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Revisiting Friedman's "On the methodology of positive economics" ("F53")

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Abstract:
In this paper, I shall defend two main claims. First, Friedman's famous paper "On the methodology of positive economics" ("F53") cannot be properly understood without taking into account the influence of three authors who are neither cited nor mentioned in the paper: Max Weber, Frank Knight, and Karl Popper. I shall trace both their substantive influence on F53 and the historical route by which this influence took place. Once one has understood these ingredients, especially Weber's ideal types, many of F53's astonishing sentences like "the more significant the theory, the more unrealistic the assumptions", make good sense. Second, I shall claim that the much-discussed question whether Friedman's essay espouses an instrumentalist or a realist position, is the wrong question to be asked. I shall illustrate that by a comparison with examples from physics in which also unrealistic assumptions are made. Also there, the question whether these assumptions are indicators of instrumentalism or realism is not appropriate. Cleared from these misunderstandings, F53 presents itself as an interesting and reasonable but much less controversial contribution to the methodology of economics.

1. Introduction

Friedman's famous 1953 essay "On the methodology of positive economics" (Friedman (1953): "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); it is "the centerpiece of postwar economic methodology" (Blaug (1980), p. 103). Regarding its echo in the literature, there were 7,900+ Google citations until Jan 2022 (out of 244,000+ 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:

> The idea that unrealistic "assumptions" are nothing to worry about, provided that the theory deduced from them culminates in falsifiable predictions, carried conviction to

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

> economists long inclined by habit and tradition to take a purely instrumentalist view of their subject.[2]

However, also this purely instrumentalist reading of F53 is not uncontroversial.[3] Alternatives 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".[4] Of course, all authors find passages in F53 supporting their specific interpretation.

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?

In this paper, I shall claim that many of the problems that F53 presents are due to the substantial influence of three authors on the paper who are neither cited nor mentioned: Max Weber, Frank Knight, and Karl Popper. Weber's influence on F53 is indicated by several occurrences of his technical term "ideal type", but his influence has hardly been discussed in the literature. Knight emerges as an indirect contributor to F53 only when one asks how Friedman may have known of Weber's work. The influence of Popper's falsificationism on F53 has already been seen in the 1970s,[5] but its precise role in F53 seems to be under-analysed. This paper is motivated by the goal to get the methodological position of one of the most important economists of the 20th century right.[6] I shall suggest that F53 has been mostly misread in the past. Furthermore, F53 may contribute to the ongoing discussion of the role of models in economics.[7]

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. As an interlude, I shall briefly discuss in Section 3 the question why F53 is so difficult to read and admits of so many different interpretations. In Section 4, I shall turn to the question whether F53's position is instrumentalist. This will lead us in Section 5 to the problem what Friedman means by assumptions in scare-quotes. In Section 6 I discuss F53's connection to Weber's ideal types; Section 7 explains Knight's mediating role in this connection. Section 8 then explains the role of Popper's philosophy for F53. In Section 9, I shall turn again to the instrumentalism question. In the final Section 10, I revisit the list of irritating statements and strange facts about F53 from Section 2, and show how much of the puzzlement dissolves under the new reading.

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[2] Blaug (1975), p. 399; for a more recent, similar understanding see Rodrik (2015), pp. 25-26.
[3] See, e.g., Caldwell (1980) in response to Boland (1979).
[4] Mäki (2009c); this was strongly objected by Mariyani-Squire (2017) who defended F53's "instrumentalist stance" (p. 69).
[5] For instance, Blaug claimed in 1975 "that Friedman is simply Popper with-a-twist applied to economics", Blaug (1975) p. 399; see also Latsis (1976).
[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, the articles of Part 4 of Mäki (2009a), and Forder (2019), pp. 159-195.
[7] See, e.g., Sugden (2000) and the ensuing extensive discussion.

2. Irritating statements in 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[8], 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 serves as 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

[8] Naked page numbers refer to the original pagination of F53, as in the facsimile reprint in Mäki (2009a).

understand how the unrealisticness of "assumptions", that is their missing descriptive accuracy, could be a particular *virtue* of significant theories.

2.5 Descriptive accuracy vs. analytic relevance

The following passage 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, and that this structure 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, almost seven 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).[9] 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."[10] 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.[11] 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 the most central epistemological concepts of F53 make its reading very difficult. Typically, for the already initiated reader the use of scare quotes may be illuminating: lacking a better expression, scare quotes 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:[12] 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 presumed instrumentalism.

