The Effects of Fiscal Stimulus: Evidence from the 2009 'Cash for Clunkers' Program

Atif Mian
University of California, Berkeley and NBER

Amir Sufi
University of Chicago Booth School of Business and NBER

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ABSTRACT

A key rationale for fiscal stimulus is to boost consumption when aggregate demand is perceived to be inefficiently low. We examine the ability of the government to increase consumption by evaluating the impact of the 2009 "Cash for Clunkers" program on short and medium run auto purchases. Our empirical strategy exploits variation across U.S. cities in ex-ante exposure to the program as measured by the number of "clunkers" in the city as of the summer of 2008. We find that the program induced the purchase of an additional 360,000 cars in July and August of 2009. However, almost all of the additional purchases under the program were pulled forward from the very near future; the effect of the program on auto purchases is almost completely reversed by as early as March 2010 – only seven months after the program ended. The effect of the program on auto purchases was significantly more short-lived than previously suggested. We also find no evidence of an effect on employment, house prices, or household default rates in cities with higher exposure to the program.

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One of the most vociferous and consequential debates in economics is whether governments can spur economic activity through fiscal interventions. The issue has received heightened attention during the most recent recession as "Keynesians" call for large and sudden fiscal interventions in the economy. However, other economists are diametrically opposed, arguing that fiscal stimulus is at best irrelevant and at worst harmful.

A key point of contention is the speed with which private sector adjustment nullifies fiscal interventions. Proponents argue that fiscal stimulus can bring forward aggregate demand from "well in the future" - when the economy will be operating at close to capacity – to today when there is substantial slack and hence higher marginal benefit of production.[1] Opponents contend that the private sector offsets the effects of fiscal interventions almost immediately, negating any benefits while worsening the nation's fiscal situation.

We address this issue by analyzing the 2009 Cars Allowance Rebate System (CARS) program, commonly referred to as "Cash for Clunkers". The program consisted of government payments to car dealers of $3,500 to $4,500 for every older less fuel efficient vehicle traded in by consumers that purchased a newer more fuel efficient vehicle. While the program was national in scope, its impact on a given city depended on the number of qualifying "clunkers". Our research design forms treatment and control groups utilizing cross-sectional variation across U.S. cities in exposure to the CARS program. We employ this research design to assess both the initial impact of the program *and* the degree to which the program shifted purchases forward from the very near future.

Why study CARS? First, the CARS program is representative of a large number of fiscal stimulus programs. Similar programs were implemented in France, Italy, and Spain (Adda and Cooper (2000)). CARS also shares features with the first-time homebuyer credit passed as part of

[1]See for example Romer (2010).

the stimulus program, which is estimated to cost $15 billion, and the “cash for caulkers” program currently being debated in Congress, which would cost $6 billion.

Second, the effect of government spending is likely to vary across the economic cycle. In particular, Auerbach and Gorodnichenko (2010) argue that the fiscal multiplier is likely to be different in times of economic weakness (see also Christiano, et al (2009) and Woodford (2010)). The debate on fiscal stimulus is most relevant for programs that are implemented in the midst of recessions, and CARS is one such program.

Perhaps most importantly, the heated debates surrounding the program are representative of broader disagreement on the effectiveness of fiscal stimulus. Leading economists strongly supported CARS both before and after its implementation. Alan Blinder introduced the basic idea in the summer of 2008, calling it “the best stimulus idea you’ve never heard of” (Blinder (2008)). Christina Romer and Christopher Carroll considered CARS a success in April 2010 because it stimulated spending by “thrifty people” which “is nearly the best possible countercyclical fiscal policy in an economy suffering from temporarily low aggregate demand” (Romer and Carroll (2010)).

The praise for CARS is not universal however. Gary Becker concluded in August 2009 that “there is little to be said at any level in defense of a cash-for-clunkers program” (Becker (2010)). The Wall Street Journal Editorial Board called the program “one of Washington’s all-time dumb ideas” (WSJ (2009)). A systematic look at the data is warranted in order to resolve this important debate.

We examine the effectiveness of the program in boosting auto sales using a city-level data set that includes information on important outcomes, such as the types of cars registered in the city as of the summer 2008, monthly auto purchases, and employment.[2] We use this data set to test whether cities with more clunkers, and therefore greater exposure to the CARS program, exhibit differential patterns in auto purchases and other economic outcomes.

We find that the CARS program induced a large increase in automobile purchases during the two months of the program. Our preferred measure of ex ante exposure is the number of clunkers in a city as of 2008 scaled by monthly automobile purchases in 2004 (the first year of our data). We find that a one standard deviation increase in ex ante exposure lead to a 2/3 standard deviation increase in auto purchases in July and August of 2009. There is no evidence of a differential pre-trend in high and low clunker cities, and the increase corresponds exactly with the timing of the program.

Under the identifying assumption that cities with very low numbers of clunkers were unaffected by the program, our estimate implies that approximately 360,000 cars were purchased under the program during July and August 2009 that would otherwise not have been purchased.

