as true or as certain to happen, without proof”: <https://en.oxforddictionaries.com/definition/assumption>, accessed January 9, 2017.

for ideal types. According to F53, “assumptions” (with scare quote) in economic theory importantly comprise ideal types, and criticism regarding their realism is completely misguided (16-23).

9.4 “[T]he more significant the theory, the more unrealistic the assumptions”

Here is again the apparently most objectionable sentence of F53:

“Truly important and significant hypotheses will be found to have “assumptions” that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense).” (14)

The first part of the sentence should now be clear: “assumptions” are here ideal types and indeed, they are “wildly inaccurate descriptive representation of reality”. The significance of a theory derives from its “analytical relevance” (33, 34) which in turn is the work of appropriately chosen ideal types. Ideal types surgically extract the fundamental characteristics of the phenomena in question, especially the “forces” that are “important in understanding a particular class of phenomena” (40). Thus, the sharper the ideal types abstract from the inessential features, the less descriptively realistic and the more analytically relevant they are. This explains why an increase in significance of a theory implies a lowering of the degree of realism of its ideal types.

9.5 *Descriptive accuracy vs. analytic relevance*

“The basic confusion between descriptive accuracy and analytical relevance that underlies most criticisms of economic theory on the grounds that its assumptions are unrealistic ...” (33)

The confusion between descriptive accuracy and analytical relevance concerns ideal types. In the Weber-Knight-Friedman picture of economic theory, ideal types can only be analytically relevant if they are unrealistic (see Section 9.4, above).

9.6 “Appearances are deceptive” vs. “a more fundamental and relatively simple structure”

F53’s full quote is:

“A fundamental hypothesis of science is that appearances are deceptive and that there is a way of looking at or interpreting or organizing the evidence that will reveal superficially disconnected and diverse phenomena to be manifestations of a more fundamental and relatively simple structure.” (33)

Again, on an instrumentalist reading, this passage does not make sense: there simply is no “more fundamental and relatively simple structure”. In the most favorable case, there is just a model that produces many good predictions; talk of appearances being deceptive (because they do not disclose by themselves an underlying structure) does not make sense for the instrumentalist. However, on an ideal-type methodology reading, the quote does make sense: the idea is that the appearances have to be decoded in terms of the pertinent ideal types which lead us to the essential aspects of the phenomena.

9.7 *The extensive, but unreferenced use of Popper*

At first sight, there is an obvious answer to the question why F53 extensively uses Popper. As

far as scientific objectivity is concerned, F53 tries to assimilate economics to physics. Popper attempted to explicate the methodology of empirical science, best exemplified by “modern theoretical physics”.²⁹ Therefore, Popper’s philosophy of science is an appropriate resource for the development of economic methodology.

However, due to the ideal type methodology there is a more specific reason for the use of Popper in F53. The introduction of ideal types for some type of situation is not directly empirically controlled; it is highly hypothetical or even speculative. On the basis of familiarity with the realm of pertinent phenomena, a researcher “isolates the features that are crucial for a particular problem” (36) and combines them into a “unified thought construct”, as Max Weber put it (see Section 6, above). Clearly, such a thought construct is empirically very poorly controlled. F53, like Popper, accepts the distinction between the “context of discovery”, in which one is free to invent testable hypotheses, and a “context of justification”, in which these hypotheses are as severely tested as possible.³⁰ Identically to Popper, he claims that the “construction of hypotheses ... must be discussed in psychological, not logical, categories” (43). However, in the second half of the following quote Friedman adds something specific:

“The construction of hypotheses is a creative act of inspiration, intuition, invention; its essence is the vision of something new in familiar material.” (43)

On the basis of the given analysis, it is very plausible to construe “the vision of something new in familiar material” as the result of the analysis of a known phenomenon in terms of a set of appropriate ideal types. However, if economics is to be a science, one needs strong measures of empirical restriction for such speculative hypotheses that are the results of “creative acts of inspiration”. It is exactly this what Popper’s philosophy delivers. So the ideal type methodology cries out for explicit strict empirical control if one wants to avoid the smell of psychologism that was often associated with a *verstehen* methodology (Max Weber called his kind of sociology *verstehende* [interpretive] sociology).³¹ The problem of a possible conflict between Popper’s refusal of instrumentalism and F53 dissolves because F53 contains a substantive dose of realism.

10. Conclusion

We have seen that Friedman’s F53 is far from an instrumentalist manifesto. However, it can also not be read realistically as some authors have tried. Both readings of F53 miss a sophisticated element of Friedman’s methodology which consists of sociologist Max Weber’s methodology of ideal types. Friedman was familiar with some of Weber’s writings through his Chicago teacher Frank Knight, who was an ardent admirer of Weber. Both Weber’s and Knight’s influence on F53 have been overlooked because nowhere in F53 have they been cited nor mentioned. In addition, the break between economics and sociology that occurred after the 1930s led to less sociologically knowledgeable economists, so Weber’s influence on F53 was not immediately visible to them. However, drawing on Max Weber was methodologically dangerous for Friedman insofar as Weber’s ideal type methodology was part and parcel of his

²⁹ “[In] modern theoretical physics ... I and others see the most complete realization to date of what I call ‘empirical science’”: Popper (1959 [1934]), p. 38.

³⁰ For Popper, see Popper (1959 [1934]), Chapter 1, Section 2, p. 31. Friedman does not use the terms “context of discovery” and “context of justification”. For an extended discussion of the context distinction, see Hoyningen-Huene (1987).

³¹ See, e.g., the subtitle of Weber’s *Economic and society* which is *An outline of interpretive sociology*: Weber (1968).

“interpretive sociology”, and in many circles, the *verstehen* component had the very bad smell of empirically uncontrollable speculation. This is where the philosophy of Karl Popper comes in; however, also Popper is neither cited nor mentioned in F53. By relegating the ideal type methodology to the context of discovery, F53 opens the door for strict empirical control in the context of justification: testing hypotheses by their predictions, irrespectively of what happened in the context of discovery. This was the lesson F53 drew from Popper’s philosophy, developed by Popper with physics in view. It is the proposed testing procedures of F53 that mainly captured the imagination of economists, apparently assimilating economics to physics, leaving however unresolved puzzles about some passages of F53. These passages can be deciphered by reference to the largely implicit ideal type methodology. In addition to leaving the readers completely in the dark about three of its most important sources, F53 excessively uses scare quotes around some of its central terms. Scare quotes only signal what the author does *not* mean, and not what the author does mean. They thus provide additional hurdles for interpretation. In summary, it is the missing references to vitally important sources and the excessive use of scare quotes which explain why the controversial discussion about F53 had been carried on for such a long time.

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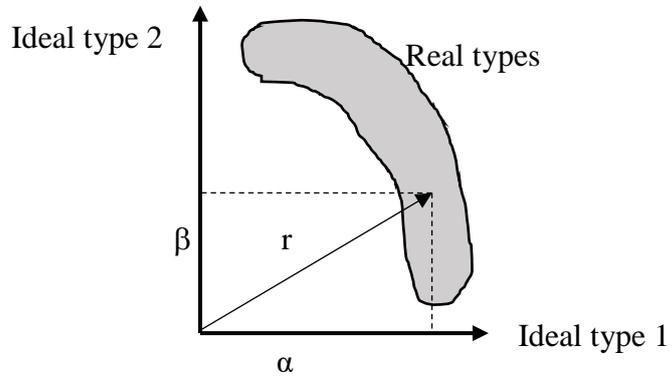


Figure 1