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[9] As far as I can see, only one other author explicitly noted Friedman's repeated use of scare-quotes: Schliesser (2005), p. 53 and 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).

[10] http://www.biomedicaleditor.com/hedging.html, accessed January 17, 2022.

[11] 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.

[12] Mayer (2009), p. 122; "philosophically naïve and confused", p. 139.

4. Is F53's position instrumentalist?

Before answering this section's title question, we should ask what instrumentalism, and its counterpart realism, is. Here is a rough answer.[13]

Instrumentalism is a (metatheoretic) position concerning the interpretation of theories (and hypotheses); its natural counter-part is realism. An instrumentalist evaluates theories solely according to their predictive power, that is, which correct empirical predictions can be derived from them (predictions not necessarily concerning the future). In this view, theories are thus just instruments with the purpose to produce correct empirical predictions. In this view, the question whether theories are true or false (or something similar) does not really come up, in the same way as the question whether scissors are true or false cannot come up. The adequate question regarding instruments is instead whether they fulfill their intended function or not. Realism, on the other hand, is more ambitious than instrumentalism. Realists want to infer from the empirical success of theories that they are at least approximately true (or some variant of this). This implies especially that the possibly unobservable entities that the theories refers to really exist. Roughly speaking, for the instrumentalist good theories are black boxes that produce correct predictions, whereas for the realist they represent reality at least in an approximate sense (and therefore produce correct predictions).

Note that the concepts of instrumentalism and realism come in two different versions, wholesale and retail.[14] In the wholesale version, people are called instrumentalists (realists) if they interpret *all* established theories of their discipline instrumentalistically (realistically). In the retail version, only single theories are at issue. In the retail version it is possible to say, for instance, I interpret theory A realistically whereas I interpret theory B instrumentalistically. Note that in the sciences themselves, the controversies about realism vs. instrumentalism exclusively concern particular theories, that is retail realism and instrumentalism. For instance, a new physical theory that produces stunning empirical predictions by introducing new kinds of entities, may in the beginning be interpreted by the majority of physicists only instrumentalistically. It may take a while until a substantive proportion of physicists is persuaded that the newly postulated entities really exist.[15] A consequence is that a scientist may be a wholesale realist but a retail instrumentalist regarding one particular theory. In philosophy, by contrast, the controversy about instrumentalism vs. realism mostly concerns the wholesale versions. This discussion is typically centered around the question whether and under which circumstances empirical success of theories licenses their realistic interpretation.

Clearly, in so far as in F53 instrumentalism vs. realism is the the issue, it is the wholesale versions that are concerned. Friedman speaks about economic theories and hypotheses in general, and specific cases serve as illustrations only. F53 indeed contains many passages that seem to support a wholesale instrumentalist position, and as I mentioned in the introduction, 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:[16]

> "Truly important and significant hypotheses will be found to have "assumptions" that are wildly inaccurate descriptive representations of reality, and, in general, the more

[13] A detailed exposition can be found, for example, in Chakravartty (2017).
[14] This important distinction has been introduced by Magnus and Callender (2004).
[15] This holds, for instance, for Copernicus' theory and the theory of quarks.
[16] 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.

> 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)

Three important things in this passage should be noted.

First, the critical subject of this statement are "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 is somehow similar to assumptions (in the usual sense)—thus the term "assumptions", but
- it is also significantly different from assumptions (in the usual sense)—thus the scare quotes around "assumptions".

What Friedman means by "assumptions" in scare quotes remains unclear in this passage – and elsewhere in F53 (I will come back to this question in the next section).

Second, in the quote Friedman states that "in general, *the more significant* the theory, *the more unrealistic* the assumptions (in this sense)." This, however, is not compatible with instrumentalism because instrumentalism passes no judgement whatsoever on the realistic or unrealistic character of "assumptions" (whatever they are) contained in hypotheses.[17] Instrumentalism judges hypotheses exclusively according to their predictive power.

Third, it should be noted that the statement seems not to be compatible with any form of realism either because for realism, it apparently cannot be *a virtue* of a theory to contain unrealistic "assumptions" (whatever they are). For the realist, unrealistic assumptions may be tolerable at best, but certainly not laudable, as F53 has it.

