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Chapter Author: Daniel K. Benjamin, Levis A. Kochin

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# 13 War, Prices, and Interest Rates: A Martial Solution to Gibson's Paradox

Daniel K. Benjamin and Levis A. Kochin

## 13.1 Introduction

The positive correlation between the price level and the interest rate—"Gibson's paradox"—is one of the best known and least understood of all economic regularities. Keynes called it "one of the most completely established empirical facts in the whole field of quantitative economics" (Keynes 1930, 2: p.198). Irving Fisher asserted that "no problem in economics has been more hotly debated" (Fisher 1930, p. 399). Even a casual glance at the literature (see, for example, Shiller and Siegel 1977, and the sources cited therein) suggests that Fisher's assertion is as true today as it was fifty years ago. And Keynes's contention seems amply supported by figure 13.1, which plots the yield on long-term bonds (consols) and the log of the price level in Britain from 1729 to 1931 (the gold standard years for which both series are available).[1] The striking visual impression of a positive relationship between the price level and interest rates is confirmed by the following regression, which estimates the yield on consols ($R$) as a function of the log of the price level (with $t$-statistics in parentheses):

(1)
$$R_t = -5.60 + 2.04 \log P_t.$$
$$(-8.83) \quad (14.5)$$
$$R^2 = .51, \text{ D.W.} = .31 .$$

As beguiling as figure 13.1 and equation (1) are, both suffer from two serious defects. First, it is well known that many economic time series

Daniel K. Benjamin is deputy assistant secretary for policy at the U.S. Department of Labor.

Levis A. Kochin is associate professor of economics at the University of Washington, Seattle, Washington.

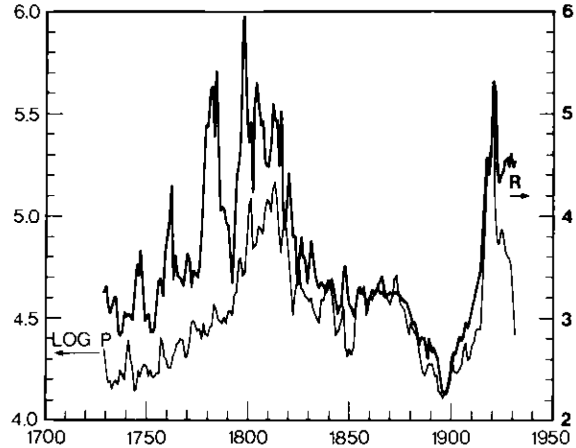


**Fig. 13.1** Yield on consols and the U.K. price level, 1729–1931.

exhibit nonstationarity, i.e., the series fail to revert to their means. As Granger and Newbold (1974) have shown, if one regresses one nonstationary series on another, the chances are high that one will observe a statistically significant relationship, even though both series are randomly generated and in fact unrelated to one another. We show that this problem is indeed present in the data for the price level and interest rates and that once the series are made stationary the apparent relationship largely disappears.

The second defect of figure 13.1 and equation (1) is that prices and interest rates are both endogenous variables. The observed relationship between them might well reflect the effects of some common force that is acting on both. Figure 13.2 suggests what that force might be. Again the log of the price level and the consol rate are plotted for 1729–1931. We have added to the figure a measure of real defense expenditures, labeled WAR. The apparent association between WAR and either prices or interest rates is, to our eyes, at least as impressive as the apparent existence of Gibson's paradox. This apparent association is no accident: large increases in defense expenditures (principally during wars) create incentives to issue fiat money, which in turn tends to generate inflation and high price levels. Wars also create a scarcity of goods *currently* available for nonwar uses relative to the amounts that will be available in the (postwar) future.[2] The ensuing attempts by individuals to smooth their consumption over time create an excess demand for current goods and thus a rise in the interest rate. Once the nonstationarity of prices and interest rates and the effects of wars are accounted for, Gibson's paradox disappears.

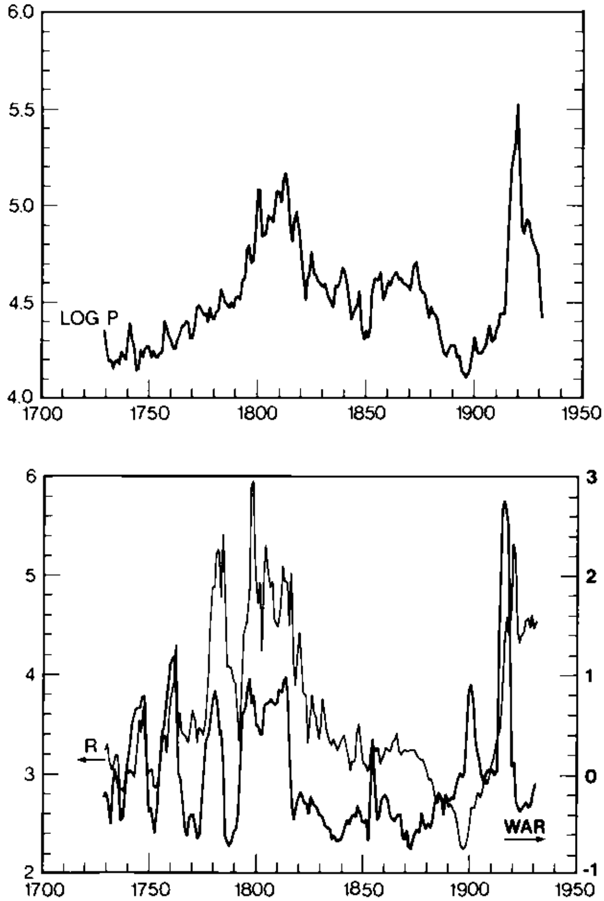


**Fig. 13.2** Yield on consols, the U.K. price level, and real defense expenditures, 1729–1931.

## 13.2 Prices and Interest Rates

Many explanations of Gibson's paradox have been proposed; perhaps the best known is the so-called Fisher effect. As first propounded by Irving Fisher (1930), this theory asserts that borrowers and lenders attempt to forecast the inflation rate that will prevail during the life of their debt contracts. Positive inflation rates imply reductions in the real value of debt obligations; hence lenders will demand and borrowers will accede to high nominal interest rates to offset that result. Negative inflation rates similarly will be associated with low nominal interest rates. The result is a positive association between interest rates and inflation rates. Since inflation rates will be correlated with price levels, the result is an observed correlation between interest rates and the price level.

Fisher (1930, chap. 19) found no evidence of a positive association between current interest rates and current inflation rates, nor between current interest rates and future inflation rates. However, he did find a positive association between current interest rates and *past* inflation. Arguing that past inflation works with a lag on future interest rates via its effects on expectations of future inflation, Fisher concluded that this observed relationship both vindicated his theory and explained Gibson's paradox. As Shiller and Siegel (1977) note, however, a distributed lag on past inflation rates must necessarily be well correlated with the current price level so that Fisher's finding may be nothing more than a restatement of the paradox. Shiller and Siegel go on to present evidence, assuming rational forecasting of inflation, suggesting that the Fisher effect is woefully inadequate in explaining movements in interest rates over the period 1729–1950.

