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WP2013-06

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

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June 17, 2013

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BEHL Working Paper WP2013-06| June 17, 2013
http://behl.berkeley.edu/wp/2013-06

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Keywords: Great Depression, Fiscal Policy, Consumption

JEL classification: N120, N320, E210, E620

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"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, 6/25/1936 (Roth 2009, p. 172).

1 Introduction

The U.S. recovery from the Great Depression was nearly as exceptional as the Depression itself. After falling 27 percent between 1929 and 1933, real GDP rose by 43 percent between 1933 and 1937. Indeed, the economy grew more rapidly between 1933 and 1937 than it has during any other four year peacetime period since at least 1869.[1] The most rapid growth came in 1936, when real GDP grew 13.1 percent and the unemployment rate fell 4.4 percentage points.[2] Conventional explanations of the rapid recovery emphasize the economy's self-correcting mechanisms and the effect of expansionary monetary policy and resulting expectations of inflation. The literature almost universally dismisses fiscal policy as a primary source of recovery before World War II.[3] Economists have generally accepted E. Cary Brown's (1956) statement that "Fiscal policy . . . seems to have been an unsuccessful recovery device in the 'thirties–not because it did not work, but because it was not tried" (pp. 863-866).

In fact, this paper demonstrates that fiscal policy, though inadvertent, was tried in 1936, and a variety of evidence suggests that it worked. The government paid a large bonus to World War I veterans in June 1936, and within six months veterans spent roughly 70 cents out of every dollar received. A back-of-the-envelope calculation suggests that absent the veterans' bonus, GDP growth in 1936 would have been about 2.5 to 3 percentage points slower and the unemployment rate 1.3 to 1.5 percentage points higher.

After years of demonstrations and lobbying by veterans' groups, in 1936 congress authorized

[1]Data are from NIPA table 1.1.6 and Romer (1989).

[2]NIPA table 1.1.1 and Darby 1976.

[3]Eggertsson (2008) argues that deficit spending under Roosevelt helped make monetary expansion credible and thus contributed to higher inflation expectations. But like other authors, Eggertsson does not emphasize the direct stimulative effects of fiscal policy in the 1930s.

a deficit-financed payment of $1.8 billion to 3.2 million World War I veterans.[4] The bonus was 2.1 percent of 1936 GDP,[5] 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,[6] and enough money to buy a new car.[7] 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 the effects of the bonus. Only one paper, Telser (2003), examines the veterans' bonus in detail. Telser studies a variety of time series and concludes that the bonus "brought a large measure of recovery to the economy" (p. 240).[8] But 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's impacts.

In addition to revisiting the time series data, I exploit three other sources of evidence on the bonus's effects. First, I use a 1935-36 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 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. I outline a proof that, given a set of reasonable assumptions, this procedure provides consistent estimates of spending from the bonus.

[4]Data on bonus amount and number of veterans are from Veterans' Administration 1936, pp. 23-24.

[5]This is the ratio of the bonus to 1936 nominal GDP (NIPA table 1.1.5).

[6]Per capita personal income was $535 in 1936 (NIPA table 2.1).

[7]According to *Automotive Industries*, 11/14/36, p. 666 the price of the cheapest Ford and cheapest Chevrolet in 1936 was $510.

[8]Using annual regressions, Telser finds evidence that federal deficits were correlated with consumption growth in the 1930s, evidence he interprets as supportive of a large effect of the bonus. Telser also graphically examines monthly data on industrial production, wholesale prices, and department store sales. He argues that department store sales in particular suggest large effects of 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 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 third source of evidence on the bonus's 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. 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.

By comparison, cross-state regressions provide little useful information on the bonus's employment impacts. This is not surprising. Insofar as veterans spent their bonus on durable, traded goods like cars, large aggregate effects would be consistent with no relationship at the state level. In fact, employment in Michigan grows far more in 1936 than in any other state, exactly as one would expect given the boom in new car sales induced by the bonus.[9]

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 consume 40 cents out of every dollar and to

[9]These results can be compared to those in Fishback and Kachanovskaya (2010). They examine the cross-state multiplier from all types of federal spending in the 1930s. Like me, they find large effects on auto sales, but their results for income and employment are mixed, possibly reflecting the effects of spillovers.

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's effects translate directly into a measure of the bonus's aggregate impact. The effect of the bonus on the economy as a whole was a function not only of the recipients' MPC, but of general equilibrium effects that could have amplified or diminished the initial spending impulse. A plausible simple calculation suggests that the multiplier associated with the bonus was slightly above one, and hence that the bonus added 2.5 to 3 percentage points to 1936 GDP growth.

This paper contributes to two literatures. The first is on what explains rapid U.S. growth after 1933.[10] 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 (Bernanke and Parkinson 1989, Friedman and Schwartz 1963). Other authors dispute that there was anything natural or inevitable about rapid recovery post-1933. Eichengreen and Sachs (1985) do not focus on the U.S. 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 U.S. 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" (p. 757). Romer complements earlier work by Temin and Wigmore (1990) who argue that the departure of the U.S. 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. More recently, Eggertsson (2012) finds that the National In-

[10]Cole and Ohanian (2004) argue that the recovery was in fact weak. This is in part because they emphasize the position of the economy in 1939 relative to that in 1933, and thus incorporate in their comparison the ground lost during the 1937-38 recession.

dustrial Recovery Act may have been expansionary by raising prices and expected inflation and thus lowering the real interest rate. While this paper does not dispute the importance of self-correcting mechanisms and of monetary policy for the recovery, it suggests that a full explanation must also include a large role for fiscal policy in 1936.

This paper also adds to the literature on the consumption response of households to fiscal transfers.[11] 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 U.S. 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's effects relatively 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 large predictable payments is likely to be small.[12] 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.

I proceed in the next section by providing background on the veterans' bonus. Section 3 reports results from the 1935-36 consumer expenditure survey. Section 4 reports results from cross-state and cross-city regressions. Section 5 compares these findings to tabulations from a large survey of veterans conducted by the American Legion and to narrative evidence. Section 6 considers reasons why the MPC from the bonus was so high. Section 7 discusses the aggregate implications of my empirical results. Section 8 concludes.

[11] Recent empirical studies include Souleles (1999), Hsieh (2003), Shapiro and Slemrod (2003a, 2009), Johnson, Parker, and Souleles (2006), and Parker, Souleles, Johnson and McClelland (2013).

[12] The bonus payment was predictable since Congress passed the legislation authorizing payment in January 1936, but payment did not occur until June.

2 Background on the Veterans' Bonus

2.1 Road to Passage

Agitation for additional payments to World War I veterans began soon after the end of the war. Veterans were motivated in part by the legacy of large pensions to Civil War veterans (Daniels 1971, Dickson and Allen 2004). Civil War pensions were roughly as generous as social security benefits are today, paying approximately 30 percent of the annual unskilled wage (Costa 1998, p. 197). Civil War pensions were also an enormous share of federal government spending. In 1893, for example, 43 cents of every dollar in the federal budget went to civil war veterans' pensions (Rockoff 2001). In addition to this historical legacy, World War I veterans could reasonably argue that they had been underpaid. Base pay for a soldier was one dollar a day (Dickson and Allen 2004). By contrast, in 1918 the average manufacturing worker earned three dollars per eight-hour day.[13]

In the early 1920s, Congress considered numerous bonus bills.[14] Against the arguments for the bonus, opponents stressed the large cost. Opposition to the bonus was also motivated by racism: many did not want to see the government make large payments to African Americans (Dickson and Allen 2004, p. 23). Despite these worries, the House and Senate passed a bonus bill in 1922, only to have it vetoed by President Harding. In 1924, a new bonus bill was introduced which proposed that the bonus not be paid until 1945, thus eliminating any immediate impact on the federal budget. President Coolidge vetoed the bill. This time, however, Congress overrode the veto. The World War Adjusted Compensation Act (the 'Bonus' Act) become law on May 19, 1924.

The law promised World War I veterans payments in 1945 of approximately $3 for each day they had served in the army in the U.S. and $4 for each day served abroad. Confusingly, the law is often described as granting veterans $1 for each day served in the U.S. and $1.25 for each day served abroad. However, these amounts were arbitrarily increased by 25 percent

[13] NBER Macrohistory series a08050.

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

and then accrued interest for 20 years. Hence the values at maturity were approximately $3 and $4 per day served. These values are approximate since, technically, veterans were issued insurance policies whose eventual 1945 payouts depended slightly on age as well as length of service (Veterans' Administration, 1936).[15] 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 to a movement for earlier cash payment of the bonus. Congress took a step in this direction 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). These loans were in effect early, discounted bonus payments since they did not need to be paid back; rather a veteran could choose to simply have the amount of the loan plus 4.5 percent per-annum interest deducted from the amount due to him in 1945. In 1932, this interest rate was lowered to 3.5 percent (Veterans' Administration, 1931). Unsurprisingly, many veterans took advantage of these loans: the government dispensed 2 million loans worth one percent of GDP between March and May 1931 (Administrator of Veterans' Affairs 1931, p. 42; Cone 1940).[16]

Despite their ability to take loans, veterans continued to demand immediate cash payment of the entire, non-discounted, value of their bonus. Tens of thousands of veterans camped in Washington, DC from May to July 1932 to lobby Congress and the President for immediate payment. Their lobbying efforts were unsuccessful, and Hoover allowed General Douglas MacArthur to use soldiers and tanks to evict the veterans from Washington. Soldiers burned down the shacks that the veterans had occupied in Anacostia and drove them out of the city.

[15]The precise features of the bill are described in the 1936 *Annual Report of the Administrator of Veterans' Affairs* (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."

[16]These loans are an interesting historical analog to a proposal from Miles Kimball that the Federal Government give loans to all Americans, as a way of providing fiscal stimulus without adding to the government's long-run debt (Kimball 2012). For a discussion of what evidence the 1931 loans to veterans provide for the possible effects of such "Federal Lines of Credit" see `http://blog.supplysideliberal.com/post/30037326807/joshua-hausman-on-historical-evidence-for-what-federal`.

This forcible eviction provoked a political reaction that helped propel Franklin Roosevelt to victory the next year.

Although a political beneficiary of the veterans' encampment in Washington, Roosevelt was no more sympathetic to their cause than Hoover. Indeed, not only did Roosevelt oppose the bonus, in his first budget he cut pension benefits for disabled veterans. But Roosevelt was more diplomatic than Hoover. When hundreds of veterans returned to Washington in May 1933, Eleanor Roosevelt went to see them. The saying went "Hoover sent the army. Roosevelt sent his wife" (quoted in Dickson and Allen 2004, p. 216). The Roosevelt administration also offered veterans employment by waiving the usual age requirements for the Civilian Conservation Corp.

A small number of veterans marched on Washington for the third time in spring 1934. In response, Roosevelt offered them employment building the overseas highway from Miami to Key West - conveniently far from Washington. This led to tragedy in 1935. On September 2nd, the most powerful hurricane to ever make landfall in the United States struck the Florida Keys (Drye 2002). The hurricane, though powerful, was small, and it made landfall in the sparsely inhabited upper Keys. Thus only 250 non-veterans died. But the hurricane obliterated several camps for veterans working on the overseas highway, killing more than 250 veterans. The Roosevelt administration was widely blamed for not evacuating the veterans in advance despite ample warning from weather forecasters. An evacuation train was sent, but it arrived too late and was itself destroyed. The hurricane likely both inspired veterans to push harder for the bonus and made it more difficult for the administration to oppose payment. Public opinion swung in favor of veterans and the bonus amidst news stories questioning the administration's handling of the disaster.[17] A December 1935 Gallup poll found that a majority of Americans favored payment of the bonus.

In addition to the hurricane, three other factors created a political climate more favorable for the veterans in late 1935. First, 1936 was an election year, making politicians understandably reluctant to alienate a large voting bloc (*New York Times*, 10/3/35, p. 22). Democratic party

[17]Most famously, Ernest Hemingway (1935), then a resident of Key West, wrote an op-ed entitled "Who Murdered the Vets?: A First-Hand Report on the Florida Hurricane."

leaders, including Roosevelt, were concerned that the demagogic Catholic priest, Father Charles Coughlin, would run in 1936 to the left of Roosevelt. Father Coughlin advocated payment of the bonus and had the potential for significant support among veterans (Ortiz 2010). Second, years of high New Deal relief expenditures had made it more difficult for the administration to argue that payment of the bonus was unaffordable (*New York Times*, 12/1/35, p. 7). Finally, whereas previous iterations of the bonus bill had proposed that the bonus be paid via money creation, the 1936 bill proposed more traditional deficit-financing (Daniels 1971).[18]

These factors made the passage of the bonus a nearly forgone conclusion. The house and senate passed the bill on January 10$^{\text{th}}$ and January 20$^{\text{th}}$. Roosevelt vetoed the bill on balanced budget grounds, but Congress easily overrode the veto, and the bill became law on January 27, 1936. No one doubted this outcome: the administration even began printing bonus application forms before congress voted to override Roosevelt's veto (Daniels 1971).

2.2 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 (Administration of Veterans' Affairs 1936). Table 1 illustrates the effect of the law on a hypothetical veteran due $1000 in 1945 who took a loan of $500, the maximum allowable, in 1931. Such a veteran - who would have been typical - gained $491 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.

[18]While the veterans' bonus was itself deficit financed, 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 demands for spending cuts.

Table 1: Example of 1936 bonus's effect

	Pre 1/27/36 Law	Post 1/27/36 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 1931-36	$ 87.50	$ 87.50
Interest accrued on loan 1931-45	$ 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%)		$ 262.34

Note: Column 1 shows the financial situation of a hypothetical veteran under the pre January 1936 bonus legislation. This hypothetical veteran was due $1000 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 ($1000) 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% interest until 1945. If he left his entire bonus with the government, he would receive $628 in 1945.

In June 1936, the government issued 1.76 billion of cashable bonds to 3.2 million veterans (Administrator of Veterans' Affairs, 1936). Therefore the average value of the cashable bonds (i.e. the face value of his adjusted service certificate net of outstanding loans) received by a veteran in June 1936 was $\frac{1.76}{.0032} = \$547$. A veteran could choose to hold onto his bonds rather than cash them. Veterans were issued bonds in $50 denominations and could cash as many or as few of them as they desired (Administrator of Veterans' Affairs, 1936 p. 23). This feature of the bill led some to predict that the immediate effect on the federal deficit would be small.[19] In fact, however, most bonds were cashed immediately. Out of $1.76 billion of bonds issued to veterans through June 30, 1936, $1.2 billion were redeemed for cash in June and July 1936. A further 200 million were redeemed in late summer and fall (Cone 1940). 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,

[19]See for instance *The Times Picayune*, 1/28/36, p. 1.

since the 3% interest paid on bonds was above the legal maximum interest rate paid on savings accounts (Telser 2003).[20]

Table 2: The magnitude of the bonus

	1936	2012	2012 bonus equivalent
Per-capita annual income	$ 535	$ 42,736	$ 43,661
Average annual wage of federal emergency workers	$ 595	-	-
Average hourly earnings in manufacturing	$ 0.62	19.08	$ 16,853
CPI (Index, 1936=100)	100	1656	$ 9,053
Nominal house prices (Index, 1936=100)	100	2506	$ 13,702
Price of cheapest Ford	$ 510	$14,000	$ 15,009

Note: Aveage hourly earnings in manufacturing are for production workers (wage earners) only. Sources: Per capita income: NIPA table 2.1; annual wage of federal emergency workers: Darby 1976; average hourly earnings in manufacturing: Sayre (1940, p. 116) and FRED series AHEMAN; CPI: FRED series CPIAUCNS; house prices: Robert Shiller, `http://www.econ.yale.edu/~shiller/data.htm`; Ford price: *Automotive Industries*, 11/14/36, p. 666 and `http://www.ford.com`.