Now we seem to be in a dilemma because the passage seems neither compatible with instrumentalism nor with realism. The core of the dilemma is that F53 *praises* "assumptions" for their massive descriptive falsity (or inaccuracy or unrealisticness). For assumptions in the normal sense, this is not comprehensible, neither under realist nor under instrumentalist presuppositions. The only way out of this dilemma is to take seriously the scare quotes around "assumptions" that F53 features abundantly.[18] What does Friedman mean by "scare-quote assumptions" (as I shall call them)? Only after we have understood this, which forces us to a longish detour, may we come back to the instrumentalism-realism issue in Section 9.

5. What are F53's "assumptions"?

Although F53 does not present a clear statement what scare-quote assumptions are, we can extract their meaning from F53, especially from its examples.

5.1 "Assumptions", 1st type: descriptive falsity due to abstraction and idealization

[17] 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.

[18] In his analysis of F53, Musgrave (1981) tries out three different meanings of "assumptions". However, none of these interpretations makes the quote (14) intelligible.

Immediately after the F53 (14) quote discussed in the last section, Friedman declares:

> "A hypothesis is important if it "explains" much by little, this is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomenon to be explained and permits valid predictions on the basis of them alone." (14, my italics, similarly on 40)

A physical example in F53 is the freely falling ball whose behavior is explained by recourse to gravity alone, by abstraction from all other forces. Even gravitation enters the scene in the idealized form of the law of free fall that sets the gravitational force constant (16-19).[19] An economic example is the effect of a tax increase on retail prices of cigarettes, which can be explained by recourse to the competition of firms alone, thereby abstracting from all other factors (36-37). Friedman continues: "To be important, therefore, a hypothesis must be false in its assumptions" (14-15). The falsity of the assumptions is generated by two operations. First, the most relevant factor is idealized and thereby simplified. In the case of free fall, instead of the varying gravity along the trajectory according to Newton's theory, constant gravity is assumed. In the cigarette case, perfect competition is assumed instead of imperfect competition. Second, it is abstracted from all other factors contributing to the real situation. In the case of free fall, other forces besides gravity act on the falling body, in the cigarette case, other factors may influence the market. Friedman continues:

"[T]he relevant question to ask about the "assumptions" of a theory is not whether they are descriptively "realistic," for they never are, but whether they are sufficiently good approximations for the purpose in hand. And this question can be answered only by seeing whether the theory […] yields sufficiently accurate predictions." (p. 15)

Thus, a concrete situation is treated as if only a few, even idealized factors were present, which is descriptively false, and this procedure is justified by sufficient predictive success.

Schematically, Friedman's characterization of the case is as follows:

- A concrete, complex situation S is treated as if it was a drastically simpler situation S*.
- Clearly, as a description of S, S* is utterly false.
- The use of S* instead of S is justified by the fact that it can be scientifically treated (for example, it can be exactly described in mathematical terms), *and* it yields sufficiently good predictions for S.

The specific falsity of S* may be called "descriptive falsity due to abstraction and idealization". It may be noted that in this case, the degree of descriptive falsity of S* can be decreased by adding corrections which partly revert the abstraction and idealizations. For example, in the case of free fall air friction may be added; instead of the law of free fall with constant acceleration, Newton's law with variable gravitation may be used; etc.[20]

[19] Friedman is wrong in saying that "[I]t is an accepted hypothesis that the acceleration of a body dropped in a vacuum is a constant—g" (16) because the acceleration is not constant. Setting it constant is an additional idealization. However, this does not affect the thrust of Friedman's argument. - For a detailed discussion of the example see Schliesser (2005).

[20] In his paper on three kinds of idealization, Weisberg (2007), pp. 640-642, 655 calls this kind "Galilean idealization".

However, Friedman also uses another method to generate descriptively false, but scientifically fruitful situations S*, unfortunately without making the difference to the 1st type explicit.[21] Of course, this contributed severely to the confusion about F53.

5.2 "Assumptions", 2nd type: descriptive falsity due to abstraction and substitution

In this second way, S is not only simplified by abstraction, but also a relevant factor F in S is substituted by a qualitatively different factor F* that is unreal, yielding S*. This abstract characterization becomes immediately transparent when considering Friedman's examples. First, he discusses the leaves of the tree which are treated as if they individually sought to maximize the amount of sunlight (F*), which they do not (19-20). The second example concerns an expert billiard player who is treated as if she solved the relevant differential equations in order to calculate the optimal shot (F*), which she does not (21). Finally, the same pattern exists in economics when firms are treated as if they were seeking rationally maximal expected returns (F*), which they do not (21-23). In all these cases, the substitution of F by F* is motivated by the claim that in the given situation S, F* has (approximately) the same effect as F but is easier to handle scientifically.

