Comment on William C. Brainard and Herbert E. Scarf's "How to Compute Equilibrium Prices in 1891"

By K. R. Sreenivasan*

Abstract. In commenting on Brainard and Scarf's essay, the engineer and physicist K. R. Sreenivasan considers why Fisher resorted to hydraulic machines in the first place.

A reading of Irving Fisher's thesis shows that mathematical economics was in its infancy at the time, and that skepticism about its value was the norm. Fisher did not seem to have received expert advice on the subject, so it is remarkable that he went as far as he did; equally impressive is his attempt to compute equilibrium prices *quantitatively* by seeking analogy to hydraulics and building an appropriate apparatus to do so. Brainard and Scarf present a highly readable account of how the apparatus may have worked, and I have little of substance to add. I myself was curious to discover why Fisher resorted to hydraulic machines in the first place, and will make a few remarks in that context. I am afraid that these remarks have not been researched as thoroughly as I would wish.

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Having written down the equations for equilibrium, why didn't Fisher attempt to find their fixed points, at least in some special instances, and examine their stability in mathematical terms? After all, he was a competent mathematician, and later, for awhile, held an appointment in the mathematics department at Yale. It was because few of the tools available in 1891 were adequate for the purpose. People had known—in fact for a long time by then—how to solve a great many initial and boundary value problems, but possessed little knowledge of the qualitative theory of differential equations (despite some impressive works by the likes of Johan Adams of the Neptune discovery). Henri Poincare's celebrated essay on the restricted three-body problem, which pioneered these methods, appeared in 1899 (although an incorrect version had appeared a few years earlier). Aleksandr Lyapunov's thesis, the first work to consider nonlinear stability systematically and provide clean definitions and rigorous results, was completed in 1892 and translated into French in 1907. So, providing a mathematical treatment was not in the cards.

Computational tools available were meager. Tables of logarithms, trigonometric functions, squares, square roots, and so on were available, as were primitive computing machines (Thomas de Colmar's "Arithometer," ca. 1850, Leon Bollee's "Multiplier," ca. 1889, perhaps Otto Steiger's "Millionaire," ca. 1890, and various special-purpose machines employed by big business and the census office). Yet, that was the age of levers and pulleys, and machines based on mechanical principles were being used as analog computers in many different ways. For instance, Lord Kelvin had built a machine to predict tides, and William Jevons (on whose shoulders Fisher stood with regard to mathematical economics) had devised a logic machine based on mechanics and Boolean logic. Thus it seems natural that Fisher turned to mechanical analogies, in particular to hydraulics.

Before assessing the value of Fisher's invention, let us ask what inspiration he may have derived locally. His own advisor does not seem to have been of direct help, nor was there an expert hydrodynamicist at Yale. Fisher acknowledged having received advice from Josiah Willard Gibbs and Hubert Anson Newton. The latter was a professor of mathematics and astronomy, but there is no record of his having evinced any interest in hydrodynamics. Gibbs, on the other hand, had a lively interest in the subject: he himself was working on a hydraulic device called the "center-vent" turbine (although it does not seem to have been completed). There was in New Haven at the time a nephew of Eli Whitney, by the name of Eli Whitney Blake, who had a strong interest in hydrodynamics; Fisher may have known about Blake's work. But the inspiration seems to have been essentially internal, and Fisher appears to have been drawn to the analogy by the ubiquitous use in economics of words of mechanics origin—*equilibrium, stability, elasticity, force, expansion, pressure, resistance, reaction, levels,* and so forth. He must have thought that a systematic representation of economic equilibria in terms of these mechanical metaphors was worthwhile. He was a gadget lover in any case, and later designed and built several other machines, including a calibrated triangular pan for mechanically adjusting and evaluating a balanced diet.

Why didn't the Fisher machines become popular in later economics work? For an analogy to be truly deep—by which I mean that the qualitative and quantitative understanding of one problem is enhanced by the workings of a second—one needs to know that the dynamical equations of one system, when suitably transformed, are the same as those of the second. This is true of the analogies between high-speed flight and shallow water waves, flow on soap films and elasticity of solids, and of a number of other problems between electricity and magnetism on the one hand and hydrodynamics on the other. Analogies without this shared feature may be suggestive of something fundamental, yet should be discarded in the face of contrary physical evidence. Witness, for instance, Rene Descartes's vortex analogy for the distribution of matter in the universe, its resurrection in another form by Lord Kelvin for modeling atomic structure—the flow of ether through the universe—all of which later fell by the wayside when our understanding became more rigorous. While it is true that equilibrium systems near a critical point behave in universal ways that transcend details of dynamical equations, the behavior of nonequilibrium systems is much more diverse. For all these reasons, it is unclear if Fisher's analogy has any inevitability to it.

Fisher himself seems to have put the machines to some pedagogical use in teaching. If a student wants to develop some intuition about fluid dynamics, it is natural for him to build a device, watch the flow, or measure its properties; after some time, he might develop an intuition that cannot be acquired by any other means. It is interesting to speculate whether a student of economics could similarly develop some intuition for prices and their fluctuations by working with Fisher's apparatus. Somehow I doubt it.

Can one learn anything more from the Fisher machines now than was learnt by the originator himself? This brings us to the simulations of Brainard and Scarf, whose most interesting conclusion concerns the *dynamical* performance of the machines. Fisher himself seems not to have worried about dynamics, and, in this respect, the simulations go beyond his original aspirations. As Brainard and Scarf point out, the dynamical behaviors of the machines depend on a large number of parameters such as the mass and surface areas of the floats, the sizes of the tubes, and water temperature. Their simulation of the machines' dynamics makes many approximations and assumptions; while they are eminently reasonable, their effects are hard to assess. It is thus unclear if the simulations have replicated the behavior of the machines truthfully. I am therefore not convinced, for instance, that the multiple equilibria unearthed by the simulations are characteristic of the machines themselves (especially when operated close to equilibrium, as Fisher seems to have done). A reliable answer must await the reconstruction of the hardware—a long-standing, even if a low-key, intention of ours!

In an accompanying Comment, Brown and Kubler have pointed out that the modern methods of computing equilibria, while based on Fisher's equations, are quite different in nature and scope. Herb Scarf's own pioneering contributions in this regard need not be repeated here.

Altogether, in this era, Fisher's machines seem to have only historical significance. But this does not detract one bit from his ingenuity and innovation: he sought the most sophisticated answers possible for his times!