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New Deal Policies and Recovery from the Great Depression

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by

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Committee in charge:

Professor Barry Eichengreen, Co-Chair
Professor J. Bradford DeLong, Co-Chair
Professor Christina Romer
Professor Maurice Obstfeld
Professor Noam Yuchtman

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Abstract

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What forces led to rapid recovery of the U.S. economy after 1933? Why was recovery derailed by a severe recession in 1937? Since Friedman and Schwartz (1963), economists have emphasized monetary explanations. This dissertation shows that in three crucial instances other factors mattered. Methodologically, it demonstrates the value of using micro data to explore macro questions.

The first chapter considers the effect of the 1936 veterans' bonus. Conventional wisdom has it that in the 1930s fiscal policy did not work because it was not tried. This chapter shows that fiscal policy, though inadvertent, was tried in 1936, and a variety of evidence suggests that it worked. A deficit-financed veterans' bonus provided 3.2 million World War I veterans with cash and bond payments totaling 2 percent of GDP; the typical veteran received a payment equal to annual per capita personal income. This chapter uses time-series and cross-sectional data to identify the effects of the bonus. I exploit four sources of quantitative evidence: a detailed household consumption survey, cross-state and cross-city regressions, aggregate time-series, and a previously unused American Legion survey of veterans. The evidence paints a consistent picture in which veterans quickly spent the majority of their bonus. Spending was concentrated on cars and housing in particular. Narrative accounts support these quantitative results. A back-of-the-envelope calculation suggests that the bonus added 2.5 to 3 percentage points to 1936 GDP growth.

In my second chapter, I consider the causes of the severe recession that followed the boom year of 1936. The 1937-38 recession was one of the largest in U.S. history. Industrial production fell 32 percent and the nonfarm unemployment rate rose 6.6 percentage points. This paper shows that there were timing, geographic, and sectoral anomalies in the recession, none of which are easily explained by aggregate macro shocks. I argue that a supply shock in the auto industry contributed both to the recession's anomalies and to its severity. Labor-strife-induced wage increases and an increase in raw material costs led auto manufacturers to raise prices in the fall of 1937. Equally important, higher costs combined with nominal rigidity to make the price increase predictable. Expectations of price increases brought auto

sales forward and thus sustained sales during the summer and early fall of 1937, despite negative monetary and fiscal factors. When auto prices did rise in October and November, sales and production plummeted. A forecasting exercise suggests that in 1938, this shock reduced auto sales by 600,000 and GDP growth by 1.2 percentage points.

In my third chapter, I consider the miraculous recovery of the U.S. economy during Franklin Roosevelt's first months in office. From March to July 1933, industrial production rose 57 percent, by far the most rapid increase since records began. The recovery was concentrated in durable goods industries, particularly autos. Using a novel dataset, I show that auto sales increased much more rapidly in farm than in nonfarm states in spring 1933. This suggests that dollar devaluation may have directly spurred recovery by raising farm incomes. The negative effect of higher crop prices on urban consumers may have been offset by the positive effect of higher crop prices on expected inflation.

To Catie and Alex.

Contents

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In 2009-10 I worked as a staff economist at the CEA. There, with much help from Christina, I learned how to understand macro data and the importance of careful fact-checking in empirical work. Soon after I returned to Berkeley in summer 2010, so did Christina. Since then, her advice has been invaluable. She encouraged me to go after big questions and trust that I would find a way to answer them. And her careful reading of my work helped me express my thoughts far more clearly. Finally, Christina's enthusiasm and encouragement sustained my morale at times when research problems felt insurmountable.

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Introduction

The U.S. recovery from the Great Depression was nearly as exceptional as the Depression itself. When Franklin Roosevelt took office in March 1933, the unemployment rate was 21 percent, real GNP per capita was lower than it had been in 1907, and much of the banking system had collapsed.[1] The Depression had done much to discredit American democracy. The prominent columnist Walter Lippmann argued, not atypically, that "A mild species of dictatorship will help us over the roughest spots in the road ahead" (qtd. in Alter 2007, p. 187). Yet seven years later, in 1940, American democratic institutions were intact. The unemployment rate had fallen to 9.5 percent and GNP per capita had grown 54 percent. Productivity had advanced at an unprecedented pace (Field 2011). But recovery had not been uniform. It was interrupted by a severe recession in 1937-38 in which the unemployment rate rose more than 3 percentage points.

Economic historians have long been interested in what caused the 1929 to 1933 U.S. downturn, but comparatively little work examines the recovery that followed.[2] This dissertation helps fill this gap. It adds to our understanding both of the rapid recovery and of the double-dip recession. I consider three episodes that illuminate the role of fiscal policy, unionization, and devaluation in this period. The first chapter examines the effects of a 2 percent of GDP fiscal stimulus program, the veterans' bonus of 1936. I document that fiscal policy worked: veterans spent a large share of their bonus. The second chapter examines the causes of the severe recession that followed the boom year of 1936. I show that a supply shock in the auto industry helps to explain the recessions' severity as well as its timing and geographic incidence. Finally, in the third chapter, I consider the sources of recovery during Franklin Roosevelt's first months in office. Just as a shock to the relative price of autos played an important role in returning the economy to recession in 1937, I suggest that changes to the relative price of farm products may have been crucial to rapid recovery in spring 1933.

In addition to its focus on a common time and place, two themes unify this work. First, all three papers show the importance of non-monetary factors during the 1930s. GDP growth of 13 percent in 1936 and -3 percent in 1938 can only be explained by the interaction of monetary

[1]The unemployment rate is from Darby (1976). Real GNP data are from Romer (1989) and NIPA table 1.7.6. Population data are from `http://www.census.gov/popest/data/national/totals/pre-1980/tables/popclockest.txt`.

[2]Exceptions include Cole and Ohanian 2004, Eggertsson 2008, and Romer 1992.

factors with fiscal policy and exogenous developments in the auto industry. Chapter three suggests that rapid recovery in spring 1933 was due in part to the relative price effects on farmers of dollar devaluation. To be clear, none of these chapters disputes the primary importance of monetary factors in explaining recovery and the double-dip recession. Rather, I show that at crucial times, non-monetary factors mattered.

The second theme of this dissertation is the role of the auto industry as an amplifying mechanism for demand and supply shocks. The veterans' bonus had large effects because veterans used their money to buy cars. Unionization in 1937 interacted with price-setting practices in the auto industry to create a large macro shock. And in spring 1933, auto production grew far more rapidly than industrial production as a whole, in part spurred by farmers enjoying higher crop prices.

Typically, such industry-specific factors are absent from economists' accounts of this period. The existing literature emphasizes either the economy's automatic recovery mechanisms or the importance of monetary policy. 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 monetary regime change. Eggertsson (2008) formalizes this argument. In the context of a DSGE model, Eggertsson shows how Roosevelt's actions, by raising expected inflation and thus lowering the real interest rate, could have spurred rapid growth.

Just as expansionary monetary policy is often thought to explain rapid growth from 1933 to 1937, contractionary monetary policy is the most popular explanation for the 1937-38 recession.[3] The Federal Reserve raised reserve requirements in August 1936 and again in March and May 1937. In December 1936, the Treasury began sterilizing gold inflows. Friedman and Schwartz argue (1963, pp. 544-545):

> The combined impact of the rise in reserve requirements and–no less important– the Treasury gold-sterilization program first sharply reduced the rate of increase in the money stock and then converted it into a decline. . . . The sharp retardation in the rate of growth of the money stock must surely have been a factor curbing expansion, and the absolute decline, a factor intensifying contraction.

[3]See, for example, Roose (1954), Friedman and Schwartz (1963), Eichengreen (1992), Romer (1992), Velde (2009), and Irwin (2011). Some authors have also considered contractionary fiscal policy as a possible cause of this recession. See chapter 2.

There is an emerging consensus that the key shock was the Treasury gold sterilization rather than the increase in reserve requirements. Irwin (2011) estimates that absent gold sterilization, in 1937 the monetary base would have been as much as 10 percent larger. The channel through which money supply declines impacted real activity in 1937-38 is, however, unclear. There was relatively little effect on interest rates. Yields briefly spiked by 10 to 40 basis points in the spring and summer of 1937, but remained close to zero over the period.

Of course when interest rates are near zero, monetary policy can still change expectations of inflation, and thus affect the real interest rate and the real economy. This is the channel through which Eggertsson (2008) and Romer (1992) argue that policy led to economic recovery after 1933. Eggertsson and Pugsley (2006) argue that policymakers' actions in the spring of 1937 lowered inflation expectations, and through this channel, caused the 1937-1938 recession. This provides one way of explaining how monetary actions, despite little impact on interest rates, may have had large contractionary effects.

This brief tour of the literature suggests a unified monetarist interpretation of the 1930s: when money growth was rapid, as it was from 1933 through 1936, and again after 1938, growth was rapid. When money growth slowed, as it did in 1937-38, so did growth. We shall see that this story is too simple.

The first chapter considers fiscal policy. The conventional wisdom has it that in the 1930s fiscal policy did not work because it was not tried. In this paper, I show that fiscal policy, though inadvertent, was tried in 1936, and a variety of evidence suggests it worked. In June 1936, a deficit-financed veterans bonus provided 3.2 million World War I veterans with cash and bond payments totaling 2 percent of GDP; the typical veteran received a payment equal to annual per capita personal income, enough money to buy a new car.

This paper uses cross-sectional data to identify the effects of the bonus. I exploit four sources of quantitative evidence: a detailed household consumption survey, cross-state and cross-city regressions, aggregate time-series, and a previously unused American Legion survey of veterans. The evidence paints a consistent picture in which veterans quickly spent the majority of their bonus. The point estimate of the marginal propensity to consume is 0.7, with at least half this spending on cars and housing. Combined with a reasonable estimate of the multiplier, this suggests that the bonus added 2.5 to 3 percentage points to 1936 GDP growth.

This chapter contributes both to the economic history of the 1930s, and to the literature on the consumption effects of fiscal transfers. In particular, I show that the marginal propensity to consume can be high even when transfer payments are very large. I argue that features of the 1936 economy, some unique to the time, some generally present in economic recoveries, were 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. Indeed, receiving the bonus made veterans 22 percentage points more likely to purchase a car. Finally, since interest rates were at the zero lower bound, the spending multiplier is likely to have been high.

Thus the effect of the bonus was not independent of the state of the economy. In particu-

lar, it mattered that the share of the population that owned cars was large and growing.[4] In chapter two, I argue that the auto industry was sufficiently large to play a central role in the 1937-38 recession. The conventional wisdom blames the double-dip recession on monetary contraction, and to a lesser extent on the backside of the veterans' bonus and the instatement of social security taxes. But while these factors were undoubtedly important, in the first half of this chapter I show that there are in fact timing, geographic, and sectoral anomalies in the recession not easily explained by macro policy. Specifically:

- The decline in industrial production from September to December 1937 is the largest three-month decline in the history of the Federal Reserve and Miron-Romer series going back to 1884.
- Manufacturing employment rises in 8 states despite a 12 percent decline nationwide.
- There is a lack of comovement between components of GDP. Durables consumption falls over 17 percent while nondurables consumption rises. The 1937-38 recession is the only time since the Great Depression in which nondurables spending rises in a recession in which annual real GDP falls more than 2 percent.

My analysis points strongly to a role for the auto industry in explaining the unusual features of the recession: manufacturing employment falls most in Michigan, and auto production falls by nearly 50 percent. Furthermore, auto prices rise while the economy-wide price level falls. In the second half of the chapter, I ask what explains the peculiar behavior of the auto industry, and how exogenous developments in this industry affected the rest of the economy. I argue that a cost shock and consequent auto price increases explain the collapse of car sales and production. Unionization and an increase in raw material costs led auto manufacturers to raise prices in fall 1937. Equally important, the increase in costs combined with nominal rigidity to make the price increase predictable. Expectations of price increases brought auto sales forward and thus sustained sales during the summer and early fall of 1937, despite negative monetary and fiscal factors. When auto prices did increase in fall 1937, sales plummeted. Narrative evidence confirms this story.

To quantify the effect on auto sales, I perform a forecasting exercise. Using criteria that penalize overfitting, I choose the model that best matches the monthly pre-recession behavior of auto sales given fiscal and monetary factors and overall economic conditions. As expected, the model underpredicts sales in the summer of 1937, when consumers expected auto prices to soon increase, and overpredicts sales in 1938. This exercise implies that without the expected increase in auto prices, the fall in auto sales between 1937 and 1938 would have been 0.8 million rather than the actual 1.6 million. Using data on the price of cars sold and an estimate of the multiplier, I find that positive price expectations added 0.3 percent

[4] In 1935, there was roughly one car on the road for every 6 people in the country; the population was 127 million and there were 22.5 million cars registered. Population data are from `http://www.census.gov/popest/data/national/totals/pre-1980/tables/popclockest.txt`; car registration data are from NBER macrohistory series a01108.

to GDP in 1937, while the subsequent drop-off in sales subtracted 1.0 percent from GDP in 1938: absent the shock, the output decline in 1938 would have been more than a third smaller.

This paper contributes to our understanding of the severe 1937-38 recession and provides an unusually clear example of how supply shocks can affect the economy. It also shows that even before World War II, the auto industry was sufficiently large for fluctuations in production and sales to have important aggregate effects.[5]

Perhaps surprisingly, the 1937-38 recession may also help us understand the 1933 recovery. My third chapter considers the causes of U.S. growth in the immediate aftermath of Roosevelt's inauguration. From its low point in March 1933, seasonally adjusted industrial production rose 57 percent in four months.[6] The existing literature has emphasized two explanations for the economy's emergence from the Depression in spring 1933: first, the effects of Franklin Roosevelt's words and actions on inflation expectations, and second, the direct effects of the dollar's devaluation on farmers. Both explanations can in theory explain the economy's turn-around and the concentration of the recovery in durable goods' industries, particularly autos.

It has been difficult to know whether there was a distinct channel leading to recovery through agriculture absent high frequency geographically disaggregated data. We have not known whether recovery proceeded more or less rapidly in North Dakota or in New York. The contribution of this chapter is to provide such data and to use it to test whether recovery proceeded more quickly in agricultural states. I exploit a previously unused dataset of monthly, state level auto sales to examine whether the share of a state population's engaged in farming was correlated with auto sales recovery in spring 1933. I find that it was. A one standard deviation increase in the share of a state's population living on farms was associated with roughly a 25 percentage point increase in auto sales growth from late winter to early summer 1933. In aggregate, my results imply that the recovery, at least as measured by auto sales, might have been as much as 50 percent slower if farm demand had not grown more rapidly than demand in the economy as a whole.

As in 1937, price expectations may have been an important transmission channel. I hypothesize that although higher crop prices only directly benefited the 25 percent of the population in agriculture,[7] they may have contributed to consumer expectations of future price increases. This in turn could have boosted consumer demand in urban and rural areas. An attraction of this explanation is that it can explain both the consumption and production boom in spring 1933 and the subsequent partial reversal in late summer 1933. Expectations of price increases would have meant more goods sold in spring 1933 and fewer sold in summer and fall.

[5]Ramey and Vine (2006) document the importance of the auto industry to postwar fluctuations.
[6]FRED series INDPRO.
[7]Share of the population in agriculture is taken from the 5% IPUMS sample of the 1930 Census.

Chapter 1

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

> *"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).

1.1 Introduction

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 chapter 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

[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.

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 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 the age, race, and location of each household. These variables also appear in the 1930 census, along with an indicator for World War I veteran status. In the first step I estimate the relationship between veteran status and age, race, and location. The second step relates these predicted values - the probability a household contains a veteran - to the change in consumption pre to post bonus payment. I outline a proof that, given a set of reasonable assumptions, this procedure provides consistent estimates of spending from the bonus.

The robust result is that the MPC was 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

[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.

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 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 spending. Thus the prospective MPC of 0.4 measured in the American Legion Survey suggests that the actual MPC was probably 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 chapter 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

[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.

[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.

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 Industrial Recovery Act may have been expansionary by raising prices and expected inflation and thus lowering the real interest rate. While this chapter 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 chapter 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 very large payments is likely to be relatively small. While a definitive explanation is beyond the scope of this paper, I argue that characteristics of the 1936 economy, some unique to the time, some generally present after deep recessions, made the MPC high despite the size of the transfer payment.

I proceed in the next section by providing background on the veterans' bonus. Section 1.3 reports results from the 1935-36 consumer expenditure survey. Section 1.4 reports results from cross-state and cross-city regressions. Section 1.5 compares these findings to tabulations from a large survey of veterans conducted by the American Legion and to narrative evidence. Section 1.6 considers reasons why the MPC from the bonus was so high. Section 1.7 discusses the aggregate implications of my empirical results. Section 1.8 concludes.

[11]Recent empirical studies include Souleles (1999), Hsieh (2003), Shapiro and Slemrod (2003, 2009), Johnson, Parker, and Souleles (2006), and Parker, Souleles, Johnson and McClelland (2011).

1.2 Background on the veterans' bonus

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.[12]

In the early 1920s, Congress considered numerous bonus bills.[13] 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 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).[14] Because the bonus was formally an insurance policy, a veteran's bonus was both *de jure* and *de facto* non-tradable.

[12] NBER Macrohistory series a08050.

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

[14] 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

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).[15]

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. 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

case was approximately two and one-half times the net service credit."

[15]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`.

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.[16] 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 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).

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). Absent liquidity constraints, the expected passage of the bill ought to have led to a spending response in January, if not before. In fact, I will show evidence that much spending from the bonus happened only after actual cash payment in June 1936.

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 1.1 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.

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

[16]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."

Table 1.1: 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.

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.[17] 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, since the 3% interest paid on bonds was above the legal maximum interest rate paid on savings accounts (Telser 2003).[18]

The bonus amount received by a typical veteran, $547, was an enormous sum. Table 1.2 provides some metrics to interpret the magnitude. The first three rows provide measures of

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

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

Table 1.2: The magnitude of the bonus

	1936	2011	2011 Bonus equivalent
Per capita annual income	$ 535	$ 41,633	$ 42,534
Ave. annual wage of fed. emergency workers	$ 595	-	-
Ave. hourly earnings in manufacturing	$ 0.62	18.94	$ 16,743
CPI (Index, 1936=100)	100	1622	$ 8,869
Nominal house prices (Index, 1936=100)	100	2438	$ 13,330
Price of cheapest Ford	$ 510	$13,200	$ 14,151

Sources: Per capita income: NIPA table 2.1; annual wage of federal emergency workers: Darby 1976; average hourly earnings in manufacturing: NBER Macrohistory series m08142 and BLS establishment data, historical hours and earnings table B-2; 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`.

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 2011, and the third column provides the 2011 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 2011 would be more than $40,000. Since manufacturing wages have risen much less than per capita income, the analogous 2011 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.

1.3 Evidence from the 1935-36 Study of Consumer Purchases

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 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.[19] 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.[20]

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,[21] one or two large cities, two or three mid-sized cities, and several small cities were selected (Natural Resources Committee 1939).[22] 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.[23] [24]

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

[20] 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.

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

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

[23] 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.

[24] 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).

Clearly the population sampled by the urban expenditure survey differed from that of the U.S. as a whole. Table 1.3 provides some indicators of how the urban expenditure sample compares to the entire U.S. population. For comparison, the table also includes statistics on the population of World War I veterans. The differences between the urban household survey sample and the U.S. population are an obvious consequence of the sampling procedures. To the extent that these differences were correlated with the size of a veteran's bonus or with what a veteran did with his bonus, my results may not apply to the entire population of veterans. 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.

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.[25]

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

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."

[25]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 table 1.8.

Table 1.3: Sample characteristics

	Population (%)	Vets (%)	1935-36 hh survey (%)
Urban, 1930	45.8	56.4	100
Married, if man age 30-50, 1930	80.3	77.7	100
Black, 1930	9.7	6.8	6.6
On relief, 1936	14.2	-	0
Unemployment rate, 1936	10.0	-	0

Note: Urban is defined as living in a city of 10,800 or larger, since 10,800 is the smallest city in the urban household survey.
Sources: Data on the percent of the U.S. population and of World War I veterans that lived in urban areas, was married, and or was black: IPUMS 5% sample of the 1930 census. Data on the number people living in households on relief comes from Chandler (1970, table 11-2, column 3). I divide this by the U.S. population in 1936, taken from `http://www.census.gov/popest/data/national/totals/pre-1980/tables/popclockest.txt`. The unemployment rate is from Darby (1976, table 3, column 16).

> and statistics, and in many cases with experience in the direction of surveys. The field agents and editors were selected from persons of clerical and professional rating on Works Progress Administration rolls by mean of aptitude tests.
> . . .
>
> 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.

The appendix contains an example of a completed expenditure schedule, and provides a detailed description of how I compute measures of consumption aggregates from the survey responses. Table 1.4 provides summary statistics for the 33 cities included in the urban sample.[26] It provides information on city populations and the number of World War I veterans

[26]Although the IPCSR 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 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.

Table 1.4: Household survey summary statistics

City	1930 pop.	% of pop. WWI vets	# consump. survey	# post bonus
Aberdeen, WA and Hoquiam, WA	34,400	4.1	34	8
Albany, GA	14,500	3.2	16	0
Atlanta, GA	270,300	3.3	239	76
Beaver Falls, PA	17,100	4.3	34	12
Bellingham, WA	30,800	4.0	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	2.6	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	2.4	68	17
Everett, WA	30,500	3.3	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	2.4	42	11
Mattoon, IL	14,600	3.1	48	20
Mobile, AL	68,200	3.5	126	31
Muncie, IN	46,500	3.5	74	31
New Britain, CT	68,100	2.7	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	4.8	196	50
Providence, RI	252,900	3.0	140	24
Pueblo, CO	50,000	3.1	77	23
Springfield, IL	71,800	4.3	122	33
Springfield, MO	57,500	3.5	73	45
Wallingford, CT	11,100	4.2	11	10
Westbrook, ME	10,800	5.0	45	5
Willimantic, CT	12,100	1.2	18	17
Total	12,810,820	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 1.5: 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 the appendix.

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 1.5 shows summary statistics for major categories of consumption. Food made up the largest share of consumption with housing a distant second. Note that housing is housing *consumption*, e.g. rent and repairs, not housing investment. It excludes spending on new home construction, structural additions or mortgage payments. Excluding housing investment from my total consumption measure aligns with the NIPA definition of consumption and the contemporary consumer expenditure (CE) survey.[27]

Specification

An ideal survey for this chapters' purpose would have asked each household if they received a bonus, and if so, how much it was. The actual survey did not do this. A second best would be if the survey had asked if the husband of the family was a World War I veteran, an excellent proxy for whether a bonus payment was received. One could then estimate

$$\begin{aligned}\text{Consumption over previous 12 mths}_i = \alpha &+ \beta_1 \cdot \text{Veteran dummy}_i + \beta_2 \cdot \text{Post bonus dummy}_i \\ &+ \beta_3 \cdot \text{Veteran dummy}_i \cdot \text{Post bonus dummy}_i \\ &+ Z_i'\beta_4 + \varepsilon_i, \end{aligned} \tag{1.1}$$

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.[28] This is

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

[28]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.

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.[29] 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 1.1. Thus the estimation equation is

$$\begin{aligned}\text{Consumption}_i = \alpha + \beta_1 \cdot \text{Prob. veteran}_i + \beta_2 \cdot \text{Post bonus dummy}_i \\ + \beta_3 \cdot \text{Prob. veteran}_i \cdot \text{Post bonus dummy}_i + Z_i'\beta_4 + \varepsilon_i, \qquad (1.2)\end{aligned}$$

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.[30] 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

$$X_j = Z_j'\gamma + \mu_j, \qquad (1.3)$$

and the second stage regression

$$Y_i = \alpha + (Z_i'\hat{\gamma})\beta_1 + P_i\beta_2 + ((Z_i'\hat{\gamma})P_i)\beta_3 + Z_i'\beta_4 + (Z_i'P_i)\beta_5 + \varepsilon_i. \qquad (1.4)$$

[29] 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.

[30] 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.

Estimating 1.4 is equivalent to estimating

$$Y_i = \alpha + Z_i'(\beta_1\hat{\gamma} + \beta_4) + P_i\beta_2 + (Z_i'P_i)(\beta_3\hat{\gamma} + \beta_5) + \varepsilon_i. \tag{1.5}$$

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 (1.3) and (1.4) by least squares provides consistent coefficient estimates. The appendix 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 is 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. The appendix 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.[31]

[31]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 (1.2).
4. Save the coefficients.

The standard error is then the standard deviation of these 1000 coefficient estimates.

Results

In the first stage regression I estimate the linear probability model of World War I veteran status on a set of age, race, and state fixed effects.[32] Specifically, I estimate:[33]

$$V_j = \sum_{h=1}^{3} \beta_h \mathbf{1}(g_j = g_h) + \sum_{k=1}^{17} \gamma_k \mathbf{1}(s_j = s_k) + \sum_{l=1}^{17} \alpha_l \mathbf{1}(g_j = 2)\mathbf{1}(s_j = s_l)$$
$$+ \sum_{m=1}^{3} \theta_m a_j^m + \sum_{n=1}^{3} \lambda_n \mathbf{1}(g_j = 2) a_j^n + \zeta r_j + \eta \mathbf{1}(g_j = 2) \cdot r_j + \mu_j. \quad (1.6)$$

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; 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.[34] The particular specification in (1.6) 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 64,144 observations, enough to precisely estimate the probability of being a veteran as a function of age, race, and location. The first stage produces large variation in the probability that the husband in the household was a veteran.[35] Table 1.6 gives some examples of how this probability varies with age, race and location.

In the second step, I estimate equation 1.2, where consumption is the dependent variable. Table 1.7 shows results for total consumption expenditure.[36] The specification in column 1 of table 1.7 uses all observations. Column 2, my preferred specification, excludes households in

[32] 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 the appendix.

[33] 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.

[34] See the proof in the appendix. As in the IV context, the key assumption is that the errors from the first stage are uncorrelated with the regressors - something that OLS guarantees is true regardless of misspecification.

[35] The first stage R^2 is 0.21. Most of the variation comes from age, rather than race or location. See table 1.8.

[36] The appendix describes exactly what is included in this aggregate.

Table 1.6: Variation in probability man is a veteran

Age in 1936	Race	City	Prob. vet
35	White	Bellingham, WA	0.20
35	White	Chicago, IL	0.17
35	White	Denver, CO	0.18
35	White	Atlanta, GA	0.13
35	Black	Atlanta, GA	0.04
38	White	Bellingham, WA	0.46
38	White	Chicago, IL	0.44
38	White	Denver, CO	0.45
38	White	Atlanta, GA	0.39
38	Black	Atlanta, GA	0.30

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 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 this 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.[37]

To interpret the coefficient on the interaction, recall that average household expenditure in the sample was $1870 (table 1.5), 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.

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, there is no direct information available about how any of these factors varied

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

Table 1.7: Total expenditure and saving regressions

	(1) Total C	(2) Total C	(3) Insurance policies settled	(4) Gifts received
Post bonus dummy	264.2***	198.2***	-5.601	0.0989
	(70.50)	(43.16)	(4.294)	(6.852)
Interaction	646.9*	403.3**	96.04***	152.3***
	(379.4)	(169.6)	(22.88)	(46.43)
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.

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 records of this data in their archives.[38]

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

$$\text{Average bonus}_i = \alpha_i + \beta_1 \text{Urban share}_i + \beta_2 \text{Black Share}_i + \beta_3 \Delta \text{Employment 29-36}_i + \varepsilon_i \quad (1.7)$$

The change in employment from 1929-36 is a proxy for a states' unemployment rate, since no state level unemployment rate estimates exist.[39] 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

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

[39]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 1929-36 employment changes are from Wallis (1989).

received but not spent.[40] Column 3 of table 1.7 replicates the specification in column 2, but with this variable rather than total spending as the dependent variable. The coefficient implies that the average veteran in the survey saved $96 of his bonus. This coefficient must, however, be interpreted with caution, since there was probably a problem of 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."[41] 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 preponderance of evidence thus suggests that the MPC was between 0.6 and 0.75. As the forgoing discussion indicates, it is difficult to say more precisely what the MPC was. In addition to uncertainty about the exact bonus amount received by veterans in the household survey, there is also 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. 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. In any case, as we shall see, a MPC in this range fits well with evidence from alternative specifications, and from other, independent sources of data.

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[40] 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.

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.

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

Robustness

Table 1.8 shows several robustness checks for the baseline total expenditure specification. For comparison, column 1 reproduces column 2 of table 1.7, 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 from the bonus. Column 2 of table 1.8 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.[42] If one ignores the econometric issues and still includes state fixed effects, the coefficient in column 2 is 446, much closer to that in column 1.

