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Were the Foundations of "*Measurement without Theory*" laid in the 1920s?

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Title?

- “Measurement without Theory” = famous paper by Tjalling Koopmans
- (Measurement without Theory. *Review of Economics and Statistics*, Vol. 29, No. 3 (Aug., 1947), pp. 161-172.)
- A defining moment for economics.
- But beware of misinterpretation!
- Koopmans is not a proponent of “measurement without theory”! On the contrary, he is critical of the epistemological stance,
- Through a critique of WC Mitchell’s last book (1946).

Koopmans 1947

the book is unbendingly empiricist in outlook (…) But the decision not to use theories of man's economic behavior, even hypothetically, limits the value to economic science and to the maker of policies, of the results obtained or obtainable by the methods developed. This decision greatly restricts the benefit that might be secured from the use of modern methods of statistical inference. The pedestrian character of the statistical devices employed is directly traceable to **the authors' reluctance to formulate explicit assumptions, however general, concerning the probability distribution of the variables**, i.e., assumptions expressing and specifying how random disturbances operate on the economy through the economic relationships between the variables.

Foundations of the critique of MwT in the 1920s

- Farewell to the “observational science” paradigm (Pliny the Elder)
- Welcome refutationism?
- … This being accomplished… between 1913 and 1928?
- This must be made more precise.
- 1. epistemological shift in the conception of theory
- (history of ideas)
- 2. assessment of the frequency distribution of various theoretical conceptions
- (history of institutions)

Point of departure

- It is certain that there is an “observation science” approach to economics as late as 1913:
- Having summarized 13 competing theories of the business cycle, WC Mitchell concludes:
- the investigation would be distorted if we set out to test each theory in turn by collecting evidence to confirm or to refute it. For the point of interest is not the validity of any writer's views, but clear comprehension of the facts. To observe, analyze, and systematize the phenomena of prosperity, crisis, and depression is the chief task. And there is better prospect of rendering service if we attack this task directly, than if we take the round about way of considering the phenomena with reference to the theories…
- (from WC Mitchell, *Business Cycles*, University of California Press, 1913)

(any difference with Juglar?)

- Without any theory, any assumption, observation of facts was enough to uncover the law of crises and of their periodicity. (1889, p. XV)
- Constant repetition of the same accidents brings real monotony to our history: we are required to pass in order through always the same phases; is it not the best confirmation for what we want to prove? (1862 in 1889, p. XIV).
- Juglar C. (1862), *Des crises commerciales et de leur retour en France, en Angleterre et aux Etats- Unis*, Paris, Guillaumin.
- Juglar C. (1889), *Des crises commerciales et de leur retour en France, en Angleterre et aux Etats- Unis, Paris*, Alcan (2e édition).

Point of arrival

- It is certain that (some) economists played a definite role in defining the Popperian "refutationist" paradigm,
- (Hayek is frequently mentioned as Conjectures and Refutations are dedicated to him)
- "The ultimate goal of a positive science is the development of 'theory' or 'hypothesis' that yields valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed."
- Friedman 1953, "Methodology of Positive Economics" in *Essays in Positive Economics*.

Economics = empirical science as soon as… 1944?

- Neyman+Pearson already but on a verificationist instead of refutationist stance:
- (see e.g. Haavelmo's "Probability approach to econometrics" p. 60)
- "In the following we shall give a brief outline of the basic principles in the Neyman-Pearson theory of testing statistical hypotheses and estimation, and, thereafter, **we shall use these principles for a statistical formulation of hypotheses constructed in economic theory**. This will, it is hoped, clear up a few controversial issues in connection with the problem of statistical 'verification' of economic relations."

Even sooner: Koopmans 1936?

- This theory [statistical testing of hypotheses] has been widely applied to data obtained from agricultural experiment or from measurements in biological populations. There are some essential differences between data of this kind and those usually encountered in economic problems.
- In economic analysis variables at the control of an experimenting institution are exceptional. (…) In a great deal of the problems variables are developing in time in cyclical oscillations, apparently to a large extent governed by some internal causal mechanism, and only besides that influenced, more or less, according to the nature of the variable, by erratic shocks due to technical inventions, variations in crop yields, etc.). At any rate, they are far from being random drawings from any distribution whatever.