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. If the size of the 1936 bonus is benchmarked to per capita income, then an analogous payment in 2012 would be more than $43,000. Since manufacturing wages have risen much less than per capita income, the analogous 2012 payment relative to manufacturing wages would be roughly $17,000. The last three rows of the table repeat this exercise for the consumer price index (CPI), Robert Shiller's house price index and the price of the cheapest Ford car. Since prices have risen less than nominal incomes, these comparisons suggest smaller amounts for an equivalent size payment today.

[20]Of course, veterans may also have wished to cash their bonds in order to pay off higher interest rate debt.

2.3 Who were World War I veterans?

To understand the implications of the veterans' bonus for the impact of fiscal transfers more generally, it is important to know who World War I veterans were. Were they similar to the general population or were they a select group? In two obvious ways, veterans were different. Veterans were all male, and they were concentrated in a narrow age range. Men who were 5 years old or 60 years old in 1917 did not serve in the Army.

Table 3: Comparison of veterans and non-veterans

	Veterans	Non-veterans
Married	77.7	78.6
Employed	90.5	89.9
Black	6.7	9.8
Urban	67.8	60.7

Note: Units are percent. Comparison is for men age 28 to 45 in 1930.
Source: IPUMS 5% sample of the 1930 Census (Ruggles et al. 2010).

Among men of service age, World War I veterans were similar to the population as a whole. Table 3 uses 1930 Census data to compare veterans and non-veterans.[21] Nearly identical proportions of veterans and non-veterans were married and employed in 1930. There is a somewhat larger gap between the groups in the last two categories in the table, the percent black and the percent living in urban areas. This reflects the difficulty blacks had in volunteering for the army in the first world war, despite their eligibility for the draft (Kennedy 1980, p. 162). That more veterans than non-veterans 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 4). Overall, however, it is the similarity between veterans and non-veterans that is striking. Their veteran status excepted, veterans looked much like the rest of the population.

[21]Throughout this paper, I use data from the 1930 rather than the 1940 Census for two reasons. First, in the 1930 Census all respondents reported on their veteran status (Bureau of the Census 1933, p. 1395). By contrast, in 1940, veteran status was a sample-line item reported only by 5 percent of respondents (Bureau of the Census 1943, p. 172). 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 me with 100 times more veteran observations than the 1940 Census.

3 Evidence from the 1935-36 Study of Consumer Purchases

3.1 The 1935-36 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 1935-36 Study of Consumer Purchases did not record veteran status, I am able to use it to infer the effect of the bonus on household spending. To do so, I use information from the 1930 Census to compute the probability that a household in the consumption survey included a veteran.

In 1935-36 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 in large, mid-sized, and small cities across the country. Likewise, the Bureau of Home Economics in 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 twelve month period. The Inter-University Consortium for Political and Social Research (ICPSR) digitized a random sample of 3,100 urban and 3,034 rural responses.[22] 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's effects. Hence in my analysis I use only the urban sample.[23]

[22] See `http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/08908/detail`.

[23] 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.

3.1.1 Survey sample

The Bureau of Labor Statistics selected households for the urban expenditure survey with the following three-step procedure. First, in each of six geographic regions,[24] one or two large cities, two or three mid-sized cities, and several small cities were selected (Natural Resources Committee 1939).[25] Then a random sample was taken of all households in the municipality, and interviewers obtained detailed income information from all U.S.-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 (Bureau of Labor Statistics 1941a). The criteria most likely to influence my results are: (1) only married couples and families were surveyed; (2) households must not have received any relief payments during the schedule year; and (3) white families must have had an income of at least \$500 in large cities and \$250 in small cities.[26][27] A priori it is not obvious in which direction the composition of the urban household survey might effect my measurement of the MPC. This is an issue to which I will return when discussing my results.

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

[25]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.

[26]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.

[27]In addition, if a family had a low income, it was included only if a member was employed as a wage earner or clerical worker; if a family had a high income, it was included only if its member(s) were employed in "the business or professional groups" (Bureau of Labor Statistics 1941a, p. 373).

There were other, noneconomic criteria for inclusion in the survey. In particular, according to the Bureau of Labor Statistics (1941a, p. 375) families were excluded from the expenditure survey if:

"

1. The family did not occupy a home in the community for at least 9 months of the schedule year.
2. The family moved from one dwelling unit to another between the end of the schedule year and the date of the interview.
3. The family did not have access to housekeeping facilities for at least 9 months of the schedule year.
4. The family had more than the equivalent of one roomer and/or boarder in the household for 52 weeks of the report year.
5. The family had more than the equivalent of one guest for 26 weeks."

3.1.2 Survey procedure

Households were interviewed over the course of 1936. In most cases, households were interviewed twice: once to obtain income information and again 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.[28]

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

> The supervisory staffs in the regional administrative offices and in the local collection officies consisted of college graduates with training in the social sciences and statistics, and in many cases with experience in the direction of surveys. . . .
>
> As a further assurance of the accuracy of the data collected, 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.

Appendix A contains an example of a completed expenditure schedule, and appendix B provides a detailed description of how I compute measures of consumption aggregates from the survey responses. Table 4 provides summary statistics for the 33 cities included in the urban sample.[29] It provides information on city populations and the number of World War I veterans

[28] One might worry that this is a source of bias if households interviewed after the bonus was paid selected whether or not to report on 1935 for reasons related to how they spent their bonus. In fact, results are qualitatively unchanged if one drops these households. See section 3.4.

[29] Although the ICPSR dataset has 3100 observations, the dataset I use has only 2745 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 data for the schedule year is missing, 3 households because the schedule period

Table 4: Household survey summary statistics

City	1930 pop.	% of pop. WWI vets	# consump. survey	# post bonus
Aberdeen, WA and Hoquiam, WA	34,400	4.2	34	8
Albany, GA	14,500	3.1	16	0
Atlanta, GA	270,300	3.3	239	76
Beaver Falls, PA	17,100	4.4	34	12
Bellingham, WA	30,800	4.4	36	17
Butte, MT	39,500	4.8	57	16
Chicago, IL	3,376,400	4.1	300	25
Columbia, SC	51,500	3.5	64	23
Columbus, OH	290,500	4.2	225	116
Connellsville, PA	13,200	2.5	32	2
Council Bluffs, IA and Omaha, NE	256,000	5.0	101	16
Denver, CO	287,800	4.1	150	26
Dubuque, IA	41,600	2.3	68	17
Everett, WA	30,500	3.5	34	12
Gastonia, NC	17,000	2.4	19	0
Greenfield, MA	15,420	4.0	46	3
Haverhill, MA	48,700	2.9	59	24
Logansport, IN	18,500	2.5	42	11
Mattoon, IL	14,600	3.3	48	20
Mobile, AL	68,200	3.7	126	31
Muncie, IN	46,500	3.5	74	31
New Britain, CT	68,100	2.8	39	9
New Castle, PA	48,600	2.0	48	16
New York, NY	6,930,400	3.2	171	48
Peru, IN	12,700	3.2	1	0
Portland, OR	301,800	4.7	196	50
Providence, RI	252,900	3.0	140	24
Pueblo, CO	50,000	3.2	77	23
Springfield, IL	71,800	4.3	122	33
Springfield, MO	57,500	3.6	73	45
Wallingford, CT	14,270	3.3	11	10
Westbrook, ME	10,800	4.7	45	5
Willimantic, CT	12,100	1.2	18	17
Total	12,813,990	3.6	2745	766

Note: The expenditure survey grouped together two pairs of cities: Aberdeen, WA with Hoquiam, WA and Council Bluffs, IA with Omaha, NE. Sources: Columns 1 and 2: IPUMS 5% sample from the 1930 Census (Ruggles et al. 2010); columns 3 and 4: ICPSR study 8908.

Table 5: Consumption category summary statistics

Category	Mean ($'s)	Standard deviation ($'s)
Total expenditure	1870	1217
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 appendix B.

living in each city in 1930. It also lists the number of observations in the ICPSR sample of the expenditure survey and the number of these observations for which the schedule year ends June 30, 1936 or later, after the bonus payment. Table 5 shows summary statistics for major categories of consumption. Food made up the largest share of consumption with housing a distant second.[30] 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 consumer expenditure (CE) survey.[31]

3.2 Specification

An ideal survey for this papers' 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

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.

[30]Perhaps the most surprising feature of table 5 is the large share of auto purchases and operations in total spending. By the 1930s, cars were not a luxury good. In 1936, there were 24.2 million registered passenger cars, almost one car for every five people (NBER macrohistory series a01108 and `http://www.census.gov/popest/data/national/totals/pre-1980/tables/popclockest.txt`).

[31]For more on consumption shares by category in the 1935-36 survey, see Costa (1999).

proxy for whether a bonus payment was received. One could then estimate

$$\text{Consumption over previous 12 months}_i = \alpha + \beta_1 \cdot \text{Veteran dummy}_i + \beta_2 \cdot \text{Post bonus dummy}_i + \beta_3 \cdot \text{Veteran dummy}_i \cdot \text{Post bonus dummy}_i + Z_i'\beta_4 + \varepsilon_i, \tag{1}$$

where "post bonus" is a dummy variable for whether the expenditure survey schedule year ended after the bonus was paid, and Z_i are control variables such as age and state.[32] This is a standard differences in differences regression with β_3 measuring the difference between the change in veteran consumption pre to post bonus and the change in non-veteran consumption pre to post bonus. Any changes to consumption common to both veterans and non-veterans will be differenced out and will not be reflected in β_3.[33] Along with a reasonable estimate of the size of the average bonus, an estimate of β_3 will provide an estimate of veterans' propensity to consume out of the bonus.

Although the survey did not ask about veteran status, it is still possible to identify β_3. To do so, 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% sample from this census to estimate the probability that a household contains a veteran conditional on age, race, and location. This probability then replaces the veteran dummy in equation (1). Thus the estimation equation is

$$\text{Consumption}_i = \alpha + \underbrace{\beta_1 \cdot \text{Prob. veteran}_i}_{\text{Not identifiable}} + \beta_2 \cdot \text{Post bonus dummy}_i + \beta_3 \cdot \text{Prob. veteran}_i \cdot \text{Post bonus dummy}_i + Z_i'\beta_4 + \varepsilon_i, \tag{2}$$

β_1 is no longer identifiable since the probability of being a veteran is a linear combination of

[32] The control variables Z are necessary if they are correlated with *changes* in veteran or non-veteran consumption. In any case, their addition is likely to improve the estimates' precision.

[33] Thus the existence of spillover (multiplier) effects from the bonus will not bias an estimate of β_3. That is, as long as spillovers from the bonus affected veteran and non-veterans equally.

the control variables Z_i. But, as I will show, the coefficient of interest, β_3 is identifiable.

The procedure is similar to the two-sample instrumental variables approach described in Angrist and Krueger (1992), Lusardi (1996), and Inoue and Solon (2010), although it differs in important ways. Most obviously, I resort to a two-sample procedure not because veteran status is endogenous, but because I do not observe it in the first sample.[34] Let Y be the outcome variable of interest, consumption, X be veteran status, and Z a vector of covariates correlated with X. In the typical two-sample instrumental variables problem Y and Z are observed in one-sample (in my case the household survey) and X and Z are observed in a second sample (in my case the 1930 census). Under the same assumptions needed for single-sample IV estimation, $\hat{\beta} = (\hat{X}'\hat{X})^{-1}\hat{X}'Y$ is a consistent estimator of β, where $\hat{X}$ are the predicted values for X from the least-squares regression of X on Z in the second sample.

My problem differs from the above in that I am interested in identifying the effect on consumption of veteran status interacted with the post bonus dummy, not the effect of veteran status on consumption. Thus if P is the post bonus dummy, the required exclusion restriction is not that $E[\varepsilon Z] = 0$ but that $E[\varepsilon(Z \cdot P)] = 0$. To see this, write the first stage regression

$$X_j = Z_j'\gamma + \mu_j, \tag{3}$$

and the second stage regression

$$Y_i = \alpha + (Z_i'\hat{\gamma})\beta_1 + P_i\beta_2 + ((Z_i'\hat{\gamma})P_i)\beta_3 + Z_i'\beta_4 + (Z_i'P_i)\beta_5 + \varepsilon_i. \tag{4}$$

Estimating 4 is equivalent to estimating

$$Y_i = \alpha + Z_i'(\beta_1\hat{\gamma} + \beta_4) + P_i\beta_2 + (Z_i'P_i)(\beta_3\hat{\gamma} + \beta_5) + \varepsilon_i. \tag{5}$$

[34] Card and McCall (1996) use a two-sample procedure for similar reasons. They wish to measure the effect of medical insurance coverage on worker compensation claims for Monday injuries, but they do not observe insurance coverage in their sample of injury claims.

It is thus possible to identify

$$\begin{pmatrix} \alpha \\ \beta_1\gamma + \beta_4 \\ \beta_2 \\ \beta_3\gamma + \beta_5 \end{pmatrix}$$

Given γ, β_3 is identifiable if and only if $\beta_5 = 0$. The variables used to predict veteran status must be uncorrelated with the pre to post bonus change in consumption except through veteran status. For example, while it poses no problem for identification of the MPC if race is correlated with consumption for reasons other than veteran status, it will bias my estimates if for reasons other than veteran status race is correlated with the pre to post bonus change in consumption. As the analogy with instrumental variables suggests, conditional on the exclusion restriction holding, estimation of (3) and (4) by least squares provides consistent coefficient estimates. Appendix C sketches a proof and provides more discussion of the necessary assumptions.

Since $Z_i\hat{\gamma}$, 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. 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.[35]

[35]Specifically, I repeat the following 1000 times:

1. Draw a bootstrap sample (i.e. a sample with replacement) of cities from each of the 9 strata in the household survey. For example, since one stratum is large cities in the east central region, the sample might have Chicago and Columbus, OH, or two instances of Chicago and no Columbus, OH. But it will for certain have at least one large city from the east central region.
2. Estimate the probability of being a veteran conditional on age, race, and location ($\hat{\gamma}$) on this bootstrap sample.
3. Estimate the equation of interest (2).
4. Save the coefficients.

