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Measuring Instruments in Economics and the Velocity of Money

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Measuring Instruments in Economics and the Velocity of Money

Mary S. Morgan[1]

Abstract

Economic measurements are generated by complicated systems of measurement involving economic and bureaucratic processes. Whether these measuring instruments produce reliable numbers: 'facts' that travel well, depends on the qualities of these systems. Ideas from metrology, and from the philosophy and sociology of science, are used to analyse various attempts to measure the velocity of money ranging from the 17th to the 20th centuries. These historical experiences suggest that numerical facts are likely to travel well in economics when the criteria implied by all three of these disciplinary approaches to measurement are met.

Introduction

In economics, facts are hard things like numbers: measurements of unemployment, for example, or of prices or money. Numbers like these that become widely accepted within the economics community, and are used without much consideration of how they were found or made, can be considered as facts that have travelled well. Yet such facts are hard to come by. This paper considers three different strands of literature which relate the construction of measuring systems to generate such numbers to their effectiveness, reliability and trustworthiness in representing the economic world. This paper looks at the history of one particular kind of numbers - measurements of the velocity of money - to investigate how these

[1] This paper was originally drafted for presentation at "History and Philosophy of Money" Workshop, Peter Wall Institute for Advanced Study, University of British Columbia, 12-14th November 2004. Revised versions were given at the Cachan/Amsterdam Research Day (December 2004), at the ESHET conference in Stirling, (June 2005), at the ANU (October 2005), at the ASSA (January 2006) and at the Workshop on Economic Measurement, Amsterdam, April 2006. I thank participants who commented on these occasions, particularly Malcolm Rutherford David Laidler, Marcel Boumans and Janet Hunter. This version has benefited from thinking about the problem in the context of "How Well Do 'Facts' Travel?", a Leverhulme/ESRC-funded project at the Department of Economic History, LSE. I thank Arshi Khan, Xavier Duran, Sheldon Steed and Bruce McDonald for research assistance; and Peter Rodenburg, Hsiang-Ke Chao and Marcel Boumans for teaching me about measurement in economics. Comments welcome to m.morgan@lse.ac.uk

requirements for good measuring systems might be understood and how they fit together. The case suggests that numbers produced according to instruments which have fulfilled the requirements implied by all three approaches are likely to travel well, while those that fail on one of these approaches are likely to crumble in our hands when we try to use them.

1. How do we get good measurements of velocity?

How should we measure velocity of money? This is a question which has intrigued, if not baffled, economists for several centuries. Even William Stanley Jevons, who proved to be one of the 19th centuries most willing and innovative measurers in economics, stated:

> I have never met with any attempt to determine in any country the average rapidity of circulation, nor have I been able to think of any means whatever of approaching the investigation of the question, except in the inverse way. If we knew the amount of exchanges effected and the quantity of currency used, we might get by division the average numbers of times the currency is turned over; but the data, as already stated, are quite wanting. (Jevons 1909 [1875] p 336).

Nowadays, this is indeed the kind of formula used in measuring velocity: some version of the values of total expenditure (usually nominal GDP) and of money stock are taken ready made from "official statistical sources", and velocity is measured by dividing the former by the latter. For example, the *Federal Reserve Chart Book* routinely charted something it called the "Income Velocity of Money" in the 1980s, namely GNP/M1 and GNP/M2 (in seasonally adjusted terms, with quarterly observations on a ratio scale).[2]

But such treatment accorded to velocity - as taken for granted, easily measured and charted - does not mean that the problems of adequately measuring the velocity of money have been solved, or that the Fed's modern measurements are any more useful than those of three centuries earlier. Let me begin by contrasting that standard late 20th century method of measuring the velocity of money with one from the 17th century.

William Petty undertook a series of calculations of the economic resources of England and Wales in his *Verbum Sapienti* of around 1665 and asked himself how much money "is necessary to drive the Trade of the Nation" having already estimated the total "expence" of the nation to be £40 millions. This set him to consider the "revolutions" undergone by money:

> if the revolutions were in such short Circles, viz. weekly, as happens among the poorer artisans and labourers, who receive and pay every *Saturday*, then 40/52 parts of 1 Million of Money would answer those ends: But if the Circles be quarterly, according to our Custom of paying rent, and gathering Taxes, then 10 Millions were requisite. Wherefore supposing payments in general to be of a mixed Circle between One week and 13. then add 10 Millions to 40/52, the half of the which will be 5½, so as if we have 5½ Millions we have enough. (Petty, in 1997 [1899], pp 112-3.)

Now Petty set out to measure the amount of necessary money stock given the total "expenses" of the nation, not to measure velocity, but it is easy to see that he had to make some assumptions or estimates of the circulation of money according to the two main kinds of payments. He supposed, on grounds of his knowledge of the common payment modes, that the circulation of payments was 52 times per year for one class of people and their transactions and 4 for the other, and guesstimated the shares of such payments in the whole (namely that payments were divided half into each class), in order to get to his result of the total money needed by the economy.

If we simple average Petty's circulation numbers, we would get a velocity number of 28 times per year (money circulating once every 13 days); but Petty was careful enough to realise that for his purpose to find the necessary money stock, these must be weighted by the relative amounts of their transactions. Such an adjustment must also be made to find velocity according to our modern ideas. If we employ the formula that velocity = total expenditure/money stock, we get a velocity equal 7.3 (or that money circulates once every 50 days).

One immediate contrast that we can notice between these two episodes

[2] For example, 1984 p5, 1986, p8.

is that in Petty's discussion, the original circulation figures for the two kinds of transaction - the figures relating to velocity - were needed to derive the money stock necessary for the functioning of the economy and having found this unknown, it was then possible (though Petty did not do this) to feed this back into a formula to calculate the overall velocity figure. We used the formula here to act as a calculation device for velocity, the measurements themselves were based on independent guesstimates of circulation by Petty. This is in contrast to the modern way used by the Fed, where their velocity number is derived from the formula V=GNP/M. This simple model formula acts as the measuring device for velocity. There are no independent or separate numbers which constitute direct measurements (or even guesses) of monetary circulation or velocity.

These two methods of measuring velocity - Petty's direct way and the modern derived way - are very different. It is tempting to think that the Fed's was a better measure because it was based on real statistics not Petty's guess work, and because its formula links up with other concepts of our modern economic theories. But we should be wary of this claim. We should rather ask ourselves: What concept in economics does the Fed's formula actually measure? And, Does it measure velocity in an effective way? For this, we need to have some ideas about measurement.

A small warning is in order here lest I be misunderstood: This paper does not in any sense pretend to be a comprehensive history of all the attempts to measure the velocity of money. Rather it picks out some particular contributions which are of interest to two sets of questions, one set about the constitution of economic measurement and the other about the history of economists' attempts to provide measurements for the concepts in their field and their claims about such numbers. The aim is to show the usefulness of the concept of measuring instruments in thinking about the history of measurement in economics.