Our purpose in this section is to demonstrate the following: (1) during the gold standard period covered by Fisher, his postulated link between past inflation and current interest rates makes no sense; (2) during the full sample of gold standard years, to explain nominal interest rates or Gibson's paradox as resulting from changes in the expected inflation rate makes no sense; (3) there may, in fact, be no Gibson's paradox to explain.

To address these issues, we begin by inquiring into the time-series properties of the price level. Table 13.1 shows the autocorrelations of the log of the price level in Britain from 1729 to 1931. The high, slowly decaying pattern revealed in table 13.1 (col. 1) is symptomatic of nonstationary series. Further evidence is contained in column (2), which displays the autocorrelations of the inflation rate ($\log P_t - \log P_{t-1}$). Except at one lag (and at lag 21), none of the autocorrelations is as much as two standard errors from zero. The fact that the autocorrelation at one lag is nearly three standard errors from zero suggests that the inflation rate follows a first-order autoregressive process. However, the price-level data that we are compelled to use are annually averaged data—they reflect the average price level during each year. As Working (1960) has shown, time-averaged data on an underlying process that follows a continuous random walk will tend to produce positive serial correlation at one lag. To see the tendency, consider a rise in the price level during year $t$. The observed (time-averaged) data will show a rise in prices from period $t - 1$ to period $t$. The observed data will *also* show a rise in the price level from period $t$ to period $t + 1$, since the observed price level for period $t$ is an averge of the lower price level that prevailed during part of period $t$ and the higher price level that prevailed during the remainder of the period. Working calculated the theoretical value of the autocorrela-

**Table 13.1 Autocorrelations, U.K. Prices, Yields, and Abnormal Defense Expenditures, 1729–1931**

| Order | Log $P_t$ (1) | $\Delta$log $P_t$ (2) | $R_t$ (3) | $\Delta R_t$ (4) | WAR$_t$ (5) | $\Delta$WAR$_t$ (6) | Standard Error of Random Model (7) |
|---|---|---|---|---|---|---|---|
| 1 | 0.953 | 0.206 | 0.936 | 0.007 | 0.894 | 0.301 | 0.070 |
| 2 | 0.886 | −0.091 | 0.870 | −0.140 | 0.724 | 0.029 | 0.069 |
| 3 | 0.826 | −0.099 | 0.823 | 0.083 | 0.548 | 0.026 | 0.069 |
| 4 | 0.773 | −0.087 | 0.764 | −0.009 | 0.365 | −0.134 | 0.069 |
| 5 | 0.728 | 0.037 | 0.707 | −0.135 | 0.213 | −0.240 | 0.069 |
| 6 | 0.679 | −0.060 | 0.666 | 0.009 | 0.111 | −0.127 | 0.069 |
| 7 | 0.635 | 0.013 | 0.625 | −0.036 | 0.035 | −0.121 | 0.069 |
| 8 | 0.591 | 0.083 | 0.589 | −0.095 | −0.015 | −0.177 | 0.068 |
| 9 | 0.538 | 0.050 | 0.563 | 0.076 | −0.028 | −0.104 | 0.068 |
| 10 | 0.482 | −0.063 | 0.522 | −0.029 | −0.019 | −0.137 | 0.068 |
| 11 | 0.429 | −0.040 | 0.484 | −0.085 | 0.021 | −0.105 | 0.068 |
| 12 | 0.379 | −0.028 | 0.461 | −0.123 | 0.085 | −0.050 | 0.068 |
| 13 | 0.335 | 0.028 | 0.455 | 0.021 | 0.165 | 0.075 | 0.068 |
| 14 | 0.289 | −0.081 | 0.445 | 0.011 | 0.229 | 0.121 | 0.067 |
| 15 | 0.251 | −0.100 | 0.435 | 0.093 | 0.268 | 0.161 | 0.067 |
| 16 | 0.221 | −0.026 | 0.418 | 0.125 | 0.274 | 0.078 | 0.067 |
| 17 | 0.193 | 0.127 | 0.388 | 0.101 | 0.261 | 0.112 | 0.067 |
| 18 | 0.153 | 0.072 | 0.346 | −0.063 | 0.221 | 0.059 | 0.067 |
| 19 | 0.108 | −0.001 | 0.313 | 0.154 | 0.169 | 0.007 | 0.066 |
| 20 | 0.063 | −0.006 | 0.263 | −0.009 | 0.115 | 0.012 | 0.066 |
| 21 | 0.019 | −0.155 | 0.214 | −0.219 | 0.057 | −0.002 | 0.066 |
| 22 | −0.010 | −0.065 | 0.192 | 0.119 | −0.002 | 0.004 | 0.066 |
| 23 | −0.033 | −0.063 | 0.156 | 0.065 | −0.061 | −0.026 | 0.066 |
| 24 | −0.052 | −0.043 | 0.112 | −0.139 | −0.116 | −0.086 | 0.066 |

*Sources*: Col. (1), annual averages of wholesale prices from Mitchell and Deane 1962, pp. 468–70, 474–75; col. (3), annual yield on annuities or consols from Homer 1963, tables 13, 19, and 57; col. (5), reported nominal defense expenditures from Mitchell and Deane 1962, pp. 389–91, 396–99.

*Notes*: Col. (5) adjusted as described in note 6, deflated by prices. The residuals from a log linear trend over the period 1729–1931 were autocorrelated.

tion coefficient to be 0.25 at one lag and zero for lags longer than one period. The autocorrelations revealed in table 13.1 (col. 2) are remarkably close to the pattern predicted by Working. The autocorrelation coefficient at lag one is 0.21, well within one standard error of Working's theoretical value, and only one (at lag 21) of the remaining autocorrelation coefficients is as much as two standard errors from zero. We cannot reject the hypothesis that the price level followed a Martingale process in Britain during the gold standard years. If correct, the Martingale hypothesis implies that there is no evidence of persistent inflation or deflation in Britain during the gold standard years. The secular "trends" in the price level that most investigators have found are the sorts of tricks of the eye that arise as a result of the human tendency to perceive order—even when none is present.[3]

The implications of this finding are striking. First, it implies that there should be no Fisher effect present during the years of the gold standard. If the price level follows a Martingale, then the best estimate of the price level in period $t + 4$ is the price level in period $t$, given available past information on the inflation rate; equivalently, the best estimate of the inflation rate in period $t + i$ for all $i$ is zero. Thus the "expected inflation rate" that will be rationally incorporated into nominal interest rates is also zero, given the past information on the inflation rate. Second, Fisher's finding that a distributed lag on past inflation rates is positively correlated with current interest rates is utterly unrelated to the Fisher effect. Given the time-series properties of the inflation rate, a distributed lag on past inflation rates is not the rational way to forecast future inflation rates. The distributed-lag method of forecasting will be rational only if inflation rates are positively serially correlated. Since they are not, no rational borrower or lender would use Fisher's proposed method of forecasting.

The final issue involves the time-series properties of the long-term interest rate. Table 13.1 (col. 3) displays the autocorrelations of the levels of the consol rate. Again, the high, slowly decaying pattern of autocorrelation coefficients is suggestive of a nonstationary series. Table 13.1 (col. 4), which displays the autocorrelation coefficients of the first difference of the consol rate, reinforces this impression. Except at lag 21, none of the coefficients are as much as two standard errors from zero. Again, however, since these are time-averaged data, we should observe some positive autocorrelation at one lag. The fact that we do not suggests that the "true" underlying data probably reflect some negative autocorrelation—which would be expected if there were episodic "crises" in which interest rates rose and then fell sharply. The important point, however, is that the interest-rate series, like the price series, is very close to being a nonstationary series.