3.3 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.[36] Specifically, I estimate:[37]

$$V_j = \sum_{h=1}^{3} \beta_h \mathbf{1}(g_j = g_h) + \sum_{k=1}^{17} \gamma_k \mathbf{1}(s_j = s_k) + \sum_{l=1}^{17} \alpha_l \mathbf{1}(g_j = 2)\mathbf{1}(s_j = s_l)$$
$$+ \sum_{m=1}^{3} \theta_m a_j^m + \sum_{n=1}^{3} \lambda_n \mathbf{1}(g_j = 2) a_j^n + \zeta r_j + \eta \mathbf{1}(g_j = 2) \cdot r_j + \mu_j. \quad (6)$$

Variables are defined as follows: 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;[38] a equals age, and r is an indicator variable for race. $\mathbf{1}$ denotes the indicator function. The predicted probabilities of being a veteran are fairly insensitive to the exact specification used, and my estimates of the MPC will be consistent regardless of whether the first stage is misspecified.[39] The particular specification in (6) is attractive because while fairly 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 U.S. born men married to U.S. born women in the 33 cities included in the urban portion of the household survey. This provides me with 64,149 observations, enough to

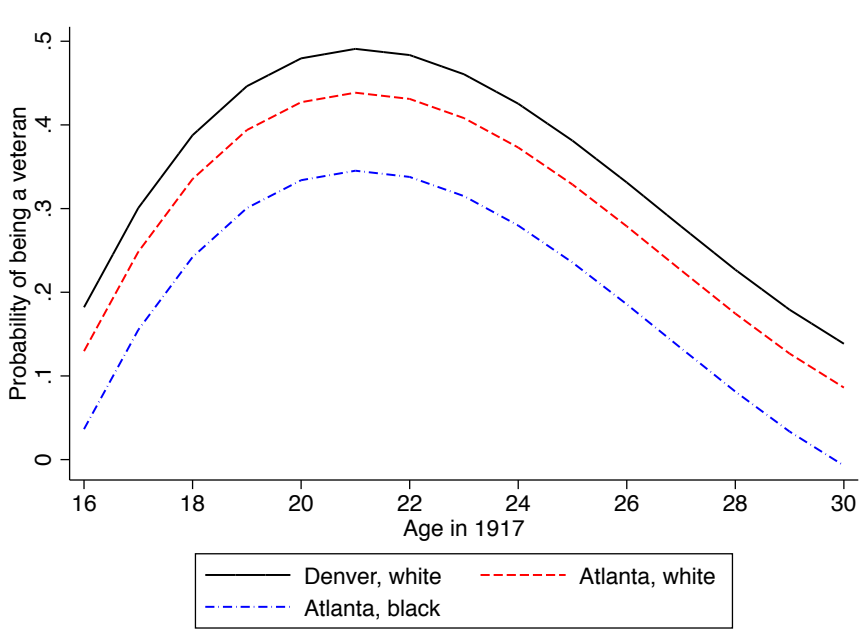
[36] Although it would undoubtedly produce more accurate estimates of the probability that a man was a veteran, using probit or logit in the first stage would be problematic since the first stage residuals would no longer be uncorrelated with the first stage regressors (an example of what's known as 'forbidden regression'). For details, see appendix C.

[37] In stata notation the right-hand side variables are "young old i.elig#i.race i.elig#i.state i.elig#c.age i.elig#c.age^2 i.elig#c.age^3" where elig = 1 - young - old.

[38] In three cases I combine states for the purposes of this regression: (1) Since the household consumption survey considers Omaha, Nebraska and Council Bluffs, Iowa as one city, I use one fixed effect for Iowa and Nebraska. (2) Since the only city 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% Census sample has only a tiny number of veterans in the city.

[39] See appendix C. As in the IV context, the key assumption is that the errors from the first stage are uncorrelated with the regressors, a property of OLS regardless of misspecification.

Figure 1: Variation in probability man is a veteran



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

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.[40] Figure 1 gives an example of how this probability varies with age, race and location.

In the second step, I estimate equation 2, where consumption is the dependent variable. Table 6 shows results for total consumption expenditure.[41] The specification in column 1 of table 6 uses all observations. Column 2, my preferred specification, excludes households in the consumer expenditure survey with total spending of more than $5000. 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 simply a linear combination of the controls. One might initially be concerned that the coefficient on the post

[40]The first stage R^2 is 0.21. Most of the variation comes from age, rather than race or location. See table 7.
[41]Appendix B describes exactly what is included in this aggregate.

bonus dummy is large and statistically significant, since this suggests that non-veteran as well as veteran spending changed pre to post bonus. But this is in fact exactly what one would expect given the upward trend in aggregate consumption over the period. Furthermore, if non-veterans 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.[42]

To interpret the coefficient on the interaction, recall that average household expenditure in the sample was $1870 (table 5), 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.

Table 6: Total expenditure and saving regressions

	(1) Total C	(2) Total C	(3) Insurance policies settled	(4) Gifts received
Post bonus dummy	264.1***	198.2***	-5.589	0.0779
	(70.52)	(43.18)	(4.292)	(6.854)
Interaction	647.0*	403.1**	95.93***	152.4***
	(379.4)	(169.6)	(22.87)	(46.44)
Omit if expen. > $5000	No	Yes	Yes	Yes
Observations	2745	2681	2681	2339
R^2	0.152	0.186	0.034	0.048

Bootstrap standard errors clustered at the city level in parentheses.

* $p < .10$, ** $p < .05$, *** $p < .01$

Note: See the text for a description of the controls.

Three factors were important in determining what size bonus a veteran received. (1) How many days a veteran served in the army; (2) whether those days were served in the U.S. or abroad, and (3) what size loan, if any, the veteran had taken against his bonus. Unfortunately,

[42]The magnitude of the coefficient on the post bonus dummy in column 2, 198, implies approximately a 10-15 percent increase in non-veteran 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).

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 Sunday June 14, the day before bonus payments were distributed, the *New Orleans Times Picayune* (section 6, first page) printed a table listing 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.[43]

Notwithstanding these caveats, I can use these figures to see how average bonus size was related to state population characteristics. In particular, I estimate across states i

$$\text{Average bonus}_i = \alpha_i + \beta_1 \text{Urban share}_i + \beta_2 \text{Black Share}_i + \beta_3 \Delta \text{Employment 29-36}_i + \varepsilon_i \quad (7)$$

The change in employment from 1929-36 is a proxy for a state's unemployment rate, since no state level unemployment rate estimates exist.[44] 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.[45] Column 3 of table 6 replicates the specification in column 2, but with this

[43]Furthermore, the totals in the table are close, but do not exactly match those from the Administrator of Veterans' Affairs.

[44]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% IPUMS sample from the 1930 Census. Data on the 1929-36 employment change are from Wallis (1989).

[45]According to the 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.

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 non-responses on the insurance-policies-settled question. (It may not have been intuitive that there 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 survey interview), households were also asked about the value of "Gifts in cash for current use from persons not members of economic family."[46] 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 many 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 inevitable uncertainty that comes with 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 whole were more likely to be poor and unemployed, it seems likely that the MPC as measured in the household survey is biased down.[47] If the results in table 6 were the only source of evidence on the MPC, it would be difficult to draw strong conclusions about the bonus's effects. Fortunately, we shall see that this initial evidence of a large MPC is confirmed by results from alternative specifications, and

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.

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

[47]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.

from other, independent sources of data.

3.4 Robustness

Table 7 shows robustness checks for the baseline total expenditure specification. For comparison, column 1 reproduces column 2 of table 6, my preferred specification. One concern about this specification 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 my estimation strategy 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. Column 2 of table 7 tests for this possibility by using only age and race, not geography, to predict veteran probability in the first stage. In fact, this results in a larger estimate of the spending response. As one can see from comparing the first stage R^2s, dropping geographic controls results in little loss of explanatory power for the probability of being a veteran. Most of the difference in the second stage coefficient is instead driven by the fact that without state fixed effects in the first stage, one must also drop state fixed effects from the second stage.[48] If one ignores the econometric issues and includes state fixed effects, the coefficient in column 2 is 446, much closer to that in column 1.

Another concern is that the same variables that predict veteran status might also predict the size of a veterans' bonus. For instance, whites might both be more likely to have been veterans 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. As a check on this, in column (3) I use only an age dummy variable to predict veteran status. The age dummy variable is equal to one for men age 28 to 45 in 1930, the range where men had a greater than 4 percent change of having served in the war. This dummy variable has little risk of being correlated with the size of the bonus. The result is a larger coefficient, again driven mostly by the absence of control variables in the second stage. Including controls in the second stage

[48]One cannot include variables in the second stage that are not in the first stage and have a consistent estimator, since consistency requires that the first stage residuals be uncorrelated with the second stage right hand side variables. See appendix C.

reduces the coefficient to 485. In any case, this result suggests that any bias resulting from variables like state and race predicting bonus size is leading me to underestimate to the MPC.

Table 7: Total expenditure robustness checks

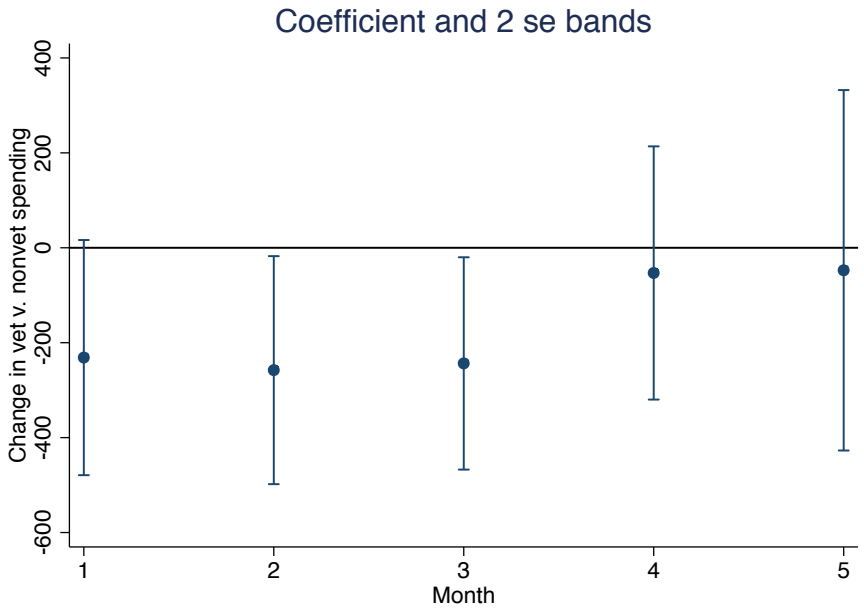
	(1) Baseline	(2) Age, race only	(3) Age dummy	(4) Post, rpt. '35	(5) Cutoff $5500	(6) Cutoff $4500
Post bonus	198.2***	117.5***	78.23*	199.9***	229.8***	197.9***
	(43.18)	(41.71)	(45.20)	(53.72)	(47.43)	(42.94)
Interaction	403.1**	520.8***	677.5***	460.1**	314.3*	396.8**
	(169.6)	(153.2)	(155.9)	(211.2)	(182.6)	(163.9)
1^{st} stage N	64,149	64,149	64,149	64,149	64,149	64,149
2^{nd} stage N	2681	2681	2681	2073	2695	2671
1^{st} stage R^2	0.213	0.212	0.124	0.213	0.213	0.213
2^{nd} stage R^2	0.186	0.136	0.029	0.205	0.182	0.189

Bootstrap standard errors clustered at the city level in parentheses
* $p < .10$, ** $p < .05$, *** $p < .01$
Notes: See the text for a description of the controls. In columns 1-4, households with total expenditure > $5000 are excluded.

A third 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, my coefficient would be biased up: I would observe more spending among veterans post bonus than actually occurred. To see if this effect is driving my results, in column (4) I drop all households interviewed after the bonus was paid (i.e. after 6/15/36) who chose to report on 1935. The measured MPC actually rises slightly.

A limitation if the household survey is the presence of outliers. As discussed when initially presenting the results, I exclude households with total expenditure greater than $5000 to avoid these problems. I chose the cut-off of $5000 since the above this a robust regression assigns households a weight of zero. The final two columns of 7 look at the effect of choosing slightly different cut-offs, $5500 in column (5) and $4500 in column (6). Raising the cut-off to $5500 means including outliers and hence reduces the precision of the estimate. By contrast, lowering the cut-off (column 6) results in little change to the coefficient or standard error.

Figure 2: Household consumption survey placebo test



A final check on my results is a placebo test. If I am measuring the true effect of the bonus, it should show up most strongly in June. There should be no discontinuity in spending between veterans and non-veterans before June. By contrast if I am picking up, say, different trends in consumption across age groups, one would expect to see this effect in other months. To test this, I perform the following exercise: first, I throw out all observations in the household survey from after the bonus payment. Then I estimate my baseline specification (equation 2) setting the post bonus dummy variable equal to months ranging from January to May 1936. Differential trends in consumption across age groups, states, or race should show up here in the same way they do in my results where I use June as 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 2 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 non-veteran spending. This is encouraging; it implies that my procedure is picking up spending from the bonus, not some underlying macro trend.

Overall these robustness tests are reassuring; they suggest that my results are not driven by

a particular specification or sample. If anything, my baseline results appear conservative: other specifications often yield higher point estimates. Nonetheless, any one dataset and estimation strategy inevitably comes with uncertainties. This motivates identifying alternative, independent sources of evidence on the bonus's effects. I turn to these in the following sections. But first I use the household survey to see what veterans bought with their bonus.

3.5 Expenditure categories

The detail of the household survey allows me to break down the spending response across consumption categories. Table 8 reproduces for several categories of consumption the baseline specification that excludes households with spending greater than \$5000.[49] 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.

[49] As argued above, these households are large outliers.

Table 8: Consumption category regressions

	(1) Auto purchases and ops.	(2) Housing	(3) Furniture / equipment	(4) Clothing	(5) Recreation	(6) Food
Post bonus dummy	47.77***	9.496	4.952	41.47***	9.325***	29.93**
	(10.77)	(8.425)	(5.244)	(7.735)	(3.132)	(12.77)
Interaction	127.4**	83.37***	21.71	32.93	24.86	35.15
	(60.30)	(29.38)	(20.43)	(27.08)	(15.76)	(45.65)
Observations	2681	2681	2681	2681	2681	2681
R^2	0.072	0.204	0.051	0.091	0.074	0.231

Bootstrap standard errors clustered at the city level in parentheses

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: See the text for a description of the controls. Households with total expenditure > \$5000 are excluded. 'ops.' means operations.

Table 9: Autos regressions

	(1) Auto purchases and ops.	(2) Auto purchases	(3) Car purchase	(4) Auto operations
Post bonus dummy	47.77***	28.26***	0.00386	19.51***
	(10.77)	(7.687)	(0.0146)	(6.903)
Interaction	127.4**	71.04*	0.215***	56.39*
	(60.30)	(37.89)	(0.0750)	(30.11)
Observations	2681	2681	2681	2681
R^2	0.072	0.039	0.047	0.075

Bootstrap standard errors clustered at the city level in parentheses

* $p < .10$, ** $p < .05$, *** $p < .01$

Notes: See the text for a description of the controls. Households with total expenditure > \$5000 are excluded.

Because of its importance for the overall spending response, in table 9 I look in more detail at the response of autos consumption to the bonus payments. Column 1 reproduces column 1 of table 8. 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.[50] 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 9 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.

4 Cross-state and cross-city evidence

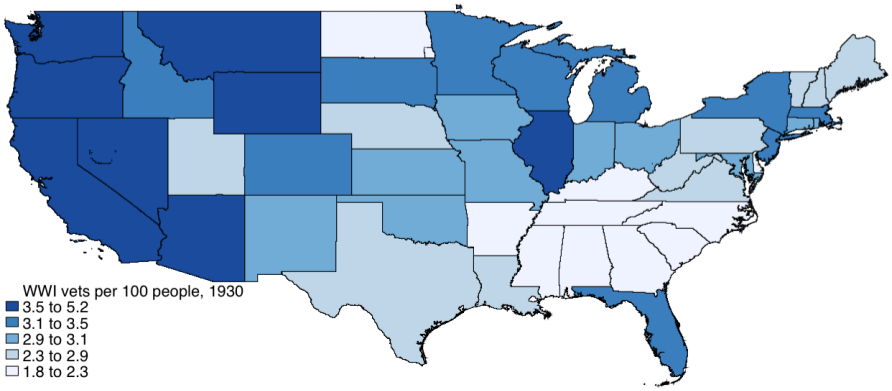
Another source of evidence on the bonus's effects comes from cross-state and cross-city regressions. States and cities varied in the proportion of their population made up of veterans, and hence in the amount of stimulus they received. Thus by relating variation across states and cities in veterans per capita with state or city level data on economic outcomes, it is possible to make inferences about the effects of the bonus.