2. Measuring Things

2.1 Ideas about Measuring from Philosophy, Metrology and History of Science

There are three kinds of literature on measurement that I want to introduce in relation to measurement of things which don't seem easy to measure. These literatures come from three different starting points: namely, from the philosophy of science, from metrology, and from the social studies/history of science. But as we shall see, they are complementary rather than otherwise.

The mainstream philosophy of science position, known as the representational theory of measurement, is associated particularly with the work of Patrick Suppes.[3] This theory was developed by Suppes in conjunction with Krantz, Tversky and Luce, and grew, out of their shared practical experience of experiments in psychology, into a highly formalized approach between the 1970s and 1990s. The original three volumes of their studies ranged widely across the natural and social sciences and has formed the basis for much further work on the philosophy of measurement.

Formally, this theory requires one to think about measurement in terms of a correspondence, or mapping: a well defined operational procedure between an empirical relational structure and a numerical relational structure. Measurement is defined as showing that "the structure of a set of phenomena under certain empirical operations and relations is the same as the structure of some set of numbers under corresponding arithmetical operations and relations" (Suppes, 1998). This theory is, as already remarked, highly formalized, but informally, Suppes himself has used the following example.[4] Imagine we have a mechanical balance - this provides an empirical relational structure whose operations can be mapped onto a numerical relational structure for it embodies the relations of equality, and more/less than, in the positions of the pans as weights are place in them. The balance provides a

[3] For the original work, see Krantz et al, 1971. For recent versions see Suppes 1998, and 2002. A more user friendly version is found in Finkelstein, 1974 and 1982.

representational model of certain numerical relations, and we can see a numerical model isomorphic to the empirical model. Though this informal example nicely helps us remember the role of the representation, and foresees how the numerical relations can lead to calculation, it is unclear how you find the valid representation.[5]

Finkelstein (1982) and Sydenham (1982), both in the *Handbook of Measurement Science,* offer more pragmatic accounts to go alongside and interpret the representational theory's formal requirements to ensure valid measurement. Finkelstein's informal definition talks of the assignment of numbers to properties of objects, stressing the role of objectivity and that "measurement is an empirical process, ... the result of observation and not, for example, of a thought experiment" (Finkelstein p 6-7).

This practical version of the representational theory closes in on the second approach - which I call the metrology approach - developed for economics by Marcel Boumans (1999, 2001 and 2005), at the University of Amsterdam. Boumans' innovation here entails taking seriously the notion that we have "measuring instruments" in economics. We may not recognise them as such, but Boumans shows us that the history of economics is full of mathematical formulae, models, or even parts of models, that we use as devices or instruments to enable us to put measurements (ie numbers) to apparently unmeasurable entities in economics. (This is not a discussion of econometrics, which seeks measurements of the relations between entities.[6]) For Boumans, the bases of successful measurement depend on creating or developing measuring instruments (formulae) that are, as far as possible, invariant to changes in environment while at the same time accurately capturing the variations of the entity being measured. His work shows how mathematical measuring devices are constructed to fulfill these requirements

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[4] At his Lakatos award lecture at LSE, 2004.
[5] See Rodenburg (2006) on how these representations are found in one area of economics, namely the measurement of unemployment.
[6] Chao (2002 and forthcoming) discusses the usefulness of the representational approach and the idea of measuring instruments in understanding econometric models applied to consumption relations.

and how economists' measuring instruments function to overcome standard problems such as extracting signal from noise, filtering, and calibrating the signal to numbers.

In parallel to these philosophical and metrological approaches, Ted Porter (1994 and 1995) in the history of the social sciences, has focussed on the ways in which social science numbers become accepted as legitimate and conventional measurements in their fields. In particular, his notion of the development of "standardized quantitative rules" focuses on the qualities necessarily for social science numbers to count as "objective". All three named elements contribute to our willingness to have "trust in numbers", that is to think of them as being "objective" measurements. "Quantitative" refers to a level of precision and exactitude we associate with the notion of measurement; "rules" refer to the set of principles, methods and techniques by which the measurement is made; and "standardized" refers to the stability of our measuring process. Numbers produced according to methods which changed each time a measurement was taken would not constitute measurements that were usable or even meaningful.[7] That is, numbers are not trustworthy in themselves, however precise and exact they seem, our trust depends on their means of production according to rules that don't change unduly. An important part of Porter's thesis is attention to the role of bureaucracy - preferably an independent trustworthy office such as a central statistical office - in the production of numbers, so that "rules" include not only statistical counting rules, but rules requiring submission and handling of information and so forth. Since these kinds of rules are obviously endemic in the production of most economic data, Porter's thesis is particularly salient to economic measurement. The onus here in his account however is on how our numbers gain trust, not on how we overcome the problem of turning our concepts and phenomena into numbers in the first place.[8]

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[7] An infamous example of this is the way in which Thatcher's government insisted on successive changes in the definitional rules of counting unemployment so that the measurements of this entity would fall.

[8] However, the development of measurement rules is not neglected by Porter, for example

An analysis of effective measurement in economics might therefore engage us in considering all three aspects of measurement entailed in these three approaches - the philosophical, the metrological and social/historical. All of these approaches are concerned with making economic entities, or their properties, measurable, though that means slightly different things according to these different ideas. For the representational theorists, it means finding an adequate empirical relational structure and constructing a mapping to a numerical relational structure. This enables measurements - numbers - to be constructed to represent that entity/property. For Boumans, it means developing a model or formula which has the ability to capture the variability in numerical form of the property or entity, but itself to remain stable in that environment. For Porter, it means developing standardized quantitative rules (by the academic or bureaucratic community or some combination thereof) that allow us to construct, in an objective and so trustworthy manner, measurements for the concepts we have. We can interpret these three notions as having in common the notion that we need a measuring instrument, though the nature of that instrument, and criteria for its adequacy, have been differently posed in the three literatures.[9]

2.2 The History of Making Economic Things Measurable

If we look back over the past century of so of how economists have developed ways of measuring things in economics, we can certainly find the kinds of formulas and models - measuring instruments - that Boumans describes. We can even find them in conjunction with Porter's standardized quantitative rules, and the kind of representational approach outlined by Suppes. For example, Boumans (2001) analysed the construction of the measuring instrument for the case of Irving Fisher's "ideal index" number. Fisher attempted to find a formula that would simultaneously fulfill a set of

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see his 1995 discussion of the development of the rules of cost-benefit analysis.