The simplest way of dealing with two nonstationary series is to difference them to produce stationary series. We have done so for both the log of the price level and the consol rate, and then estimated the relationship between the two series. The results are as follows for 1729–1931 (with $t$-statistics in parentheses):

$$\text{(2)} \qquad \Delta R_t = .006 + .365\ \Delta\log P_t. \\ \qquad (.33) \quad (1.62) \\ R^2 = .01, \ \text{D.W.} = 2.00.$$

At either the 5 percent level for a two-tailed test or the 10 percent level for a one-tailed test, there is no significant relationship between the two series. The only paradox here is whether there is anything left to explain.

### 13.3 War and Interest Rates

General William Tecumseh Sherman informed us that war is hell. We have no fault with this description of the military attributes of war. But if one is interested in the economic attributes of war, it would be more appropriate to say that war is purgatory. Hell is permanent; war is temporary, and its salient economic effects arise from the temporary increase in government expenditures that occurs during war.

The temporary rise in government expenditures during war is itself the product of the change in intertemporal demands that occurs at the outbreak of war. If a nation succeeds at war, the permanent incomes of its citizens will be higher than if it fails. Success demands that resources be available for use during the war: a rifle not produced until 1919 was of no use in fighting the battles of World War I. Thus during a war the demand for currently available goods rises relative to the demand for goods available in the (postwar) future. The change in intertemporal demands is converted into an intertemporal reallocation of resources via a rise in the real rate of interest.

To see how temporary wartime expenditures produce a rise in the interest rate, we shall employ a simple two-period model of a closed economy in which the current period is war and the future period is postwar peace. Strictly for expositional convenience, we assume that all individuals are identical so that we can employ the device of a representative individual. The model is depicted in figure 13.3 where current goods are measured along the horizontal axis and future goods along the vertical axis. The economy's initial endowment is shown by the point $(E_0, E_1)$ through which runs its productive transformation locus, $LL$. Absent the war and the representative individual will choose a productive (and consumptive) optimum at $A$ where the indifference curve $U_0$ is tangent to $LL$. The common values of the marginal rates of substitution and transformation equal $-(1 + r)$, where $r$ is the real interest rate. Current and future consumption are shown by $(Y_0, Y_1)$ and investment (which equals savings) is shown by $E_0 - Y_0$.

Consider now the onset of war, and assume that the war is financed wholly via current taxes. If these taxes equal $E_0 - E_0'$ then the endowment available for nonwar activities becomes $(E_0', E_1)$ and the new transformation locus for nonwar activities becomes $LL'$, which is everywhere to the left of $LL$ by the amount $E_0 - E_0'$, i.e., by the amount of the current resources being used to fight the war. How will society respond to the lack of goods currently available for nonwar activities? Consider first point $C$, which involves holding current consumption unchanged and

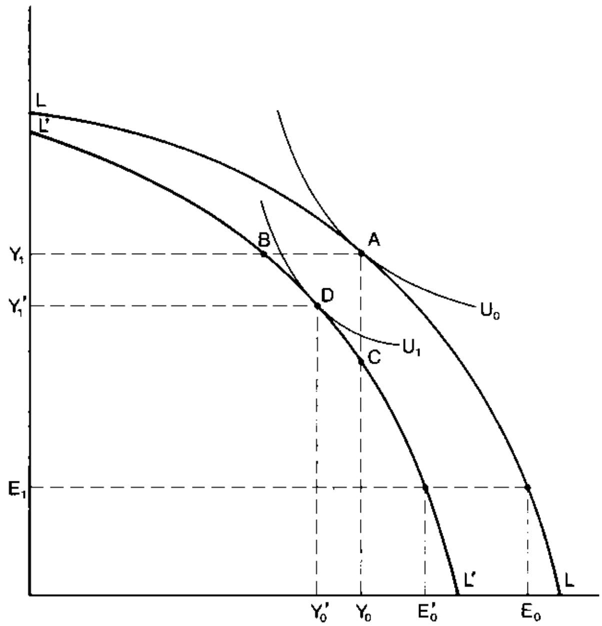


**Fig. 13.3** Current and future consumption in war and peace.

reducing investment to such a level that the burden of the war is borne solely out of future consumption. Since $LL'$ is a horizontal transformation of $LL$, the slope of $LL'$ at $C$ must be steeper than the slope of $LL$ at $A$. However if both current and future consumption are superior goods, the indifference curve passing through $C$ must be flatter than the indifference curve passing through $A$, implying an excess demand for future goods and an excess supply of current goods. Hence the wartime optimum must be above $C$ on $LL'$. Consider now point $B$, which would involve maintaining prewar levels of investment and future consumption, and thus absorbing the entirety of the rise in taxes out of an equal reduction in current consumption. Since $LL'$ is simply a horizontal transformation of $LL$, the slope of $LL'$ at $B$ equals the slope of $LL$ at $A$. However, given superiority of both current and future consumption, the slope of the indifference curve passing through $B$ must be steeper than the slope of the indifference curve passing through $A$. Hence at $B$ there would be an excess demand for current goods and an excess supply of future goods, implying a new optimum, such as at $D$, between points $B$ and $C$. The new optimum thus involves reductions in both current consumption and investment. Since the optimum must lie below $B$, the marginal rate of transformation (and substitution) must be greater than before—the interest rate must rise.

Our assumption that current taxes are used to finance the war bears little relationship to the observed behavior of governments, which in fact finance wars largely through borrowing. However, the analysis developed for taxation applies *exactly* if domestic borrowing is the means of finance. It is ultimately *neither* taxation nor borrowing that "finances" the war: it is current goods that otherwise would be consumed or invested that finance the war. In the case of taxation the confiscation of current goods for war use produces an excess demand for current goods; as individuals attempt to borrow to finance current consumption, the interest rate rises until the excess demand is eliminated. In the case of deficit finance the government signals the excess demand by bidding up the interest rate via its borrowing activities. In either event, if $E_0 - E_0'$ units of current goods are to be extracted from private use, their price must be bid up—the interest rate must rise.

Several facets of the preceding analysis are worth emphasizing. First, it is the *temporary* nature of the war and the prospect of future peace that is central to the analysis. In the event of a permanent rise in "wasteful" government expenditures (such as defense expenditures), involving an equal reduction in both current and future goods available for private uses, a rise in the interest rate would be no more likely than a decline.[4] Wars fundamentally involve a paucity of goods currently available for consumption and investment purposes relative to the amount of those goods that will be available in the future. The attempts by individuals to smooth their consumption over time creates an excess demand for current goods and the ensuing rise in the interest rate.