4.1 The Geographic Distribution of Veterans

Veterans were unevenly distributed across the U.S. Figure 3 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

[50]It would be natural to estimate the response of car purchases with a tobit rather than a linear model. Indeed, measurement of household auto purchases is the example Tobin (1958) uses to motivate his estimator. However, my two-step procedure provides consistent estimates only if the second stage is linear. This reasoning is also why I estimate a linear probability model rather than a probit in column 3.

Figure 3: Veterans per 100 people in 1930



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

veterans per 100 people in Wyoming, the District of Columbia, and Nevada. States in the south tended to have fewer veterans while states west of the Mississippi tended to have more.

Two forces combined to lower the share of veterans in the south. First, blacks were less likely than whites to have served in World War I. In 1930, 2.1 percent of blacks were veterans, whereas 3.1 percent of whites were veterans.[51] Since black populations were much larger in the south, this mechanically led to lower overall shares of veterans in the population. This is not, however, the entire explanation for the low share of veterans in the former Confederacy. Even among whites, participation in World War I was rarer in the south. Data from the 1930 census show that in the states in the south census region, only 2.6 percent of the white population was a World War I veteran. Outside this region, the proportion was 3.3 percent. The difference could reflect different propensities to volunteer or different proportions of the white population qualified for military service for age or other reasons.

Some of the highest population shares of veterans were in western states. This is partly explained by veterans settling homesteads after World War I. The 1909 Enlarged Homestead Act and the 1916 Stock-Raising Homestead Act allowed larger homesteads. People were able to establish homesteads on arid land suitable for ranching but not for intensive cultivation (Gates 1977). The Federal government did not give specific preference to veterans, but many states

[51]Figures cited in this paragraph were computed from the IPUMS 5 percent sample of the 1930 Census.

did. For example, California gave loans to veterans that allowed them to purchase farms with only a 10 percent downpayment (Rowlands, 1943). Veterans were attracted to Wyoming in particular (Roberts, undated).

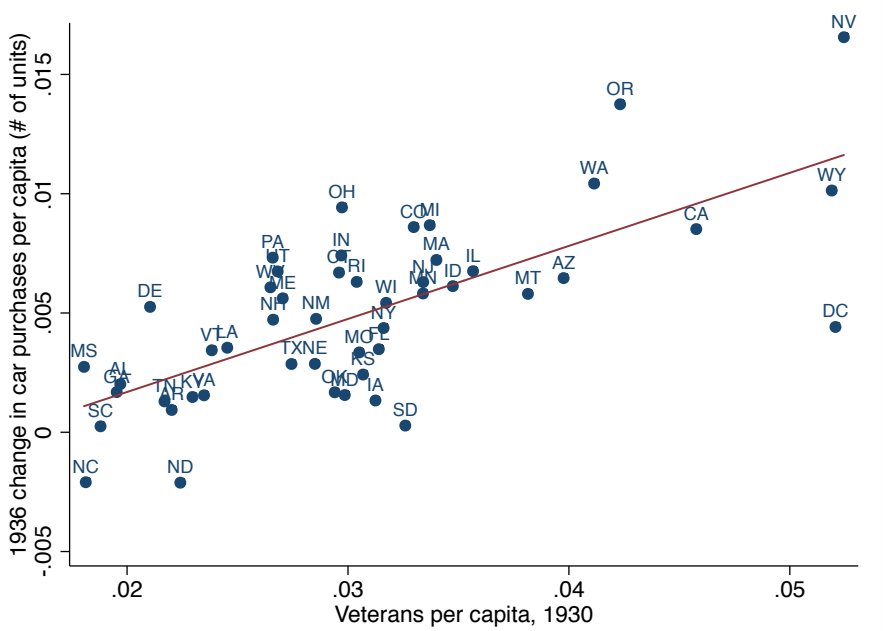
Variation in the number of veterans per capita in each state translated into variation in the fiscal impulse received in 1936. As discussed in section 2, veterans received different bonus payments depending on how long they had served in the military, whether they served overseas, and whether they took loans against their bonus. In practice, however, there was relatively little variation across states in the average payment received. 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 Administrator of Veterans Affairs (1936).[52] Hence I use the census data on veterans' share as a proxy for the actual stimulus in my estimation. However, it is worth noting that in the American Legion numbers, the correlation between veterans per capita and bonus payments per capita is .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.

4.2 Auto Sales

The effect of the veterans' bonus on new car sales is both of independent interest and a useful proxy for the bonus's general macro effects. Although cars are only one type of consumption, 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, spillover effects are likely to show up less in cross-sectional regressions using auto sales data. If a veteran spent his bonus on a car, the employment impact would show up in Michigan, but the car sale would show up in his home state.

[52]I obtained the American Legion figures from the *Times-Picayune* newspaper, 7/14/36. Reference librarians at the American Legion library were unable to tell me the source of the numbers.

Figure 4: Auto sales per capita and veterans per capita



Sources: Auto sales: *Automotive Industries* 2/22/36, p. 243 and 2/25/39, p. 208; population: BEA table SA1-3; veterans per capita: IPUMS 5% sample of the 1930 Census (Ruggles et al. 2010).

Figure 4 shows the per capita change in auto sales 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. An obvious concern is that this simply 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.

Regression results are shown in table 10. The first column shows estimates from the simple regression analogous to the scatter plot. Column 2 controls for the level of per capita new car sales in 1929, the interwar era sales peak. Per capita 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. However, adding this control to the regression has almost no effect on the coefficient (column 2). 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. While the size of the coefficient on veterans per capita falls, it remains highly statistically significant. The last two columns of the table return to the simple regression of the change in auto sales on veteran share, but restrict the sample of states. Column 6 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 7 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.

Table 10: Regression results for new car sales

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Veterans per capita, 1930	0.306***	0.315***	0.344***	0.209***	0.214**	0.276***	0.332*
	(0.0648)	(0.0875)	(0.0806)	(0.0692)	(0.0829)	(0.0750)	(0.191)
Per capita new car sales in 1929		-0.0116					
		(0.0600)					
Change in 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					-0.00302		
					(0.00462)		
Farm share of the population					-0.00654**		
					(0.00264)		
Excludes states with vets per cap < 0.02 or > 0.04						X	
Northeast and midwest only							X
Observations	49	49	49	49	49	38	21
R^2	0.492	0.493	0.500	0.604	0.585	0.225	0.155

Robust standard errors in parentheses

* $p < .10$, ** $p < .05$, *** $p < .01$

Sources: Auto sales: *Automotive Industries* 2/22/30, p. 267, 2/22/36, p. 243, and 2/25/39, p. 208; population: BEA table SA1-3; veterans per capita, farm share, and black share: IPUMS 5% sample of the 1930 Census (Ruggles et al. 2010).

The estimates in table 10 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, then 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.

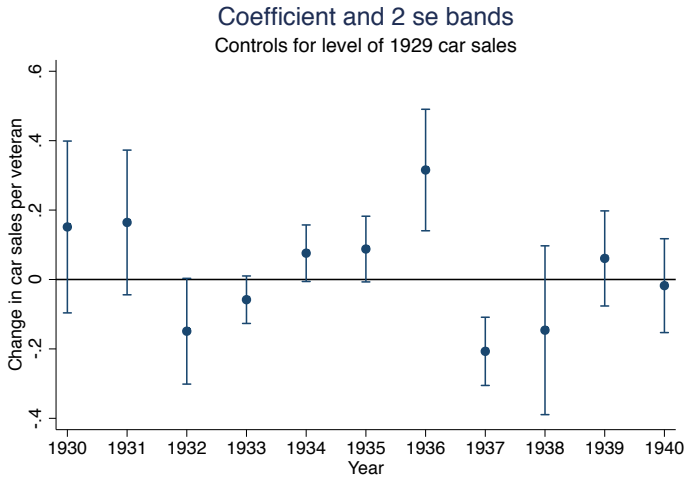
The true number is probably somewhere between these two. An informative way to understand these numbers is to compute the change in new car spending in a state per bonus dollar received. This equals $\frac{\text{increase in new car spending}}{\text{average bonus}}$ = roughly 25 to 35 cents depending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

The effect of the bonus on new car sales in table 10 is larger than that measured in the household survey. Whereas the household survey results (section 3, table 9) 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, the larger effect as measured from cross-state regressions could reflect a local Keynesian multiplier. If the presence of more veterans in a state benefited non-veterans, inducing some of them to buy new cars, it would explain part of these findings.

4.2.1 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 I measure in 1936 is a causal impact of the bonus rather than a spurious correlation. But if veterans per capita

Figure 5: 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* 2/22/30, p. 267, 2/28/31, p. 309, 2/27/32, p. 294, 2/25/33, p. 224, 2/24/34, p. 220, 2/22/36, p. 243, 2/25/39, p. 208, 3/1/1940, p. 186, and 3/1/41, p. 214; population: BEA table SA1-3; veterans per capita: IPUMS 5% sample of the 1930 Census (Ruggles et al. 2010).

affected sales negatively in 1937, on the backside of the 1936 payments, this would be reassuring evidence that I am measuring the true effect of the payments.

Figure 5 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 figures 5 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 U.S. economy in that year. Since veterans were not distributed randomly across the country, insofar as some of the economic shocks hitting the U.S. 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 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, as shown in table 11, the coefficients in figure 5 imply that the veterans' bonus increased the *level* of car sales in 1937 as well as in 1936.

Table 11 assumes that the relationship across states between veterans per capita and auto sales hold at the aggregate level. This may be wrong for a variety of reasons.[53] Nonetheless the table provides a useful indication of the rough magnitude of the bonus's possible aggregate effects. One initially puzzling result in the table is that absent the bonus car sales would have declined in 1936. This is in fact not unreasonable, since 1935 sales were boosted by two new model year introductions in the calendar year: one in January and one in November (Cooper and Haltiwanger 1993).[54]

4.3 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.

[53]See Nakamura and Steinsson 2011 and Mendel 2012.

[54]Readers may also wonder why actual car sales fell so much between 1937 and 1938. In Hausman (2012), I argue that this was in part due to the effect of unionization-induced wage and price increases in the auto industry.

Table 11: Implications of the cross-state regressions for aggregate car sales

	1936	1937	1938
Change in car sales (# of units)			
Actual	660,589	79,255	-1,592,731
No bonus	-585,188	790,398	-919,171
Level of car sales (# of units)			
Actual	3,404,497	3,483,752	1,891,021
No Bonus	2,158,720	2,949,118	2,029,947
Cumulative sales, 1936-38 (# of units)			
Actual	3,404,497	6,888,249	8,779,270
No bonus	2,158,720	5,107,838	7,137,785

Notes: No bonus is the pattern of aggregate car sales implied by the regression of the change in auto sales on veterans per capita, the level of 1929 car sales per capita, and a constant. No bonus corresponds to the predicted value from this regression when veterans per capita equals zero and the level of 1929 car sales per capita equals that in the U.S. as a whole.
Source: Data on the level of auto sales from *Automotive Industries* 2/22/36, p. 243, 2/25/39, p. 208, and 3/1/1940, p. 186.

In the 1930s, the Bureau of Labor Statistics collected data on the value of residential building permits issued for over 300 cities.[55] To measure the effect of the bonus, I estimate a regression similar to the cross-state specification for auto sales. But in this case, the left-hand side variable is the change in the per capita dollar value of building permits in a city.

Table 12 shows regression 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 clearly large. As was the case with the cross-state autos regressions, the natural 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

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

Table 12: Regression results for residential building permits

	(1)	(2)	(3)	(4)	(5)
Veterans per capita, 1930	262.0***	200.0**	121.4**	114.9***	257.9**
	(84.36)	(77.22)	(47.52)	(37.43)	(103.0)
Δ per capita bldg. permits 1935				0.832***	
				(0.109)	
Black share of the population				4.820*	
				(2.766)	
State fixed effects		X	X		X
Outliers excluded			X	X	
Cities with pop. $>$ 50,000 only					X
Observations	302	302	282	282	185
R^2	0.082	0.275	0.364	0.415	0.344

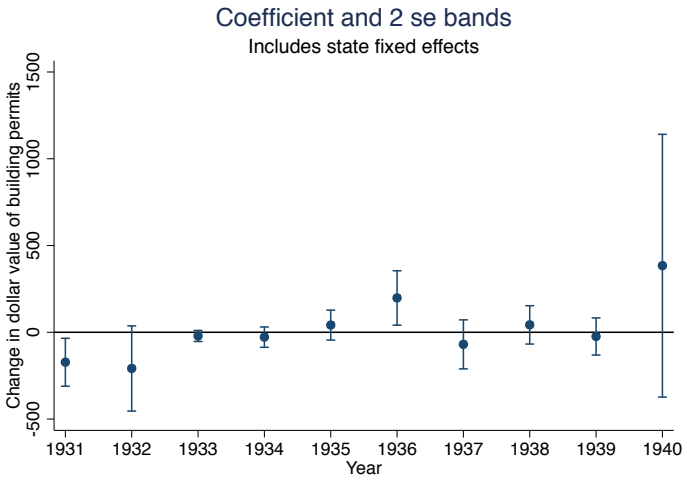
Standard errors clustered at the state level in parentheses
* $p < .10$, ** $p < .05$, *** $p < .01$
Sources: The building permit data are from Price Fishback who collected them from various editions of the Bureau of Labor Statistics *Monthly Labor Review* and from Bureau of Labor Statistics (1938) and (1940).
Veterans per capita and black share come from the IPUMS 5% sample of the 1930 Census (Ruggles et al. 2010).

of a simple regression will be captured by these state specific trends: for instance, if veterans per capita is correlated with agricultural production of a specific crop or climate shocks.

The coefficient shrinks somewhat with the addition of state fixed effects. More important is whether one drops outliers. In column 3, I drop the 10 cities with the smallest and largest per capita building permit changes. This further shrinks the coefficient, but it remains highly economically and statistically significant. Column 4 also excludes outliers and adds controls for the lagged change in building permit values and the share of the population that was black. This results in a coefficient very similar to that in column 3. A different way to test the robustness of the results is to limit the sample. In column 5, the sample is limited 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. Figures 6 and 7 show the coefficient and two standard error bands for 1931-1940 for the regression of the change in building permits per

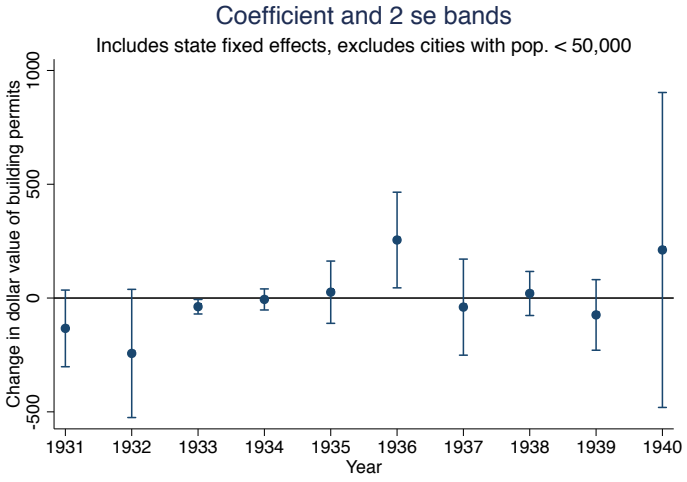
Figure 6: Residential building permits and veterans per capita 1931-1940.



Note: Standard errors are clustered at the state level.

Sources: See table 12.

Figure 7: Residential building permits and veterans per capita 1931-1940.



Note: Standard errors are clustered at the state level.