[9] For a more general discussion of the design of measuring instruments in economics, see

axioms or requirements that he believed a good set of aggregate price measurements should have. Boumans showed how Fisher came to understand that, although these were all desirable qualities, they were, in practise, mutually incompatible in certain respects. Different qualities had to be traded-off against each other in his ideal index formula - the formula that became his ideal measuring instrument. Fisher's initial design criteria, his axioms, can be interpreted within the representational theory of measurement as the empirical relational criteria that the numbers had to fulfil. In these terms, Boumans' finding can be interpreted that Fisher's empirical relational structure could not be fully mapped onto the economic world: it failed Suppes' test in the sense that one or two of the axioms or criteria had to be relaxed. However, the actual index number formula that Fisher developed on the modified criteria can be interpreted as successful using Boumans' own invariance criteria for measuring instruments. In addition, the kinds of measurement procedures that were developed with such similar instruments can be understood within Porter's discussion of standard quantitative rules. The fact that numbers produced with such measuring instruments, are, by and large, taken for granted is evidence of our trust in these numbers, and when that trust is lost when we notice something amiss with the rules. For example, see Banzhaf (2001) for an account of how price indices lost their status as trustworthy numbers when quality changes during the second world war undermined the index number formula which assumed constant qualities.[10]

In looking at the history of velocity measurements then, we need to look out for the measuring instruments and so to the other issues raised in the literature on measurement, namely as to whether such measuring instruments fulfil Suppes', Boumans' and Porter's requirements for the characteristics of measuring systems.

Morgan 2001, and 2003.

[10] The recent Boskin report on the US cost of living index offers another case for the

3. Measuring Velocity: Episodes from History

3.1 Direct Measurements of Transactions Velocity

If we go back again to Petty's calculations, we recall that he had guesstimated the amounts of money circulating on two different circuits in the economy of his day. He characterised the two circuits both by the kind of monetary transactions and the economic class of those making expenditures in the economy. I label these "guesstimates" because these two main circuits of transactions and their timing were probably well understood within the economy of his day. We find further heroic attempts, using a similar approach, to estimate the velocity or "rapidity" of circulation in the late 19th century. For example, Willard Fisher (1895) drew on a number of survey investigations into check and money deposits at U.S. banks in 1871, 1881, 1890 and 1892 to estimate the velocity of money. Although these survey data provided for two different ways of estimating the amount of money going through bank accounts, the circulation of cash was less easy to pin down, and he was unhappy with the ratio implied from the bank data that only 10% of circulation was in the form of cash transactions. On the basis of an estimate of the total currency in circulation, Willard Fisher was able to frame, with some plausibility, the limits of cash money circulation against check money circulation: that is, he argued that it would be implausible to assume a cash circulation (as for credit) of only once every 3 weeks, and that cash circulating at the more plausible 3 times a week would make credit and cash transactions roughly equal in making up the circulation of money. The method was similar to that used by Petty, except that now he had some statistical evidence on one part of the circulation, and his categories involved different kinds of payments rather than classes and types of expenditures.

This was the "age of economic measurement" (see Klein and Morgan, 2001), a period when serious data collection as a means of observation and measurement was beginning to become an obsession in economics. The question of how much work money did, and how far that had changed over the

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investigation of trusty numbers.

previous years, was the subject of much debate in the American economics community in the middle 1890s. Wesley Clair Mitchell (1896), for example, claimed both a substantial increase in the money in the economy and an increase in the velocity of circulation even while he estimated there had been a fall in the share of cash transactions, from 63% to 33% over the period 1860 to 1891. David Kinley's 1897 paper used evidence from an 1896 bank survey investigation, and, with a little more information at his disposal but still on the basis of guess work on the plausible circulation of cash, placed the figure at 75% credit and 25% cash transactions. Yet, empirical numerical information on the velocity of circulation, and cash transactions in particular, remained elusive.

A further flurry of measurement activity took place around the end of the first decade of the 20th century. Edwin Kemmerer (1909), made full use of the various banking and monetary statistics of his day, and built on these earlier 1890s investigations and estimations to arrive at an estimated velocity of money ("rate of monetary turnover") of 31 (or 47, if money was taken ex. bank reserves) for 1896. He then applied these circulation rates, and other estimates for 1896 to the whole period 1879-1908 to construct a series that summed two different kinds of money (cash and checks) times their respective velocities (ie MV in a Fisherian equation of exchange: Money x Velocity = Price x Transactions). In the final summary chapter of Kemmerer's book, these estimates were combined to form an index number of the "relative circulation" (ie MV/T) and compared with his separately constructed prices series (P) and trade series (T) to check the overall coherence of the separate measurements. These other measurements are not in themselves of interest here - rather the point is that velocity measurements were estimated directly from various banking statistics.

In terms of Suppes' representational theory of measurement, we can interpret Kemmerer's actions as taking the equation of exchange to operate as an empirical relational structure indicating the numerical relational structure that his series of numbers needed to possess. He did not use that empirical relational formula to derive measurements, either directly or indirectly, for any of the unmeasured items, but did assume that the numerical relations should hold in the same format as his empirically defined relations. Thus, he constructed measurements of all the elements independently and a numerical difference between the two sides of the relation MV=PT was taken to indicate how far his series of measurements of both sides of the equation might be in error. The formula operated neither as a calculation device nor as a measuring instrument, but it was part of a post-measurement check system which had the potential to create trust or confidence in his measurements.

This indeed was the same use that Irving Fisher made of his equation of exchange MV=PT, but in a much more explicit way that takes us back immediately to Suppes' informal example of the mechanical balance. In my previous examination of Irving Fisher's use of the analogy of the mechanical balance for his equation of exchange (see Morgan, 1999), I wrote briefly about the measurement functions of his mapping of the various numbers he obtained for the individual elements of the equation of exchange onto a visual representation of a double-armed balance (see his 1911/1922, fig.17, p 306).[11] I suggested that the mechanical balance was not a measuring instrument in this case, for, like Kemmerer, he measured all the elements on the balance in separate procedures and both tabled and graphed the series to show how far the two sides of the equation were equal. Nevertheless, he did use the mechanical balance visual representation to discuss various measurement issues: the mapping enabled him to show the main trends in the various series at a glance and in a way which immediately made clear that the quantity theory of money could not be "proved" simply by studying the equation of exchange measurements. He was also prompted by this analogy to solve the problem of weighted averages by developing index number theory. All this takes us somewhat away from the point at issue - the measurement of velocity - but the use of equations of exchange returns again later.

[11] See also Harro Maas (2001) on how Jevons used the mechanical balance analogy to bootstrap a measurement of the value of gold and better understand unobservable utility.