Second, the relevant real-world interest rate is the one that applies between war and (postwar) peace. There is no reason for intrawar (e.g., very short-term) interest rates to rise, unless the probability is positive that the war will end before the short-term obligation comes due. To see this point, consider a simple world in which there are only two types of debt obligations: short-term and long-term. The short-term obligations are of annual duration and are issued (and repaid) in June of each year. The long-term obligations are consols, i.e., perpetuities with fixed coupon payments. Suppose that, absent war, both short- and long-term interest rates would be 5 percent per annum. Assume now that in June 1913 it becomes known with certainty that a war will begin in August 1914 and continue with uniform intensity until November 1918. Since there still will be peace in June 1914, the relative amounts of income available for nonwar uses in June of 1913 and 1914 are unchanged and the corresponding short rate of interest will be unchanged. In June 1914, short-term rates will be substantially below 5 percent (and perhaps even negative), reflecting the abundance of goods available then relative to (wartime) June 1915. Short-term obligations issued in June 1915 (and 1916 and 1917) will again bear 5 percent interest rates since the (depleted) amounts of resources available for nonwar uses will be identical through June 1918. In the latter month, newly issued short-term obligations will bear sharply higher interest rates, reflecting the paucity of goods available then relative to (postwar) June 1919.

Long-term rates will follow a slightly more complex pattern. The "announcement" in June 1913 of the future war will be accompanied by a slight *decline* in long-term rates since the depressed 1914–15 short rate implicit in the long rate is closer in time than is the elevated 1918–19 short rate. The long rate will continue to decline slowly until the onset of the war, at which point it will rise sharply (since it now only incorporates the elevated 1918–19 short rate). The long rate will rise slowly as the war progresses, reflecting the increasing importance of the advancing 1918–19 short rate.[5] The long rate will peak just before the end of the war, droppping thereafter to the peacetime level of 5 percent.

The third point to note is that opening the economy to allow for borrowing from abroad does not change the qualitative effects of our analysis unless the supply of funds from other nations is perfectly elastic. As long as the supply of foreign funds is positively sloped, the attempt by belligerent nations to borrow will generate an inflow of foreign capital (from neutrals) that will dampen but not eliminate the rise in the real interest rate. The dampening effect of borrowing from neutrals arises because such borrowing enables a belligerent nation to convert a purely temporary war into a partially permanent war.

The final point worth emphasizing is that the wartime rise in interest rates is *not* a consequence of "Keynesian myopia" (the sufferers of which fail to perceive the future tax liabilities implied by current deficit finance). In fact, the Fisherian paradigm we have used to generate our implications corresponds in spirit most closely with "Ricardian omniscience," since our representative consumer clearly perceives the current and future implications of all actions. If the world is characterized by Keynesian myopia, the effects of war on the interest rate will be enhanced, since consumers, thinking their future incomes to be higher than they will in fact be, will attempt even more strenuously to maintain current consumption, thus producing a larger increase in the interest rate. One important implication is that attempts to distinguish between Keynesian and Ricardian views of the world must control for the effects of wars on interest rates.

To test our theory, we have constructed a measure of "abnormal" defense expenditures; we refer to this series as WAR, subscripted to indicate whether it is contemporaneous ($t$) or precedes the current period by $i$ periods ($t - i$). We began by summing together for each year British expenditures on army, navy, and ordnance and then deflating the sum by our measure of the price level.[6] We refer to this deflated sum as real defense expenditures ($D$). We then estimated a log linear trend ($\log D =$

$\alpha + \beta t + \epsilon$) over the sample period 1729–1931. The residuals from this regression (the deviations of real defense expenditures from their trend value) are the series we call WAR; its values are shown graphically in figure 13.2 above and its autocorrelations in table 13.1 (col. 5).[7]

Our rationale for this measure is two-fold. First, if defense expenditures are really defensive, their long-run absolute level should be related to that which is being defended—the wealth of the country (see Thompson 1974). Using measured, current income as a proxy for wealth, the ratio of defense expenditures to income should thus tend to be a stationary (mean-reverting) series. We lack an annual time series on British national income prior to 1855, but know from other work (Benjamin and Kochin 1979, 1982) that the log of real income in Britain can be simply, albeit crudely, represented as a time trend (but see Nelson and Kang 1981, for precautions about placing too literal an interpretation on this representation). Hence our supposition that "normal" defense expenditures follow a log linear trend.

Our second reason for using this measure is based on our notion that there are identifiable periods of abnormal and normal defense expenditures. When real expenditures are extraordinarily high or low, people expect them to return (eventually, if not immediately) to historically typical levels; it is this expectation that generates the forces on the interest rate that we have discussed. The use of deviations from the log linear trend of real defense expenditures captures this notion because the trend representation implicitly forces the deviations from trend to be mean reverting.[8]

Given our measure of abnormal defense expenditures, there remains the question of the correct method of estimating its effects on the interest rate. Our theory implies that the level of defense expenditures relative to their normal level should be positively related to the level of the interest rate. If the interest rate were a stationary variable, we could simply estimate $R_t = a + b\,\text{WAR}_t + \epsilon_t$. Given the nonstationarity of the interest rate, that course is inappropriate; statistical considerations dictate that we estimate the effect of war on changes in the interest rate. Fortunately, our theory implies that changes in the level of abnormal defense expenditures should be positively related to changes in the interest rate. Estimating the relationship over the period 1729–1931 yields the following (with $t$-statistics in parentheses):

$$\text{(3)} \qquad \underset{}{\Delta R_t = \underset{(.35)}{.006} + \underset{(3.51)}{0.24}\ \Delta \text{WAR}_t.}$$
$$R^2 = .06,\ \text{D.W.} = 2.11.$$

Two points are clear from this regression. First, our theory is consistent with the evidence. At a high level of confidence we can reject the hypothesis that interest rates are independent of abnormal defense expenditures. Second, changes in the consol rate are largely determined by factors other than defense expenditures, at least as judged by the low $R^2$ of this regression. In part the low "explanatory power" results from our use of the consol rate as a measure of "the" interest rate. Although the consol rate has the advantage of incorporating the implicit short rate between *any* future period of war and peace, it has the disadvantage of incorporating all other future short rates as well. In part our choice of the empirical counterpart to "the" interest rate suffers from the defect inherent in *any* interest rate measure: it is impossible ex post to measure the interest rate relevant between war and peace ex ante.

Although equation (3) is a relatively poor predictor on average for the full sample period as a whole, it does surprisingly well when it matters most: periods of war. For example, from 1913 until the World War I peak in 1917, the consol rate rose 119 basis points; equation (3) predicts a change of 65 basis points. To take a more remote example, of the 104-basis-point rise in the consol rate from 1753 to 1761 (during the French and Indian War), equation (3) predicts a rise of 43 basis points. For the seven wars in our sample, equation (3) predicts between one-third and one-half of the movements (from either trough to peak or peak to trough) in the consol rate. The apparent predictive inadequacies of equation (3) are thus largely a result of its failure in peacetime—when we would not expect it to predict well in any event.

## 13.4 War and Prices

War and inflation have tended to occur together. During the two centuries covered by our data the highest price levels are observed during and immediately after the Napoleonic Wars and World War I. The highest inflation rates in our data are observed during and immediately after the same wars. The concurrence between war and inflation is no accident.

The best-known cause of the link between war and inflation is the tendency for the governments of belligerent nations to issue fiat money during the course of the hostilities. The onset of a (less-than-fully-anticipated) war implies a rise in the present value of current and future government expenditures. If these expenditures are to be financed, the present value of current and future taxes must necessarily rise. If the marginal resource costs of taxation increase as the value of the tax collections increase relative to the value of what is being taxed, then efficient public finance requires that (1) both current and future taxes be raised to keep the marginal costs of taxation constant over time, and (2) taxes be raised "across the board" to keep the marginal costs of taxation equal across taxed entities. The issue of fiat money, and the ensuing inflation it produces, provides the means of taxing (non-interest-bearing) money during wars.