Sources: See table 12.

capita on veterans per capita. Since, as was clear in table 12, the exclusion of certain cities has large effects on the coefficients, I show two different forms of the placebo test. Figure 6 includes all cities for which observations exist for the entire decade. Figure 7 excludes cities with a population less than 50,000. While the results are not completely clear (and are graphically distorted by large standard errors in 1940), they are generally reassuring. In both graphs, 1936 is the only year with a positive and statistically significant coefficient. Also reassuring is that in both cases, 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 12 and figures 6 and 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, column 4, implies enormous aggregate effects. In the 302 cities in the sample, total building permits rose from $282 million in 1935 to $620 million in 1936. The coefficient of 115 suggests that more than half this increase was accounted for by the bonus.[56] 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 6, I consider the possible link between these depressed conditions and veterans' spending on housing.

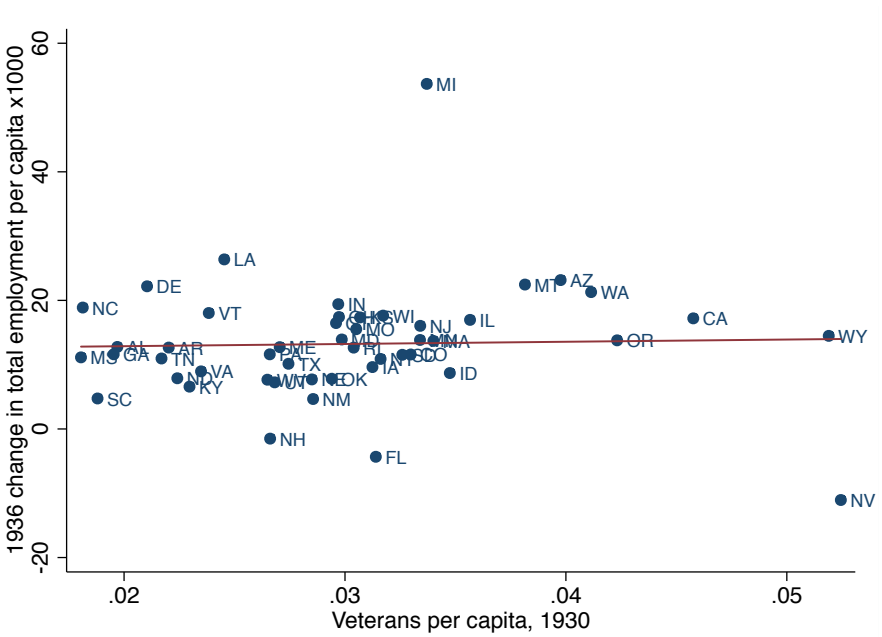
4.4 Employment

One of the best aggregate indicators of state-level economic activity in the 1930s is employment.[57] The Bureau of Labor Statistics (BLS) measured employment using an establishment survey like that conducted by the BLS today. The establishment survey in the 1930s covered

[56] In these 302 cities, there were 1.7 million veterans. The regression coefficient of 115 means that absent the bonus the value of building permits would have been roughly 1.7 million · $115 less. As was the case with the cross-state autos sales regressions, interpreting the coefficient as measuring an economy-wide aggregate effect is problematic. Still, it provides an indication of the scale of the bonus's effect on housing.

[57] Data on state level personal income are also available; unfortunately these data exist only in nominal terms. Hence, any multiplier computed using these data would conflate price and real activity effects of the bonus payments. Furthermore, cross-state regressions with personal income would likely be affected by spillovers in the same way that these obscure the relationship between the bonus and total employment (see below).

Figure 8: 1936 employment change and veterans per capita.



Sources: Employment: Wallis (1989) and the 1940 Census; population: BEA table SA1-3; veterans per capita: IPUMS 5% sample of the 1930 Census (Ruggles et al. 2010).

between 4 and 7.5 million workers or 12 to 20 percent of total non-farm employment (Wallis 1989).[58] Importantly, however, construction employment was excluded from the state figures (*Monthly Labor Review*, 8/1936, p. 496). Thus the effect of veterans using their bonus to build or remodel homes is not captured.

Figure 8 shows the per capita change in private nonfarm employment (multiplied by 1000) on the vertical axis and veterans per capita on the horizontal axis. The figure suggests that there was no relationship between the two variables. Regressions confirm this result. Across a variety of specifications, there is no robust relationship between veterans per capita in a state and the change in employment in 1936. Michigan is a notable outlier in figure 8: according to Wallis (1989) employment in Michigan rose by an astonishing 23 percent (275,000) in 1936, more than in any other state in that year. That Michigan is such an outlier is a clue that

[58]In the *Monthly Labor Review*, the BLS reported raw results (essentially indexes) from its surveys of manufacturing and nonmanufacturing establishments. In order to compute actual employment, the numbers need to be benchmarked. Wallis (1989) does this and reports annual employment indexes for all 48 states in the 1930s. To translate Wallis's employment indexes into values for actual employment by state, I apply the percent change in his indexes to the levels of total employment in each state reported in the 1940 Census.

the lack of a relationship between veterans per capita and employment outcomes in 1936 may reflect what veterans bought with their bonus, rather than a small multiplier. Suppose in the extreme case that every veteran bought a new car with his bonus check and nothing else. In this case, the aggregate effect of the bonus would obviously have been large: 3.2 million more new cars would have been sold. And this effect would show up in a cross-state regression of auto sales on veterans' share. But the employment effects would be concentrated in Michigan and other heavily industrialized states. Hence there might be no relationship between state level employment changes in 1936 and veterans per capita. Of course, veterans did not only buy cars. But the results of section 3 suggest that they primarily bought tradable goods (and housing).

5 Direct survey and narrative evidence

5.1 The American Legion Survey

In the recent literature on consumption responses to transfers there are two dominant approaches. One is to use Consumer Expenditure (CE) Survey data to directly 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, Souleles, Johnson and McClelland (2013) on the effect of the 2008 stimulus payments. An alternative approach is to ask people whether they plan to spend a transfer payment. Shapiro and Slemrod (2003a, 2009) do this 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's passage (Daniels 1971). The survey is shown in figure 9. While the actual survey responses no longer exist, the American Legion library has a copy of unpublished tabulations prepared by an Indi-

Figure 9: American Legion survey form

PLEASE CO-OPERATE
BY
FILLING OUT AND HANDING TO POST ADJUTANT AT ONCE

DEAR LEGIONNAIRE:

Members of The American Legion are asked to lend their co-operation to National Headquarters to determine as accurately as possible in advance how money derived from payment of the Adjusted Service Certificates will be spent. You can do this by filling out this questionnaire and handing it to your Post Adjutant.

To fill out, put a check mark in the square in front of the product listed which you now think you will purchase, if and when you receive payment on your adjusted compensation certificate, and after the name of the product indicate approximately how much you think you will spend for that particular item. It is important that the amount of the estimated expenditure for each particular item be shown on the questionnaire.

On the blank lines at the bottom, list any other articles, items or ways in which you contemplate the expenditure of your adjusted compensation not contained in the printed list.

It is also important that the total amount of adjusted compensation to be received should be shown by you at the bottom of the questionnaire in the space provided.

Percentages will be obtained from all the questionnaires filled out which will be projected against the total payments to be made. *Each individual questionnaire is confidential; no names or identification marks of any sort should be placed on it.*

ARTICLES TO BE BOUGHT	Approximate Amount to Be Expended Therefor
☐ Agricultural Implements	$
☐ Automobile	$
Do you own car to trade in? Yes ____ No ____	
☐ Auto Truck	$
☐ Battery for Auto or Truck	$
☐ Tires for Auto or Truck	$
☐ Build New House	$
☐ Clothes for Children	$
☐ Suit or Overcoat for Self	$
☐ Clothes for wife	$
☐ Education, Home Study Course	$
☐ Electric or Gas Refrigerator	$
☐ Farm	$
☐ Furniture	$
☐ House Furnishings	$
☐ Insurance (Life, Health or Accident)	$
☐ Invest in Own Business	$
☐ Invest in Stocks or Bonds	$
☐ Lot for Home Site	$
☐ Men's Shirts	$
☐ Men's Furnishings (Ties, Socks, Underwear)	$
☐ Men's Hats	$
☐ Men's Shoes	$
☐ Oil or Gas Furnace	$
☐ Paint House	$
☐ Pay Notes, Mortgages, Loans or Old Bills	$
☐ Purchase Home	$
☐ Radio	$
☐ Repair House	$
☐ Rugs	$
☐ Start or Increase Savings Accounts	$

OTHER ARTICLES TO BE BOUGHT

	$
	$
	$
	$
	$
Total Amount of Adjusted Compensation Due	$

anapolis accounting firm on behalf of the American Legion. Table 13 shows these tabulations.

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 tabulations of the survey 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. Interestingly, nearly all of this is accounted for by debt repayment: perhaps unsurprisingly, in 1936 few appeared eager to invest their bonus in financial markets. 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 amount that was spent, not whether it was spent on investment or consumption.

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 1/3 from the 2001 tax rebates. By contrast, Johnson, Parker and Souleles (2006) investigate spending from the 2001 tax rebate using the consumer expenditure (CE) survey and find an MPC on nondurables alone of roughly two-thirds. It appears that at least part of the difference between these methodologies comes from the measurement of the lagged spending response.[59] 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 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,

[59]See Shapiro and Slemrod (2009), pp. 378-379 for a discussion of this issue.

Table 13: American Legion survey tabulations

Item	Amount per veteran	Percent of bonus
Repair present house	$ 37.90	6.71%
Paint house	$ 9.72	1.72%
Housing consumption total	$ 47.62	8.43%
Furniture	$ 17.37	3.07%
Rugs and carpets	$ 2.83	0.50%
Other house furnishings	$ 12.93	2.29%
Electric or gas refrigerator	$ 6.04	1.07%
Oil or gas furnace	$ 2.57	0.45%
Radio	$ 2.49	0.44%
Other durable gds total	$ 44.22	7.82%
Suit or overcoats	$ 9.84	1.74%
Shirts	$ 0.87	0.15%
Shoes	$ 0.99	0.18%
Hats	$ 0.48	0.08%
Other men's furnishings	$ 2.48	0.44%
Clothing for children	$ 12.01	2.13%
Clothing for wife	$ 13.10	2.32%
Clothing total	$ 39.76	7.04%
Passenger automobiles	$ 30.86	5.46%
Trucks	$ 4.02	0.71%
Automobile tires	$ 1.52	0.27%
Automobile batteries	$ 0.15	0.03%
Autos total	$ 36.55	6.47%
Purchase farm	$ 18.97	3.36%
Farm implements	$ 12.46	2.20%
Invest in own business	$ 37.90	6.71%
Build new house	$ 26.28	4.65%
Purchase home	$ 36.80	6.51%
Purchase lot for homesite	$ 9.03	1.60%
Investment total	$ 141.43	25.03%
Purchase insurance	$ 19.11	3.38%
Education	$ 5.08	0.90%
Miscellaneous	$ 22.72	4.02%
Other total	$ 46.91	8.30%
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%

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 (2006) 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).

It is also possible to compare the results of these two methodologies for the consumer response to the 2008 tax rebates. Shapiro and Slemrod (2009) find an MPC roughly 1/3. 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 of $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 1935-36 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.

5.2 Narrative Evidence

A useful check on the quantitative evidence of previous sections comes from newspaper reports at the time: given my results, it would be troubling if newspapers did not report high spending by veterans. In fact, they reported a spending boom.

For example, the *Los Angeles Times* wrote on June 19, 1936, four days after the bonus was distributed (p. A1):

> 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.

The *Wall Street Journal* reported a couple weeks later, on July 3 (p. 1):

> Unusual gains in retail sales of new passenger cars the latter part of last month lifted the June retail sales totals of the largest automobile units to new peaks for the year. . . . Such a development was not expected, the belief of automobile people being that June sales would not be able to maintain the fast pace of April and May, usual months for peak in new car sales. No doubt the bonus had something to do with pushing sales into new high ground, but generally strong business throughout most of the country played an equal part in providing support.

Of course, the "generally strong business" referred to by the *Wall Street Journal* may itself reflect the effect of the bonus.

Dun and Bradstreet Monthly Review noted the effects of the veterans' bonus in its July 1936 report on business conditions around the country during June (pp. 45-47). Some cities reported little effect, at least in the first two weeks of bonus distribution. In others, however, effects were evidently visible and large. For example, the report from the Minneapolis region said (p. 46):

> The depressing results of severe drought conditions which have developed in North Dakota, Montana, and portions of South Dakota have been more than offset by the exhilarating effect of the spending of bonus money.

The negative effects of drought and heatwave come up frequently in contemporary narrative reports. In 1936, drought afflicted much of the country and was particularly severe in the Dakotas and Kansas.[60] And July 1936 was extraordinarily hot in much of the midwest. Temperatures reached 110 degrees in Iowa and Wisconsin.[61] As noted in the above quote, this makes it all the more remarkable that large effects of the bonus are detectable both in the data and in narrative evidence.

The front page of the *Chicago Tribune* on June 14, 1936 - the day before the bonus was distributed - printed a cartoon showing the businesses hoping for a share of the bonus money (figure 10). The cartoon suggests that contemporaries expected the money to be used to purchase a wide variety of consumer goods. Note in particular the prominent position of the auto dealer (and Ye Olde Tappe Room). The savings bank is far in the rear.

6 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). Empirical evidence for this view comes from Hsieh (2003) who looks at the consumption response to annual payments from the Alaska Permanent Fund. He 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 funds. 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. Certainly the bonus payments were larger. And although they were not regular in the sense that annual payments from the Alaska Fund are, they varied less across individuals than income tax refunds and were relatively straightforward to calculate. A veteran simply had to take the value of his

[60] See `http://www.ncdc.noaa.gov/paleo/pdsiyear.html`.
[61] See `http://www.crh.noaa.gov/arx/events/heatwave36.php`.

Figure 10: From the *Chicago Tribune*, 6/14/36, p. 1

adjusted service certificate and subtract the amount of any loans taken.

Behavioral economists have also argued 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, p. 635) put it this way: "The larger is a windfall, the more wealth-like it becomes, and the more likely it will be included in the less tempting Assets account."

Why then did World War I veterans spend so much of their bonus? Although an explanation is necessarily speculative, I believe that three features of the 1936 economy were critical: (1) people faced substantial liquidity constraints; (2) future income was expected to be higher, and (3) there was substantial pent-up demand for durables, in particular cars and housing.

6.1 Liquidity constraints

The interwar period saw an explosion of 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 (less than three years) and often came with onerous terms (Olney 1991). A typical car loan required a 33 percent downpayment if a new car, and a 40 percent downpayment if a used car (Olney 1991, p. 113). 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, p. 115). 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 downpayment requirements. And of course harsh loan terms provided 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 that are identified off the timing of the bonus disbursement and narrative evidence suggests that liquidity con-

straints remained binding for many if not most veterans throughout spring 1936. The *Magazine of Wall Street* wrote in May 1936 "[A] great many veterans lack credit standing and have not been able to spend in anticipation of the windfall" (5/9/36, p. 77).[62]

6.2 Expectations of higher future income

Liquidity constraints alone cannot generate a high MPC. There must be a reason why the constraint binds, why the consumer would like to borrow but cannot. If consumers wish to save, liquidity constraints do not increase the MPC. In fact, the possibility of liquidity constraints binding in the future increases the incentive to save today. 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 reasonably 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 prior to the bonus. Real per capita personal income rose 19 percent from 1933 to 1935.[63] And while it is not possible to directly measure veterans' expectations, economic forecasters in early summer 1936 expected good times to continue. A representative quote comes from the *Magazine of Wall Street* 6/20/36, 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.