However, we also find that most vehicle purchases induced by the program were borrowed from purchases that would have otherwise occurred in the very near future. In the subsequent ten months after the program (September 2009 through June 2010), high clunker cities purchased significantly fewer automobiles than low clunker cities. By the end of March 2010, seven months after the program, the cumulative purchases of high and low clunker cities from July 2009 to March 2010 were almost the same. In other words, the relative impact of the program on high clunker cities was almost completely reversed in just seven months.

There are a number of challenges in estimating the incremental effect of the CARS program using cross-sectional variation across U.S. cities. For example, U.S. cities with a large

[2] Throughout, the term "cities" refers to core based statistical areas (CBSAs), which are defined by the Office of Management and Budget and the Census. For example, Chicago-Naperville-Joliet, IL is a CBSA that includes Cook, DeKalb, DuPage, Grundy, Kane, Kendall, McHenry, and Will counties.

number of clunkers differ on characteristics that could be responsible for the results we find. In particular, high CARS exposure cities have a more rural composition, higher unemployment, lower house prices, and less exposure to the recession's household defaults and house price declines. However, our results are robust to the inclusion of controls for these differences and other covariates. Further, the precise pattern we witness—a sharp rise in auto purchases during the program with subsequently lower purchases afterward—is difficult to reconcile with an explanation that is unrelated to the CARS program.

Statistical inference is also confounded by the fact that shocks to auto purchases for different cities in the same month are likely not independent. We handle the inference problem in a number of ways. One test is to estimate the full set of placebo tests using every other month in our sample from January 2004 to July 2008 as a starting point. We construct the full set of realizations of the placebo tests and find that the initial response to the CARS program in high CARS exposure cities in July and August of 2009 is much larger than any other realization during our sample period. Further, the reversal from September 2009 to June 2010 is the sharpest drop in high CARS exposure cities seen in any of the placebo tests.

We also utilize city-level quarterly data on house prices, household defaults, and employment to test whether the CARS program had a measurable effect on other economic outcomes. Cities with high CARS exposure show no noticeable difference in economic outcomes from before the program to after the program relative to cities with low CARS exposure. We also examine economic outcomes for cities that have a high number of employees working in the auto industry. There is some evidence that high auto employment share cities had a relative increase in employment after the CARS program, but there is no noticeable effect on either house prices or household defaults. We should caution however that the effect of CARS on employment in the automobile industry is difficult to separate from the federal bailouts of General Motors and Chrysler in early 2009.

While there are important advantages of our microeconomic empirical approach, there are some important caveats that we discuss in the last section. One we want to mention at the outset is the difficulty in measuring the aggregate effect of the program on the entire economy. Given our reliance on low CARS exposure cities as a control group, if the CARS program had an aggregate level effect on the entire economy, our empirical strategy would be unable to detect it. However, any argument that the CARS program had such a level positive level effect must be consistent with (a) the sharp relative reduction in auto purchases we find in high CARS exposure cities after the program, and (b) the lack of any discernable relative impact on employment, house prices, or household defaults in high versus low CARS exposure cities.

We are not the first to use variation at the micro-level to estimate the effect of government stimulus programs. Using random variation in the timing of stimulus payments, Johnson, Parker, and Souleles (2006) find a strong effect of the 2001 tax cuts on non-durable consumption, especially for balance sheet constrained individuals (see also Agarwal, Liu, and Souleles (2009)). Parker, Souleles, Johnson, and McClelland (2009) find a similar effect on consumption from the 2008 tax rebates. A recent study by Wilson (2010) uses across-state variation in fiscal stimulus outlays to estimate the jobs multiplier.

Relative to the previous literature, we view our results on the swift reversal of the CARS program as the main contribution of our analysis. Our estimates imply a much faster reversal of purchases than has been found in the literature. For example, the Council of Economic Advisors (2009) argues that reversal from CARS would take five years. The National Highway

Transportation Safety Administration (2009) estimates a period of reversal of three years.[3] In contrast to our findings for CARS, Parker, Souleles, Johnson, and McClelland (2009) find no evidence of reversal in the three months after the economic stimulus payments of 2008. Given the very swift program reversal associated with CARS, our results suggest that the net impact of the stimulus program – a program that cost $2.85 billion - is far smaller than initial gains in auto purchases suggest.

The inter-temporal "crowding out" effect that we document in this paper is consistent with models that incorporate the Ricardian equivalence hypothesis, such as Barro (1974, 1979). However, we want to caution against an over-interpretation of our findings. The ultimate impact of fiscal stimulus also depends on the *form* of the stimulus package. Our paper tests for a particular form of intervention – namely a monetary incentive for durable goods purchases. It is conceivable that there may be alternative forms of fiscal stimulus that have longer-lasting effects than CARS.

The rest of our study proceeds as follows. In Section 1 we describe the program in more detail and discuss the extant empirical evidence on its success. In Section 2 we present the data. In Section 3 we present the empirical methodology. Sections 4 and 5 present the results and Section 6 concludes.