The incentive to use fiat-money issue as a means of taxation exists for all countries. But for countries on the gold standard there is an additional attraction to issuing fiat money. The gold reserves held by the government of a gold standard country can be thought of as an investment in brand-name capital, valuable to the extent that they induce confidence in the country's money. As discussed above, the onset of war produces an increase in the value of goods now relative to goods later. The ensuing rise in the rate of interest creates an incentive to convert capital goods into currently available goods. One means of doing so is by disinvesting in the stock of brand-name capital behind the belligerent's money supply—printing fiat money, driving up the price level, and allowing gold reserves to flow abroad in search of lower-priced imports. Equivalently, one can think of the citizens of a belligerent nation as simply substituting paper for gold and shipping the latter to neutrals, thereby converting their confidence capital into imports of current goods and services that can be used to fight the war.

There is a second reason for wartime inflations in belligerent nations, one that is less well recognized but one that also helps explain the substantial deviations from purchasing-power parity that are commonly observed in wartime.[9] The onset of war implies that (1) current goods rise in value relative to future goods in belligerent nations and (2) currently available goods rise in value in belligerent nations relative to their value in neutral nations. The latter change in relative valuations implies that the real terms of trade should change so as to increase the real price of goods in belligerent nations relative to their real price in neutral nations. This provides the impetus for the large current-account deficit (excess of imports over exports of current goods) that enables belligerents to wage war. The rise in real interest rates implied by (1) provides the means by which the current-account deficit is financed by capital "exports," i.e., increased borrowing from abroad, for the relative rise in interest rates in belligerent nations attracts the foreign capital necessary to finance the current-account deficit. The rise in interest rates in belligerent nations is also the mechanism by which the relative prices in belligerent nations increase and the means by which the deviation from purchasing-power parity occurs. Absent the rise in belligerent interest rates, imports of goods could exceed exports only by the net outflow of gold. The rise in belligerent interest rates simultaneously permits (1) the direct link between a net inflow of goods and outflow of gold to be broken; (2) a rise in the relative price of goods in belligerent nations to occur; and (3) a deviation from purchasing-power parity that "favors" belligerent nations. The first of these events is a self-evident matter of accounting. The other two are more intricately related. Absent the rise in the interest rate in belligerent nations, the relative price level in those countries would not deviate (far) from the price level in neutral countries, since the outflow of gold would equalize price levels quickly. A relative change in prices and the associated rise in imports of goods relative to exports of goods are made possible only by the rise in interest rates in belligerent nations, since it is this rise in interest rates that makes people willing to hold the belligerent's obligations. A concomitant effect is that the purchasing-power value of a belligerent currency rises relative to the value to be expected simply by looking at price levels because of the increased value of holdings of the belligerent's obligations.

The positive association between war and inflation from 1729 to 1931 is graphically illustrated in the following simple regression (with $t$-statistics in parentheses):[10]

$$\Delta \log P_t = \underset{(.031)}{.002} + \underset{(4.15)}{.037}\ \mathrm{WAR}_{t-1}. \tag{4}$$

$$R^2 = .09,\ \mathrm{D.W.} = 1.62.$$

Some may find our discussion of war and prices puzzling in a paper prepared for a conference on the gold standard. After all, the issue of fiat money at rates unrelated to gold holdings seems hardly descriptive of any gold standard rule. Yet war is not a recent invention. Looking ahead from any of the dates in our sample, few except the aged could have felt confident that war was ruled out in their lifetimes. Even in the aftermath of victory in the War to End All Wars the prospect of war remained.

> In 1919 Lloyd George told the service chiefs that they need not anticipate a major war within the next ten years. In 1925 the service chiefs asked again and were given the same answer: no major war within the next ten years. This answer was repeated in 1926 and 1927. Finally, in 1928, the service chiefs were told, on Churchill's prompting, that they need ask no more: the ten years' freedom from major war began automatically each morning. This instruction was revoked only in 1932. (Taylor 1965, p. 228)

In our sample the yield on consols averaged 3.6 percent; thus on average, half of the present value was due to payments made beyond twenty years. The possibility of war and of a consequent departure from peacetime gold standard rules must surely have been a nontrivial consideration for the holders of consols. Moreover, much of the apparent price-level security of the gold standard derives from a tendency for past observers to focus on the success stories. It is true that Britain returned to gold at prewar parity after both the Napoleonic Wars and World War I, as did the Union after the Civil War and the United States after World War I. Yet when France returned to gold after World War I, she did so at a rate that was only one-fifth of the prewar level—a rate that surely would have overjoyed the holders of Confederate currency in 1866 or Reichsmarks in 1923. The price-level security of a gold standard is in this respect no different from the price-level security of any monetary standard—it exists only so long as the government remains committed to a stable price level.

## 13.5 War, Prices, and Interest Rates

The results we have obtained thus far yield two conclusions: (1) the apparent association between the interest rate and the price level is largely spurious, and (2) prices and interest rates tend to move together due to the common influence of wars on both. The question we briefly address in this section is the obvious one: Is the combined effect of these two facts enough to eliminate the apparent association between the interest rate and the price level? As equation (4) and footnote 10 show, only the lagged value of abnormal defense expenditures ($WAR_{t-1}$) has an effect on the inflation rate. In equation (5) we hold the inflation rate constant. Thus it is necessary to decompose $\Delta WAR_t$ into its components ($WAR_t$ and $WAR_{t-1}$) so that their possibly differential effects on $R$, given that the inflation rate is held constant, can be taken into account. Doing so yields the following results (with $t$-statistics in parentheses):

(5)
$$\begin{aligned} \Delta R_t = \underset{(.29)}{.005} + \underset{(1.30)}{.29}\ \Delta\log P_t + \underset{(4.10)}{.28}\ WAR_t \\ \underset{(3.08)}{-\ .21}\ WAR_{t-1}. \end{aligned}$$
$$R^2 = .10,\ D.W. = 2.18.$$

Once the effects of wars are accounted for, the weak relationship between prices and interest rates present in equation (2) disappears for all practical purposes. Gibson's paradox is the spurious product of war's effect on both prices and interest rates and not the result of any independent effects of prices on interest rates.[11]

## 13.6 Conclusion

During the more than two centuries that Britain was (with two interruptions) on the gold standard, there is *no* evidence of persistent trends in either the price level or the consol interest rate. Both the price level and the yield on consols moved in what was essentially a random walk. The comovement of the price level and the level of the interest rate so apparent to the eye is largely visual spurious regression. In significant part the movements of both the interest rate and the price level have been produced by war. Once the influence of war is taken into account, there is virtually no evidence of any linkage between the price level and the long-term interest rate.

# Notes

1. Our interest-rate series is from Homer 1963, tables 13, 19, and 57. We use annuities until 1752 and consols thereafter except for 1880–88 when the possibility of redemption at par made consol yields abnormally high. Following Homer's suggestion we use yields on 2.5 percent annuities for 1880–88. The wholesale price index (Mitchell and Deane 1962, pp. 468–70, 474–75) links the Schumpeter-Gilboy (consumers' goods), Gayer, Rostow and Schwartz (domestic and imported commodities), and the Sauerbeck-*Statist* (overall) indexes.

