I have valued statistics as an instrument to help fulfill one of the great ambitions of my life, namely, to do what I could toward making economics into a genuine science. — Irving Fisher (1947a)

Introduction

Irving Fisher was unusual among great economists in that he not only made lasting contributions to economic theory (particularly the theory of interest and of intertemporal economics in general), but he took care to confront his theories with the facts in a way that no economist had done before. In this paper I have selected three of Fisher’s contributions to this end. The first is his attempt to find a way to measure marginal utility from data on consumer demand in competitive markets. The second is his attempt to test the quantity theory of money. And the third is his pioneering work on disequilibrium dynamics, in particular, his method of relating cyclical fluctuations in output and employment to distributed lags of rates of inflation — thus providing quantitative confirmation of an important insight that goes back to Hume (1752).

As will become apparent in the following analysis, Fisher was not perfect. In the first of these investigations, ingenious as it was, he concluded that (with hypothetical data) a progressive income tax would be justified, whereas a contradiction between his assumptions and his hypothetical data brings to light the fact that had he made use of his implicit assumptions (in particular, the assumption that expenditure shares are independent of prices), his own analysis would have concluded that the optimal income tax was proportional. In the second of these investigations, somehow he overlooked, or allowed himself to overlook, the fact that his method of testing the equation of exchange violated his own “factor reversal test” of index-number theory, so that he ended up testing whether a Laspeyres price index furnished a good approximation to a Paasche price index. In the third of these investigations, his failure to predict and explain the downturn of output and employment in the Great Depression appears to have been the result of his failure to believe in his own theory, which turned out to explain the circumstances remarkably well.

1 Measuring the Marginal Utility of Money

Approaching economics from the perspective of mechanics, Fisher in his doctoral dissertation (1892) showed himself to be a firm believer in measurable utility. However, he strongly disagreed with the psychophysical argument used by Edgeworth (1881, p. 99): “Just perceivable increments of pleasure are equatable.” Instead, Fisher countered (1892, p. 5):

I have always felt that utility must be capable of a definition which shall connect it with its positive or objective commodity relations. A physicist would certainly err who defined the unit of force as the minimum sensible of muscular sensation.

... This foisting of Psychology on Economics seems to me inappropriate and vicious.

He thus set himself the task of justifying the measurability of utility on the basis of economic considerations alone.

He started out by stating that: “The laws of economics are framed to explain facts” (p. 11). The fallacy of equating utility with pleasure was exposed by the example: “the last dollar’s worth of sugar (we are told) represents the same quantity of pleasurable feeling as the last dollar’s worth of dentistry.” Instead of pleasure, he argued, the correct criterion was “desire,” and this was to be inferred simply from the “economic act of choice.”

The basis of Fisher’s construction is the hypothesis of independence of commodities in the sense that the consumer’s utility function has the additively separable representation

\[ U(q_1, q_2, \ldots, q_n) = \sum_{i=1}^{n} u_i(q_i), \]

where \( q_i \) is the quantity consumed of commodity \( i \). This implies that the marginal utility of any commodity depends on the quantity of that commodity alone, i.e., \( \partial U/\partial q_i = u_i(q_i) \). Fisher proceeded to argue that on this hypothesis it would be possible to measure utility, in the sense that, given an arbitrary value for \( u_1(q_1) \), then for any other \( q_i \), say \( q_1^2 \), one could determine the value of \( u_1(q_1^2) \), i.e., one could determine the ratio of marginal utilities \( u_1(q_1^1)/u_1(q_1^2) \). His argument for this was, however, very perplexing. He proceeded as follows (1892, pp. 10-11), where “utility” means “marginal utility” unless preceded by the adjective “total”, and “infinitesimal utility” means the same as “marginal utility”:

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Already there is a problem. Let us say that the marginal utility of the 100th loaf of bread is $u'_4(100)$ and that of the 10th gallon of oil $u'_6(B)$. Fisher does not define $B$ units of oil as that number whose marginal utility is equal to that of 100 loaves of bread; rather, the quantity $B$ is apparently chosen independently (prices are not mentioned), and it must be $B + \Delta B = B + \beta$ whose marginal utility is equal to that of 100 loaves of bread; but there is no reason why $\beta$ should be "small" — and it would be "infinitesimal" only if $u'_6(B) = u'_4(100)$. Fisher continues:

Now in substituting the hypothesis of 150 loaves let us not permit our individual to alter $B$, his consumption of oil. The utility of the 150th loaf will be pronounced by him equal (say) to the utility of $\frac{1}{2}\beta$. Then the utility of the 150th loaf is said to be half the utility of the 100th.

But on what basis can our individual "pronounce" that the marginal utility of 150 loaves of bread is equal to $\frac{1}{2}\beta$? Already there is a confusion: the marginal utility of the 100th loaf of bread is now said to be $\beta$, and that of the 150th loaf $\frac{1}{2}\beta$, whereas $\beta$ was previously defined as that increment in $q_2 = B$ whose marginal utility is equal to that of the 100th loaf of bread. Further, so long as $u'_4$ and $u'_6$ are decreasing functions, obviously a larger increment in $q_2$ (say $2\beta$) would be needed in order for $u'_6(B + 2\beta) = u'_4(150)$. A continuation of Fisher's text confirms the basic confusion:

That is, if:

\[
\begin{align*}
\text{ut. of 100th loaf} &= \text{ut. of } \beta, \quad B \text{ being the total}, \\
\text{and ut. of 150th loaf} &= \text{ut. of } \beta/2, \quad B \text{ being the total again},
\end{align*}
\]

the ratio is defined:

\[
\frac{\text{ut. of 100th loaf}}{\text{ut. of 150th loaf}} = \frac{\beta}{\beta/2} = 2.
\]

Thus, "ut. of $\beta$" in the first displayed formula is replaced by "ut. of $\beta$" in the second, and "ut. of $\beta/2$" is replaced by "ut. of $\beta/2$" in the second. Perhaps the ambiguity in the phrase "the utility of $\frac{1}{2}\beta$" is what led Fisher astray: it could mean either "$u'_6(B + \frac{1}{2}\beta)$" or "the utility to the amount of $\frac{1}{2}\beta$". But it is curious indeed that one who argued so strongly in favor of the

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1This assumes $\beta > 0$. If, however, $u'_6(B) < u'_4(100)$ then $u'_6(B + \beta) = u'_4(100)$ requires $\beta < 0$. It is then logically possible that $u'_6(150) = u'_6(B + \frac{1}{2}\beta) < u'_6(B + \beta) = u'_4(100)$. Even if this is so, however, so long as the functional form of $u_6(q_2)$ is not known—hence there is no numerical determination of $u'_6(B + \beta)/u'_4(B + \frac{1}{2}\beta)$—there is then no numerical determination of $u'_4(100)/u'_6(150)$ by this method. The second ratio can no more be determined than the first; all we can say is that the ratios are equal.
pictorial functional form specified for \( u_i(q_1) \), and this is a special question. One possible form is \( u_i(q_1) = \alpha_1 \log q_1 \), yielding \( u_i(q_1^1) / u_i(q_1^2) = q_1^2 / q_1^1 \); another is \( u_i(q_1) = \alpha_1 q_1^\rho \) where \( 0 < \rho < 1 \), yielding \( u_i(q_1) / u_i(q_1^2) = (q_1^2 / q_1)^{1-\rho} \); these are related by the transformation \( u_i(q_1) = \alpha_1 (\exp(u_i(q_1^2)))^{1/\alpha_1} \), which is certainly not linear. The problem of finding the right functional form for each \( u_i(q_1) \)—given the hypothesis (1.1)—is essentially the integrability problem of utility theory (specialized to this case), and for this one needs data on prices and demand functions. In thinking that it was possible to measure marginal utility without any data on prices and consumers’ choices in competitive markets, Fisher went quite far astray.