1. Car Allowance Rebate System (CARS): Description and Extant Research

As the Council of Economic Advisors (2009) notes, "The Car Allowance Rebate System (CARS) is one of several stimulus programs whose purpose is to shift expenditures by households, businesses, and governments from the future to the present." The legislation approving the program was signed into law by President Obama on June 24th, 2009. While the

[3] We discuss these studies in more detail in Section 1 and the appendix.

earliest mention of the program was in an opinion piece in the summer of 2008 (Blinder (2008)), an examination of Google Trends suggests no substantial public interest in the program until June 2009. Indeed, the volume of trade-ins at the beginning of the program came as a surprise to government officials (NHTSA (2009)).

The actual program began on July 24th, 2009 and ended on August 24th, 2009. The program, which was administered by the National Highway Traffic Safety Administration (NHTSA) under the Department of Transportation, provided car dealers with a $3,500 to $4,500 credit for every trade-in of a less fuel efficient vehicle for a more fuel efficient vehicle. The clunker qualification rules depended on two main characteristics of a vehicle: its miles per gallon and its estimated trade-in value.[4] The specific amount of the credit depended on the fuel efficiency improvement of the transaction. Using the eligibility rules set forth by the NHTSA, the company Edmunds.com published a list of all vehicles that qualified for the program.[5]

The number of total purchases under the program was 677,842, with the average voucher being $4,209. The total expenditures under the program were $2.85 billion (NHTSA (2009)). Figure 1 shows aggregate auto purchases for the economy going back to 2004. The data are from R.L. Polk, which we describe in further detail in the next section. As Figure 1 demonstrates, there is a noticeable spike in auto purchases during the program. There is also a noticeable decline afterwards.

What was the effect of the CARS program on auto purchases? The fundamental empirical challenge to answering this question is that the counter-factual outcomes in the absence of the program are unobserved. In order to evaluate the program, a research design must form a reasonable estimate of the pattern in auto purchases and other economic outcomes if the program

[4] For the specific set of eligibility rules, see Table 1 of the CARS report to Congress by the NHTSA (2009).
[5] The list can be found here: http://www.edmunds.com/cash-for-clunkers/eligible-vehicles.html

had not been implemented. This involves not only purchases during the program, but also program reversal given that some of the purchases were likely "pulled forward" from the future.

Previous research on this question estimates counter-factual outcomes using a combination of aggregate sales patterns and consumer surveys (CEA (2009), NHTSA (2009)). We provide a detailed discussion of these approaches and their drawbacks in the appendix. To summarize here, it is extremely difficult to estimate counter-factual outcomes using aggregate data. As Figure 1 shows, auto sales patterns were volatile in the months preceding the program. Further, there is some evidence of a rebound in sales in the spring of 2009 which may have continued through the summer even in the absence of CARS.

Finally, many spikes in auto purchases are due to incentive programs by car dealers and manufacturers, and it is therefore impossible to assess the impact of CARS using aggregate data alone. For example, the increase in auto purchases in March 2010 was due to Toyota's offers of subsidized leases and interest-free financing which were mimicked by other manufacturers.[6] Would this have occurred in the absence of CARS? It is almost impossible to address such questions using aggregate data.

Our approach is to form counter-factual outcomes based on cross-sectional variation across U.S. cities in their exposure to the CARS program.[7] The advantages of this approach are clear. By forming a control group that we can follow over time, we can more realistically assess the counter-factual outcome for cities with a large number of clunkers. The control group is allowed to respond to aggregate shocks such as the March 2010 incentives.

Figure 2 shows that there was a large degree of variation across U.S. states in purchases under the CARS program. Each state is shaded by the ratio of total purchases under CARS

[6] See the NewYork Times story at: http://www.nytimes.com/2010/04/02/business/02auto.html
[7] We describe this methodology in detail in Section 3.

scaled by 2004 auto purchases, with darker shades being a higher ratio.[8] Purchases under the program were highest in the upper plains states and the northwest and extreme northeast. They were lowest in the south. It is such cross-sectional variation in CARS take-up that we use to assess the impact of the program.

2. Data and Summary Statistics

A. Data

Our data set includes 957 U.S. metropolitan or micropolitan statistical areas (CBSAs) for which we have a number of economic outcomes available from January 2004 through June 2010. Throughout the text, we refer to these CBSAs as "cities." These 957 U.S. cities include 92% of the U.S. population and 96% of total auto sales as of 2004.

Data on monthly purchases of new cars and new light trucks are from R.L. Polk, a company specializing in automotive intelligence. R.L. Polk collects the data from new vehicle registrations at the county level. The data are available from 2004 onwards. The R.L. Polk aggregate data closely tracks the aggregate Census retail sales data on new motor vehicles: a regression of the aggregate monthly Polk data on the aggregate monthly Census data from January 2004 to June 2010 has an R^2 of 0.92.

In order to measure exposure to the CARS program, we need to estimate the number of clunkers in a given city as of the time of implementation. For this purpose, we obtain a data set from R.L. Polk on cars and light trucks registered in every county as of July 2008. All registered vehicles are broken down by model year and by broad vehicle type. There are 26 broad vehicle categories and R.L. Polk provides a set of vehicle makes that are included in each category. For

[8] The number of cars purchased under CARS is available at the state level, but not at a more disaggregated level. See NHTSA (2009).

example, one category is Sport Utility Light Duty Trucks, which includes (among other models) all years of the Ford Explorer, the Jeep Grand Cherokee, and the Acura MDX. For each county, we have a data set that gives the number of registered cars by vehicle category and model year as of 2008.

