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Fiscal Policy and Economic Recovery: The Case of the 1936 Veterans' Bonus[†]

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Conventional wisdom has it that in the 1930s fiscal policy did not work because it was not tried. This paper shows that fiscal policy was tried in 1936. The veterans' bonus of 1936 paid 2 percent of GDP to 3.2 million veterans; the typical veteran received a payment equal to per capita income. Multiple sources, including a household consumption survey, show that veterans spent the majority of their bonus. Point estimates of the MPC are between 0.6 and 0.75. Spending was concentrated on cars and housing in particular. (*JEL* E21, E32, E62, N32, N42)

> *The gov't last week paid a soldiers' bonus of over two billion and as a result the veterans have been buying cars, clothing, etc. Streets are crowded and the highways are jammed with new cars. It begins to look like old times again.*
>
> —Benjamin Roth's diary, June 25, 1936 (Roth 2009, p. 172)

In most years of the 1930s, increases in government spending were matched by increases in taxes. 1936 was an exception.[1] Over Franklin Roosevelt's veto, congress

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[1] The seminal reference is E. Cary Brown (1956), who analyzes movements in the full employment deficit over the 1930s. Brown (1956, pp. 863–866) famously concludes that "Fiscal policy, then, seems to have been an unsuccessful recovery device in the 'thirties—not because it did not work, but because it was not tried." This quote is often given in isolation, ignoring Brown's statement that there was significant spending in 1931 and 1936, when the government paid veterans (Brown 1956). Brown may also underestimate the size of the veterans' bonus in 1936. He says that the bonus was $1.4 billion in 1936 (Brown 1956). This is the dollar value of the bonds issued to veterans that were cashed (see Section IB). The more appropriate measure of the bonus for comparison to other tax and transfer programs is the amount given to veterans. This was $1.8 billion. The magnitude of the bonus in 1936 is what leads me to argue that, contra Brown's statement, fiscal policy was tried in the 1930s, at least in 1936.

authorized a deficit-financed payment of $1.8 billion to 3.2 million World War I veterans.[2] The bonus was 2.1 percent of 1936 GDP, roughly the same magnitude as annual spending from the American Recovery and Reinvestment Act (the Obama stimulus) in 2009 and 2010 (Council of Economic Advisers 2010). The typical veteran received $550 dollars, more than annual per capita income, and enough money to buy a new car.

This paper marshals a variety of evidence to learn whether and how veterans' spent their bonus. I find that within 6 months veterans spent roughly 70 cents out of every dollar received. The primary evidence comes from a household consumption survey. This survey allows me not only to measure veterans' marginal propensity to consume, but also the composition of veterans' spending. Spending was concentrated on durables, particularly automobiles. These findings are confirmed by cross-state and cross-city regressions and by an ex ante American Legion survey of veterans' spending plans.

Given its size, economic historians have sometimes suspected that the bonus had a positive impact on 1936 growth. However, there is almost no systematic work analyzing whether or how the bonus was spent. Only one prior paper, Telser (2003), examines the veterans' bonus in detail. Telser (2003, p. 240) studies a variety of time series and concludes that the bonus "brought a large measure of recovery to the economy." Although a useful start, Telser's work is limited by his exclusive use of time series evidence. Since the bonus was a one-time event, this makes it impossible for Telser to conduct formal statistical tests of the bonus' impacts.

In addition to revisiting the time series data, I exploit three other sources of evidence on the bonus' effects. First, I use a 1935–1936 household consumption survey to estimate veterans' marginal propensity to consume (MPC) out of the bonus. Since this consumption survey did not ask about respondents' veteran status, I use a two-step estimator with auxiliary information from the 1930 census. The consumption survey has information on the age, race, and location of each household. These variables also appear in the 1930 census, along with an indicator for World War I veteran status. In the first step, I estimate the relationship between veteran status and age, race, and location. The second step relates these predicted values—the probability a household contains a veteran—to the change in consumption pre- to post bonus payment. The key identifying assumptions are (i) that the consumption of veterans and nonveterans would have followed a parallel trend in 1936 absent the bonus, and (ii) that the variables that I use to predict veteran status affect the change in consumption only through veteran status. Given these assumptions and the ability of the first stage regression to reasonably predict veteran status, I provide a proof (in online Appendix C) that this procedure provides consistent estimates of spending from the bonus.

The point estimates are of an MPC between 0.6 and 0.75. This high MPC likely reflects the state of the economy in 1936, in particular the combination of liquidity constraints, expectations of higher future income, and a low stock of durables. The household consumption survey also allows me to estimate marginal propensities to consume for subcategories of consumption. These estimates imply that veterans

[2] Here, and throughout, data sources are listed in the data Appendix.

spent almost a quarter of their bonus on car purchases and vehicle operations. The bonus increased the probability of a car purchase by 22 percentage points relative to a baseline probability of purchasing a car of less than 20 percent. Results also suggest substantial spending on housing consumption. Estimates for other categories of consumption are less precise but point to spending on furniture/appliances, clothing, recreation, and food.

A second source of evidence on the bonus' effects are cross-state and cross-city regressions. Significant variation in the share of veterans in a state or city's population meant significant variation in the fiscal stimulus received in 1936. As expected given the household survey results, there is a strong relationship across states between veterans per capita and the change in car sales in 1936. On average, one additional veteran in a state was associated with 0.3 more new cars sold.[3] There is also a strong association between the proportion of a city's population made up of veterans and the change in residential building permits from 1935 to 1936. An additional veteran in a city was associated with at least $100 more residential building.

A final source of evidence on veterans' spending behavior comes from an unpublished American Legion survey that asked 42,500 veterans how they planned to use their bonus. Veterans told the American Legion that they planned to spend 40 cents out of every dollar on consumption goods and to spend an additional 25 cents out of every dollar on residential and business investment. Evidence from the 2001 and 2008 tax rebates suggests that such ex ante surveys may understate the MPC. Thus the prospective MPC of 0.4 measured in the American Legion Survey suggests that the actual MPC may well have been higher. It is evidence that the MPC of 0.6 to 0.75 that I measure in the household consumption survey is not an artifact of the particular sample or estimation method.

Neither household survey nor cross-state estimates of the bonus' effects translate directly into a measure of the bonus' aggregate impact. The effect of the bonus on the economy as a whole was a function not only of the recipients' MPC, but also of general equilibrium effects that could have amplified or diminished the initial spending impulse. The microeconomic data used in this paper have little to say about the magnitude or direction of these general equilibrium effects. Still, by making an assumption about the spending multiplier, it is possible to perform a back-of-the-envelope calculation of the multiplier associated with the bonus. A spending multiplier of 1 would imply that the bonus added roughly 1.6 percentage points to 1936 GDP growth.

This paper contributes to two literatures. The first is the literature on the consumption response to fiscal transfers.[4] Quite apart from its historical interest, features of the veterans' bonus make it a useful natural experiment. First, for its recipients, the bonus was far larger than recent US tax cuts or transfer programs. Second, the identity of the recipients was determined solely by whether or not one had served in World War I. This makes identification of the bonus' effects relatively

[3] These results can be compared to those in Fishback and Kachanovskaya (2015). They examine the cross-state multiplier from all types of federal spending in the 1930s. Like me, they find large effects on auto sales, although they find relatively small effects on income and employment.

[4] For an overview of this literature, see Jappelli and Pistaferri (2010). Recent empirical studies include Souleles (1999); Hsieh (2003); Shapiro and Slemrod (2003a, 2009); Johnson, Parker, and Souleles (2006); Parker et al. (2013); and Jappelli and Pistaferri (2014).

straightforward. Finally, unlike most transfer programs that have been studied, the veterans' bonus was paid during the recovery from a financial crisis. This makes it of particular interest and relevance today.

My results pose a puzzle for the traditional view that the MPC from very large payments is likely to be small. While a definitive explanation is beyond the scope of this paper, I argue that characteristics of the 1936 economy, some unique to the time, some generally present after deep recessions, made the MPC high despite the size of the transfer payment.

This paper also adds to the literature on US recovery from the Great Depression. Some authors argue that output growth after 1933 reflected the disappearance of temporary negative shocks (DeLong and Summers 1988) or the economy's strong self-correcting mechanisms (Friedman and Schwartz 1963, Bernanke and Parkinson 1989). Other authors dispute that there was anything natural or inevitable about rapid recovery post-1933. Eichengreen and Sachs (1985) do not focus on the US experience, but their finding that across countries devaluation was positively correlated with recovery suggests that monetary factors were important. Romer (1992) forcefully articulates the case for a monetary explanation of US recovery. She finds that "rapid rates of growth of real output in the mid- and late 1930s were largely due to conventional aggregate-demand stimulus, primarily in the form of monetary expansion" (Romer, p. 757). Romer complements earlier work by Temin and Wigmore (1990) who argue that the departure of the United States from the Gold Standard in April 1933 was a regime change that directly led to rapid recovery, in part by raising prices for agricultural products. Eggertsson (2008) formalizes this argument. While I do not dispute the importance of self-correcting mechanisms and of monetary policy for the recovery, this paper suggests that a full explanation may also include a role for fiscal policy in 1936.

I proceed in the next section by providing background on the veterans' bonus. Section II reports results from the 1935–1936 Consumer Expenditure (CE) Survey. Section III reports results from cross-state and cross-city regressions. Section IV compares these findings to tabulations from a large survey of veterans conducted by the American Legion and to narrative evidence. Section V considers reasons why the MPC from the bonus was so high. Section VI discusses the possible aggregate implications of my empirical results. Section VII concludes.

I. Background on the Veterans' Bonus

A. *Road to Passage*

Motivated by a sense that they had been underpaid, veterans began agitating for additional payments after the end of World War I.[5] In the early 1920s, Congress took up numerous bonus bills. None became law, in part because of disagreements over how to finance the payments. This issue was resolved in 1924, when Congress passed a bill granting veterans a bonus that would only be paid in 1945, thus eliminating any immediate effects on the budget. The law promised World War I veterans

[5] Unless otherwise noted, the following paragraphs draw on facts and figures from Dickson and Allen (2004).

payments in 1945 of approximately $3 for each day they had served in the army in the United States and $4 for each day served abroad.[6] Because the bonus was formally an insurance policy, a veteran's bonus was both de jure and de facto non-tradable.

The Great Depression led veterans to demand earlier payment of the bonus. Congress partially acquiesced in February 1931, when it raised the amount that a veteran could borrow against the face value of his bonus from 22.5 to 50 percent (Daniels 1971). The loans were in effect early, discounted bonus payments since they did not need to be paid back; rather a veteran could choose to have the amount of the loan plus 4.5 percent per-annum interest deducted from the amount due to him in 1945 (Veterans' Administration 1931).[7]

Despite their ability to take loans, veterans continued to demand immediate cash payment of the entire, nondiscounted value of their bonus. From May to July 1932, thousands of veterans camped in Washington, DC and lobbied for immediate payment. In 1932, they were unsuccessful, but by 1936 the political climate had shifted in the veterans' favor (Hausman 2013). The house and senate passed the Bonus bill on January 10 and January 20, 1936. Roosevelt vetoed the bill on balanced budget grounds, but Congress overrode the veto, and the bill became law on January 27.

B. *Payment of the Bonus*

In June 1936, veterans received the entire face value of their bonus, less any loans they had taken. Thus they received a payment in 1936 equal to what they had been supposed to receive in 1945. Importantly, interest accrued after October 1931 on loans taken against the bonus was forgiven (Veterans' Administration 1936). Table 1 illustrates the effect of the law on a hypothetical veteran due $1,000 in 1945 who took a loan of $500, the maximum allowable, in 1931. Such a veteran—who would have been typical—gained $491 ($1,000–$500–1931 interest) of disposable income in 1936. The increment to the present value of a veteran's total lifetime income was equal to the value of the loan interest forgiven plus the value of receiving the face value of the bonus in 1936 rather than in 1945. Assuming a discount rate of 4 percent, in this hypothetical case the change in present value total income was $262.

[6] Confusingly, the law is often described as granting veterans $1 for each day served in the United States and $1.25 for each day served abroad. These amounts were, however, arbitrarily increased by 25 percent and then accrued interest for 20 years. Hence the values at maturity were approximately $3 and $4 per day served. The precise features of the bill are described in the 1936 *Annual Report of the Administrator of Veterans' Affairs* (Veterans' Administration, pp. 21–22):

> *Essentially, the act provided a basic service credit of $1 a day for each day's service in the United States and $1.25 a day for each day's service overseas, with a maximum credit of $625 for overseas service and $500 for home service. To those veterans who had basic credits of $50 or less the act provided that the payments be made in cash …. [T]o the basic credit of $50 or more there was added 25 percent, and this sum (the basic service credit plus 25 percent) was used as a single net premium to purchase for the veteran at his then attained age a paid-up endowment certificate maturing upon the death of the veteran or at the end of the 20-year period. While the amount of insurance procurable by a fixed credit varied according to the age of the insured, the face value of the adjusted-service certificate in the average case was approximately two and one-half times the net service credit.*

[7] Many veterans took advantage of these loans: the government dispensed 2 million loans worth 1 percent of GDP between March and May 1931 (Veterans' Administration 1931; Cone 1940). For more discussion of these loans and their effects, see Joshua Hausman, "Joshua Hausman on Historical Evidence for What Federal Lines of Credit Would Do," *Confessions of a Supply-Side Liberal* (blog), August 23, 2012, http://blog.supplysideliberal.com/post/30037326807/joshua-hausman-on-historical-evidence-for-what.

TABLE 1—EXAMPLE OF THE 1936 BONUS' EFFECT

	Pre-January 27, 1936 law	Post-January 27, 1936 law
Face value of adjusted service certificate	$1,000.00	$1,000.00
Loan taken in April 1931	$500.00	$500.00
Interest accrued on loan 1931–1936	$87.50	$87.50
Interest accrued on loan 1931–1945	$245.00	—
Interest forgiven	$ —	$78.75
Amount of bonus available in cash in 1936	$ —	$491.25
Amount of bonus available in cash in 1945	$255.00	$ 628.40
Change in 1936 disposable income		$ 491.25
Change in lifetime income (discount rate = 4 percent)		$ 262.34

Notes: Column 1 shows the financial situation of a hypothetical veteran under the pre January 1936 bonus legislation. This hypothetical veteran was due $1,000 in 1945 but took the maximum possible loan in 1931 ($500). From 1931 to 1945, $245 of interest would accrue on the loan. Hence he would receive $255 in 1945. After the January 1936 bonus legislation (column 2), the veteran could receive $491 in cash in 1936: the initial amount of his bonus ($1,000) minus his loan ($500) minus the small amount of interest not forgiven. Alternatively he could leave all or some portion of this sum with the government where it would earn 3 percent interest until 1945. If he left his entire bonus with the government, he would receive $628 in 1945.