One might have expected this subject to come up again in Fisher’s “little treatise” on the calculus (1897) which appeared five years later; but most of the illustrations in that book were from physics. \(^2\) In his elementary textbook (1912) there was a thorough treatment of the principle that “the market price of any good is equal to the ratio between its marginal desirability [read: marginal utility] and the marginal desirability of money for each and every buyer” (p. 295)—but no attempt to show how this “desirability” could be measured.

We may suppose that Fisher became unconvinced by his own 1892 argument and sought a method of measuring utility on the basis of observed data on quantities purchased at various prices and incomes. This is the problem he set himself in his contribution to the

\(^2\) There was one example of compound interest (p. 32a), one of a profit-maximizing competitive firm (p. 48), and one from demography (pp. 50–51), but the rest were from geometry and physics. The only discussion of utility was the mention of the distinction between marginal and total utility (p. 65).
In 1925, he reported on data of a family in Denmark, and as a "measuring rod" (p. 100) of "measuring rod" (p. 100), while observation sets 1 and 3 were interpreted as drawn from "Oddland" in such a way that, prices differing as between the two countries, family incomes were such that family 1 in Oddland consumed the same amount of food, and family 3 in Oddland the same amount of housing, as family 2 in Evendland (I shall identify food and housing as commodities 1 and 2 respectively).

Setting marginal utilities proportional to prices we have

\[ \frac{\partial}{\partial q_i^t} U(q_1^t, q_2^t, \ldots, q_n^t) = \omega_i^t p_i^t, \]

where \( \omega_i^t \) is the marginal utility of income of family \( i \). Further, defining the demand functions by \( q_i = h_i(p, Y) \), where \( p = (p_1, p_2, \ldots, p_n) \), the share of commodity \( i \) in the consumer's expenditure (assumed equal to income) is defined as

\[ \theta_i^t = \frac{p_i^t q_i^t}{Y^t} = \frac{p_i^t h_i(p^t, Y^t)}{Y^t}. \]

The quantities demanded are therefore obtained from \(^4\)

\[ q_i^t = h_i(p^t, Y^t) = \frac{\theta_i^t Y^t}{p_i^t}. \]

\(^4\)The circumstances that led Fisher to publish this study are recounted in a circular enclosed by Fisher with a reprint of his article sent to a group of interested economists, which included Jacob Marschak—who quoted an extract from it in his habilitation thesis (1927, pp. 128–130). The following extract from Marschak's footnote seems worth reproducing here:

...After the enclosed paper was published I learned, for the first time, of Dr. Ragnar Frisch's "Sur un Probleme d'Economie Pure" ([1928]) in which, by different methods, the same problem has been attacked. Dr. Frisch not only devised a method but applied it to obtain definite statistical estimates with which my own tentative and unpublished figures were, at least, consistent.

To Dr. Frisch, therefore, belongs the honor of being, so far as I know, the first to publish anything on this difficult subject.

Although the publication of my own method comes later, I had, in unpublished lectures, employed it at least as early as 1922. In an article [Fisher (1928c)] I referred to the intention of publishing this method, but publication was put off from year to year in the hope of first making a full statistical application. Publication would have been still further delayed had it not been for the invitation to furnish immediately a paper for the volume of Economic Essays in honor of Professor J. B. Clark. This led to the decision to publish my method by itself and defer the statistical applications to a later time. . . .

\(^4\)As will be mentioned later in this section, in practice one can obtain data only on groups of commodities, and for these one can generally obtain data on price indices and expenditure shares but not quantity indices other than those defined by (1.4), which explains Fisher's setup.
\[(1.6) \quad \frac{\theta_1^1 y^1}{p_1^1} = \frac{\theta_2^2 y^2}{p_2^2} \quad \text{and} \quad \frac{\theta_3^3 y^3}{p_3^3} = \frac{\theta_4^4 y^4}{p_4^4}.\]

From (1.5) and the assumption of identical preferences it follows immediately that
\[u_1(q_1^1) = u_1(q_1^2) \quad \text{and} \quad u_2(q_2^3) = u_2(q_2^4),\]
hence by (1.2),
\[(1.7) \quad \omega^1 p_1^1 = \omega^2 p_2^2 \quad \text{and} \quad \omega^3 p_3^3 = \omega^4 p_4^4.\]

Now, given the additively separable form of the utility function (1.1), we have from (1.2),
\[(1.8) \quad \omega^3 = \frac{u_1'(q_1^3)}{p_1^3} = \frac{u_2'(q_2^3)}{p_2^3},\]
and this marginal utility is taken by Fisher as numéraire, hence equations (1.7) and (1.6) constitute four equations in the four unknowns \(\omega^1, \omega^3, y^1, \) and \(y^3,\) which are easily solved to obtain
\[(1.9) \quad \omega^1 = \omega^3 \frac{p_2^2}{p_1^1} \quad \text{and} \quad \omega^3 = \omega^3 \frac{p_2^3}{p_3^3}\]
and
\[(1.10) \quad y^1 = \frac{\theta_1^1 p_1^1}{\theta_3^3 p_3^3} y^2 \quad \text{and} \quad y^3 = \frac{\theta_3^3 p_3^3}{\theta_2^2 p_2^2} y^2.\]

From (1.9) and (1.10) as well as data on \(y^2\) and the arbitrary choice of \(\omega^3,\) one can interpolate a curve through the points \((Y^t, \omega^t), t = 1, 2, 3,\) to obtain the marginal utility of income as a function of income.

While Fisher's method is ingenious, it is rather strange, since conditions (1.5) are completely gratuitous—one would never expect to find them fulfilled unless the prices and family incomes in the two countries were experimentally chosen precisely to achieve this outcome. On the other hand, the assumption that data on \(p_1^1, \theta_1^1,\) and \(y^1,\) as well as \(p_3^3, \theta_3^3,\) and \(y^3,\) are missing seems equally artificial. Let us consider, however, what Fisher does with the above assumptions.

First let us examine precisely how Fisher justified taking \(\omega^3\) as given. Before quoting his text it is necessary to comment on his terminology. The expression "marginal utility" appears in the title of his paper in quotation marks, since it was a terminology of which he

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5 This criticism was made by Marschak (1931, p. 139), who also provided a nice graphical exposition of Fisher's procedure. Marschak was quite critical (p. 139n) in an article by Silinović (1930) commenting on Fisher's method, so I have not thought it necessary to add my own comments.
may be regarded as an abbreviation either of “wantability” or of “want tab” (i.e., a unit for keeping tab on the strength of a want).

Returning now to equations (1.6) and (1.7), but replacing Fisher’s notation by the above, we have in his words (p. 171):

What has been done is to solve these four equations to obtain the four unknowns \( \omega^1, \omega^3, Y^1, Y^3 \), assuming \( Y^2 \) and \( \omega^2 \) as known, the former in dollars and the latter being, for convenience, taken as the standard for measuring wantability since no other unit has previously been established.

Let us then take \( \omega^2 \) in (1.8) as a fixed number, say \( \omega^2 = 1 \); then the remaining two \( \omega^j \) are obtained from (1.9). To find how they relate to income one must evaluate (1.10). Here we must note how Fisher supposes the \( \theta_j^1 \) to be obtained. Regarding \( \theta_j^1 \) he states (p. 167):

This percentage is readily found from the budget tables. Suppose it to be 50%. That is, the budget tables of Evenland show that in a family there which has an income and annual expenditure of only $200, 50% thereof is spent for food.

Likewise for \( \theta_j^1 \) (p. 168):

The family budget tables in Oddland show, let us say, that a family which spends $400 for food is one which spends thereof 40% of its total expenditure; that is, \( \theta_j^1 = .4 \).