Or Frisch?

- “No statistical technique, however refined, will ever be able to [solve all the problems of testing “significance” with which the economic statistician is confronted]. The ultimate test of significance must consist in a network of conclusions and cross checks where theoretical economic considerations, intimate and realistic knowledge of the data and a refined statistical technique concur.”
- (1934)

Etc. is Hotelling 1927 the first?

- Sir Isaac Newton set a bad example for statisticians in his mode of establishing the relation which has been the admired model of scientific achievement for two centuries and a half. Were the solar system subject to a complicated set of unknown forces of as great an order of magnitude as the sun's attraction – such a set, for example, as may exist in a nebula or near a multiple star – Newton could not have established gravitation by means of Kepler's laws, which deal with an orbit as a whole. A statistical method would have been necessary…

Hotelling continued

- If only our tyrannical sun were smaller, the family of planets would enjoy some of the chaos of democratic societies, and the astronomer would be closer to the statistician. Science would have arisen later and statistics earlier…
- Hotelling, “Differential Equations Subject to Error, and Population Estimates”, *Journal of the American Statistical Association*, Vol. 22, No. 159 (Sep., 1927), pp. 283-314.
- (also mentioned by Koopmans 1947)

Hotelling 1927 on causation

- Much attention has been fixed upon the "business cycle." A rhythmical contraction and expansion of the economic system as a whole seem to exist independently of seasonal variation and numerous incidental fluctuations, which are considered to be superimposed upon the fundamental swing.
- Theories of the business cycle fall into two classes, considering respectively what are called in mechanics free and forced oscillations. Forced-oscillation theories require some regularly recurring cosmic cause which influences the economic system but is not influenced by it. The best known theorist in this field is Henry Ludwell Moore, who has suggested some effect of the planet Venus as an explanation of the ups and downs of prices and production. (…). Even if a statistical test should yield a very high correlation, the odds thus established in favor of such an hypothesis would have to be heavily discounted on account of its strong a priori improbability.
- Free oscillations are those which result from shifting internal stresses, and do not require the periodic application of an outside force.
- … the relative importance of free oscillations and mere random wiggles is fairly measured by the coefficient of correlation between a series and its second differences, and that the period may be determined from the regression equation.

The origin of all this

- Is…
- As Michel would have said…
- R. A. Fisher, *Statistical Methods for Research Workers*

How it happened before the 1920s

- Pareto 1897 (Cours)
- Considérons, en général, certains phénomènes: A, B, C. Nos connaissances sur leur mutuelle dépendance peuvent passer par trois. degrés successifs (97). (α) Nous pouvons seulement savoir que cette dépendance existe: que la présence de A et les variations de la grandeur de A influent sur B, C... ; que la présence de B influe sur A, C, D, ... , etc. (β) Nous pouvons, en outre, avoir une idée des liaisons qui existent entre A, B, C,... Savoir, par exemple, que quand A croît, B décroît, C croît, etc. En d'autres termes, nous pouvons connaitre le sens des variations de B, C, D,... provoquées. par une variation déterminée de A. (γ) Enfin, nous pouvons non seulement connaître le sens de ces variations, mais encore en calculer exactement la grandeur. Arrivée à ce point, notre connaissance de l'ensemble des phénomènes A, B, C,… est complète et parfaite.

How it happened before the 1920s

- Pareto 1897 (Cours)
- Let us consider, in general, some phenomena: A, B, C. Our knowledge on their mutual dependance may move along three successive degrees (97). (α) We can just know this dependence to exist: that the presence of A and variations of A's value impact B, C... ; that the presence of B impact A, C, D, ... , etc. (β). We can furthermore have an idea of the linkages between A, B, C,... Knowing, for instance, that when A grows, B decreases, C increases, etc. In other terms, we can know the sense of variation of B, C, D,... caused by a determinate variation of A. (γ) Eventually, we can not only know the sense of variations but compute exactly the values. At this point, our knowledge of the whole of A, B, C,… is complete and perfect.
- (there is not much gradation between (β) and (γ))