In keeping with the convention in the literature estimating MPCs, this paper measures the MPC from the veterans bonus as $\frac{\Delta C}{\Delta Y}$ where ΔY is the amount paid to veterans by the US government in 1936 (net of outstanding loans). ΔY is *not* the change in total lifetime income. In the above hypothetical example of a veteran receiving $491 in 1936, if the veteran spent $300 of this payment, the MPC would be $300/491 = 0.61$.

In June 1936, the government paid 1.76 billion to 3.2 million veterans.[8] The 1.76 billion was equal to the face value of veterans' adjusted service certificates ($3.5 billion) minus the value of outstanding loans. Veterans received cashable bonds whose average value was $\frac{1.76}{0.0032} = \$547$. Veterans could choose to hold onto their bonds rather than cash them; they were issued bonds in $50 denominations and could cash as many or as few of them as they desired (Veterans' Administration 1936). The bonus began to be distributed on June 15, 1936, with the majority of the disbursement happening in the initial few days. By midnight on June 15, the Treasury Department had mailed $1.6 billion (91 percent of the bonus) to 2.9 million veterans (91 percent of veterans) (US Department of the Treasury 1937). And it appears that the post office delivered most of these payments on June 15, 1936 itself.[9]

After receiving their cashable bonds, veterans' quickly took advantage of the option to convert them to cash. Out of the $1.76 billion paid to veterans through June 30, 1936, of which all except odd amounts was issued in form of bonds, $1.2 billion was redeemed for cash in June and July 1936. A further 200 million was redeemed in late summer and fall (Cone 1940).[10] Thus 80 percent of the dollar value of the bonds was cashed in 1936. This in itself suggests a high MPC from the bonus: if veterans wished to save, they were in general better off not cashing their bonds,

[8] This was the total amount "certified as payable after deducting outstanding liens" by June 30, 1936 according to the Veterans' Administration (1936, p. 24).

[9] *New York Times*, "Delivery of Bonus to 3,518,000 in U.S. Will Begin Today." June 15, 1936, p. 1 and *Chicago Tribune*, "3 1/2 Millions Await Bonus" June 14, 1936, p. 1.

[10] NIPA table 3.1 shows a $1.1 billion increase in federal transfer payments in 1936, slightly less than the $1.4 billion of adjusted service certificates cashed by veterans. The implication is that other federal transfer payments fell by $300 million in 1936, perhaps because of improving economic conditions.

Table 2—The Magnitude of the Bonus

	1936	2012	2012 bonus equivalent
Per capita annual income	$530	$39,409	$40,673
Average annual wage of federal emergency workers	$595	—	—
Average hourly earnings in manufacturing	$0.62	$19.08	$16,853
CPI (Index, 1936 = 100)	100	1,656	$ 9,053
Nominal house prices (Index, 1936 = 100)	100	2,506	$13,702
Price of cheapest Ford	$510	$14,000	$15,009

Note: The third column equals the average 1936 bonus amount, $547, times the ratio of the second to the first column $\left(\text{e.g., in the first row, } \$40{,}673 = \$547 \times \frac{\$39{,}409}{\$530}\right)$.

Sources: Per capita income: NIPA table 2.1; annual wage of federal emergency workers: Darby (1976); average hourly earnings in manufacturing (production workers only): Sayre (1940) and FRED series AHEMAN; CPI: FRED series CPIAUCNS; house prices: Robert Shiller, http://www.econ.yale.edu/~shiller/data.htm; Ford price: *Automotive Industries*, "Ford Prices." November 14, 1936, p. 666, and http://www.ford.com.

since the 3 percent interest paid on bonds was above the legal maximum interest rate paid on savings accounts at Federal Reserve member banks (Telser 2003).[11] The speed with which veterans cashed their bonds also suggests that spending from the bonus occurred soon after disbursement.

The bonus amount received by a typical veteran, $547, was an enormous sum. Table 2 provides some metrics to interpret the magnitude. The first three rows provide measures of income and wages in 1936. The typical bonus was slightly greater than annual per capita income and roughly equal to average annual wages on federal relief projects. It was the equivalent of average earnings from 884 hours or 22 weeks of work in manufacturing. The second column displays the same measures of income and wages in 2012, and the third column provides the 2012 equivalent of the 1936 payment, adjusted by the ratio of the second to the first column. The average bonus, $547, was equal to $\frac{\$547}{\$530} = 103$ percent of 1936 per capita income. Per capita income in 2012 was $39,409, so a similarly-sized bonus today would be $1.03 \times \$39,409 = \$40,673$. Since 1936, manufacturing wages have risen much less than per capita income, so the analogous 2012 payment relative to manufacturing wages is only about $17,000 (the ratio of the 1936 bonus to 1936 hourly earnings, 884, times the average hourly earnings in manufacturing in 2012, $19).

In addition to considering the magnitude of the bonus relative to income, we can measure the 2012 equivalent in terms of purchasing power. Measured by the CPI, 2012 prices were roughly 16.5 times higher than those in 1936 (Table 2, row 4). So it would require a 16.5 times larger bonus (roughly $9,000) to purchase the same basket of goods today. The last two rows of the table repeat this exercise for Robert Shiller's house price index and the price of the cheapest Ford car.

C. *Who Were World War I Veterans?*

To understand the effects of the veterans' bonus itself, what matters is the MPC of veterans. But to understand the implications of the veterans' bonus for the effect

[11] Of course, veterans may also have wished to cash their bonds in order to pay off higher interest rate debt. Or one might wonder if veterans distrusted the government, and hence wanted their bonus in cash. Though possible, I am aware of no narrative or other evidence suggesting such distrust.

of fiscal transfers more generally, it is important to know how similar or different World War I veterans were to the population as a whole. In two obvious ways, veterans were different. Veterans were all male, and they were concentrated in a narrow age range. Among men of service age, however, World War I veterans were similar to the general population. Table 3 uses 1930 census data to compare veterans and nonveterans along seven dimensions.[12] I compare veteran and nonveteran men age 28–45 in 1930 (men younger than 28 or older than 45 had less than a 4 percent chance of being a veteran), and in two smaller age ranges, 28–36 and 37–45. Along most dimensions, veterans and nonveterans were similar. In particular, nearly identical proportions of veterans and nonveterans were employed in 1930. The largest gap between the groups is in the share that was black. This reflects the difficulty blacks had in volunteering for the army in the first world war, despite their eligibility for the draft (Kennedy 1980). That more veterans than nonveterans lived in urban areas is likely a consequence of the fact that the South had fewer veterans per capita than the rest of the country (see Section III). Likely reflecting the underrepresentation of blacks and men from the South among veterans, veterans earned somewhat higher income and paid higher monthly rent than nonveterans. Overall, however, it is the similarity between veterans and nonveterans that is striking. Their veteran status excepted, veterans looked much like the rest of the population.

II. Evidence from the 1935–1936 Study of Consumer Purchases

A. *The 1935–1936 Study of Consumer Purchases*

One would like direct information about how the spending patterns of individuals and households who received the bonus compared to the patterns of those who did not. Although the 1935–1936 Study of Consumer Purchases did not record veteran status, one can use it to infer the effect of the bonus on household spending. To do so, I use information from the 1930 census to estimate the probability that a household in the consumption survey included a veteran.

In 1935 and 1936 the Works Progress Administration financed and provided personnel for a detailed survey of household consumption (United States Department of Labor et al. 2009; Natural Resources Committee 1939). The Bureau of Labor Statistics was charged with interviewing urban households. Likewise, the Department of Agriculture interviewed households in villages and on farms. In total, 26,000 urban and 35,000 rural households provided a detailed inventory of their expenditures over a 12 month period. The Inter-University Consortium for Political and Social Research (ICPSR) digitized a random sample of 3,100 urban and 3,034

[12] Throughout this paper, I use data from the 1930 rather than the 1940 census for three reasons. First, in the 1930 census all respondents reported on their veteran status (Bureau of the Census 1933). By contrast, in 1940, veteran status was a sample line item reported by only 5 percent of respondents (Bureau of the Census 1943). Second, whereas IPUMS provides a 5 percent sample from the 1930 census, they provide only a 1 percent sample from the 1940 census (Ruggles et al. 2010). Thus the 1930 census provides 100 times more veteran observations than the 1940 census. Finally, whereas the 1930 census specifically identified World War I veterans, the 1940 census only identified whether an individual was a member of a group including: (i) veterans of any war; (ii) spouses of veterans of any war; and (iii) children of veterans of any war.

Table 3—Comparison of Veterans and Nonveterans

	Age 28–45 in 1930		Age 28–36		Age 37–45	
	Veterans	Nonveterans	Veterans	Nonveterans	Veterans	Nonveterans
Married (percent)	77.7	78.6	79.3	73.6	75.2	83.4
Black (percent)	6.7	9.8	6.5	10.6	7.1	9.0
Urban (percent)	67.8	60.7	68.3	60.8	67.0	60.6
Employed (percent)	90.5	89.9	91.2	89.8	89.2	90.0
Annual income (1930)	$1,917	$1,708	$1,919	$1,681	$1,913	$1,734
Own house (percent)	42.3	41.7	40.1	35.7	46.0	47.4
Monthly rent (1930)	$38	$34	$38	$33	$39	$35

Notes: Comparison is for men only. Income is the occupational income score from the IPUMS 5 percent sample of the 1930 census (Ruggles et al. 2010). It measures the 1950 income of an individuals' occupation. I convert this to 1930 dollars using the CPI. Own house is a dummy variable for whether or not an individual owned his house. Monthly rent is the monthly contract rent if an individual did not own his house.

Source: IPUMS 5 percent sample of the 1930 census (Ruggles et al. 2010)

rural responses.[13] Unfortunately, fewer than 400 of the digitized rural responses include data on consumption after the bonus was paid, providing insufficient variation to identify the bonus' effects. Hence, in my analysis I use only the urban sample.[14]

Survey Sample.—The Bureau of Labor Statistics selected households for the urban expenditure survey with a three-step procedure. First, in each of six geographic regions,[15] one or two large cities, two or three mid-sized cities, and several small cities were selected (Natural Resources Committee 1939).[16] Then a random sample was taken of all households in the municipality, and interviewers obtained detailed income information from all US-born white families for which the husband and wife were present during the schedule year. Black families were also included in New York, Columbus, Ohio, and the South. Households from this income sample were selected for the expenditure survey if they met several additional criteria (BLS 1941a). The criteria most likely to influence my results are: (i) only married couples and families were surveyed; (ii) households must not have received any relief payments during the schedule year; and (iii) white families must have had an income

[13] See http://www.icpsr.umich.edu/icpsrweb/DSDR/studies/8908 (accessed September 2, 2014). Although the ICPSR urban dataset has 3,100 observations, the dataset I use has only 2,745 observations. I drop 34 households because they may have multiple veterans, 1 household because no city is specified, 37 households because either the start or end date for the schedule year is missing, 3 households because the schedule period is under a year, 20 households because the schedule period is over a year, 18 households because there is a discrepancy between the schedule year listed on the household's income schedule and the schedule year listed on the expenditure schedule, 2 households because the husband is listed as being under age 16, 11 households because the husband's age is missing, and 229 households because the husband's race is unknown.

[14] In addition to the lack of pre- to post bonus variation in the rural sample, there are other reasons to not pool the urban and rural samples together for the analysis. Urban and rural households filled out different schedules, complicating expenditure comparisons. And the urban and rural surveys were conducted by different agencies (the Department of Labor and the Department of Agriculture) on somewhat different timelines, also complicating comparisons. Furthermore, extreme drought and heat around the time of the bonus payments likely affected the rural responses.

[15] New England, East Central, West Central, Southeast, Rocky Mountain, and Pacific Northwest.

[16] Some of the small cities were surveyed by the Department of Agriculture and are not included in the ICPSR urban sample. I exclude these from my analysis.

of at least $500 in large cities and $250 in small cities.[17] A priori it is not obvious in which direction the composition of the urban household survey might affect my measurement of the MPC. I will return to this issue when discussing my results.

Survey Procedure.—Households were interviewed over the course of 1936. In most cases, households were interviewed twice: once to obtain income information and a second time to obtain expenditure information. The interviews were typically about two months apart, although in some cases both income and expenditure information were obtained in the same interview. Households generally reported on consumer expenditures over a 12-month period ending at the end of the month prior to the initial interview. Regardless of when they were first interviewed, however, households could choose to instead report income and expenditure for calendar year 1935.

The survey appears to have been carefully done. For instance, according to the Natural Resources Committee (1939, p. 108):

> [*A*] *system of check interviewing was adopted, under the guidance of the regional office staffs. In general 1 out of every 8 or 10 families visited by each agent was revisited by a supervisor, editor, or squad leader, to check enough of the entries on the schedule to prove that the agent had obtained the information from the family and had reported it correctly.*

Online Appendix A contains an example of a completed expenditure schedule, and online Appendix B provides a detailed description of how I compute measures of consumption aggregates from the survey responses. Table 4 shows summary statistics for major categories of consumption.

Average household consumption expenditure was $1,870, roughly four times per capita income. This reflects the fact that households have multiple members. On average, food made up the largest share of consumption with housing a distant second. Note that housing is housing *consumption*, e.g., rent and repairs, not housing investment. It excludes spending on new home construction, structural additions or mortgage payments. Excluding housing investment from my total consumption measure aligns with the NIPA definition of consumption and the modern CE Survey.[18]

Online Appendix Tables 1 and 2 provide further summary statistics on the consumption survey sample. Online Appendix Table 1 shows the number of observations in each city and of these the number of households that likely included a World War I veteran as imputed using the procedure described below. Online Appendix Table 2 shows the timing variation in the survey, the number of households reporting on consumption for a 12-month period ending in each month from December 1935 to December 1936. It shows that of the 2,745 households in the sample, 766 reported on consumption for a 12-month period ending after the bonus was paid. Of these

[17] For this purpose, large cities were defined as Atlanta, GA, Chicago, IL, Columbus, OH, Denver, CO, New York, NY, Omaha, NB—Council Bluffs, IA, Portland, OR, and Providence, RI. In New York, black families making less than $500 were excluded, and in Columbus black families making less than $250 were excluded. For a more detailed description of the criteria for inclusion in the survey see BLS (1941a, pp. 373–375).

[18] Note that unlike the NIPA definition of consumption, the definition of housing consumption used here does not include the service flow from owner-occupied housing. For more on consumption shares by category in the 1935–1936 survey, see Costa (1999).

Table 4—Consumption Category Summary Statistics

Category	Mean ($)	Standard deviation ($)
Total expenditure	1,870	1,217
Auto purchases and operations	183	263
Housing	232	267
Furniture and equipment	55	102
Clothing	205	195
Recreation	73	136
Food	583	278

Source: ICPSR study 8908. For details on these consumption categories, see online Appendix B.