What these two quotations show is that Fisher is making the implicit assumption that the share of a commodity in total expenditure depends on that total expenditure only, independently of the prices; but since the shares \( \theta_j(p, Y) \) must be homogeneous of degree 0, it follows that they are also independent of total expenditure (income), that is, constant. It follows in turn from this that (1.1) must be of the loglinear type

\[
U(q) = \alpha \sum_{i=1}^{n} \theta_i \log q_i, \quad \text{where } \alpha > 0, \quad 0 < \theta_i < 1, \quad \text{and } \sum_{i=1}^{n} \theta_i = 1. \tag{1.11}
\]

This is proved in the following theorem, which may be skipped by the reader interested only in the result.

THEOREM. Let a system of demand functions \( q_i = h_i(p, Y) \) be generated by maximizing an additively separable utility function (1.1) subject to the budget constraint \( \sum_{i=1}^{n} p_i q_i = Y \), and suppose the expenditure shares \( \theta_j(p, Y) = p_j h_j(p, Y)/Y \) are constants, \( \theta_j \). Then the
Applying the Antonelli–Allen–Roy partial differential equation $\partial V/\partial p_i = -h_i \partial V/\partial Y$ we obtain

$$-u'_j \left( \frac{\theta_j Y}{p_j} \right) \frac{\theta_j Y}{p_j} = -\frac{\theta_j Y}{p_j} \sum_{i=1}^{n} u'_i \left( \frac{\theta_i Y}{p_i} \right) \frac{\theta_i}{p_i}.$$ 

Cancelling like terms from both sides and introducing the transformation of variables $p_i = \theta_j Y/q_i$, this becomes

\begin{equation}
\tag{2.1}
u'_j (q_j) q_j = \theta_j \sum_{i=1}^{n} u'_i (q_i) q_i.
\end{equation}

Now differentiating this equation with respect to $q_k$ for $k \neq j$, we obtain

$$0 = \theta_j \left[ u'_k (q_k) + q_k u''_k (q_k) \right], \quad \text{hence} \quad \frac{d \log u'_k (q_k)}{d \log q_k} = \frac{q_k u''_k (q_k)}{u'_k (q_k)} = -1.$$ 

Since $j$ was arbitrary, this must hold for every $k = 1, 2, \ldots, n$. Integrating this equation we get

\begin{equation}
\tag{2.2}
u'_k (q_k) = \alpha_k / q_k \quad \text{for} \ \alpha_k > 0,
\end{equation}

and integrating once again gives

\begin{equation}
\tag{2.3}
u_k (q_k) = \alpha_k \log q_k + \text{constant} \quad (\alpha_k > 0).
\end{equation}

Substituting (2.3) into (2.1) for $k = j$ we see that

$$\alpha_j = a \theta_j$$

where

$$a = \sum_{i=1}^{n} \alpha_i.$$ 

Consequently, substituting (2.3) into (1.1) but ignoring the superfluous constant term, we obtain the sought result. Likewise, substituting (2.3) into (1.2) we obtain $\omega(p, Y) = \partial V(p, Y)/\partial Y = a / Y$. Q.E.D.

This implication of constant expenditure shares (combined with his assumption of identical tastes within and between Oddland and Evenland) is in fact violated by Fisher’s numbers, which show $\theta_1^2 = 0.5 > 0.4 = \theta_1^1$ (likewise $\theta_3^2 = 0.20 < 0.25 = \theta_3^1$) in Table 1 below. Likewise, (1.12) shows that $\omega^Y = a = a$ property not satisfied by Table 2 below.

The following table displays the numerical values assumed by Fisher:
from (1.10). This allowed Fisher to draw a curve through the points \((Y^t, \omega^t)\) given by the following completed table for these variables:

<table>
<thead>
<tr>
<th>(t)</th>
<th>(Y^t)</th>
<th>(\omega^t)</th>
</tr>
</thead>
<tbody>
<tr>
<td>2</td>
<td>000</td>
<td>1.00</td>
</tr>
<tr>
<td>1</td>
<td>1000</td>
<td>0.75</td>
</tr>
<tr>
<td>3</td>
<td>1440</td>
<td>0.384\frac{1}{3}</td>
</tr>
</tbody>
</table>

(p. 170, Chart II, except that Fisher shows only the curve through the last two of these points).

Although Fisher did not explicitly mention it, his method also allows measurement of the marginal utilities of the individual commodities, provided additional data are available. From formula (1.2), it is clear that to obtain these measurements one must infer what the missing price data must have been in order to bring about condition (1.5). If data were available on \(q_3^t\) and \(\delta_3^t\) one could infer \(\rho_3^t\), and similarly if data were available on \(q_1^t\) and \(\delta_1^t\) one could infer \(\rho_1^t\). Then one could tabulate pairs \((q_i^t, \omega_i(q_i^t)) = (q_i^t, \omega^t p_i^t)\) for \(t = 1, 2, 3\) and interpolate a marginal utility function, and in particular determine \(\omega_i(q_i^t)/\omega_i(q_i^2)\) provided \(q_i^1 \neq q_i^2\), thus solving the problem he posed in 1892.

There is one aspect of Fisher’s model that I have not mentioned, in order to avoid mixing up pure theory and the problem of aggregation. Fisher thought of the price variables in the above analysis as price indices of groups of commodities. With statistical application in view, this is only reasonable. But he did not comment on the difficulties that might be faced in formulating the pure theory in terms of aggregates. On the other hand, he did use aggregation as a way of defending his hypothesis of independent marginal utilities. Thus (p. 176), he considered it reasonable to assume that there were no substitutes or complements (in the intuitive sense of positive or negative signs of \(\partial U/\partial x_i \partial x_j\) \((i \neq j)\)) between the food group and housing group, etc., while this would not apply within groups.

Another aspect of Fisher’s model that requires comment is that, while it is described as a “statistical method”, it is not statistical in the usual meaning of the term, since it has exactly enough observations to estimate the parameters and no more; in other words, the model has zero degrees of freedom. In contemporary terminology it would be described as a method of “calibration” rather than “estimation.” Fisher himself used the term “triangulation” to describe his procedure (p. 137). In fact on several occasions he stated that he considered Fries’s procedure (1906, 1932) superior to his own; cf. Fisher (1930a, p. 288; 1933, p. 11; 1941, p. 190). And there is no doubt that he would have approved heartily the modern progress that has been attained in estimating systems of demand functions.
By common sense we cut our Gordian knots. We may not know really what goes on in the mind of a dog, but practically we can tell by his behavior when he is hungry, or pleased. We have somehow learned to interpret the wagging of his tail, and the sound of his bark. Even more have we learned to interpret the feelings of another human being.

A return to Fisher's approach may be said to have been made by Samuelson (1947) (but not with Fisher's special assumptions) in his posting of a social welfare function. Fisher's principle of equal sacrifice (p. 189), which assumed that "taxes are small so as not appreciably to affect the income and the want-for-one-more-dollar" (an assumption that could not be accepted today), entailed that for two different families in Oddland, their sacrifices \( \omega^1 \tau^1 Y^1 \) and \( \omega^3 \tau^3 Y^3 \) be equal, where \( \tau \) is the proportionate tax on income (strictly speaking, expenditure) \( Y \). By a series of simple deductions from (1.9) and (1.10) (pp. 184-6) he was able arrive at the second equality of the following formula:

\[
\frac{\tau_3}{\tau_1} = \frac{\omega^1 Y^1}{\omega^3 Y^3} = \frac{\theta_1^2/\theta_2^1}{\theta_3^2/\theta_2^3}.
\]

From the third term in (1.13) he was able to compute, as we see from Table 1,

\[
\frac{\theta_1^2/\theta_2^1}{\theta_3^2/\theta_2^3} = \frac{0.5/0.4}{0.20/0.25} = 1.5625.
\]

Thus, he concluded:

By formula (1.12) we can now find the theoretically just rate of progression (or regression, as the case may be) of an income tax. This formula gives, in our hypothetical example, 1.56. Thus, if out of \( Y^1 = \$1000 \), a tax of 1%, or \$10 is paid, then out of \( Y^3 = \$1440 \) a tax of 1.56% or \$22.46 should be paid (instead of \$14.40 as would be the case under proportional taxation).