The claimed effect equivalence of F and F* is preliminarily justified by a theory that connects F and F*. In the three examples, the connecting theory is (a sketch of) selection theory: the optimizing effects of *selection processes* on F results in a F that is as optimal as an *optimal rational choice process* F*. However, the *ultimate* justification for the substitution of F by F* in S* is that it yields sufficiently good predictions for S (as in "assumptions", 1st type). Clearly, as a description of S, S* is false. In this second case, the specific falsity of S* may be called "descriptive falsity due to abstraction and substitution". In contrast to the first case, the transition from S to S* cannot be gradually reverted by adding corrections, because F* is a qualitatively different substitute for F. One problem of understanding F53 is that whenever Friedman speaks *in general* about false assumptions in economics, he in fact refers to "descriptive falsities due to abstraction and idealization" and not to "descriptive falsity due to abstraction and substitution".[22]

It should be noted that also physics contains not only "descriptive falsities due to abstraction and idealization" which Friedman exclusively uses (law of free fall), but also "descriptive falsities due to abstraction and substitution" (2nd type). This is important because it sheds light on the question whether the use of such descriptive falsities is an indicator of an instrumentalist position of the respective author. For example, a very important model in nuclear physics is the liquid drop model of the atomic nucleus, on the basis of which, for instance, the first atomic bombs were built.[23] The liquid drop model counterfactually assumes that nuclear matter is an incompressible continuous liquid with surface tension and viscosity, as opposed to being composed of discrete particles, which is the realistic view. The corpuscular structure of the nucleus is thus *substituted* by a continuous liquid. In a strict sense,

[21] Other authors have also seen that F53's "assumptions" are not all of the same kind: see, for example, Mäki (2009c), pp. 99-101, 104-106; Kuorikoski, Lehtinen, and Marchionni (2010), p. 547; Lehtinen (2013).

[22] In his paper on three kinds of idealization, what Weisberg (2007), pp. 642-645, 655 calls "minimalist idealization" is very close to what I have called here "descriptive falsity due to abstraction and substitution". Weisberg writes about one of such minimalist models that "it is extremely simple, building in almost no realistic detail about the substances being modeled. What it seems to capture are […] the core causal factors giving rise to the target phenomenon", pp. 642-643.

[23] See, e.g., Stuewer (1994) and Nyrup (2020).

this is not an approximation to the real situation (it cannot be improved by adding corrections), but a qualitatively counterfactual assumption. Nevertheless, in the dialect of physicists such a substitution may be called an approximation. Interestingly, physicists accepted such a model exactly for the reasons that Friedman describes as relevant for model choice:

- The model is good enough regarding its predictions for the given purpose (14-15)
- An alternative model that is predictively better is not available (at the time of the introduction of the model) (23, 31)

Given the understanding of scare-quote assumptions that we have reached now, we can characterize their difference to "ordinary" assumptions. Lexica tell us that an assumption is "a thing that is accepted as true or as certain to happen, without proof", or "a fact or statement (such as a proposition, axiom, postulate, or notion) taken for granted".[24] Thus, an ordinary assumption implies a certain commitment to its truth, but without solid epistemic grounds. By contrast, scare-quote assumptions in Friedman's sense do not imply any epistemic commitments, they are just used as an *ansatz*. For example, when an economist models a certain economic situation with agents who are *homines economici*, this does not commit her to the belief that human beings really behave like *homines economici*. It is just an *ansatz* that is evaluated according to its predictive power in the pertinent situation. It is this difference to ordinary assumptions that motivated Friedman to put scare-quotes around the term "assumptions". However, it would have been tremenduously better if Friedman had made explicit what he means by scare-quote assumptions, insead of just using scare quotes without explanation.

Now I will investigate what Friedman has to say in more concrete terms about the descriptive falsity of his scare-quote assumptions. He does so in Section V, in which he turns to "some implications for economic issues" (30), after having discussed the "abstract methodological issues" (30) that we have hitherto treated.

6. F53's connection to Weber's ideal types

We can see what "assumptions" concretely refer to in passages in which Friedman connects descriptive accuracy, analytical relevance and ideal types.[25] First, descriptive accuracy and analytical relevance are not the same.[26] Second, analytic relevance implies not being realistic in the sense of descriptively accurate.[27] Finally, at this point ideal types come into play: they are not descripively accurate (on the contrary, they are intentionally descriptively false), but

[24] https://www.lexico.com/definition/assumption and https://www.merriam-webster.com/dictionary/assumption, accessed January 19, 2022.