One might also be concerned that different time patterns of consumption between whites and blacks could bias my coefficient. Hence in column 3, I use only age to predict veteran status. The point estimate again rises, and again this increase is driven primarily not be different estimates of the probability of being a veteran but by the change in the second stage control variables.[43] As a further check for the effect of race on my results, in column 4 I perform both the first and second stage estimation for whites only. The coefficient is essentially unchanged from that in column 1. Column 5 restricts the first stage sample to men who are employed in 1930. Since the 1936 household survey included only employed men, it would be a concern if the relationship between the covariates and veteran status were different for the employed and the unemployed.[44] In practice, this does not appear to be concern: the coefficient on the interaction in column 5 is essentially identical to that in column 1.

Finally, one might worry that the ability of households to choose to report on calendar year 1935 rather than the most recent 12 month period is a source of bias. Suppose, for instance, that veterans who spent much of their bonus were *less* likely to report on 1935. This could be because households were eager to report recent salient purchases. In this case, the coefficient would be biased up: I would observe more spending among veterans post bonus than actually occurred. To see if this effect is driving my results, in column 6 I drop

[42] 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 the appendix.

[43] If one were to ignore the econometric problems and includes the baseline controls, the coefficient in column 3 would be 437.

[44] Of course, veterans who were unemployed in 1930 were different people from those unemployed in 1936. This is why in my baseline specification I do not drop those who were unemployed in 1930 from the first stage.

Table 1.8: Total expenditure robustness checks

	(1) Baseline	(2) Age, race only	(3) Age only	(4) White only	(5) Emp. only	(6) Post, rpt. '35
Post bonus	198.2***	117.5***	80.81*	205.2***	199.3***	199.9***
	(43.16)	(44.50)	(44.18)	(45.28)	(41.08)	(50.01)
Interaction	403.3**	520.9***	662.8***	380.1**	395.5**	460.4**
	(169.6)	(159.4)	(181.0)	(178.5)	(168.6)	(204.7)
1^{st} stage N	64,144	64,144	64,144	57,699	57,390	64,144
2^{nd} stage N	2681	2681	2681	2501	2681	2073
1^{st} stage R^2	0.213	0.212	0.209	0.220	0.214	0.213
2^{nd} stage R^2	0.186	0.136	0.054	0.110	0.186	0.205

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.

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.

Overall these robustness tests are reassuring; they suggest that the 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.

Expenditure categories

The detail of the household survey allows me to break down the spending response across consumption categories. Table 1.9 reproduces for several categories of consumption the baseline specification that excludes households with spending greater than \$5000.[45] 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 consumption 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 economically nontrivial amounts of spending but are less precisely estimated.

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

Table 1.9: Consumption category regressions

	(1) Auto purchases and ops.	(2) Housing	(3) Furniture / equipment	(4) Clothing	(5) Recreation	(6) Food
Post bonus dummy	47.77***	9.516	4.952	41.47***	9.323***	29.93**
	(10.77)	(8.425)	(5.244)	(7.733)	(3.131)	(12.76)
Interaction	127.5**	83.28***	21.72	32.99	24.89	35.16
	(60.29)	(29.40)	(20.44)	(27.08)	(15.76)	(45.66)
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 1.10: Autos regressions

	(1) Auto purchases and ops.	(2) Auto purchases	(3) Car purchase	(4) Auto operations
Post bonus dummy	47.77***	28.26***	0.00387	19.51***
	(10.77)	(7.685)	(0.0146)	(6.902)
Interaction	127.5**	71.04*	0.215***	56.45*
	(60.29)	(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, table 1.10 looks in more detail at the response of autos consumption to the bonus payments. For comparison, column 1 reproduces column 1 of table 1.9. 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.[46] 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 1.10 shows the response of all auto-related spending except car purchases. This includes gasoline, repairs, and purchases such as tires and car radios. The response is large, suggesting that even veterans who did not purchase a car responded to the bonus by investing in existing cars and / or driving more.

1.4 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.

The Geographic Distribution of Veterans

Veterans were unevenly distributed across the U.S. Figure 1.1 shows data from the 1930 Census on the percent of each state's population made up of World War I veterans.[47] There is large geographic variation, from a low of 1.8 veterans per 100 people in Mississippi to a high of 5.2 veterans per 100 people in Wyoming, the District of Columbia, and Nevada. States in the south tended to have fewer veterans while states west of the Mississippi tended to have more.

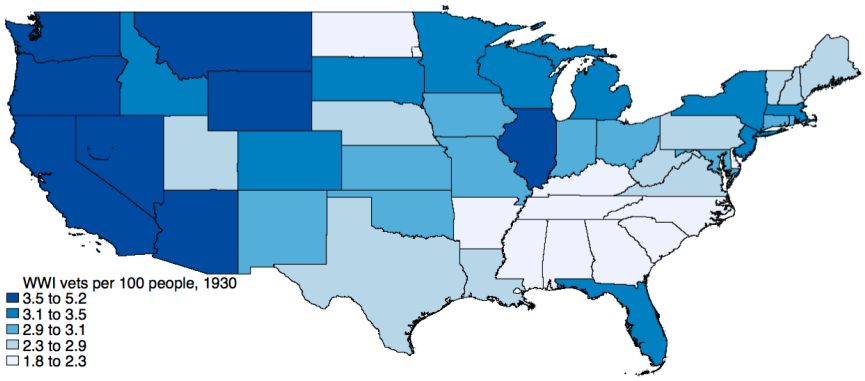
Two forces combined to lower the share of veterans in the south. First, blacks were less likely than whites to have served in World War I. In 1930, 2.1 percent of blacks were veterans, whereas 3.1 percent of whites were veterans.[48] Since black populations were much larger in the south, this mechanically 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.

[46]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.

[47]Data are from the IPUMS 5 percent sample of the 1930 Census (Ruggles et al. 2010).

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

Figure 1.1: 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).

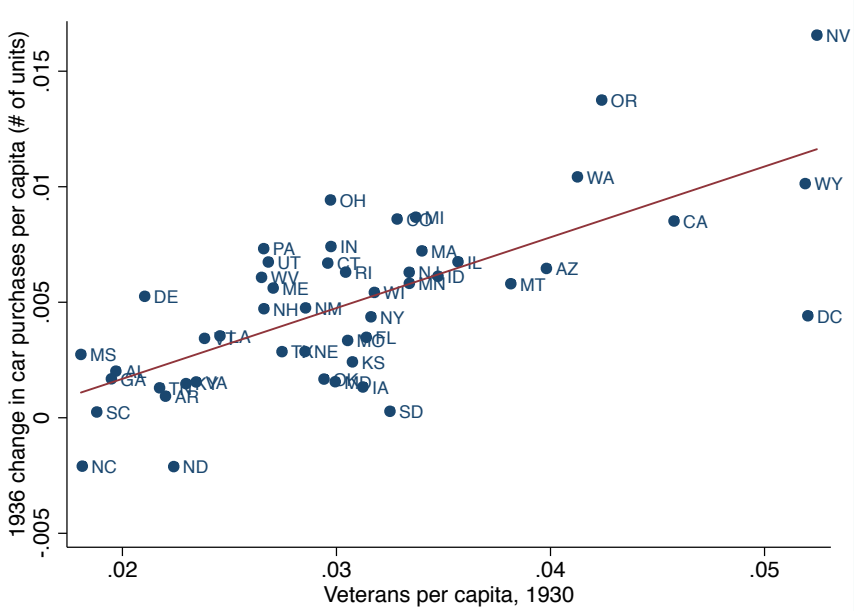
Even among whites, participation in World War I was rarer in the south. Data from the 1930 census show that in the states in the south census region, only 2.6 percent of the white population was a World War I veteran. Outside this region, the proportion was 3.3 percent. The difference could reflect different 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 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 1.2, 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).[49] 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

[49]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 1.2: 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).

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.

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.

Figure 1.2 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 1.11. 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.

The estimates in table 1.11 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 only bought 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.

Table 1.11: Regression results for new car sales

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Veterans per capita, 1930	0.306***	0.316***	0.344***	0.209***	0.214**	0.276***	0.334*
	(0.0646)	(0.0872)	(0.0805)	(0.0690)	(0.0828)	(0.0748)	(0.190)
Per capita new car sales in 1929		-0.0116					
		(0.0599)					
Change in per capita new car sales in 1935			-0.122				
			(0.150)				
Midwest				-0.00180*			
				(0.00103)			
South				-0.00263***			
				(0.000728)			
West				0.000975			
				(0.000969)			
Black share of the population					-0.00300		
					(0.00462)		
Farm share of the population					-0.00653**		
					(0.00265)		
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	0.493	0.494	0.501	0.604	0.585	0.224	0.157

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 effect of the bonus on new car sales in table 1.11 is larger than that measured in the household survey. Whereas the household survey results (section 1.3, table 1.10) 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.

Placebo tests

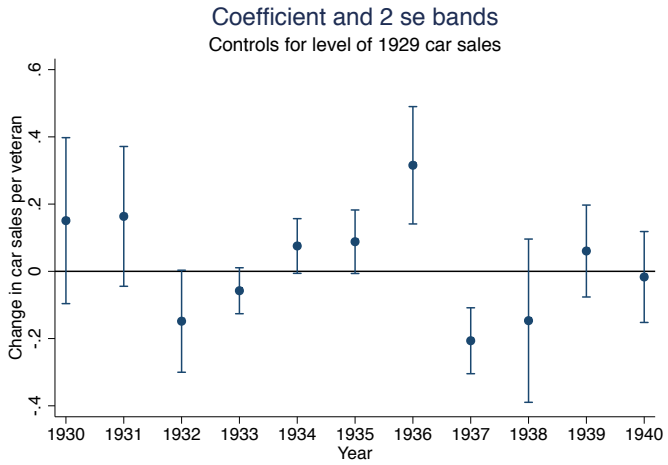
A useful robustness test is to examine whether the share of veterans affected auto sales in other years when no bonus was paid. If veterans per capita had a statistically significant relationship with auto sales in, say, 1940, this would cast doubt on whether the effect I measure in 1936 is a causal impact of the bonus rather than a spurious correlation. But if veterans per capita 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 1.3 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 1.3 have coefficients on veterans per capita that are large and 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

Figure 1.3: 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).

table 1.12, the coefficients in figure 1.3 imply that the veterans' bonus increased the *level* of car sales in 1937 as well as in 1936.

Table 1.12 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.[50] 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).[51]

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

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

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

Table 1.12: 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	-585,816	788,489	-916,699
Level of car sales (# of units)			
Actual	3,404,497	3,483,752	1,891,021
No Bonus	2,158,092	2,946,581	2,029,881
Cumulative sales, 1936-38 (# of units)			
Actual	3,404,497	6,888,249	8,779,270
No bonus	2,158,092	5,104,672	7,134,553

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.

suggests that veterans may also have spent their bonus on housing investment, in particular new houses. One way to test this is to examine the relationship between changes in building construction in a city and the proportion of its population made up of veterans.

In the 1930s, the Bureau of Labor Statistics collected data on the value of residential building permits issued for over 300 cities.[52] 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 1.13 shows regression results. The first column has 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 266 implies that for every additional veteran in a city, the value of residential building permits rose by $266. 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 of a simple regression will be captured by these state

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

Table 1.13: Regression results for residential building permits

	(1)	(2)	(3)	(4)	(5)
Veterans per capita, 1930	265.9***	209.6***	127.9***	119.7***	204.4***
	(82.25)	(76.98)	(47.20)	(37.15)	(61.22)
Δ per capita bldg. permits 1935				0.823***	
				(0.107)	
Black share of the population				4.884*	
				(2.776)	
State fixed effects		X	X		X
Outliers excluded			X	X	X
Cities with pop. $>$ 50,000 only					X
Observations	302	302	282	282	172
R^2	0.086	0.275	0.362	0.422	0.486

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

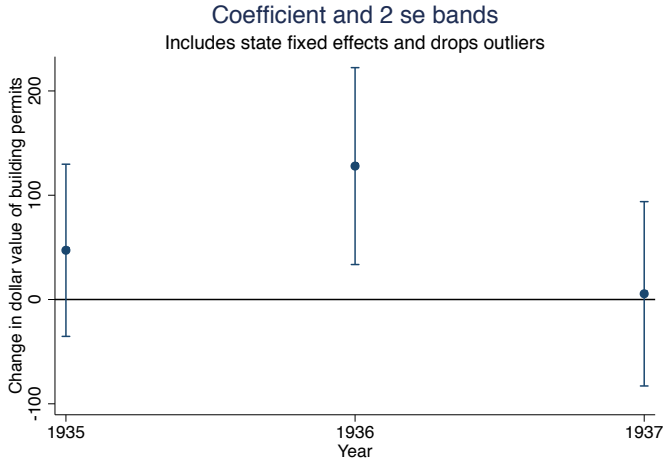
specific trends: for instance, if veterans per capita is correlated with agricultural production of a specific crop or climate shocks.

In fact, the coefficient is little affected by the addition of the 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 shrinks the coefficient, but it remains highly economically and statistically significant. Column 4 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 with state fixed effects. A different way to test the robustness of the results is to limit the sample. In column 5, the sample is restricted to cities with a population greater than 50,000 (in 1930). This significantly increases the size of the coefficient.

A final check on these results is placebo tests. Figure 1.4 shows the coefficient and two standard error bands for the three years for which I have data - 1935, 1936, and 1937. The only year with a large and statistically significant effect is 1936. Interestingly, unlike for auto sales, the coefficient in 1937 is not negative. This suggests that positive effects of the bonus on residential construction may have persisted beyond 1936. Given the lags involved in planning construction and obtaining a building permit, this is unsurprising.

Taken together, the results in table 1.13 and figure 1.4 imply that the bonus had large and

Figure 1.4: Residential building permits and veterans per capita 1935-1937.



Note: Standard errors are clustered at the state level.

Source: See table 1.13.

possibly long-lasting effects on residential construction. The precise quantitative magnitude is difficult to determine exactly given the estimate's sensitivity to the exact sample of cities used. 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 120 suggests that more than half this increase was accounted for by the bonus.[53] 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 1.6, I consider the possible link between these depressed conditions and veterans' spending on housing.

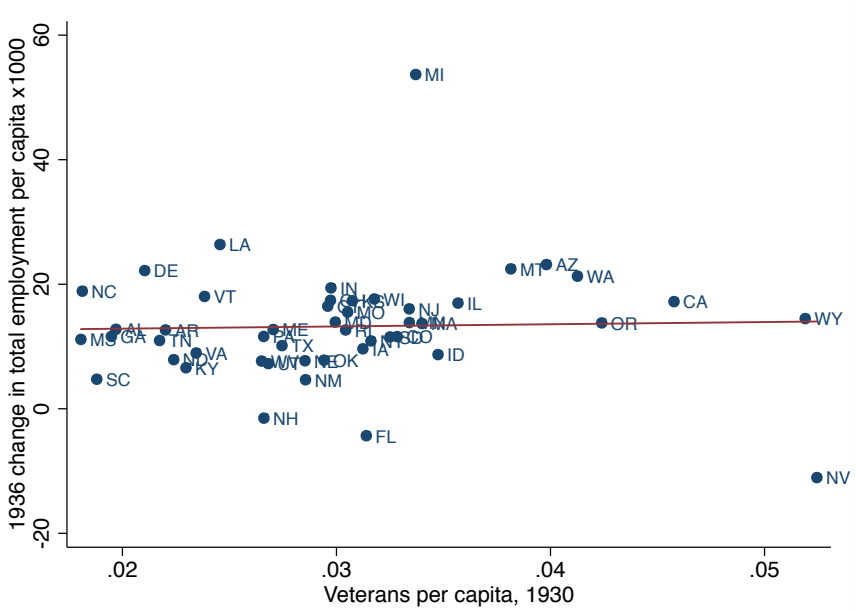
Employment

One of the best aggregate indicators of state-level economic activity in the 1930s is employment.[54] The Bureau of Labor Statistics (BLS) measured employment using an establishment

[53]In these 302 cities, there were 1.7 million veterans. The regression coefficient of 120 means that absent the bonus the value of building permits would have been roughly 1.7 million · $120 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.

[54]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 1.5: 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).

survey like that conducted by the BLS today. The establishment survey in the 1930s covered between 4 and 7.5 million workers or 12 to 20 percent of total non-farm employment (Wallis 1989).[55] 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 1.5 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 1.5: 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 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

[55]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.

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 1.3 suggest that they primarily bought tradable goods (and housing).

1.5 Direct survey and narrative evidence

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 (2011) 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 (2003, 2009) do this by adding a question to the Michigan Survey of Consumers.

A 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 1.6. While the actual survey responses no longer exist, the American Legion library has a copy of unpublished tabulations prepared by an Indianapolis accounting firm on behalf of the American Legion. Table 1.14 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

Figure 1.6: 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	$

Table 1.14: 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%

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 (2003, 2009) find an MPC of roughly 1/3 for both the 2001 and 2008 tax cuts. By contrast, Parker et al. (2006, 2011) find an actual MPC using the CE survey of 2/3 in 2001 and between 0.5 and 0.9 in 2008. It appears that at least part of the difference between these methodologies comes from the measurement of the lagged spending response.[56] 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 (2003). 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, 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.

Thus it is possible that the American Legion survey accurately predicted the immediate response but not continued spending. In any case, the modern evidence suggests that the finding of an MPC in the Legion survey of 0.38 is fully consistent with the MPC of 0.6 to 0.75 found using the 1935-36 consumption survey. Indeed, 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 evidence. Together these two sources provide strong evidence that veterans spent a significant portion of their bonus on housing investment.

Direct comparison of survey results for consumption categories with results from the household survey are complicated by differences in category definitions. Still, it is possible to make some comparisons. Naturally, since the overall MPC is lower, in most categories the American Legion survey also suggests lower spending. For example, the household survey suggests that the average veteran spent $83 of his bonus on housing consumption while the American Legion survey suggests they planned to spend $48. (Recall that the average bonus received by a veteran was $547.) The difference for auto purchases is larger: $71 versus $35, possibly reflecting the fact that demand for cars in the 1930s was generally low in winter, when the American Legion survey was given, but high in summer, when the bonus payments arrived. Buying a car was more attractive when roads were not mired in mud.[57]

[56]See Shapiro and Slemrod (2009), p. 378 for a discussion of this issue.

[57]Busse, Pope, Pope, and Silva-Risso (2012) provide modern evidence for this kind of effect in the car market. They find that SUV sales are unusually high when the weather is cold or snowy. They argue that this reflects 'projection bias' - people assuming that their future utility from a car purchase will be similar to the current utility. In the 1930s, when SUVs did not exist and unpaved roads could be literally impassable in winter, it is likely that veterans' surveyed in January were unable to fully imagine how much they would

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.[58] And July 1936 was extraordinarily hot in much of the midwest. Temperatures reached 110 degrees in Iowa and Wisconsin.[59] 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.

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want to buy a car come summer.

[58] See `http://www.ncdc.noaa.gov/paleo/pdsiyear.html`.

[59] See `http://www.crh.noaa.gov/arx/events/heatwave36.php`.

Figure 1.7: From the *Chicago Tribune*, 6/14/36, p. 1

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 1.7). 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.

1.6 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 very large, as the bonus was, the MPC should be relatively small. 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 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.

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 constraints 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).[60]

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.[61] 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:"

[60]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."

[61]NIPA table 2.1.

> [T]here is every indication that the usual summer lag is to be less than normal. Automobile assemblies have held up remarkably well; the steel industry is operating at a high rate of capacity; while retail trade will be stimulated at an unusual time by the payment of the solders' bonus. 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.[62] 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.[63] Thus market participants, at least, were optimistic. And the bonus disbursement may have increased their optimism: between June and July 1936, the market rose 6 percent.

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 the bank are similar, for the aggregate economy, the implications are entirely different: in the former case, a transfer payment succeeds at increasing spending, in the latter it does not.

What will determine whether households choose to save a transfer or purchase a car? Berger and Vavra (2012) consider a business cycle model in which consumers face fixed costs of adjustment when buying durables.[64] 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,

[62]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).

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

[64]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).

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.[65] 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.[66] Thus just as many veterans were likely anxious to purchase a new car, many were also likely eager to buy a new house or move to a nicer apartment.

1.7 Aggregate effects of the bonus

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. This chapter 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.[67] And a surprising number of estimates from both theory and empirics

[65] NIPA table 1.1.3.

[66] BEA fixed asset table 5.2 and `http://www.census.gov/popest/data/national/totals/pre-1980/tables/popclockest.txt`

[67] 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

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 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:

$$\underbrace{(\frac{Y_{36}}{Y_{35}} - 1)}_{\text{Actual '36 growth}} \quad \underbrace{-(\frac{Y_{36} - B \cdot \frac{P_{37}}{P_{36}} \cdot 0.7 \cdot S_m}{Y_{35}} - 1)}_{\text{Counterfactual '36 growth}} = \underbrace{\frac{B \cdot \frac{P_{37}}{P_{36}} \cdot 0.7 \cdot S_m}{Y_{35}}}_{\text{Bonus's effect on growth}}, \tag{1.8}$$

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;[68] real output is measured in chained 1937 dollars.[69] Table 1.15 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.[70]

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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).

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.

[68] NIPA table 1.1.4.

[69] NIPA table 1.1.6a.

[70] 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 1.15: 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.

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 1.16. 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.[71]

Table 1.16: 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 anything contractionary in 1936. Broad money supply growth slowed from 14 percent in 1935

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

to 11 percent in 1936.[72] And in August 1936, the Federal Reserve raised reserve requirements. While the reserve requirement increase probably had little direct effect on the economy,[73] 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.[74] 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.

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 (2011), and the next chapter, 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.[75] But despite the lack of an overwhelming direct effect, there are also at least two channels through which the bonus conceivably contributed indirectly to the 1937-38 recession.

First, Irwin (2011) 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 2011). 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.

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

[73]See Calomiris, Mason, and Wheelock 2011 and Irwin 2011.

[74]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.

[75]NIPA table 1.1.1.

Second, in the next chapter I argue 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. Chapter two 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.

1.8 Conclusion

This chapter studies the effects of a 1936 payment to World War I veterans totaling 2 percent of GDP. It uses cross-sectional data to identify the effects of the so-called veterans' bonus on consumption. I find that within six months of receipt, veterans 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. While no source is alone definitive, together the evidence of a high MPC is convincing.

A Appendix

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 Whitehead 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. Daughter	F	17	52	0
4. Daughter	F	13	52	—
5. Daughter	F	4	52	—
6. Son	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)

6 1 3 F 16

1. Type of living quarters
2. Total number of *rooms* (excluding bathrooms) 6
3. Total number of *persons* occupying these rooms (including family, roomers, paid help, and others) 3

1.00

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

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

HOUSING FACILITIES

5. Water supply: 1
   a. ☑ In living quarters.
   b. ☐ Indoors, other.
   c. ☐ Outdoors.
6. Running water:
   a. ☑ Hot or cold. 1
   b. ☐ Cold only.
   c. ☐ None.
7. Location of toilets: 1
   a. ☑ In living quarters.
   b. ☐ Indoors, other.
   c. ☐ Outdoors.
8. Number of toilets:
   a. Flush 1 1
   b. Other
9. Heating (check principal method):
   a. ☐ Central, steam or water. 3
   b. ☐ Central, air.
   c. ☑ Stoves (not kitchen). 1
   d. ☐ Kitchen stove only.
   e. ☐ Fireplace.
   f. ☐ None.
10. Lighting:
    a. ☑ Electricity. 1
    b. ☐ Gas.
    c. ☐ Kerosene.
    d. ☐ Other.
11. Cooking fuel: 4
    a. ☐ Gas.
    b. ☐ Electricity.
    c. ☐ Wood or coal.
    d. ☑ Kerosene or gasoline.
    e. ☐ Other. 3

V. HOUSING EXPENSE (during schedule year) 1

6 1 3

A	B Present home	C Other home
RENTED HOME (excluding vacation 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		

(1)

16—8390

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 Mar Apr May		Months June July 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	132									136.72
6. Electricity	X	X	xxx	42	xxx	10	xxx	9	xxx	10	xxx		41.99
7. Gas			xxx		xxx		xxx		xxx		xxx		
8. Ice	NR	NR					R	8					19 7.50
9. 10 TOTAL (1–8)			xxx	NR	xxx	NR	xxx	NR	xxx	NR	xxx		186

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	15 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)	198 197.65

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
13. Eye glasses		
14. Medical appliances and supplies		
15. Health and accident insurance		18
16. Other		
17. TOTAL (1–16)		21.00

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				

14—8380

IX. RECREATION

A	B Adults Number	C Adults Price	D Children Number	E Children Price	F Expense for year
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. Total (1–19)					

X. TOBACCO

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

XI. READING

	Expense for year
1. Newspapers: Daily @	
2. Weekly @	
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)	

XII. EDUCATION

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

A	B Expense for year	C Previous education
6. Total, tuition (1–5D)	$	Highest grade completed by:
7. Total, books and supplies (1–5E)		13. Husband
8. Special lessons		14. Wife
9. Other (excluding board and rent)		15. Son or daughter over 16 years with most schooling:
10. Total (6–9)		a. Sex
11. Board at school or college		b. Age
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

XV. GIFTS, COMMUNITY WELFARE, AND TAXES

	Expense for year
1. Gifts (Christmas, birthday, other) to persons not members of economic family (not charity)	
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)	
7. Other	
8. Total (1–7)	

XVI. USUAL FOOD EXPENSE DURING EACH SEASON OF SCHEDULE YEAR

A	Latest season of year		Earlier seasons							
FOOD AT HOME	Months Dec Jan Feb		Months Mch Apr May		Months June July Aug		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, AT HOME, OR RECEIVED AS GIFT OR PAY DURING SCHEDULE YEAR
21. Food at home (item 10)	563.20	[illegible]
22. Food away from home (item 20)		[illegible]
23. TOTAL	563.20	[illegible]

14—6290

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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	4.50
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 Charter	1100
4. Other (specify vehicle)	
PURCHASE AND UPKEEP DURING YEAR	
5. Of motorcycle	
6. Of boat, airplane, other vehicle	
7. TOTAL (1–6)	1100
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: 52 cakes at .10	5.20
6. Tooth paste and powder, mouth wash, etc.	
7. Shaving soap and cream	1.80
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	1.79
11. TOTAL (1–10) (10)	9.99

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, $ ___

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	
3. Bank service charges, safe deposit box		7. Other	
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

A	B	C
Money, stocks, real estate, other assets	Changes in assets during schedule year: Net amount of increase	Changes in assets during schedule year: Net amount of decrease
1. Money in savings accounts	$	$
2. In checking accounts		
3. On hand		
4. Investments in business		
5. Real estate: Purchased		x x x x x x x x
6. Sold	x x x x x x x x	
7. Stocks and bonds: Purchased		x x x x x x x x
8. Sold	x x x x x x x x	
9. Other property: Purchased		x x x x x x x x
10. Sold	x x x x x x x x	
11. Improvements on owned home		x x x x x x x x
12. Improvements on other real estate		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
14. Frequency of payment Weekly	1	
15. Insurance policies surrendered	x x x x x x x x	
16. Insurance policies settled	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
18. Repayments to family on loans made before schedule year	x x x x x x x x	
19. All other (specify)		
20. TOTAL (1-19)	39 00	—

CHANGES IN DEBTS OWED BY FAMILY

D	E	F
Liabilities	Changes in liabilities during schedule year: Net amount of increase	Changes in liabilities during schedule year: Net amount of decrease
21. Mortgages on owned home	$	$
22. Mortgages on other real estate		
23. Notes due to banks, insurance companies, small loan companies	150 00	
24. Notes due to individuals		
25. Back rents (due before schedule year)	x x x x x x x x	
26. Rents due in schedule year, unpaid	48 00	x x x x x x x x
27. Back taxes (due before schedule year)	x x x x x x x x	
28. Taxes due in schedule year, unpaid Paid	3 00	x x x x x x x x
29. Charge accounts due		
30. Other bills due Grocery	75 00	
31. Payments on installment purchases made prior to schedule year (specify goods purchased):		
(a)	x x x x x x x x	
(b)	x x x x x x x x	
(c)	x x x x x x x x	
32. Balance due on installment purchases made in schedule year (specify goods purchased):		
(a)		x x x x x x x x
(b)		x x x x x x x x
(c)		x x x x x x x x
33. All other (specify)		
34. TOTAL (21-33)	276 00	—

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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.

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 IPCSR dataset, housing equals V359+ V360+ V361+ V362+ V381+ V382+ V383+ V384+ V389+ V390+ V385+ V386+ V387.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.)

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.

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.

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).

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

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

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

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

Note the subscripts j rather than i in the second regression, since it is estimated on a separate sample from (A.1). I will show that given a set of reasonable assumptions, estimation of (A.1) and (A.2) 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 A.1.