What, however, would Fisher have concluded if instead of using the right-most expression of (1.13) he had used the middle expression? Consulting Table 2 we see that

\[
\frac{\omega^1 Y^1}{\omega^3 Y^3} = \frac{600}{480} = 1.25.
\]
2 Testing the Quantity Theory of Money

Central to Fisher's concerns throughout his career were (1) the scientific problem of explaining variations in the purchasing power of money, or its reciprocal, the "price level" or "cost of living" and (2) the policy problem of stabilizing the above. A curious feature of his writings is that, although they took place during the same period, there was a notable lack of integration between these two endeavors. I shall start with the first.

In Fisher (1911b, 1911c, 1913d) we have the formulation of the scientific problem. It starts with the basic "equation of exchange" first written in the form

\[ M^t V^t + M^t V^t = \sum_{i=1}^{n} q_i^t \cdot v_i^t \]

where \( M^t \) denotes the quantity of "money" (hand-to-hand currency) and \( V^t \) its velocity of circulation, both at time \( t \) (I have added the time superscripts), \( V^t \) denotes the quantity of deposits in checking accounts and \( V^t \) their velocity of circulation, both at time \( t \), and \( q_i^t \) and \( v_i^t \) denote the price and quantity traded of commodity \( i \) at time \( t \). It must be kept in mind that although Marshall (1890) had introduced the concept of "national dividend," it was not until Kuznets's researches in the 1930s, culminating in his monograph (Kuznets, 1941), that there were any data available on national income or product, hence Fisher's equation of exchange (2.1) referred to the sum total of transactions including those in intermediate goods and financial securities.

Fisher's first task was to obtain data on the two stock variables \( M^t \) and \( M^t \) and the two flow variables \( M^t V^t \) and \( M^t V^t \). \( V^t \) and \( V^t \) were estimated by taking the quotient of the flow variables and the corresponding stock variables. If \( t \) stands for a year, \( M^t V^t \) was estimated by the total of checks drawn in year \( t \), it being assumed that "each check circulates against goods once and but once" (Fisher, 1911c, 1913d, p. 282). \( M^t V^t \) was estimated by means of a very careful graph-theoretic analysis of the flows of money to and from (as well as within) three groups—depositors, other depositors, and non-depositors (chiefly wage-earners)—using the "reservoir principle" (1909, pp. 511-2) that the net outflow of a reservoir "must equal the net decrease in its contents during the same time" (this latter being judged to be relatively small). He divided \( M^t V^t \) into eight components for

\[^{6}\text{Fisher used } Q \text{ in place of } q \text{ to denote a commodity quantity.}\]
Conversely any particular formula for $T^1$ implies a correlative form for $P^1$. For, since

$$P^1 T^1 = \sum p^1 q^1,$$

it follows that

$$P^1 = \frac{\sum p^1 q^1}{T^1}.$$

By means of this equation, if we have given any particular formula for $T^1$, we may obtain a resultant particular formula for $P^1$.

As an example illustrating the derivation of the formula for $P^1$ from a given formula for $T^1$, let $T^1$ be defined as $\sum p^0 q^1$; then

$$P^1 = \frac{\sum p^1 q^1}{T^1} = \frac{\sum p^1 q^1}{\sum p^0 q^1}.$$

Here, the formula for the index of trade, $T^1 = \sum p^0 q^1$, is the numerator of a Laspeyres quantity index, and the above formula for $P^1$ is of course that of a Paasche price index. While the names Laspeyres and Paasche were not yet attached to these formulas by Fisher (1911c, 1913d)—although the attribution had long before been made by Walsh (1901, p. 99)—the distinction between the two concepts was certainly very clear to him.

The data used by Fisher for the volume of trade $T^*_0$ were expressed in “billions of dollars at the prices of 1909” (1911c, 1913d, p. 290; cf. pp. 478–480); thus, the formula he used for the volume of trade in year $t$ may be expressed as

$$T^*_0 = \sum_{i=1}^{n} p^0_i t_i,$$

(2.2)

For consistency with the notation being used in the text I have taken the liberty of replacing Fisher’s $Q$ by $q$ and his time subscripts by the corresponding superscripts, in this and the following quotation.
hence Fisher’s equation of exchange (2.1) becomes

\[ M^{*}V^{*} + M^{tt}V^{tt} = P^{*}T^{*}. \]

Fisher’s procedure for testing the quantity theory of money, following Kenmerer (1907), was to compute a time series for the variable

\[ \frac{M^{*}V^{*} + M^{tt}V^{tt}}{T^{*}}. \]

which of course is an estimate of the Paasche price index (2.3), and compare it with an independent price-index series. Unlike his description of the construction of the index of trade, Fisher’s description of his method for calculating the price index was quite vague. He stated (1913d, p. 485): “The table in the text for index numbers is taken from the last column of the table on page 487.” The data in the last column of this table are a reusing to 1909 of a weighted average of three series: wholesale prices for 258 commodities obtained from the United States Labor Bureau, wages per hour obtained from the Bulletin of the Bureau of Labor, and prices of forty stocks obtained from Wesley C. Mitchell, “The Prices of American Stocks, 1869–1909,” Journal of Political Economy, May 1910, the weights being 30:1:8. No mention was made by Fisher of the method of constructing the component price indices, but from the subsequent discussion in Fisher (1920, p. 4) it may be assumed that at least the first one was a Laspeyres price index. Consequently, we may assume that the price index with which Fisher compared (2.6) was a Laspeyres index, which may be written as

\[ P^{*}_{0} = \frac{\sum_{i=1}^{n} p_{i}^{0} q_{i}^{0}}{\sum_{i=1}^{n} q_{i}^{0}}. \]

Thus, Fisher’s method of testing the quantity theory of money consisted in seeing how well a Paasche price index (2.3) is approximated by a Laspeyres price index (2.7). It seems inconceivable, given the precise attention he paid to the need to choose a price index of a “correlative form” to the chosen quantity index, that Fisher was unaware of this inconsistency. He could have pointed out the inconsistency and pleaded that the relevant Paasche price-index series were not available; but instead he seems to have papered over the inconsistency, hoping perhaps that the reader would not notice it.\(^8\)

\(^8\)The first edition (1911d) contains the misprint “page 105.”

\(^9\)There was of course the further inconsistency that while the price index was reusing to 1909, the weights were presumably those of an earlier year, perhaps 1890. Even if the weights were those of 1909 (the final year), it would of course still have been a Laspeyres price index, since the crucial property of a Paasche price-index series is that the weights change from year to year.
There is a close analogy between Fisher's procedure, namely that the equation of exchange (2.5)—as Haberler (1924) was subsequently to point out in his criticism of a similar formulation by Schumpeter (1918)—is a mere tautology. If the data correctly cover the indicated variables, the equation should hold identically; any departure from equality can only be attributed to measurement error. But Fisher went even beyond this, to fit not this tautology to the data, but to fit a bad approximation of this tautology to the data! To be sure, Fisher was well aware of the objection that (2.5) was a tautology, but defended himself in the following terms (1911c, 1913d, p. 157):

One of the objectors to the quantity theory attempts to dispose of the equation of exchange ... by calling it a mere truism. While the equation of exchange is, if we choose, a mere "truism," based on the equivalence, in all purchases, of the money or checks expended, on the one hand, and what they buy, on the other, yet in view of supplementary knowledge as to the relation of $M$ to $M'$, and the non-relation of $M$ to $V$, $V'$, and the $Q$'s, this equation is the means of demonstrating the fact that normally the $p$'s vary directly as $M$, that is, demonstrating the quantity theory. "Truisms" should never be neglected. The greatest generalizations of physical science, such as that forces are proportional to mass and acceleration, are truisms, but, when duly supplemented by specific data, these truisms are the most fruitful sources of useful mechanical knowledge.

