[The Macroeconomics of War and Peace]: Comment

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capital. They remain at the level of the macroeconomist. They do not descend to the level of the economic historian, where complementarity of capital goods calls for demonstration that the productivity of each kind of capital good was greatly multiplied by the presence of the other.

So I conclude that Braun and McGrattan have not made a case that procyclical productivity in World War II is consistent with a competitive model. Labor productivity increased by 30% in the United States during World War II—an extra gain in output of roughly 60 billion 1958 dollars a year. If this had been generated by additional capital yielding 20% per year, the investments would have to be on the order of 300 billion 1958 dollars. But Braun and McGrattan have only 25 billion dollars of GOPO capital. The effects they point to are there but are only one tenth the size needed to account for the World War II experience.

What about the other 90% of the productivity gains of World War II? What accounts for them? Some part is surely generated by increased work effort: People work harder if the job is not just to earn money but is worth doing for its own sake to defeat Hitler. Some part is surely generated by the imperfect competition mechanisms of Hall, and Rotemberg and Woodford. Some part is surely the high valuation we place on military goods—whose value is determined not by consumers but by governments. But we do not have a good breakdown. It is good to have a good account of even 10%.

Comment

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This paper is in three parts. The first part is my favorite. It lays out a great number of interesting facts concerning the two world wars in both the United Kingdom and the United States. There are clearly some similarities between the two experiences because output and productivity rose in all four episodes. But, what is probably even more interesting are the differences between the U.S. and the U.K. behavior of employment and real wages.

The second part of the paper presents a general equilibrium model that captures Braun and McGrattan's theory of why productivity and output rose in the first place. Their theory of the initial expansion in output is traditional: Output rose because there was an increase in labor supply, which was itself the result of the wealth reduction brought
about by the war. What is more novel is their theory of why productivity rose. They attribute this to an increase in government-owned privately operated (GOPO) capital. In calling for increased focus on GOPO, the authors are surely right. GOPO capital is, as pointed out initially by Robert Gordon (1969), a very large component of capital by the end of the war.

The third part of the paper argues that the model and, thus, GOPO capital, can explain the behavior of productivity and real wages in the aftermath of increases in military spending. This is the weakest part of the paper. I feel that much more effort needs to be spent analyzing the specific consequences of the GOPO program before we will know its contributions to productivity and real wages.

I will start my discussion by talking about the model. Then I will talk about the evaluation of the contribution of GOPO capital. Then, finally I will have some comments on the difference between the U.S. and the U.K. behavior of real wages.

1. The Model

Their model is one where output is produced with a production function that depends on the aggregate labor input, the aggregate private capital stock, and GOPO capital. The model assumes that, except for the choice of GOPO capital, the other economic decisions are made by a central planner who maximizes expected discounted utility. I must say that I preferred the previous version of this paper where the outcome was decentralized and production decisions were made by firms. In that earlier version, the firms were perfect competitors who had each been given an endowment of GOPO and sold their output at marginal cost.

Both versions suffer from the problem that output in the model differs from measured GNP. In the current paper, the value of output is the value of what is produced with all three inputs. That would correspond to measured GNP only if all this output were sold in the market. In the previous version, this was even clearer because the firms did indeed sell all their output to the government at marginal cost.

1. One advantage of dealing with a market equilibrium is that there it gives a rationale for aggregating the outputs of different goods by using their prices as weights as is done in the National Income Accounts. The reason is that, in a competitive equilibrium a good that gives ten times as much utility as another will have a price ten times as large. There is then some basis for saying that one unit of this good adds ten times as much to consumption, investment, or government purchases. By contrast, prices don't need to play any role in a centrally planned economy.
But, at least in the United States, the government was also the sole purchaser of the output produced with GOPO. This matters because there is no reason for the government to pay the firms the entire value of the output. Indeed, if the output is sold at marginal cost even though the firms have been given a GOPO endowment, then the firms will make positive profits.

In the United States, plants with GOPO were operated under two regimes. In one regime, the government hired the private company as a contractor and made a payment for the services provided by the contractor. In another (called "Cost Plus Fixed Fee"), the government reimbursed the contractor for its cost and added a prespecified profit. In either case, we would not expect the firms to make any unusual profits; there would be no reason for the government to pay for the services of the capital that it had itself provided. Thus, the payments from the government would fall well short of what it would have to pay to buy the same goods in the open market. But, it is only these payments from the government, rather than the value of total output, that are counted in GNP. (The reason is that GNP does not include the services provided by government-owned capital.) Thus, the authors overstate the theoretical effects of government purchases on GNP.