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

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 A.1 substituting in A.2:

$$\begin{aligned} Y_i &= \beta_1 X_{1i} + Z_i'[(\hat{\gamma} + \gamma - \hat{\gamma})\beta_2 + \beta_4] + \beta_3 X_{1i} Z_i'(\hat{\gamma} + \gamma - \hat{\gamma}) + \beta_2 \upsilon_i + \beta_3 X_{1i} \upsilon_i + \varepsilon_i \\ &= \beta_1 X_{1i} + Z_i'(\hat{\gamma}\beta_2 + \beta_4) + \beta_3 X_{1i} Z_i' \hat{\gamma} + \tilde{\varepsilon}, \end{aligned} \tag{A.3}$$

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 A.3, 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

$$\begin{aligned} &= (\hat{X}'\hat{X})^{-1}\hat{X}'Y \\ &= (\hat{X}'\hat{X})^{-1}\hat{X}'(\hat{X}\beta + \tilde{\varepsilon}) \\ &= \beta + (\hat{X}'\hat{X})^{-1}\hat{X}'\tilde{\varepsilon}. \end{aligned} \tag{A.4}$$

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, $X_{1i}^2 = X_{1i}$

$$cov(X_{1i}, \tilde{\varepsilon}_i) = (\beta_2 + \beta_3)cov(X_{1i}, \upsilon_i) + cov(X_{1i}, \varepsilon_i) \tag{A.5}$$

   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 $X_{1i}^2 = X_{1i}$,

$$\begin{aligned} cov(X_{1i}Z_i'\hat{\gamma}, \tilde{\varepsilon}_i) &= cov(X_{1i}Z_i'\hat{\gamma}, \beta_2\upsilon_i) + cov(X_{1i}Z_i'\hat{\gamma}, \beta_3 X_{1i}\upsilon_i) + cov(X_{1i}Z_i'\hat{\gamma}, \varepsilon_i) \\ &= (\beta_2 + \beta_3)E[X_{1i}Z_i'\hat{\gamma}\upsilon_i] + E[X_{1i}Z_i'\hat{\gamma}\varepsilon_i] \\ &= (\beta_2 + \beta_3)E[X_{1i}]E[Z_i'\hat{\gamma}\upsilon_i] + E[X_{1i}]E[Z_i'\hat{\gamma}\varepsilon_i] \end{aligned} \tag{A.6}$$

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

4. Finally,

$$cov(Z_i, \tilde{\varepsilon}) = \beta_2 cov(Z_i, \upsilon_i) + \beta_3 cov(Z_i, X_{1i}\upsilon_i) + cov(Z_i, \varepsilon_i) \tag{A.7}$$

   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 1.3.

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 A.1 shows this classification.

Table A.1: 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

Chapter 2

What was Bad for GM was Bad for America: The Automobile Industry and the 1937-38 Recession

2.1 Introduction

The 1937-38 recession is prominent in economists' accounts of the interwar period and in current macro policy debates. After growing rapidly between 1933 and 1937, the U.S. economy plunged back into recession. From 1937 to 1938, real GDP fell 3.5 percent and the nonfarm unemployment rate rose 6.6 percentage points.[1] Because the recession was brief, annual numbers disguise the downturn's severity. Industrial production fell 32 percent between May 1937 and May 1938. This contrasts with a peak to trough industrial production decline in the recent U.S. recession of 17 percent.[2]

Economists typically take the 1937-38 recession as a lesson in the perils of reversing expansionary monetary and fiscal policy too soon. In June 2009, Christina Romer wrote a column for *The Economist* on "The Lessons of 1937." In January 2010, Paul Krugman wrote a *New York Times* column entitled "That 1937 Feeling." Both Romer and Krugman cautioned that a decline in government spending or shrinking of the Federal Reserve's balance sheet could push the economy back into recession. Their arguments reflect a near-consensus in the academic literature that the 1937-38 recession was caused by contractionary monetary policy, fiscal policy, or both (Eggertsson and Pugsley 2006, Eichengreen 1992, Friedman and Schwartz 1963, Romer 1992, Velde 2009, Irwin 2011).[3] Both the monetary and fiscal shock had multiple sources. The Federal Reserve raised reserve requirements in August 1936 and

[1] Real GDP data are from National Income and Product Accounts (NIPA) table 1.1.6a. The nonfarm unemployment rate is from Lebergott (1964) table A-3. Including the entire labor force, the unemployment rate rose from 14.3 to 19.1 percent, or, if federal work-relief workers are counted as employed, from 9.2 to 12.5 percent (Darby 1976, table 3).

[2] Industrial production data from FRED series INDPRO.

[3] A prominent exception is Cole and Ohanian (2001). See section 2.2.

again in March and May 1937. At the same time, the Treasury began to sterilize gold inflows rather than passively allow the money supply to expand as gold flowed to the U.S. from Europe (Friedman and Schwartz 1963, Irwin 2011). Contemporaneously, a decline in outlays and an increase in revenues caused the federal budget deficit as a percent of GDP to fall 5.4 percentage points.[4] Outlays decreased after the payment of the veterans' bonus. Much of the revenue increase came from social security taxes, which were first collected in January 1937.

Given its importance for the interwar U.S. economy and contemporary policy debates, there is a striking absence of research asking whether existing explanations of the recession are consistent with the timing, geographic, and sectoral patterns of output and employment in 1937-38. The first half of this chapter shows that there are in fact timing, geographic, and sectoral anomalies in the recession. Specifically:

- The decline in industrial production from September to December 1937 is the largest three-month decline in the history of the Federal Reserve and Miron-Romer series going back to 1884.
- Manufacturing employment rises in 8 states despite a 12 percent decline nationwide.
- There is a lack of comovement between components of GDP. Durables consumption falls over 17 percent while nondurables consumption rises. The 1937-38 recession is the only time since the Great Depression in which nondurables spending rises in a recession in which annual real GDP falls more than 2 percent.

These facts are anomalous both in the sense of being unusual relative to other severe recessions and in the sense of not being easily explained by monetary or fiscal policy.

What could explain these facts? The analysis points strongly to a role for the auto industry in explaining the anomalous behavior of the economy: manufacturing employment falls most in Michigan, and auto sales and production fall more than 40 percent. Furthermore, auto prices *rise* at the same time as the overall price level falls. In the second half of the paper, I ask what explains the peculiar behavior of the auto industry, and how exogenous developments in this industry affected the rest of the economy. I argue that a cost shock in the auto industry and consequent auto price increases explain the industry's extraordinary behavior. The unionization of General Motors and Chrysler and an increase in raw material costs led auto manufacturers to raise prices in fall 1937. Equally important, the increase in costs combined with nominal rigidity to make the price increase predictable. Expectations of price increases brought auto sales forward and thus sustained sales during the summer and early fall of 1937, despite negative monetary and fiscal factors. When auto prices did increase in fall 1937, sales plummeted. Narrative evidence confirms this explanation. To quantify the impact on auto sales, I perform a forecasting exercise. Using information criteria, I choose the model that best fits the monthly pre-recession behavior of auto sales given fiscal and monetary factors and overall economic conditions. As expected, the model underpredicts

[4]Office of Management and Budget, Historical Table 1.2.

sales in the summer of 1937, when consumers expected auto prices soon to increase, and overpredicts sales in 1938. I interpret the difference between predicted and actual sales as the impact of the auto sector supply shock.

This exercise implies that without the auto shock, the fall in auto sales between 1937 and 1938 would have been 0.8 million rather than the actual 1.6 million. Using data on the price of cars sold and an estimate of the multiplier, I find that positive price expectations added 0.3 percent to GDP in 1937, while the subsequent drop-off in sales subtracted 1.0 percent from GDP in 1938: absent the shock, the output decline in 1938 would have been more than a third smaller.

In the final section of the chapter I explore how the auto shock contributed to the recession's anomalies. I use an unpublished 38x38 input-output table for the 1939 U.S. economy constructed by Wassily Leontief to quantify the links between the motor vehicle industry and other sectors. Using this information, I provide suggestive evidence that the auto industry shock contributed to the rapid decline in industrial production in fall 1937 and to the long left tail of negative state employment growth. I also suggest that the auto shock may resolve much of the mysterious lack of comovement in the recession. Without this shock, durables consumption would have fallen 12 rather than 18 percent. More speculatively, absent the relative price impact of the auto shock, nondurables spending may not have risen, and there may have been fewer states with positive employment growth.

This chapter contributes to our understanding of the 1937-38 recession by proposing an additional explanation of the recession and by refining our understanding of existing explanations. It also relates to a long-standing theoretical literature on the importance of sectoral and firm level shocks for explaining aggregate fluctuations. In particular, it provides support for Gabaix's (2011) hypothesis that shocks to large firms may account for a significant portion of aggregate fluctuations.

I proceed in the next section by reviewing the literature on the 1937-38 recession. Section 2.3 documents anomalies in the timing, sectoral and geographic incidence of the recession. This motivates section 2.4, which discusses and explains the auto price increase. Section 2.5 quantifies the impact on auto sales of the supply shock by constructing a forecast for the path of auto sales had sales only been impacted by monetary policy, fiscal policy, and the business cycle. Section 2.6 explores the link between the auto shock and the recession's anomalies. Section 2.7 concludes and suggests that acknowledging the impact of the auto shock makes monetary and fiscal policy explanations of the remainder of the recession more plausible.

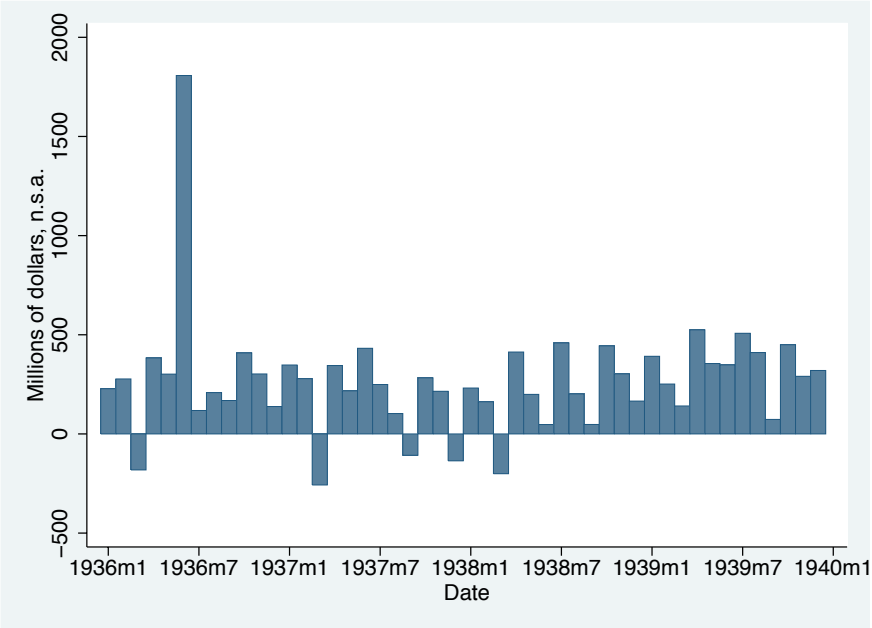


Figure 2.1: The Federal budget deficit. Source: NBER macrohistory series m15025c. Underlying data from the U.S. Treasury Department, daily treasury statements.

2.2 Previous literature and policy developments

Fiscal Policy

Many authors have placed full or partial blame for the 1937 recession on increasing taxes and decreasing transfers.[5] After peaking at 5.5 percent of GDP in fiscal year 1936 (7/1/35-6/30/36), the federal government's budget deficit shrank to 2.5 percent of GDP in fiscal 1937 and 0.1 percent of GDP in fiscal 1938.[6] This large swing was divided equally between an increase in receipts and a decrease in outlays. Much of the revenue increase came from social security taxes, which began to be collected in January 1937.[7] Both workers and firms paid a one percent payroll tax on the first $3,000 of earnings (Barro and Sahasakul 1986). Corporate tax rates also rose with the enactment of the undistributed profits tax in June 1936. Federal government transfers fell after the payment of the veterans' bonus in the summer of 1936.

[5]See, for example, Lewis (1949), Roose (1954), and, more recently, Eichengreen (1992), Romer (1992), and Velde (2009).

[6]Office of Management and Budget, Historical Table 1.2.

[7]Total federal government receipts rose from 3.923 billion in fiscal 1936 to 6.751 billion in fiscal 1938. 53 percent of this revenue increase is accounted for by social insurance taxes, a category that included both social security taxes and railroad retirement receipts. See Office of Management and Budget, Historical Tables 2.1 and 2.4.

Figure 2.1 shows the monthly federal budget deficit between 1936 and 1939.[8] It makes clear the significance of the veterans' bonus. In June 1936, the federal government paid veterans cash and bonds worth roughly 2 percent of GDP. Not only was this payment to veterans enormous, the previous chapter argues that it was mostly spent in 1936.

Monetary Policy

Along with fiscal policy, the most popular explanation for the 1937-38 recession is restrictive monetary policy.[9] The Federal Reserve raised reserve requirements in August 1936 and again in March and May 1937. In December 1936, the Treasury began sterilizing gold inflows. Friedman and Schwartz argue (1963, pp. 544-545):

> The combined impact of the rise in reserve requirements and–no less important– the Treasury gold-sterilization program first sharply reduced the rate of increase in the money stock and then converted it into a decline. . . . The sharp retardation in the rate of growth of the money stock must surely have been a factor curbing expansion, and the absolute decline, a factor intensifying contraction.

Figure 2.2 shows the behavior of the money supply (currency held by the public plus demand and time deposits of commercial banks) from 1936-1939. The money stock plateaus in the spring and summer of 1937 and then declines in the fall. There is an emerging consensus that the key shock was the Treasury gold sterilization rather than the increase in reserve requirements. Calomiris, Mason, and Wheelock (2011) use individual bank data to investigate how tighter reserve requirements affected bank behavior. They find no evidence that the changes in reserve requirements affected bank reserve demand in 1936-37. Irwin (2011) examines the time series of the deposit-reserve ratio and cannot detect an impact of the reserve requirement changes. By contrast, Irwin argues that gold sterilization had quantitatively large impacts on the monetary base and thus on the money supply. He estimates that absent gold sterilization, in 1937 the monetary base would have been as much as 10 percent larger. However, the channel through which money supply declines impacted real activity in 1937-38 is unclear. There was relatively little impact on interest rates. Figure 2 shows the path of the yield on three-month treasury bills. Although yields briefly spike by 10 to 40 basis points in the spring and summer of 1937, they remain close to zero over the period. Other interest rates rose only slightly. For example, in 1937 the prime commercial paper rate rose from 0.75 to 1 percent (Irwin 2011).

Of course when interest rates are near zero, monetary policy can still change expectations of inflation and thus affect the real interest rate and the real economy. This is the channel through which Romer (1992) argues that money supply growth led to economic recovery after

[8]Monthly budget deficit figures are derived from daily treasury statements and reported in NBER macrohistory series m15025c.

[9]See, for example, Roose (1954), Friedman and Schwartz (1963), Eichengreen (1992), Romer (1992), Velde (2009), and Irwin (2011).

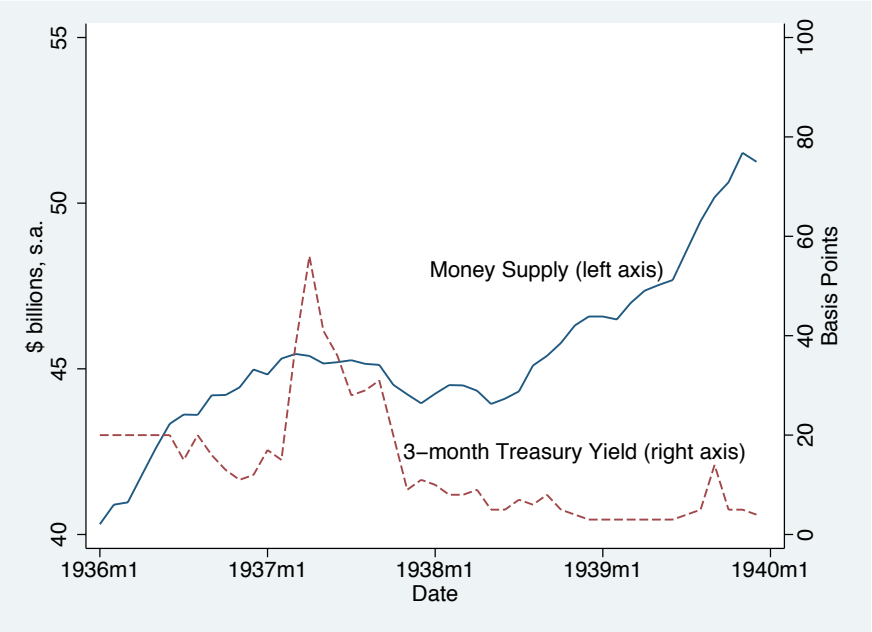


Figure 2.2: The money supply and 3-month treasury yield. The money supply is currency held by the public + demand and time deposits of commercial banks. Sources: NBER macrohistory series 1444a; FRED series TB3MS.

1933. Eggertsson and Pugsley (2006) argue that policymakers' actions in the spring of 1937 lowered inflation expectations, and through this channel, caused the 1937-1938 recession. This provides one way of explaining how monetary actions, despite little impact on interest rates, may have had large real effects.

Unionization

Cole and Ohanian (2001, 2009) argue that New Deal policies and unionization led to high wages in manufacturing and a slow recovery of output and employment after 1933. They briefly suggest that a spike in wages due to the upholding by the Supreme Court of the National Labor Relations Act (the Wagner Act) in April 1937 contributed to the 1937-38 recession. Since this is not the focus of their work, they provide no quantitative evidence - beyond the documentation of wage increases - for this effect. My work supports their argument insofar as it shows how unionization in the automobile industry did substantially contribute to the recession. The transmission mechanism in my paper is, however, different from that in Cole and Ohanian. I argue that unionization had large impacts in the auto industry because it affected expectations of the future price of automobiles, leading to substantial intertemporal substitution of auto purchases. Furthermore, whereas Cole and Ohanian suggest that wage increases induced by unionization were the primary cause of the recession, I argue that understanding the auto industry shock and its contributions to the recessions' anomalies

may in fact strengthen the case for monetary and fiscal policy explanations of the recession.

2.3 Anomalies

Timing

The NBER dates the business cycle peak as May 1937. During the summer, however, production and employment declined only slightly. Indeed, Romer's (1994) business cycle dating algorithm selects August as the peak. The period between May and August was marked by a decoupling of nondurable and durable goods manufacturing. Nondurables peaked in May, with production falling by 6.1 percent over the ensuing three months. Durables production rose 3.7 percent over the same period, peaking in August.[10]

In early fall, the character of the recession changed entirely. Seasonally adjusted industrial production fell 24 percent from September to December 1937. The decline in output was accompanied by large declines in employment. From September 1937 to January 1938, 22 percent of durable goods employment - 917,000 jobs - disappeared.[11] These job losses were reflected in a rapidly rising unemployment rate. The monthly series from the National Industrial Conference Board shows the unemployment rate rising from 11.6 percent in September 1937 to 16.9 percent in January 1938.[12]

Figure 2.3 shows the path of industrial production around the business cycle peak in 1937, and, for comparison, in 1920 and 1929. It makes clear how extraordinarily rapid the contraction was in 1937. Indeed, the decline in industrial production from September to December 1937 is the largest three-month decline in the history of the Federal Reserve Board series (which begins in 1919), and is larger than any three-month decline in the Miron-Romer series that goes back to 1884.

I am unaware of any existing work that emphasizes or offers explanations for the speed of the economy's collapse in fall 1937. Existing theories of the recession are unpromising in this regard. The timing of the decrease in transfers (the second half of 1936) or the increase in taxes (the beginning of 1937) fits poorly with the rapid economic decline in the fall of 1937. Theory and evidence suggest that changes in taxes and transfers should have immediate impacts. Parker et al. (2011), for instance, find that the largest impact of the 2008 stimulus payments was in the quarter of payment.[13] The decline in the money supply in 1937 was small compared to that in the Great Depression and is thus ill-suited to explain a much more rapid decline in industrial production. Finally, the collapse in production occurred 6 months after April 1937, the key date in Cole and Ohanian's supply shock story.

[10]Seasonally adjusted data are from the Federal Reserve Bulletin, August 1940, p. 765.

[11]Factory employment data are from the Federal Reserve Bulletin, October 1939.

[12]NBER macrohistory series m08292a.

[13]Using a structural VAR, Blanchard and Perotti (2002) also find that output declines in the quarter in which taxes rise, however, they find that the peak impact on output occurs 5-7 quarters after a tax shock, a result more consistent with the 1937-38 experience.

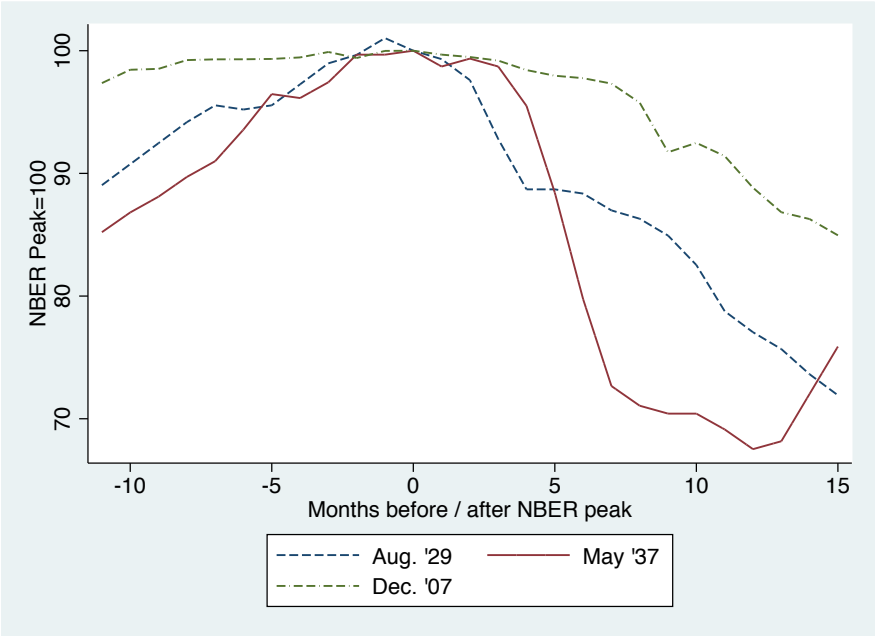


Figure 2.3: Seasonally adjusted industrial production around business cycle peaks in 1920, 1929, and 1937. Source: Industrial production is the seasonally adjusted Fed index, FRED series INDPRO.

Geography

A generally unappreciated aspect of the 1937-38 recession was its varying impact across states.[14] Figures 2.4 and 2.5 show the percent change in private nonfarm and manufacturing employment between 1937 and 1938. Despite a 5 percent decline in the country as a whole, total private employment rose in 11 states. Perhaps more remarkably, while manufacturing employment fell 12 percent nationwide, it rose in 8 states. Of course, recessions always effect some states more than others. States have different concentrations of cyclically sensitive industries and different pre-recession employment trends. But it is unclear how these factors can explain actual increases in employment in some states during a severe recession. And there is strong evidence that the degree of geographic dispersion in 1937-38 was unusual relative to other interwar downturns.

Tables 2.1 and 2.2 compare the variation in the change in private and manufacturing employment between 1937 and 1938 with that between 1929 and 1932. In the case of manufacturing, it is also possible to extend the comparison to 1919-21. The tables show a remarkably consistent story: in each year of the initial downturn from 1929-32, the coefficient of variation is close to or less than 0.5, and employment rises in no more than 2 states. By contrast, in 1937-38, the coefficient of variation is over 1 and employment rises in many

[14] An exception is Wallis (1989, p. 61) who notes the "markedly differential regional impact" of the 1937-38 recession but does not compare the geographic variation to that in other interwar downturns.

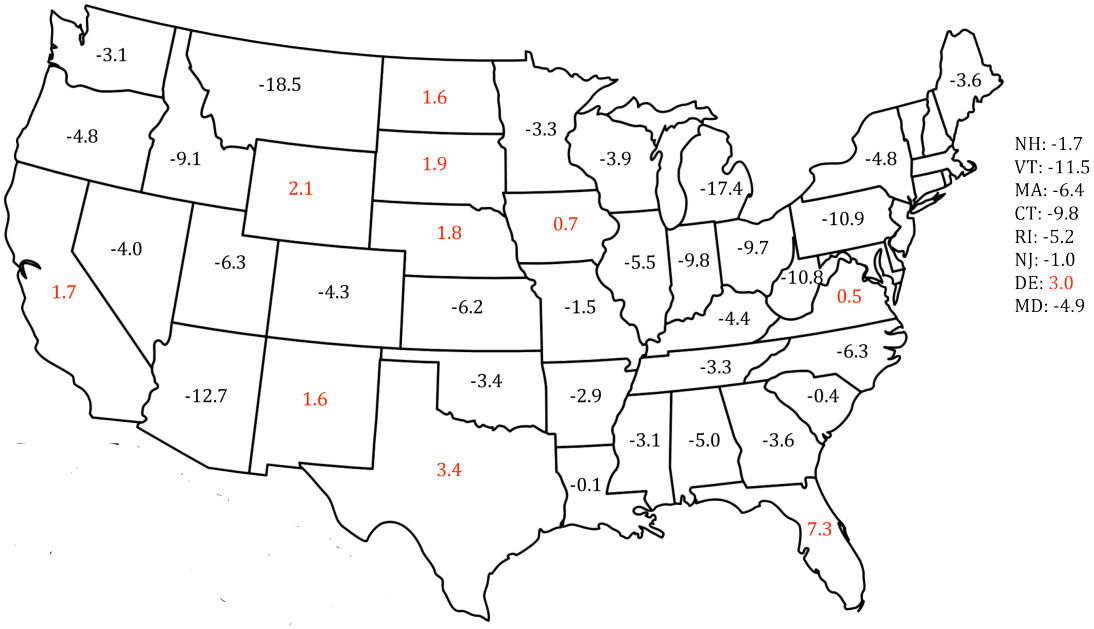


Figure 2.4: Change in private nonfarm payroll employment, 1937-38 (%). Source: Wallis (1989).

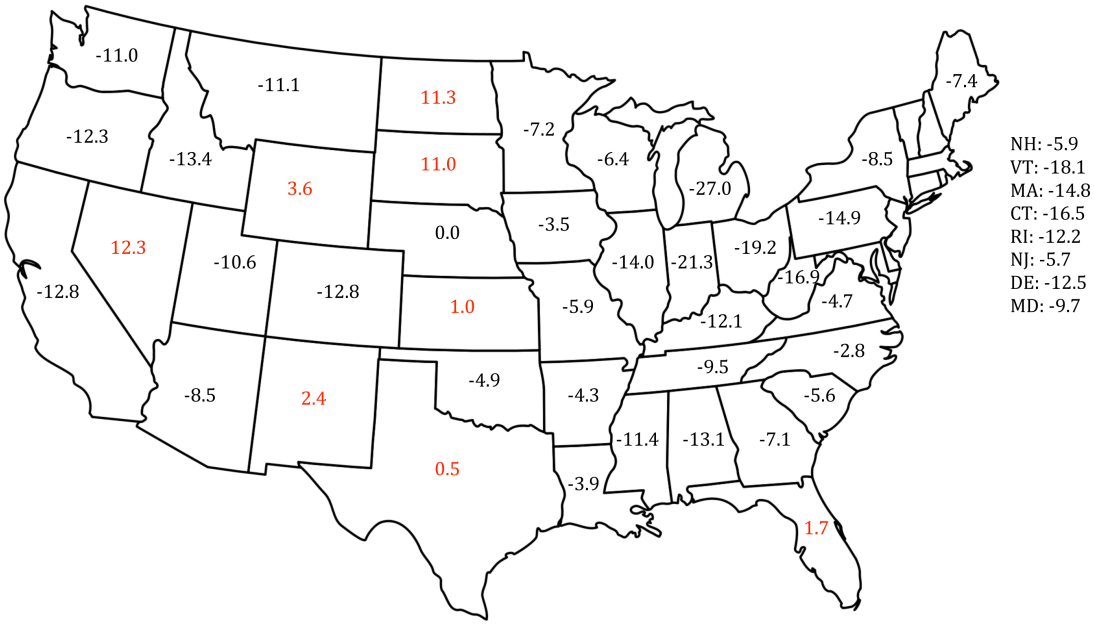


Figure 2.5: Change in manufacturing employment, 1937-38 (%). Source: Wallis (1989).