Our list of qualifying clunkers comes from Edmunds.com.[9] They use qualification rules based on miles per gallon and trade-in value to form a list of all model-years that qualify for the program. We match this list to the registered vehicles data from R.L. Polk. Given that the R.L. Polk data includes only the number of vehicles by year-broad category, the match is not perfect. For most broad R.L. Polk categories, there are models within the category that are considered clunkers and some that are not considered clunkers. Our main approach is to count all vehicles in an R.L. Polk category-year as a clunker if any of the model-years that are under the broad category-year are part of the Edmunds.com list.[10]

There is clearly measurement error given that the R.L. Polk data on registered cars is not available at the model-year level. However, the measurement error is likely to be random and is therefore more likely to bias the results toward finding no initial impact of the program. As we show below, our measure of the amount of clunkers in a given state strongly predicts actual purchases under CARS at the state level, and we find a strong initial effect of the program on auto purchases at the city-level.

We also use data from other sources. City-level data on demographics, median household income, and median house prices come from the 2000 Decennial Census. City-quarterly

[9] See: http://www.edmunds.com/cash-for-clunkers/eligible-vehicles.html
[10] This approach will over-estimate the number of clunkers if some of the other model-years in a broad category do not qualify for the clunkers program. An alternative approach is to scale each category by the number of total model-years within the category that qualify for clunkers. However, this approach is likely to underestimate the number of clunkers. We choose the former approach because the top 10 trade-ins included a disproportionate amount of sport utility vehicles and the former approach does a better job of reflecting the importance of SUVs. All results are qualitatively similar using the latter approach. See the Appendix for further information.

information on household default rates, credit card utilization rates, and debt levels are from Equifax Predictive Services (see appendix of Mian and Sufi (2009)). City-quarterly information on house prices is from FHFA, city-quarterly data on unemployment is from the Bureau of Labor Statistics, and city-annual data on household income is from the IRS. Many of these data sets are available at the zip code level. We use data from the company Zip-codes.com to match zip codes to counties and CBSAs.

Finally, we choose to conduct our analysis at the CBSA-level because CBSA is defined as the naturally integrated economic unit. For example, in the Chicago-Naperville-Joliet, IL CBSA, many individuals living in Cook County may choose to purchase cars in DuPage County.

B. Summary Statistics

Table 1 presents summary statistics for the 957 cities in our sample. The first four variables measure exposure to the CARS program. The average number of clunkers in a city scaled by 2004 auto purchases in the cities is almost 10. The standard deviation is 3.4 which implies a substantial degree of variation in exposure to the program across cities. The ratio of clunkers to households is about 1 and the ratio of clunkers to automobiles on the road is about 0.5. These may appear high in part because of our classification method (see footnote 10 and the Appendix). If we use an alternative method of counting as clunkers only the fraction of total models that are listed as a clunker in each R.L. Polk category, the number of clunkers per automobile is 0.17. All results are qualitatively similar using the latter ratio, which is likely to underestimate the number of clunkers on the road.[11]

Table 1 also summarizes data on other characteristics and monthly auto purchases. The data show the difficulties facing the country in June 2009. The unemployment rate is on average

[11] A city-level regression of clunkers under the first definition scaled by 2004 auto purchases on clunkers under the second definition scaled by 2004 auto purchases yields and R^2 of 0.92.

10% across cities, and the household default rate is 8%. House price growth from June 2006 to June 2009 is -3% on average, but there are much sharper declines at the low end of the distribution. Average monthly auto purchases at the city level from 2004 to 2009 is 1,200, with the median being 218. This reflects the existence of several very large cities that skew the distribution of both population and auto purchases.

3. Empirical Methodology

A. Research Design

Our empirical strategy exploits across-city variation in exposure to CARS to assess the broader economic impact of the program. The thought experiment is as follows. Suppose there are two cities, one in which everyone owns a clunker and one in which no one owns a clunker. The experiment uses the non-clunker city as a control group to assess the counter-factual level of auto purchases in the absence of the program for the clunker city. This allows for an estimate of the marginal impact of the CARS program.[12]

In order to implement this research design, we must first make a decision on the precise measure of exposure to CARS. Conceptually, we would like to measure which cities contain clunker owners that are most likely to be induced to make a purchase under the program. This is not straightforward. Two cities with the same number of clunkers may have different propensities to take up the program. For example, cities may vary in their propensities to buy new cars. Cities may also vary in the number of individuals that own cars. Further, some clunker owners may be unable to afford a high fuel efficiency vehicle even with the tax credit. Finally,

[12] In theory, the control group may react to the program by reducing auto purchases given the expectation of future taxes to make up for the subsidy to the high CARS cities. However, such a reaction is likely to be small. The size of CARS is small relative to total fiscal stimulus spending and households would reduce consumption of all goods in response, not just autos.

clunker owners may buy a new car on behalf of their extended family or friends and such propensity may vary across cities.