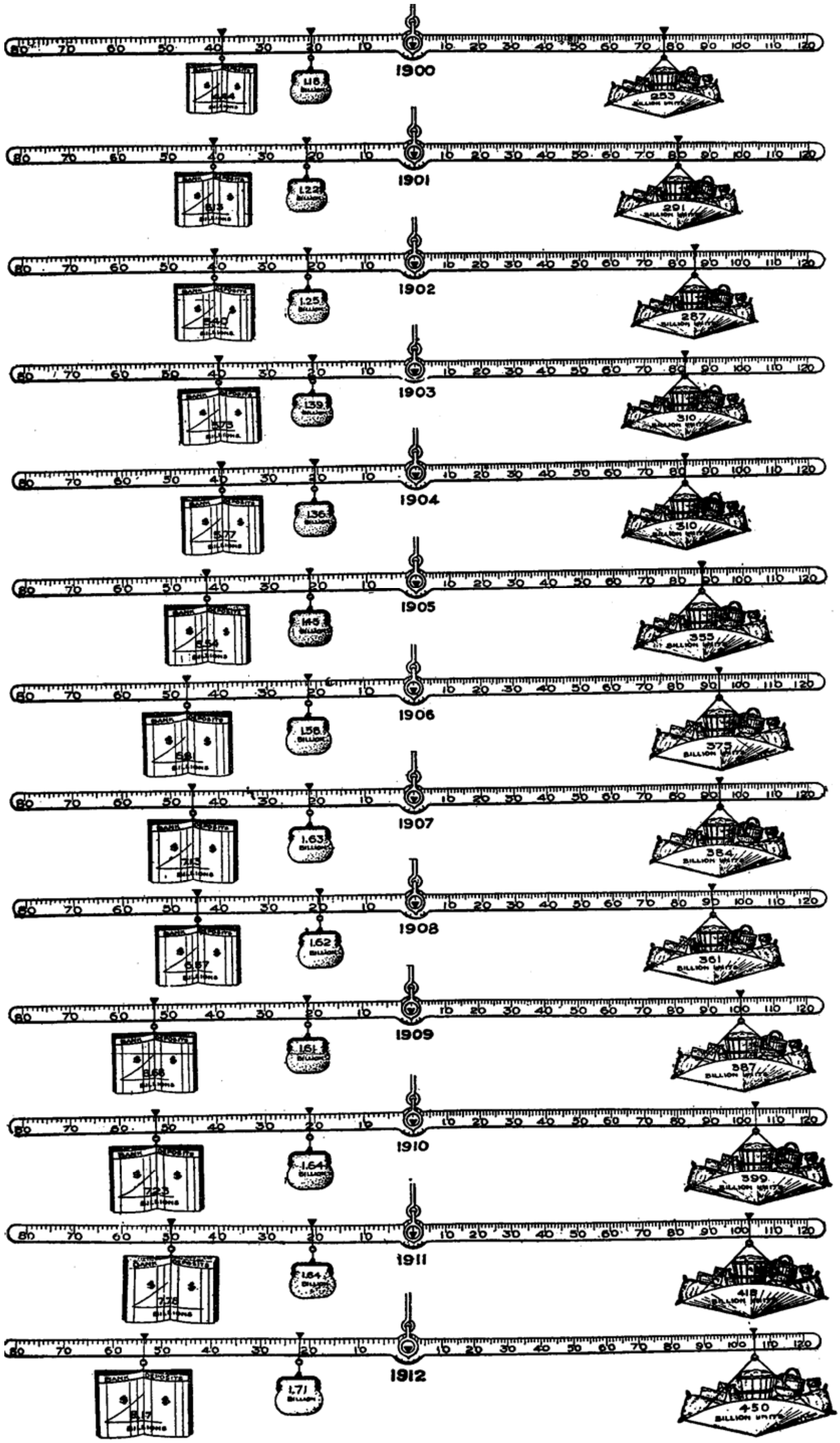
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Fig. 17.

In thinking about all these individual measurement problems, Irving Fisher took the opportunity to develop not only the fundamentals of measuring prices by index numbers, but also two neat new ways of measuring the velocity of money. He regarded his equation of exchange as an identity which defined the relationships of exchange: it was based on his understanding that money's first and foremost function was as a means of transaction. Thus, he thought it important to measure velocity at the level of individuals: it was individuals that spent money and made exchanges with others for goods and services. He developed two new ways to measure velocity.

I will deal with the second innovation first as it can be understood as working within the same tradition as that used by Petty and Kemmerer, but instead of simply estimating two number for the two different circulations as had Petty, or two different circulations of cash and check money as had Kemmerer, Irving Fisher proposed a more complex accounting in which banks acted as observation posts in tracing the circulation of payments in and out of a monetary "reservoir". This innovation in measuring velocities was introduced as follows:[12]

> The method is based on the idea that money in circulation and money in banks are not two independent reservoirs, but are constantly flowing from one into the other, and that the entrance and exit of money at banks, being a matter of record, may be made to reveal its circulation outside. .... We falsely picture the circulation of money when we think of it as consisting of a perpetual succession of transfers from person to person. It would then be, as Jevons said, beyond the reach of statistics. But we form a truer picture if we think of banks as the home of money, and the circulation of money as a temporary excursion from that home. If this be true, the circulation of money is not very different from the circulation of checks. Each performs one, or at most, a few transactions outside of the bank, and then returns home to report its circuit. (1909, p 604-5)

He began by dividing all people into three classes: commercial depositors, other depositors, and non-depositors and thence developed two models to

[12] This work was reported in Fisher's 1909 paper "A Practical Method of Estimating the Velocity of Circulation of Money" and repeated in his 1911 book under the title "General Practical Formula for Calculating *V*", Appendix to his Chapter XII, para 4, p 448-460.

help him map the circulation of money in exchange for goods - first a visual representation, and, from using that, a second model, an algebraic formula which allowed him to calculate velocity.

The first visual model (his Figures 18 - reproduced here - and 19, p 453 and 456 of 1911) portrayed the circulation from banks into payment against goods or services, possibly on to further exchanges, and thence back to banks. This “cash loop” representation enabled him to define all the relevant payments that needed to go into his formula and to determine which ones should be omitted. The relevant payments that he wanted to count for his calculation of velocity were ones of circulation for exchanges of money against goods and services, not those into and out of banks, that is, the ones indicated on the triangle of his diagram, not on the horizontal bars, where **B**=Banks, **O**=Ordinary depositors (salaried men), **N**=Non-depositors (wage-earners), and **C**=Commercial depositors. But banks were his observation posts - they were the place where payment flows were registered and so the horizontal bars were the only places where easy counting and so measurement could take place. Thus his argument and modelling were concerned with classifying all the relevant payments that he wanted to make measurable and then relating them, mapping them, in whatever ways possible, to the payments that he could measure using the banking accounts.[13] He used the visual model to create the mathematical equation for the calculation using the banking statistics, and this in turn used the flows that were observed (and could be measured) in order to bootstrap a measurement of the unobservable payments and thus calculate a velocity of circulation.

[13] In doing this, he argued through an extraordinarily detailed array of minor payments to make sure that he had taken account of everything, made allowances for all omissions, and so forth.