One can agree with this and still raise the question, does a truism require empirical verification, and the "supplementary knowledge" not require it? The equation of exchange is consistent, for example, with the possibility (during a period of unemployment and excess reserves) of an exogenous rise in the volume of trade bringing about an increase in $M'$; in fact, the events leading to Fisher's subsequent proposal for 100% money (1935b) suggest that the "supplementary knowledge" could have benefited from more systematic empirical study.

In what is perhaps Fisher's last published work (1947b), he challenged "the idea that the velocity of the circulation of money has no theoretical principle behind it" and expressed the opinion that (p. 175):

there is no other situation in the whole economic realm that is subject to so much adjustment and readjustment as the cash balance in relation to its use. This

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It was shown by Bortkiewicz (1925, pp. 376–8) that if the $p_i/q_i$ and $q_i/q_i'$ are negatively correlated for $i = 1, 2, \ldots, n$, then

$$ P^* = \frac{\sum_{i=1}^{n} p_i q_i^*}{\sum_{i=1}^{n} p_i^* q_i} < \frac{\sum_{i=1}^{n} p_i q_i}{\sum_{i=1}^{n} p_i q_i'} = P^*. $$

See also the discussion in Haberler (1927, p. 93), as well as the later and more elaborate treatment by Bortkiewicz (1932).
recommendations he proposed for stabilization of the price level. These latter are contained in his proposal for a “compensated dollar” (1913a–c, 1918c) culminating in his *Stabilizing the Dollar* (1920). This book, calling to mind a similar claim by Goschen (1854), forecast (pp. xxv–xxvi) that “there is coming, slowly but surely, a revolution in economic thought similar to the revolution in astronomical thought begun by Copernicus” owing to the fact that we now possess “in the ‘index number’ of prices the necessary instrument for measuring the value of the dollar in terms of its power to purchase goods.” His proposal was that in each month, say, the level of the index number would be used to change the amount of gold the government would give or take for a gold certificate. A fixed market basket corresponding to the weights in the price index would retain a fixed value.

Leaving aside the disadvantage later pointed out by Haberler (1927, 1929) and Frisch (1936) that the Laspeyres price index overestimates the true rise in the cost of living, Fisher’s scheme assumes that there is a determinate relationship between the stock of money (defined in his case as currency) and the price level. This is what is usually considered to be the quantity theory of money, as opposed to Fisher’s formula which replaces the stock of money ($M$) by the quantity $(M'V + M'V')/T$. In his *Stabilizing the Dollar* Fisher in fact adhered to the theory in the first sense, saying (1920, p. xxxii): “The price level fluctuates largely with the fluctuation in the quantity of money,” and displaying a graph (p. 11) exhibiting the quantity of “money in circulation and in banks,” and the “index number of [prices of] responsive commodities” between 1914 and 1919 (but not providing data sources), and remarking that “changes in the price level [seem] usually to follow changes in the quantity of money one to three months later.” As far as I know, this is the only attempt on Fisher’s part to confront the quantity theory of money (as usually understood) with empirical evidence. But Fisher adopted a strange terminology, as the following passage indicates (1920, pp. 215–6):

The impression that the plan is dependent on acceptance of the quantity theory of money is presumably due to the fact that I have espoused that theory (in a modified form) in my *Purchasing Power of Money*. But there is nothing in the plan itself which could not be accepted equally well by those who reject the quantity theory altogether. On the contrary, ... the plan should seem even simpler to those who do not accept the quantity theory but believe that a direct relationship exists between the purchasing power of the dollar and the bullion from which it is made, than to believers in the quantity theory.

It will be clear to any one who follows the reasoning and explanations in this book, that the only money theory assumed is that common to all theories, and accepted universally; namely, that a large quantity of gold will buy more goods

15
We have emphasized the fact that the strictly proportional effect on prices of an increase in \( M \) is only the normal or ultimate effect after transition periods are over. The proportion that prices vary with money holds true only in comparing two imaginary periods for each of which prices are stationary or are moving alike upward or downward and at the same rate.

Fisher’s principal concern, however, was with transition periods in which a rise in \( M \) would bring about, in addition to a rise in \( M' \), a rise in \( T \) and increases in \( V \) and \( V' \), leading to a more than proportionate rise in the price level, \( P \). His analysis was based on the theory developed in his earlier works (1896, 1907) relating interest rates to price changes. Thus, he argued (1911c, 1913d, pp. 55ff.) that the initial effect of an increase in the money supply (\( M \)) was to cause banks to increase their loans (\( M' \)), which would put upward pressure on commodity markets and therefore raise prices; however, since rents and wages are set on a contractual basis, profits will increase more than proportionately to prices, and businesses will be stimulated to increase their activities. Most important of all, the real rate of interest

\[
(2.8) \quad r = \frac{1 + i}{1 + P/P} - 1 \approx i - \frac{\dot{P}}{P},
\]

(where \( i \) is the nominal rate of interest, \( P \) is the price level, and \( \dot{P} = dP/dt \) is its rate of change over time) will have fallen, since the money rate of interest is sluggish, too, in adjusting upwards. In this, Fisher was of course influenced by Wickell (1897, 1898). But this type of analysis actually goes as far back as Hume (1752, p. 50):

A nation, whose money decreases, is actually, at that time, much weaker and more miserable, than another nation, who possesses no more money, but is on the increasing hand. This will be easily accounted for, if we consider, that the alterations in the quantity of money, either on the one side or the other, are not immediately attended with proportionable alterations in the prices of commodities. There is always an interval before matters be adjusted to their new situation; and this interval is as pernicious to industry, when gold and silver are diminishing, as it is advantageous, when these metals are encreasing. The workman has not the same employment from the manufacturer and merchant; tho’ he pays the same price for everything in the market. The farmer cannot dispose of his corn and cattle; tho’ he must pay the same rent to his landlord.

The Hume–Wickell–Fisher process just described is highly complex, since one has to account for the short-term changes in commodity prices resulting from the initial change in the money supply, as well as the gradual adjustment of factor prices to commodity prices.
The Distributed Lag of Real Output, Employment, and Interest Rates behind the Inflation Rate

Let $R$ be any response variable such as $T$ (volume of trade) or $E$ (employment). If it depends inversely on (2.8), then it depends inversely on the nominal interest rate and directly on the rate of inflation, $P/P$. Likewise, from (2.8) the nominal interest rate, $i$, will vary directly with the rate of inflation. Fisher in his series of fourteen studies (1923b, 1924, 1925, 1925a, 1926, 1927a, 1930b, 1931, 1932, 1933a, 1933b, 1935b, 1936a, 1937) investigated the relationship of the above three variables to the rate of inflation; he mentioned (1931, p. 154) that a similar study of the influence of the nominal interest rate would be “well worth doing,” but this was never accomplished.