766, roughly 120–125 are likely to have included a World War I veteran. This table also shows that most of the post-bonus households in the survey reported on consumption for a 12-month period ending in the third quarter of 1936; there is little information in the survey on household consumption beyond September 1936. Since the bonus was paid in June, this means that I measure the short-run spending response to the bonus.

B. *Specification*

An ideal survey for this paper's purpose would have asked each household if they received a bonus, and if so, how much it was. The actual survey did not do this. A second best would be if the survey had asked if the husband of the family was a World War I veteran, an excellent proxy for whether a bonus payment was received. The actual survey did not do this either. But I am nonetheless able to recover a consistent estimate of veterans' MPC.

I describe my specification in two steps. First, I show how one could estimate spending from the bonus if veteran status were recorded in the household consumption survey. Then I describe how this specification can be modified to replace unobserved veteran status with the probability of being a veteran.

If one knew who was a veteran in the household consumption survey, one could estimate

$$C_i = \mathbf{Z}_i'\boldsymbol{\beta}_1 + \beta_2 V_i + \beta_3 P_i + \beta_4 V_i P_i + \varepsilon_i, \tag{1}$$

where C_i is consumption of household i, V_i is a dummy variable equal to 1 if the household includes a World War I veteran, and $\mathbf{Z}_i$ is a $k \times 1$ vector of covariates (e.g., age, geography, and race). P_i is a dummy variable equal to 1 if a household reported on consumption for a period including time after the bonus was paid. In other words, if a household reported on consumption for the 12 months ending in May 1936, P_i would equal 0 (since the bonus was paid on June 15), whereas if a household reported on consumption for the 12 months ending June 30, 1936 or later, P_i would equal 1.

This is a standard differences in differences regression with β_4 measuring the difference between the change in veteran consumption pre to post bonus and the change in nonveteran consumption pre to post bonus. The crucial identifying assumption is that absent the bonus, the change in veteran and nonveteran consumption pre-to-post

bonus would have been the same. Given this identifying assumption, an estimate of β_4 provides an estimate of veterans' propensity to consume out of the bonus.

A complication is that unlike a typical difference-in-difference exercise, the treatment—the bonus—affected nonveterans as well as veterans. Insofar as the bonus led to higher aggregate income, it likely induced nonveterans as well as veterans to increase consumption. But insofar as payment of the bonus led to expectations of higher future taxes, it could have induced nonveterans to cut consumption. Neither of these effects is problematic for my identification, provided that each effect was the same for veterans and nonveterans. In this case, these economy-wide spillover effects will be captured by β_3, and β_4 will still measure actual first-round spending from the bonus.

More problematic is if—as seems quite possible—there were *local* multiplier effects from the bonus, such that the extent of spillover effects on consumption depended on the share of veterans living in the local area. This would lead to omitted variable bias in equation (1). One can control for these possible local multiplier effects by adding a term to equation (1),

$$C_i = \mathbf{Z}_i'\beta_1 + \beta_2 V_i + \beta_3 P_i + \beta_4 V_i P_i + \beta_5 P_i \pi_s + \varepsilon_i, \tag{2}$$

where π_s is the share of state's population made up of veterans. I report results for this specification in the robustness section below.

So far, this discussion has assumed that veteran status is observable in the household consumption survey, which it was not. Even so, it is possible to identify β_4. To do this, I proxy for veteran status with a measure of the probability that the husband in a household was a veteran. I take advantage of the fact that the household survey includes information on age, race, and location. Since the 1930 census asked everyone if they were a World War I veteran, I can use the IPUMS 5 percent sample from this census to estimate the probability that a household contains a veteran conditional on age, race, and location. Of course, it is crucial that this first stage have sufficient predictive power for veteran status. As we shall see below, fortunately demographic variables do a good job of predicting veteran status.

Thus I estimate a first stage

$$V_j = \mathbf{Z}_j'\gamma + \eta_j \tag{3}$$

on 1930 census data. As above V_j is veteran status, and $\mathbf{Z}_j$ are covariates. The subscript j indicates that this equation is estimated on a different dataset than equation (1). Substituting the first stage (equation (3)) into the second stage (equation (1)) yields

$$C_i = \mathbf{Z}_i'\beta_1 + \beta_2(\mathbf{Z}_i'\gamma + \eta_i) + \beta_3 P_i + \beta_4(\mathbf{Z}_i'\gamma + \eta_i)P_i + \varepsilon_i. \tag{4}$$

Online Appendix C provides a formal econometric proof that two stage least squares produces consistent estimates of β_4, conditional on reasonable identifying

assumptions. In addition to certain technical and standard assumptions presented in the online Appendix, three conditions for identification merit discussion here.

First, the variables used to predict veteran status (the $\mathbf{Z}_i$) cannot be correlated with the pre to post bonus change in consumption except through their correlation with veteran status. This is the parallel trends assumption that is always necessary for identification in difference-in-difference models. It is made more plausible by the short time horizon of interest. It is implausible, for instance, that whites and blacks were on the same long-run consumption trend in the 1930s, but it is quite plausible that, absent the bonus, the change in their consumption between June and September 1936 would have been the same. I present graphical evidence as well as robustness checks suggesting that violations of this assumption do not drive my results.

A second important assumption is that whether or not households were interviewed after the bonus was paid is independent of the variables used to predict veteran status (the $\mathbf{Z}_i$) as well as the unobserved factors determining veteran status (the η_i). The formal condition is $E[P_i|\mathbf{Z}_i, \eta_i] = E[P_i]$. Two pieces of evidence suggest that this assumption is met. First, nothing in the description of the survey suggests a systematic effort to survey households of a certain age, race, or veteran status before or after the bonus payment. Second, within my sample there is little evidence of correlation between the P_i and $\mathbf{Z}_i$. The correlation between P_i and age is 2.4 percent, between P_i and race, 2.9 percent (neither is statistically significant). Online Appendix Table 1 suggests a more significant correlation between P_i and location, since there was significant variation in the share of households in a city surveyed before bonus payment. For example, in Chicago 92 percent of the sample reported on consumption before the bonus was paid, whereas in Springfield, MO only 38 percent did. Even so, a full set of state fixed effects explains less than 7 percent of the variation in P_i. And I show below that controlling for the interaction of state and P_i has little effect on my results.

The third crucial assumption is that the first stage relationship between $\mathbf{Z}_i$ and veteran status be the same in the 1930 census and in the 1936 household consumption survey. To satisfy this, the first stage estimation is done on a sample from the 1930 census that approximates the household survey as closely as possible. I use the IPUMS 5 percent sample from the 1930 census for all US-born men married to US-born women in the 33 cities included in the urban portion of the household survey. Migration between 1930 and 1936 means that location is the most obvious candidate to violate this assumption. It is much less clear how the relationship between age or race and veteran status would have changed in the six years between 1930 and 1936. Thus in robustness checks below I avoid using location to predict veteran status.

Since $\mathbf{Z}_i'\hat{\gamma}$, the probability of being a veteran, is a generated regressor, the usual formulas will underestimate standard errors. Further complicating the calculation of correct standard errors, the household survey data are from a stratified sample. Each primary sampling unit—a city—was drawn from 15 region-city-size strata. To avoid problems with strata that contain only one primary sampling unit, I collapse the 15 strata to 9. Online Appendix D provides details. To account for the generated regressor problem, possible correlation of standard errors within cities (clustering), and the stratified survey design, I compute block bootstrap standard errors.

C. *Results*

In the first stage regression I estimate a linear probability model of World War I veteran status on a set of age, race, and state fixed effects. Specifically, I estimate

$$(5)\quad V_j = \sum_{h=1}^{3} \beta_h \mathbb{1}(g_j = g_h) + \sum_{k=1}^{17} \gamma_k \mathbb{1}(s_j = s_k) + \sum_{l=1}^{17} \alpha_l \mathbb{1}(g_j = 2)\mathbb{1}(s_j = s_l)$$
$$+ \sum_{m=1}^{3} \theta_m a_j^m + \sum_{n=1}^{3} \lambda_n \mathbb{1}(g_j = 2) a_j^n + \zeta r_j + \eta \mathbb{1}(g_j = 2) \cdot r_j + \mu_j.$$

V is World War I veteran status; g is a generation indicator variable for whether a man was younger than 28, between 28 and 45, or older than 45 in 1930 (men younger than 28 or older than 45 had less than a 4 percent chance of being a veteran); s is an indicator variable for state;[19] a equals age, and r is an indicator variable for race. $\mathbb{1}$ denotes the indicator function. The predicted probabilities of being a veteran are fairly insensitive to the exact specification used. The particular specification in (5) is attractive because while reasonably parsimonious, it results in separate slope coefficients for men age 28 to 45, the age range in which men had a reasonable chance of having served in the war.

The first stage estimation is done on a sample from the 1930 census that approximates the household survey as closely as possible. Thus I use the IPUMS 5 percent sample from the 1930 census for all US-born men married to US-born women in the 33 cities included in the urban portion of the household survey. This provides me with 64,148 observations, enough to precisely estimate the probability of being a veteran as a function of age, race, and location. The first stage produces large variation in the probability that the husband in the household was a veteran.[20] Figure 1 gives an example of how this probability varies with age, race and location.

There is no way to directly check the accuracy of the first stage. But following Card and McCall (1996), I check the reasonableness of this procedure by using it to predict a variable observed both in the 1930 census and in the household consumption survey. I use the exact specification in equation (5) to predict the age of a man's wife in the household consumption survey. This provides a sensible test since like veteran status, age is invariant to the macroeconomic upheaval that occurred between 1930 and 1936. The correlation between predicted and actual age is 0.92; and the mean of the predicted values for wife age is 33.4 compared to an actual mean of 32.8. This suggests that this procedure does a good job of using

[19] In three cases I combine states for the purposes of this regression: (i) Since the household consumption survey considers Omaha, Nebraska and Council Bluffs, Iowa as one city, I use one fixed effect for Iowa and Nebraska. (ii) Since the only city in the sample in North Carolina, Gastonia, had a 1930 population of just 17,000, I combine North Carolina with South Carolina. For the same reason, I combine Maine with Massachusetts, since the only city in the sample in Maine, Westbrook, had a population of just 10,800. This avoids problems in calculating veteran probability when the 5 percent census sample has only a tiny number of veterans in the city.

[20] The first stage R^2 is 0.21. Most of the variation is explained by age rather than by race or location. For the first stage estimates and standard errors, see online Appendix E.

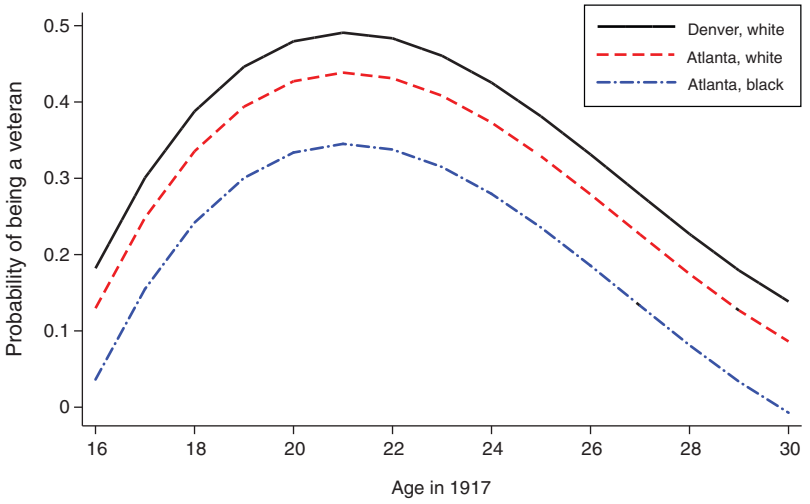

Figure 1. Variation in Probability Man is a Veteran

Source: IPUMS 5 percent sample of the 1930 census (Ruggles et al. 2010)

demographic characteristics in the 1930 census to predict outcomes in the 1936 consumption survey.[21]

In the second step of my estimation, I estimate equation (4), in which consumption is the dependent variable. Before turning to these second stage regression results, a graph provides a useful way of both seeing the relevant variation in the data, and of gauging the plausibility of the parallel trends assumption. I construct a graph as follows. First, I divide the household survey into upper and lower halves of veteran probability. In the lower half, only about 1 percent of households are likely to include a veteran; in the upper half, nearly a third do. I then graph average consumption expenditure in each of these groups for survey years ending in December 1935, between January and March 1936, between April and May 1936, between June and July 1936, and between August and December 1936.[22] Thus the first three time periods show consumption expenditures from households unaffected by the bonus, while the latter two time periods show the consumption expenditure of households who, if they included a veteran, received the bonus.

Figure 2 panel A shows results. Reassuringly, prior to the bonus payment, there is no evidence of different trends between the two groups. After bonus payment, average spending of households in the upper half of the veteran probability distribution

[21] Other variables that can be compared across the census and consumption survey are age of eldest child, a 0/1 dummy for radio ownership, a 0/1 dummy for home ownership, and the amount of monthly rent paid if a household did not own its home. Online Appendix Table 6 shows several different metrics of how the baseline first stage (equation (5)) (as well as alternative stage specifications) do at predicting these variables. Forecasting performance is good for age of eldest child, but worse for the economic variables which—unlike veteran status—were likely affected by the economic disruption of the Great Depression.

[22] These time divisions were chosen so as to have a roughly equal number of observations in each period. See online Appendix Table 2 for data on the number of households reporting on consumption for 12-month periods ending in each month from December 1935 to December 1936.