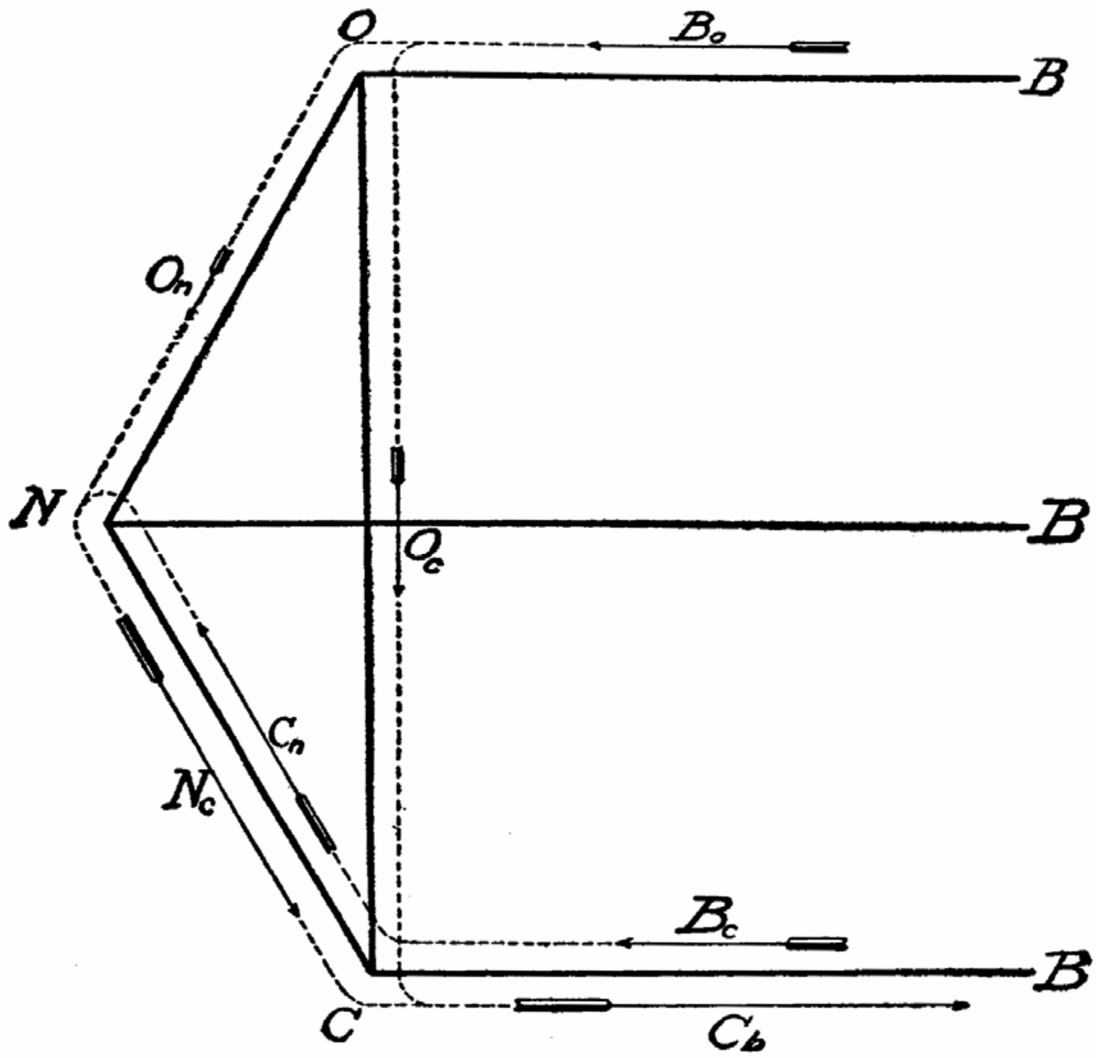

FIG. 18.

Irving Fisher applied his calculation formula - his measuring instrument for velocity - to the 1896 statistics on banks that Kinley had discussed earlier. This part of his work is also very careful, detailing all the assumptions and adjustments he needed to make as he went along (for example for the specific characteristics of the reporting dates). Some of these steps enabled him to improve on his model-based formula. For example, his diagram assumed that payments to non-depositors circulated straight back to depositors, so such money changes hands only twice before it returns to banks, not more. Yet in the process of making a consistent set of calculations with the statistical series, he found that he could specify how much of such circulated money did change hands more than twice. In other words, his measuring instrument formula acted not just as a rule to follow in taking the measurement, but as a tool that allowed him to interrogate the statistics given in the banking accounts and to improve his measurements.

The velocity measure that Irving Fisher arrived at by taking the ratio of the total circulation of payments (calculated by his formula) to the amount of money in circulation for 1896 was 18 times a year (or a turnover time of 20 days). Kinley (1910) immediately followed with a calculation for 1909 based on Fisher's formula and showing velocity at 19. Kinley's calculations paid considerable attention to how wages and occupations had changed since the 1890 population census, and Fisher in turn responded by quoting directly this section of Kinley's paper, and his data, in his *The Purchasing Power of Money* (1911). With Kinley's inputs, and after some further adjustments, Fisher had two measurements for velocity using this cash loop analysis: 18.6 for 1896 and 21.5 for 1909. The calculation procedure had been quite arduous and required a lot of judgement about missing elements, plausible limits, substitutions and so forth. Nevertheless, on the basis of this experience and the knowledge gained from making these calculations, he felt confident in claiming that a good estimate of velocity could be made from the "measurable" parts (rather than the "conjectural" parts) of his formula (p 475, 1911), that is, he used the following measuring instrument equation (referring to his cash-loop diagram):

$$MV = Oc + On + Cn + Nc = \text{bank deposits } (Oc+Nc) + \text{wages } (On+Cn)$$

He concluded that "*money deposits plus wages, divided by money in circulation*, will always afford a good barometer of the velocity of circulation." (1911, p 476) . It is perhaps surprising that he did not use this modified measuring instrument, his "barometer", to calculate the figures for velocity between 1897-1908! Rather, the two end points acted as a calibration for interpolation. Nevertheless, the way that he expressed this shows that his cash loop model and subsequent measurement formula can be classified as a sophisticated version of Petty's tradition of using the class of payers and payments to determine the velocity measurement.

The other (chronologically earlier) new way of measuring velocity introduced by Irving Fisher was an experimental sample survey that he undertook himself and reported briefly in 1897. (A fuller report of this survey was included in his 1911 book.) In his 1897 paper, he wrote of the possibility of taking a direct measurement of velocity:

> ... just as an index number of prices can be approximately computed by a judicious selection of articles to be averaged, so the velocity of circulation of money may be approximately computed by a judicious selection of persons. Inquiry among workmen, mechanics, professional men, &c., according to the methods of Le Play might elicit data on which useful calculations could be based, after taking into account the distribution of population according to occupations. (p 520).

Again, this has shades of Petty's approach, a groups of spenders whose varied transactions behaviour are the key to understanding the overall transactions velocity.