Our measure of CARS exposure is dictated by state-level data on actual purchases under CARS (NHTSA (2009)). In other words, we constrain ourselves to use the measure of exposure that best predicts state level purchases under the program. More specifically, we estimate the following specification at the state level:

$$\frac{CARS\ Purchases}{Scalar}_s = \alpha + \beta * \frac{Clunkers}{Scalar}_s + \gamma * X_s + \varepsilon_s \quad (1)$$

We use three potential scalars: the number of 2004 auto purchases, the number of households, and the number of automobiles. The matrix *X* includes control variables that are likely to be correlated with auto purchases in a given city. These control variables are discussed in more detail below.

The results are in Table 2. The results show that the number of clunkers scaled by 2004 auto purchases is the most powerful and robust predictor of CARS purchases. A one standard deviation increase in clunkers per 2004 auto purchases leads to a 2/5 to 3/5 standard deviation increase in CARS purchases. The clunkers per capita ratio also predicts CARS purchases per capita, but the prediction is less robust to the inclusion of control variables. The clunkers per automobile measure does not predict actual purchases under CARS.

Figure 3 presents the scatter plot of CARS purchases per 2004 auto purchases against qualifying clunkers per 2004 auto purchases. States with a large number of qualifying clunkers—North Dakota, South Dakota, Iowa, and Kentucky—had a larger propensity to take-up the program. On the other extreme, state with few qualifying clunkers—Arizona, Florida, Nevada, and Hawaii—bought fewer new cars under the program.

Why is the ratio of qualifying clunkers to total 2004 auto purchases the best predictor of actual take-up? One potential reason is the large amount of across-city variation in automobile turnover. Cities vary significantly in the frequency with which residents buy new cars. This could be related to factors such as income, job mobility, and demographic makeup. Thus comparing two cities with equal turnover is the best way of isolating the effect of the program.

B. Covariates

Research designs produce the cleanest estimates when treatment and control groups are randomly assigned. Unfortunately, the number of clunkers in a given city is not randomly assigned. Given non-random assignment, the concern is that other characteristics unrelated to the CARS program are responsible for the differential purchase patterns in high and low CARS exposure cities.

In order to address this concern, we first provide evidence on the correlation of our measure of CARS exposure with variables that are likely to affect auto purchase patterns. These variables include measures of earnings (median household income, unemployment, and the change in unemployment), measures of balance sheet strength (household default rate, the change in the household default rate, the credit card utilization rate, the median house price, and recent house price growth), and measures of demographics (fraction of zip codes in the city that are urban, the total number of households). The measures of balance sheet strength follow from Mian and Sufi (2010) who show that household defaults, credit card utilization, and house price declines are strongly correlated with auto purchase patterns.

Each row of Table 3 regresses our measure of CARS exposure—the number of clunkers scaled by 2004 auto purchases—on one of the covariates listed above. High CARS exposure cities have lower income and house prices, and are less populated and more rural. However, despite lower incomes, high CARS exposure cities were less affected by the financial crisis. They have lower default rates, less dependence on credit card borrowing, and enjoyed relatively higher house price growth during the crisis period (2006-09). There is no significant correlation between the level or change in unemployment and exposure to the CARS program.

Table 3 shows that exposure to the CARS program is not random, which could pose a challenge to our empirical methodology. For example, it may be that demand for autos grows differentially during our sample period for high versus low income households, perhaps because low income households are impacted harder by the recession. Similarly, households with greater financial distress may display a greater slowdown in durable consumption (Mian and Sufi (2010)). However, such attributes are not consistently correlated with CARS exposure in the same direction: high exposure cities have lower incomes, but they also have lower financial distress. Thus a priori it is not obvious that the significant correlates in Table 3 will bias our estimate of interest in a specific direction.

Nevertheless, we employ two empirical strategies that help us mitigate concerns regarding alternative explanations. First, the CARS program was implemented in just two months: July and August 2009. The sharp and short-lived nature of the program makes it difficult to argue that something else causes the sharp increase in high CARS exposure cities. Similarly, our methodology tests for both an initial increase and subsequent reversal. It is hard to construct alternative stories unrelated to CARS that explains both a short term boost and long term reversal. Second, we explicitly incorporate all observable correlates of the CARS exposure in Table 3 as additional controls. As we show later, none of these variables significantly affect the results.

4. The Effect of CARS on Auto Purchases

A. Initial Evidence

In Figure 4, we split the top 250 U.S. cities by population into quintiles based on the ratio of clunkers to 2004 auto purchases.[13] High CARS exposure cities are in the top quintile and low CARS exposure cities are in the bottom quintile. Quintiles are weighted so that each quintile has the same number of 2004 auto purchases. The top panel plots the ratio of monthly purchases to average 2008 monthly purchases from January 2009 through June 2010 for high and low CARS exposure cities.