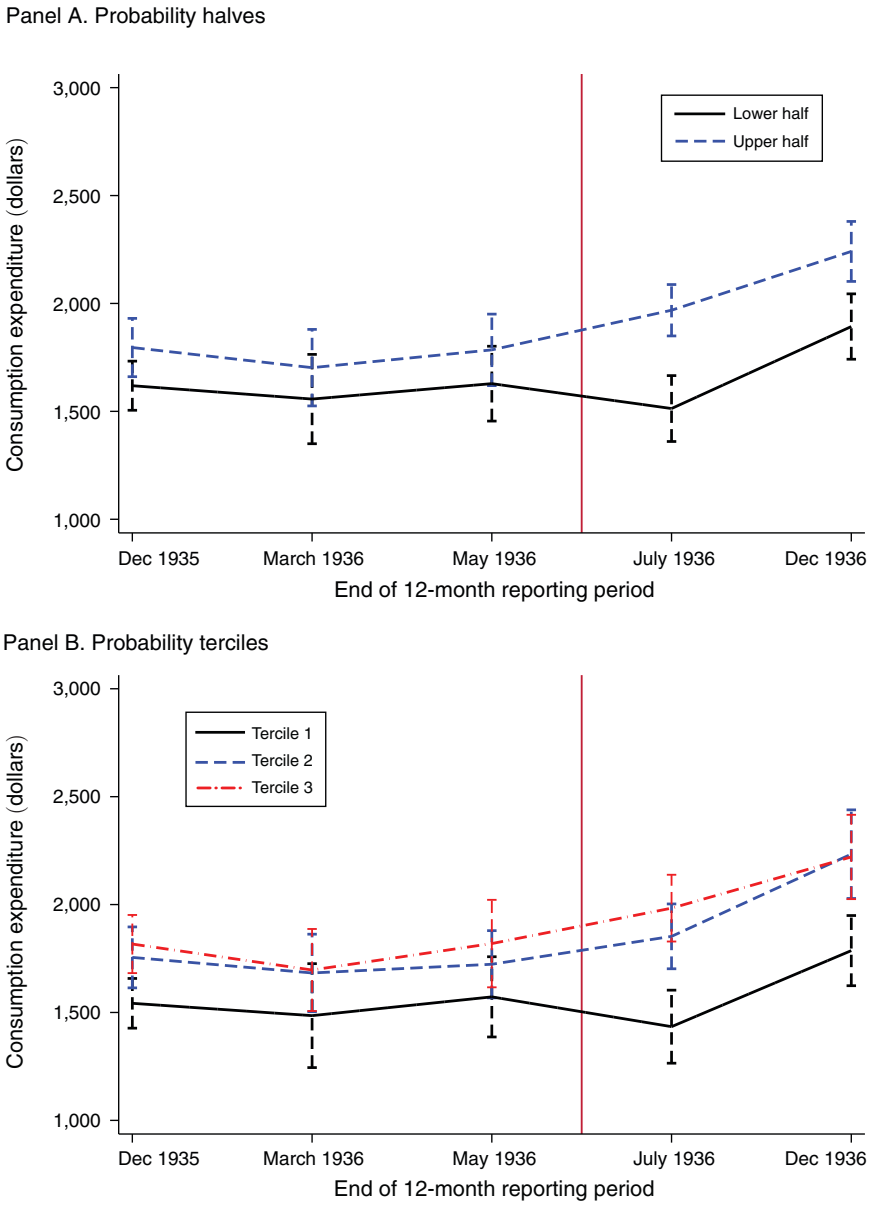

FIGURE 2. CONSUMPTION TRENDS

Notes: Each point is the mean consumption expenditure of households reporting on consumption for a schedule year ending in a given time period. The probability that a household includes a veteran averages 1.2 percent in the lower half, and 31.1 percent in the upper half; it averages 0.2 percent in tercile 1, 8.8 percent in tercile 2, and 39.1 percent in tercile 3. The solid vertical line denotes the date of bonus payment (June 15, 1936). Dashed vertical lines show two standard error confidence bands computed from 1,000 bootstrap replications. As in the baseline specification in the paper, all graphs exclude outliers, e.g., households with consumption expenditure above $5,000.

Sources: See text.

rises more than that of households in the lower half. The rise (with some delay) of spending by households in the lower half of the veteran probability distribution is consistent with the overall rise in consumption in the United States in 1936.

Table 5—Total Expenditure and Saving Regressions

	Total C (1)	Total C (2)	Insurance policies settled (3)	Gifts received (4)
Post bonus dummy	264.1***	198.2***	−5.590	0.0742
	(70.52)	(43.17)	(4.292)	(6.855)
Interaction	647.2*	403.1**	96.0***	152.4***
	(379.4)	(169.7)	(22.88)	(46.45)
Omit if expenditure > $5,000	No	Yes	Yes	Yes
Observations	2,745	2,681	2,681	2,339
R^2	0.152	0.186	0.034	0.048

Notes: All columns include the full set of second stage controls for age, race, and geography. See the text for details. Bootstrap standard errors clustered at the city level in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

To provide further graphical evidence, I divide the sample into veteran probability terciles rather than veteran probability halves. Results are shown in Figure 2, panel B. Again, there is no evidence of different pretrends among the veteran probability terciles. And after the bonus payment, average consumption spending in the top two terciles rises relative to that in the bottom tercile. But there is no obvious difference in the consumption paths of the top two terciles, despite the presence of many more veterans in the top tercile. As reflected in the large confidence bounds, however, the small sample size limits how much can be inferred from a comparison of these two terciles.

To see the quantitative implication of the variation in Figure 2, Table 5 shows results of estimating equation (4) for total consumption expenditure. The specification in column 1 uses all observations. Column 2, my preferred specification, excludes households in the CE survey with total spending of more than $5,000. These households are large outliers in the regression: a robust regression assigns them approximately zero weight. In each specification, two coefficients are shown: that on the post bonus dummy, and that on the interaction term between the post bonus dummy and the probability of being a veteran. No results are shown for the probability of being a veteran itself, since this is a linear combination of the controls. Consistent with Figure 2, the coefficient on the post bonus dummy is large and statistically significant, suggesting that nonveteran as well as veteran spending changed pre to post bonus. As noted above, this is in fact exactly what one would expect given the upward trend in aggregate consumption over the period. Furthermore, if nonveterans also benefited from the bonus, i.e., if there was a multiplier, than even absent trend consumption growth one would expect to see an increase in their consumption after the bonus payment.[23]

[23] The magnitude of the coefficient on the post bonus dummy in column 2, 198, implies approximately a 10–15 percent increase in nonveteran consumption pre to post bonus. While not directly comparable, this is roughly in line with the NIPA data which show aggregate consumption rising by 10.2 percent in 1936 (NIPA table 1.1.1).

To interpret the coefficient on the interaction, recall that average household expenditure in the sample was \$1,870 (Table 4), and that the average bonus received by a World War I veteran was \$547. If it were the case that the average bonus paid to veterans in the sample was the same as that in the population as a whole, then the coefficient in column 2 would imply a MPC of 0.74 (\$403/\$547). However, the average bonus in the urban household survey population may have differed from that in the population as a whole.

Three factors were important in determining what size bonus a veteran received: (i) how many days a veteran served in the army; (ii) whether those days were served in the United States or abroad; and (iii) what size loan, if any, the veteran had taken against his bonus. Unfortunately, there is no direct information available about how any of these factors varied with population characteristics such as race, income, or unemployment rates. But some evidence comes from cross-state information on bonus amounts. On June 14, 1936, the day before bonus payments were distributed, the *New Orleans Times Picayune*[24] listed the number of veterans and bonus amounts to be paid by state. Unfortunately, it is unclear how the numbers were calculated. The source is listed as the American Legion, but the American Legion has no record of these data in their archives.[25]

Notwithstanding these caveats, I can use these figures to see how average bonus size was related to state population characteristics. Across states s, I estimate

$$(6)\qquad \textit{Average bonus}_s = \alpha_s + \beta_1 \textit{Urban share}_s + \beta_2 \textit{Black Share}_s + \beta_3 \Delta \textit{Employment } 29\text{–}36_s + \varepsilon_s.$$

The change in employment from 1929–1936 is a proxy for a state's unemployment rate, since no state level unemployment rate estimates exist.[26] Substituting the household survey values for each of the right-hand-side variables yields a prediction that the average bonus in the household survey sample was \$601, implying a MPC of 0.67.

Another source of evidence on the MPC comes from questions on the survey that directly reflected amounts saved and spent from the bonus. The survey did not explicitly ask whether a household had received the bonus and if so how much was spent. But under the category, "insurance policies settled," interviewers were supposed to record bonus money that was received but not spent.[27] Column 3 of

[24] "States Share in Bonus Payments," *New Orleans Times Picayune*, Section 6, first page.

[25] Furthermore, these numbers are close, but do not exactly match those from the Veterans' Administration.

[26] The barrier to calculating state unemployment rates in the 1930s is the lack of good estimates of the labor force by state. In the above regression, data on urban share and black share come from the 5 percent IPUMS sample from the 1930 census. Data on the 1929–1936 employment change are from Wallis (1989).

[27] According to BLS (1941b, p. 15):

> *Some families which furnished expenditure schedules received money during the schedule year from cash gifts, inheritances, or the soldiers' bonus. That part of such receipts which was used for current living expenses was treated as current money income. The remainder was either saved or invested, and was thus represented by an increase in one or another appropriate item—savings accounts, real estate, or the like. To balance such increases, a decrease was entered under "Other assets" in the case of cash gifts or inheritance and under "Insurance policies settled" in the case of the soldiers' bonus.*

Note that this variable could be non-zero even if one did not receive a bonus. This was where the interviewer was supposed to record the payout from any insurance policies held by the household.

Table 5 replicates the specification in column 2, but with this variable rather than total spending as the dependent variable. The coefficient implies that the average veteran in the survey saved \$96 of his bonus. This coefficient must, however, be interpreted with caution, since there was probably a problem of nonresponses on the insurance-policies-settled question. (It may not have been intuitive that this is where one was supposed to state the amount of a bonus not spent.) Hence, the coefficient may underestimate how much veterans actually saved.

In the initial interview (usually about two months before the expenditure interview), households were also asked about the value of "Gifts in cash for current use from persons not members of economic family."[28] Interviewers were supposed to record here the amount of the bonus spent on consumption. As with the saving question, one suspects that many did not. Column 4 shows results for this variable. As expected, the coefficient on the interaction term is highly significant. Not surprisingly, the sum of this coefficient with that for the measure of bonus saving (column 3) implies an implausibly low average bonus amount of \$152 + \$96 = \$248. This is as expected if respondents mistakenly entered zero. Still, assuming that the measurement error for the two measures was similar, the ratio $\frac{\$152}{\$96+\$152} = 0.61$ provides an alternative estimate of the MPC.

The point estimates thus suggest that the MPC was between 0.6 and 0.75. Of course, these estimates come with standard errors that reflect the uncertainty that accompanies a small sample. In addition, there is uncertainty about the exact bonus amount received by veterans in the household survey, and there is reason to doubt that the MPC of veterans in the survey was identical to that of veterans as a whole. Since veterans as a whole were more likely to be poor and unemployed, it seems likely that the MPC as measured in the household survey is biased down. This would be the case if unemployed veterans—who do not show up in the household survey—were hand-to-mouth consumers who spent their entire bonus.[29] If the results in Table 5 were the only source of evidence on the MPC, it would be difficult to draw strong conclusions about the bonus' effects. Fortunately, we shall see that this initial evidence of a large MPC is confirmed by results from alternative specifications, and from other, independent sources of data.

D. *Robustness*

Table 6 shows robustness checks for the baseline total expenditure specification. For comparison, column 1 reproduces column 2 of Table 5, my preferred specification. As discussed above, a possible source of omitted variable bias in this specification is local multiplier effects, if areas with more veterans saw larger increases in consumption pre to post bonus. To account for this possible effect, column 2 of

[28] See the urban family schedule in the ICPSR documentation for study 8908 and BLS (1939, p. 196).

[29] A related question concerns the external validity of the measured MPC: is the MPC measured for veterans indicative of the MPC in the population as a whole? One way to test this is to see whether the reduced form relationship between consumption and income in the household survey is affected by veteran status. I check this by estimating $C_i = \beta_0 + \beta_1 Y_i + \beta_2 \hat{V}_i + \beta_3 \hat{V}_i Y_i + \varepsilon_i$, where Y_i is household income, and $\hat{V}_i$ is the predicted probability that the household includes a veteran. I estimate this on observations before bonus payment, so that it is not in any way contaminated by the MPC from the bonus itself. Regardless of whether or not demographic controls variables are added, $\hat{\beta}_2$ and $\hat{\beta}_3$ are statistically insignificant. This is some evidence that the relationship between income and consumption for veterans and nonveterans was similar. Full regression results are available upon request.

Table 6—Total Expenditure Robustness Checks

	Baseline (1)	Local mult. (2)	Age, race only (3)	State×Post (4)	Age 18 dummy (5)	Post, rpt. '35 (6)	Cutoff $5,500 (7)	Cutoff $4,500 (8)	OLS (9)
Post bonus dummy	198.2***	390.7**	189.4***	—	156.7**	199.8***	229.8***	197.9***	
	(43.17)	(174.6)	(44.62)		(67.45)	(53.72)	(47.42)	(42.94)	
Interaction	403.1**	438.2**	446.3**	496.8***	483.0*	460.1**	314.3*	396.8**	
	(169.7)	(186.7)	(178.4)	(187.9)	(275.5)	(211.2)	(182.7)	(163.9)	
Income									0.701***
									(0.016)
First stage N	64,148	64,148	64,148	64,148	37,789	64,148	64,148	64,148	—
Second stage N	2,681	2,681	2,681	2,681	1,704	2,074	2,695	2,671	1,642
First stage R^2	0.213	0.213	0.212	0.212	0.135	0.213	0.213	0.213	—
Second stage R^2	0.186	0.186	0.186	0.191	0.172	0.205	0.182	0.189	0.713

Notes: All columns except 9 include the full set of second stage controls for age, race, and geography. See the text for details. In columns 1–6, households with total expenditure > $5,000 are excluded. In column 9, households reporting on consumption after bonus payment and households with income or expenditure > $5,000 are excluded. Bootstrap standard errors clustered at the city level in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Table 6 controls for the share of veterans in a state. The implied spending by veterans from the bonus is little changed.[30]

One concern about these specifications is that the exclusion restriction may be violated if cities have different seasonal consumption patterns. Suppose that, for example, cities in the north have more veterans and always consume more in fall than in spring. Then the estimates would ascribe part of this increase in consumption from spring to fall to higher proportions of veterans in these cities, and hence would overestimate the MPC. I conduct two checks for this possibility. In these, I continue to include the same set of second stage control variables. Column 3 of Table 6 uses only age and race, not geography, to predict veteran probability in the first stage. In fact, this results in a very similar but slightly larger estimate of the spending response. This is unsurprising, since most of the predictive power for veteran status in the first stage comes from age rather than location.

The results in column 3 are also useful since they suggest that the exact form of the first stage is not critical to the qualitative magnitude of the MPC. This conclusion is supported by online Appendix E which explores other possible first stage specifications. Across several different reasonable specifications, the implied MPC is qualitatively similar.

Column 4 further tests for the possibility of differential trends in consumption across states contaminating the results by including state fixed effects interacted with the post bonus dummy on the right-hand side. As in column 3, only age and race are used to predict veteran status: otherwise, it would be impossible to separately identify veteran spending from the bonus and consumption trends across states. In any case, including this control *raises* estimated spending from the bonus, although results are qualitatively (and statistically) unchanged.

[30] A qualitatively similar result obtains if one controls for the share of veterans in a city rather than in a state. Results are available upon request.