2. See Hall 1980 for an alternative discussion of the effects of temporary defense expenditures on the interest rate.

3. We refer to the log of the price level as a Martingale rather than a random walk because a Martingale has serially uncorrelated changes but not necessarily random and independently distributed changes. Random changes would be random in either actual or absolute values. We find no evidence in the autocorrelation of price changes in Britain under the gold standard of systematic inflations or deflations, but there is systematic evidence of disturbed and quiet subperiods. The autocorrelations of the *absolute value* of changes in the log of the price level are significantly positively autocorrelated at lags out to six periods. This, together with the earlier results, says that when the price level changed under the gold standard there was no way of telling what direction the change would go in the next year. On the other hand, there was strong reason to anticipate that large changes would be followed by large changes and small changes by small changes.

4. Indeed, with homothetic preferences, the interest rate would remain unchanged.

5. The intrawar rises in the long rate will be further stimulated by the ongoing depreciation of the capital stock.

6. Reported nominal defense expenditures, from Mitchell and Deane (1962, pp. 389–99), present several difficulties. First, during most of World War I and some of the Napoleonic Wars, the extraordinary military expenditures were "financed" with "votes of credit," with little or no detail shown in the records as to the disposition of the expenditure among army, navy, and ordnance. We added the votes of credit to the detail where shown and used the votes of credit alone where no detail at all is shown. The second problem is that the available defense-expenditure data are for fiscal rather than calendar years. Third, the dating of fiscal years changes four times during our sample. For 1727–54, the fiscal year ends 26 September; for 1755–99, it ends 10 October; for 1800–1854 it ends 5 January; in 1855, it ends 5 April; from then until the end of our sample, it ends 31 March. Finally, the series reports disbursements of funds rather than authorizations of expenditures.

The principal economic effects of defense expenditures that we are concerned with would be expected to occur when the men and materials are ordered rather than when the cash disbursements happen to be made. We arbitrarily assumed that the lag between orders and disbursements is three months. Thus the disbursements reported for fiscal year 1740 (ending 26 September 1740) would reflect orders placed during the last months of 1739 and the first six months of 1740. On the further assumption that disbursements (and the accompanying orders) are uniform throughout a given fiscal year, the value of the orders placed during *calendar* year 1740 equal one half of the disbursements for fiscal year 1740 and one half of the disbursements for fiscal year 1741. We also assumed that (1) 26 September and 10 October are the same date (1 October); (2) 5 January is 1 January; and (3) 5 April is

31 March. Indexing calendar years by $t$ and fiscal years by $\tau$, we thus have the following algorithms for converting fiscal-year data into calendar-year data. For 1727–99,

$$t = 1/2(\tau) + 1/2(\tau + 1);$$

for 1800–1854,

$$t = 3/4(\tau + 1) + 1/4(\tau + 2);$$

and for 1855–1931,

$$t = \tau + 1.$$

7. The serial correlations of ΔWAR display—in addition to the Working autocorrelation at one lag—a significant tendency for negative autocorrelation at lags 4 to 10. There is a significant tendency for positive values of ΔWAR to be followed by negative values in four to ten years. Wars (unlike "high" prices) tend to end.

8. An alternative to our method is to fit a time-series model to real defense expenditures and to use the difference between actual expenditures and forecasts of future expenditures as a measure of war. Two shortcomings of the alternative may be noted. First, what is the relevant future against which the present is to be compared? Second, the magnitude of defense spending in World War I is so large relative to the rest of the sample that the fitted model will be dominated by the few observations of those years, a problem that is less significant with the approach we chose.

9. See, for example, Friedman and Schwartz 1963, pp. 199–203.

10. Similar results are obtained by regressing the inflation rate on the current value of WAR. However, when both current and lagged values are included, only the coefficient of the lagged value is significant. These results suggest that it takes about a year for defense expenditures to have an impact on the inflation rate.

11. Even if wars are the complete explanation for Gibson's paradox, the price level may still appear to exert an independent influence on the interest rate in a regression such as equation (5). As noted earlier, the outbreak of a war involving Britain will drive up interest rates and prices in Britain. This will produce an outflow of gold in search of lower-priced goods from abroad and a search for borrowers abroad who are willing to lend at lower rates of interest. Both forces will tend to generate a rise in prices abroad as well as a rise in interest rates abroad. The same analysis holds in reverse for foreign wars: hostilities elsewhere will generate a rise in both the price level and the interest rate in Britain, holding the level of defense expenditures in Britain constant. We have fragmentary evidence of this force from America; during the Mexican-American War of 1846–47 and the Civil War, both the interest rate and the price level rose in Britain, even as the real level of British defense expenditures was declining. Controlling for foreign wars would of course require global information on real defense expenditures.

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## Comment Phillip Cagan

### Mr. Gibson's Paradox—Was It There?

The Gibson paradox has played a long and notorious role in the history of monetary theory. It appears to defy the principles of classical monetary

Phillip Cagan is professor of economics and chairman of the department at Columbia University, a research associate of the National Bureau of Economic Research, and adjunct and visiting scholar with the American Enterprise Institute.

theory. According to the quantity theory of money, long-run movements in the price level reflect corresponding movements in the stock of money. Since increases in the money supply also tend to ease financial markets, monetary increases that raise prices should also depress interest rates. The implication is that interest rates should be low when prices are rising and high when prices are falling. The Gibson paradox, that interest rates and prices move together in the same direction, appears to contradict the classical theory.

I have argued elsewhere (1972) that insofar as monetary changes affect interest rates because of movements along the money-demand schedule, the so-called liquidity effect, such an effect will be transitory. If monetary changes have long-run effects on interest rates, they must reflect a saving of the revenue from money creation whereby the revenue is not treated as ordinary income and spent on consumption. That the revenue from money creation would be treated differently from ordinary income is open to question, however, so far as long-run movements are concerned. Therefore, conventional monetary theory does not necessarily imply that increases in the money stock reduce interest rates, and, while conventional theory cannot explain the paradox, they are not in opposition.

Nevertheless, many antiquantity theorists delight in pointing to the paradox in support of their various anticlassical arguments. One of the earliest of these theorists, Thomas Tooke, who had a cost-of-production theory of the price level, argued that interest rates were a cost of production and had a positive effect on prices. Ricardo and Wicksell disposed of that explanation on the correct argument that interest rates influence relative prices, not the general price level, though Tooke's theory continues to be popular with the public and the press. Other explanations have never been in short supply. Gibson himself believed that the "cost of living" influenced saving, so that low prices increased the supply of loanable funds and lowered interest rates. That explanation is unsatisfactory, as was noted by Keynes (1930), who brought Mr. Gibson's articles to the notice of economists and enshrined his name on the paradox. Hicks (1950, p. 154n) proposed that short-term rates rose more sharply in the cyclical upswings when the secular growth in money is higher and presumably more variable; consequently, bond yields, which are an average of short-term rates, tend to remain higher when secular monetary growth is higher. Hawtrey (1913) had the traditional view that wages lag prices and do so by more, the faster prices rise; lower real wages raise profits and the real rate of interest. Macaulay (1938) claimed that rising prices are favorable to investment expenditures, an expansion of which increases borrowing and interest rates. More recently Shiller and Siegel (1977) have proposed that rising prices are unanticipated and shift wealth from lenders to borrowers, which reduces the net demand for financial assets and raises interest rates, and conversely for falling prices.