[25] It should be noted that F53 uses the expression "ideal types" six times: 34, 35 (three times), 36 (twice). In addition, F53 speaks on p. 36 of "ideal and real entities in a particular problem", which refers to ideal and real types. Therefore, the expression should be taken seriously.

[26] The "confusion between descriptive accuracy and analytical relevance […]", 34, 35.

[27] "[T]he more significant the theory, the more unrealistic the assumptions", 12. - However, 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" (fn. 12). 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).

they are analytically relevant for a particular problem situation. In other words: they function as scare-quote assumptions.[28] Thus, ideal types for Friedman

- are intentionally not descriptive (34, 36),
- do not directly and fully correspond to entities in the real world (34),
- are chosen in dependence of the purpose of the model (34): their function is "to isolate the features that are crucial for a particular problem" (36, 14).[29]

Ideal types are, of course, part and parcel of Weber's 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 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*.)

Here is an illustration of ideal types (Weber (1958 [1922])). There are three ideal types of "legitimate rule" (*legitime Herrschaft*, also translated as "legitimate authority"); in legitimate rule, 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 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:

[28] "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).

[29] It is, of course, a triviality for every model builder that a model should "isolate the features that are crucial for a particular problem".

- They exhaust all *pure* types of legitimate rule
- They are mutually exclusive
- Real cases of legitimate rule, the "real types", are mixtures of the ideal types.

Friedman's 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).[30] Clearly, also these two ideal types fulfill Weber's above-mentioned three fundamental properties.

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 (real) 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.

According to this analysis, economic models have two heterogeneous elements. First, there is a set of ideal types that span the vector space of the real phenomena in question—F53 calls them the "abstract model" (35). Second, there are rules how to analyze real types in terms of the given ideal types (35-36). In the given reconstruction, these are rules how to determine the coefficients α and β. In Friedman's example of the cigarette industry, the ideal types are perfectly competitive firms and monopolistic firms. In some concrete situations, it works well to treat the firms as if they were perfect competitors, that is to put $\alpha = 1$ and $\beta = 0$; in other situations, this does not work (36-38).

It should be noted that an analysis in terms of ideal types also works well for Friedman's main physics example, free fall, although it would in physics not be called an ideal type, but an idealization. The two ideal types would be free fall in a vacuum and free fall with strong friction (which leads to constant velocity). For low velocities, free fall in air can be treated as if it was free fall in a vacuum.

[30] 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.

Even if one finds the analysis of F53 in terms of Weberian ideal types plausible, there is a problem: Weber is nowhere cited. Did Friedman know Weber's work in 1953?

7. On Knight's mediating role

Is is plausible that Friedman uses Weber's work without citing it? Given that there is one more author that Friedman uses without citation (Popper, see Section 8), this can certainly not be excluded. But are there indicators that Friedman knew of Weber's work?

According to American sociologist Edward Shils, Friedman attended a seminar on the work of 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 Shils[31] himself and later Nobel laureate in economics George Stigler.[32] 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.[33] Knight was not only the first translator of one of 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" (Noppeney (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:

> 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

[31] Edward Shils became a well-known sociologist and also a translator of Max Weber: see Weber (1949).
[32] Shils (1981), p. 184; Shils and Grosby (editor) (2006), pp. 3, 50.
[33] 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."

> 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).[34] 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), Friedman 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

> 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 Weber's ideal types.[35] Hoover (2009), p. 310 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.

However, Hoover neither mentions 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 author who realizes the connections between Weber, Knight, and Friedman (and Stigler and Parsons).[36] 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:

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

[35] For instance, Mäki (2009a) contains 14 papers on various aspects of F53, but Weber is mentioned only once and insignificantly: Mayer (2009), p. 129. Donato Rodriguez (2016) sends a half page on F53's use of Weber's ideal types, p. 96

[36] Also Galbács (2019) refers to this connection, however after having read a precursor version of the present article, see p. 37.

> 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'".[37] In the same way, also Weber's work might have disappeared from sight in economics.[38]

8. F53's connection to Popper' philosophy

Given that we have established now the central role of Weber's ideal types methodology in F53, we may inquire after F53's connections to Popper's philosophy. In fact, there are three questions. First, Popper is not cited in F53. Where are Popper topics in F53? Second, is there a historic connection between Friedman and Popper? Third, is there a substantive reason why Friedman used Popper's philosophy?[39]

First, F53 has many 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.*[40]

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[37] 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)

[38] 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.