2. The Evaluation of the Model

The existence of price controls and rationing make it unlikely that any general equilibrium model is exactly right, and this may well be the reason why the authors moved to a specification where prices play no role. It would have preferred it if, instead, they had made a larger effort in specifying which parts of the model can be tested even in the presence of rationing. Given the widespread rationing of consumer goods, it seems reasonable to believe that the marginal rate of substitution between consumption and leisure could well be different from the published ratio of the nominal wage to the CPI. My sense is that, instead, Braun and McGrattan are mainly concerned with explaining the behavior of productivity and real wages with a model where price is always equal to marginal cost and where there are diminishing returns to labor. Insofar as firms were not rationed in the amount of inputs that they could buy or in the amount of output they could sell, the validity of this part of the model can be ascertained with productivity and real wage data for the period.

Braun and McGrattan are surely right that GOPO contributed to productivity growth and to the rise in real wages during the World War II period in the United States. The question they have not settled is how
Table 1  OUTPUT AND THE AVERAGE PRODUCT OF LABOR IN SELECTED INDUSTRIES

<table>
<thead>
<tr>
<th>Year</th>
<th>Stone, clay, and glass</th>
<th>Food and food products</th>
<th>Tobacco products</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Output</td>
<td>APL</td>
<td>Output</td>
</tr>
<tr>
<td>1940</td>
<td>100</td>
<td>100</td>
<td>100</td>
</tr>
<tr>
<td>1941</td>
<td>125.97</td>
<td>104.57</td>
<td>112.40</td>
</tr>
<tr>
<td>1942</td>
<td>125.48</td>
<td>104.25</td>
<td>126.19</td>
</tr>
</tbody>
</table>

The output series is the industrial production index from the Federal Reserve Bulletin. The hours series needed to construct the average product of labor are constructed by multiplying “Factory Employment” with “Average Hours Worked per Week” from the same source.

much GOPO contributed. It appears from their discussion that they think GOPO explained a very large fraction of the increase in productivity and real wages. That seems unlikely to me. First, this view is inconsistent with the post-World War II facts. As Hall (1988) and Rotemberg and Woodford (1992) have shown, productivity and real wages also rose together with military purchases after World War II even though GOPO did not exhibit unusual growth during the Korean or Vietnam Wars. Whatever led productivity and real wages to rise in this other period must have been at work in World War II as well.

There is also some more direct evidence that GOPO can’t be the whole story. First, I analyzed the behavior of output and average labor productivity in industries where GOPO was not important. Table 1 shows the behavior of output and the average product of labor in Stone, Clay, and Glass; Food; and Tobacco for the years 1940–1942.

The output and the productivity of all three industries rose from 1940 to 1941. The Food and Tobacco industries became important suppliers for the war effort, and their output continued rising from 1941 to 1942. What is interesting is that, in spite of the absence of significant GOPO in these industries productivity rose as well. From 1941 to 1942, the Stone, Clay, and Glass industry contracted because construction slowed down considerably. As we would expect from Hall’s findings for the postwar era, productivity in this industry fell as well.2

Figure 1 shows evidence that GOPO does not explain the entire increase in real wages either. This figure, which was part of early drafts

2. If one takes the view of Rotemberg and Woodford (1992) that increases in military purchases raise economic activity by raising labor demand, then the fall in construction activity also rationalizes the fall in the real wages of skilled building workers documented in Bry (1960) and cited in Braun and McGrattan’s paper.
of Rotemberg and Woodford (1992), shows the changes in real military purchases, in privately produced real value added and the changes of the logarithm of real wages. Real wages are deflated by the deflator for private value added, which is what is relevant if one is asking about the relationship between the marginal product of labor and the real wage.\textsuperscript{3} We see that there was already a substantial increase in real wages between 1940 and 1941, even though GOPO capital at the end of 1940 (i.e., capital we would typically view as available for 1941 production) was trivial.\textsuperscript{4} It also shows that real wages declined together with private output and military purchases in 1944–1946. While this is not shown in Figure 1, productivity declined as well at the end of the war. But, GOPO capital rose significantly from 1942 to 1945 and stayed high thereafter. Thus, I think it is impossible to explain either the timing of the increase in real wages and productivity or their subsequent fall at the end of the war by attributing them exclusively to the changes in GOPO.

\textsuperscript{3} Braun and McGrattan use the real wage deflated by the CPI. This is the wage that would be relevant if they were looking at the first-order condition relating the real wage to the marginal rate of substitution of consumption for leisure.