Fisher made the interesting comment (1925, p. 181n) that “the idea of [using] the rate of change of the price level, or of its reciprocal, the purchasing power of the dollar, was emphasized in my Appreciation and Interest (1896) . . . . It was used by Prof. J. M. Clark (1917) . . . .” I do not think he was claiming credit for Clark’s “acceleration principle”; Clark did not refer to Fisher (1896), nor did he discuss prices. I think that Fisher was simply struck by the analogy between Clark’s model in which the rate of net investment varied as the rate of change of consumption, and his own in which the levels of “trade” and employment varied as the rate of change of prices. If one of them could provide an exploration for turning-points in the business cycle (e.g., a slackening of the rate of growth of consumption leading to an actual decline in net investment, according to Clark), so of course could the other. In fact, the Fisher relationship is not subject to the criticism of Clark (1917) made by Frisch (1931) that the acceleration principle does not hold for gross investment, so it is rather curious that Fisher never specifically pointed out that his law could explain turning-points in the business cycle on the basis of slackening in the rate of change in the price level.\footnote{An effect analogous to those of Clark and Fisher is that cited by Cards (1903) who observed, following Marshall (1891, p. 637), that a small percentage rise in a firm’s revenues could lead to a large percentage rise in its profits; hence, capitalizing the expected stream of profits at the going discount rate, a proportionate rise in the price of the firm’s product would lead to a magnified rise in the value of the firm. This influenced Aftalion (1906, p. 249) in his early formulation of Clark’s subsequent acceleration principle.}

The first of Fisher’s series of studies affirmed (1923b, p. 1024) that “the principal force affecting the cycle is the real rate of interest, the sum of the money rate of interest and the rate of appreciation (positive or negative) of the purchasing power of the dollar.” He then presented “some preliminary findings as to one only of the two components of the real rate of interest, namely, . . . the rapidity of fall (or rise) of the price level.” He exhibited a
\[ p'_t = \frac{p_{t+1}}{p_t} - 1 = \frac{\Delta p_t + \Delta p_{t-1}}{p_{t-1}} = \frac{\Delta p_t}{p_t} + \frac{\Delta p_{t-1}}{p_{t-1}} \]

where \( \Delta p_t = p_{t+1} - p_t \). This is a rather curious definition, when the definition

\[ p'_t = \frac{p_{t+1}}{p_t} - 1 = \frac{p_t + \Delta p_t}{p_t} - 1 = \frac{\Delta p_t}{p_t}, \]

would seem so much more natural as an approximation to the instantaneous percentage rate of change, or logarithmic derivative, \( \frac{d}{dt} \log p_t \).

Fisher took as dependent variable a series for the volume of trade \( T_t \) in month \( t \), namely the business barometer of the American Telephone and Telegraph Company; the great innovation introduced in this paper was the formula

\[ p'_t = \sum_{i=0}^{n-1} w_i p'_{t-i} \quad \text{where} \quad w_i = \frac{n-i}{(n+1)n/2} \]

of a distributed lag of past rates of change of the price level, the coefficients descending linearly by month (he took \( n = 8 \)). In his picturesque language, Fisher described \( p'_t \) as the “dance of the dollar,” and the dependent variable \( T_t \) as the “dance of business.” He found a 70% correlation between \( T_t \) and \( p'_t \) over the period 1914–1922, using monthly observations. A graph in which the ordinates of \( p'_t \) were plotted four months ahead of those for \( T_t \) showed close agreement except for the months following the outbreak of the First World War in 1914 and the months preceding the entrance of the United States into the War in 1917, as well as the period from June 1921 to December 1922 which he attributed to the overly simple weighting scheme in the distributed lag, which he promised to modify “according to probability principles” in future work.

In his second contribution (1924) Fisher replaced the linear weighting scheme of (3.3) by a 14-month scheme that was unimodal and very slightly skewed to the right, defined by the sequence of lag coefficients

\[ (0, 2, 6, 11, 18, 23, 25, 28, 18, 12, 7, 4, 2, 1) \]

divided by their sum, 152, with mode 7 and mean 7.24, and replaced the AT&T series by the volume series that had been recently issued by the Harvard Committee on Economic

\[ \text{Fisher's exposition was ambiguous; } p'_t \text{ was described merely as "a weighted average, the weights being 1, 2, 3, 4, 5, 6, 7, 8, for each eight consecutive ordinates of the 'derivative' } p' \text{" (1923b, p. 1027), but his later exposition (1924, p. 50) confirmed that the weights were in descending order. Since } \sum_{i=0}^{n-1} i = (n+1)n/2, \text{ the coefficients } w_i \text{ of the } p'_{t-i} \text{ sum to 1.} \]
time introduced (p. 180), and the log-normal theory, which, among the many others, best fits the facts" (p. 184), was the lognormal, yielding

\[ P_t' = \int_0^\infty l(w, \mu, \sigma^2) P_t' dw, \]

where

\[ l(w, \mu, \sigma^2) = \frac{1}{w\sigma \sqrt{2\pi}} e^{-\frac{1}{2}(\log w - \mu)^2 / \sigma^2} \]

is the density function of the (two-parameter) lognormal distribution of the random variable \( W, \mu \) and \( \sigma^2 \) being the mean and variance of \( X = \log W \) which has the corresponding normal density

\[ n(\omega, \mu, \sigma^2) = \frac{1}{\sigma \sqrt{2\pi}} e^{-\frac{1}{2}(\omega - \mu)^2 / \sigma^2} \]

(see for instance Aitchison and Brown, 1957, or Johnson and Kotz, 1970, Ch. 14). Fisher transformed the parameters \( \mu \) and \( \sigma^2 \) to two new parameters \( T \) ("target distance"—not to be confused with the "trade" variable)—defined by \( T = e^\mu \), and \( A \) ("accuracy ratio")—defined by \( A = e^{-0.67445\sigma} \). He estimated the "target distance" to be 9.5 months and the "accuracy ratio" to be 0.58. The latter (according to his discussion on p. 189) derives from the first quartile of the normal distribution, \( \nu = \mu - 0.67445\sigma \) (the factor 0.67445 was known at that time as the "probable error"—cf. R. A. Fisher, 1928, p. 45), and \( e^\nu \) was estimated to be 5 months; consequently, the accuracy ratio is \( A = e^\nu / e^\mu = e^{\nu - \mu} = e^{-0.67445\sigma} = 5/9.5 = 0.5258 \). Fisher remarked further that (p. 188):

It is therefore quite possible that the type finally chosen is not absolutely the best. But we may be sure that it is not far from it, if for no other reason than simply because there remains so little room for improvement, between the extraordinary high correlation which this method yields and 100 per cent.

It is interesting to observe that this is precisely the point of view of contemporary optimization heuristics in cases where computation of a precise optimum is known to be impossible in finite time.

Fisher slightly changed his definition of the approximation to \( P_t' \) to the following (p. 182, note 3): "The slope for any given month is measured by subtracting the index number for the preceding month from that for the succeeding month and reducing the result to a

\[ 16 \]Fisher's footnote on pp. 186-7 states that \( \log A = 6745\sigma \), which is clearly a misprint for \(-6745\sigma\); there is another obvious misprint in the typesetting of the formula for the lognormal density. Fisher omitted the factors \( 1/(\sigma \sqrt{2\pi}) \) needed for (3.5) to be a true density function.
but of two-month moving averages of past inflation rates. The monthly index of trade that he used, from Persons (1926), and which he smoothed by a three-month moving average (with weights $\frac{1}{4}, \frac{1}{2}, \frac{1}{4}$), covered the period August 1915 to March 1923, and the monthly price data went back to January 1908; the correlation was 0.941. The same numerical lag distribution was applied to different periods, going back as far as 1877, with high correlations as well. Fisher stated (p. 198) that he had thought of using the real rate of interest (formula (2.8) above), following Fisher (1906, 1909), but had decided against it because “the rate of interest (in money) is always perceived while the rate of appreciation of money is not,” hence they were best studied separately (whether this meant jointly as distinct independent variables or separately after correction for the other influence was not made clear); however, this was never done.14 He noted (p. 199) that Alvin Hansen and Holbrook Working had “pointed out that, conformably to the quantity theory, the price level follows bank deposits with a few months’ lag,” but no such published work was cited;15 however, the discussion made clear that he regarded the general question of studying the dynamics of the monetary system as one of highest importance and one that had just begun. He expressed his strong opinion that such a complete system, when subject to random outside forces, would still not exhibit regular cycles, but would be much more akin to a theory of “the swaying of the trees.”