An influential explanation was proposed by Wicksell (1936) and independently by Keynes (1930). They posited secular fluctuations in investment demand. These fluctuations affected the supply of money—through the banking system for Wicksell and through the central bank's reserve ratio for Keynes. These fluctuations caused corresponding fluctuations in both interest rates and the money supply, and the latter in turn accounted for the behavior of prices. This explanation therefore relied on the quantity theory for the price effect. While velocity movements could conceivably finance part of investment fluctuations over the business cycle, an explanation of the long-run price movements in the Gibson paradox could not be attributed to velocity and required corresponding movements in the money supply. The fact that rising prices were accompanied by rising rather than higher interest rates could also be explained by supposing that the banking system or central bank responded to increases in investment by gradually expanding the money supply and raising interest rates. Each rise in interest rates was necessary to induce a further decline in reserve ratios and increase in the money supply.

The fluctuations in investment demand were not explained but were taken as given, which was easy to accept in an age concerned with the instability of the private economy as symbolized by Kondratieff cycles, Schumpeter's creative destruction of capitalistic industry, and Keynes's animal spirits of businessmen.

The Keynes-Wicksell theory is a plausihle theory, and I have no objections to it except that it is inconsistent with the evidence, as I pointed out in my study of the U.S. money supply (1965). Their theory implies that secular movements in the money stock are due to the reserve ratio of banks or the central bank, whereas in fact such secular movements were due to high-powered money and growth of the gold stock in relation to total output. The gold stock is not related positively to prices or interest rates. If anything, the relation would be inverse. Higher prices and interest rates discourage gold production, and its positive association with prices reflects the quantity theory of money.

The explanation for the Gibson paradox which is accepted by most monetary theorists today, as noted by Benjamin and Kochin, is Irving Fisher's—that nominal interest rates adjust to the rate of depreciation in the value of money. It has spawned a lively research industry to determine whether it fits the evidence. Benjamin and Kochin intend to spoil the fun by announcing, "Gentlemen, the object of your obsession does not exist!" which leaves nothing to explain except the obsession itself.

They make the impressive argument that in the British data for the period 1729 to 1931, government defense expenditures account for the large fluctuations in prices and interest rates. These expenditures have the same effect as investment does for Keynes and Wicksell, but the source of the instability in the economy is cleverly shifted from the private sector to the government. Benjamin and Kochin argue that defense expenditures will be partly financed by money creation and partly by borrowing and that the latter will raise interest rates. They go to great pains to argue that this is a "rational" response of the economy. According to the latest fashion, if it isn't rational it is not supposed to exist. But whether rational or not, I have no difficulty in accepting such an effect of defense expenditures as simply an historical fact. When these large fluctuations in interest and prices owing to defense expenditures are removed, not much worth mentioning remains.

So far, so good. Then the authors put the Gibson paradox to the acid test by regressing the change in interest rates on the change in defense expenditures and the change in the log of prices. Amazingly, the paradox hangs on. The price variable is significant, but only marginally. In the version of the paper presented at the conference, their present equation (5) had only one WAR term and the $t$-statistic for $\Delta \log P_t$ was 1.97. In the present version of (5), $WAR_{t-1}$ has been added and the $t$ on $\Delta \log P_t$ drops to 1.3.

*B* and *K* are relentless debunkers. At the conference they argued that the significant but anemic evidence of the paradox probably reflects foreign wars, since defense expenditures abroad would have repercussions on Britain similar to the domestic effect. In the present version of the paper they have found an equation form that reduces the significance of the paradox.

I would be more sympathetic to the paradox. Their regression correlates *concurrent changes* in the interest rate and prices, putting a heavy empirical burden on the paradox and the Fisher explanation. This is so, because the concurrent changes may miss the long-run swings in these series and probably do not properly represent the expected changes in prices. I suspect one could shore up the significance of the Fisher effect with a lagged term or two. The authors justify the use of concurrent changes on the grounds that the level of interest rates and prices in the period tested display the property of random walks. This is not surprising since the cyclical movements are quite erratic, and the series display little overall trend from the beginning to the end of the period. The statistical problem with random walks is that they produce drift, and two such series can be correlated even if they are completely independent.

However, the longer-run upward and downward movements that form the Gibson paradox number only three in the British data and essentially but two in the U.S. data from the mid-1800s to World War I. Even these few observations of the longer-run movements are partly obscured by the large short-run fluctuations due to wars and crises. Therefore, it is not surprising that when these fluctuations are not removed and the periods not specially selected, little evidence of the paradox or Fisher effect remains.[5] In a recent paper Lawrence Summers (1982) correlated the lower frequency movements in U.S. interest rates and price changes, on the assumption that the Fisher effect pertained mainly to such longer-run movements, and found no consistent relation for a variety of periods. Contrary to the earlier more sympathetic search for the Fisher effect, the recent skeptical investigations conclude that there is no solid evidence of it.

I count myself among the first to welcome this skeptical turnaround in econometrics. For a long time the journals have been filled, and unfortunately still are, with allegedly significant regressions that probably mean nothing. But now the new ARIMA techniques of purging the data of systematic movements to avoid spurious correlation are showing that it is difficult to demonstrate anything. That is probably as it should be. Yet when Benjamin and Kochin classify interest and prices under the gold standard as random walks and nothing more, I get uneasy. I am not sorry to see the Gibson paradox go, but I think the Fisher effect is getting the bum's rush.

I do not wish to deprecate the long overdue revival of skeptical econometrics. Indeed, I welcome it and support it. But at the same time I would not forget about type-two error in the new zeal not to commit type one. Type-two error, as some may need reminding since it is so seldom mentioned in economics, is the rejection of hypotheses that are in fact true. I am not an expert, but I suspect that many of the new statistical tests that are rejecting very plausible relationships are weak tests, in the sense of having type-two errors. For one thing, regression equations impose a rigid timing relationship on data when in fact the relationship is probably variable. My eye may see random walks as trends, as Benjamin and Kochin warn, but by the same token my eye may be able to make allowances that are beyond the capability of dumb regression equations. My nostalgia for the old-fashioned qualitative analysis is not meant to be critical of Benjamin and Kochin, who have given us an excellent paper. I am simply reluctant to accept completely their well-reasoned conclusion.

How far then might a case for the Fisher effect be carried? Let me start with the Cartesian proposition that the Fisher effect is not a figment of our imaginations. It really does exist! No one could be sure of this before the 1970s, but today there is no doubt. There is simply no way to account for the high nominal interest rates since the late 1960s, which have remained far above historical levels, without the Fisher effect.