One might also worry that results are driven by differential changes in consumption across age groups. It is impossible to directly test for this, since age is the primary predictor of veteran status. But one way to address this concern is to limit the sample to a relatively narrow age range in which differential consumption trends unrelated to veteran status are less plausible. The small household survey sample makes a truly narrow age range impossible, but in column 5 the sample is restricted to households with men age 26 to 46 in 1936, and I use only one variable to predict the probability that this man was a veteran: a dummy variable for whether the man was over 18 in 1918. This addresses the concern that the same variables that predict veteran status might also predict bonus size. For instance, a white man might both be more likely to have been a veteran and, conditional on being a veteran, have been more likely to serve in the army longer and hence receive a larger bonus. This could bias my calculation of the MPC. But a dummy variable for whether or not a man was over 18 in 1918, and thus legally eligible to serve, has less risk of being correlated with the size of the bonus.[31] Of course this check also again tests whether the exact form of the first stage specification is critical to the results. Fortunately, it is not. This exercise throws out nearly 1,000 observations, so results are unsurprisingly less statistically significant. But the implied MPC $(483/547 = 0.88)$ is qualitatively similar to the baseline.

A further worry is that the ability of households to choose to report on calendar year 1935 rather than the most recent 12 month period is a source of bias. Suppose, for instance, that veterans who spent much of their bonus were *less* likely to report on 1935. This could be because households were eager to report recent salient purchases. In this case, the coefficient would be biased up: one would observe more spending among veterans post bonus than actually occurred. To see if this effect is driving the results, column 6 drops all households interviewed after the bonus was paid (i.e., after June 15, 1936) who chose to report on 1935. The measured MPC actually rises slightly.

A limitation of the household survey is the presence of outliers. As discussed earlier, I exclude households with total expenditure greater than $5,000 to avoid these problems. I chose the cutoff of $5,000 since above this a robust regression assigns households zero weight. The final two columns of Table 6 look at the effect of choosing slightly different cutoffs, $5,500 in column 7 and $4,500 in column 8. Raising the cutoff to $5,500 means including outliers and hence reduces the precision of the estimate. By contrast, lowering the cutoff (column 8) results in little change to the coefficient or standard error.

Column 9 considers a quite different estimate of the MPC. It reports results from a naïve OLS regression of consumption on income in the household survey. To avoid complications from misrecording of the bonus in the household survey measure of income, I use only observations from households reporting on income and

[31] The age discontinuity is somewhat less clear than one might think. Of men age 18 in 1918, 23 percent were veterans, among those who were 17, 13 percent were veterans, of those who were 16, 6 percent. (These numbers are based on veteran status as recorded in the 1930 census.) There are at least three reasons why the age discontinuity is not sharp. (i) Some of those who were 17 in April 1918 would have turned 18 by September 1918 and would thus have been eligible for service. (ii) Some respondents may have misreported their age to the 1930 census. (iii) Although no one who was under 18 could legally serve in the military, there is substantial anecdotal evidence of men under 18 enlisting in the army. Age checks were lax.

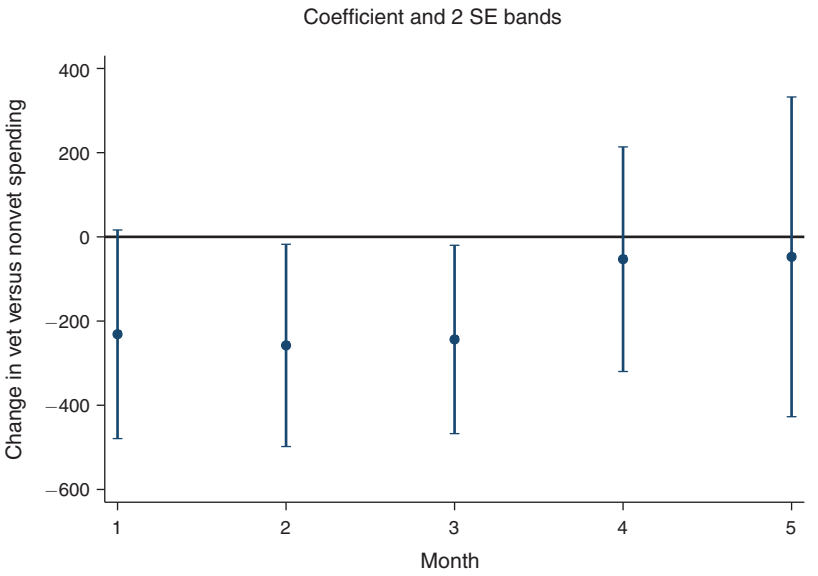

FIGURE 3. HOUSEHOLD CONSUMPTION SURVEY PLACEBO TEST

consumption before the bonus was paid. The coefficient on income is a precisely estimated 0.7, very similar to the estimate of the MPC I obtain using variation from the veterans' bonus. Of course, this OLS estimate of the MPC may be biased, since consumption and income are jointly determined, and since this measure of income includes both temporary and permanent shocks. But although it measures something different than the MPC from the veterans' bonus, it is perhaps reassuring to see that the coefficient is similar.

A final check is a placebo test. If I am measuring the true effect of the veterans' bonus, it should show up most strongly in June. There should be no discontinuity in spending between veterans and nonveterans before June. If, by contrast, the estimates are picking up different trends in consumption across age groups, one would expect to see this effect in other months. To test this, I first throw out all observations in the household survey from after the bonus payment. I then estimate my baseline specification (equation (4)) setting the post bonus dummy variable equal to months ranging from January to May 1936. Differential trends in consumption across age group, state, or race should show up here in the same way they do in the results in which June is the post bonus dummy month. Any effect of the bonus disbursement should not show up in this placebo test, since I throw out all households reporting on consumption after the bonus payment. Figure 3 shows results. Before the bonus payment in June, the pre to post change in veteran spending was if anything *less* than the pre to post change in nonveteran spending. This is encouraging; it implies that the estimates are picking up spending from the bonus, not some underlying macro trend.

Overall these robustness tests are reassuring. Nonetheless, any one dataset and estimation strategy inevitably comes with uncertainties. And the uncertainties in this context may be unusually large since the results rely on reasonable specifications

Table 7—Consumption Category Regressions

	Auto purchases and operations (1)	Housing (2)	Furniture/ equipment (3)	Clothing (4)	Recreation (5)	Food (6)
Post bonus dummy	47.77***	9.497	4.954	41.48***	9.326***	29.94**
	(10.77)	(8.425)	(5.244)	(7.734)	(3.132)	(12.77)
Interaction	127.5**	83.38***	21.71	32.92	24.86	35.10
	(60.30)	(29.39)	(20.44)	(27.09)	(15.76)	(45.66)
Observations	2,681	2,681	2,681	2,681	2,681	2,681
R^2	0.072	0.204	0.051	0.091	0.074	0.231

Notes: See the text for a description of the controls. Households with total expenditure > $5,000 are excluded. Bootstrap standard errors clustered at the city level in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

of both a first and second stage. This motivates identifying alternative, independent sources of evidence on the bonus' effects. I turn to these in the following sections. However, first I use the household survey to see what veterans bought with their bonus.

E. *Expenditure Categories*

The detail of the household survey allows me to break down the spending response across consumption categories. Table 7 reproduces for several categories of consumption the baseline specification that excludes households with spending greater than $5,000. The first column reports results for total automobiles-related spending, including new and used car purchases, gasoline, and repairs. The coefficient is economically and statistically highly significant. It implies that roughly one-third of the total spending response was in this category. Column 2 shows results for housing. This is housing consumption (rent and repairs), not residential investment, so it excludes house purchases and structural additions. Still, results suggest a large effect of the bonus. The coefficients for the remaining categories of consumption imply nontrivial amounts of spending but are less precisely estimated.

Because of its importance for the overall spending response, in Table 8 I look in more detail at the response of autos consumption to the bonus payments. Column 1 reproduces column 1 of Table 7. In column 2, the dependent variable is money spent on auto purchases (new and used) only. The result suggests that more than half the autos spending response came from car purchases. The third column shows results from the linear probability model in which the left-hand-side variable is a dummy corresponding to whether or not a household purchased a car in the schedule year. The coefficient implies that the bonus increased the probability of a car purchase by 22 percentage points. This is large: in the sample as a whole, the probability that a household bought a car was 19 percent. The final column of Table 8 shows the response of all auto-related spending except car purchases. This includes gasoline, repairs, and purchases such as tires and car radios. The sizable response suggests that veterans who did not purchase a car often responded to the bonus by investing in existing cars and/or driving more.

TABLE 8—AUTOS REGRESSIONS

	Auto purchases and operations (1)	Auto purchases (2)	Auto purchase dummy (3)	Auto operations (4)
Post bonus dummy	47.77***	28.25***	0.00386	19.51***
	(10.77)	(7.685)	(0.0146)	(6.903)
Interaction	127.5**	71.09*	0.215***	56.39*
	(60.30)	(37.89)	(0.0750)	(30.11)
Observations	2,681	2,681	2,681	2,681
R^2	0.072	0.039	0.047	0.075

Notes: See the text for a description of the controls. Households with total expenditure > $5,000 are excluded. Bootstrap standard errors clustered at the city level in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

III. Cross-State and Cross-City Evidence

Another source of evidence on the bonus' effects comes from cross-state and cross-city regressions. States and cities varied in the proportion of their population made up of veterans, and hence in the amount of stimulus they received. Thus by relating variation across states and cities in veterans per capita with state or city level data on economic outcomes, it is possible to make inferences about the effects of the bonus. In this section, I consider the effects of the bonus on auto sales and new residential building permits. As we shall see, these two outcomes variables have several desirable properties. In particular, like the household survey, they measure spending. But there are data on other economic outcomes at the state level. In online Appendix F, I consider the effects of the bonus on income and employment. These results also suggest a large MPC from the bonus.

Unlike the analysis using the household consumption survey, cross-state and cross-city regressions do not measure only the direct, first-round spending by veterans from the bonus. By comparing spending in areas with many veterans to spending in areas with fewer veterans, the estimates below measure both first-round spending from the bonus and local spillovers. If nonveterans also spent more on cars in Wyoming, where there were many veterans, than they did in Georgia, where there were few veterans, this will increase the cross-state estimate. Online Appendix G provides an algebraic derivation of how local multipliers affect cross-state and cross-city estimates of bonus spending. If the dominant local spillover comes from higher veteran consumption driving higher local incomes, than estimates of spending from cross-state and cross-city regressions will be an upper bound on veterans' true MPC.

In the example described in online Appendix G, the cross-state estimate always exceeds the true MPC, but it is also possible that a cross-state estimate will understate the true MPC. This would be the case, for instance, if spending by veterans pushed up local prices, reducing nonveteran spending. The general point is that unlike the household survey estimates which measured the true MPC, the results in this section

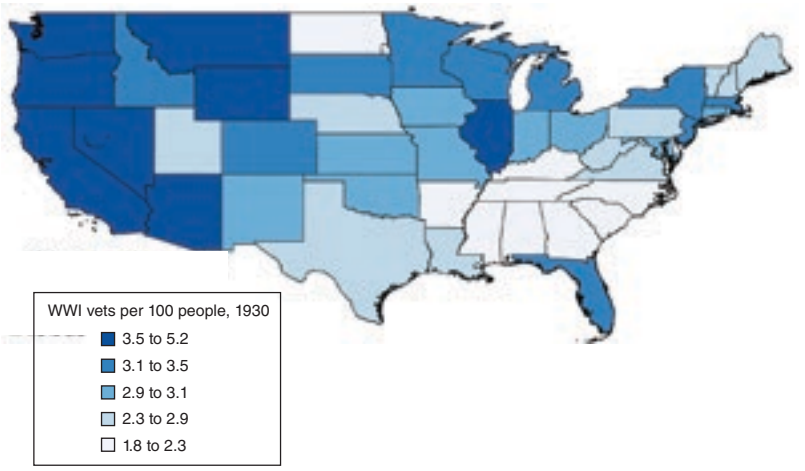

FIGURE 4. VETERANS PER 100 PEOPLE IN 1930

Note: Darker colors denote more veterans per capita in a state.
Source: IPUMS 5 percent sample of the 1930 census (Ruggles et al. 2010)

measure a general equilibrium effect of the bonus. This general equilibrium effect is both itself of interest and also contains information about the MPC. All else equal, the higher the cross-state estimate of spending from the bonus, the larger the MPC is likely to have been.

A. *The Geographic Distribution of Veterans*

Figure 4 shows data from the 1930 census on the percent of each state's population made up of World War I veterans. There is large geographic variation, from a low of 1.8 veterans per 100 people in Mississippi to a high of 5.2 veterans per 100 people in Wyoming, the District of Columbia, and Nevada. Southern states tended to have fewer veterans while western states tended to have more.

Variation in the number of veterans in a state translated into variation in the fiscal impulse received in 1936. As discussed in Section I, veterans received different bonus payments depending on how long they had served in the military, whether they served overseas, and whether they took loans against their bonus. In practice, however, there was relatively little variation across states in the average payment. The *Times Picayune* reported American Legion measures of both the number of veterans and bonus payments received in each state in 1936. As mentioned earlier, the source of the American Legion numbers is unknown, and they do not quite match totals reported by the Veterans' Administration (1936). Hence I use the census data on veterans' share as a proxy for the actual stimulus in the estimates below. However, it is worth noting that in the American Legion numbers, the correlation between veterans per capita and bonus payments per capita is 0.96. Thus the proportion of veterans in a state's population is likely to be an excellent proxy for the amount of bonus money received.

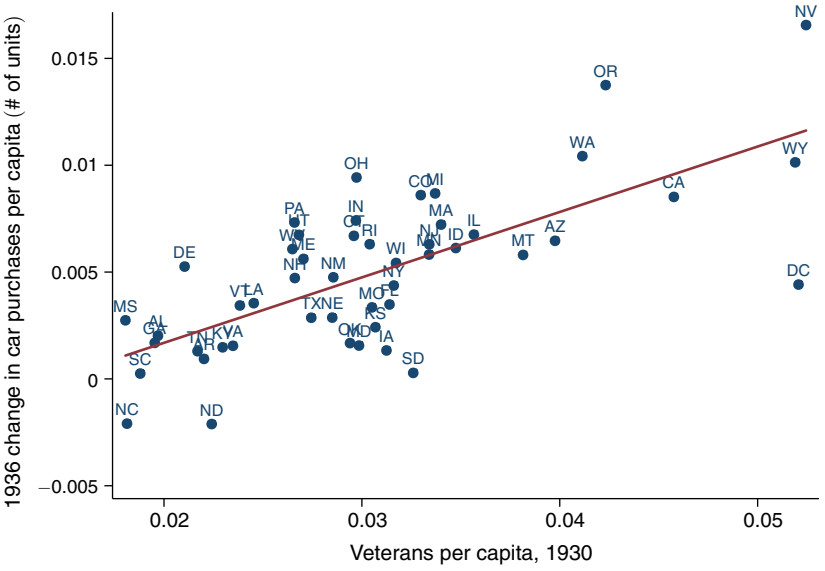

FIGURE 5. AUTO SALES PER CAPITA AND VETERANS PER CAPITA

Sources: Auto sales: *Automotive Industries* "New Motor Vehicle Registrations by States," February 22, 1936, p. 243 and February 25, 1939, p. 208; population: BEA table SA1-3; veterans per capita: IPUMS 5 percent sample of the 1930 census (Ruggles et al. 2010).