Downturn	U.S. nonfarm private payroll employment decline	Mean of state employment declines	Standard deviation of state employment changes	Coefficent of variation	Number of states in which employment rose
1929-32	-21.7%	-24.0%	9.1%	-0.38	1
1929-30	-5.1%	-6.9%	3.5%	-0.51	0
1930-31	-12.9%	-8.8%	4.8%	-0.55	1
1931-32	-10.1%	-10.7%	5.4%	-0.50	2
1937-38	-4.9%	-4.1%	5.3%	-1.28	11

Table 2.1: Variation in employment outcomes across states. Source: Wallis (1989).

Downturn	U.S. manufacturing employment decline	Mean of state employment declines	Standard deviation of state employment changes	Coefficent of variation	Number of states in which employment rose
1919-21	-23.2%	-22.6%	9.3%	-0.41	1
1929-32	-35.2%	-35.4%	7.8%	-0.22	0
1929-30	-10.4%	-13.3%	3.8%	-0.29	0
1930-31	-14.2%	-13.3%	4.0%	-0.30	0
1931-32	-15.7%	-14.3%	5.6%	-0.39	2
1937-38	-11.9%	-7.8%	8.1%	-1.03	8

Table 2.2: Variation in manufacturing employment outcomes across states. Note: Data for 1919-21 is for wage earner employment only. Sources: Wallis (1989) for 1929-38, the 1947 *Census of Manufactures* for 1919-21.

states.

To understand to what extent this geographic variation reflects the interaction of state industry structure with industry-specific outcomes, I estimate the following regression:

$$\Delta E_j = \beta_0 + \beta_1 CEI_j + \beta_1 Trend_j + \epsilon_j. \tag{2.1}$$

ΔE_j is the percent change in total manufacturing employment in state j;[15] $Trend_j$ is the average annual growth rate of employment in state j from 1933 to 1937; $CEI_j = \sum_i S_{ij}\Delta E_i$, where S_{ij} is the share of employment in industry i in state j in 1937 and ΔE_i is the percent change in wage earner employment in industry i nationwide from 1937 to 1938.[16] Thus CEI_j shows how employment in state j would have changed had employment changes in each

[15] I focus on manufacturing employment for two reasons. (1) The manufacturing employment data are likely to have significantly less measurement error than the total employment numbers (Wallis 1989), and (2) the 1937 sectoral composition of employment in each state is available only for manufacturing.

[16] CEI_j is similar to the composite employment index calculated by Wallis (1989). Wallis (1989) investigates the impact of industry structure on state outcomes in the 1930s but not specifically in the 1937-1938 recession.

Dependent variable is percent change in state manufacturing employment 1937-38 (average decline is 7.8%)

CEI		1.721***	1.295***		1.576***	1.668***
		(0.192)	(0.279)		(0.211)	(0.318)
Impact of 1 standard deviation increase (percentage points)		*6.5*	*4.9*		*5.1*	*5.4*
33-37 growth	-1.318***		-0.488**	-1.167***		0.132
	(0.191)		(0.239)	(0.312)		(0.335)
Impact of 1 standard deviation increase (percentage points)	*-5.8*		*-2.1*	*-3.6*		*0.4*
Constant	3.194*	17.66***	15.44***	0.658	15.40***	15.69***
	(1.799)	(2.924)	(3.030)	(3.005)	(3.470)	(3.597)
Sample		All States		States with > 50,000 wage earners in manufacturing		
Observations	48	48	48	31	31	31
R-squared	0.509	0.637	0.668	0.325	0.658	0.659

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.3

industry in the state exactly matched the nationwide changes in employment by industry.[17] Put differently, variation in CEI_j across states reflects only differences in industry structure across states. To compute state shares of employment by industry, I collected data on employment in every industry by state from the 1937 *Census of Manufactures*. Data on nationwide changes in employment by industry were taken from the October 1939 Federal Reserve Bulletin.

Table 2.3 shows results both for all states and for the 31 states with more than 50,000 wage earners in manufacturing in 1937.[18] The measure of industry structure, CEI, and to a lesser extent, trend growth in the state from 1933 to 1937 are robustly statistically and economically significant. A one standard deviation decline in CEI is associated with more than 5 percentage points lower manufacturing employment growth. Measured by the R^2, CEI alone explains nearly 2/3 of the variation in state employment changes. The negative coefficient on 1933-37 growth means that on average states that grew faster after 1933 did worse in the 1937-38 recession. However, this result disappears when the measure of industry structure is included in the 31 state sample.

Industry structure's overwhelming importance suggests that the geographic anomalies in 1937-38 are primarily a manifestation of an unusual pattern of output movements across industries.[19] To understand why state outcomes were so different in 1937-38, one must

[17]This statement is approximate, since the left hand side variable of the regression is total manufacturing employment, while data limitations force me to compute CEI_j using data on wage earner employment only.

[18]Data on the number of manufacturing wage earners in each state come from the 1937 *Census of Manufactures*.

[19]Existing explanations of the recession also suggest possible factors that could have directly caused

understand sectoral output patterns in the recession. The next section turns to this topic.

Sectoral

Macroeconomists view comovement as a common element of recessions. There is widespread disagreement about the causes of particular recessions, but there is a widespread agreement that recessions are not simply the result of dramatic declines in output in one sector of the economy. Rather they are events in which nearly all sectors decline, albeit to different degrees. Long and Plosser (1987) summarize this conventional view, stating: "Comovement among a wide variety of economic activities is an essential empirical characteristic of business cycles" (p. 333). This view of business cycles has shaped nearly all macroeconomic models. Models ranging from old Keynesian to real business cycle are constructed so that negative shocks lead to declines in output and employment across all forms of consumption and investment.

To a surprising degree, the 1937-38 recession does not fit this accepted wisdom. The following describes movements in components of consumption and investment in 1937-38 versus in other recession years.

Consumption

Between 1937 and 1938 real durables consumption fell 17.1 percent. New motor vehicles consumption fell 42 percent. This decline in durables, and particularly motor vehicles, consumption was extraordinarily unusual given the behavior of GDP.

The solid line in figure 2.6 shows the median behavior of durables consumption in postwar U.S. recessions. The dashed line shows the behavior in 1937. Of course, given that the decline in GDP is more severe in 1938, we would expect durables to fall more. To adjust for the decline in GDP, Figure 2.7 shows a scatter plot of the change in durables consumption and the change in GDP in all years with negative output growth since 1929, excluding 1945-47.

The decline in durables consumption spending in 1938 is anomalous when compared to postwar recessions or to the Great Depression. In postwar recessions, durables consumption spending has never fallen by more than eight percent in a single year. In 2009, when GDP declined by 3.1 percent, just slightly less than the decline in 1938, durables consumption

unusual geographic dispersion in the recession. Insofar as the fiscal shock was due to the beginning of social security tax collection, states with more workers subject to social security taxes may have fared worse in the recession (Wallis 1989). Insofar as the monetary shock took the form of a reserve requirement increase, states with a larger percent of bank deposits in Federal Reserve member banks may have suffered more. And insofar as general, rather than sector specific, unionization induced wage increases mattered, states with more workers unionized may have performed worse (Wallis 1989).

To test for the importance of each of these factors, I added proxies for the percent of workers covered by social security, the share of bank deposits in Federal Reserve member banks, and the union membership rate to (2.1). None of these variables are robustly economically or statistically significant with the possible exception of social security coverage. Full results available on request. See also Wallis (1989).

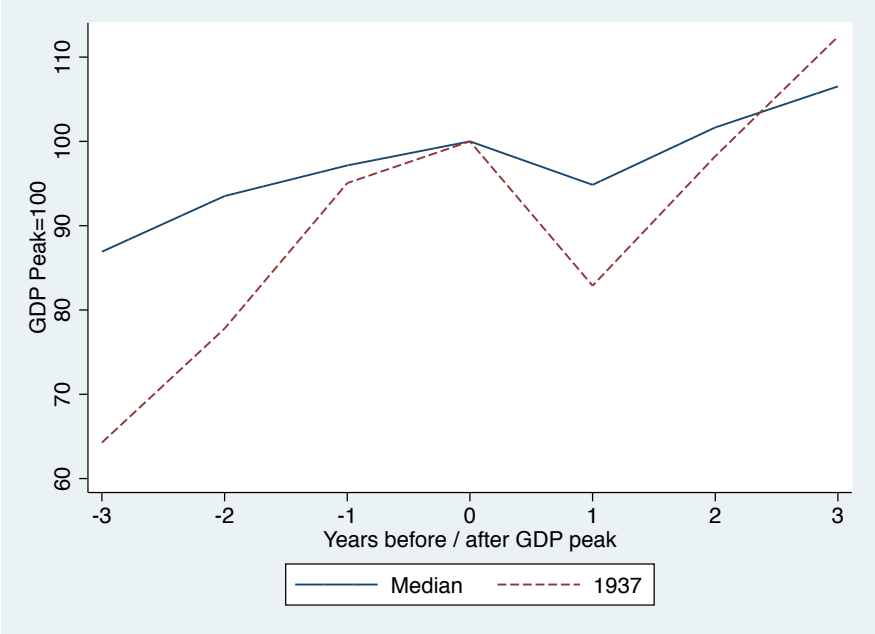


Figure 2.6: Real durables consumption around business cycle peaks, postwar median and 1937. Median includes all post World War II business cycles in which annual GDP fell (1948, 1953, 1957, 1973, 1980, 1981, 1990, and 2007). Source: NIPA table 1.1.3.

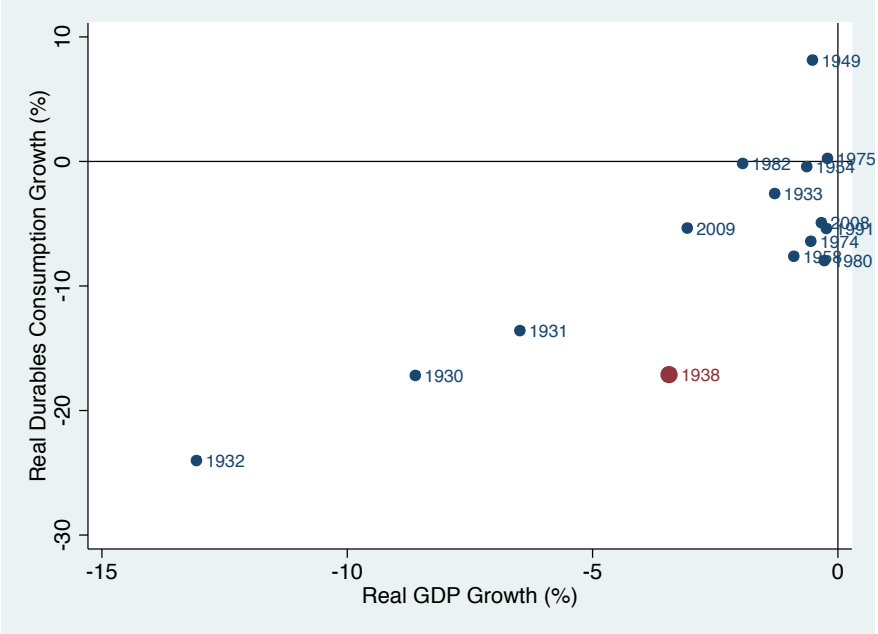


Figure 2.7: Scatter plot of durables consumption growth against GDP growth. Includes all years of negative GDP growth since 1929, excluding 1945-1947. Source: NIPA table 1.1.3.

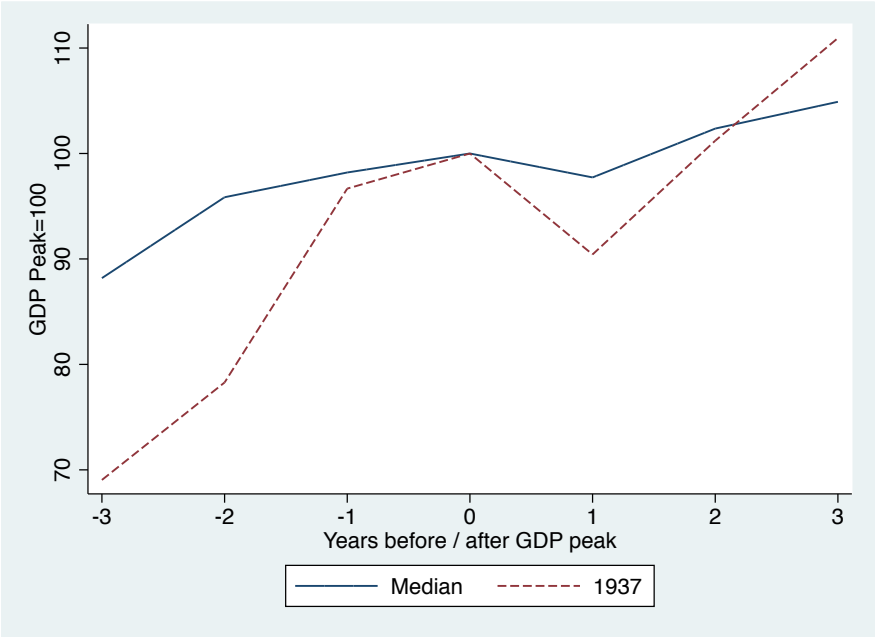


Figure 2.8: Real household durables consumption around business cycle peaks, postwar median and 1937. Median includes all post World War II business cycles in which annual GDP fell (1948, 1953, 1957, 1973, 1980, 1981, 1990, and 2007). Source: NIPA table 2.4.3.

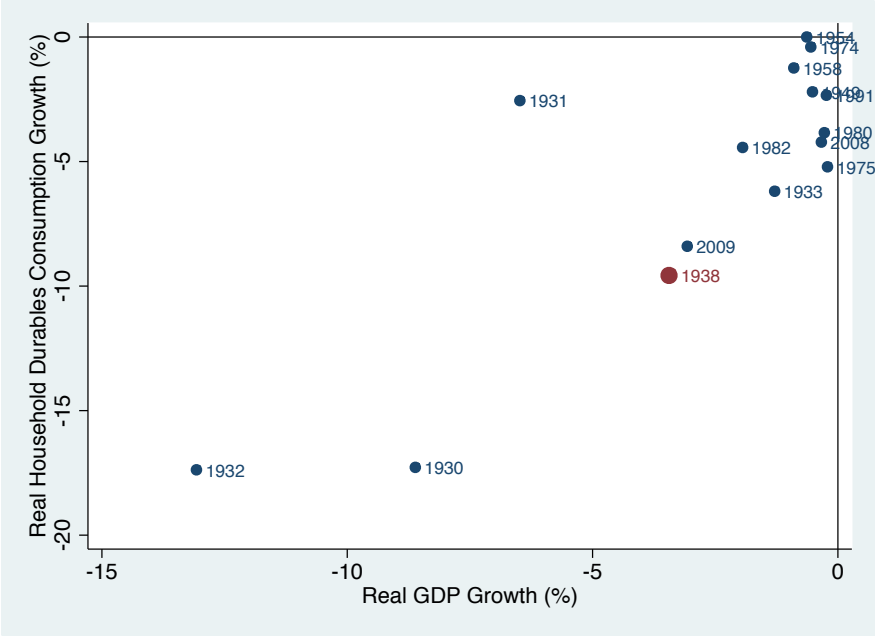


Figure 2.9: Scatter plot of household durables consumption growth against GDP growth. Includes all years of negative GDP growth since 1929, excluding 1945-1947. Source: NIPA table 1.1.3 and 2.4.3.

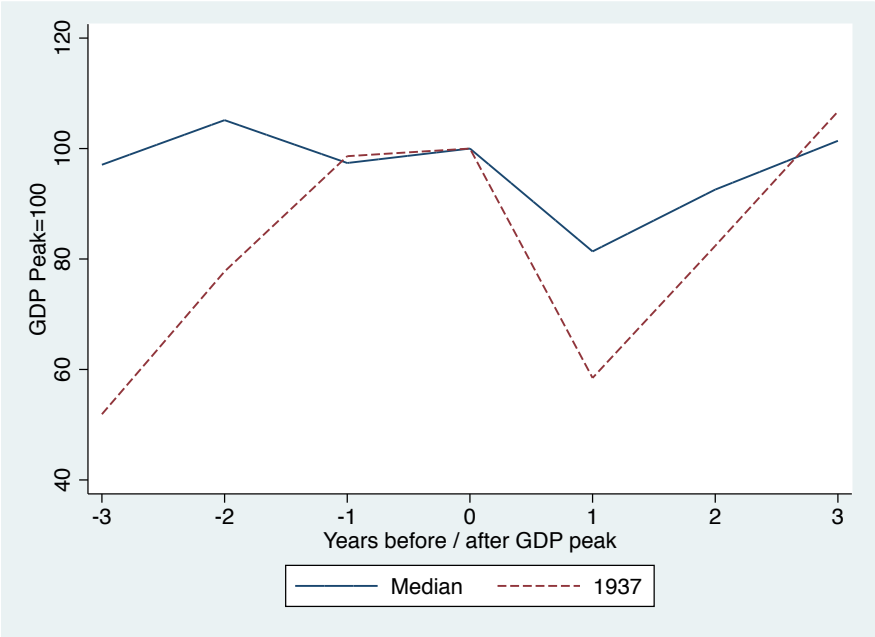


Figure 2.10: Real new motor vehicle consumption around business cycle peaks, postwar median and 1937. Median includes all post World War II business cycles in which annual GDP fell (1948, 1953, 1957, 1973, 1980, 1981, 1990, and 2007). Source: NIPA table 2.4.3.

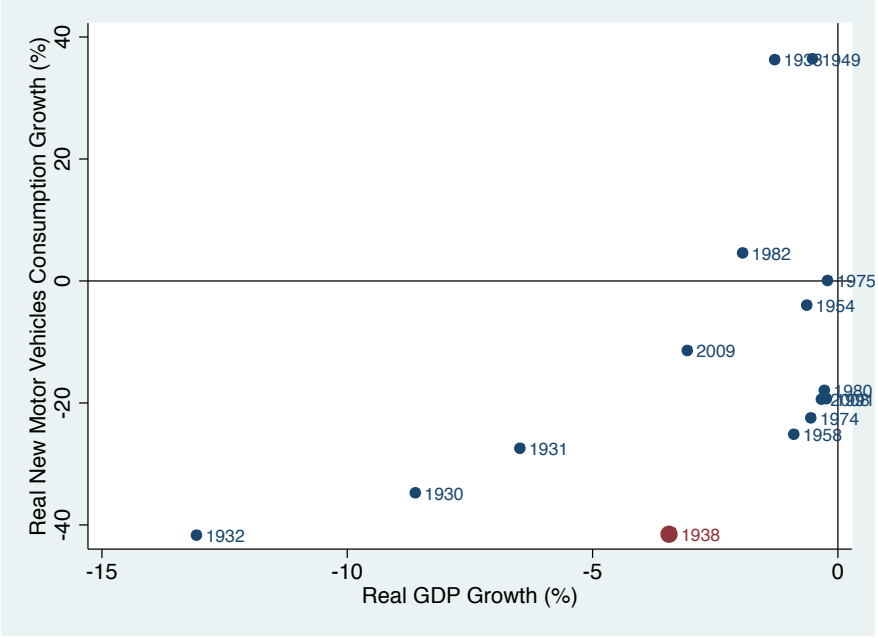


Figure 2.11: Scatter plot of new motor vehicle consumption growth against GDP growth. Includes all years of negative GDP growth since 1929, excluding 1945-1947. Source: NIPA table 1.1.3 and 2.4.3.

spending fell by 'only' 5.4 percent.[20] Many economists have noted and attempted to explain the 17.2 percent decline in durables consumption in 1930 (Mishkin 1978, Romer 1990, Olney 1999). Less noted is the fact that durables spending falls by almost exactly the same amount in 1938 (17.1 percent), despite a decline in output only 40 percent as large.

Figures 2.8-2.11 repeat figures 2.6 and 2.7 for the two largest components of durables consumption spending, household durables (including furniture and appliances), and new motor vehicles. From 1937 to 1938, household durables spending declines by 9.6 percent and new motor vehicles spending falls by 41.5 percent. Figure 2.8 shows that the decline in household durables spending was far larger than in a typical recession, but figure 2.9 suggests that given the large decline in GDP, the decline in household durables spending was not too unusual. By contrast, the 41.5 percent decline in new motor vehicles spending was remarkable (figures 2.10, 2.11) even given the magnitude of the 1938 recession. Since the BEA series began in 1929, only one year saw a larger decline in auto spending: in 1932, spending fell by 41.7 percent, 0.2 percentage points more than in 1938. But whereas in 1938 real GDP fell 3.5 percent, in 1932 it fell 13.0 percent.[21]

Nondurables Consumption

The poor performance of durables spending in 1937-38 was matched by unusually strong nondurables spending. From 1937-38, nondurables spending *rose* by 1.4 percent (figure 2.12). This reflected an increase in real spending on food and beverages of 2.1 percent, an increase in spending on clothing and footwear of 1.4 percent, and a 1.1 percent decline in spending on energy.[22] A scatter plot of changes in GDP and nondurables spending (figure 2.13) shows how unusual it is for nondurables spending to rise during a large recession. The 1937-1938 recession is the only time since the Great Depression in which nondurables spending rose in a recession in which annual GDP fell more than 2 percent.

Investment

Economists often expect that consumer durables and investment will behave similarly. Both ought to be sensitive to interest rates and to expectations of future income. To some extent, this intuition holds true in 1937-1938. Business investment - nonresidential structures, equipment, and inventories - does poorly. Oddly, on an annual basis, residential investment rises in 1938. However, this result is sensitive to the use of annual data. Gordon and Krenn's (2010) quarterly estimates of residential investment show a 25.4 percent decline in residential investment between the second quarter of 1937 and the second quarter of 1938.[23]

[20]NIPA table 1.1.1.

[21]NIPA table 1.1.6a.

[22]NIPA table 2.3.1.

[23]Gordon and Krenn (2010) provide monthly estimates of real GDP components back to 1919. To construct these estimates, they use the Chow and Lin (1971) interpolation procedure. Monthly interpolaters are chosen from the NBER macrohistory database. For years after 1929, the average of the monthly estimates is constrained to be equal to the NIPA data. Before 1929, monthly estimates average to the annual data in

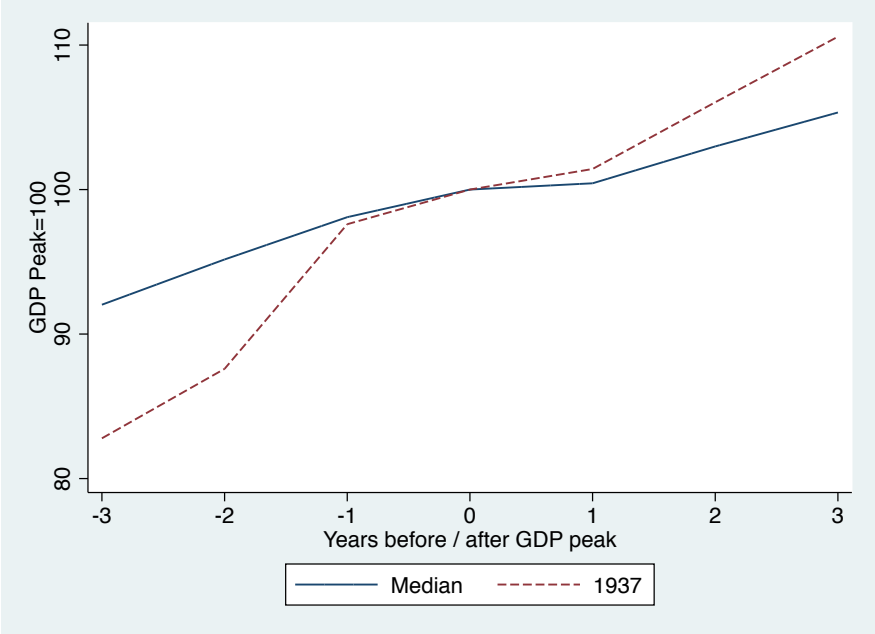


Figure 2.12: Real nondurables consumption around business cycle peaks, postwar median and 1937. Median includes all post World War II business cycles in which annual GDP fell (1948, 1953, 1957, 1973, 1980, 1981, 1990, and 2007). Source: NIPA table 1.1.3.

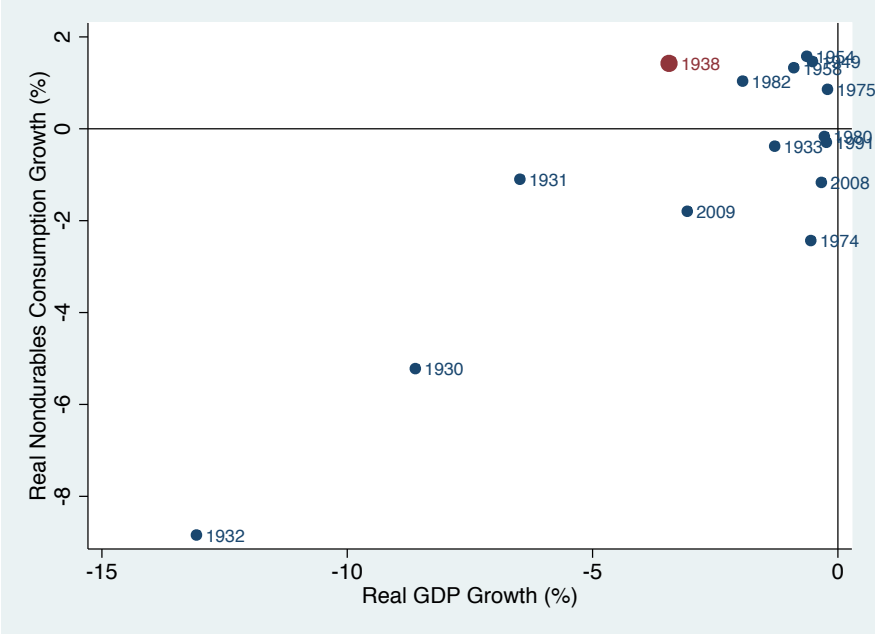


Figure 2.13: Scatter plot of nondurables consumption growth against GDP growth. Includes all years of negative GDP growth since 1929, excluding 1945-1947. Source: NIPA table 1.1.3.

Change in Real Output of Consumer Goods		
	1919-20	1920-21
Perishables	1.9%	6.2%
Semi-Durables	-7.5%	9.3%
Durables	2.4%	-24.6%
Household appliances	14.2%	-38.0%
Motor Vehicles	11.7%	-23.2%
Memo: Real GNP	-1.1%	-2.4%

Table 2.4: Consumption 1919-21. Sources: Consumption - Shaw (1947); GNP - Romer (1989).

The 1920-21 Recession

The preceding analysis uses data from the National Income and Product Accounts (NIPA) which begin only in 1929. It is also of interest to compare the 1937-38 recession with that in 1920-21. Shaw (1947) provides data on commodity output for different components of consumption in this period. While not directly comparable to NIPA data, Romer (1989) argues that the Shaw data is accurate and unbiased. Table 2.4 shows the behavior of components of consumption between 1919 and 1921. Interestingly, as in 1937-38, nondurables consumption rises between 1920-1921 while durables consumption falls by a large amount. Relative to the decline in durables consumption as a whole, the decline in auto purchases is, however, not unusually large in 1920-21. Consumption of household appliances falls by significantly more than that of motor vehicles.

Sectoral behavior and existing theories of the recession

The sectoral pattern of output is not easily explained by any existing theories of the 1937-38 recession. Theory and empirics suggest that increases in federal taxes (and decreases in transfers) should decrease all forms of consumption, not only durables consumption. Parker et al. (2011), for example, find that the economic stimulus payments of 2008 increased nondurables consumption. Despite the attraction of autos to veterans, even the veterans' bonus of 1936 was likely spent on nondurables as well as durables (see the previous chapter). And it is unclear why a monetary policy shock should impact one credit sensitive sector, autos, so much more than other durable goods or housing. Cole and Ohanian's (2001) union

Gordon and Veitch (1986).

The combination of the interpolation procedure and controversy over the correct annual figures for pre-1929 real GDP suggest that these data must be used with caution. For example, the Gordon and Krenn data show a 5.4 decline in real GDP from 1919-21, whereas Romer (1989) finds a 3.5 percent decline. And the Gordon and Krenn real GDP series has a peak in the first quarter of 1929, at odds with the usual view that the Great Depression began in the late summer of 1929. For these reasons, in this section I focus on more standard annual data. However, appendix table 1 provides a quarterly comparison of interwar and postwar recessions using the Gordon and Krenn data.

Price Index	Change 1937-38 (%)
GDP	-1.9
Consumption (PCE)	-2.3
Durables	-0.1
Furniture and furnishings	-3.2
Household appliances	0.4
New motor vehicles	5.0
Nondurables	-4.9
Services	0.1
Private Investment	-3.2
Nonres. structures	-1.9
Equipment	2.4
Trucks, buses, trailers	12.0
Autos	3.6
Residential	3.2
Exports	-4.6
Imports	-7.6
Government	0.2

Table 2.5: Prices 1937-38. Source: NIPA tables 1.1.4, 2.4.4., and 5.5.4.

wage shock hypothesis also does not easily explain the disproportionate decline of the auto industry. If wages rose in many industries, why was the auto industry uniquely affected?