[62]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 (6/20/36, p. 857): "While some advance business has been done on credit in anticipation of the bonus, the amount is said to be small."

[63]NIPA table 2.1.

> 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. A wafted straw indicates the direction of the wind and below will be found a number of straws showing the wind is still a fair one.

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

A concrete measure of expectations of future income is the behavior of the stock market.[64] In the four years from its low in June 1932, the market rose 208 percent. In just the year prior to the bonus disbursement, from June 1935 to June 1936, the market rose 45 percent.[65] Thus market participants, at least, were optimistic. And the bonus disbursement may have increased their optimism: from June to July 1936, the market rose 6 percent.

6.3 Demand for autos and housing

In theory, expectations of higher future income combined with liquidity constraints could generate a MPC as high as 1. In practice it seems unlikely that this alone could explain the high MPC that I observe. After all, even if many veterans expected higher future income, there must also have been some who remained pessimistic and uncertain after the very recent declines in income during the Depression itself. It seems likely that a key additional contributor to high spending from the bonus was pent-up demand for autos and housing.

Of course, 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

[64]Not only are stock prices an indicator of market participants' expectations, the behavior of the stock market was a salient indicator that may have been viewed by the broader public as a predictor of future income. This could have been the case despite the fact that only a small percent of American households actually owned stocks (Romer 1990).

[65]Stock price data are for the S&P composite index and are from Robert Shiller. See `http://www.econ.yale.edu/~shiller/data.htm`.

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 (2012) consider a business cycle model in which consumers face fixed costs of adjustment when buying durables.[66] 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 1996, 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 Van Szeliski 1939, p. 50). 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.[67] 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.[68] Thus just as many veterans were likely anxious to purchase a new car,

[66]For other models of auto purchases, see Adda and Cooper (2000) and House and Leahy (2004). While neither of these papers studies the implications of their models for the MPC, their implications would likely be similar to those in Berger and Vavra (2012).

[67]NIPA table 1.1.3.

[68]BEA fixed asset table 5.2 and

many were also likely eager to buy a new house or move to a nicer apartment.

7 Aggregate effects of the bonus

7.1 A back-of-the-envelope calculation

The aggregate multiplier associated with the bonus was a function of two things: (1) how much of each dollar of bonus payments was spent, and (2) the spending multiplier.[69] This paper provides evidence on the former. Many recent theoretical and empirical papers provide estimates of the later. A robust conclusion of this literature is that multipliers are likely to be significantly higher when interest rates are at the zero lower bound, as they were in 1936. Many papers find multipliers in the range of 1.5 to 2 (or possibly higher) in these conditions.[70] And a surprising number of estimates from both theory and empirics are clustered in the narrow range of 1.6 to 1.8. For instance, Hall (2009) finds a spending multiplier of 1.7 in a simple New Keynesian model at the zero lower bound, and Gordon and Krenn (2008) empirically estimate

`http://www.census.gov/popest/data/national/totals/pre-1980/tables/popclockest.txt`

[69]Here I assume that the multiplier attached to higher consumption spending is similar to that for higher government spending. This seems to be a reasonable assumption. 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."

[70]For example, Gordon and Krenn (2010) find that the multiplier for government spending in 1940 was 1.8, and Alumnia et al. (2010) estimate that the defense spending multiplier in the 1930s was 2.5. New Keynesian DSGE models suggest that these estimates are reasonable. See, for instance, Hall (2009), Woodford (2011) and Christiano, Eichenbaum, and Rebelo (2011). Of course, the exact value of the spending multiplier is controversial. While much of the literature suggests that 1.5 to 2 is a reasonable range at the zero lower bound, the multipliers measured in the empirical literature inevitably come with confidence intervals that encompass much smaller and larger values. And multipliers derived from New Keynesian models are sensitive to assumptions about the form of the utility function, the degree of price stickiness and the persistance of the shock. For an argument that the multiplier may be somewhat smaller, see Ramey (2011). For empirical evidence that the defense spending multiplier may be only 0.5 in normal times, see Barro and Redlick (2011).

There are two 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 U.S. economy is open, and thus a significant amount of spending leaks abroad. By contrast, in 1936 the U.S. 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.

that the spending multiplier in the U.S. in 1940 was 1.8. 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 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:

$$\underbrace{(\frac{Y_{36}}{Y_{35}} - 1)}_{\text{Actual '36 growth}} \quad - \underbrace{(\frac{Y_{36} - B \cdot \frac{P_{37}}{P_{36}} \cdot 0.7 \cdot S_m}{Y_{35}} - 1)}_{\text{Counterfactual '36 growth}} = \underbrace{\frac{B \cdot \frac{P_{37}}{P_{36}} \cdot 0.7 \cdot S_m}{Y_{35}}}_{\text{Bonus's effect on growth}}, \tag{8}$$

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;[71] real output is measured in chained 1937 dollars.[72] Table 14 shows the result of this calculation for several possible values of the spending multiplier. Regardless of one's prior for the spending multiplier, the high MPC that I find 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. For the most likely values of the multiplier, around 1.7, this calculation suggests an effect on GDP growth of 2.5 to 3 percentage points. An Okun's law coefficient of 2 would then imply a decline in the unemployment rate due to the bonus of 1.3 to 1.5 percentage points.[73]

[71] NIPA table 1.1.4.

[72] NIPA table 1.1.6a.

[73] Readers may wonder whether Okun's law provides a good approximation in the 1930s. In fact, the evidence suggests that Okun's law has been remarkably constant over time. For instance, Romer (1986) finds an Okun's law coefficient near 2 for the period 1893 to 1927.

Table 14: Effect of the bonus on 1936 GDP growth

Spending multiplier	Effect on '36 growth (percentage points)
0.5	0.8
1.0	1.7
1.5	2.5
1.7	2.8
2.0	3.3

Source: See text.

7.2 The aggregate time series

Since the 1936 veterans' bonus was a single event, it is not possible to use aggregate time series to establish causal effects of the bonus. Still, the time series are suggestive. One way to see if the above calculation is plausible is to compare the path of GDP, consumption, and investment in 1936 to that in other years of the recovery. This is done in table 15. The rapid increase in GDP in 1936 is obvious: whereas GDP grew 10.9 percent in 1934 and 8.9 percent in 1935, it grew over 13 percent in 1936. Most of the increase in growth was driven by a dramatic increase in consumption. Consumption *growth* increased by four 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.[74]

Table 15: Real output, consumption, and investment (billions of 2005 dollars)

	GDP	Consumption	Investment
1933	715.8	600.8	18.9
1934	793.7	643.7	34.1
1935	864.2	683	63.1
1936	**977**	**752.5**	**80.9**
1937	1027.1	780.4	101.1

Source: NIPA table 1.1.6.

All this is not easily explained by factors other than the bonus. Monetary factors were if

[74]See *Survey of Current Business*, August 1936, p. 6. Telser (2003) provides a more detailed analysis of the monthly time series and the evidence they provide in favor of large effects from the bonus.

anything contractionary in 1936. Broad money supply growth slowed from 14 percent in 1935 to 11 percent in 1936.[75] And in August 1936, the Federal Reserve raised reserve requirements. While the reserve requirement increase probably had little direct effect on the economy,[76] it signaled a monetary regime shift that could have lowered inflation expectations and thus raised real interest rates (Eggertsson and Pugsley 2006). 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.[77] But a more plausible story is that the bonus was the key factor raising 1936 growth well above its 1934 or 1935 levels. Compared to monetary policy, the bonus can also more easily account for why so much of 1936 growth was driven by consumption.

7.3 The Bonus and the 1937-38 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. I provided evidence for exactly this scenario from the cross-state auto sales regressions. These suggested that the bonus significantly lowered autos sales *growth* in 1937, despite slightly increasing the *level* of sales in that year.

As pointed out by Romer (1992), Irwin (2012), and Hausman (2012) it is less clear that fiscal policy, and the veterans' bonus in particular, can account for the 1937-1938 recession. One problem is the magnitude of the bonus. The bonus, a 2 percent of GDP payment, cannot account for the change from 13.1 percent GDP growth in 1936 to -3.4 percent GDP growth in 1938.[78] But despite the lack of an overwhelming direct effect, there are also at least two

[75]The source is NBER macrohistory series m14144a, which is computed from underlying data in Friedman and Schwartz (1970), *Monetary Statistics of the United States.*

[76]See Calomiris, Mason, and Wheelock 2011 and Irwin 2012.

[77]If one estimates a simple regression of output growth on lags of output growth and the money supply with annual data from 1919-1940, one obtains a good fit for all years after 1933 except 1936 and 1938. In 1936, growth is 4.5 percentage points above the prediction from this forecast regression. At least in this reduced form setting, the lagged effect of money supply growth appears not to be a plausible explanation for rapid 1936 GDP growth.

[78]NIPA table 1.1.1.

channels through which the bonus conceivably contributed indirectly to the 1937-38 recession.

First, Irwin (2012) argues that the 1937-38 recession was primarily caused by the Treasury's decision to sterilize gold inflows beginning in December 1936. This decision was partly motivated by increases in wholesale prices in the second half of 1936 (Irwin 2012). And these price increases were undoubtedly boosted by the veterans' bonus. Indeed, insofar as the bonus increased inflation and thus lowered real interest rates, this is one of the mechanisms through which it facilitated economic recovery. But the Treasury cut-off this virtuous cycle when it began sterilization in December.

Second, Hausman (2012) argues that monetary factors alone cannot explain the full extent of the 1937-38 recession. In addition to the backside of the veterans' bonus, other fiscal factors, in particular the beginning of social security tax collection in January 1937, contributed to slower growth. A separate contractionary factor was a supply shock to the auto industry. Hausman (2012) argues that unionization-induced auto price increases took over a percentage point off output growth in 1936. More generally, economy-wide unionization may have had a negative effect on growth in 1937 and 1938 (Cole and Ohanian 2001). But again, the veterans' bonus may not escape all blame. The decision of auto and other manufacturers to accede to union demands was plausibly in part a result of the high level of sales in 1936 induced by the bonus. Had sales been lower, manufacturers might have felt more pressure to deny workers wage increases.

8 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 1935-36 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 implies an aggregate effect on 1936 GDP growth of 2.5 to 3 percentage points.

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 display an increase in output and consumption in 1936 that is difficult to explain by factors other than the bonus.

Would the bonus have been equally effective 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 bang for the buck.

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A Survey schedule

This example is from the codebook accompanying ICPSR study 8908.

B. L. S. 938

CONFIDENTIAL

The information requested in this schedule is strictly confidential. Giving it is voluntary. It will not be seen by any except sworn agents of the cooperating agencies and will not be available for taxation purposes.

U. S. DEPARTMENT OF LABOR
BUREAU OF LABOR STATISTICS
IN COOPERATION WITH
NATIONAL RESOURCES COMMITTEE
WORKS PROGRESS ADMINISTRATION
DEPARTMENT OF AGRICULTURE
WASHINGTON

STUDY OF CONSUMER PURCHASES
A Federal Works Project
EXPENDITURE SCHEDULE—URBAN

Code No. 41X - 5035
Schedule No. 736
City Westbrook 3
C. T. or E. D. 3-103 03
Agent W. Litchfield 04
Date of interview June 9, 1936

I. YEAR COVERED BY SCHEDULE R

12 months beginning Jan 1, 1935
and ending Dec 31, 1935

II. COMPOSITION OF ECONOMIC FAMILY

A MEMBERS OF FAMILY	B Sex	C Age	D Number of weeks— At home	E Away
1. Husband	M	42	52	—
2. Wife	F	38	52	—
3. [illegible]	F	17	27	0
4. [illegible]	F	13	52	—
5. [illegible]	F	4	52	—
6. [illegible]	M	2	52	—
7.				
8.			1	3

III. RESIDENCE

In city during schedule year 12 months

IV. LIVING QUARTERS OCCUPIED (at end of schedule year)

1. Type of living quarters F
2. Total number of *rooms* (excluding bathrooms) 3
3. Total number of *persons* occupying these rooms (including family, roomers, paid help, and others) 3

4. If family is now renting, does rent include:

	Yes	No		Yes	No
a. Garage.	☐	☑	e. Light.	☐	☑
b. Furnishings.	☐	☑	f. Refrigerator (mechanical).	☐	☑
c. Heat.	☐	☑	g. Refrigeration.	☐	☑
d. Water.	☑	☐			

HOUSING FACILITIES

5. Water supply:
   a. ☑ In living quarters.
   b. ☐ Indoors, other.
   c. ☐ Outdoors.
6. Running water:
   a. ☑ Hot or cold.
   b. ☐ Cold only.
   c. ☐ None.
7. Location of toilets:
   a. ☑ In living quarters.
   b. ☐ Indoors, other.
   c. ☐ Outdoors.
8. Number of toilets:
   a. Flush 1
   b. Other
9. Heating (check principal method):
   a. ☐ Central, steam or water.
   b. ☐ Central, air.
   c. ☑ Stoves (not kitchen).
   d. ☐ Kitchen stove only.
   e. ☐ Fireplace.
   f. ☐ None.
10. Lighting:
    a. ☑ Electricity.
    b. ☐ Gas.
    c. ☐ Kerosene.
    d. ☐ Other.
11. Cooking fuel:
    a. ☐ Gas.
    b. ☐ Electricity.
    c. ☐ Wood or coal.
    d. ☑ Kerosene or gasoline.
    e. ☐ Other.