B. *Auto Sales*

The effect of the veterans' bonus on new car sales is both of independent interest and is a useful indicator of the bonus' effect on consumption as a whole. Cars are only one type of consumption, but they have two advantages as a macro indicator over aggregate data such as state income or employment. First, they have little measurement error. I collected data on annual passenger car sales by state directly from the annual statistical issues of the industry trade publication, *Automotive Industries*. Since state laws mandated the registration of new cars, these data are well measured. Second, because of spillover effects, car sales are likely to be a better proxy for the consumption effects of the bonus in a state than employment or income. If a veteran spent his bonus on a car, the employment impact would show up primarily in Michigan, but the car sale would show up in his home state.

Figure 5 shows the per capita change in auto sales from 1935 to 1936 on the vertical axis and veterans per capita on the horizontal axis. The figure suggests a strong positive relationship between bonus payments received and the change in auto sales. A concern is that this reflects a correlation between veteran population in a state and some other factor driving auto sales growth. For instance, veterans per capita could be correlated with agricultural income, given the prevalence of veterans in the west. I address this concern in three ways: first, by estimating regressions with a variety of control variables, second, by estimating regressions on subsets of states, and finally by running placebo tests to see if the presence of veterans is correlated with auto sales in other years of the 1930s.

Table 9—Regression Results for the 1936 Change in per Capita New Car Sales

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Veterans per capita, 1930	0.306***	0.281***	0.344***	0.209***	0.214**	0.360***	0.276***	0.332*
	(0.0648)	(0.0801)	(0.0806)	(0.0692)	(0.0829)	(0.0587)	(0.0750)	(0.191)
Per capita new car sales in 1929		−0.0207						
		(0.0591)						
1930 income per capita/1,000		0.00244						
		(0.00269)						
Δ per capita new car sales in 1935			−0.121					
			(0.150)					
Midwest				−0.00180*				
				(0.00103)				
South				−0.00263***				
				(0.000728)				
West				0.000979				
				(0.000970)				
Black share of the population					−0.00279			
					(0.00462)			
Farm share of the population					−0.00582**			
					(0.00250)			
Δ drought indicator					−0.000730			
					(0.000458)			
Δ New Deal grants per capita/1,000						0.0539		
						(0.0551)		
Observations	49	49	49	49	49	48	38	21
R^2	0.492	0.508	0.500	0.604	0.594	0.607	0.225	0.155

Notes: The dependent variable is the change in new car sales (in number of units per capita) from 1935 to 1936. The drought indicator equals 1 if the Palmer drought index averaged −3 or below in June–August. The change in New Deal spending is measured in fiscal year 1936 (July 1, 1935–June 30, 1936). No data are available for New Deal spending in Washington, DC; thus the sample drops from 49 to 48 in column 6. Robust standard errors in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

Sources: Auto sales: *Automotive Industries* "Passenger Car Sales by States - 1929," February 22, 1930, p. 267; and "New Motor Vehicle Registrations by States," February 22, 1936, p. 243, and February 25, 1939, p. 208; population: BEA table SA1-3; veterans per capita, farm share, and black share: IPUMS 5 percent sample of the 1930 census (Ruggles et al. 2010); drought: http://www.ncdc.noaa.gov/temp-and-precip/drought/historical-palmers.php; New Deal spending: Fishback and Kachanovskaya (2015) and Fishback (2015).

In Table 9, I show results from estimating specifications of the form

$$A_{s,1936} - A_{s,1935} = \alpha + \beta \textit{Veterans per capita}_s + \mathbf{X}'_{s,t}\boldsymbol{\theta} + \varepsilon_{s,t}, \tag{7}$$

where $A_{s,t}$ is per capita new passenger car sales in state s in year t, and $\mathbf{X}_{s,t}$ is a vector of control variables. What does this measure? Online Appendix G works through a simple example to show that in the presence of local spillovers from consumption to income, $\hat{\beta}$ will be

$$\hat{\beta} = \frac{b}{1 - \gamma b}, \tag{8}$$

where b is the true MPC, and γ is the amount by which local income rises when local consumption rises by a dollar. As noted above, this means that the estimates in Table 9 are of a general equilibrium effect of the bonus, an effect related to but likely not identical to the actual MPC.

The first column of Table 9 shows estimates from the simple regression with no control variables analogous to the scatter plot in Figure 5. Column 2 controls for per capita new car sales in 1929 and per capita income in 1930. 1929 was the interwar era car sales peak, and per capita car sales in 1929 had regional variation that was correlated with that of veterans per capita: auto sales were higher in the sparsely populated west than they were in the east or south. The level of income per capita in a state in 1930 is included since one might worry that this would be correlated with the amount of borrowing from the bonus in early 1931, when veterans were allowed to take large loans. Despite these possible concerns, adding these controls to the regression has almost no effect on the coefficient.

Results are also little changed when one controls for the lagged change in car sales per capita (column 3). The above discussion of the geographic distribution of veterans and its causes suggests it is important to control for region fixed effects, or the share of a state's population living on farms and the share of a state's population that was black. This is done in columns 4 and 5. Column 5 also includes a control for drought severity, given the possible economic effects of the Dust Bowl. While the size of the coefficient on veterans per capita falls, it remains highly statistically significant. Column 6 controls for the change in New Deal grant spending (excluding the veterans' bonus) between fiscal year 1935 (July 1, 1934–June 30, 1935) and fiscal year 1936 (July 1, 1935–June 30, 1936). Total New Deal grants in fiscal year 1936 were 6.3 billion dollars, or roughly 8 percent of GDP.[32] Thus, changes in New Deal grant spending had the potential to strongly influence economic activity (Fishback and Kachanovskaya 2015; Fishback, Horrace, and Kantor 2005). Controlling for the change in New Deal spending, however, has little effect on the economic or statistical significance of the coefficient on veterans per capita.

The last two columns of Table 9 return to the simple regression of the change in auto sales on veteran share, but restrict the sample. Column 7 drops the five states with fewer than two veterans per capita (Alabama, Georgia, Mississippi, North Carolina, and South Carolina) and the six states with more than four veterans per capita (California, Nevada, Oregon, Washington, Washington, DC, and Wyoming). The sample is limited in column 8 to the 21 states in the Midwest and Northeast. In both cases, the coefficient on veterans per capita is little changed from that in column 1, although when the sample size is cut to 21, the standard error is unsurprisingly larger.[33]

[32] I am indebted to Price Fishback for providing me the data on New Deal grant spending by state and year (Fishback 2015 and Fishback and Kachanovskaya 2015). I arrive at the 8 percent of GDP figure by using the mean of calendar year nominal GDP in 1935 (74.3 billion dollars) and calendar year nominal GDP in 1936 (84.9 billion) (NIPA table 1.1.5).

[33] A possible confounding variable not controlled for in Table 9 is military spending. One might worry that if military spending were rising, and if veterans lived where military spending were taking place, this could be driving the results. This concern is alleviated in two ways: first, military spending was low and changed little in 1936. According to NIPA table 1.1.5, total federal defense spending in 1936 was $1.4 billion (1.6 percent of GDP); this was $200 million more than in 1935. Second, one can perform a partial check for whether differential trends in military spending might be biasing the results by using data on shipbuilding. Biolsi (2015) documents that much of the increase in US military spending in the mid-1930s was used to build ships for the navy. Thus I try excluding all states that contained a navy shipyard in 1934, the year for which Biolsi provides such a list (Biolsi 2015, table V). This means excluding 14 states: Alabama, California, Connecticut, Delaware, Maine, Maryland, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, South Carolina, Virginia, and Washington. In the single variable regression of the change in auto sales per capita on veterans per capita (column 1 of Table 9), excluding these states changes the coefficient from 0.31 to 0.32. Full results are available upon request.

The estimates in Table 9 suggest that the coefficient on veterans per capita lies between 0.2 and 0.35. This means that for every additional veteran living in a state, 0.2 to 0.35 more new cars were sold in 1936. The average retail price of a car in 1936 was $781 (Suits 1958), thus if veterans bought average-priced cars, than an additional veteran increased new car spending by roughly $200. Of course, it is likely that veterans tended to buy lower price cars that could be afforded with a bonus check. If, for example, veterans bought only the cheapest Fords or Chevrolets, than this calculation implies that an additional veteran raised new car spending by roughly $140.

The true number is probably somewhere between these two. An informative way to understand these numbers is to compute the change in new car spending in a state per bonus dollar received. This equals $\frac{\text{increase in new car spending}}{\text{average bonus}}$ = roughly 25 to 35 cents depending on one's assumption about the price of new cars purchased by veterans.

The effect of the bonus on new car sales in Table 9 is larger than that measured in the household survey. Whereas the household survey results (Section III, Table 8) suggest that on average each veteran spent roughly $70 of his $550 bonus on new and used car purchases, the cross-state regressions show spending on new car purchases alone rising by roughly $150 per veteran living in a state. There are at least two reasons why the effect in the cross-state regression may be larger. First, the cross-state regression uses annual car sales data. Thus it picks up purchases of cars made by veterans in all of 1936. Second, and as discussed above, the larger effect as measured from cross-state regressions could reflect local spillovers.

Placebo Tests.—A useful robustness test is to examine whether the share of veterans affected auto sales in other years when no bonus was paid. If veterans per capita had a statistically significant relationship with auto sales in, say, 1940, this would cast doubt on whether the effect in 1936 is a causal impact of the bonus rather than a spurious correlation.

Figure 6 shows coefficient estimates and two standard error bounds from cross-sectional regressions of the change in new car sales per capita on veterans per capita for the years 1930 to 1940. Each regression controls for the level of car sales per capita in the state in 1929. As discussed above, this is correlated with the regional variation in veterans per capita. It is also correlated with the dynamics of new car sales in the 1930s. States with higher car sales in 1929 experienced larger declines in car sales during the Depression and larger recoveries thereafter.

Three years in Figure 6 have coefficients on veterans per capita that are statistically different from zero: 1932, 1936, and 1937. The negative coefficient in 1932 likely reflects the backside of the loan payments to veterans in 1931. The absence of a statistically significant positive coefficient in 1931 may reflect the absence of controls in the regression for the multitude of shocks hitting the US economy in that year. Since veterans were not distributed randomly across the country, insofar as some of the economic shocks hitting the US economy had a regional component, this could confound the relationship between veterans per capita and 1931 auto sales.

As expected, the coefficient in 1936 is both far larger and far more statistically significant than that in any other year. The precisely estimated negative coefficient in 1937 is also consistent with a boost to car sales in 1936 and then a return to more

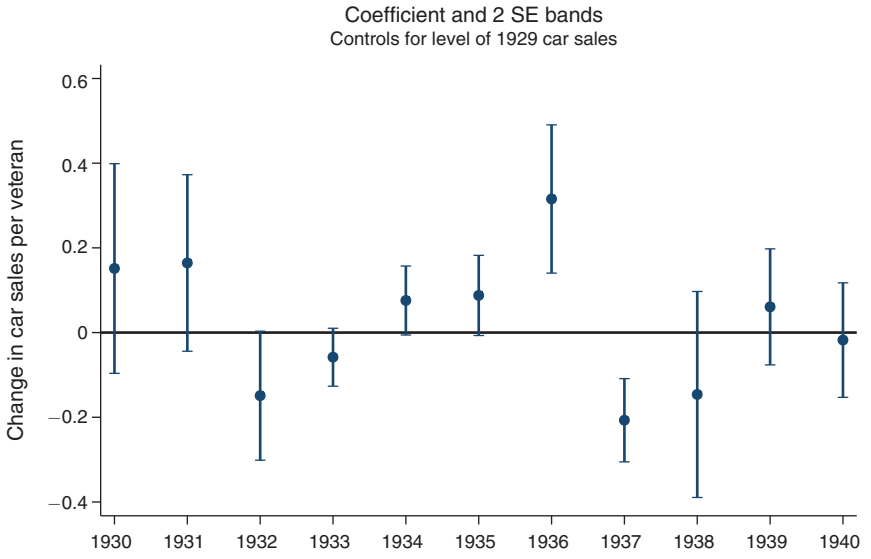

FIGURE 6. AUTO SALES AND VETERANS PER CAPITA 1930–1940.

Note: Standard errors are the max of conventional and heteroscedasticity robust standard errors.

Sources: Auto sales: *Automotive Industries* "Passenger Car Sales by States - 1929," and "[1929–1931] New Passenger Car and Truck Sales by States," February 22, 1930, p. 267, February 28, 1931, p. 309, February 27, 1932, p. 294; "Sales Outlets and Passenger Car Sales by States," February 25, 1933, p. 224, February 24, 1934, p. 220; "New Motor Vehicle Registrations by States," February 22, 1936, p. 243, February 25, 1939, p. 208, March 1, 1940, p. 186, and March 1, 1941, p. 214; population: BEA table SA1-3; veterans per capita: IPUMS 5 percent sample of the 1930 census (Ruggles et al. 2010).

normal levels in 1937. Note that the negative coefficient in 1937 does *not* mean that the bonus simply shifted car sales forward from 1937 to 1936 or that states with more veterans were worse off in 1937. It means that states with more veterans had car sales further above normal in 1936, and hence saw larger relative declines when sales returned to trend in 1937. In fact, the coefficients in Figure 6 imply that the veterans' bonus increased the *level* of car sales in 1937 as well as in 1936.

C. *Housing*

The household survey results showed that the largest category of spending from the bonus after autos was housing. This was housing consumption, e.g., rent and repairs, but it suggests that veterans may also have spent their bonus on housing investment, in particular new houses. One way to test this is to examine the relationship between changes in building construction in a city and the proportion of its population made up of veterans.

In the 1930s, the Bureau of Labor Statistics collected data on the value of residential building permits issued for over 300 cities.[34] To measure the effect of the bonus, I estimate a regression similar to the cross-state specification for auto sales:

$$(9) \qquad R_{c,1936} - R_{c,1935} = \alpha + \beta \textit{Veterans per capita}_c + \mathbf{X}'_{c,t}\boldsymbol{\theta} + \varepsilon_{c,t},$$

[34] I am deeply indebted to Price Fishback for providing me a digital copy of these data.