The only question is whether the effect would operate under the traditional gold standard. One might argue that it wouldn't. The gold standard, after all, gave a strong guarantee of a certain degree of price stability, seemingly eradicating expectations of persistent price changes. But prices were not in fact completely stable. U.S. wholesale prices, based on reference-cycle averages centered at reference peaks, fell from the cycle in which the 1879 resumption occurred to the 1893–95 cycle over a decade later at a rate of 2.4 percent per year and rose thereafter to the 1907–10 cycle at a rate of 2.5 percent per year. It is a question relevant to the topic of this conference whether during these periods of the gold standard there was a change in expectations, first of declining prices and then of rising prices. It was not impossible, certainly, for expectations to change. No one at the time could be sure that the gold stock would grow just the right amount to produce perfect price stability. I think it is quite plausible that the price movements of the pre–World War I period, which though random from year to year persisted for about two decades first downward and then upward, would be recognized as trends whose duration was unknown. A gradual recognition would be rational if there was difficulty distinguishing permanent from transitory movements. It is likely that the corresponding movements in bond yields were not accidental but a response that would have come gradually, as it did.[1]

I don't see any other explanation for these long-correlated movements. My eye rejects them as wholly accidental. There was no change in government expenditures of significance during these periods. Did the real rate of interest follow such a pattern? Doubtfully, since the possible causes of such a movement would not explain the gold and money-stock movements. No other equally attractive explanation to Fisher's has been presented. The evidence is circumstantial and plausible but not, to be sure, definitive. Regressions will not support it unless we select the periods properly and abstract from the shorter-run movements and special periods. But so what? The Fisher effect under the gold standard is bound to be small and easily lost among stronger disturbances at work, so we must be especially on guard to avoid type-two errors.

I would turn the traditional sequence of analysis around. Do not ask first whether the Gibson paradox was there, and then what caused it. Instead let us assert that the Fisher effect exists, therefore the Gibson paradox was there.

## Notes

1. "Bond yields fell and rose during [the respective periods between reference cycle averages reported in the text] somewhat less: a fall of 1.4 percentage points in the first period (or 1.7 points allowing for the lagged upturn) and a rise of 0.1 in the second period (or 1.0 point from their trough in 1899–1902 to a prewar high for the 1913–18 reference cycle . . . ). Bond yields in money terms, therefore, seem to have accounted gradually and slowly for roughly half the average rate of initial appreciation and subsequent depreciation of money; and, the longer the movement of commodity prices in one direction, the larger was the adjustment" (Cagan 1965, p. 307).

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# General Discussion

KOCHIN started with a clarifying comment. He pointed out that both authors believe that the Fisher effect exists in the United States, Brazil, and Israel today. Nominal interest rates in Israel are different from those in Switzerland, and the difference is connected to the difference in inflation. But in Great Britain under the gold standard there was no rational basis to anticipate future inflation on the basis of past inflation, and thus no presumption that there should have existed a Fisher effect.

In response to Barro, who recommended that Benjamin and Kochin use the levels of variables in at least some of their tests, Kochin cited the work of Granger and Newbold. Their work indicates that there is a 25- or 30-percent probability of observing a $t$-statistic suggesting a significant relationship when the variables are both random walks, if the levels of trended variables rather than first differences are used.

MCCLOSKEY urged the authors to follow Barro's suggestion regarding causality statistics, but he argued against using those in Lewis F. Richardson's 1960 book, *Statistics of Deadly Quarrels* (Pittsburgh: Boxwood Press). He cited an even more up to date compilation by J. David Singer and Melvin Small in a book entitled *The Wages of War, 1816–1965: A Statistical Handbook* (New York: John Wiley), published in 1972. McCloskey also urged the authors to undertake similar exercises for other countries and asked whether the authors' argument concerning temporary military expenditures also applies to other temporary expenditures. Will it apply to investment booms or to oil shocks?

BENJAMIN agreed that it will, but emphasized the problem of measuring such temporary shocks in a systematic and credible way.

FREEDMAN noted the authors' argument that the price level is a Martingale. Would this argument still apply if, when war begins, the rate of inflation accelerates suddenly, and everyone expects price levels to remain high and to continue to rise for awhile before coming back down to their normal levels in peacetime? If that is the case, one has to be very careful to take into account the existence of expected inflation during wartime.

BENJAMIN suggested that the problem is the distinction between conditional and unconditional forecasts. His initial reaction had been much the same as Freedman's. The problem appears to be that once a war ended, residents could be reasonably confident, unless they lived in Germany, that the price level ultimately would fall. However, even in the British case, prices do not fall right after a war; instead, inflation continues, sometimes for very brief periods, sometimes for as long as two-and-a-half years. Thus, forecasters face two problems: When is the war going to end; and once it ends, is the price level really going to fall? Ultimately, it did fall in Britain, but that was not the case in many other countries. Residents could not predict with confidence what would happen in their country of residence.

WEINTRAUB pointed out that everyone agrees that the Fisher effect has existed in recent years, after the demise of the gold standard. The question is whether it existed under the gold standard? Benjamin and Kochin conclude that it did not exist for the United Kingdom and Lawrence Summers found it did not exist for the United States. Irving Fisher, using less sophisticated techniques, also found it did not exist for the United States. It is not surprising that we *fail to observe* a Fisher effect under the gold standard, for prices were reasonably stable for long periods of time. Under a gold standard, we should fail to observe a Fisher effect since prices are not expected to change; however, that doesn't mean the Fisher effect *doesn't exist*. Interest rates could have been vastly different had prices been expected to change.

MELTZER argued that an accurate reading of David Hume suggested that the lag between price changes and interest rates was highly variable—not constant, as Benjamin and Kochin had modeled it. Wars and other discontinuous events would show up quickly, whereas changes in the world money supply might show up more gradually.

McGouldrick suggested that international comparisons embracing the 1850s and 1860s would strengthen the authors' case. In this earlier period, there was not only the Civil War in the United States but the Crimean War, two wars between Austria, Prussia, and Denmark, and finally the Franco-Prussian War in 1870. In comparison, after 1870 there were two decades of peace with no fighting except against the natives of the Third World. This development makes the rise in interest rates between 1896 and 1914, a period of general peace broken only by the Russo-Japanese War, somewhat puzzling.

Harley expressed his disagreement with previous statements about what a reasonable expectation for price changes under a gold standard mught be. Reasonable expectations would have been based upon the economics of a nonrenewable resource, namely gold. Accordingly, the price of gold should have been expected to rise gradually over time, as stocks of other assets were augmented more rapidly than stocks of the exhaustible resource. Unanticipated gold discoveries would of course modify this story in important ways.

Kochin drew attention to the distinction between ex post and ex ante interest rates and prices. While ex post one can discern trends in stock prices, surely few observers would dispute the efficiency of the stock market. That is, ex ante the best predictor of future stock prices is current stock prices. The same property holds for commodity prices under a gold standard.

Zecher inquired whether this notion should also be true for a period such as 1790–1815, when prices doubled? He recommended the use of indirect evidence to answer the question. For example, if one observes that people had formed political parties and organized in order to change the monetary system, as they did in the 1890s, this suggests they were expecting prices to fall, as prices in fact did. The free-silver movement provides a classic example of this phenomenon.

Weintraub argued that there is considerable evidence that during the last part of the nineteenth century, the public was frightened of what was perceived as the downward trend of prices. There is a considerable literature in which people describe their fears and thoughts. Similarly, after 1896 there was much discussion of upward trends in prices. Many observers would have bet at that time (1896–1916), early on rightly but later on wrongly, that over periods of five, ten, or more years the price level would have risen, as they would have bet between 1873 and 1893 that the price level would have fallen, again early on rightly but later on wrongly.