[39] For those who saw F53 as an instrumentalist manifesto, the additional question arose how to reconcile this thrust with Popper's declared anti-instrumentalism; see, e.g., Frazer and Boland (1983) and Pheby (1991 [1988]), p. 88. I shall not discuss this question because it dissolves, as we shall see in Section 9,

[40] 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). Deduced facts must be "well enough defined so that observation can show them to be wrong" (p. 13). A more attractive hypothesis "has more implications capable of being contradicted, and has failed to be contradicted under a wider variety of circumstances" (20). "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).

Second, F53 appeared in 1953, the English edition of Popper's *Logic of Scientific Discovery* appeared only in 1959, and the original German edition appeared in 1934, but apparently Friedman did not read German. So how did Friedman get access to Popper's philosophy? The answer to this question was 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'.[41]

Third, why does F53 use Popper's falsificationism at all? The obvious answer is that Friedman wanted to assimilate positive economics to the physical sciences (4, 25, 30), and Popper attempted to explicate the methodology of empirical science, best exemplified by "modern theoretical physics".[42] Therefore, Popper's philosophy of science appears to be an appropriate resource for the development of economic methodology.

However, there is a deeper reason for the attractiveness of Popper's falsfication ism to Friedman. According to the ideal types methodology, economic theorizing contains a highly hypothetical or even speculative element, namely the identification, or rather construction, of the relevant ideal types for a specific problem. 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 Weber put it (Weber (1949 [1905]), partially reprinted in Weber (2008 [1949, 1905]), p. 71). 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.[43] Identically to Popper, Friedman 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. All this happens in the context of discovery. 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". In other words, we need strong rules for the context of justification. In Friedman's understanding, this is exactly what Popper's philosophy delivers. It should be noted that the ideal type methodology cries out for explicit strict empirical control if one wants to avoid, for instance, the smell of

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[41] http://hayekcenter.org/?p=5317, accessed 8 Jan 2017. Unfortunately, this interview was not well documented and does not seem to be available anymore (Jan. 2022). 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 and hinted at in Mäki (2009c), p. 93 fn. 3.

[42] "[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.

[43] 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).

psychologism that was often associated with a *Verstehen* methodology. After all, Weber called his kind of sociology *verstehende* [interpretive] sociology.[44] In empiricist circles, all *Verstehen* smacked of speculation and missing empirical control. So Friedman's marriage of Weber' ideal types with Popper's falsificationism indeed promised economics to be a respected science.

9. Again: Is F53's position instrumentalist?

After the long detour via Weber, Knight and Popper we can finally come back to the question already asked in Section 4: Is F53's position instrumentalist? Of course, the position at issue is wholesale instrumentalism (see Section 4). Thus the question is: Do economic theories and hypotheses in general claim to represent reality more or less accurately, or are they mere instruments for prediction without *any* claim to realistically represent the systems in question?

Friedman's central claim is that economic hypotheses contain "assumptions" that are descriptively false but analytically relevant. I have distinguished two types of "assumptions": "falsities due to abstraction and idealization" (e.g., free fall, perfect competition) and "falsities due to abstraction and substitution" (e.g., differential equations solving billiard players, rationally calculating managers). Both types of "assumptions" are ideal types: for example, "perfect competition" and "rational behavior of management". Using these ideal types in hypotheses implies the claim that they represent, even in their idealized form, the relevant causal factors in the given problem. Trivially, these counterfactual claims cannot be directly empirically tested, but must demonstrate their usefulness by successful predictions.

In the first case of "falsity due to abstraction and idealization", one *real* factor is highlighted and idealized (e.g., competition), and other factors neglected. In this case, the ideal type approach is clearly realistic, because the pertinent ideal type directly targets real factors (e.g., competition). In the second case of "falsity due to abstraction and substitution", one real factor is highlighted but substituted by another factor that is, in a credible way, causally equivalent to the real factor (e.g., optimized behavior in billiard or in management: substituted by behavior based on rational calculations). In this case, the ideal type approach has an instrumentalist flavor, but is certainly not fully instrumentalist. As explained in Section 4, full instrumentalism treats the real system as a black box and tries to model the relevant behavior (typically input-output relations) by whatever means; the *only* evaluative standard is predictive success. In Friedman's examples, a part of the real system is substituted by something else, for whose functional equivalence arguments have to be given (although finally, predictions are decisive). For example, *real* thinking process by expert billiard players or successful managers are substituted by the respective *rational calculations*. The argument for the substitution is that the success of the actors is only explainable if it is equivalent to the result of rational behavior. Thus, an ideal-type methodology is realist by targeting on the most important *real* factors, but is willing to replace scientifically intractable elements by, for example, equivalent rational substitutes. Does this move make the ideal-type methodology instrumentalist?