In Fisher’s next two studies (1926a, 1926b) the dependent variable was employment. Both papers reported on the same research. Because of the computational labor,16 Fisher reverted to the “short-cut” method of approximating the lognormal lag distribution by the above linear one (3.9); he nevertheless found a correlation of 90% in the relationship. The same numerical lag distribution was applied to different periods with equally satisfactory results. This led him to reiterate his belief that since such a large proportion of business fluctuations could be explained by price changes, the prevailing emphasis in business-cycle theory were misplaced. A sixth paper (1927a, pp. 00-00) applied his methodology to bankruptcies, showing a very good fit between bankruptcies and a distributed lag of rates

14It seems that the idea of multiple correlation (or multiple regression) was not well known among economists at the time; not until the publication of Ekelund’s text (1930), and the studies of Frisch (1934), Koopmans (1937), and Tinbergen (1939), did the idea become part of the stock-in-trade of the profession.

15The reference is evidently to Working (1925), who concluded that there was approximately an eight-month lag between circulating medium (M + M') and the price level (I am indebted to Thomas Humphrey for this reference). Hansen’s (1921) contribution was the establishment of the close relation between cash reserves and bank deposits.

16Fisher remarked (1926a, p. 786): “During the last three years ... I have had at least one computer in my office almost constantly at work on this problem ...” in those days the word “computer” referred to a live human being working with pencil and paper and, no doubt, a slide rule. It seems that the “computer” referred to was Max Swolly. The phrasing in Fisher (1926a, p. 25) was: “During the last two years I have had someone at work on this problem, doing statistical computations quite constantly.”
If the price level falls in such a way that merchants may expect for themselves a shrinking margin of profit, they will be cautious about borrowing unless interest falls, and this very unwillingness to borrow, lessening the demand in the money market, will tend to bring interest down. On the other hand, if inflation is going on, they will scent rising prices ahead and so rising money profits, and will be stimulated to borrow unless the rate of interest rises enough to discourage them, and their willingness to borrow will itself tend to raise interest.

He proceeded to cite historical events supporting this theory, but was especially interested in applying his distributed-lag analysis to obtain definite quantitative results.

For Great Britain, using bond yields from Gibson (1923), he obtained a correlation of 0.87 in a distributed lag of interest behind price changes “when effects of price changes are assumed to be spread over 28 years or for a weighted average of 9.3 years” (1930b, p. 428), while for the United States, using bond yields taken from the Statistical Bulletin, 1929-1930, of the Standard Statistics Company, the highest correlation obtained (0.867) was “for a distribution of the influence due to price changes over 20 years or a weighted average of 6.7 years.” The British data were yearly, and the American data quarterly. He concluded (p. 428):

...the results and other evidence indicate that, over long periods at least, interest rates follow price movements. The reverse, which some writers have asserted, seems to find little support. Experimental, made with United States short term interest rates, to test the alternative hypothesis of distributed influence of interest rate changes instead of price changes, gave results of negligible significance. Our investigations thus corroborate convincingly the theory that a direct relation exists between $P^t$ and $i$, the price changes usually preceding and determining like changes in interest rates.

These findings, which were either ignored or forgotten for forty years, had to be rediscovered by macroeconomist in the 1970s. Fisher provided the following explanation for the long lag length (p. 429): “A further probable explanation of the surprising length of time by which the rate of interest lags behind price change is that between price changes and interest rates a third factor intervenes. This is business, as exemplified or measured by the volume of trade. It is influenced by price change and influences in turn the rate of interest.”

An eighth, largely retrospective, study (1931) noted that the correlations of trade, employment, interest, and bankruptcies with $P^t$ had become much lower following 1928. In particular, using the Federal Reserve Board index of manufactures and mining as the variable for the volume of trade, corrected for secular trend, he noted that for the period January
However, the crash of 1920–21 was one that was perfectly explained by his 1925 analysis. Panics and distress selling might have been partly the cause of the large price decline of that period, but the explanation of the volume of trade by past rates of inflation, based on a distributed lag over 35 months with a weighted average of 9½ months, held up perfectly in the 1925 analysis. The only question is whether he needed a better explanation of price changes than the purely monetary one.

In the tenth of the series of studies (1933a), Fisher found that the relation between $P'$ and employment persisted up to 1932. He quoted a correlation coefficient of 0.84 (p. 156), though his exposition did not make clear whether this referred to the relation over the entire period 1906–1932, or just to the period 1919–1932. While he referred to his Booms and Depressions (1932), the rationale that he presented for the relationship between employment and $P'$ did not differ from that presented in his 1925 paper. This presents an interesting paradox. His 1925 analysis—in which he had concluded that the previous empirical relationships he had found between trade and employment and the distributed lag of previous inflation rates no longer held up after 1923—at least provided him with a good excuse for his failure to predict the 1929 depression. Now, however, it turned out that the relationship between employment and $P'$ stood up extremely well at least up to the middle of 1932 (1933a, p. 155, Chart III). This suggests that if he had had by the fall of 1929 the data on inflation rates up to that time that he employed in Fisher (1933a), he should have been able to predict the Great Depression without having to resort to the debt-deflation hypothesis; and by the end of 1930 he should at least have been able to explain it. This 1933 study employed data up to close to the end of 1932, so the explanation cannot be that there was a substantial delay in the availability of data. It is truly a puzzle that he felt the need to resort to new hypotheses to explain the Great Depression when his 1925 "law" (as he had called it) already explained it so well.

Another puzzle is that if his distributed-lag explanations of trade and employment are to be reconciled with his equation of exchange, it must be assumed that rises in trade and employment induced by inflation must be financed by either an increase in the money supply and deposits or by an increase in velocity, and declines in trade and employment must be accompanied by either a fall in the money supply and deposits or a decline in velocity. Unless the entire accommodation is to be provided by changes in velocity, currency and deposits must be considered to some extent as endogenous variables during the transition process. To be sure, the price changes that lead to the changes in trade and employment are precipitated by changes in the money supply, but since the changes in trade and employment occur with a lag, and the equation of exchange must hold identically at all points of time, some change in $MV + M'V'$ must take place following the initial change in $M$ or $M'$ that
includes, as a special case, the case of countries on the same monetary standard, where the divergence is practically zero. In short, price levels among nations behave, in general, according to their monetary standards.

Fisher also took the opportunity to establish his priority over Cassel (pp. 7–8):

We conclude that, with a few exceptions ..., a rise or fall in the price of gold is accompanied immediately by a rise or fall in the level of commodity prices. This principle was stated and verified statistically in *Stabilising the Dollar* [Fisher (1920)] (see, especially, pp. 26–28). It was discussed and further verified by Professor Gustav Cassel in *Money and Foreign Exchange After 1914* [Cassel (1922)], published in 1922—he called it the principle of purchasing power parity.

Fisher took the opportunity in this 1925 article to return to his distributed-lag analysis for the United States (pp. 8–10), which he brought down to the end of 1930 (starting at the beginning of 1908), for both employment and trade. In the case of employment, in which he had obtain a correlation of 0.9 in the first study (1926a, 1925b) and 0.64 in the second (1925a), he stated:

This makes the third time that the original study of 1922 [sic] as to employment ($E$) has been extended; and each time the correlation between the two curves continues to be striking; despite the fact that in no case was any attempt made to change, or improve upon, the original method of distributing the lag.