Fisher began this task by enlisting the help of Yale students. After an initial disappointing survey, in which he asked respondents for annual amounts of expenditure and cash balances (and which he supposed that they merely guessed), he then asked for volunteers to undertake a more systematic survey. He asked them to keep records of their cash expenditure and cash balances each day for a month as a way to get some reasonably accurate statistics on velocity or turnover of money in exchange for goods and services. He gained 116 good quality responses, of which 113 were students. These data provided him with an average velocity (money spent during the month divided by average cash balance in their pocket) of 66. The returns enabled him to divide the total sample into sub-samples according to total expenditure, and so to calculate associated velocities, showing a rising scale of velocities from 17 (for the lowest category of total expenditure) to 137 (for the highest). The average stock of money in the pocket overnight rose with the day's average expenditure, but fell as a proportion of that expenditure. These investigations fed into his discussions about the determinants of velocity - and what made it change, and about what effects changes in velocity had on other entities in the equation of exchange.[14]

In this period of work, attempts to measure velocity take it for granted that velocity has to be measured as far as possible directly - either by sample survey, or by figuring out circulations of money - or indirectly by trying to get at velocity measures through other payment measurements or even by plausible guess work with them. Though the equations of exchange come in, they are regarded as calculation checking devices rather than measuring instruments in their own right. With Irving Fisher, we see two new kinds of measuring instruments being developed to apply to the problem of measuring velocity, one based on a sampling strategy, the other on a representational model. Neither of these instruments seems to have turned into the kind of standardized quantitative rules - to use Porter's terminology - that betoken well accepted measuring instruments, although as we shall see, there have been later uses of the cash-loop model, and there have been later sample survey investigations.

3.2 Interlude on Concepts

Concepts form one of the important elements in measuring instruments. It is often held that we need a theory of what governs the behaviour of an entity before we can measure it. This seems to require too much. First, there are good examples in measurement history where reliable measuring instruments have been constructed on theories which turn out later to be wrong - eg thermometers.[15] Second, the history of economic measurement suggests that conceptual material is required, but not necessarily a causal account or theory of the behavioural variations in the element being measured. For example, we need a clear concept of the difference between the cost of living and the standard of living to determine relevant index number measuring

[14] See Chapter VIII of Fisher 1911, to which the report of the Yale experiment forms an Appendix, pp379-382.

formulae, but we don't have to commit to the causes of changes in the cost of living to determine the relevant measuring formula.

In this context, Holtrop's 1929 discussion of early theories of the velocity of circulation, makes an interesting distinction between two concepts of velocity. One, held by Petty (and by Cantillon), understands the idea of velocity as a circular course in which we measure the time taken by money to travel around the circle of payments: ie the relation between circulation distance and time - a "motion-theory" concept. Holtrop's expression of his insight is striking: "The partisans of the motion theory are more or less inclined to regard the velocity of circulation as a property of money, as a kind of energy which is inherent to it. ... If, however, the velocity is a property of money, then the supply of money is not a singular but a compound magnitude, being constituted of the product of quantity and velocity." (1929, p 522.) In contrast, according to Holtrop, we also find in early work, the concept of a "cash-balance theory" or the total need of money as in a position of rest in the economy - to which the velocity of circulation is inversely proportional (a position he attributes to Locke). Holtrop suggests we understand this concept as a focus on the demand side of money in which "the size of cash balances is [dependent] .... on the will of the owner, which is governed by economic motives." (1929, p 523.)

In looking at these earlier methods of taking measurements of velocity (rather than theorizing about it), we can see that they rely on concepts of velocity, but the positions do not seem to map onto Holtrop's account. We saw a "need of money" argument made by Petty, not for cash balances or money at rest, but as an amount of money needed for circulation - ie Holtrop's motion-theory concept. Irving Fisher's ideas are also difficult to characterize. Holtrop (p 522) argues that Fisher had a cash-balance concept of velocity, but I find this doubtful for his methods of measuring velocity were essentially designed to measure the money flow. Although his sample survey method appears to be measuring the cash balance, Fisher's idea was to measure the cash as it went

[15] See Chang's 2004 book on the history of measuring heat.

through his student subjects' pockets each day, just as, in his cash loop method, he used the point of payments into and out of a position of rest as a way to get at the payment flows themselves. His idea of money velocity can be well characterized by Holtrop's idea of an "energy" or compound property of money, somehow inseparable from its quantity.

Open disagreement about conceptual issues in discussions of velocity in the late nineteenth century continued into the interwar period. The masterly treatment of these arguments by Arthur Marget (1938) in the context of the theory of prices provides an exhaustive analysis of theorizing about velocity. But Marget's analysis and critique could not stem the tide; in place of the earlier "transactions velocity" (the number of times money changes hands for transactions during a certain period and the concept that we have found in the examples of measurement from Petty to Irving Fisher), velocity was re-conceived as "income velocity": the number of times the circular flow of income went around during the period. Although the velocity measures of the later 20th century are based on thinking about the individuals' demand for money in relation to their income, and might even be considered a cash-balance approach, this "hybrid" concept of velocity came to be "expressed" as the ratio of national income to money in circulation. As we shall see, the issue of compound properties comes back to strike those grappling with the problem of uncertainty and variability in these measurements of monetary aggregates at the U.S. Federal Reserve Board.

3.3 Indirect Measurement of Income Velocity

Economists considering questions about velocity in the latter half of the 20th century have tended to stick with this income notion of velocity, not only in discussion, but also in measurement. Yet their measurements are far from providing numbers that fit the concept of the individual's demand for money implied by Holtrop or understood by the early Cambridge School. Rather, since 1933, measurements have been made on the basis of aggregates, far from the conceptual requirements of individual demand.

Michael Bordo's elegant *New Palgrave* piece on Equations of Exchange (1987), discussed how equations of aggregate exchange, considered as identities, have been important in providing building blocks for quantity theories and causal macro-relations. Not only for theory building, for, as we have seen, equations of exchange provided resources for measuring the properties of money. In the work of Kemmerer and Fisher, their equation of exchange, the identity MV=PT, provided a checking system for their independent measurements of transactions circulation and so velocity. In the more recent history of velocity, the income equation of exchange, namely M=PY/V, has formed the basis for measuring instruments that enable the economist to measure velocity indirectly without going through the complicated and serious work of direct measurement done by Fisher and Kemmerer.

This income equation of exchange, rearranged to provide: V=PY/M (velocity = nominal income divided by the money stock), became a generic measuring instrument for velocity in the mid 20th century, generic in the sense that different money stock definitions provide different associated velocities, and different income definitions and categories alter the measurements of velocity made. For example, Richard Selden's 1956 paper on measuring velocity in the US reports 38 different series of "estimates" for velocity made by economists between 1933 and 1951 and adds 5 more himself. They use various versions of aggregate income as the numerator (personal, national income, even GDP) and various versions of M as the denominator. These are called "estimates" both because the measurers could not yet simply take their series for national income (or equivalent) and money stock ready made from some official source (national income figures were only just being developed during this time), and because many of the measurers, as Selden, wished to account for the behaviour of velocity. They wished to see if it exhibited long-term secular changes, so understood themselves to be estimating some kind of velocity function to capture the changing level of velocity as the economy developed. Like the late nineteenth century measurers whose work we have considered already, there was considerable variation in the outcome measurements.