The pattern in purchases before the program is similar in both groups. However, there is a dramatic relative increase in auto purchases during the CARS program in high CARS exposure cities. In August 2009, auto purchases in high CARS exposure cities are almost 20% higher than the average monthly purchases in 2008. In contrast, there is only a small increase for low CARS exposure cities. Following September 2009, there is strong evidence of program reversal. Auto purchases in high CARS exposure cities are significantly lower than low CARS exposure cities for five straight months.

The bottom panel shows the cumulative difference in the ratios. Relative to their respective level of 2008 purchases, high CARS exposure cities have 40% higher auto purchases than low CARS exposure cities from July to August 2009. However, by March 2010, almost all of this increase disappears. The cumulative purchases as of March 2010 are only 5% higher in high CARS exposure cities. The evidence strongly suggests swift program reversal.

Figure 5 presents the regression version of Figure 4. In the top panel, we present estimates of the following specifications for each month m from July 2009 to June 2010:

[13] We use only the top 250 cities given that smaller cities add considerable volatility to the averages. The top 250 cities represent 82% of the sample population and 87% of the sample 2004 auto purchases.

$$\frac{Autopurchases_{cm}}{Autopurchases_{c0}} = \alpha^m + \beta^m * CARS\ Exposure_c + \Gamma^m * Controls_c + \varepsilon_{cm} \qquad (2)$$

Each specification represents a weighted cross-sectional regression (with total population of the city as weights) that relates auto purchases in month *m* in city c ($Autopurchases_{cm}$) scaled by average monthly auto purchases in the previous year[14] in city c ($Autopurchases_{c0}$) to our measure of CARS exposure. The CARS exposure measure is total clunkers in city *c* scaled by 2004 auto purchases in city *c* divided by the standard deviation of the same variable to ease interpretation. The solid line presents estimates of β^m without control variables and the dotted line presents estimates of β^m with control variables. The control variables are all variables listed in Table 3. For the specification without control variables, the sample includes all 957 cities. The specification with control variables includes only the 380 cities for which house price growth data are available.[15]

The estimates of β^m for two months into the program (August 2009) imply that a one standard deviation in CARS exposure lead to a 12 to 16% increase in monthly auto purchases relative to the previous year. This is between ½ and 3/4 standard deviation of the left hand side variable. The estimates for September 2009 through February 2010 are negative for both specifications. In other words, cities with higher CARS exposure experienced lower auto purchases for five straight months after the program concluded. For the specification without (with) control variables, the point estimate is negative in nine (seven) of the subsequent 10 months after the program.

[14] That is, July 2008 to June 2009.
[15] These 380 cities make up 87% of the population and 92% of 2004 auto purchases. We show in the appendix that the results are qualitatively similar if we use all control variables except house price growth and the full sample of 957 cities.

The bottom panel presents coefficients from the cumulative version of equation (2):

$$\frac{CumulativeAutopurchases_{cm}}{Autopurchases_{c0}} = \alpha^m + \beta^m * CARS\ Exposure_c + \Gamma^m * Controls_c + \varepsilon_{cm} \qquad (3)$$

From July 2009 to August 2009, the two months in which the CARS program was implemented, cumulative auto purchases were much higher in high CARS exposure cities. The estimate for August 2009 for the specification without control variables suggests that a one standard deviation increase in CARS exposure lead to a 1/2 standard deviation increase in the left hand side variable.

What do these estimates imply about the magnitude of the short-term impact of the CARS program on auto purchases? To answer this question we first split the sample into deciles based on the CARS exposure variable and treat the lowest decile as the control group. We then multiply the August 2009 coefficient (0.21) estimate by each decile's CARS exposure minus the control group's CARS exposure. This produces an effect of the program on auto purchases relative to last year's purchases for each decile, where the effect for the lowest decile—the control group—is zero by construction.

Once we have the effects in terms of the ratio to lagged purchases for each decile, we multiply by lagged purchases to get the effect in terms of the number of cars. We then sum the number of cars for all deciles to get the total aggregate effect. Under the assumption that the bottom decile is a legitimate control group, we find that 340,000 cars were purchased under the program that would have otherwise not been purchased. Using the specification with control variables yields an estimate of 380,000 cars. Therefore, our estimates imply that the cross-city variation in exposure to the CARS program explains between 340,000 and 380,000 automobile purchases during July and August of 2009. This is more than half of the total purchases under the CARS program of 677,842.

While the initial effect of CARS on auto purchases is large, there is strong evidence of swift program reversal. From September 2009 to March 2010, the cumulative difference in auto purchases quickly approaches zero. By June 2010, there is no significant difference in total cumulative purchases. The evidence suggests that most of approximately 360,000 purchases under the CARS program would have occurred by March 2010, and all of them by June 2010.

In the Appendix, we present coefficients from five alternative specifications of equation (3). These specifications include: (a) specifications that are equally weighted instead of population weighted, (b) specifications where the left hand side variable is scaled by 2004 purchases instead of the year prior to CARS, (c) specifications using clunkers per capita as the measure of CARS exposure, (d) specifications where CARS exposure is measured with the more narrow definition of clunkers (see footnote 9), and (e) specifications that include all controls except for house price growth which allows for the use of the full sample of 957 cities. As the Appendix figure shows, the results are stable across these specifications.