+ metaphors

- Cf. Jevons, (John Bates) Clark and others, such as Pareto:
- Quand on veut étudier la cristallographie, on commence par étudier la géométrie, non pas parce qu'on croit que les cristaux sont des corps géométriques parfaite, mais parce que l'étude de ceux-ci fournit des éléments indispensables pour l'étude de ceux-là. De même nous avons commencé par l'étude de l'économie pure, non pas parce que nous croyions que les phénomènes abstraits de celte science étaient identiques aux phénomènes concrets, mais simplement parce que cette première étude nous était utile pour entreprendre la seconde! Dans les chapitres vii et viii nous avons déjà com- mencé à étudier des phénomènes concrets, en recherchant les caractères de certains capitaux ; nous arrivons maintenant aux phénomènes concrets de l'économie en général.

+ metaphors

- Cf. Jevons, (John Bates) Clark and others, such as Pareto (1909):
- Before studying crystallography, one must first study geometry, not because one believes crystals are perfect geometric bodies, but because the study of the latter offers indispensable elements for the study of the former. Likewise, we started with pure economics, not because we believed the abstract phenomena of this science to be identical to concrete phenomena, but because we thought this first pursuit was useful to undertake the next one…

Hotelling & later

- No longer metaphors of natural sciences
- But attempt at estimating parameters of stable relationships
- No longer metaphors of natural sciences… Except if econometrics is said to be a metaphor of agricultural research
- (although there is some denial)

CCL on history of ideas

- Economics moved from observational definition
- (some offered scientific = physical metaphors)
- to a science making & testing assumptions (→ = physics in Popper's view)
- This is not the result of another metaphor
- But of an appropriation of statistical techniques… which economists contributed to developing & disseminating --> institutional history

Why cycles?

- Cycle theory was a meal of choice for the evolution of the economics because:
- there are many causes at work
- none is large enough to make the other negligible
- (this may look like the Lindeberg's condition)
- independence?
- …would have enabled a purely statistical approach…
- → (at least) Back
- background noise + random structure
- (where?)

Where

- The USSR research program on cycles was brought to an abrupt halt…
- Reflection on cycles in Europe was intense in the pre-Fisherian era
  - Juglar + des Essars in France → nothing
  - Jevons → Kitchin in UK
  - Germany → Kuznets showed in “Monetary Business Cycle Theory in Germany“ (JPE 1930) how cycle theory was emerging
- But Germany was shut down as well and the names mentioned by Kuznets went to the US

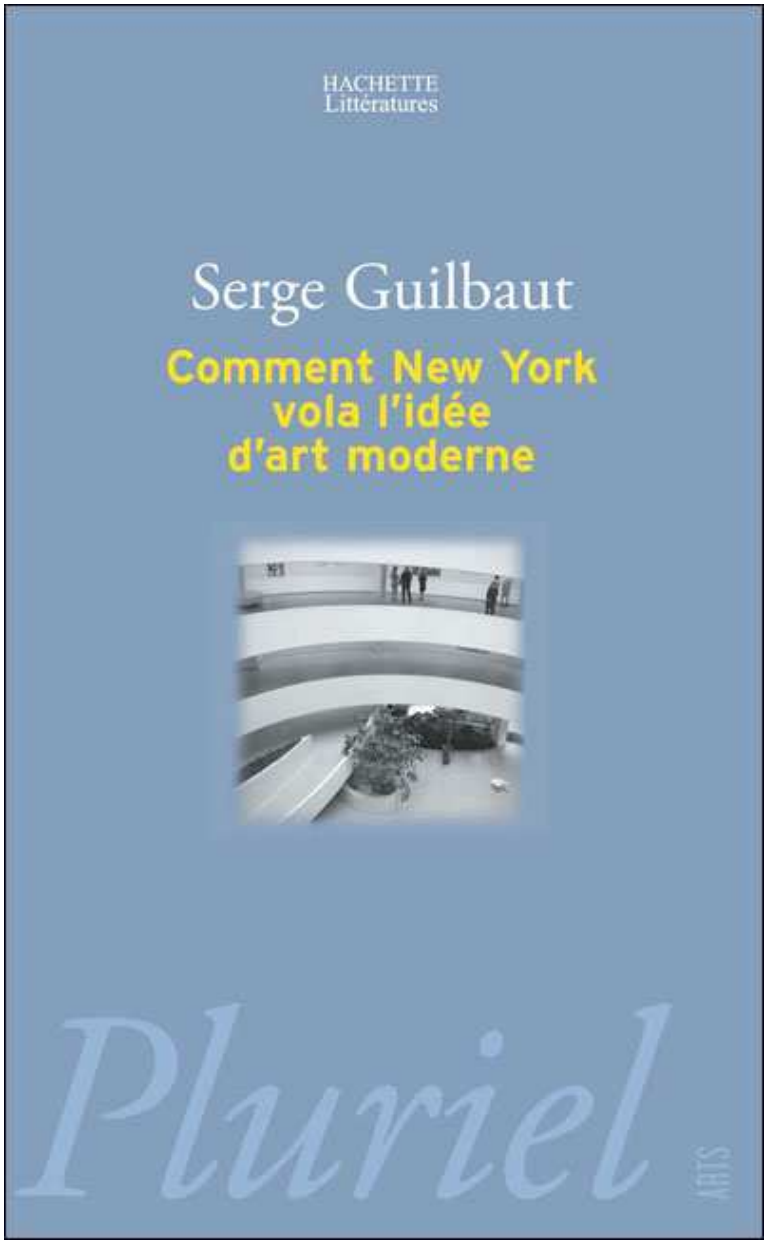
What were economists like?