2.4 An auto industry supply shock

The recession's anomalies strongly point to a role for the auto industry. I have shown that manufacturing employment falls most in Michigan and motor vehicle spending falls over 40 percent. The mystery is deepened by the fact that auto prices *rise* in the fall of 1937.

Table 2.5 shows the percent change from 1937 to 1938 in the price indexes for the major components of GDP. Prices economy-wide (measured by the GDP deflator) fell 1.9 percent. Consumer prices (measured by the personal consumption expenditures deflator) fell 2.3 percent. Prices for most sub-categories of output also fell. A notable exception was the price of autos. Prices of autos purchased by consumers rose 5.0 percent; prices of autos purchased by businesses increased 3.6 percent. Truck and bus prices rose 12 percent.

Other measures of auto prices confirm a large price spike with the introduction of 1938 model year cars in the fall of 1937. According to the BLS wholesale auto price index, nominal auto prices rose by more than 14 percent from December 1936 to December 1937.[24] Since most prices were falling, this meant that the real price of autos rose by even more. This price increase is more extraordinary when compared to the behavior of auto prices during the recovery from 1933-1937. Figure 2.14 shows the path of auto wholesale prices and the PPI. Until fall 1937, nominal auto prices were essentially flat, while economy-wide prices rose

[24]NBER macrohistory series m04180b.

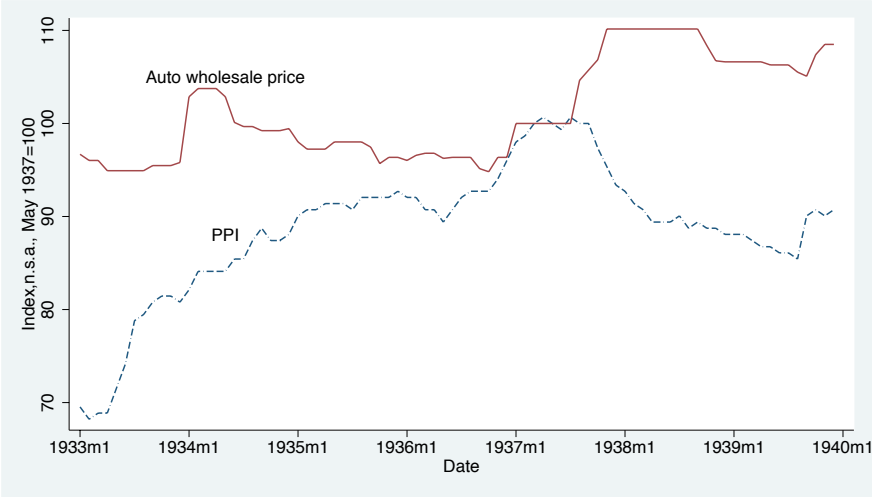


Figure 2.14: Auto wholesale prices and the PPI. Sources: Auto price - NBER macrohistory series m04180b, underlying data from the BLS and NBER calculations; PPI - FRED series PPIACO.

steadily. Thus the real price of autos trended down. In fall 1937, this trend was abruptly reversed.

One might worry that this price increase reflects a change in the type of automobiles being sold, i.e. an increase in the quality of the product. However, a wealth of data suggest that this was not the case. Adjusted for the weight of cars purchased, nominal prices rose by 9.0 percent from 1937 to 1938; prices per horsepower rose 8.1 percent (Ward's Reports 1938). Raff and Trajtenberg (1996) use hedonic regressions to compute a more sophisticated bi-annual quality-adjusted price index for new cars. They find that real quality adjusted prices fell continuously from 1922 to 1936, but then rose 16 percent from 1936-38. I find that even prices for unchanged models rose in 1938. For example, Chevrolets were essentially identical in the 1937 and 1938 model years. The car's engine displacement, horsepower, and wheelbase were exactly the same in the two model years.[25] Yet the price of a Chevrolet Master De Luxe Two Door Town Sedan, the most popular model, rose 8.7 percent from $690 for the 1937 model year to $750 for the 1938 model year.[26]

The prices I observe are 'Delivered in Detroit' prices. These were the manufacturer recommended prices including basic accessories and federal taxes. Consumers could expect to pay different amounts depending on their state taxes and on transportation charges (General Motors, 1937, pp. 19-20). A possible concern is that these prices do not accurately reflect the actual transaction prices which consumers' paid. The main margin through which trans-

[25] *Automotive Industries*, 76:9, February 27, 1937 p. 291; *Automotive Industries*, 78:9, February 26, 1938, p. 264.

[26] Quantity data (for 'most popular model' statement) from Heasley 1977; prices from *Automotive Industries*, 77:20, November 13, 1937, p. 718.

Figure 2.15: Advertisement for a 1938 Plymouth. Source: http://www.flickr.com/photos/paulmalon/5744021193/.

action prices could differ was through varying allowances for used car trade-ins (Federal Trade Commission 1939). This concern is mitigated by two facts. First, the delivered in Detroit prices match those published in advertisements, suggesting that they were strongly correlated with the actual transaction prices a consumer could expect to pay. Figure 2.15 shows an advertisement for a 1938 model year Plymouth sedan. The price on the ad - $685 - exactly matches that published in the industry trade journal *Automotive Industries* on November 13, 1937. (The same car cost $620 in the 1937 model year.) The second reason to believe that transaction prices did indeed rise for the 1938 model year is that industry observers were unanimous in stating that this was the case (*Automotive Industries*, fall 1937 issues).

Why did auto prices rise?

Auto prices rose because manufacturer's costs rose. Those in control of the auto industry took as given that they ought to respond to higher costs by raising prices. For example, Alfred Sloan, the CEO of General Motors, said in July 1937 (*Automotive Industries*, 7/31/37, p. 142): "Naturally, this trend toward rapidly increasing costs must, of necessity reflect itself in the form of a substantial increase in selling prices, with due regard for the effect of

volume." In explaining a planned price increase, a vice-president at Hudson Motor Company said (*Automotive Industries*, 8/14/37, p. 206): "Practically every product entering into the manufacture of our cars, including wages, has risen substantially in price. Naturally, this affects the price of our own product." The increase in costs referred to by these executives had two components: a strike / unionization induced wage increase and a raw material price increase.

Unionization

Unionization in the auto industry occurred in the context of rapid unionization in many sectors.[27] The percent of nonagricultural employees organized in unions rose from 13.1 percent in 1935 to 24.5 percent in 1940. Much of this increase was the result of successful efforts by the Congress of Industrial Organizations (the CIO) to organize heavy industry. In 1936, two factors made the automobile industry particularly ripe for unionization. First, worker discontent was widespread. One important factor was the seasonal aspect of employment, in which workers lost their jobs during the model year changeover with no guarantee that they would be rehired. Second, the dependence of manufacturers on certain key parts-producing plants made it easy for a strike at one plant to cripple an entire firm.

The United Automobile Workers (UAW), a largely independent affiliate of the CIO, began its organizing drive in November 1936. A key aspect of their strategy was the sit-down strike, in which workers occupied factories, preventing any production from occurring. The most important sit-down strike began on December 28 and 29 at Fisher body plants in Cleveland and Flint. Fisher body was a division of General Motors, and these plants were crucial links in the corporation's supply chain. Halting production at these two plants was sufficient to disrupt three-quarters of General Motor's total production. Strikes spread to other plants, and by early February, weekly vehicle production at General Motors had declined to 1,500 units from 53,000 units in mid-December.

With the help of Frank Murphy, Michigan's democratic governor, and pressure from President Roosevelt, these strikes led to an agreement between the UAW and General Motors on February 11, 1937 in which General Motors agreed to recognize the UAW as a bargaining agent for its members. The agreement itself secured no wage increases, but General Motors unilaterally increased wages while the sit-down strikes were ongoing. After organizing GM, the UAW organized sit-down strikes at Chrysler and reached a collective bargaining agreement on April 6. Despite UAW efforts, however, Ford was not successfully organized until 1941. In large part this reflected extreme ideological opposition to unions by Henry Ford and Ford's willingness to use physical force to keep unions out.

The UAW's efforts to organize the industry led to industry-wide wage increases. Between October 1936 and July 1937, average hourly earnings rose 22 percent from 79 cents to 96 cents (figure 2.16). Even at Ford, wages rose.[28] Unionization increased labor costs both by

[27]Unless otherwise noted, facts in this section are drawn from Galenson (1960).

[28]Personal communication from Briggs Depew.

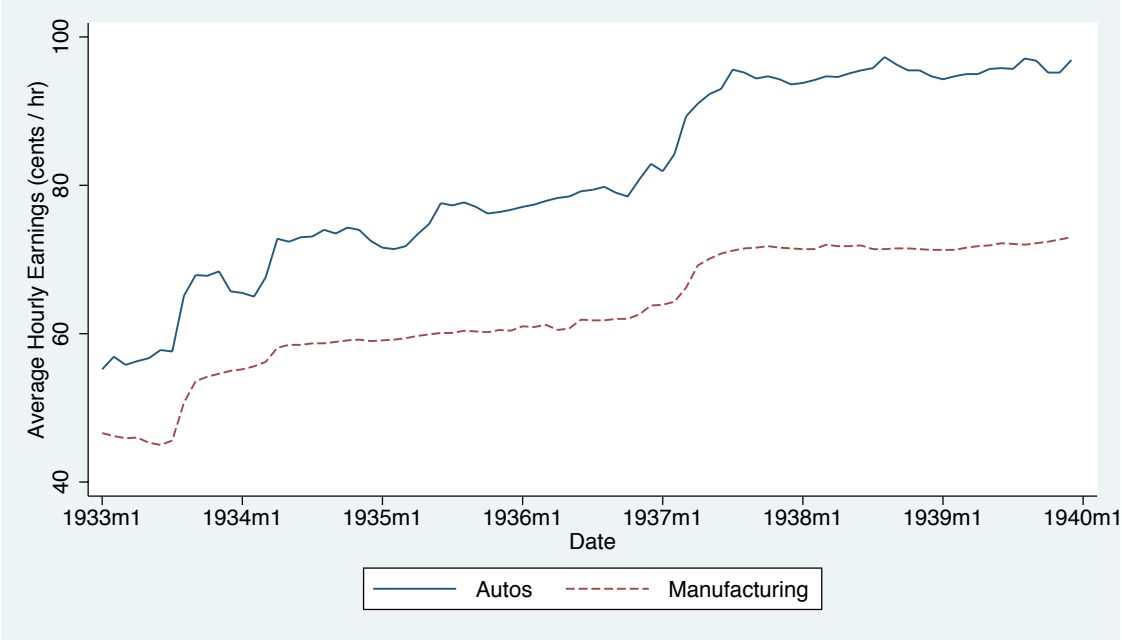


Figure 2.16: Average hourly earnings in auto manufacturing and average across 25 manufacturing industries. Sources: Beney (1936) and Sayre (1940).

raising wages and by forcing manufacturers to run plants overtime to make up for strike-related production shortfalls. This continued beyond the spring of 1937, since even after the UAW negotiated collective bargaining agreements, divisions within the UAW led to sporadic wildcat strike activity (*Automotive Industries*, 7/31/1937).

Raw Material Price Increases

Automobile production consumes large quantities of raw materials. Ward's automotive yearbook (1938) reports that to produce the typical 'small' car it took 1,919 pounds of steel, 357 pounds of cast iron, 89 pounds of cotton, 70 pounds of rubber, 51 pounds of glass and smaller quantities of copper, lead, zinc, manganese, aluminum, tin, wool and mohair, chromium, antimony, nickel, and cadmium.

The prices of many of these commodities rose rapidly in late 1936 and in the spring of 1937. Table 2.6 shows the prices for some of the commodities used in auto manufacturing. At least part of these price increases are explained by actual and expected rearmament demand in Europe. The League of Nations (Economic Intelligence Service 1938, p. 77) wrote:

> From the autumn of 1936 until the spring of 1937, there was a period of rapidly rising prices for primary products. The recovery of industrial production and intensification of rearmament expenditure in many countries had increased the demand for raw materials, until the point was reached at which real shortages

Material	Average 1936 Price (cents / pound)	July 1937 Price (cents / pound)	Percent Change
Sheet steel	3.0	3.6	20.0%
Forging steel	1.6	1.9	16.8%
Cast iron	0.9	1.1	21.8%
Wool	88.1	98.0	11.2%
Rubber	16.5	19.0	15.2%
Copper	9.5	13.8	45.3%
Lead	4.7	6.0	27.7%
Zinc	5.3	7.3	37.7%

Table 2.6: Raw material prices. Source: *Automotive Industries*, 9/18/1937, p. 374.

> of such materials were anticipated in spite of the relaxation of many restriction schemes controlling their production and export. At this point, in view of the fear of future shortages, a speculative demand for commodities developed and was superimposed upon the high normal demand.

The timing of the auto price increase

Manufacturers' labor and raw material costs rose in spring 1937. However, auto prices were little changed until the introduction of new model year vehicles in the fall. The BLS wholesale auto price index was literally unchanged from January to August 1937 (figure 2.14). This nominal rigidity distinguished the auto industry: in many other industries, costs rose in late 1936 and early 1937. In these other industries, prices tended to rise at the same time as costs rather than with a several month lag. Figure 2.17 shows the behavior of auto wholesale prices along with those for processed food, home furnishing goods, and textiles. All prices rise in late 1936 and / or early 1937. Auto prices are unique in rising further in fall 1937 and remaining high during the recession.

Importantly, in the summer and early fall of 1937 contemporaries recognized that auto manufacturers' costs had risen and that this would lead to higher car prices in future months. For example, in an article published on July 31, 1937, the industry trade publication, *Automotive Industries*, reports (p. 135):

> General Motors Corp. divisions had only ten days uninterrupted work from Feb. 11, the date an agreement was signed with the United Automobile Workers, through the June quarter, stated Alfred P. Sloan, Jr., chairman of the board, bitterly assailing the union for its inability to prevent wildcat strikes. He blamed the union for a decline in earnings during the second quarter compared with the like quarter of 1936. He said the company was unable to meet the demand for its cars because of interference with production. Demand currently is equal to

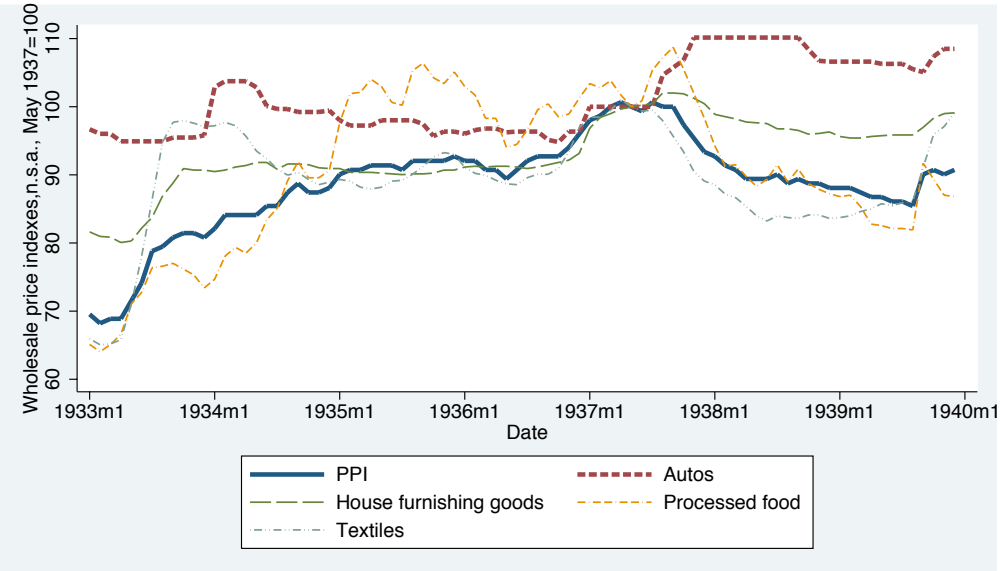


Figure 2.17: Wholesale prices 1933-39. Sources: PPI - FRED series PPIACO; house furnishing goods - NBER macrohistory series m04095a; textiles - NBER macrohistory series m04064a; autos - NBER macrohistory series m04180b; processed foods - NBER macrohistory series m04055.

> the company's ability to supply. Indirectly, labor troubles had another effect upon earnings, Mr. Sloan pointed out. Net income declined in relation to unit and dollar sales volume as a result of two general wage increases, numerous adjustments, and the need for using high-cost night shift operations in an effort to make up production deficiencies. Earnings were also affected by higher materials costs. High car prices for 1938 will result, he forecasts.

Ford and General Motors, but not Chrysler, did raise prices some in August 1937, before new model year vehicles were introduced (*Automotive Industries*, 8/7/1937, 8/14/1937, and 10/16/1937), but this price increase was seen as insufficient to cover the increase in costs that had occurred (*Automotive Industries* 9/18/1937, p. 374). Hence further price increases were expected. The president of Buick told the press on September 13, 1937 (qtd. in *Automotive Industries* 9/18/1937, p. 374): "We are not yet ready to disclose our price structure but I can tell you that due to the increased cost of labor and materials the prices will have to be somewhat higher."

2.5 How did the price shock impact sales?

Theory

Like an investment good, consumer durables purchases are likely to have a high intertemporal elasticity of substitution (Barsky, House, and Kimball 2007). In particular, expected future price increases will significantly increase current durables purchases as sales are pulled forward. The bulge in sales will be followed by a large drop in sales after the price increase occurs. Thus the elasticity of demand with respect to a price change that is expected will be much larger than the (static) elasticity of demand with respect to price. I shall argue that this phenomenon is the key to understanding how the price increase for 1938 model cars had significant macroeconomic effects.

Narrative Evidence

As discussed above, during the summer and early fall of 1937 price increases were widely expected within the auto industry. Contemporary industry observers reported that consumers also expected prices to increase and that this drove up sales prior to the introduction of new model year vehicles in October and November. For example, an article on the front page of the July 24, 1937 issue of *Automotive Industries* stated (p. 103): "The public has the impression, probably well founded, that 1938 cars will be somewhat higher in price and not much different from the present models. This is leading to current buying in considerable volume." A confirmation of this view comes from an ad appearing in the *San Francisco Chronicle* on August 8 1937 (figure 2.18). This ad, for a Graham Supercharger, encourages customers to "buy now . . . before the price rise." It suggests that higher prices were expected, and that they motivated purchases.

In 1938, General Motors sponsored a study on the impact of auto prices on demand which was presented at a joint meeting of the American Statistical Society and the Econometric Society in Detroit. The published volume begins with an essay by a General Motors employee S. L. Horner (1939) summarizing the reasons General Motors sponsored the study and providing an overview of the determinants of auto demand. Horner emphasizes the importance of price expectations, particularly in fall 1937 (p. 14-15):

> During periods of rising costs, such as that which took place in 1937 when labor costs were rising so rapidly, prospective automobile purchasers tend to anticipate an increase in the price of automobiles. This is particularly apt to be the case when the advance in costs continues over a considerable period of time without an increase in automobile prices. Automobile buyers are sufficiently well informed to realize that any such increase in costs must, in time, cause an increase in automobile prices. And, if they do not arrive at this conclusion themselves, they are almost certain to have it impressed upon them by dealers and salesmen within the industry. Thus, during the spring and summer of 1937, dealers and salesmen were urging people to buy their new cars, before prices increased. This tends to

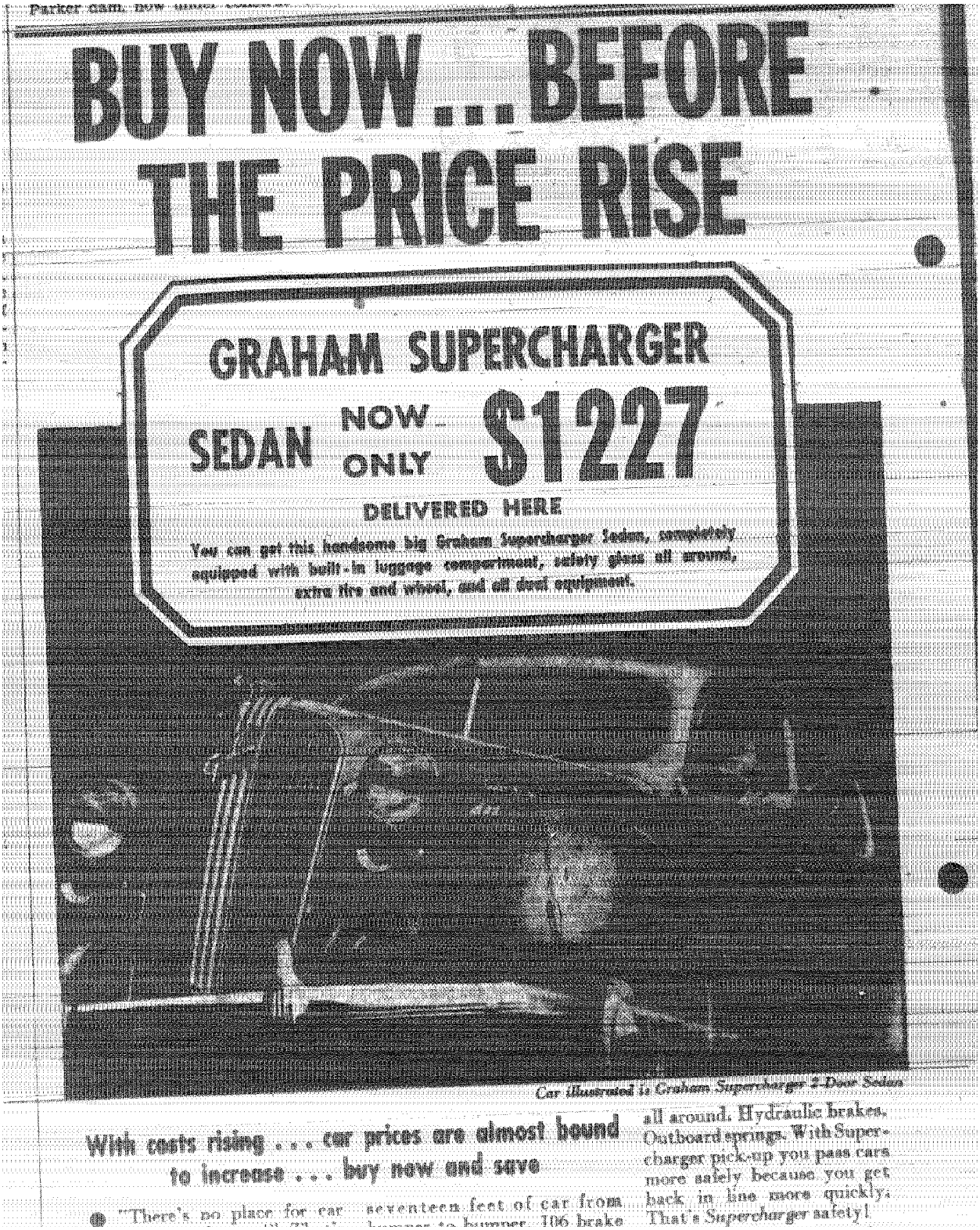


Figure 2.18: From the *San Francisco Chronicle*, August 8 1937.

> cause a bulge in sales which is followed, after prices are increased, by a decline in sales.

Horner attributes low sales in 1938 to the fact that "many people, anticipating that an increase in automobile prices would be made when the 1938 models were announced, purchased cars in the 1937 model year which they otherwise would have purchased in 1938" (p. 14).

Quantitative evidence

The hypothesis that price expectations boosted sales in summer and early fall 1937 and depressed sales in 1938 has a testable implication. If the hypothesis is true, then a statistical model of auto sales that incorporates determinants of auto demand other than auto prices and price expectations should underpredict sales in the summer of 1937 and overpredict sales in 1938. In this subsection, I construct such a model and use it to quantify the impact of the auto price shock.

In addition to autoregressive lags and seasonal dummies, there exist an almost limitless number of possible variables that might have predictive power for auto sales. Even just 10 possible right hand side variables, each with 12 possible lags means $10^{13} = 10,000,000,000,000$ possible models, far too many to consider. I proceed by considering only variables that measure the stance of monetary and fiscal policy and / or were deemed important by contemporary and current forecasters of auto sales.

Horner (1939) provides a useful guide to how contemporaries thought about the determinants of auto sales. He argues that the following equation fits annual sales well:

$$log(Sales)_t = \beta_0 + \beta_1 log(GNP)_t + \beta_2 log(GNP_t/GNP_{t-1}) + \epsilon_t \tag{2.2}$$

As a guide to how modern economists forecast sales, I consider the reduced form forecasting equation for light vehicle sales in the Michigan model of the U.S. economy (Hymans et al. 2002, equation C33). This equation includes two proxies for the state of the macroeconomy: a distributed lag of real personal disposable income and the change in the unemployment rate.

Below I describe each variable I use and the reason for its inclusion.

Seasonal Effects

In the interwar period, auto sales exhibited a strong seasonal pattern, with winter sales far below summer sales. Seasonal patterns were partly driven by the weather: muddy roads made driving unpleasant or infeasible in much of the country during the winter and spring. But a large driver of seasonal patterns was also the introduction of new model vehicles. Until 1935, new model vehicles were introduced in January of each year. Thereafter, in a change coordinated through the National Industrial Recovery Act, new models were introduced in October or November (Cooper and Haltiwanger 1993). To account for the resulting change in seasonal patterns, I include both a full set of monthly dummies, and a set of monthly dummies interacted with a dummy variable equal to one in 1935 and after.

Real Personal Disposable Income

Both Horner (1939) and the Michigan model include a measure of national income. Horner (1939) uses GNP and the Michigan model uses real personal disposable income. For my purposes, disposable income has the attractive feature that it both reflects the evolution of wages and profits in the private sector and reflects changes in taxes and transfers. Thus it proxies for the state of the economy and fiscal policy actions. Unfortunately, there is no monthly series for disposable income in the interwar period. But Harold Barger constructed a seasonally adjusted quarterly measure of nominal personal disposable income.[29] I deflate this by the CPI and convert it to a monthly series using a cubic spline.

Note that since auto sales cause income as well as vice versa, including real disposable income in the forecasting model will bias the results against finding a large effect of the exogenous auto shock. For example, if sales were unusually low in 1938, this would have lowered real disposable income, leading my forecast to underestimate the level of sales that would have occurred absent the shock. Since I measure the impact of the shock as the difference between forecast and actual sales, this would have the effect of reducing the size of the measured shock.

The Money Supply

Neither Horner (1939) nor the Michigan model include a measure of the money supply or interest rates. However, since I wish to know what part of the decline in auto sales cannot be accounted for by macro policy, I need a measure of monetary policy in the forecasting equation. Theory suggests that interest rates rather than the quantity of money ought to be most directly linked to sales. The proportion of cars sold on credit exceeded 50 percent in every year from 1919-1939 (Olney 1999, Table II), providing an obvious channel through which interest rates would be expected to impact sales. Since interest rates changed little in 1937-38 (figure 2.2), however, they will be unable to forecast the decline in auto sales between 1937 and 1938. To make sure I do not understate the possible impact of monetary policy on sales, I instead include the quantity of money, specifically the seasonally adjusted broad money stock (currency held by the public plus demand and time deposits of commercial banks).[30]

Recession Dummy

The Michigan model includes the unemployment rate. Unfortunately this is unavailable at a monthly or quarterly frequency before 1929. As an indicator of general business cycle conditions, I instead incorporate a dummy variable equal to 1 in a month between an NBER business cycle peak and trough (inclusive of the peak and trough month).

[29]NBER macrohistory series q08282a. Underlying source is "Unpublished data from the files of Dr. Harold Barger."

[30]NBER macrohistory series m14144a. Computed by the NBER from underlying data in Friedman and Schwartz (1970), *Monetary Statistics of the United States.*

Variable	Lags considered
Log Auto Sales (Dependent variable)	1-12
Log Real Personal Disposable Income	0-12
Log Difference Money	0-12
Recession Dummy	0-12
Total number of specifications	26,364

Table 2.7: Lag possibilities for forecasting model

Lag selection

Ideally, one would like to consider all possible lag combinations of the above variables as well as run the model with all variables in levels, logs, differences, and log differences. To make computational time reasonable, I limit these possibilities in several ways. First, I consider only models with the dependent variable (auto sales) in logs. Preliminary regressions suggested this yielded as good a forecast (measured by information criteria) as any other transformation of the dependent variable, and it is the form preferred by Horner (1939). Since there may be a trend to log auto sales, I include a time trend. Second, based on results from preliminary regressions, I include only the log of real personal disposable income, the log difference of the money supply, and the level of the recession dummy. Third, I do not consider specifications in which lags are skipped. Thus I consider, for example, all possible specifications with 4 lags or 12 lags of log disposable income, but I do not consider a model with just the 4th and the 12th lag. Table 2.7 summarizes the specifications I consider.