Table 10—Regression Results for the 1936 Change in Per Capita Residential Building Permits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Veterans per capita, 1930	262.0***	200.0**	203.2***	208.5***	193.7***	121.4**	257.9**
	(84.36)	(77.22)	(74.80)	(74.66)	(61.04)	(47.52)	(103.0)
Δ per capita bldg. permits in 1935			0.624**	0.591**	0.625**		
			(0.267)	(0.272)	(0.290)		
Black share of the population				4.036			
				(5.461)			
Δ New Deal grants per capita/1,000					150.3**		
					(59.18)		
State fixed effects		X				X	X
Outliers excluded						X	
Cities with pop. > 50,000 only							X
Observations	302	302	302	302	301	282	185
R^2	0.082	0.275	0.174	0.178	0.185	0.364	0.344

Notes: The dependent variable is the change in residential building permits (in dollars per capita) from 1935 to 1936. The change in New Deal spending is the change in grant spending, excluding the veterans' bonus, from fiscal year 1935 (July 1, 1934–June 30, 1935) to fiscal year 1936 (July 1, 1935–June 30, 1936). No data are available for New Deal spending in Washington, DC; thus the sample drops from 302 to 301 in column 5. Standard errors clustered at the state level in parentheses.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

Sources: The building permit data are from Price Fishback who collected them from various editions of the Bureau of Labor Statistics Monthly Labor Review and from BLS (1938, 1940). Veterans per capita and black share come from the IPUMS 5 percent sample of the 1930 census (Ruggles et al. 2010). New Deal spending data come from Fishback (2015) and Fishback and Kachanovskaya (2015).

where $R_{c,t}$ is the per capita dollar value of residential building permits in city c in year t, and $\mathbf{X}_{c,t}$ is a vector of control variables.

Table 10 shows results. The first column reports results from the simple regression of the change in building permit value from 1935 to 1936 on veterans per capita in a city. The coefficient of 262 implies that for every additional veteran in a city, the value of residential building permits rose by $262. Given that the average bonus was $550, this is large. As was the case with the cross-state autos regressions, a concern is that the proportion of veterans living in a city is correlated with some other economic factor. One way to control for many such factors is to add state fixed effects. This is done in column 2. Since the left-hand-side variable is the *change* in building permits per capita, adding state fixed effects allows for state specific trends in building permit values. Many of the factors that might bias the results of a simple regression will be captured by these state specific trends.

The coefficient shrinks somewhat with the addition of state fixed effects. As an alternative to fixed effects, one can control for the lagged change in per capita building permits (column 3). The coefficient on veterans per capita is almost identical to that in the specification with fixed effects (column 2). Column 3 controls for the black share of the population in a city, and column 4 controls for the change in New Deal grant spending between fiscal year 1935 (July 1, 1934–June 30, 1935) and fiscal year 1936 (July 1 1935–June 30, 1936). Neither of these controls has much effect on the coefficient on veterans per capita.

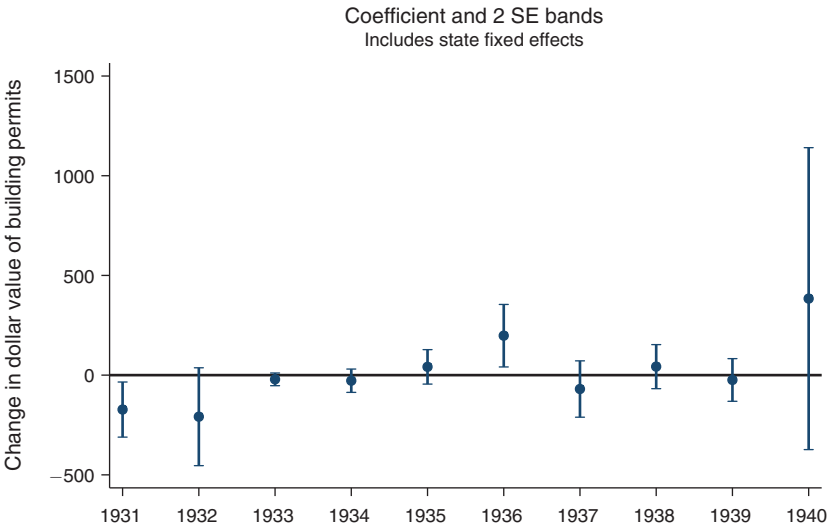

FIGURE 7. RESIDENTIAL BUILDING PERMITS AND VETERANS PER CAPITA 1931–1940.

Note: Standard errors are clustered at the state level.

Source: See Table 10.

More important is whether one drops outliers. In column 6, I return to the specification with state fixed effects, and I drop the ten cities with the smallest and largest per capita building permit changes. This shrinks the coefficient, but it remains highly economically and statistically significant. Column 7 limits the sample to cities with a population greater than 50,000 (in 1930). This modestly increases the size of the coefficient relative to the same specification with the full sample of cities (column 2).

A final check on these results is placebo tests. Figure 7 shows the coefficient and two standard error bands for 1931–1940 for the regression of the change in building permits per capita on veterans per capita. While the results are not completely clear (and are graphically distorted by large standard errors in 1940), they are generally reassuring. 1936 is the only year with a positive and statistically significant coefficient. Also reassuring is that the coefficient in 1937 is negative, though not statistically significant. The negative coefficient may reflect a backside of the bonus.

Taken together, the results in Table 10 and Figure 7 imply that the bonus had large effects on residential construction. The precise quantitative magnitude is difficult to determine given the estimate's sensitivity to the sample of cities. But even the smallest estimate in the table, in column 6, is large. This impetus to housing occurred against a background of a construction sector that remained depressed throughout the 1930s (Field 1992). When I discuss the sources of high bonus spending in Section V, I consider the possible link between these depressed conditions and veterans' spending on housing.

IV. Direct Survey and Narrative Evidence

A. *The American Legion Survey*

In the recent literature on consumption responses to transfers there are two dominant approaches. One is to use CE survey data to measure spending from a transfer payment. Influential examples are Johnson, Parker, and Souleles (2006) on the effect of the 2001 Bush tax cuts, and Parker et al. (2013) on the effect of the 2008 stimulus payments. An alternative approach is to ask people how they would spend a hypothetical transfer (Jappelli and Pistaferri 2014) or how they plan to spend an actual transfer. Shapiro and Slemrod (2003a, 2009) examine actual spending plans by adding a question to the Michigan Survey of Consumers.

A very rough equivalent to Shapiro and Slemrod's exercise can be replicated for the 1936 Veterans' Bonus. In January 1936, the American Legion surveyed 42,500 of its members about what they planned to do with their bonus. At the time, the American Legion was the leading World War I veterans' group, and it played a large role in lobbying for the bonus' passage (Daniels 1971). The survey is shown in online Appendix H. While the actual survey responses no longer exist, the American Legion library has a copy of unpublished tabulations prepared by an Indianapolis accounting firm. Table 11 summarizes these tabulations; details are in online Appendix H.

Two caveats are needed before discussing the results. First, the survey was done before the exact legislative details of the bonus were known, and in particular before it was known whether veterans would be allowed to leave their bonus with the government and earn 3 percent interest on it. Thus the survey estimates may somewhat understate total saving from the bonus. Second, the American Legion population was not identical to that of all veterans. It is possible that what was true about spending for Legion members was not true for veterans as a whole. Despite these caveats, the survey tabulations do provide information on the spending plans of a large population of veterans.

The results show that veterans planned to use 37 percent of the bonus to save or pay down debt. Nearly all of this is accounted for by debt repayment. Of the remaining 63 percent of the bonus, veterans said they planned to spend 25 percent on business and residential investment and the remainder on consumption: the implied MPC is 0.38. Of course, in determining the short-run aggregate effects of the bonus, what matters is the total marginal propensity to spend (0.63) not whether it was investment or consumption spending.

For comparison, using a question on household spending plans added to the Michigan Survey of Consumers, Shapiro and Slemrod (2003a, 2003b) find an MPC of roughly one-third from the 2001 tax rebates. By contrast, Johnson, Parker, and Souleles (2006) investigate spending from the 2001 tax rebate using the CE survey and find an MPC on nondurables alone of roughly two-thirds. It appears that at least part of the difference between these results comes from the measurement of the lagged spending response.[35] It is also possible to compare the results of these

[35] Johnson, Parker, and Souleles (2006) find a MPC in the quarter the 2001 tax rebate was received similar to that measured in Shapiro and Slemrod (2003a). But Johnson, Parker, and Souleles also find a substantial spending

Table 11—American Legion Survey Tabulations

Item	Amount per veteran ($)	Percent of bonus
Housing consumption	47.62	8.43
Durable goods excluding autos	44.22	7.82
Clothing	39.76	7.04
Passenger automobiles	30.86	5.46
Trucks	4.02	0.71
Automobile tires and batteries	1.67	0.30
Nonresidential investment	69.33	12.27
Residential investment	72.11	12.76
Other spending	46.91	8.30
Spending total	**356.60**	**63.08**
Pay old bills and debts	**177.26**	**31.36**
Savings accounts	25.26	4.47
Purchase stocks or bonds	6.15	1.09
Savings total	208.68	36.92

Source: Complete tabulations from the American Legion. See online Appendix H.

two methodologies for the consumer response to the 2008 tax rebates. Shapiro and Slemrod (2009) find an MPC of roughly one-third. Using the CE survey, Parker et al. (2013) find an MPC of 0.5 to 0.9. The difference may primarily be due to auto purchases. Parker et al. (2013) find an MPC on car purchases alone of 0.48 (Table 7). Intriguingly, the household consumption survey in 1936 also shows much more spending on autos than does the American Legion Survey. Whereas the household survey results show that veterans on average spent $71 of the bonus on car purchases, the American Legion survey shows that only $31 was spent on car purchases.

Overall, a comparison of contemporary evidence on the MPC from direct spending data and self-reported behavior suggests that the lower MPC observed in the American Legion survey is unsurprising. The modern evidence suggests that the MPC in the Legion survey of 0.38 is consistent with the MPC of 0.6 to 0.75 found using the 1935–1936 consumption survey. The discrepancy may well be due to auto purchases and/or a lagged spending response not captured in the American Legion survey. Furthermore, since the American Legion results suggest substantial spending on residential and business investment as well as consumption, they imply that the total marginal propensity to *spend* was somewhat above the marginal propensity to *consume*. The substantial spending on new home purchases and construction indicated by the American Legion survey fits with the cross-city building permit results. Together these two sources provide strong evidence that veterans spent a significant portion of their bonus on housing investment.

response in the following quarter. A possible explanation is that many households told the Michigan survey that they planned to use the rebate to pay down debt and that these households initially did exactly this. But over the months following the rebate, households may have responded to lower debt by spending more, a response they may not have considered when predicting how the rebate would affect their spending. Agarwal, Liu, and Souleles (2007) analyze credit card spending and debt in 2001 and find evidence for this behavior. A puzzle, however, is that when asked later by the Michigan Survey, households did not report this additional, lagged spending (Shapiro and Slemrod 2009).

B. *Narrative Evidence*

A useful check on the quantitative evidence of previous sections comes from newspaper reports at the time. There were many reports of high spending by veterans. For instance, the *Los Angeles Times*, "Bonus Boom Takes Leap" (June 19, 1936, p. A1) wrote four days after the bonus was distributed: "All signs yesterday pointed to a real spending spree by veterans .... Downtown department stores reported yesterday's sales were more than 30 percent above a week ago." Online Appendix I provides more examples from contemporary reporting of newspapers around the country.

V. Why Was the MPC So High?

A variety of evidence suggests that veterans quickly spent the majority of their bonus. This is in some ways puzzling. The conventional wisdom is that when transfer payments are large and predictable, as the bonus was, the MPC should be relatively small (Browning and Crossley 2001).[36] Empirical evidence for this view comes from Hsieh (2003) who looks at the consumption response to annual payments from the Alaska Permanent Fund and finds a MPC of zero. By contrast, Souleles (1999) looks at the consumption response to annual income tax refunds and finds a MPC of between 0.35 and 0.65. Hsieh (2003) speculates that he finds a lower response because payments from the Alaska Permanent Fund differ in important ways from income tax refunds. In particular, they are larger and more predictable. In both respects, the bonus was probably more similar to the Alaska Fund Payments than to income tax refunds.

Why then did World War I veterans spend so much of their bonus? Although an explanation is necessarily speculative, three features of the 1936 economy were likely critical: (i) people faced substantial liquidity constraints; (ii) future income was expected to be higher; and (iii) there was substantial pent-up demand for durables, in particular cars and housing.

A. *Liquidity Constraints*

The interwar period saw growing consumer debt; consumer non mortgage debt as a percent of income rose from 4.6 percent in 1919 to 11.4 percent in 1939 (Olney 1999, Table I). However, all consumer debt was short-term and often came with onerous terms (Olney 1991). A typical car loan required a 33 percent down payment if a new car, and a 40 percent down payment if a used car (Olney 1991). The maximum maturity length of a car loan was 18 months, with a typical interest rate, including all finance costs, of 20 to 40 percent (Olney 1991). Thus the typical veteran, while able to take out a loan to finance a car or other large durable good purchase, in practice may still have been constrained by the large down payment

[36] Jappelli and Pistaferri (2010) discuss this argument in more detail. A related argument made by behavioral economists suggests that the MPC should decline with the size of the payment. Whereas small payments may be put in mental "income" account from which the MPC is large, large payments, such as the veterans' bonus, may be put in a mental "asset" account from which the MPC is small (Shefrin and Thaler 1988).

requirements. And high interest rates were an incentive to buy goods with cash rather than on credit, even if this meant an uneven time path of consumption.

Veterans may have found borrowing easier as the date of bonus disbursement approached: some narrative evidence reports loans being targeted to veterans in advance of the bonus payment. However, the combination of the household consumption survey results identified off the timing of the bonus disbursement and narrative evidence suggests that liquidity constraints remained binding for many veterans through spring 1936. The *Magazine of Wall Street* wrote on May 9, 1936 (Weldon 1936, p. 77), "[A] great many veterans lack credit standing and have not been able to spend in anticipation of the windfall."[37]

B. *Expectations of Higher Future Income*

For liquidity constraints to generate a high MPC, there must be a reason why the constraint binds, why the consumer would like to borrow but cannot. The most obvious reason why veterans may have wished to borrow is that they expected their income in the future to be higher. A consumption smoothing motive would then lead them to want to borrow against their future income. In this situation, veterans might have spent much or even all their bonus in the months after receipt.

Veterans may have expected higher future income because most probably saw their incomes rise rapidly in the three years leading up to the 1936 payment of the bonus. Real per capita disposable personal income rose 19 percent from 1933 to 1935. And while it is not possible to directly measure veterans' expectations, economic forecasters in early summer 1936 expected future growth.[38] A concrete measure of these expectations is the behavior of the stock market. In the four years from its low in June 1932, the S&P composite index rose 208 percent. In the year prior to the bonus disbursement, from June 1935 to June 1936, it rose 45 percent.