- Continental Europe: statistic = “science of the state”
  - German Statistik
  - + French « économie politique » as taught in law faculties (originated in the post-1871 war trauma to copycat Germany)
  - = law + public finance + geography + …
- + French ‘liberal school’ (around the *Journal des Economistes*), cf. Juglar
- Also German Historical School, Methodenstreit → the forerunners of the “scientific revolution” in economics did not come to German universities before the 1970s (or more accurately, they flew to the US in the 1930s).

A mainly Anglo-Saxon issue?

- No, since mathematical economics was practiced by French mathematicians (Borel, Fréchet, Darmois → Morlat, Barbut) and engineers (Divisia → Massé, Allais) and actuaries (Laurent)
- But *institutions* were merely US:
  - NBER (which resisted the new method well into the 1940s)
  - Econometric society
  - Cowles Commission
  - (academic centers = ?Chicago, ?Statistical research group at Columbia in the 1940s)

How New York stole the concept of Art Moderne



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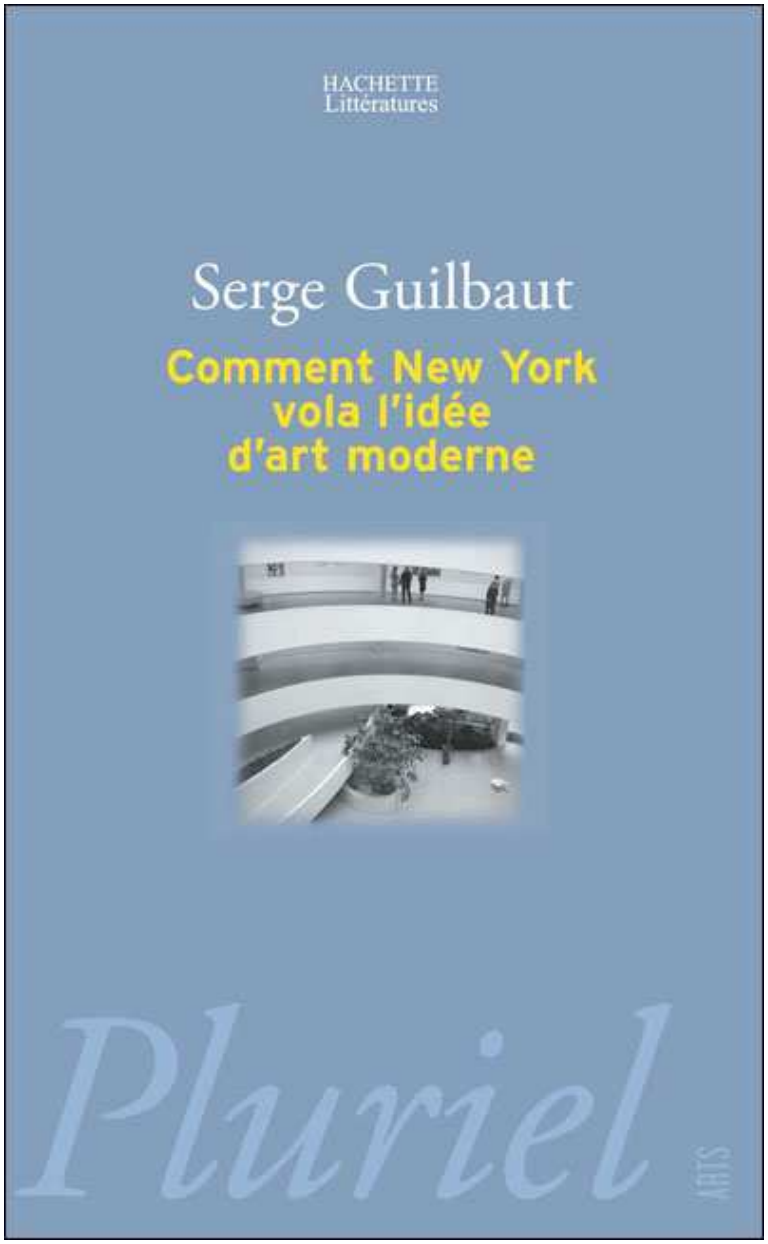
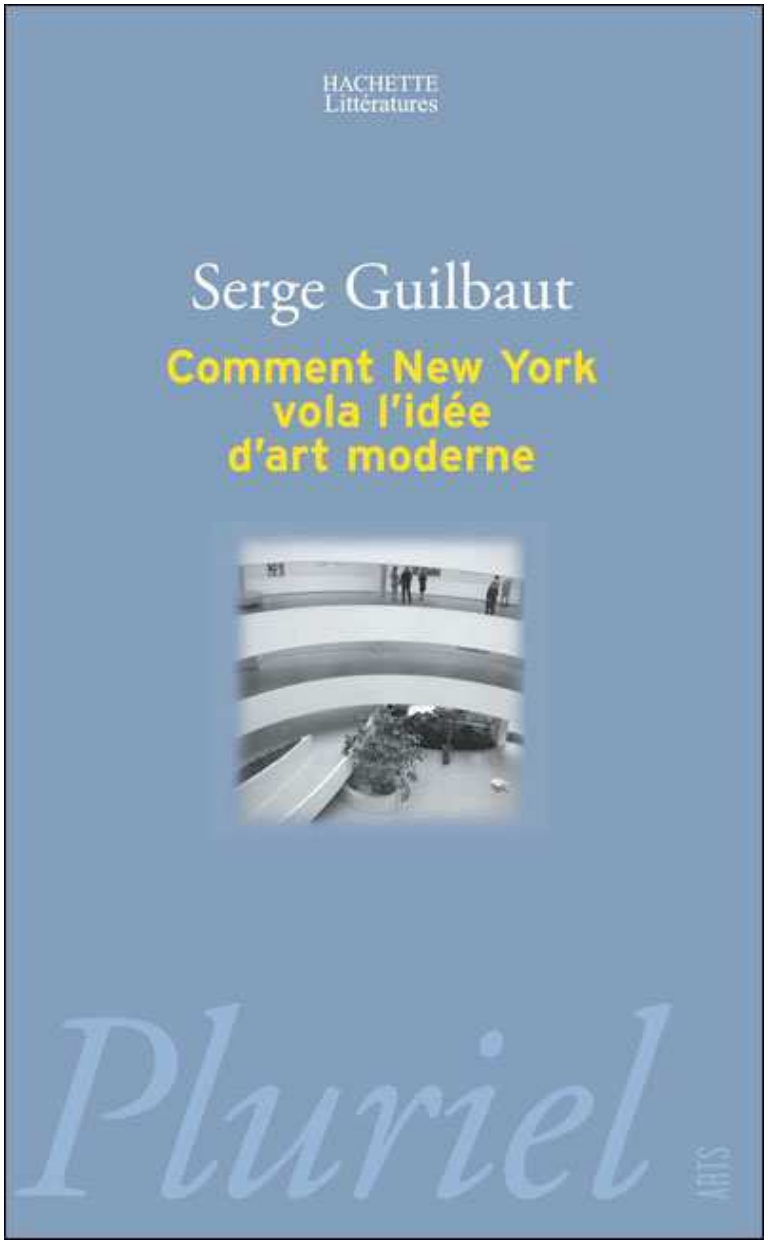