B. Statistical Inference

The increase and reversal in the preceding section are statistically significant. Panel A of Table 4, presents the coefficient estimates on the CARS exposure variable from specification (2) along with standard errors for each month from July 2009 to June 2010. Each row reports the estimate from a separate cross-sectional regression, and we report specifications without (left) and with (right) controls. The effect of CARS exposure on the initial increase in auto purchases is statistically significant at the 1 percent confidence level. The program reversal coefficients are statistically significant at the 1 percent level for December 2009 and January, February, and May 2010. The results are similar with control variables.

Panel B reports coefficients for the initial cumulative increase through August 2009 and the cumulative increase through the end of our sample period (June 2010). The initial increase in auto purchases in high CARS exposure cities is statistically significant at the 1 percent level. For the cumulative increase to June 2010, the coefficient estimate on the CARS exposure variable is -0.016 and the standard error is 0.053. This implies a 95% confidence interval of -0.121 to 0.089. In other words, we can be 95% confident that a one standard deviation increase in CARS exposure does not increase cumulative auto purchases through June 2010 scaled by last year's purchases by more than 0.09. We can soundly reject the hypothesis that the coefficient in June 2010 is 0.210, which means we can reject the hypothesis that the CARS program led to a one time increase that was not reversed by June 2010.[16]

An alternative inference approach is to exploit the availability of monthly data going back to 2004. This allows for estimation of placebo specifications to assess how statistically "unusual" the patterns are during the months of and following CARS. In the top panel of Figure 6, we plot coefficients on the CARS exposure variable from estimation of equation (3) with starting points for every month from January 2005 to July 2008.[17] The plots examine the difference in cumulative auto purchases for high and low CARS exposure cities for years before the program. The solid line starts at the "true" experiment—when CARS was implemented in July 2009.

As the top panel shows, the sharp increase in auto purchases in high CARS exposure cities after the CARS program is unprecedented. The dramatic decline from September 2009 to

[16] The standard errors are substantially larger in the specification with control variables, in large part due to the smaller sample size. We can still reject at the 5% confidence level the hypothesis of no program reversal. If we exclude the house price growth control which enables use of the full sample, the 95% confidence interval with other controls is -0.09 to 0.14.
[17] We start in January 2005 as opposed to January 2004 because one year of lagged data is needed to construct the left hand side variable.

March 2010 is also unprecedented. To highlight the decline, we replicate the placebo tests in the middle panel but we start the CARS experiment in August 2009. The decline in high CARS exposure areas in cumulative purchases is larger immediately after the program than for any other month in the sample.

The bottom panel plots the placebo tests that begin in July of each year. The results are instructive on several dimensions. First, the July 2008 placebo test represents the year prior to CARS and it shows no noticeable pre-trend, which is consistent with evidence in Figure 4. In other words, high CARS exposure cities did not show a significantly different pattern in purchases from July 2008 to June 2009. Second, the bottom panel helps to rule out concerns regarding seasonal differences in purchase patterns in high versus low CARS exposure cities. Finally, the decline from September 2009 to March 2010 is larger than any other decline in any of the July placebo tests. Compared to the other years, the pattern from July 2009 to June 2010 is unique in its initial jump and sharp subsequent decline.

One additional concern relates to measurement error. One could argue that the long run effect of any experiment or program will approach zero because of random shocks and imprecise measurement. The further away from the experiment the data get, the more coefficients are biased toward zero. Figure 7 contradicts this argument by showing evidence of a shock that has a permanent effect on cumulative purchases. The test follows Mian and Sufi (2010) who show that U.S. counties with large increases in household leverage reduced durable consumption disproportionately during the housing bust and mortgage default crisis. Figure 7 splits cities into quintiles based on the increase in household leverage from 2002 to 2006 and shows that the beginning of the mortgage default crisis lead to a permanent shift in cumulative purchases patterns going forward in high leverage growth cities.

5. The Effect of CARS on Other Economic Outcomes

The previous section shows evidence of a large effect of CARS that very quickly reverses. In this section, we address whether pulling forward auto purchases by seven to 10 months had measurable effects on economic activity. In the first subsection, we examine other economic outcomes in high and low CARS exposure cities. In the second subsection, we examine economic outcomes in cities with a large fraction of the population working in the automotive industry.

A. Other Economic Outcomes in High and Low CARS Exposure Cities

Before presenting results, we want to emphasize that our analysis here relies on the assumption that the fiscal multiplier is stronger in local economies than distant economies. In other words, if CARS had an effect on employment or other economic outcomes, we should see this effect stronger in the cities in which CARS induced a high number of auto purchases. If car purchases in a given city have an equally strong effect on economic outcomes in both the city in question and other cities, we will not see an effect on economic outcomes even if CARS had a broader effect on the economy.[18]

We have three economic outcomes available at the city-quarterly level: employment, household defaults, and house prices. Figure 8 shows the evolution of these outcomes in the four quarters prior and four quarter after CARS for the largest 250 cities in the sample. High and low CARS exposure cities are defined as in Figure 4. The top panel shows a slightly larger drop in employment growth for high CARS exposure cities from the second quarter of 2008 to the first

[18] We do not know of any research that examines how "local" fiscal multipliers are for auto purchases or other durable consumption. The closest is Wilson (2010) who examines the effect of stimulus on employment using variation in fiscal stimulus payments across states and Moretti (2010) who examines employment multipliers on the local economy.

quarter of 2009, with a sharp relative increase in the second quarter of 2009. After the implementation of CARS, there was a slowdown in job losses in both the high and low CARS exposure cities. There is no evidence of a dramatic structural break in employment growth in high and low CARS exposure cities.