\textsuperscript{4} According to the BLS Fixed Reproducible Capital in the United States, 1925–1979 (1982), there was less than $1 billion 1972 dollars of GOPO capital at the end of 1940. This compares with $8 billion at the end of 1941, $28 billion at the end of 1942, and $47 billion at the end of 1944.
3. The United Kingdom versus the United States

I want to close by commenting on the difference in the behavior of real wages in the United Kingdom and the United States. This difference has been noted before by Correia, Neves, and Rebelo (1992), who argue that the negative correlation between real wages and military purchases in the United Kingdom provides evidence for the standard neoclassical model of government spending. This negative correlation is principally due to the behavior of British real wages during World War I. The problem is that measuring British real wages in this period is apparently subject to great difficulty.

For the period with which I will be concerned, i.e., that from 1914 to 1924, the authors use the index of wages presented in Feinstein (1972). As pointed out by Dowie (1975), this index is identical to that which Bowley first published in 1921 and then supplemented in monthly issues of the London and Cambridge Economic Service Bulletin. Dowie (1975) criticizes this index and recommends that for the period 1914–1924, one use instead Feinstein’s (1972) index of weekly earnings. The use of this alternative index makes a big difference.

Figure 2 presents data on both real expenditures on national defense and two series for British real wages. The first is the wage series in Feinstein (1972) that Braun and McGrattan use, though I have deflated it by the GDP deflator. The second, alternative series, uses the Feinstein wage series up until 1914. From 1914 to 1924, it uses instead the percent-

Figure 2 WAGES AND NATIONAL DEFENSE PURCHASES IN THE UNITED KINGDOM
age changes in the Feinstein earnings series, which it applies to the 1914 base. Finally, I follow the authors and use the percentage changes in the Feinstein wage index from 1924 on. Because the alternative series grows 20% more between 1914 and 1924, the line representing the alternative wage remains above the original series after 1924. If one uses this alternative wage series, real wages rise together with military purchases during World War I.

As Dowie (1975) shows, the problem with the original Bowley (1921) series, which is the basis of the Feinstein (1972) wage series, is that Bowley himself later changed his mind about the growth rate in nominal wages from 1914 to 1924. While his original index grew by 74% in this period, he concluded in Bowley (1952) that earnings for a "normal week" grew by 94% in this period. Moreover, Bowley is quite explicit that this growth in weekly earnings does not involve any changes in hours worked. He says, "The intention throughout has been to measure the change in wages paid for unchanged work in the number of hours that at each date constitute a normal week without overtime or short time" (Bowley, 1952, p. 500). As further confirmation that he viewed hourly wages as having risen by 94%, Bowley (1952) provides a new wage series that grows by that amount between 1914 and 1924. The problem with this newer Bowley series is that, as pointed out by Dowie (1975), it is quite arbitrary. It simply spreads evenly over the 1914–1919 period the difference between the 74% increase of the original series and the 94% that he ultimately deemed accurate.

These problems lead Dowie (1975) to prefer the Feinstein (1972) series on weekly earnings. In principle, changes in this series could be due not only to wage changes but also to changes in hours worked. However, as Dowie reconstructs this series, changes in hours worked appear to play no role in the measurements between 1914 and 1924. In particular, Feinstein's (1972) earnings series grows by 94% from 1914 to 1924 as the hourly wage series is supposed to. Moreover, the movements in the series from 1920 to 1924 are based on the movements of published wage data that are not affected by changes in hours.

My conclusion from this is not that I am sure that real wages really did indeed rise with military purchases during World War I. It is rather, as Dowie (1975) also concludes, that we have very little reliable information on this question. The data do seem more reliable for World War II. British real wages also rose in World War II although, admittedly, only well after military purchases became substantial. What is striking is that, as in the United States, real wages fell together with military purchases at the end of the war.
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Discussion

Bob Gordon, "Mr. GOPO himself" according to Olivier Blanchard, had several remarks. First, he agreed with the basic idea expressed in the Comment by Brad De Long, which is that at $25 billion GOPO was just too small to explain why real GDP in 1958 dollars increased by more than $100 billion. Second, Gordon noted that the experience in the U.S. economy immediately following World War II is also instructive. Per capita GDP fell substantially after the war until around 1950. Similarly, productivity growth declined before picking up during the period 1948–1951. The reason for these movements, Gordon suggested, is the Korean War: Much of the GOPO capital sold to private operators after the war was specifically tailored to wartime use. Military expenditures as a share of GDP jumped to 10% during the Korean War, and this post-World War II variation may be useful in pinning down the contributions of GOPO capital.

Finally, Gordon emphasized the importance of capacity utilization in understanding the procyclicality of labor productivity. Capital services rather than the capital stock is the correct input into the production function, and if the number of worked shifts increased during the war, this difference can be important. In terms of separating this effect from the contribution of GOPO capital, Gordon suggested that examining the
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