How Europe got stuck in the nineteenth century



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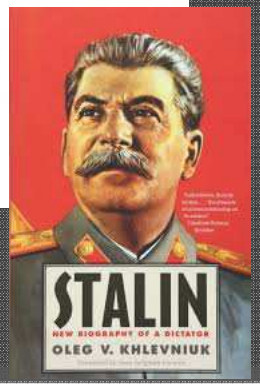

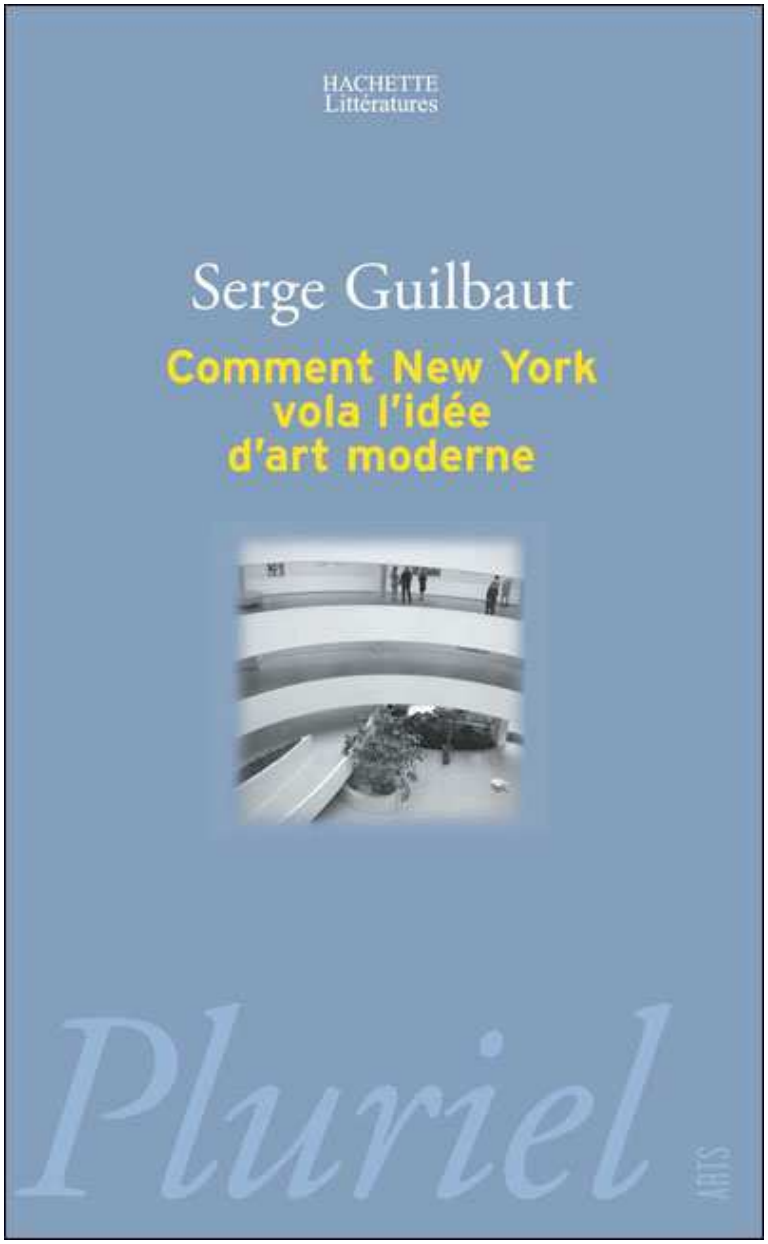

Merci de votre attention

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