Boumans (2005) has placed considerable emphasis on variation and invariance in measurement. It is useful to think about that question here. Clearly, we want our measuring instrument to be such that it could be used reliable over periods of time, and could be applied to any country for which there are relevant data, to provide comparable (ie standardized) measurements of velocity. At the same time, we want our measuring instrument to capture variations accurately, either between places or over time. In the context of this measuring instrument, clearly if the ratio were absolutely constant, velocity would also be unvarying, so that the formula would not be needed once we had found the constant, something like a natural constant perhaps. If velocity is not a natural constant, does the formula work well as a measuring instrument - like for example a thermometer - to capture that variation? From the formula, V=PY/M, we can see that variation in both the numerator and denominator may cause alterations in the measurements for V. It appears to operate as a measuring instrument capturing variations in velocity, but in fact these variations are reflections of changes in one or other of the money supply or nominal income. How are we to interpret the velocity that we measure in this way? And what are the sources of velocity's independent freedom for variation when the equation V=PY/M is used as a measuring instrument?

One economist who, without using this language of measuring instruments, has taken an interpretation close to denying velocity any independence or autonomous variation is Benjamin Friedman (1986). He, for the most part, keeps "velocity" in quotes, partly to remind us that the velocity measured with nominal income is not a true velocity in the sense of the transactions velocity of the older measurement, but partly as well to point to its lack of independent conceptual content:

> .... it is useful to point out the absence of any economic meaning of "velocity" as so defined - other than, by definition, the income-to-money ratio. Because the "velocity" label may seem to connote deposit or

> currency turnover rates, there is often a tendency to infer that "velocity" defined in this way does in fact correspond to some physical aspect of economic behavior. When the numerator of the ratio is income rather than transactions or bank debits, however, "velocity" is simply a numerical ratio. ... The issue of money or credit movements versus their respective "velocities", in a business cycle context, is just the distinction between movements of nominal income that match movements of money or credit and movements of income that do not, and hence that imply movements in the income-to-money or income-to-credit ratio. (Friedman, 1986, p 411-2.)

If we observe variations in the numbers produced for such "velocity", it alerts us to changes in the nominal income that are not due to increases in the money (or credit) supply. It offers a way to decompose changes in nominal income across different business cycles, but it is not something that can represent independent variation in velocity: "Saying that money growth outpaced income growth because velocity declined is like saying that the sun rose because it was morning." (Friedman, 1988, p 58.) Friedman is effectively denying an autonomous or independent status to such a velocity: the equation operates to produce numbers, and these are taken as an indicator for something else, but in terms of the representational theory, there is no entity - no independent well-conceptualized thing called velocity - there to be measured.

In the 1980s, the Governors of the Federal Reserve Board also grappled with the problem of what velocity is when measured by such an equation. For example, the transcript of the Federal Open Market Committee Meeting (FOMC) Meeting for July 6-7th of 1981 finds its members arguing over which version of M1 to target (M1, M-1A or M-1B). The level of uncertainty in setting the target ranges for money supply growth was high, and it was an uncertainty that came from several sources. First there was the normal problem of predicting the economic future of the real economy and the monetary side of the economy in relation to that. Secondly, and equally problematic, seemed to be the uncertainty associated with the difficulty of locating a reliable measure of money supply when the stable trend in its growth broke down in the early 1980s. This may have been to do with institutional changes and people reacting by "blurring" distinction between transactions and savings balances. As Chairman Volcker expressed it is "not that we know any of these things empirically or logically" (p 81).

The difficulties of locating a money supply definition that provides stability in measuring the relevant concept of money was matched in - and indeed, intimately associated with - the problem of velocity measurement.[16] The target ranges discussed in the committee were understood to be dependent on both what happened to a money stock that was unstable and a velocity that was subject to change. The instability of the money stock measurements were understood to be not only normal variation as interest rates changed, but also more unpredictable changes in behaviour because of innovations in the services offered to savers.[17] Those factors in turn were likely to affect the velocity of money, if conceived as an independent entity. Here though, the situation is further confused by the fact that, as the Governors were all aware, the velocity numbers that they were discussing were not defined nor measured as independent concepts, but only by their measurement equation - namely as the result of nominal income divided by a relevant money supply. Thus, variations in velocity were infected by the same two different kinds of reasons for variations as the money supply. Velocity was as problematic as the money stock. The difficulties are nicely expressed in this contribution from Governor Wallich:

> We seem to assume that growth in velocity is a special event due to definable changes in technology. But if people are circumventing the need for transactions balances right and left by using money market funds and overnight arrangements and so forth, then really all that is happening is that M-1B is becoming a smaller part of the transactions balances. And its velocity isn't really a meaningful figure; its just a statistical number relating M-1B to GNP. But it doesn't exert any

[16] For background to the troubles the Fed had in setting policy in this period, see Friedman, (1988).

[17] This may be interpreted as Goodhart's Law, that any money stock taken as the object of central bank targeting will inevitably lose its reliability as a target. However, the reasons for the difficulty here were not necessarily financial institutions finding their way around constraints but the combination of expected savings behaviour in response to interest rates and unexpected behaviour by savers in response to new financial instruments.

> constraints. That is what I fear may be happening, although one can't be very sure. But that makes a rise in velocity more probable than thinking of it in terms of a special innovation. (FOMC transcript, July 1981, p 88.)

Because of these causes of variations in the money supply, velocity variations also appeared unpredictable. Either velocity was a meaningful concept, but is variations were unpredictable, and so it provided no anchor or constraints; or it was merely the ratio of GDP to M-1B, and so provided no independent anchor or constraint. Either way, it was no help in the problem of predicting the future range of money supply and so targeting.

These knotty problems experienced in the early 1980s are neatly dissected in a presentation on velocity to the October 1983 meeting of the FOMC by Stephen H. Axilrod from the Fed staff:

> Velocity is of course the link between money and GNP in the equation of exchange (MV=PY), but whether its behavioral properties are sufficiently stable or predictable to provide a strong basis for monetary targeting as a means of attaining ultimate economic objectives over time has, as we all know, been a continuing subject of intensive economic debate. At one extreme, velocity might be considered as no more than the arithmetic by-produce of forces acting independently on the supply of money and other forces acting independently on GNP - hence, an economically meaningless number and making the whole equation of exchange useless as a policy framework. At the other extreme velocity might be found to have a trend all of its own - hence providing a reasonably predictable link between money and GNP, and giving policy content to the equation of exchange.
>
> From another viewpoint, velocity can be considered as the inverse of the demand for money relative to GNP. If we can know what influences the demand for money - and among the factors explaining money demand are income, transactions needs, interest rates, wealth and institutional change - then we can predict the money needed, for, say, a given GNP. But the more one has to go beyond income or transactions needs in explaining money demand, the weaker is the argument for pure or rigid monetary targeting. (Axilrod, 1983 p 1)

So, measured velocity has three faces. On one side, it is simply the measured ratio between two things, each of which are determined elsewhere than the equation of exchange: because velocity has no autonomous causal connections, it provides for no measure of V that can be used for policy setting. On the second, it exhibits its own (autonomous) trend growth rate (sometimes unreliably so) which could be useful for prediction and so policy setting for the two elements from which it is measured. On the third, it has a relationship to the behaviour of money demand, a relationship which is both potentially reliable and potentially analysable, so that it could be useful for understanding the economy and for policy work, but here the focus seems to have reversed itself: understanding the determinants of velocity now seems to be the device to understand the behaviour of the money stock, even while the measuring instrument works in the opposite direction.