A comparison with physics is useful at this point. A physicist using Galileo's law of free fall or the liquid drop model of the atomic nucleus would not at all see herself *thereby* committed to (wholesale) instrumentalism. A realist interprets Galileo's law of free fall as

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

representing, in idealized form, the real force governing the free fall. She interprets the liquid drop model as a (possibly temporary) device that successfully approximates the *real* nucleus, as can be inferred from the predictive power of the model. Again, as an observer one may object that a liquid drop is not really an approximation to a bunch of particles held together by the strong force, but a substitution. A perhaps less problematic way of expressing the status of a model like the liquid drop model that is common in physics is to speak of an "effective" theory. An effective theory does not, in contrast to a "fundamental" theory, claim that its ingredients really represent reality, but are only effective to produce good predictions.[45] All physicists use effective theories where necessary, which does not affect their metatheoretic persuasions as realists or instrumentalists at all. Realists hope that effective theories will be replaced one day by fundamental theories. Instrumentalists will also welcome the advent of such theories if their predictive power is higher then that of the earlier effective theories. However, they will not believe in the fundamentality of the new theories even if this is not reflected in their research practise at all. The result is: the use of contrafactual models does not commit to instrumentalism.

By analogy, Friedman's ideal type methodology does not commit him to (wholesale) instrumentalism.[46] According to Friedman, the descriptively false "assumptions" contained in economic hypotheses concern the most important real causal factors of the pertinent problem situation, but in idealized and purified form, or – 2nd type - unreal factors that are plausibly causally equivalent with the most important real factors in idealized and purified form. In both cases, the identification of the most important real causal factors or their functional equivalents, respectively, is hypothetical and in need of empirical control. Therefore, economic hypotheses have to be tested empirically by their predictions. These empirical tests have the final say on the appropriateness of the ideal types used in the hypotheses' articulation.

In a sense, the disussion about F53's status with respect to instrumentalism is misguided, as a comparison with physics reveals. In the concrete research practice of a physicist, one typically cannot find indicators whether she is a realist or an instrumentalist, because the language used in informal discourse and in publications is typically fully realist.[47] In other words, in their research practise, scientists are *trivially* realists, as everyone else is in everyday life. In their research practise, they only give up this unreflected wholesale realism if forced to do so, say in (certain interpretations of) quantum mechanics or in the development of models that are intentionally purely instrumentalist. In their research practise, astrophysicists for instance, both wholesale realists and wholesale instrumentalists, deal with black holes or dark matter typically in exactly the same way: these things simply exist.[48] Their difference comes only to the fore when the epistemic status of hypotheses like those about black holes or about dark matter are discussed in general. For the wholesale realist, these hypotheses approximately represent reality, for the wholesale instrumentalist they are just useful devices for predictions of certain phenomena, but otherwise mute about the

[45] Thus, in physics wholesale instrumentalism can be expressed by saying that all physical theories are only effective theories.

[46] Also Weisberg (2007), p. 657 claims that "all three kinds of idealization" which he discusses in his paper and of which the first two are very close to Friedman's first and second type of falsities (see Section 5, fn. 20 and 22), "are compatible with […] realism".

[47] Exceptions may include quantum mechanics and all kinds of modeling that aim at purely instrumental models.

[48] For an example, see Hoyningen-Huene (2018), pp. 4-5.

constitution of reality. Thus, wholesale realism and instrumentalism are *meta-theoretic* positions, typically not appearing in scientific discourse.