No figure for this “striking” correlation coefficient was quoted, however. In the case of the volume of trade, in which he reverted to Personal’s index, he stated that “this relationship has, for the first time, been calculated beyond 1922 and brought down to date. For the whole period the same striking correspondence is found, again sustaining the conclusion that deflation depresses trade and reflation revives it,” although again no correlation coefficient was quoted. The upbeat spirit is in stark contrast to his pessimistic assessment of 1931. The charts (pp. 24–5) indicated that the predictive power of $P'$ was still strong for trade and employment (the weighted average covering the preceding 25 months), except for the period mid-1931 to mid-1932. For this period he concluded that “some non-monetary factor dominates and was present almost all over the world” (p. 12).

His suggested explanation, offered with great diffidence, was that, on the basis of a comparison of time series on “business conditions” with time series on $P'$ for sixteen countries (pp. 26–7), the curve of business conditions “was put out of line by new tariffs and other trade impediments following the growth of ‘nationalism’, and also by the sudden and unforeseen developments in the international debt situation.” As far as I know, this was
The discussion of the source of prices (1935c) retained to the data-based lag analysis, brought up to the end of 1935. Only charts for employment were shown, and no correlation coefficients were reported. But some light on the puzzles alluded to above is thrown by Fisher’s observation that (p. 498):

... the correspondence between the actual and the computed fluctuation in employment is naturally far from exact, since many other causes operate concurrently.

Among other influences is the direct influence of money-shortage on T irrespective of the intermediation of \( P \) ....

Some quite convincing specific reasons were advanced as to why his 1925 relationship did not hold up during certain periods (pp. 499–500): (1) the failure of factory employment to rise with \( P' \) during 1917–18 was attributed to the military draft; (2) the falling-off of employment in 1931–32 while \( P' \) was rising was explained as previously (1935a); (3) the apparent lack of any relationship between \( E \) and \( P' \) from August 1933 to November 1935 was attributed to the quantitative restrictions on production and trade introduced by the federal government (reducing hours but not pay in the N.R.A., the dole and relief work, and governmental restrictions on production), counterbalancing the favorable effects of price increases. It is interesting that the amount of debt was invoked only in the second of these three cases (in Fisher, 1935a). His chart (p. 499) showed a turn-down of \( P' \) in 1935 which might have foreshadowed the depression of 1937.

One of the contributions of this 1935 article of Fisher’s is his reiteration (also made in Fisher, 1935a, p. 9) of an important methodological point from his 1925 article (1935a, p. 498):

Some critics, unfamiliar with mathematical statistics, have argued that to adjust the parameter or parameters, so as to get the best fit, is making the fit to suit. This is answered in the 1925 article referred to. Suffice it here to say that, for such critics, a convincing answer is that, after finding the best fit for one period, if the very same formula, thus found best for that period, be then applied to any other period, it is found to give almost as good a result as if it had been based on the statistical data for this second period.

This article was criticized by Copeland (1936) who among other points raised the following (p. 504):

Thus, \( E \) is shown to lag behind the curve of monthly increments in the price index ... It may conceivably be true at the same time that \( P \) lags analogously behind the curve of monthly increments in factory employment. When two time
Fisher responded (1936b, p. 508) that Copeland had not shown that the two series were cyclical, but agreed that it would be worth while to examine the reverse relationship (thus anticipating the type of causality analysis later employed by Sims, 1972).

Fisher's fourteenth and final contribution to his distributed-lag methodology was his article (1937) explaining his short-cut method for computing distributed lags of the form (3.8). This paper needs to be read in conjunction with the work of Fisher's long-time collaborator, Saszuly (1934). Fisher made clear that the method consisted in regressing the dependent variable on (3.8) for successive values of \( \alpha = 1, 2, \ldots \) until a (local) maximum of the correlation coefficient was obtained. For large \( \alpha \) the computational burden increased, and for this calculation Saszuly's formula:

\[
\sum_{t=1}^{n} \alpha_{t} = (n+1)S_{n}^0 - S_{n}^1 \quad \text{where} \quad S_{n}^0 = \sum_{t=1}^{n} \alpha_{t} \quad \text{and} \quad S_{n}^1 = \sum_{t=1}^{n} S_{t}^0 = \sum_{t=1}^{n} \sum_{\tau=1}^{t} \alpha_{\tau} \]

proved very useful (Fisher, 1937, p. 329; Saszuly, 1934, pp. 135, 147). With today's high-speed computers such formulae are hardly needed, but in Fisher's time they were clearly highly advantageous. Fisher provided few clues as to how he proceeded in the case of the lognormal lag other than to refer the reader to Chapter X of Saszuly (1934). From the discussion there (especially pp. 201–2) one may conjecture that a piecewise linear approximation to the lognormal density may have been used.

As Fisher noted (1937, p. 327), the distributed lag was used by Roos (1934) in the analysis of building construction. Fisher's work stimulated Alt's (1942) article on distributed lags, which was followed by Tinbergen's (1949) study in which Fisher's idea was applied to the estimation of long-term foreign-trade elasticities; instead of Fisher's method, however, Tinbergen estimated the lag coefficients essentially freely without constraining them by any form of lag distribution. Koyck (1953) criticized Tinbergen's procedure and suggested instead (without knowledge of Fisher's work) a lag distribution in which the lag coefficients decline proportionately instead of linearly. This was soon followed by Berger's paper (1953) which compared Koyck's method with Fisher's, and found that they led to very similar estimates, but that Koyck's method appeared more natural and was computationally simpler. Koyck's monograph (1953) soon followed, and included a discussion of Berger's comparisons (pp. 30–32) between Fisher's approach and his own. With Koyck's monograph, the topic of distributed lags became a standard tool of econometrics, and there soon followed important contributions by Klein (1959), Solow (1960), Almon (1965), Jorgenson (1966), Learner (1972), Shiller (1973) and many others. It is clear that Fisher had introduced an important and influential econometric technique.

Discussion of Fisher's innovative distributed-lag studies would not be complete without some comment on his so-called anticipation of the Phillips curve. Donner and McCollum
The rationale for Phillips's relationship was clearly expressed in his opening sentence (1958, p. 233):

> When the demand for a commodity or service is high relative to the supply of it we expect the price to rise, the rate of rise being greater the greater the excess demand. Conversely when the demand is low relatively to the supply we expect the price to fall, the rate of fall being greater the greater the deficiency of demand. It seems plausible that this principle should operate as one of the factors determining the rate of money wage rates, which are the prices of labour services.

Thus stated, the relationship in question can be said to go back at least to John Stuart Mill, if not long before. Quantitatively expressed by Samuelson (1947) as a differential equation expressing the rate of change of a price as an increasing function of the excess demand for the corresponding commodity, this has been well known in economic theory for a long time, and was given statistical precision by Phillips. However, Fisher's relationship differs from this one in a number of important respects. In the first place, Fisher dealt with a relation not between money wages and employment, but between money prices and employment; secondly, in Fisher's theory, the rate of inflation of commodity prices was a component of the independent variable, while employment was the dependent variable; thirdly, Fisher stated many times that he had tried to find a significant relationship between employment (as well as the volume of output) and the concurrent (or even past) rate of inflation of commodity prices, but without success, and found a significant relationship only between employment (or "trade") and a distributed lag of past rates of inflation. The rationale behind Fisher's relationship was that when prices rise, wages (and other cost components) lag behind, raising profits and inducing firms to expand output and employment (and conversely when prices fall). If money wages rose in response to a rise in prices, most of the Fisher effect would be removed. Put another way, the Fisher effect depends on the real wage to fall in order to stimulate employment.

There was no distributed lag in Phillips's relationship, although he had employed distributed lags in a previous study (1956). The only thing Fisher's and Phillips's relationships have in common is that employment is one of the variables (dependent and independent respectively), and that a rate of change of a price (commodity prices and money wage rates respectively) is an element of the other variable. It is difficult to understand how anybody could confuse such different relationships. It is akin to confusing a supply relationship with a demand relationship.

I close with an unanswered question. Fisher passionately advanced the cause of price stability, and advocated reflation as a means to restore previous debtor-creditor relationships
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