Standing back and using our ideas on measuring instruments, it seems clear that the problem in the early 1980s was not so much that the instrument was just unreliable in these particular circumstances, but that the instrument itself has design flaws. In taking the formula V=nominalGDP/money stock as a measuring instrument that is reliable for measuring velocity, there is a certain assumption of stability within the elements that make up the measuring instrument and within their relationships. If the dividing line between velocity and money supply is not strict, the latter cannot be used as a reliable component in a measuring device intended for the former. It is rather like using a thermometer where the glass tube and the mercury column keep dissolving into each other. There are two senses in which this problem might be understood in the velocity case. First the changes in behaviour of people and in categorization of elements mean that there is a switching between what counts as the money quantity and what counts as the velocity category. This seems to be a generic problem in this field of economics, for as Tom Humphrey has so astutely remarked in his history of the origins of velocity functions, “one era’s velocity determinants become another’s money-stock components.” (Humphrey, 1993, p 2.) The second is, as Holtrop characterised it - we may really have a compound property, and so, despite the measurement formula, velocity can not be separated out from the money stock. In terms of Porter’s trust in numbers, we have a standard quantitative rule to measure velocity, one supported by a well-respected bureaucracy and vast amounts of data collection and manipulation, but the measuring device lacks certain characteristics which make us believe that its numbers are trustworthy. It lacks the requirements of invariance specified by Boumans for measuring instruments because the device does not capture the independent variations in the thing being measured. It fails also in Suppes' representational theory of measurement in that the mapping between empirical and numerical structures seems not to be operational.

3.4 Regression Measures of Velocity

These 2nd and 3rd faces we find mentioned by Axilrod are ones that many economists have taken up when they assume that velocity does indeed have its own behaviour. Arguments over what determines the behaviour of velocity and whether it declines or rises with commercialization were a feature of our late 19th and early 20th century measurers. They can be seen as following suggestions made by many earlier economists (mostly non-measurers) who discussed both the economic reasons (such as changes in income and wealth) as well as the institutional reasons (in changes in level of monetization or in financial habits) for velocity to change over time.[18]

In the twentieth century, economists have assumed that velocity can be investigated just like any other entity through an examination of the patterns made by its measurements. Some, like Selden (1956) have used correlations and regressions to try to fix the determinants of variations in velocity. Particular attention was paid to the role of interest rates in altering people's demand for money and so velocity. Selden himself reported regressions using bond yields, wholesale prices and yields on common stocks to explain the behaviour of velocity (though without huge success). More recently and notably, Michael Bordo with Lars Jonung (1981 and 1990), have completed a considerable empirical investigation into the long run behaviour of velocity

[18] Excellent accounts of economists' attempts to explain velocity behaviour can be found in Humphrey, 1993 and of these institutional changes can be found in Friedman, 1986.

measurements using regression equations to fix the causes of these behaviours statistically and thence to offer economic explanations for the changes implied in the velocity measurements. Others have argued that there is no economically interesting behavioural determinant, that velocity follows a random walk and can be characterised so statistically (for example Gould and Nelson, 1974).

The use of regression equations in the context of explaining the behaviour of velocity is but one step removed from using regression equations as a measuring instrument to measure velocity itself. Regression forms a different kind of measuring instrument from those of direct concept based measurement used from Petty to Irving Fisher and from the indirect measurement using equations of exchanges (such as those surveyed by Seldon or in the Fed's formulae). The principles of regression depend on the statistical framework and theories which underlay all regression work. An additional principle here is that by tracing the causes which make an entity change, we can track the changes in the entity itself. Alfred Marshall suggested this as one of the few means to get at velocity behaviour: "The only practicable method of ascertaining approximately what these changes are is to investigate to what causes they are due and then to watch the causes." (Marshall, 1867-74, 1975, p 170.) Marshall did not of course use regression for this, but his point is relevant here: regression forms a measuring instrument not only for tracing these changes but for measuring them - and so velocity - too.

One option has been to use regression to measure or "estimate" velocity by using the opportunity cost of holding money as the estimator (see for example, Orphanides and Porter, 1998). The velocity concept they measured here is an entirely different one from the directly observable entity of the kind supposed or measured by Fisher. By constitution, it is an idealized entity named the "equilibrium velocity". This may be well-defined conceptually, but it is a different concept altogether from those sought and measured in earlier times.

Another example combines the regression measuring instrument with that of Fisher's transactions loop model. Mars Cramer (1986) set out to measure the transactions velocity for the US in the post-war period. He began with Fisher's cash loop idea to get at measures of currency velocity, and then developed the equation of exchange into a form which included a parameter for "hypothetical pure transactions velocity". This parameter was measured using regression and then plugged back into his equation of exchange to provide the series of measurements of velocity over the period showing a rise in the transaction velocity of demand deposits. Clearly, Mars Cramer rivals Irving Fisher's inspired ingenuity as a measurer. And he brings us back almost to where we started, but not quite, for he too is now purporting to measure a different concept - an idealized version of the transactions velocity.

Conclusion: Measurements that Travel Well

It is easy to put a number to the "velocity of money" using an indirect calculation via the equation of exchange V=PY/M, but a difficult thing to measure with any confidence using that standard method. The standardized quantitative rules this measuring instrument entails do not seem to provide numbers that adequately represent the property we are trying to measure even though the data fed in come from trustworthy sources. The model seems to act as a measuring instrument, yet does not have the reliability we require in such instruments. We can understand this equation of exchange as a numerical relational structure, but it is not quite clear that it is an empirical relational structure. In past times, when the monetary economy was rather stable, the numbers it produced were considered reliable and travelled well. When times proved unstable, it became clear that this was a case of feeding trustworthy data into an untrustworthy measuring instrument: the velocity of money numbers it produced could not be used as "hard facts" upon which action could be based.

We have also seen that earlier economists tried to secure trustworthy measurements of velocity using a variety of other kinds of measuring instruments: sample surveys, direct calculation, and so forth. These often relied on feeding in guesstimated data, of rather poor quality compared to modern data. Consequently, the numbers generated in these earlier times have not travelled well into the present. Yet these earlier measuring instruments were rather better designed to create good measurements. Even though these instruments have their individual problems, their design treats velocity as a separate, conceptually well defined, entity, and so the measurements they produce might well be more sustainable and so trustworthy.

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