To choose what lags to include, one does not simply want to choose the model with the best fit. Such a procedure would select an overparameterized model with poor out-of-sample performance. An approach to dealing with this problem is to select models by minimizing an information criterion. An information criterion rewards in-sample fit but penalizes additional right hand side variables (or lags). For each model, I compute the Akaike information criterion (AIC) and Bayesian information criterion (BIC), where

$$AIC = Nln(SSR) + 2P, \tag{2.3}$$

and

$$BIC = Nln(SSR) + ln(N)P. \tag{2.4}$$

N is the number of observations, SSR is the sum of squared residuals (computed relative to the level of auto sales), and P is the number of parameters estimated (Enders 2004).

Both the AIC and BIC reward better fit and punish additional variables, but the BIC punishes additional variables more. The BIC will never select a model with more right hand side variables than the model selected by the AIC. Both information criterion are standard and have theoretical appeal. Asymptotically, the BIC will select the correct model. But in a small sample, Monte Carlo studies suggest the AIC may perform better (Enders 2004). I evaluate both the AIC and BIC for each possible specification.

Dependent variable is log auto sales	Preferred by min. AIC	Preferred by min. BIC
Lag log auto sales	0.694***	0.539***
	(0.0854)	(0.0625)
Lag 2 lag auto sales	-0.312***	
	(0.101)	
Lag 3 log auto sales	0.151*	
	(0.0777)	
Log real disposable income	1.100***	1.066***
	(0.248)	(0.212)
Log difference money	2.214***	2.489***
	(0.708)	(0.722)
Recession dummy	0.0392	0.0557
	(0.0587)	(0.0604)
Lag recession dummy	-0.0896	-0.162**
	(0.0806)	(0.0626)
Lag 2 recession dummy	-0.0620	
	(0.0619)	
Time trend	-0.00193***	-0.00194***
	(0.000482)	(0.000463)
Sample period	Jan. 1926-March 1937	
Observations	135	135
R-squared	0.969	0.965
AIC	1461	1463
BIC	1557	1551

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.8: Estimation results for specifications selected by AIC and BIC criteria. Seasonal terms and a constant are omitted from the table.

Results

Monthly auto sales data begins in 1925.[31] To allow for up to 12 possible lags, I estimate each model over the period January 1926-March 1937. I choose March 1937 as the end date, since it is roughly when unionization was complete, and is thus the earliest consumers would have started to expect price increases for the next model year. Table 2.8 shows the estimation results for the optimal specifications chosen by the AIC and BIC criterion. Both specifications are parsimonious and have excellent in-sample fit.

Figure 2.19 shows the predicted and actual path of auto sales in 1937 and 1938. Since the forecast is somewhat sensitive to the start date, I graph the AIC and BIC forecasts for all start dates from January 1937-July 1937. The models perform poorly and disagree

[31] As is standard in the literature (e.g. Cooper and Haltiwanger 1993), I use new registrations as the measure of sales. Unfortunately, data on actual sales are not reported in the interwar period. Purchasers of new cars were legally obligated to register their vehicles although some lag was possible (*Survey of Current Business* 1934). Data on registrations was reported monthly in the *Survey of Current Business*. I take the data from NBER macrohistory series m01109.

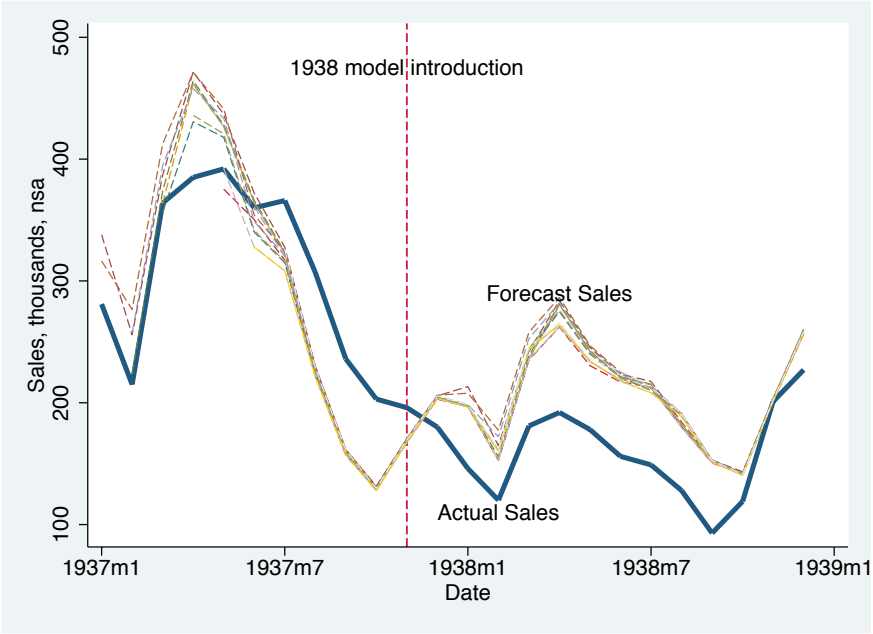


Figure 2.19: Actual and forecast auto sales.

significantly in the spring of 1937, perhaps reflecting the effects of strikes in the auto and other industries.[32] But beginning in mid-summer 1937, there is remarkable agreement among the forecast models. All predict sales below actual from July-November 1937, and all predict sales above actual from December 1937 to December 1938.

The forecast exercise fits - both quantitatively and qualitatively - remarkably well with the narrative evidence and theoretical consumption response to an expected durable goods price increase. Up until the price increase occurs - in October and November 1937 - sales are higher than they otherwise would have been; after the price increase, sales are far lower than they would have been. One might ask why forecast sales are still below actual sales in November 1937? There are two possibilities: one is that 1937 model vehicles may still have been sold at the old lower price in the first part of the month. Second, since the sales data are registrations, it may have some lag relative to actual sales.

Table 2.9 shows the quantitative implications of the forecasting exercise for the aggregate economy. Columns 1 and 2 show annual auto sales and forecast auto sales in 1936, 1937, and 1938.[33] If not for the price shock, unit sales would have fallen by 'only' 25 percent between 1937 and 1938; instead, they fell by 46 percent. Columns 3 and 4 show the level and percent

[32] *Automotive Industries*, 7/31/37, p. 138 wrote: "Psychological unrest, combined with an actual loss of purchasing power due to the strikes in various industries this year, can probably be blamed more than anything else for the failure of automobile sales to set new records in May and June."

[33] Forecast sales in 1937 equal actual sales through March plus forecast sales for April to December. The forecast is the median of the specifications minimizing AIC and BIC for all start dates from January to July 1937.

Year	Auto sales (thousands)	Auto sales w/o auto price shock (thousands)	Real GDP (1937 $s, billions)	Change in real GDP	Real GDP w/o auto price shock (1937 $s, billions)	Change in real GDP w/o auto price shock
1936	3404		87.4	13.1%		
1937	3485	3309	91.9	5.1%	91.6	4.9%
1938	1890	2489	88.7	-3.5%	89.6	-2.3%

Table 2.9: Aggregate implications of forecasting exercise. Sources: Real GDP is from NIPA table 1.1.6A. Auto sales data from NBER macrohistory series m01109 (see text).

change in real GDP. Column 5 shows counterfactual estimates of GDP without the auto price shock. This is computed as

$$GDP_{cf} = GDP + (Sales_{cf} - Sales) \cdot P \cdot M, \tag{2.5}$$

where X_{cf} is the counterfactual estimate of X, P is the average retail price of a passenger vehicle, and M is the multiplier for automobile spending. For P, I use the estimate in Suits (1958, p. 279) for the average retail value of cars sold in 1937, $808. For the value of the multiplier, I use 1.8, the estimate of Gordon and Krenn (2010) for the government spending multiplier in 1940.[34]

The final column reports estimates of GDP growth without the auto shock. Expectations of price increases boosted sales and thus GDP growth in 1937, but made the recession in 1938 much more severe. The auto price shock explains over a third of the 1938 GDP decline.

2.6 Can the auto price shock explain the recession's anomalies?

Timing

Figure 2.20 shows the path of seasonally adjusted auto production and sales in 1937 and 1938. As was generally true in the interwar period, production and sales are highly correlated (Cooper and Haltiwanger 1993). Auto manufacturers appeared to emphasize a target

[34]For more discussion of the likely size of the multiplier in conditions like those in 1937, see chapter 1, section 1.7.

My results scale linearly with the size of the multiplier. The table below shows the implications of two alternative assumptions about the multiplier's size.

Year	Change in real GDP	Change in real GDP w/o auto price shock, M=1	Change in real GDP w/o auto price shock, M=2.5
1937	5.1%	5.0%	4.7%
1938	-3.5%	-2.8%	-1.8%

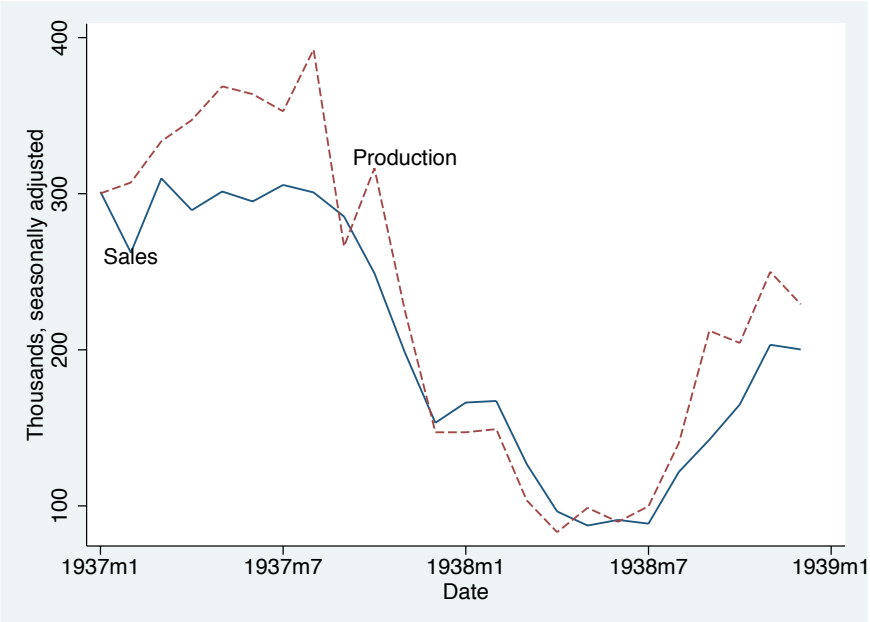


Figure 2.20: Seasonally adjusted auto sales and production. Sources: NSA data from NBER macrohistory series m01109 and m01107a. Seasonally adjusted by regressing on monthly dummies and monthly dummies interacted with post 1935 dummy variable.

inventory level rather than production smoothing (Kashyap and Wilcox 1993). Thus as sales collapsed in fall 1937, so did production. To what extent does this decline explain the rapid decline in industrial production as a whole?

Automobile production had a 4.79 percent weight in the Federal Reserve industrial production index (Federal Reserve Board 1940). Hence even very large swings in auto production can account only for modest swings in industrial production as a whole. The key to the quantitative importance of auto production for movements in the IP index is the large input-output linkages between auto production and other manufacturing sectors. Fortunately, while working for the Bureau of Labor Statistics (BLS), Wassily Leontief constructed a 38x38 input-output table for the 1939 U.S. economy (BLS, undated). Although a smaller version was published in Leontief (1951), until now the unpublished larger table appears to have been unnoticed and unused by economists. The larger table is a valuable source of information on the linkages connecting auto production with the rest of the economy.

Table 2.10 shows the share of net output consumed by the motor vehicle industry for each Federal Reserve industry group. Demand from auto manufacturers was particularly important for steel production. Aside from the construction sector, the auto industry was the largest purchaser of steel products. To get a rough quantitative sense of the importance of the auto industry for the timing of IP movements, I construct an industrial production index that excludes autos and the share of other sectors consumed by the auto industry

Industry group	Share of net output purchased by motor vehicles industry
Durable manufactures	
Iron and steel	12.5%
Machinery	1.7%
Transportation equipment ex. autos	0.0%
Nonferrous metals and their products	5.2%
Lumber and products	0.7%
Stone, clay, and glass Products	2.7%
Nondurable manufactures	
Textiles and products	0.9%
Leather and products	0.4%
Manufactured food products	0.0%
Alcoholic beverages	0.0%
Tobacco products	0.0%
Paper and products	0.1%
Printing and publishing	0.0%
Petroleum and coal products	0.4%
Chemicals	0.9%
Rubber products	19.8%
Minerals	
Fuels	0.4%
Metals	10.6%

Table 2.10: Share of industry group output purchased by the motor vehicle industry. Calculated from 1939 38x38 input-output in BLS (undated).

('purged IP'). Formally,

$$IP_{xauto,t} = IP_t - v_{auto} IP_{auto,t} - \sum_{i=1}^{18} w_i v_i IP_{i,t}, \tag{2.6}$$

where w_i is the share of industry group i net output purchased by the auto industry, v_i is the share of industry group i in total industrial production, and $IP_{i,t}$ is the seasonally adjusted value of the industrial production index for industry group i (indexed to 1935-39=100). Industrial output weights and indexes are from the Federal Reserve (1940).

Figure 2.21 shows the path of total and purged industrial production in 1937 and 1938. In the summer of 1937, when auto sales are brought forward due to expected higher prices, total industrial production (which includes autos) exceeds the purged index. This result confirms the view expressed in the *The Review of Economics and Statistics* summary of the 1937 economy that "During the spring and summer months [of 1937], the maintenance of manufacturing output at the high level attained in 1936 was due almost entirely to sustained activity in the iron and steel and automobile industries" (Crum, Gordon, and Wescott 1938, p. 45). In the late fall, once auto prices rise, total IP falls below purged IP. This exercise suggests that the auto shock likely contributed to the very rapid decline of industrial pro-

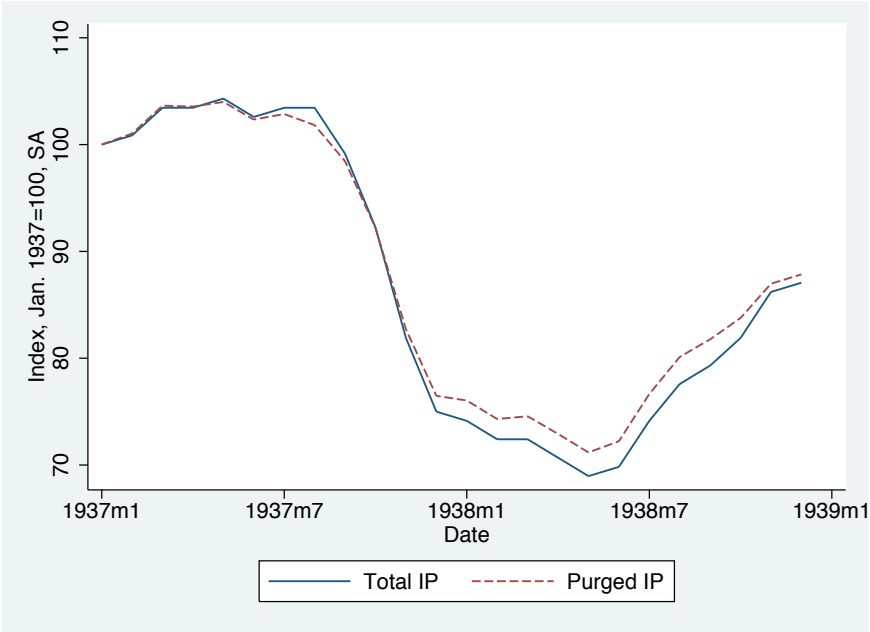


Figure 2.21: Total and purged industrial production. Sources: Total IP - Federal Reserve 1940; purged IP - see text.

duction in fall 1937 by boosting production in late summer and lowering production in early winter. However, quantitatively, the impact of autos on IP is modest. The overall IP index falls 24 percent from September to December while the purged index falls 22 percent.

Geography

To see how the auto shock may explain variation in state outcomes during the recession, I construct a measure of the share of manufacturing employment in each state in a 'synthetic' auto sector ($Share$).

$$Share_j = e_{auto,j} + \sum_{i=1}^{16} w_i e_{i,j}, \tag{2.7}$$

where $e_{auto,j}$ is the share of state j wage earner employment in the motor vehicle industry, w_i are the sectoral linkages from table 2.10 (the share of industry i output used in the production of motor vehicles), and $e_{i,j}$ is the share of wage earner employment in state j in industry i. Employment shares are from the 1937 *Census of Manufactures*. Table 2.11 shows the values of $Share$ for each state. Not surprisingly, the highest values are in the traditional centers of the auto industry, Michigan, Indiana, and Ohio.

Michigan, Indiana, and Ohio - in that order - were also the three states with the worst manufacturing employment outcomes in the recession (figure 2.5). This link is almost certainly causal: since demand and employment in the auto industry were determined nation-

State	Share	State	Share
AL	4.6%	NE	0.6%
AZ	2.4%	NV	0.1%
AR	0.7%	NH	0.7%
CA	4.7%	NJ	5.3%
CO	3.0%	NM	0.5%
CT	4.8%	NY	4.1%
DE	4.0%	NC	0.9%
FL	1.9%	ND	0.1%
GA	1.4%	OH	9.8%
ID	0.5%	OK	1.7%
IL	4.0%	OR	1.6%
IN	10.4%	PA	5.5%
IA	1.8%	RI	2.5%
KS	1.6%	SC	0.9%
KY	2.2%	SD	0.2%
LA	2.7%	TN	1.8%
ME	3.4%	TX	2.9%
MD	7.1%	UT	1.0%
MA	3.2%	VT	1.2%
MI	29.7%	VA	6.5%
MN	2.7%	WA	2.5%
MS	0.7%	WV	3.8%
MO	3.5%	WI	8.0%
MT	0.3%	WY	0.3%

Table 2.11: Share of employment in each state in the synthetic auto sector. Sources: See text.

wide, it cannot have been poor conditions in these states that caused the decline in auto industry employment. And it is unclear what, if any, third factor would have caused both an outsized decline in auto industry employment and poor employment in outcomes in these three states.

There is some evidence that contemporaries noticed this link and blamed the auto industry. A.A. Berle Jr., the assistant secretary of state, advocated government planning of auto production noting that "the motor industry in 1937 undertook largely to increase its output and sales. It did this at the cost of suspending much of its activities in 1938 and causing widespread distress in the Detroit and Ohio areas" (*Automotive Industries*, 8/27/1938, p. 242). General Motors in particular attracted public attention when on one day, January 1 1938, it laid off 30,000 workers (*Automotive Industries*, 1/1/38 and 1/15/38).

To more formally assess the link between the auto sector and state employment outcomes, I estimate the simple regression of state employment change on *Share*. Results are in table 2.12. Panel A includes Michigan while panel B excludes Michigan. Not surprisingly, the high share of employment linked to the auto industry explains why Michigan does so poorly in the recession, and hence excluding Michigan from the regression reduces the statistical significance of *Share* and the R^2. The tables also shows results for subsets of states with

Panel A: Including Michigan

	Private employment change 37-38	Private employment change 37-38	Private employment change 37-38	Manufacturing employment change 37-38	Manufacturing employment change 37-38	Manufacturing employment change 37-38
Share	-0.473***	-0.565***	-0.528***	-0.988***	-0.783***	-0.743***
	(0.154)	(0.134)	(0.130)	(0.215)	(0.170)	(0.175)
Impact of 1 standard deviation increase (percentage points)	*-2.2*	*-3.0*	*-3.2*	*-4.5*	*-4.1*	*-4.5*
Constant	-2.497***	-1.466	-1.870*	-4.451***	-6.327***	-6.815***
	(0.873)	(0.937)	(1.060)	(1.225)	(1.184)	(1.421)
Sample	All states	States with > 50,000 wage earners	States with > 100,000 wage earners	All states	States with > 50,000 wage earners	States with > 100,000 wage earners
Observations	48	31	22	48	31	22
R-squared	0.171	0.379	0.450	0.314	0.424	0.475

Panel B: Excluding Michigan

	Private employment change 37-38	Private employment change 37-38	Private employment change 37-38	Manufacturing employment change 37-38	Manufacturing employment change 37-38	Manufacturing employment change 37-38
Share	-0.398	-0.693**	-0.561*	-1.637***	-1.152***	-1.072**
	(0.291)	(0.291)	(0.305)	(0.392)	(0.360)	(0.401)
Impact of 1 standard deviation increase (percentage points)	*-1.0*	*-1.7*	*-1.5*	*-4.1*	*-2.9*	*-2.9*
Constant	-2.696**	-1.005	-1.730	-2.745*	-4.996***	-5.423**
	(1.093)	(1.327)	(1.590)	(1.473)	(1.645)	(2.086)
Sample	All states	States with > 50,000 wage earners	States with > 100,000 wage earners	All states	States with > 50,000 wage earners	States with > 100,000 wage earners
Observations	47	30	21	47	30	21
R-squared	0.040	0.169	0.151	0.280	0.267	0.274

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 2.12: State employment outcomes and synthetic auto share. Sources: Employment data from Wallis (1989). Construction of *Share* described in text.

significant employment in manufacturing, since many small states have trivially low values of *Share*. Even in the regressions excluding Michigan, there is strong evidence that the share of employment linked to the auto industry was negatively correlated with state outcomes during the recession. Results are generally statistically and economically significant. In the specification including all states, a one standard deviation higher value of *Share* lowered total state employment growth in the recession by 2.2 percentage points and lowered manufacturing employment growth by 4.5 percentage points. Among the 22 states with more than 100,000 manufacturing wage earners, employment in the synthetic auto sector explains 48 percent of the variation in manufacturing employment outcomes. This is evidence that the auto shock played a role in generating large geographic variation in 1937-38.

Sectors

The auto shock can clearly explain the anomalous decline in motor vehicle consumption during the recession. As noted above, the forecasting exercise suggests that absent the price shock, unit auto sales would have fallen by 25 percent between 1937 and 1938 rather than

the actual 46 percent. Measured in chained 1937 dollars, total durables consumption fell 17.6 percent from 1937 to 1938.[35] With the conservative assumption of no multiplier effect on other durables purchases, and that 70 percent of passenger car sales were to consumers, the decline in durables consumption would have been 11.8 percent absent the auto shock.[36]

The principle other anomaly in the recession is that nondurables consumption *rises*. It is possible that this reflects a relative price effect of the auto shock. Households may have spent some of the income that they would have spent on autos on nondurable items like clothing and food. In February 1937, a pound of lard cost 18.2 cents and a standard two-door Ford sedan cost \$585.[37] Buying the Ford was equivalent to giving up 585/.182 = 3214 pounds of lard. In February 1938, the price of a pound of lard had fallen to 14.2 cents while the price of the Ford had risen to \$665. Buying the Ford in 1938 was equivalent to giving up 665/.142 = 4683 pounds of lard. Thus it is perhaps unsurprising that in 1938 consumers bought more lard and fewer Fords.[38] This effect could also rationalize why employment rose in so many states. In all states in which manufacturing employment rose, the value of *Share* is below 3 percent. Manufacturing employment in these states had little direct connection to auto demand. The increase in nondurables consumption - perhaps due to the relative price shock - may explain the unusually good performance of these states in the recession.

2.7 Conclusion

The 1937-38 recession exhibits timing, sectoral, and geographic anomalies not easily explained by monetary policy, fiscal policy, or economy-wide unionization. The decline in industrial production from September to December 1937 is the largest three-month decline in the history of the Federal Reserve and Miron-Romer series going back to 1884. Manufacturing employment rises in 8 states despite a 12 percent decline nationwide, and durables consumption falls over 17 percent while nondurables consumption rises.

This chapter argues that a supply shock in the auto industry explains a significant part of these anomalies. Labor strife and raw material price increases raised costs for auto manufacturers in early 1937. However, nominal price rigidity meant that prices for new autos did not rise until late summer and fall. For several months consumers accurately expected the price increase, leading to a bulge in purchases in the summer and early fall of 1937 followed by a collapse of auto sales and production in 1938. This hypothesis is confirmed both by narrative evidence and a quantitative forecasting exercise. I construct a forecast of how sales would have behaved absent exogenous developments in the auto industry. This

[35]NIPA table 1.1.6a. In section 2.3, I report percent changes of the chained 2005 dollar quantities, which differ slightly.

[36]It is standard to assume that 70 percent of passenger car sales are to consumers. See Olney 1991, p. 95.

[37]Lard prices are from NBER macrohistory series m04027 and are the retail price of lard in New York. Ford prices are from *Automotive Industries*, 11/13/1937.

[38]Data on lard production are from NBER macrohistory series m01142a; data on Ford production are from Heasley (1977).

exercise implies that in 1938, 600,000 more cars would have been sold, and GDP growth would have been 1.2 percentage points higher. Equally important, there is strong evidence that the auto shock contributed to the anomalous timing, geographic, and sectoral patterns of output in the recession. Absent this shock, the 1937-38 recession would have been both less severe and less unusual.

Table A.1: Quarterly output data

GDP Peak-Trough	Change in real GDP	Change in durables consump.	Change in non-durables consump.	Change in equipment and software investment	Change in nonresidential structures investment	Change in residential investment
1920:3-1921:4	-3.4%	-14.5%	6.2%	-41.1%	84.0%	-12.6%
1923:4-1924:2	-0.5%	-5.5%	1.4%	-9.2%	2.6%	-4.1%
1926:3-1927:3	-1.3%	-5.8%	0.2%	-11.5%	-10.9%	0.3%
1929:1-1932:4	-32.7%	-51.0%	-17.2%	-72.4%	-72.9%	-78.9%
1937:2-1938:2	**-6.6%**	**-24.2%**	**1.3%**	**-38.1%**	**-32.3%**	**-25.4%**
1948:4-1949:4	-1.6%	16.2%	1.4%	-16.5%	-14.5%	10.8%
1953:2-1954:1	-2.6%	-6.4%	0.2%	-5.3%	-2.0%	-3.6%
1957:3-1958:1	-3.7%	-7.7%	-1.5%	-14.2%	-9.6%	-4.1%
1960:1-1960:4	-1.6%	-1.1%	0.6%	-5.8%	-1.5%	-11.1%
1969:3-1970:1	-0.6%	-1.9%	1.5%	-1.0%	-1.2%	-7.1%
1973:4-1975:1	-3.2%	-8.7%	-3.4%	-9.5%	-10.1%	-30.2%
1980:1-1980:3	-2.2%	-6.8%	-1.6%	-5.9%	-4.4%	-17.1%
1981:3-1982:1	-2.9%	-4.2%	0.6%	-2.7%	0.0%	-15.4%
1990:2-1991:1	-1.4%	-6.4%	-0.8%	-2.3%	-3.8%	-16.2%
2007:4-2009:2	-5.1%	-13.3%	-3.7%	-22.1%	-21.0%	-36.1%

Note: Excludes the post World War II recession. The 2001 recession is also excluded since GDP barely declines. Sources: Gordon and Krenn (2010) for interwar recessions; thereafter NIPA table 1.1.3.

A Appendix

Chapter 3

Growth After a Financial Crisis: The U.S. in Spring 1933

3.1 Introduction

From its low point in March 1933, seasonally adjusted industrial production rose 57 percent in four months.[1] The speed of the recovery is surprising given Reinhart and Rogoff's (2009, 2010) argument that recoveries after financial crises are usually slow.[2] Yet after the nearly complete collapse of the banking system in March 1933, the U.S. experienced its most rapid four months of growth since at least World War I. The existing literature has emphasized two explanations for the economy's emergence from the Depression in spring 1933. Eggertsson (2008) argues that Franklin Roosevelt's words and fiscal actions raised inflation expectations thus lowering real interest rates and stimulating consumption and investment. Temin and Wigmore (1990) also argue that inflation expectations mattered, but in addition stress the importance of the direct effect on farmers of the U.S. abandonment of the gold standard. They argue that devaluation raised crop prices, thus increasing farm income and consumption.

It has been difficult to disentangle a distinct farm channel from the overall expectations channel absent high frequency geographically disaggregated data. We have not known whether recovery proceeded more or less rapidly in North Dakota or in New York. But knowledge of the sources of recovery in spring 1933 is critically important and not only for our understanding of the end of the Depression. Spring 1933 is an analogy for policymakers today. Then, like now, interest rates were at the zero lower bound, and the economy had suffered a deep, financial-crisis induced recession. Did the economy start to recover because

[1] FRED series INDPRO.