What Friedman does in F53 is to describe the research practise of economists who, as most scientists in all disciplines, presuppose unreflective wholesale realism as a matter of course. He is neither defending nor attacking realism, nor instrumentalism; this is not his issue in F53. Instead, he brings to the fore that economic theorizing makes use of ideal types which are not designed to accurately represent *real* situations, because they abstract, idealize, and even substitute. For Friedman, this is good and unavoidable practise in economics. Especially substitutions may have an instrumentalist flavour,[49] but not more than that, because they exchange causally relevant factors in a particular situation by qualitatively different, but causally equivalent ones. The situation is similar to physics: it may be a matter of taste if one calls the liquid drop model of the atomic nucleas just a predictive device (an "instrument"), or an approximation to reality. A wholesale realist may work with the liquid drop model anyway whatever it is called, without putting her wholesale realism in jeopardy in the least.

10. 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 reconstructive goal of this paper if most of the puzzlement disappears.

10.1 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.[50] However, he thinks that he does not have to address this problem because the status of physics as "objective" is unchallenged.

To Friedman, the discussion of the free fall example (16-19) revealed that physics works with what is usually called "idealizations" or "approximations" much the same way as economics work with ideal types; in fact, both procedures are essentially the same. In both fields, these simplifications of complex situations isolate the most relevant factors in a given situation, and in both fields idealizations and ideal types, respectively, must demonstrate their usefulness through the generation of empirically successful predictions. Thus, both fields are similar enough, and as physics counts as objective, so does economics.[51]

10.2 Economic theory as "a set of tautologies"

[49] For Mäki (2009c), p. 105, such substitutions are so "fictionalist" (which they are) that his "realist rereading of F53 will ignore" them. By contrast, I suggest comparing them with similar situations in theoretical physics to assess their epistemic status with respect to the instrumentalism vs. realism issue.

[50] 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]).

[51] Because of its narrow focus, 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 them possible. For such a possible framework see, e.g., Hoyningen-Huene (2013).

> 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)

One should remember that in the neopositivist tradition that was dominant in the U.S. in the 1950s, 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 serves indeed as 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).

10.3 "Assumptions" in scare quotes

This topic was treated extensively in Section 5.

10.4 "[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 speaks about ideal types. The significance of a theory derives from the "analytical relevance" of the ideal types it uses (33-34). 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), or replace them by causal equivalents. 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 descriptive accuracy of the ideal types it is build up of.

10.5 Descriptive accuracy vs. analytic relevance

See above, Section 10.4.

10.6 "Appearances are deceptive" vs. "a more fundamental and relatively simple structure"

Appearances are deceptive in not immediately disclosing what the truly relevant factors of the situation are. The more fundamental and simple structures are described by the ideal types that underly the empirical situation.

10.7 The extensive, but unreferenced use of Popper

I discussed Popper's role for F53 in Section 8.

11. Conclusion

We have seen that Friedman's F53 is far from an instrumentalist manifesto. However, it can also not be read as a *defense* of realism because it takes, like most sciences, realism simply for

granted. The core of F53 is the appropriation of Weber's methodology of ideal types, together with the postulate of strong empirical control by attempted falsifications à la Popper. Strong empirical control of economic hypotheses immunized Friedman against the suspicion of unfounded speculation often associated with the *Verstehen* component of an ideal type based interpretive sociology. This result is in direct contradiction to the widespread sentiment that "there is no unambiguous doctrine or argument presented in F53" (Mäki (2009c), p. 90).

The problem of F53 is that it leaves its readers almost completely in the dark about two of its most important sources, Max Weber and Karl Popper. In addition, F53 excessively uses scare quotes around some of its central terms, especially around "assumptions", instead of carefully explaining what these terms are supposed to mean. Thus, technically speaking F53 is a very badly written paper, but with a brilliant content. Small wonder that it has elicited such a prolonged and controversial discussion.

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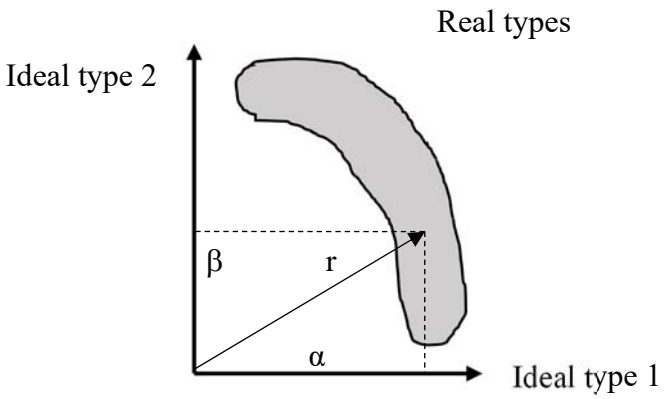

Figure 1