[37] The full quote is as follows:

> *Beyond question* [*the bonus*] *will prove an important stimulus to total business activity, although, of course, two major uncertainties are involved in attempting to forecast the results with any degree of accuracy. For one thing, not all of the approximately $2,000,000 of bonus money will be spent. For another thing, it is quite certain that some part of it has already been spent in anticipatory resort to credit and, in less measure, in withdrawals from present savings. On the other hand a great many veterans lack credit standing and have not been able to spend in anticipation of the windfall.*

On the subject of car purchases in advance of the bonus payment the industry trade publication *Automotive Industries* reported on June 20, 1936 (Gronseth 1936, p. 857) "While some advance business has been done on credit in anticipation of the bonus, the amount is said to be small."

[38] A representative quote comes from the *Magazine of Wall Street* (June 20, 1936, p. 296) under the headline "Signs of Business Progress":

> [*T*]*here is every indication that the usual summer lag is to be less than normal. Automobile assemblies have held up remarkably well; the steel industry is operating at a high rate of capacity; while retail trade will be stimulated at an unusual time by the payment of the solders' bonus.*

The *Magazine of Wall Street* did not venture a guess about economic conditions beyond the summer, but the tone suggests optimism.

C. *Demand for Autos and Housing*

In theory, expectations of higher future income combined with liquidity constraints could generate a MPC as high as one. In practice it seems unlikely that this alone could explain the veterans' high MPC. Even if many veterans expected higher future income, many must also have remained pessimistic after the decline in income between 1929 and 1933. It seems likely that a key additional contributor to high spending from the bonus was pent-up demand for autos and housing.

From the point of view of a household, spending on durables and housing is a form of saving. Thus there is little conflict between standard neoclassical models of consumption and a large marginal propensity to spend on durables. But although from the household's perspective purchasing a car and depositing money in a bank may be similar, for the aggregate economy, the implications are entirely different: in the former case, a transfer payment succeeds at increasing spending, in the latter it does not.

What will determine whether households choose to save a transfer or purchase a car? Berger and Vavra (2015) consider a business cycle model in which consumers face fixed costs of adjustment when buying durables. This leads to *Ss* policy functions. The key determinant of the marginal propensity to spend on cars (or other durables) is the gap between consumers' current stock of durables and their target stock. When this gap is large, many households will be close to their trigger point. In this case, a transfer payment—even a relatively small one—may lead many households to buy a car. This was likely the case in 1936. Despite the Depression, the period from 1929 to 1936 saw a rapid expansion of the paved road network (Field 2011) and a rapid decline in the quality-adjusted price of autos. Real, quality-adjusted car prices fell 17 percent from 1928 to 1936 (Raff and Trajtenberg 1997, table 2.6). Yet, the stock of passenger cars in use was lower at the end of 1935 than it was at the end of 1929 (Roos and Von Szeliski 1939). Many households were almost surely anxious to buy a new car as soon as their incomes permitted. The existence of many households close to such a threshold can explain why the bonus had such large effects on car purchases. Indeed, insofar as some veterans used the bonus to purchase a car that cost more than the bonus (either by borrowing or drawing down savings), it is even possible that for some the MPC exceeded 1.

For housing, a similar story likely applied. To a remarkable degree, residential investment did not recover in the 1930s. Total residential investment over the three years from 1933 to 1935 was less than that in the single year 1929. Field (1992) argues that the persistent low level of residential construction was due in large part to the physical and legal barriers left by haphazard land development in the 1920s. The effect was to make the stock of residential fixed assets, like the stock of automobiles, lower in 1935 than it had been in 1929 despite 4.5 percent population growth. Thus just as many veterans were likely to have been eager to purchase a new car, many were also likely to have been eager to buy a new house.

VI. Aggregate Effects of the Bonus

A. *A Back-of-the-Envelope Calculation*

This paper is primarily concerned with the effect of the bonus on veterans' spending, not with the aggregate effect of the bonus on output. But the two are related. The aggregate multiplier associated with the bonus was a function of two things: (i) how much of each dollar of bonus payments was spent, and (ii) the spending multiplier.[39] This paper provides evidence only on the former. Many recent theoretical and empirical papers provide estimates of the latter. Ramey (2011) summarizes this literature on the spending multiplier. She finds that the multiplier "is probably between 0.8 and 1.5" (p. 683) but "[r]easonable people could argue that the multiplier is 0.5 or 2.0 without being contradicted by the data" (p. 681).

There are, however, two difficulties in applying modern estimates of the multiplier to the veterans' bonus. First, most modern estimates of the multiplier, such as those considered by Ramey (2011), concern the effect of government spending on output, not the effect of consumption spending on output. It seems reasonable to assume that in its aggregate effects on output, consumer and government spending will be similar. For instance, Hall (2009, p. 184) writes: "[I]t is a fair presumption that the effects of higher consumer purchases are similar to the effects of higher government purchases." But of course this need not be the case. Second and more problematic, most estimates of the multiplier come from economic conditions quite different from those in 1936 and thus may not be directly applicable to spending from the bonus. In the United States in 1936, multipliers may have been larger than modern estimates suggest, given that nominal interest rates were at the zero lower bound, and that there were large amounts of excess capacity in the economy. Some papers find multipliers in the range of 1.5 to 2 in these conditions.[40]

The evidence from the household consumption survey suggests that the MPC from the bonus was 0.6 to 0.75. And the American Legion survey and cross-city building permit regressions suggest that veterans spent significant amounts of their

[39] As is customary, I consider the multiplier for the bonus ignoring any political economy effects of the bonus' passage on other spending and taxing decisions. The veterans' bonus was itself deficit financed, but its passage led to political pressure for higher taxes. Thus the veterans' bonus contributed to the enactment of the undistributed profits tax (the Revenue Act of 1936) in June 1936 (Romer and Romer 2012). This bill imposed taxes on undistributed corporate profits and also raised taxes on dividends. It did not affect other personal taxes. The political dynamic through which the veterans' bonus contributed to the passage of this tax increase can be compared to the way the American Recovery and Reinvestment Act (the Obama Stimulus) may have contributed to later spending cuts.

[40] For example, Gordon and Krenn (2010) find that the multiplier for government spending in 1940 was 1.8, and Almunia et al. (2010) estimate that the defense spending multiplier in the 1930s was 2.5. New Keynesian DSGE models can also generate multipliers in this range at the zero lower bound. See, for instance, Hall (2009), Woodford (2011), and Christiano, Eichenbaum, and Rebelo (2011).

There are two further reasons to think that applying modern estimates of the multiplier to conditions in 1936 may be conservative. First, the high MPC among veterans that I measure suggests that the MPC in the population as a whole may have been high. This implies that the spending multiplier in the mid-1930s may have been larger than is usually thought. In standard New Keynesian DSGE models, factors likely to raise the MPC, such as a large proportion of hand-to-mouth consumers, also tend to raise the multiplier. Second, modern empirical estimates of the multiplier reflect the fact that the US economy is open, and thus a significant amount of spending leaks abroad. By contrast, in 1936 the United States was much closer to being a closed economy: imports were 4 percent of GDP then versus 18 percent today (NIPA table 1.1.5). Furthermore, the level of real imports was unchanged from 1935 to 1936, despite the large increase in output and consumption (NIPA table 1.1.6). This strongly suggests that very little of the bonus was spent on foreign produced goods. Other things being equal, this ought to have increased the multiplier associated with the bonus.

bonus on investment as well as consumption. Thus it is reasonable to think that the total marginal propensity to spend on consumption and investment is likely to have been near if not above the high end of this range. In the following, I assume that it was 0.7. The implied multiplier associated with the bonus is then $0.7 \cdot S_m$, where S_m is the spending multiplier. One can compute the aggregate effect of the bonus on GDP growth as this multiplier times a measure of the bonus in real terms divided by real 1935 GDP:

$$(10) \qquad \underbrace{\left(\frac{Y_{36}}{Y_{35}} - 1\right)}_{\text{Actual '36 growth}} - \underbrace{\left(\frac{Y_{36} - B \cdot \frac{P_{37}}{P_{36}} \cdot 0.7 \cdot S_m}{Y_{35}} - 1\right)}_{\text{Counterfactual '36 growth}} = \underbrace{\frac{B \cdot \frac{P_{37}}{P_{36}} \cdot 0.7 \cdot S_m}{Y_{35}}}_{\text{Bonus' effect on growth}},$$

where B is the nominal bonus amount and P_t is the price index in year t. I measure prices using the GDP price index; real output is measured in chained 1937 dollars. Table 12 shows the result of this calculation for several possible values of the spending multiplier. For most values of the spending multiplier, the high MPC suggests a significant effect of the bonus. Even if the spending multiplier were only 0.5, the bonus added nearly a percentage point to 1936 GDP. A multiplier of 1 implies an effect on GDP growth of 1.6 percentage points. An Okun's law coefficient of 2 would then imply a decline in the unemployment rate due to the bonus of 0.8 percentage points.

B. *The Aggregate Time Series*

Since the 1936 veterans' bonus was a single event, it is difficult to use aggregate time series to establish causal effects of the bonus. Still, the time series are suggestive. Table 13 compares the path of GDP, consumption, and investment in 1936 to that in other years of the recovery. The rapid increase in GDP in 1936 is obvious: whereas GDP grew 10.8 percent in 1934 and 8.9 percent in 1935, it grew 12.9 percent in 1936. Most of the increase in growth was driven by consumption. Consumption *growth* increased by 4 percentage points from 1935 to 1936. This aggregate increase in consumption was reflected in subcategories: June to June increases in department store sales, variety store sales, and auto sales were all larger in 1936 than they had been in 1934 or 1935 (US Department of Commerce, August 1936; Telser 2003).

All this is not easily explained by factors other than the bonus. Federal government spending apart from the bonus did rise by roughly 2.5 percent of GDP in 1936, but this was less than the increase in 1934, and thus seems ill-suited to explaining more rapid output and consumption growth. And monetary factors were if anything contractionary in 1936. Broad money supply growth slowed from 14 percent in 1935 to 11 percent in 1936. (In 1936, short-term interest rates were at the zero lower bound, as they had been since 1933.) No doubt it would be possible to construct a story in which because of long lags, money growth in past years explains the extraordinary growth of output in 1936. But a more plausible story is that the bonus was the key factor raising 1936 growth well above its 1934 or 1935 levels.

Table 12—Effect of the Bonus on 1936 GDP Growth

Spending multiplier	Effect on 1936 growth (percentage points)
0.5	0.8
1.0	1.6
1.5	2.4
2.0	3.3

Source: See text.

Table 13—Real Output, Consumption, and Investment (*Billions of 2009 Dollars*)

	GDP	Consumption	Investment
1933	778.3	637.6	27.3
1934	862.2	683.1	45.2
1935	939.0	724.6	78.6
1936	1,060.5	798.3	99.3
1937	1,114.6	827.8	122.3

Source: NIPA table 1.1.6

Compared to monetary policy, the bonus can also more easily account for why so much of 1936 growth was driven by consumption.[41]

VII. Conclusion

This paper studies the effects of a 1936 payment to World War I veterans totaling 2 percent of GDP. I find that veterans quickly spent the majority of their bonus. The primary evidence comes from the 1935–1936 Study of Consumer Purchases. Using a differences-in-differences estimation strategy, I estimate that the marginal propensity to consume was between 0.6 and 0.75. This result is robust across a variety of specifications and suggests potentially large aggregate effects on 1936 GDP growth.

This evidence of a high MPC is supported by four additional sources. Cross-state and cross-city regressions demonstrate that auto sales and residential building permits increased more in places with more veterans in the population. An American Legion survey shows that out of every dollar of bonus payments, veterans planned to spend more than 60 cents. Narrative evidence, in particular newspaper articles, suggests considerable spending out of the bonus. And the aggregate time series show an increase in consumption in 1936 that is difficult to explain by factors other than the bonus.

[41] Readers may wonder what role, if any, the bonus played in the 1937–1938 recession. The boom year of 1936 was followed by slower growth in 1937 and actual contraction in 1938. The backside of the bonus can explain why growth slowed from 1936 to 1937: if large amounts were spent from the bonus in 1936, then a return of spending to trend in 1937 would be reflected in slower growth. This argument does not imply that the bonus lowered the level of output or welfare in 1937, only that it reduced *growth* in that year.

As pointed out by Romer (1992), Irwin (2012), and Hausman (2015) it is less clear that fiscal policy, and the veterans' bonus in particular, can account for the 1937–1938 recession. The key problem is the magnitude of the bonus. The bonus, a 2 percent of GDP payment, cannot account for the change from 12.9 percent GDP growth in 1936 to −3.3 percent GDP growth in 1938 (NIPA table 1.1.1).

Would the bonus have led to as much spending at other times? Perhaps not. Several features of the 1936 economy were uniquely conducive to large effects from a transfer payment. Liquidity constraints were pervasive and were made binding by expectations of higher future income. Stocks of autos and housing were low, so many households chose to buy a car or house rather than save their bonus. And since the economy was in a liquidity trap, the spending multiplier is likely to have been high.

This argument suggests that the MPC and therefore the aggregate effects of government transfers can depend as much on the state of the economy as on the structure of the payment. The period after a business cycle trough could well be when transfer payments have the largest effects, since recipients may expect higher future incomes and are anxious to replace old durables. Conversely, transfer payments may not be the ideal policy tool at the beginning of recessions, since recipients may then fear declines in income and are less likely to need to replace durables. Periods when liquidity constraints are widespread and unemployment is high but falling may well be when fiscal transfers will have the greatest impact.

Data Appendix

Introduction:

- The size of the bonus and number of World War I veterans: Veterans' Administration (1936), pp. 23–24.
- The bonus as a share of 1936 GDP: the ratio of the bonus to 1936 nominal GDP (NIPA table 1.1.5).
- Per capita income (for comparison to the bonus amount): It was $530 in 1936 (NIPA table 2.1).
- New car price (for comparison to the bonus amount): in 1936, the cheapest Ford and cheapest Chevrolet were $510 (*Automotive Industries*, "Ford Prices," November 14, 1936, p. 666).

Section VB:

- Real per capita disposable personal income increase from 1933 to 1935: NIPA table 2.1.
- Stock price data: These are from Robert Shiller. See http://www.econ.yale.edu/~shiller/data.htm.

Section VC:

- Residential investment from 1933 to 1935: NIPA table 1.1.3.
- Stock of residential fixed assets: BEA fixed asset table 5.2.
- Population growth: http://census.gov/popest/data/national/totals/pre-1980/tables/popclockest.txt.

Section VIA:

- GDP price index: NIPA table 1.1.4.
- Real output in chained 1937 dollars: NIPA table 1.1.6a.

Section VIB:

- Government spending: NIPA table 1.1.5.

- Broad money supply growth: NBER macrohistory series m14144a, which is computed from underlying data in Friedman and Schwartz (1963), *Monetary Statistics of the United States*.

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