[2] Reinhart and Rogoff argue that the Great Depression experience fits with their story. They date the financial crisis associated with the Great Depression to 1929 and hence include the Depression itself in their calculations of growth after the crisis. One obtains quite different results if one dates the financial crisis to 1933 instead. Regardless, rapid growth beginning in 1933 poses a puzzle for the view that a financial crisis dooms an economy to a slow recovery.

Franklin Roosevelt's leadership changed inflation expectations? Or did it recover because of the direct effects of dollar devaluation on farm income? The answer has implications for contemporary macropolicy. For instance, as of this writing, Japan is attempting a monetary policy regime shift, in part modeled on Roosevelt's actions (Aso 2013). A better understanding of spring 1933 may inform economists' predictions about current Japanese policy (Romer 2013).

This is not to imply that Japan's situation is exactly analogous to that of the U.S. in spring 1933. Among many other differences, Japan is a far more open economy now than the U.S. was in 1933. Total trade (exports plus imports) were 7.4 percent of U.S. GDP in 1933,[3] whereas total trade is 29.8 percent of GDP in Japan today.[4] The small share of trade in the U.S. meant that departure from the gold standard in 1933 could not contribute significantly to growth by raising exports and reducing imports. And in fact, the trade deficit widened in 1933 (NIPA table 1.1.6).[5] By contrast, trade is a large enough share of Japanese GDP today that depreciation of the yen may well boost growth through this conventional channel. But while trade is more important for Japan now than for the U.S. in 1933, agriculture is far less important. In 1930, the farm share of the population in the U.S. was 25 percent,[6] in Japan today it is 5 percent.[7] Thus the farm income channel that is in part the focus of this paper is unlikely to be significant in Japan today.

The contribution of this paper is to provide the data needed to test whether there was a direct channel, distinct from general expectation effects, on U.S. recovery through agriculture in 1933. If, as Eggertsson (2008) argues, recovery was due to economy-wide inflation expectations with no distinct role for farmers, then the increase in consumption demand ought to have been relatively uniform across states. If, by contrast, production was spurred by demand from farmers, then consumer demand ought to have risen more rapidly in states with larger rural population shares. I exploit a previously unused dataset of monthly, state level auto sales to see whether the share of a state population's engaged in farming was correlated with auto sales recovery in spring 1933. I find that it was. A one standard deviation increase in the share of a state's population living on farms was associated with roughly a 25 percentage point increase in auto sales growth from late winter to early summer 1933. In aggregate, my results imply that the recovery, at least as measured by auto sales, might have been as much as 50 percent slower if farm demand had not grown more rapidly than demand in the economy as a whole. This suggests that recovery was not simply due to a uniform change in inflation expectations but also to some factor that differentially benefited farmers. An explanation of recovery in spring 1933 should account for this fact.

This paper relates most obviously to the literature on U.S. recovery after 1933. Most

[3]NIPA table 1.1.6

[4]FRED series JPNEXPORTQDSNAQ, JPNIMPORTQDSNAQ, JPNRGDPQDSNAQ.

[5]Eichengreen (1992, pp. 343-344) argues that this is explained by expectations of further dollar depreciation; exporters had an incentive to wait to sell goods until the dollar was weaker.

[6]Data are from the IPUMS 5% sample from the 1930 Census (Ruggles et al. 2010).

[7]`http://www.ers.usda.gov/topics/international-markets-trade/countries-regions/japan/basic-information.aspx#.UZG6-ysjo9s`.

of this work (e.g. Romer 1992, Cole and Ohanian 2004) does not focus on the economy's turn-around in March and the initial spurt of growth thereafter. Prominent exceptions are Temin and Wigmore (1990) and Eggertsson (2008). Both argue that the key to growth was a regime change. In particular, by taking the U.S. off the gold standard and explicitly voicing his desire for higher prices, both papers credit Roosevelt with inducing inflation expectations. The argument is that higher expected inflation lowered real interest rates, thus stimulating demand for investment goods and consumer durables.[8] [9] At the same time, Eggertsson and Temin and Wigmore stress that Roosevelt's actions also led to expectations of higher future output. Consumers and businesses expecting better times in the future were more willing to spend.[10]

While both Temin and Wigmore and Eggertsson emphasize the effect of higher inflation expectations, their interpretation of the source of higher expected inflation is somewhat different. Temin and Wigmore highlight Roosevelt's decision to take the U.S. off the gold standard, arguing that this was the key signal that prices would rise. Eggertsson does not disagree that departure from the gold standard was a necessary condition for recovery, but he focuses more on Roosevelt's fiscal actions. He suggests that larger deficits gave the government an incentive to inflate away nominal debt, thus making higher future inflation credible.

In keeping with their focus on the effect of dollar devaluation, Temin and Wigmore argue that a weaker dollar not only led to higher expected inflation, but also was expansionary through its effect on current and expected farm incomes. In their words (p. 495):

> "Grain and cotton prices rose as the value of the dollar fell. Farmers and the rural community looked forward to higher incomes as the higher prices for both production and inventories worked through the rural community. These people stepped up their purchases of durables accordingly, of which the most important was automobiles. This encouraged a rise in auto production, steel production, and industrial production in general."

Temin and Wigmore provide circumstantial evidence for this view. But absent monthly data on state auto sales, they are unable to show that farmers bought more durables than non-farmers in spring 1933.[11] This paper fills this gap. It confirms Temin and Wigmore's hypothesis that auto sales grew more rapidly in more rural states. However, I argue that

[8] Romer (1992) argues that a similar channel led to growth between 1933 and 1940.

[9] Eichengreen (1992) emphasizes not the rapidity of the 1933 recovery but its incompleteness. After rising 57 percent from March to July, seasonally adjusted industrial production fell 19 percent from July to November, before rising again (FRED series INDPRO). Despite this, March 1933 was a decisive break: before, the trend had been steadily downward, thereafter upward. While the unevenness of the recovery in 1933 is of obvious interest, the focus of this paper is narrower: what caused the initial turn-around and growth spurt in spring 1933?

[10] In the context of a continuous time New Keynesian model, Werning (2012) shows the importance of higher expected future output in a liquidity trap.

[11] Temin and Wigmore's principal evidence comes from a state-level regression of the level of auto sales in all of 1933 on farm income and other income in 1933. They interpret a larger coefficient on farm income

this fact is consistent with a number of explanations of the recovery, not only a farm income channel through crop prices. Firmly establishing why auto sales grew so much more rapidly in rural states is beyond the scope of this paper. But I consider the implications and evidence for two possible explanations: Temin and Wigmore's farm income channel and a channel through farm debt and rural bank health.

At one extreme, consider a channel from devaluation to income through relative price changes. In the short run, devaluation increased crop prices but had little effect on the overall price level. Thus the relative price of farm products rose. Farmers were better off. At the same time, consumers of farm products, whether urban workers or manufacturing firms, were worse off. As rural consumption rose, urban consumption ought to have declined. In order to break this symmetry, one must argue that there was some factor that made the positive effect of higher crop prices on farmers larger than the negative effect of higher crop prices on urban workers. One such factor may have been inflation expectations. I suggest that higher crop prices directly raised inflation expectations, so that the depressing effect on urban consumption from lower incomes was balanced by a stimulus to consumption from expected price increases.

While understanding the economy's behavior in spring 1933 is itself important, this chapter is also relevant to the growing literature on the role of macro policy at the zero lower bound. In the U.S. in spring 1933, as in Japan since the mid-1990s, and much of the developed world since 2008, short-term interest rates were near zero, and hence conventional monetary policy was ineffective. Economists continue to debate the extent to which unconventional monetary policy can stimulate an economy in these conditions.[12] This chapter adds to this literature by providing evidence on how policy led to rapid growth in 1933.

The next section revisits the aggregate data and documents the sectoral composition of the recovery in spring 1933. Section 3.3 uses monthly data on state auto sales to show that recovery proceeded more rapidly in agricultural states. Section 3.4 considers possible implications of these quantitative results. Section 3.5 concludes and suggests directions for future research.

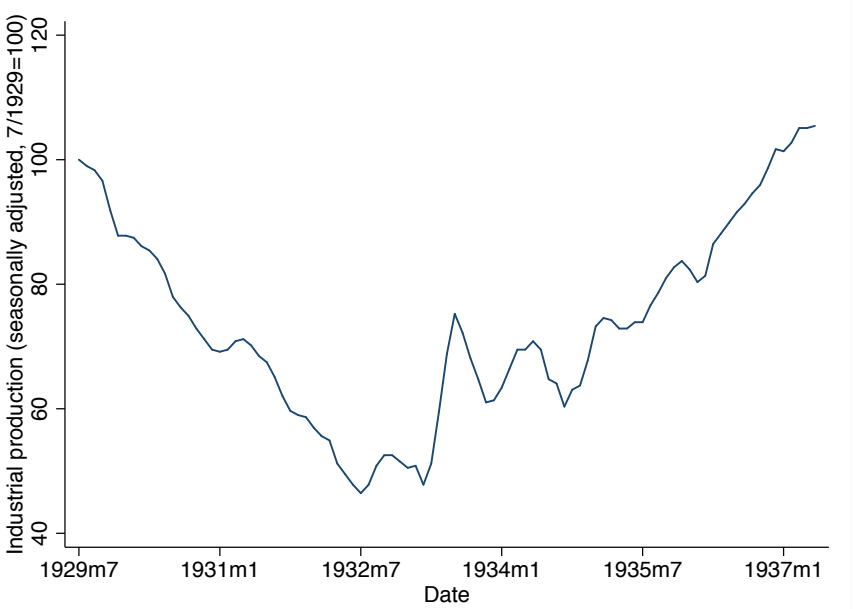
3.2 Aggregate data

Before turning to explanations of the economy's growth in the spring of 1933, it is useful to reexamine the data. Given that the period of especially rapid growth is just four months, from March to July 1933, it is natural to ask how sensitive our measurement of growth

as evidence in support of their hypothesis. While suggestive, this regression has two limitations: first, the left-hand side variable is the level of auto sales, while their hypothesis is about the *growth* of auto sales. Second, the regression uses annual data, hence it conflates auto sales in the period of interest, spring 1933, with sales later in the year.

[12]The existence of the liquidity trap (i.e. the zero lower bound on interest rates) and its implications for monetary policy was first described by Keynes (1936) and Hicks (1937). Interest was reignited after rates fell to near zero in Japan in the mid-1990s (Krugman 1998). Svensson (2003) and Woodford (2012) provide useful overviews.

Figure 3.1: Industrial Production, 1929-1937



Source: FRED series INDPRO.

is to seasonal adjustment methods or alternative measures of economic activity. The data suggest that the economy indeed grew extraordinarily rapidly in spring 1933. There is no evidence that measured industrial production growth is an artifact of data construction or measurement error.

Industrial production

Figure 3.1 shows total, seasonally adjusted industrial production from 1929 to 1940. The almost vertical increase in spring 1933 is obvious. The 57 percent growth from March to July 1933 is extraordinary both in absolute and relative terms. Other than in spring 1933, the Federal Reserve industrial production index has never risen by more than 21 percent in four months.[13] Table 3.1 shows seasonally adjusted monthly growth rates of total industrial production with its three primary subcomponents, durable manufacturing, nondurable manufacturing, and minerals. Since they are central to Temin and Wigmore's farm income channel, table 3.1 also shows the monthly growth rate of auto production.[14] All components

[13]FRED series INDPRO. The 21 percent increase occurred from September 1934 to January 1935.

[14]Auto production itself had only a 4.8 percent weight in the Federal Reserve industrial production index (Federal Reserve 1940, p. 761). However, Temin and Wigmore argue that as a large consumer of steel and other inputs, developments in the auto industry had large effects on industrial production and the economy as a whole.

of industrial production rose from March to July 1933, but the most dramatic increase was in durable manufacturing, with auto production rising even more rapidly than durable manufacturing as a whole. In the four-month period, seasonally adjusted durable manufacturing grew 138 percent and autos production 152 percent, while nondurable manufacturing rose 42 percent and mining 17 percent.

Table 3.1: Industrial production, monthly growth rates in 1933 (%)

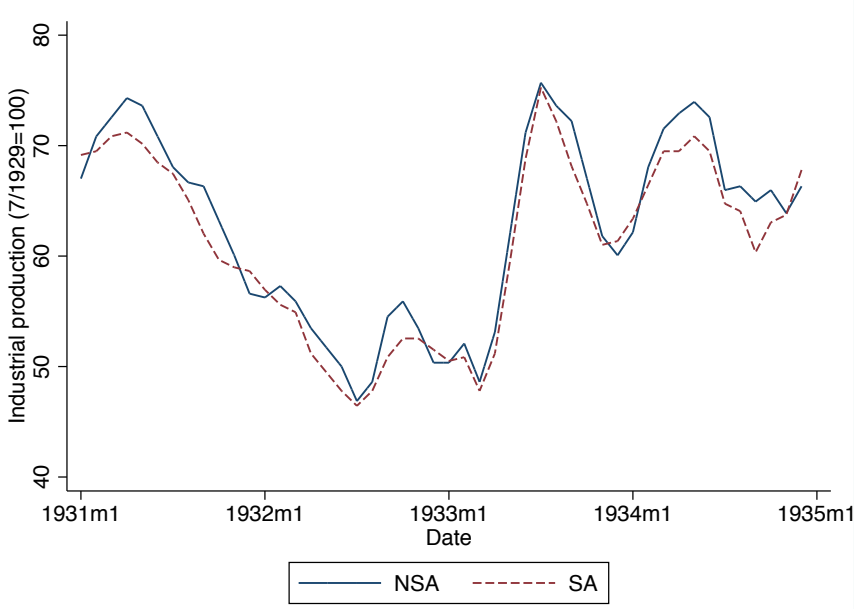
Month	Total IP	Dur. Manu.	Autos	Nondur. Manu.	Minerals
January	0.0	0.0	25.7	0.0	1.5
February	-1.7	-5.1	-25.0	-1.4	4.5
March	-5.3	-13.5	-18.2	-5.7	4.3
April	7.4	21.9	51.9	9.1	-9.7
May	17.2	25.6	19.5	13.9	15.4
June	14.7	28.6	20.4	11.0	5.3
July	10.3	20.6	15.3	3.3	6.3
August	-4.7	-3.9	5.9	-6.4	3.6
September	-6.1	-8.2	2.8	-4.5	-5.7
October	-5.2	-4.5	-2.7	-4.8	-7.3
November	-5.5	-15.6	-51.4	-2.5	1.3
December	1.4	5.6	2.9	0.0	-1.3

Note: All series were seasonally adjusted by the Federal Reserve (1940).
Source: Data are from Federal Reserve (1940).

The other striking feature of the table is the decline in industrial production after July. From July to November 1933 total industrial production fell 19 percent. This partial reversal of the spring recovery is apparent in all the major components of industrial production. Auto production also fell back, with an extraordinarily bad month in November 1933. While this chapter's focus is on the initial burst of growth in spring 1933, an explanation for this growth ought also to provide a clue as to why production fell in fall 1933.

Such rapid growth rates over a short period naturally lead to questions of data quality: should one believe that seasonally adjusted industrial production rose 57 percent in spring 1933 or might this increase be a figment of data construction problems? The data suggest the former. The first check is to consider the behavior of non-seasonally adjusted production. This is shown in figure 3.2. The rapid increase in industrial production is also present in the raw, non-seasonally adjusted data and is not a regular seasonal phenomena. Only in 1933 does one see such a dramatic increase in spring.

Figure 3.2: Non-seasonally adjusted and seasonally adjusted industrial production



Source: Federal Reserve Board, G.17 data release.

Other production indicators

A second check is to consider alternative seasonally adjusted indicators of economic activity. Figure 3.3 shows two such indicators: the Federal Reserve index of freight car loadings and nonagricultural employment (Federal Reserve, 1941). Freight car loadings measure the real quantity of shipments by rail in the period, with underlying data from the railroads themselves. The broad picture is similar to that for industrial production. After reaching a trough in March 1933, seasonally adjusted freight car loadings grew rapidly through July. In these four months, the seasonally adjusted series rose 40 percent.

It is also natural to examine the evolution of employment. Caution is necessary since the employment data are not entirely independent of the industrial production data. For some industries, the industrial production figures rely heavily on the Bureau of Labor Statistics establishment survey, which is the employment data's source (Federal Reserve, 1940, p. 761). Nonetheless, it is reassuring that, like industrial production, employment rose rapidly in spring 1933. Total, seasonally adjusted, nonagricultural employment grew from 26.7 million in March 1933 to 28.4 million in July.[15] Seventy-three percent of this employment increase was accounted for by an astonishing 20 percent increase in manufacturing employment.[16]

[15]Note that these employment data exclude relief workers. Data are from Federal Reserve (1941) p. 534.

[16]Manufacturing employment rose from 6.12 million in March to 7.36 million in July (Federal Reserve, 1941, p. 534).

Figure 3.3: Freight car loadings and employment

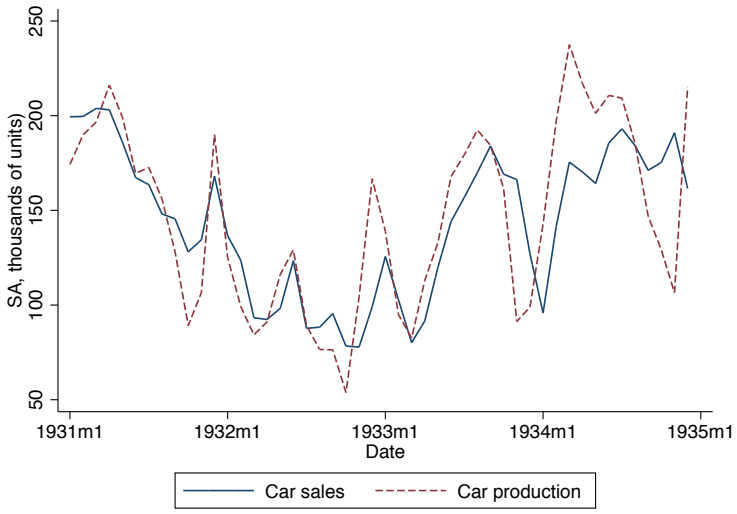
Source: Federal Reserve (1941).

Sales

Together, the data on industrial production, employment, and freight car loadings leave little doubt that output rose rapidly in spring 1933. But was the recovery of production due to contemporaneous consumer demand or to expectations of future demand? If the former, the historians' task is to explain the increase in consumption. If the latter, to explain why firms expected higher future sales. Therefore I examine the behavior of sales in spring 1933. Figure 3.4 shows seasonally adjusted passenger car sales and production from 1931 through 1934.[17] Seasonally adjusted sales behave similarly to production in spring 1933, roughly doubling from March to July. Figure 3.5 presents the analogous data for trucks. Interestingly, the recovery of truck sales is even more rapid than that of car sales in spring

[17]Sales data are from NBER macrohistory series m01109; production data are from NBER series m01107a. Neither series was seasonally adjusted by the source. I seasonally adjust the series by regressing the natural logarithm of each series on monthly dummies and monthly dummies interacted with a post 1935 dummy variable. The second set of dummies are necessary since the date of new model introduction - an important determinant of car sales and production - changed in 1935 (Cooper and Haltiwanger 1993). The sample period for the regression is January 1925 to December 1940. The series graphed in figure 3.4 is $e^{\hat{\varepsilon_t}} \cdot \frac{\bar{y}}{\bar{x}}$, where $\hat{\varepsilon_t}$ are the residuals from the regression of the natural log of sales or production on the monthly dummies, $\bar{y}$ is the mean of non-seasonally adjusted sales over the period, and $\bar{x}$ is the mean of $e^{\hat{\varepsilon_t}}$.

Figure 3.4: Seasonally adjusted car sales and production



Note: See text for details on the seasonal adjustment. Note that production data are measured in actual units produced and hence differ slightly from the index of auto production produced by the Federal Reserve as part of its industrial production index. Thus the production data graphed here are similar but not identical to the auto production figures reported in table 3.1.
Source: Sales data are from NBER macrohistory series m01109; production data are from NBER series m01107a.

1933: they rise 163 percent from March to July.[18] Unfortunately, the more rapid growth of truck sales does little to distinguish between the overall inflation expectations channel and a farm specific channel. It is consistent both with high demand for trucks from businesses and from farmers.

As with cars, the difference between truck production and sales is not obviously anomalous in spring 1933. Figures 3.4 and 3.5 suggest a roughly parallel movement in production and sales in the motor vehicles sector. Thus explanations of the recovery, at least of this important sector, must explain a rise not only in production, but also in consumer and investment demand.[19]

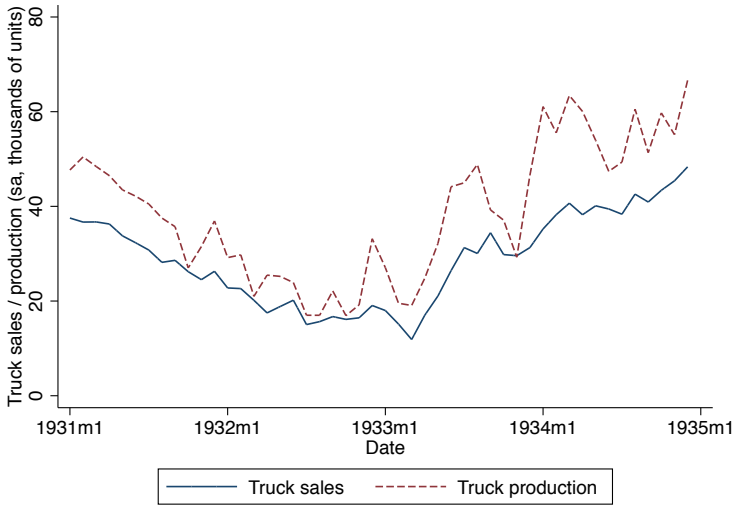
Department store sales provide another measure of consumer demand. Figure 3.6 shows the behavior of seasonally adjusted department store sales.[20] They show a rather different

[18]Sales data are from NBER macrohistory series m01146a; production data are from NBER series m01144a. The seasonal adjustment procedure is identical to that for passenger cars. See footnote above.

[19]This casts doubt on Kindleberger's (1973) statement that recovery in spring 1933 was "[b]ased on inventory accumulation rather than long-term investment" (p. 233).

[20]The seasonal adjustment procedure is identical to that for cars and trucks (see the footnote above),

Figure 3.5: Seasonally adjusted truck sales and production



Note: The seasonal adjustment procedure is identical to that for passenger cars. See text. Source: Sales data are from NBER macrohistory series m01146a; production data are from NBER series m01144a.

pattern in spring 1933 from that of auto and truck sales. After jumping in April, they decline slightly through July, before jumping again in August. Department stores sold a mix of durable goods such as furniture and nondurable goods such as clothing (Romer 1990). Thus the slower growth of department store sales relative to auto sales fits with the slower growth of nondurables production relative to durables and autos production.[21]

Prices

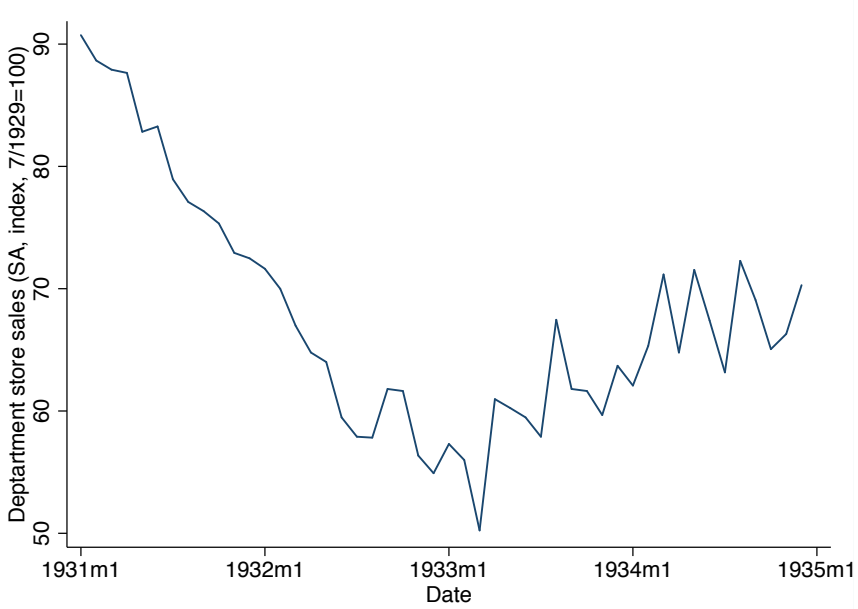
So far we have examined the behavior of quantities in 1933. But central to explanations of U.S. recovery is the behavior of actual and expected prices. Since nominal interest rates did not fall in spring 1933,[22] any explanation that stresses the importance of real interest rates, like Eggertsson (2008), must appeal to movements in expected inflation. Similarly, explanations that emphasize the direct effects of dollar devaluation must appeal to the effect

except that only one set of monthly dummies is used.

[21]Here I am following Federal Reserve convention and considering textiles a nondurable (Federal Reserve 1940, p. 761). It would no doubt be more accurate to think of textiles as a semidurable with properties between, say, autos and food. But semidurable was not and is not a Federal Reserve or NIPA classification.

[22]Safe, short-term interest rates were essentially zero. The yield on three-month treasury notes fluctuated between 0.25 and 0.5 percent in spring 1933 (NBER macrohistory series m13029b). The exception is in March 1933 when the yield rose to 2.3 percent, presumably due to a disruption related to the Banking Holiday.

Figure 3.6: Seasonally adjusted department store sales



Note: Seasonally adjusted by regressing on monthly dummies. See text.
Source: Data are from NBER macrohistory series m06002b.

of a weaker dollar on some prices. As discussed earlier, the direct trade effects of devaluation were actually in the wrong direction. Table 3.2 shows several measures of prices in 1933. In the first of half of 1933, there was little change in consumer prices (the CPI); by contrast, wholesale prices (the PPI) rose a significant 14 percent from March to July. Despite the enormous increase in auto production and sales, prices for passenger cars were almost completely stable in 1933. This reflects the stickiness of auto prices in this period.[23]

In contrast to the small or moderate movements in the CPI, PPI, and auto prices, the exchange rate moved substantially in spring 1933. After the U.S. went off the gold standard on April 19th, the dollar depreciated by 29 percent relative to the British pound over the following three months. The exchange rate vis a vis other currencies behaved similarly. This explains part of the increase in the dollar price of commodities such as wheat and cotton (the final two columns of table 3.2). Commodity prices were set in world markets; when the dollar depreciated, the dollar price of commodities rose. This is Temin and Wigmore's explanation for higher farm prices and hence incomes in spring 1933.

The depreciation of the dollar cannot, however, explain the entire increase in commodity prices. Whereas the dollar fell by 29 percent from April to July, wheat and cotton prices rose by 55 percent. Eichengreen (1992, pp. 340-341) argues that this can be explained by

[23]For more on price setting in the auto industry, see chapter 2.

Table 3.2: Prices in 1933 (NSA, Index, 1/1933 = 100)

Month	CPI	PPI	Autos	Exchange rate ($ / pounds)	Wheat	Cotton
1	100	100	100	100	100	100
2	98	98	99	102	100	98
3	98	99	99	102	111	113
4	98	99	98	107	134	111
5	98	103	98	117	156	139
6	98	107	98	123	165	155
7	102	113	98	138	207	174
8	102	114	98	134	191	155
9	102	116	99	139	181	156

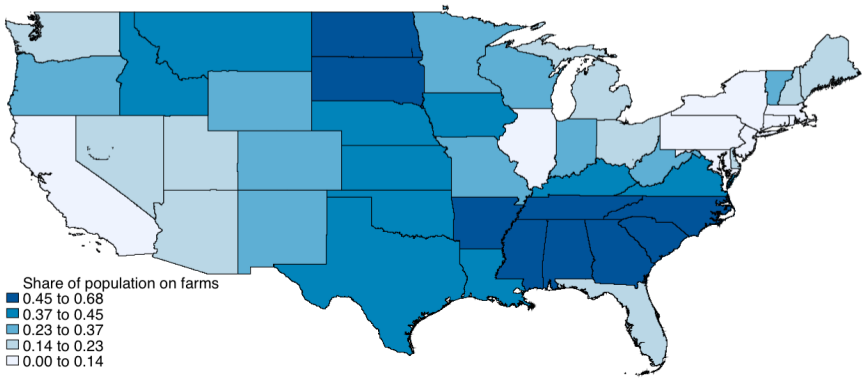
Sources: CPI: FRED series CPIAUCNS; PPI: FRED series PPIACO; autos: NBER macrohistory series m04180b (as reported in FRED); exchange rate: *Survey of Current Business*, 12/33, p. 31; wheat: NBER series m04001a (as reported in FRED); cotton: NBER series m04006a.

inflation in Britain. The price of traded agricultural products was determined in London, so the change in their dollar price was equal to the sum of the rate of dollar depreciation and the British inflation rate. With positive British inflation, crop prices rose more rapidly than the dollar depreciated. The result of large crop price increases was an improvement in the terms-of-trade for farmers: at the same time as a wheat farmer's product price nearly doubled, the price of a typical consumption basket or of a car remained roughly constant. As the U.S. Department of Agriculture (1935, p. 25) put it: "Revaluing the dollar benefited agriculture because prices of the raw-material farm products responded promptly, while prices of many of the things that farmers buy increased more slowly." Temin and Wigmore hypothesize that this led to a surge in demand for autos and other goods by farmers. The next section looks at evidence for this hypothesis.