V. HOUSING EXPENSE (during schedule year)

A RENTED HOME (excluding vacation home)	B Present home	C Other home
1. Number of months occupied	12	
2. Monthly rental rate	$16.00	$
3. Rental concessions	—	
4. Total rent	192.00	
5. Repairs paid for by family	—	
6. Total expense (4+5)	192.00	

OWNED HOME (excluding vacation home)	Present home	Other home
Number of months:		
7. Owned		
8. Occupied as owner		
9. Structural additions to home during year	$	$
10. Paid on principal of mortgage during year		
EXPENSE FOR MONTHS OWNED		
11. Interest on mortgage	$	$
12. Refinancing charges		
13. Taxes payable in schedule year, except back taxes		
14. Special assessments		
15. Repairs and replacements		
16. Insurance, fire, tornado		
17. Other		
18. Total for months owned (11–17)		
19. Total for months occupied as owner		
20. Total for family's home (6+19)	192.00	—

VACATION HOME	Expense for year
21. Vacation home owned: Net expense for months occupied	$
22. Vacation home rented: Rent and repairs for months occupied	
23. Lodging while traveling or on vacation	
24. Total 21–23	

MONEY VALUE OF HOUSING RECEIVED (without direct money payment)	Value
25. Rental value of housing received as gift or pay	
26. Net money value of occupancy of family's owned home	
27. Net money value of occupancy of owned vacation home	

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(1)

6 1 3 6 4 4 1

VI. HOUSEHOLD OPERATION

A	B	C	Latest season		Earlier Seasons								Total expense for year (for office use)
FUEL, LIGHT, and REFRIGERATION	Unit	Price	Months Dec Jan Feb		Months Feb Apr May		Months June Jul Aug		Months Sept Oct Nov		Months		
			Quantity	Expense	Quantity	Expense	Quantity	Expense	Quantity	Expense	Quantity	Expense	
1. Coal: Bit. ☐ Anth. ☐				$		$		$		$		$	$
2. Coke ☐ Briqts. ☐													
3. Fuel oil													
4. Wood ☐ Kindling ☐													
5. Kerosene ☐ Gasoline ☐ Range oil	GAL	.08½	1650										136.75
6. Electricity	X	X	xxx	15	xxx	10 38	xxx	9 [illegible]	xxx	10 [illegible]	xxx		42.99
7. Gas			xxx		xxx		xxx	8 [illegible]	xxx		xxx		
8. Ice	NR	NR					R						19 7.50
9. 10 TOTAL (1–8)			xxx	NR	xxx	NR	xxx	NR	xxx	NR	xxx	18	[illegible]

10. Value of fuel gathered by family or received free, $

A	B	C	D	E	F	G	H	J
PAID HOUSEHOLD HELP	Wks.	Lives In	Lives Out	Employed F.T.	Employed P.T.	Expense per week No. meals	Expense per week Dollars	Expense for year
11. Cook or general worker								$
12. Cleaning man or woman								
13. Laundress								
14.								
15.								
16. Aprons, uniforms, and gifts to paid help								
17. TOTAL (11–16)								

A	B
OTHER HOUSEHOLD EXPENSE	Expense for year
18. Water rent	$
19. Telephone: Number mos. ____; per mo. $	
20. Laundry sent out: Number wks. ____; amt. $	
21. Specify service	
22. Laundry soap and other cleaning supplies	5 00
23. Stationery, postage, telegrams	1 00
24. Moving, express, freight, etc.	
25. Other	6 00
26. TOTAL (18–25)	12 00
27. TOTAL household operation (9+17+26)	198 197.63

VII. MEDICAL CARE

A	B	C
	Check if any free	Expense for year
1. Physician: ____ office visits at $		$
2. Physician: ____ home calls at $		
3. Dentist		
4. Oculist		
5. Other specialist (specify)		
6. Clinic visits: Number ____ at $		
7. Hospital room or bed: ____ days at $		
8. Private nurse: In hosp. ____ days at $		
9. Private nurse: At home ____ days at $		
10. Visiting nurse: ____ visits at $		
11. Examinations and tests (not included above)		
12. Medicines and drugs		3 [illegible]
13. Eye glasses		
14. Medical appliances and supplies		
15. Health and accident insurance		18 [illegible]
16. Other		
17. TOTAL (1–16)		21.[illegible]

VIII. RURAL—URBAN BACKGROUND

A	B	C	D	E
Lived—	Husband No. years	Husband State	Wife No. years	Wife State
1. On a farm or in open country				
2. In village of less than 2,500		N (R)		
3. In city of 2,500 to 10,000				
4. In city of 10,000 or more				

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IX. RECREATION

A	B	C	D	E	F
	Adults		Children		Expense for year
	Number	Price	Number	Price	
Paid admissions to—					
1. Movies: Winter		$		$	$
2. Spring					
3. Summer					
4. Fall					
5. Plays, pageants, concerts, lectures, forums					
6. Ball games, other spectator sports					
7. Dances, circuses, fairs					

GAMES AND SPORTS

8. Equipment, supplies, fees, licenses (enter year's expense for each item):
   Hunting, $____; Fishing, $____;
   Camping, $____; Trapping (sport), $____;
   Hiking, $____; Riding, $____;
   Baseball, $____; Tennis, $____; Golf, $____;
   Bicycles, $____; Skates, sleds, skis, $____;
   Billiards and bowling, $____; Boats, $____,
   Cards, chess, other games, $____; Other, $____
9. Total (all items 8)

OTHER RECREATION

10. Radio: Purchase
11. Batteries, tubes, repairs,
12. Musical instruments (specify)
13. Sheet music, phonograph records
14. Cameras, films, photo supplies
15. Children's toys, play equipment — (2) 00
16. Pets (purchase and care)
17. Entertaining in and out of home
18. Dues to social and recreational clubs
19. Other (specify)
20. (7) Total (1–19) TOT OTH (2)

X. TOBACCO

	Expense for year
1. Cigarettes: Packages per week (4) @ .15 ¢	(3)
2. Cigars: Number per week (8) @ ¢	(16)
3. Tobacco: All other	
4. Smokers' supplies	
5. Total (1–4) (47)	

XI. READING

3 2

	Expense for year
1. Newspapers: Daily 52 @ .15	(8)
2. Weekly 26 @ .10	(2)
3. Magazines (subscriptions and single copies)	
4. Books (not school books) bought during year: Number (—)	
5. Book rentals and library fees, public and rental libraries	
6. Books borrowed from public and rental libraries: Number (—)	XXXX
7. Total (1–6)	(10)

XII. EDUCATION

A	B	C	D	E
School attended during schedule year	Members attending		Expense for year	
	Public	Private	Tuition and fees	Books and supplies
1. Nursery school, kindergarten			$	$
2. Elementary school	1	—	-	—
3. High or preparatory school				
4. Business or technical school				
5. College, graduate, or professional school				

A	B	C
	Expense for year	Previous education
6. Total, tuition (1–5D)	$	Highest grade completed by:
7. Total, books and supplies (1–5E)		13. Husband X 6 3
8. Special lessons		14. Wife 8 2
9. Other (excluding board and rent)		15. Son or daughter over 16 years with most schooling: 10
10. Total (6–9)		a. Sex F
11. Board at school or college		b. Age 17
12. Room rent at school or college		c. Member of economic family? Yes ☑ No ☐

XIII. OCCUPATIONAL EXPENSE

(not reported as business expense or as deduction from gross income)

	Expense for year
1. Union dues, fees	$
2. Business and professional association dues	
3. Technical books and journals	
4. Supplies and equipment	
5. Other	
6. Total (1–5)	

XIV. PREVIOUS OCCUPATION OF HUSBAND

1. Was husband's occupation same during schedule year as in 1929? Yes ☑ No ☐
2. If not, his occupation in 1929 was same

XV. GIFTS, COMMUNITY WELFARE, AND TAXES

	Expense for year
1. Gifts (Christmas, birthday, other) to persons not members of economic family (not charity)	(2) 00
2. Contributions to support relatives not members of economic family	
3. Donations to other individuals	
4. Community chest and other welfare agencies	
5. Church, Sunday school, missions	
6. Taxes: Poll, income, personal property (payable in schedule year, except back taxes)	87 00
7. Other	
8. Total (1–7)	(5) 00

14—6780

XVI. USUAL FOOD EXPENSE DURING EACH SEASON OF SCHEDULE YEAR

A

FOOD AT HOME	Latest season of year: Months Dec Jan Feb		Earlier season: Months Mar Apr May		Earlier season: Months June July Aug		Earlier season: Months Sept Oct Nov		Months	
	Per week	Per month	Per week	Per month	Per week	Per month	Per week	Per month	Per week	Per month
Food expense at—										
1. Grocery or general store (excluding soap, matches, etc.)	$10.50	$	$10.50	$	$9.50	$	$9.50	$	$	$
2. Meat and fish market										
3. Dairy		3.60		3.60		3.60		3.60		
4. Vegetable and fruit market or wagon										
5. Bakery										
Additional expense for food at home—										
6. Ice cream, candy										
7. Soft drinks, beer, etc.										
8. Other food at home										
9. TOTAL for week or month (1–8)	10.50	3.60	10.50	3.60	9.50	3.60	9.50	3.60		
10. TOTAL for season	147.30		147.30		134.30		134.30			

FOOD AWAY FROM HOME (Excluding meals while away at school, and meals carried from home)	Per week	Per month	Per week	Per month	Per week	Per month	Per week	Per month	Per week	Per month
Expense for—										
11. Meals at work										
12. Lunches at school										
13. Meals while traveling or on vacation										
OTHER MEALS AWAY—										
14. Breakfasts										
15. Lunches										
16. Dinners										
17. Ice cream, candy										
18. Soft drinks, beer, etc.										
19. TOTAL for week or month										
20. TOTAL for season										

TOTAL FOOD EXPENSE DURING SCHEDULE YEAR		FOOD RAISED, GATHERED OR RECEIVED AS GIFT OR PAY DURING SCHEDULE YEAR	
21. Food at home (item 10)	563.20		
22. Food away from home (item 20)			
23. TOTAL	563.20		

14—6290

7119 .090 .088 2

XVII. AUTOMOBILES
(owned at any time during year)

1. How many months during year did you own:
   a 1 automobile, ___ mos.; *b* 2 automobiles, ___ mos.;
   c 3 automobiles, ___ mos.; no automobiles, 12 mos.

AUTOMOBILES OWNED AT END OF SCHEDULE YEAR

A Year bought	B New	C Used	D Make	E Price
2. 19				$
3. 19				
4. 19				
5. Gross price of car bought during year $				x x x x
6. Trade-in allowance for used car, $				x x x x
7. Net price of car bought (5 minus 6)				

8. Month purchased ___ Terms: Cash ☐ Installment ☐
9. Total number of miles driven during year (all owned cars) ___ miles.
10. Average miles per gallon of gasoline, ___ miles.

GASOLINE

A Season	B Number of gallons	C Expense
11. Latest		$
12.		
13.		
14.		
15.		
16. TOTAL FOR YEAR (11–15)		

A	B Expense for year
17. Oil: Number of quarts	$
18. Tires, tubes: Purchase	
19. Repairs, replacements, service	
20. Garage rent, parking	
21. Licenses, including registration fee	
22. Fines, damages paid to others	
23. Automobile insurance (all types)	
24. Tolls (bridge, ferry, tunnel)	
25. Accessories (including automobile radio)	
26. Other (including association dues)	
27. TOTAL (7, 16, and 17–26)	
28. Proportion of automobile expense chargeable to business	x x x x

XVIII. CLOTHING EXPENSE

(Make no entry if check list is used)

	Expense for year
1. Wife	$ 1.00
2. Husband	4.00
3. Daughter	
4. Daughter	5.00
5. Daughter	
6. Son	
7.	
8.	
9. TOTAL	17.00

XIX. OTHER TRAVEL AND TRANSPORTATION

LOCAL—TO WORK, SCHOOL, STORES, ETC.	Expense for year
1. Bus, trolley, taxi, train, ferry boat, rent of automobile	$
OTHER TRAVEL (Excluding business travel)	
2. Railroad (including Pullman)	
3. Interurban bus Trailers	11.00
4. Other (specify vehicle)	
PURCHASE AND UPKEEP DURING YEAR	
5. Of motorcycle	
6. Of boat, airplane, other vehicle	
7. TOTAL (1–6)	11.00
8. Proportion of motorcycle ☐ or other vehicle ☐ expense chargeable to business	x x x x

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XX. PERSONAL CARE

A SERVICES	B Expense for year
1. Wife: Haircut (usual price, ...), shampoo, waves, manicures, facials, other	$
2. Husband: Haircut (usual price, ...), shaves, shampoos, other	
3. Children under 16: Haircuts (usual price, ...), other	
4. Other members of family: Haircut (usual price, ...), other	
TOILET ARTICLES AND PREPARATIONS	
5. Toilet soaps: 12 cakes at .10	5.20
6. Tooth paste and powder, mouth wash, etc.	
7. Shaving soap and cream	1.50
8. Cold cream, powder, rouge, nail polish, perfume	1.00
9. Brushes, etc., combs, razors, files BLADES	1.20
10. Other toilet articles and preparations	.10
11. TOTAL (1–10) (10)	9.00

XXI. EQUIPMENT OWNED BY FAMILY

A KIND OF EQUIPMENT	B Owned at end of schedule year: Yes	C Owned at end of schedule year: No	D If purchased in schedule year: Price	E If purchased in schedule year: Season purchased
1. Piano X		✓	x x x x	
2. Phonograph X		✓	x x x x	
3. Radio ✓		✓	x x x x	
4. Refrigerator, electric		✓	$	
5. Other mechanical refrigerator		✓		
6. Ice box	✓		X	
7. Pressure cooker		✓		
8. Washing machine, power		✓		
9. Washing machine, other		✓		
10. *Ironing machine*		✓		
11. Vacuum cleaner	✓		X	
12. Sewing machine, electric		✓		
13. Other sewing machine	✓		X	

XXII. FURNISHINGS AND EQUIPMENT

(Make no entry if check list is used)

Purchased in schedule year not included in items 4–13, section XXI. Total expense for year, $ ___

38 4 1

14—8290

XXIII. OTHER FAMILY EXPENSE

	Expense for year		Expense for year
1. Interest on debts incurred for family living other than mortgage on owned home	$ 9 00	5. Loss, other than business loss	$
2. Did family have checking account at any time during schedule year? Yes ☐ No ☒		6. Funeral, cemetery	
		7. Other	
3. Bank service charges, safe deposit box			
4. Legal expense (not business)		8. TOTAL (1–7)	9 00

XXIV. CHANGES IN FAMILY ASSETS AND LIABILITIES DURING SCHEDULE YEAR Jan 1 1935 to Dec 31 1935

(Excluding changes due to increases or decreases in the value of property which has not changed hands)

CHANGES IN PROPERTY OWNED BY FAMILY AND AMOUNTS DUE FAMILY			CHANGES IN DEBTS OWED BY FAMILY		
A	B	C	D	E	F
Money, stocks, real estate, other assets	Changes in assets during schedule year		Liabilities	Changes in liabilities during schedule year	
	Net amount of increase	Net amount of decrease		Net amount of increase	Net amount of decrease
1. Money in savings accounts	$	$	21. Mortgages on owned home	$	$
2. In checking accounts			22. Mortgages on other real estate		
3. On hand			23. Notes due to banks, insurance companies, small loan companies	150 00	
4. Investments in business			24. Notes due to individuals		
5. Real estate: Purchased		x x x x x x x x	25. Back rents (due before schedule year)	x x x x x x x x	
6. Sold	x x x x x x x x		26. Rents due in schedule year, unpaid	45 00	x x x x x x x x
7. Stocks and bonds: Purchased		x x x x x x x x	27. Back taxes (due before schedule year)	x x x x x x x x	
8. Sold	x x x x x x x x		28. Taxes due in schedule year, unpaid Poll	3 00	x x x x x x x x
9. Other property: Purchased		x x x x x x x x	29. Charge accounts due		
10. Sold	x x x x x x x x		30. Other bills due Grocery	75 00	
11. Improvements on owned home		x x x x x x x x	31. Payments on installment purchases made prior to schedule year (specify goods purchased):		
12. Improvements on other real estate		x x x x x x x x	(a)	x x x x x x x x	
13. Insurance premiums paid (life, endowment, annuity)	39 00	x x x x x x x x	(b)	x x x x x x x x	
14. Frequency of payment Wukly	1		(c)	x x x x x x x x	
15. Insurance policies surrendered	x x x x x x x x		32. Balance due on installment purchases made in schedule year (specify goods purchased):		
16. Insurance policies settled	x x x x x x x x		(a)		x x x x x x x x
17. Loans made by family to others during schedule year (balance not repaid)		x x x x x x x x	(b)		x x x x x x x x
18. Repayments to family on loans made before schedule year	x x x x x x x x		(c)		x x x x x x x x
19. All other (specify)			33. All other (specify)		
20. TOTAL (1–19)	39 00	—	34. TOTAL (21–33)	276 00	—

(9)

99

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B Measures of consumption calculated from the 1935-36 Study of Consumer Purchases

Unfortunately, the schedules for the expenditure survey did not include a measure of total expenditure; this measure must be built-up from spending on subcategories of consumption. Sometimes the schedule provides a total for a subcategory (like recreation spending). Other times, the totals provided on the schedule must be adjusted because they intermingle consumption expenditures with non-consumption expenditures. For instance, the total provided for housing expenditure includes spending on taxes. Below I describe the construction of 16 categories of consumption, the sum of which equals total consumer expenditure. Given the structure of the survey, and the purposes for which I use it, these categories inevitably do not correspond exactly to their NIPA definitions. For example, I exclude the imputed rental value of owner-occupied housing from my measure of consumption.

B.1 Housing

Housing expenses are detailed in section V of the urban expenditure schedule. I measure consumer spending on housing as the sum of rent paid (if renting), spending on repairs to the house, and spending on home insurance. Also included are expenses for housing while on vacation.

Specifically, housing is the sum of the following lines in section V of the schedule: line 4 + 5 + 15 + 16 + 17 + 21 + 22 + 23. In terms of the variables in the ICPSR dataset, housing equals V359+ V360+ V361+ V362+ V381+ V382+ V383+ V384+ V389+ V390+ V385+ V386+ V387.

B.2 Household operation

Household operation expenses are detailed in section VI of the schedule. This category includes spending on utilities, paid household help, and postage.

For this variable, I use the total provided on the schedule in section VI, line 27. This corresponds to ICPSR variable V639.

B.3 Medical Care

Medical expenses are detailed in section VII of the schedule. This category includes out-of-pocket spending on medical costs as well as spending on health and accident insurance.

For this variable, I use the total provided on the schedule in section VII, line 17. This corresponds to ICPSR variable V686.

B.4 Recreation

Spending on recreation is detailed in section IX of the schedule. This category includes spending on movies, games and sports, activities such as camping and skiing, and purchases of radios, musical instruments, children's toys and other recreational items.

For this variable, I use the total provided on the schedule in section IX, line 20. This corresponds to ICPSR variable V788.

B.5 Tobacco

Spending on tobacco is detailed in section X of the schedule. This category includes spending on cigarettes, cigars, and "smokers' supplies."

For this variable, I use the total provided on the schedule in section X, line 5. This corresponds to ICPSR variable V798.

B.6 Reading

Spending on reading materials is detailed in section XI of the schedule. This category includes spending on newspapers, magazines, and non-school books.

For this variable, I use the total provided on the schedule in section XI, line 7. This corresponds to ICPSR variable V812.

B.7 Education

Spending on education is detailed in section XII of the schedule. This category includes spending on elementary, secondary, and post-secondary education.

For this variable, I use the total provided on the schedule for tuition and supply expenses (section XII, line 10) plus spending on board and room rent at school (lines 11 and 12). This corresponds to ICPSR variables V837 + V838 + V839.

B.8 Occupational Expenses ("not reported as business expenses or as deduction from gross income")

Spending on occupational expenses is detailed in section XIII on the schedule. It includes spending on union and professional association dues, technical books, and miscellaneous supplies and equipment.

For this variable, I use the total provided on the schedule in section XIII, line 6. This corresponds to ICPSR variable V850.

B.9 Gifts

Spending on gifts "to persons not members of economic family (not charity)" are reported in section XV, line 1. This corresponds to ICPSR variable V853.

B.10 Food

Spending on food is detailed in section XVI of the schedule. This category includes spending on food and drink (including alcohol) both at home and away from home.

For this variable, I use the total provided on the schedule in section XVI, line 23, which corresponds to ICPSR variable V1077.

B.11 Autos

Spending on auto related expenses is detailed in section XVII of the schedule. This category includes spending on auto purchases (new or used), repairs and accessories, as well as costs of auto operation like gasoline, parking, and tolls.

For this variable, I use the total provided on the schedule, in section XVII, line 27 minus the amount of auto expenses chargeable to business, line 28. The amount chargeable to business is sometimes reported as a dollar amount and sometimes reported as a proportion. I assume the latter if the variable is less than 1. This corresponds to ICPSR variable V1147 - V1148 or V1147*(1-V1148). Note that this measures the value of a car purchased as the net price, i.e. the gross price less the trade-in allowance for a used car, if any.

I measure the amount spent on auto purchases as the net price of any car purchased (section XVII, line 7, V1114). Spending on vehicle operations is defined as total autos spending minus spending on purchases. Finally, to create a dummy variable for whether or not a household purchased a car, I create a variable equal to one if the household spent a positive amount on auto purchases and equal to zero otherwise.

B.12 Clothing

Spending on clothing is in section XVIII of the schedule. For this variable, I use the total provided on the schedule in line 9, which corresponds to ICPSR variable V1254.

B.13 Other travel and transportation

This is section XIX of the schedule. It includes spending on local public transit, long-distance rail and bus, and purchases and maintenance of motorcycles and boats.

For this variable, I use the total provided on the schedule in section XIX, line 7, which corresponds to ICPSR variable V1262. (Unfortunately, there are no usable answers to the question asking what proportion of these expenses were chargeable to business.)

B.14 Personal Care

Spending on personal care is detailed in section XX of the schedule. This category includes spending on services such as haircuts and products like soap and toothpaste.

For this variable, I use the total provided on the schedule in section XX, line 11, which corresponds to ICPSR variable V1290.

B.15 Equipment

This corresponds to section XXI on the expenditure schedule and to the total from a separate furniture and equipment checklist. Nearly all furniture and appliances are included in this category, for instance, refrigerators, washing machines, and lawn mowers. This category also includes spending on household linens and kitchen supplies (silverware, pots, china, and so on).

For this variable I use the total from the check list, if available. This is line 98, column D, ICPSR V2868. If the total from the checklist is missing, as is true in 570 cases, I use the total of all the lines in section XXI (lines 4-13). This corresponds to the ICPSR variables V1298 + V1301 + V1304 + V1307 + V1310 + V1313 + V1315 + V1318 + V1321 + V1324.

B.16 Other

This corresponds to section XXIII on the schedule. In my measure of consumption I include bank service charges (line 3, V1336), non-business legal expenses (line 4, V1337), funeral and cemetery costs (line 6, V1339), and miscellaneous other expenses (line 7, V1340).

C Consistency of the household survey two-step estimator

Define variables as follows: Y_i is consumption of household i; X_{1i} is a dummy variable equal to 0 if a household was surveyed before the bonus payment (i.e. before June 1936) and equal to 1 if surveyed after; X_{2i} is a dummy variable equal to 1 if the husband in the household is a veteran and 0 if not; Z equals a set of control variables for age, geographic location, and race (including a constant).

The goal is to obtain a consistent estimate of β_3 in

$$Y_i = \beta_1 X_{1i} + \beta_2 X_{2i} + \beta_3 (X_{1i} X_{2i}) + Z_i' \beta_4 + \varepsilon_i. \tag{9}$$

Since X_{2i} (veteran status) is unobserved in the household survey, I proxy for it by estimating

$$X_{2j} = Z_j' \gamma + \upsilon_j \tag{10}$$

Note the subscripts j rather than i in the second regression, since it is estimated on a separate sample from (9). I will show that given a set of reasonable assumptions, estimation of (9) and (10) by OLS provides a consistent estimate of β_3.

Assumptions

1. $E[Z_i \varepsilon_i] = 0$. This assumption says that age, race, and location are exogenous in equation 9.

2. $E[X_{1i} \varepsilon_i] = 0$. This says that whether one was surveyed before or after June 1936 is exogenous in equation 9.

3. $E[Z_i \upsilon_i] = 0$, i.e. the errors in the first stage are uncorrelated with the control variables. This is true by the properties of OLS. Note that it is because of this condition that the first stage must be estimated with OLS rather than with a probit or logit. Although a probit or logit model would provide better predictions of the probability that a household includes a veteran, the residuals would not necessarily be uncorrelated with the control variables.

4. X_{1i} is independent of Z_i and X_{2i}. This amounts to assuming that households were randomly assigned to be interviewed before or after June 1936. While not literally true, this assumption is reasonable. There is no significant correlation in the data between observables like age and race and when the household was surveyed.

5. γ is the same in both samples. This is perhaps the strongest assumption; it means assuming that the true relationship between the control variables (age, race, location)

and veteran status is the same in the 1930 census and in the 1936 household survey. In order to come as close as possible to satisfying this assumption, I estimate the first stage on a sample from the 1930 Census that closely matches the sample of households in the household survey. For instance, in the first stage I include only married men living in urban areas.

6. The samples are independent.

Sketch of proof
Rewrite 9 substituting in 10:

$$\begin{aligned} Y_i &= \beta_1 X_{1i} + Z_i'[(\hat{\gamma} + \gamma - \hat{\gamma})\beta_2 + \beta_4] + \beta_3 X_{1i} Z_i'(\hat{\gamma} + \gamma - \hat{\gamma}) + \beta_2 \upsilon_i + \beta_3 X_{1i}\upsilon_i + \varepsilon_i \\ &= \beta_1 X_{1i} + Z_i'(\hat{\gamma}\beta_2 + \beta_4) + \beta_3 X_{1i} Z_i'\hat{\gamma} + \tilde{\varepsilon}, \end{aligned} \tag{11}$$

where $\tilde{\varepsilon} = \beta_2 Z_i'(\gamma - \hat{\gamma}) + \beta_3 X_{1i} Z_i'(\gamma - \hat{\gamma}) + \beta_2 \upsilon_i + \beta_3 X_{1i}\upsilon_i + \varepsilon_i$.

Let $\hat{X}$ be the right-hand-side variables in equation 11, i.e. $\hat{X}' =$

$$\begin{pmatrix} X_1' \\ (X_1 Z\hat{\gamma})' \\ Z' \end{pmatrix}$$

and let β be the vector of identifiable coefficients, i.e. β is the k by 1 vector

$$\begin{pmatrix} \beta_1 \\ \beta_2\gamma + \beta_4 \\ \beta_3 \end{pmatrix}$$

Then the estimates from my two-stage least squares procedure, $\hat{\beta}_{2s}$, are equal to

$$\begin{aligned} &= (\hat{X}'\hat{X})^{-1}\hat{X}'Y \\ &= (\hat{X}'\hat{X})^{-1}\hat{X}'(\hat{X}\beta + \tilde{\varepsilon}) \\ &= \beta + (\hat{X}'\hat{X})^{-1}\hat{X}'\tilde{\varepsilon}. \end{aligned} \tag{12}$$

My procedure will provide a consistent estimate of β if the probability limit of $(\hat{X}'\hat{X})^{-1}\hat{X}'\tilde{\varepsilon}$ is zero. To show that this is true, I show that each element of $\hat{X}$ is uncorrelated with the $\tilde{\varepsilon}$.

1. First note that by assumption (5) and the properties of OLS, as the size of the second (census) sample goes to infinity, $\hat{\gamma}$ converges in probability to γ, so plim $\tilde{\varepsilon}$ = plim $\beta_2\upsilon_i + \beta_3 X_{1i}\upsilon_i + \varepsilon_i$. This result will be used in each step that follows.

2. Noting that since X_{1i} is a dummy variable, $X_{1i}^2 = X_{1i}$

$$cov(X_{1i}, \tilde{\varepsilon}_i) = (\beta_2 + \beta_3)cov(X_{1i}, \upsilon_i) + cov(X_{1i}, \varepsilon_i) \tag{13}$$

By assumption (4) the first term on the right hand side equals zero, and by assumption (2) the second term equals zero.

3. Again using the fact that $X_{1i}^2 = X_{1i}$,

$$\begin{aligned} cov(X_{1i}Z_i'\hat{\gamma}, \tilde{\varepsilon}_i) &= cov(X_{1i}Z_i'\hat{\gamma}, \beta_2\upsilon_i) + cov(X_{1i}Z_i'\hat{\gamma}, \beta_3 X_{1i}\upsilon_i) + cov(X_{1i}Z_i'\hat{\gamma}, \varepsilon_i) \\ &= (\beta_2 + \beta_3)E[X_{1i}Z_i'\hat{\gamma}\upsilon_i] + E[X_{1i}Z_i'\hat{\gamma}\varepsilon_i] \\ &= (\beta_2 + \beta_3)E[X_{1i}]E[Z_i'\hat{\gamma}\upsilon_i] + E[X_{1i}]E[Z_i'\hat{\gamma}\varepsilon_i] \end{aligned} \tag{14}$$

   The last line uses assumption (4), the fact that X_{1i} is independent of Z_i and X_{2i}. The first term of (14) is zero by assumptions (3) and (6), and the second term is zero by assumption (1).

4. Finally,

$$cov(Z_i, \tilde{\varepsilon}) = \beta_2 cov(Z_i, \upsilon_i) + \beta_3 cov(Z_i, X_{1i}\upsilon_i) + cov(Z_i, \varepsilon_i) \tag{15}$$

   Assumption (3) means the first term on the right hand side is zero; assumptions (3) and (4) make the second term zero, and assumption (1) means the last term is zero.

Thus plim $\frac{1}{N}\hat{X}'\tilde{\varepsilon}$ is zero, and plim $\hat{\beta}_{2s} = \beta$. Given assumptions 1-6, two-stage least squares provides consistent estimates of β.

Of course, the two-step procedure means that the standard OLS standard errors will be incorrect. Because of this, I compute bootstrap standard errors; see section 3.2.

D Strata

According to the Bureau of Labor Statistics (1941a, p. 372)

> The cities included in the Consumer Purchases Study were chosen to represent the metropolis, the large city with a population from 250,000 to 300,000, the middle-sized city with a population from 35,000 to 70,000, and the small city with a population of from 10,000 to 20,000.

Large and middle sized cities were selected from each of 6 regions: Northeast, Southeast, East Central, West Central, Rocky Mountain, and Pacific Northwest. In addition, the two 'metropolises' New York, and Chicago were included, as were several small cities in the northeast and east central regions.

To avoid problems with strata of 1 sampling unit, I collapse these 15 region-city-size strata into 9. This is conservative, since having more strata reduces standard errors (Deaton 1997). Table 16 shows this classification.

Table 16: Strata classification

City	Region	Size classification	Strata assignment
Aberdeen-Hoquiam, WA	Pacific Northwest	Middle-sized	Pacific Northwest - large
Albany, GA	Southeast	Small	Southeast - small
Atlanta, GA	Southeast	Large	Southeast - large
Beaver Falls, PA	East Central	Small	East Central - small
Bellingham, WA	Pacific Northwest	Middle-sized	Pacific Northwest - large
Butte, MT	Rocky Mountain	Middle-sized	Rocky Mountain - large
Chicago, IL	East Central	Large (metropolis)	East Central - large
Columbia, SC	Southeast	Middle-sized	Southeast - large
Columbus, OH	East Central	Large	East Central - large
Connellsville, PA	East Central	Small	East Central - small
Council Bluffs, IA/Omaha, NE	West Central	Large	West Central - large
Denver, CO	Rocky Mountain	Large	Rocky Mountain - large
Dubuque, IA	West Central	Middle-sized	West Central - large
Everett, WA	Pacific Northwest	Middle-sized	Pacific Northwest - large
Gastonia, NC	Southeast	Small	Southeast - small
Greenfield, MA	Northeast	Small	Northeast - small
Haverhill, MA	Northeast	Middle-sized	Northeast - large
Logansport, IN	East Central	Small	East Central - small
Mattoon, IL	East Central	Small	East Central - small
Mobile, AL	Southeast	Middle-sized	Southeast - large
Muncie, IN	East Central	Middle-sized	East Central - large
New Britain, CT	Northeast	Middle-sized	Northeast - large
New Castle, PA	East Central	Middle-sized	East Central - large
New York, NY	Northeast	Large (metropolis)	Northeast - large
Peru, IN	East Central	Small	East Central - small
Portland, OR	Pacific Northwest	Large	Pacific Northwest - large
Providence, RI	Northeast	Large	Northeast - large
Pueblo, CO	Rocky Mountain	Middle-sized	Rocky Mountain - large
Springfield, IL	East Central	Middle-sized	East Central - large
Springfield, MO	West Central	Middle-sized	West Central - large
Wallingford, CT	Northeast	Small	Northeast - small
Westbrook, ME	Northeast	Small	Northeast - small
Willimantic, CT	Northeast	Small	Northeast - small