3.3 Geographical variation of auto sales

From time series for the U.S. as a whole it is difficult to distinguish between different explanations of the recovery. Both the inflation expectations and the farm income channel are potentially consistent with rapid growth concentrated in durable goods industries. But further progress is possible by making use of geographically disaggregated data on consumer demand. If, as Eggertsson (2008) argues, recovery was due to economy-wide inflation expectations, then the increase in consumption demand ought to have been relatively uniform across states. If, by contrast, production was spurred by demand from farmers, then consumer demand ought to have risen more rapidly in states with larger rural population shares.

Figure 3.7: Farm residents per 100 people in 1930



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

In order to compare the evolution of consumer demand across different states, I collected data on monthly passenger car sales by state from 1929 through 1934 from the *Automotive Daily News Review and Reference Book* (1935).[24] Admittedly, car sales are only one part of consumption, but they are likely to be a good measure of consumption for present purposes, because they were likely a significant driver of the recovery in industrial production, and because they are likely to have been correlated with overall consumption.

If Temin and Wigmore are correct, then auto sales ought to have grown more in primarily agricultural states. Figure 3.7 shows the share of each state's population living on farms in 1930.[25] Darker shading indicates more of the population living on farms. States with large cities, such as Illinois or New York, had small shares of their population on farms. The Great Plains and the South had high shares. Figure 3.8 shows a scatter plot of the farm share of the population and the change in car sales from February-March 1933 to June-July.[26] There is a clear positive relationship, pointing to faster auto sales growth in rural areas.

An obvious concern is that this relationship is the result of different seasonal patterns or other confounding variables. Hence I estimate cross-sectional regressions of the form:

$$\%\Delta\text{Auto sales}_i = \beta_0 + \beta_1 \text{farm share}_i + \gamma' X_i + \varepsilon_i, \tag{3.1}$$

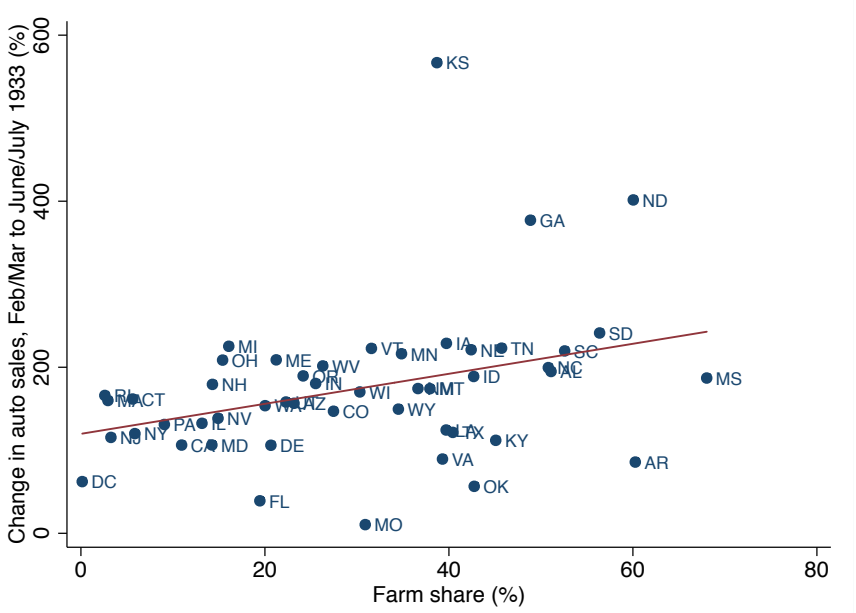
where X is a set of seasonal and other control variables. It is not obvious what measure is correct for the change in auto sales. As measured by industrial production, the economy

[24]Data are from pp. 22-23. The *Automotive Daily News Review and Reference Book* is available in the Detroit Public Library's National Automotive History Collection.

[25]Data come from the IPUMS 5 percent sample of the 1930 Census (Ruggles et al. 2010). Farm residents are those who answered yes to the question "Does this family live on a farm?"

[26]I examine the change between two-month averages since the single month values have large amounts of noise that appears more likely to be due to idiosyncratic variation than macro shocks.

Figure 3.8: Change in auto sales and farm share



Source: See text.

grew most rapidly between March and July 1933. However, seasonally adjusted car sales peaked later, in September, and car sales show substantial month-to-month variability that may obscure the macro pattern of interest. Hence I look not only at the change in car sales from March to July 1933, but also at the change from the February-March average to the June-July average, and at the change from the first quarter to the third quarter.

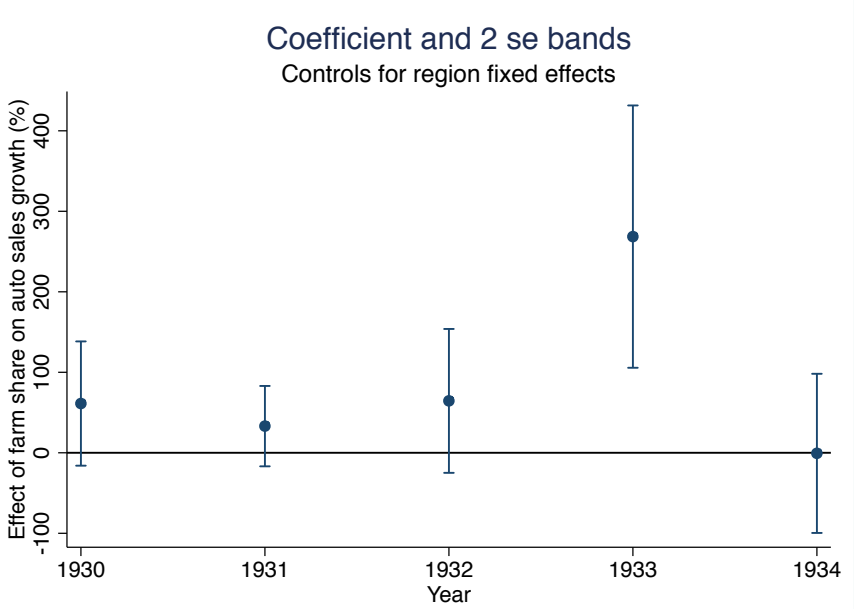
The first three columns of table 3.3 show results from a regression of the percent change in auto sales on the share of a state's population living on farms and four lags of the change in auto sales. The lags control for the normal seasonal pattern of auto sales in each state. In column 1, the change in auto sales is measured from March to July 1933, in column 2, from the February-March average level of sales to the June-July average, and in column 3, from the first to the third quarter of 1933.

Table 3.3: Auto sales growth in spring 1933

	(1) Mar. to Jul.	(2) Feb.-Mar. to Jun.-Jul.	(3) Q1 to Q3	(4) Mar. to Jul.	(5) Feb.-Mar. to Jun.-Jul.	(6) Q1 to Q3
Pop. share on farms	1.174	1.795**	1.480*	2.763**	2.686***	2.957***
	(1.137)	(0.855)	(0.751)	(1.356)	(0.815)	(0.867)
1932 Δ auto sales	0.726*	0.644*	1.755***			
	(0.422)	(0.342)	(0.475)			
1931 Δ auto sales	-0.436	-1.402**	-1.286**			
	(0.798)	(0.576)	(0.573)			
1930 Δ auto sales	1.517**	1.280	0.909			
	(0.715)	(0.980)	(0.722)			
1929 Δ auto sales	0.218	-0.171	0.207			
	(0.396)	(0.422)	(0.329)			
Midwest				0.430	0.0928	-0.217
				(0.654)	(0.393)	(0.418)
South				-0.251	-0.917**	-0.909**
				(0.662)	(0.398)	(0.423)
West				-0.104	-0.481	-0.680*
				(0.626)	(0.376)	(0.400)
Constant	1.235***	1.166***	0.954***	0.982**	1.340***	1.256***
	(0.333)	(0.301)	(0.186)	(0.459)	(0.276)	(0.293)
Observations	49	49	49	49	49	49
R^2	0.383	0.377	0.522	0.152	0.317	0.266

The max of conventional and robust standard errors are in parentheses.
* $p < .10$, ** $p < .05$, *** $p < .01$

Figure 3.9: Placebo tests



Note: The dependent variables is the percent change in auto sales from the February-March average to the June-July average.
Source: See text.

In all cases, the coefficient on the farm share of the population is large. The coefficient of 1.17 in column 1 means that moving from none of the population on farms to all the population on farms would increase the growth rate of auto sales by 117 percentage points. Or, more realistically, the coefficient means that if a state's farm share rose from 11 percent (that in California) to 51 percent (that in North Carolina), then auto sales growth would be 47 percentage points higher.[27] The standard deviation of the farm share of the population is 0.17, so these coefficients imply roughly 25 percentage points higher auto sales growth for a one standard deviation increase in the farm share of the population. While the coefficients on farm share in columns 1-3 are economically quite significant, their statistical significance is borderline. In part, this reflects the presence of outliers, particularly when auto sales growth is measured from March to July. While non-seasonally adjusted auto sales in the country as a whole grew 135 percent over this four-month period, in Kansas they grew 865 percent.

The next three columns in table 3.3 control for region fixed effects rather than lags of the dependent variable. Both the size and statistical significance of the coefficients increase in these specifications. The estimates imply an enormous aggregate effect of agriculture on

[27] $(0.51 - 0.11) \cdot 1.17 = 0.47$.

car sales in spring 1933. One way to see this is to consider what the coefficient implies for a counterfactual world in which the U.S. had zero farm share of the population in 1933. Since 25 percent of the population lived on farms,[28] a coefficient of 2.7 means that a farm channel accounted for 68 percentage points[29] of overall auto sales growth in the U.S., or roughly half of actual auto sales growth in the period. This calculation should be taken more as an illustration of the scale of the effect rather than as an exact quantitative measure, both because the coefficient is sensitive to the controls (columns 1-3 versus 4-6) and because the coefficient from a cross-state regression may be a poor measure of the aggregate effect. I return to the latter issue in the next section.

A natural question is whether auto sales always grew more rapidly in spring in states with large populations living on farms. If so, this would suggest that the preceding evidence is not in fact indicative of a specific farm channel in the economy's 1933 recovery. If farm states saw more rapid auto sales growth in years when there was no dollar devaluation or change in crop prices, then the preceding results would not be evidence about the effects of these policies in spring 1933. Figure 3.9 shows coefficients and two standard error bands from placebo tests, regressions of spring auto sales growth on farm share for each year from 1930 to 1934. The large, statistically significant effect on auto sales growth is unique to 1933. This is strong evidence that the relationship between agriculture and auto sales growth reflects something specific to the 1933 recovery rather than a general relationship between agriculture and auto sales.

3.4 Implications

The above results show that auto sales increased more in agricultural than in non-agricultural areas in spring 1933. While this fact is consistent with Temin and Wigmore's farm income hypothesis, there are other stories that could explain higher growth of rural consumption. A definitive explanation of differential urban and rural consumption growth in spring 1933 is beyond the scope of this paper. But in this section I consider two possibilities: (1) a channel through farm income and (2) a channel running from higher crop prices to lower farm debt and healthier rural banks.

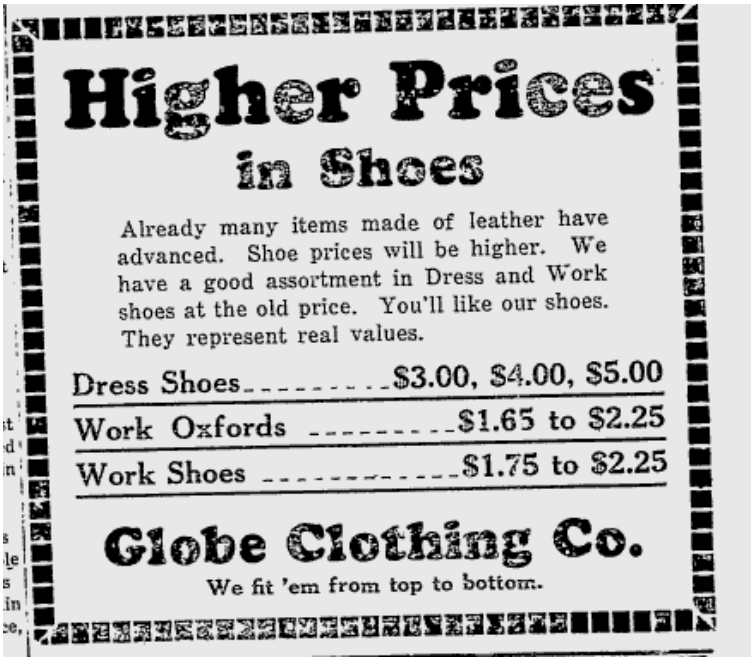
Farm income

As discussed in the introduction, Temin and Wigmore argue that dollar devaluation led to higher crop prices and thus higher current and expected income for farmers. This, in turn, led to a rise in farmers' consumption demand, particularly for autos. This hypothesis fits well with the above evidence on the geographic dispersion of car sales growth in spring 1933. Less clear is whether, as Temin and Wigmore suggest, a farm income channel can explain a significant part of the aggregate economy's recovery. The problem is that insofar

[28] IPUMS 5 percent sample of the 1930 Census (Ruggles et al. 2010).

[29] $0.25 \cdot 2.7 = 0.68$

Figure 3.10: Expected price increases for shoes



Source: *The Warsaw Union*, 5/3/1933, p. 2.

as higher farm product prices made farmers richer, they ought also to have made others poorer. If higher farm prices were passed through to higher food prices, they made urban workers poorer. If they were not passed through, they lowered the profits of food wholesalers and manufacturers. Whether through poorer urban workers or lower profits, higher farm incomes and consumption demand ought to have been matched by lower urban income and consumption demand. Thus the channel leading from farm prices to farm income could explain the much larger growth in car sales in farm states without explaining *any* of the nationwide growth in car sales in spring 1933. Sales could have risen a lot in North Dakota and fallen slightly in New York with no net aggregate effect.

How can one break this symmetry between the benefits of higher farm prices for farmers and the costs for non farmers? One plausible possibility are different marginal propensities to consume. If non farmers had lower marginal propensities to consume than farmers, a transfer of income from non farmers to farmers would have had overall expansionary effects. It seems unlikely, however, that marginal propensities to consume were sufficiently different for this to channel to have produced large aggregate effects.[30]

[30] A related hypothesis is that the larger positive effects on farm incomes were more salient than the smaller negative effects on urban incomes. Insofar as farmers noticed and responded to their higher incomes and urban workers did not notice and did not respond to their lower incomes, this could have lead higher farm product prices to have positive aggregate effects.

Figure 3.11: Expected price increases for tires

Source: *Los Angeles Times*, 5/11/1933, p. 5.

Figure 3.12: Expected price increases for fur

Note: This is an excerpt from the advertisement.
Source: *Los Angeles Times*, 6/8/1933, p. 5.

A story through which higher farm product prices could have benefited the economy as a whole is through inflation expectations. Narrative evidence suggests that higher farm product prices, while certainly lowering real urban incomes, may have raised urban consumption by creating expectations of higher future prices. For instance, figure 3.10 shows an ad that appeared in the Warsaw, Indiana newspaper on May 3, 1933. It tells consumers that "Already many items made of leather have advanced," but that this store still has "shoes at the old price." Similarly, figure 3.11 shows an ad for Firestone tires that appeared in the *Los Angeles Times* on May 11, 1933 (p. 5). It announces "Tire Prices Going Higher" so consumers should "Buy Now!" It explains that "increasing prices of rubber and cotton are sure to bring higher tire prices." These are not isolated instances. A month later, on June 8th, the *Los Angeles Times* ran an ad (figure 3.12) advising readers that "[F]ur prices are going UP" (p. 5). The ad has an inset quoting an article on increases in the price of new

furs.

While a definitive argument awaits more systematic work, these advertisements suggest that higher commodity prices may have *caused* expected inflation. Since at least Hamilton (1992), economists have known that commodity prices provide information on expected inflation. My hypothesis goes further: in spring 1933 higher commodity prices not only provide evidence of expected inflation, but, I suggest, were themselves a source of expected inflation. This points to an interpretation of expected inflation in 1933 somewhat different from that of Eggertsson (2008). Whereas Eggertsson argues that Roosevelt's words and fiscal actions were crucial since they signalled a commitment to higher inflation, this narrative evidence suggests a more mechanical possibility: As the dollar weakened, commodity prices rose. This in turn led consumers to expect broader inflation.

An expansion of Temin and Wigmore's hypothesis follows: As commodity prices advanced in spring 1933, the consumption of farmers rose along with their permanent income. At the same time, real urban incomes fell. Initially, however, the depressing effect on urban consumption from lower incomes was balanced by higher consumption spending in advance of expected price increases. This explanation has the virtue of explaining both the very rapid recovery in spring 1933 and the subsequent pause in recovery in summer and fall 1933. If consumers brought forward purchases in expectation of higher prices, this would explain why recovery faltered after proceeding rapidly. Expectations of price increases would have meant more shoes, tires and fur coats sold in spring 1933 and fewer sold in summer and fall. That intertemporal substitution in response to expected price increases could have significant macro effects is made more plausible by the evidence in the previous chapter. There I showed that expectations of higher car prices contributed to the severity of the 1937-38 recession by shifting car purchases forward to 1937 from 1938.

Farm debt and bank failures

A complementary explanation of the farm consumption boom in spring 1933 is the effect of higher farm prices on farm debt burdens and rural banks. Although Temin and Wigmore do not consider this hypothesis, it is a plausible channel through which higher farm product prices spurred overall recovery.

An astonishing 45 percent of farm owners at the beginning of 1933 were delinquent on mortgage debt (Bernanke 1983). This strained rural banks. The effect was to restrict credit more in agricultural areas than elsewhere during the Depression (USDA 1935). Higher farm product prices promised to allow farmers to service this debt, thus improving the prospects of rural banks. As *Business Week* put it in their first issue after Roosevelt abandoned the gold standard (4/26/33, cover page), "70-cent wheat makes a lot of sick banks well again."[31]

If, as seems likely, the state of rural banks meant that higher farm product prices improved rural bank health more than they hurt urban bank health, then this is a channel through

[31]Wheat prices averaged 52 cents a bushel in March 1933, 63 cents a bushel in April, and 73 cents a bushel in May (NBER macrohistory series m04001a).

which improved agricultural terms-of-trade may have helped the economy overall. To obtain some evidence on this channel I estimate the following regression:

$$\begin{aligned}\%\Delta\text{Auto sales}_i = \beta_0 + \beta_1 &\text{farm share}_i + \beta_2 \text{bank suspensions}_i \\ &+ \beta_3 \text{farm share}_i \cdot \text{bank suspensions}_i + \gamma' X_i + \varepsilon_i, \end{aligned} \tag{3.2}$$

where bank suspensions is a measure of bank distress in state i and X_i is a vector of control variables. I measure bank distress as the total number of commercial bank suspensions from 1930 to 1932 in a state as a share of the number of commercial banks in a state in 1929. Data are from the Federal Reserve's 1943 *Banking and Monetary Statistics*.[32] β_3 is the coefficient of interest. It measures whether the effect of farm share on auto sales growth was larger in states with more banking problems. A positive coefficient would suggest that improving bank conditions in rural states was part of the explanation for the spring 1933 growth of car sales in those states.

Table 3.4 shows results for six specifications. The first two columns use the change in car sales from February-March to June-July as the dependent variable and include lags of the dependent variable to control for seasonality. Columns three and four instead control for region fixed effects. In all four specifications the coefficient of interest on the interaction term is measured imprecisely. The econometric problem is multicollinearity: there are only 49 observations and the interaction term has a correlation of over 0.8 with both bank distress and farm share. At the cost of a more difficult to interpret coefficient, the final two columns avoid the multicollinearity problem by dropping the non-interaction terms from the regression. Interestingly, the interaction of farm share and bank suspensions alone has less explanatory power than farm share alone. The R^2 in columns 5 and 6 is 0.20 and 0.12 versus 0.32 and 0.27 when the same specifications are run with farm share alone (see columns 5 and 6 of table 3.3).

Large standard errors make precise conclusions difficult. But these results provide at least weak evidence against the view that improving rural bank health was the mechanism behind rapid auto sales growth in agricultural areas. Thus a channel through farm income may have been more important.

[32]Data on the number of banks in 1929 come from table 8, pp. 24-32; data on the number of bank suspensions from 1930 to 1932 come from table 67, p. 284.

Table 3.4: Bank distress and auto sales growth in spring 1933

	(1) Feb.-Mar. to Jun.-Jul.	(2) Q1 to Q3	(3) Feb.-Mar. to Jun.-Jul.	(4) Q1 to Q3	(5) Feb.-Mar. to Jun.-Jul.	(6) Q1 to Q3
Pop. share on farms	2.527	1.721	3.675**	4.424**		
	(1.834)	(1.478)	(1.688)	(1.779)		
Bank distress	1.365	-0.0921	0.384	0.523		
	(1.670)	(1.534)	(2.348)	(2.475)		
Interaction	-3.522	-0.635	-3.287	-4.841	3.386*	3.275
	(5.295)	(4.408)	(5.865)	(6.182)	(1.990)	(2.145)
1932 Δ auto sales	0.630*	1.760***				
	(0.354)	(0.464)				
1931 Δ auto sales	-1.342**	-1.257**				
	(0.536)	(0.525)				
1930 Δ auto sales	1.334	0.892				
	(1.012)	(0.767)				
1929 Δ auto sales	-0.175	0.191				
	(0.427)	(0.344)				
Midwest			0.0664	-0.250	0.429	0.191
			(0.482)	(0.508)	(0.408)	(0.440)
South			-0.943**	-0.944*	-0.478	-0.382
			(0.449)	(0.473)	(0.398)	(0.429)
West			-0.545	-0.769	-0.203	-0.353
			(0.474)	(0.500)	(0.392)	(0.423)
Constant	0.909**	0.949**	1.225***	1.088**	1.611***	1.557***
	(0.396)	(0.355)	(0.387)	(0.408)	(0.283)	(0.305)
Observations	49	49	49	49	49	49
R^2	0.385	0.523	0.329	0.293	0.201	0.118

Max of robust and conventional standard errors in parentheses
* $p < .10$, ** $p < .05$, *** $p < .01$

3.5 Conclusion

This chapter provides evidence on the sources of U.S. recovery in spring 1933. One view is that higher expected inflation drove the recovery (Eggertsson 2008). Another is that devaluation raised farm incomes, stimulating demand for autos and other goods in rural areas. This paper provides the high-frequency geographically disaggregated data necessary to test whether auto sales in fact rose more rapidly in mostly agricultural states. They did. A one standard deviation increase in the share of a state's population living on farms was associated with roughly a 25 percentage point increase in auto sales growth from late winter to early summer 1933.

What explains the disproportionately rapid recovery in rural areas? Temin and Wigmore argue that devaluation, through its effect on the relative price of farm products, raised agricultural income. While this argument could explain the geographic pattern of auto sales, it is unlikely to explain overall recovery. The positive effect on farm income of higher crop prices ought to have been balanced by a negative effect of higher crop prices on urban incomes.

I considered two possible ways in which this symmetry may have been broken, in which the positive effect on farmers may have been larger than the negative effect on urban workers. First, I argued that higher crop prices were not only an indicator of expected inflation (Hamilton 1992), but may also have been a cause of expected inflation. Insofar as higher crop prices raised expected inflation throughout the economy, this relative price change could have increased consumption in urban as well as rural areas.

Second, I considered whether higher crop prices had particularly large effects in rural areas by eroding farm debt burdens and thus improving rural bank health. I find no evidence for this channel. Agricultural states with more bank suspensions during the Depression recovered no more quickly in spring 1933.

Conclusion

This dissertation is a work of economic history. It contributes to our understanding of rapid recovery and renewed recession during the New Deal. But the lessons are not only historical. As of this writing, in 2013, the developed world is mired in a period of high unemployment that has no parallel since the Great Depression. One cannot think about the macroeconomics of the 1930s without considering lessons for economists and policymakers today. Conversely, policymakers have almost unavoidably sought analogies for their actions in the events of the 1930s (Eichengreen 2012).

For fiscal policy, analogies have been lacking. The conventional wisdom is that little fiscal policy was undertaken in the 1930s and hence that little could be learned about fiscal policy from the 1930s. In the first chapter I showed that there was in fact a large fiscal policy program in 1936. In June 1936, the U.S. government paid World War I veterans over 2 percent of GDP. As a share of GDP, this is as large as the 2009 American Recovery and Reinvestment Act (the Obama stimulus). Since the 1936 payment went to World War I veterans, the typical recipient received a large payment; the average amount was $550 dollars, slightly more than contemporary per capita income.

I look at four sources of evidence on the bonus's effects: a household consumption survey, cross-state and cross-city regressions, an American Legion survey of veterans' spending plans, and the aggregate time series. These sources paint a consistent picture in which veterans spent the majority of the bonus. From the household consumption survey, I estimate a marginal propensity to consume of 0.7. Combined with a reasonable estimate of the spending multiplier, this implies that the bonus added 2.5 to 3 percentage points to 1936 GDP growth.

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. Future work ought to test this hypothesis.

Fortunately, the double-dip recession of 1937-38 has not (yet) been a relevant analogy for the U.S. Unfortunately, the same cannot be said for Europe. What can economists and policy-makers learn from this interruption to recovery in the 1930s? The conventional wisdom is that contractionary macro policy, particularly monetary policy, caused the recession. Economists often use the specter of 1937-38 to argue against austerity.[33]

In the first part of chapter two, I showed that macropolicy cannot account for timing, sectoral, and geographic features of the recession. I argue that an exogenous supply shock in the auto industry explains these anomalies as well as the recession's extraordinary severity. The famous American Keynesian Alvin Hansen concludes his book on the 1937-38 recession (1938, p. 274): "Peculiarly, in the year 1938 a conjuncture of circumstances, partly accidental, combined to produce the recession." Hansen was not writing of the auto shock, but understanding this shock confirms his point. No successful explanation of 1937-38 is likely to be monocausal. The coincidence of monetary and fiscal contraction with a large output and price decline points to the primary importance of aggregate demand. But an understanding of exogenous developments in the auto industry is needed to explain the recession's timing, geographic and sectoral incidence, and severity. Policymakers today are wise to focus on the dangers of contractionary policy, but they should not ignore industry-specific developments. Likewise, economists would do well to study how supply and demand shocks can interact to cause macroeconomic volatility.

Spring 1933 provides a more optimistic reference for policy. It suggests that even when nominal rates are at zero, the right policy can cause rapid growth. With only aggregate data, however, it has been difficult to disentangle what policy was responsible for the economy's remarkable turn-around. This chapter makes progress by exploiting monthly state-level auto sales data. If, as Eggertsson (2008) argues, the primary shock was an increase in inflation expectations with no distinct link to agriculture, then one might expect a relatively uniform recovery of consumer demand across states. If, by contrast, the key shock was the effect of dollar devaluation on farm product prices, as Temin and Wigmore (1990) argue, then one would expect to see a more rapid recovery in agricultural states. In fact, the data suggest the latter. A one-standard deviation increase in the share of the population living on farms was associated with roughly a 25 percentage point increase in the growth rate of auto sales in a state. It would be useful for future work to explain why farm states recovered so much more quickly. I hypothesize that higher farm product prices benefited rural areas through the farm income channel identified by Temin and Wigmore and also benefited the entire economy through their effect on inflation expectations.

This dissertation is unified not only by themes but also by methodology. Throughout, I

[33]See, for instance, Christina Romer's June 2009 column in *The Economist* entitled "The Lessons of 1937."

use disaggregated data to shed light on macro questions. I examine the micro implications of macro hypotheses: if the veterans' bonus was effective, households with veterans ought to have spent more; if an auto shock contributed to the 1937-38 recession, employment ought to have fallen most in Michigan; and if higher crop prices contributed to recovery in spring 1933, recovery ought to have proceeded most rapidly in agricultural states. Of course, such micro tests are no panacea. The challenge is often to interpret what the micro results say about the macro question. What does a household level marginal propensity to consume reveal about the aggregate effect of the veterans' bonus? What might the geographical dispersion of auto sales say about aggregate recovery in spring 1933? The mapping from the micro data to such macro questions often requires further assumptions. But while inevitably subject to debate, this approach has great promise for future research.

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