Household defaults and declines in house prices were much more severe in low CARS exposure counties prior to CARS, a result that is shown above in Table 3. Once again, there does not appear to be any major structural break in the patterns after the CARS program. If anything, there is some evidence that the increase in household defaults moderated more in high CARS exposure cities.

Table 5 presents the regression version of these figures. For employment and house prices, we regress the difference in growth rates from four quarters before to four quarters after the CARS program. For default rates, we use the difference in the changes. As the coefficient estimates show, there is no noticeable effect of the program on employment growth. If anything, there is evidence that high CARS exposure cities experienced a relative increase in the default rate pattern and a relative decrease in the house price growth pattern.

B. Economic Outcomes in High versus Low Auto Employment Share Cities

One of the central arguments of proponents of the CARS program is that it had an effect on industrial production and manufacturing employment. We evaluate this argument by examining cities in which a large fraction of employees work in the auto sector. City-level data on share of employees working in each sector of the economy comes from Business Statistics published by the Census. The five cities with the highest auto sector employment share as of 2006 are Elkhart-Goshen, IN; Detroit-Livonia-Dearborn, MI; Holland-Grand Haven, MI; Flint, MI; and Clarksville, TN-KY. The mean share of employees working in the auto sector across cities is 0.033 and the standard deviation is 0.019.

Figure 9 examines auto purchases. It replicates the top panel of Figure 4 except that it splits cities by auto sector employment instead of CARS exposure. There is no evidence of a differential effect on auto purchases in high versus low auto sector employment cities. Figure 10 examines the other three outcomes. High auto sector employment cities experience a more severe drop in employment before the CARS program, and there is some evidence of a stronger rebound in employment in these areas, especially in the second quarter of 2010. In contrast, there is no evidence of a significant structural break for house prices or household defaults.

Table 6 replicates Table 5 with the right hand side variable being the fraction of the population that is employed in the auto sector. The coefficient estimate in column 1 suggests that high auto sector employment cities experienced a relative increase in employment growth from the second quarter of 2009 to the second quarter of 2010. The estimate implies that a one standard deviation increase in auto sector employment share leads to a 1/4 standard deviation increase in employment growth. There is no difference in the patterns for default rates or house prices.

The evidence in Table 6 and Figure 10 suggest that the CARS program was associated with a slow-down in job losses in cities with a large number of employees working in the auto sector. One caveat is in order. Government intervention in the auto industry came in many forms in the early part of 2009, including the unprecedented bailouts of General Motors and Chrysler in April 2009. Given that both CARS and other government intervention occurred at almost exactly the same time, it is difficult to separate their independent effects.

6. Concluding Remarks

Recessions are often met with massive government intervention in the economy. Given that such interventions lead to a large build up of public debt, there is an important debate to be had on whether the benefits outweigh the costs.[19] However, the empirical evidence on this issue remains limited. This paper evaluates the effects of a popular form of fiscal stimulus—providing monetary incentives to boost consumption in the short-term.

The novelty of our analysis lies in part from our empirical strategy that exploits cross-city variation in exposure to a stimulus program. Doing so enables us to more effectively control for aggregate fluctuations in the economy and estimate the direct short and medium run effects of fiscal stimulus. Our results reveal a swift reversal in auto purchases at the expiration of CARS, which highlights a strong inter-temporal substitution that quickly "crowds out" the initial effect. Our evidence suggests that the 'cash for clunkers' program, a program that cost $2.85 billion, had no long run effect on auto purchases.

The results of our analysis should be useful for assessing the feasibility of other durable goods stimulus programs, where the degree of inter-temporal substitution is suspected to be large (e.g., Eberly (1994), Ogaki and Reinhart (1998)). Recent examples of such proposals include the first-time home buyer credit or the cash for caulkers program. Indeed, aggregate evidence on existing home sales suggests a sharp reversal after the expiration of the first-time home buyer credit in the summer of 2010.

Our findings do not warrant the claim that *all* forms of fiscal stimulus fail to boost long-run economic output. It is conceivable that alternatively designed stimulus programs (e.g. extending unemployment benefits) have different implications for the economy. However, we

[19] Reinhart and Rogoff (2010) examine the costs of public debt across countries and time periods.

hope that our methodology of exploiting cross-sectional variation in exposure to a stimulus program at a micro-level can be fruitful in answering such related questions.

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Figure 1
Aggregate Monthly Auto Purchases

This figure plots aggregate auto purchases. The data are from R.L. Polk. The vertical dotted lines mark the beginning and end of the CARS program, which took place in July and August, 2009.

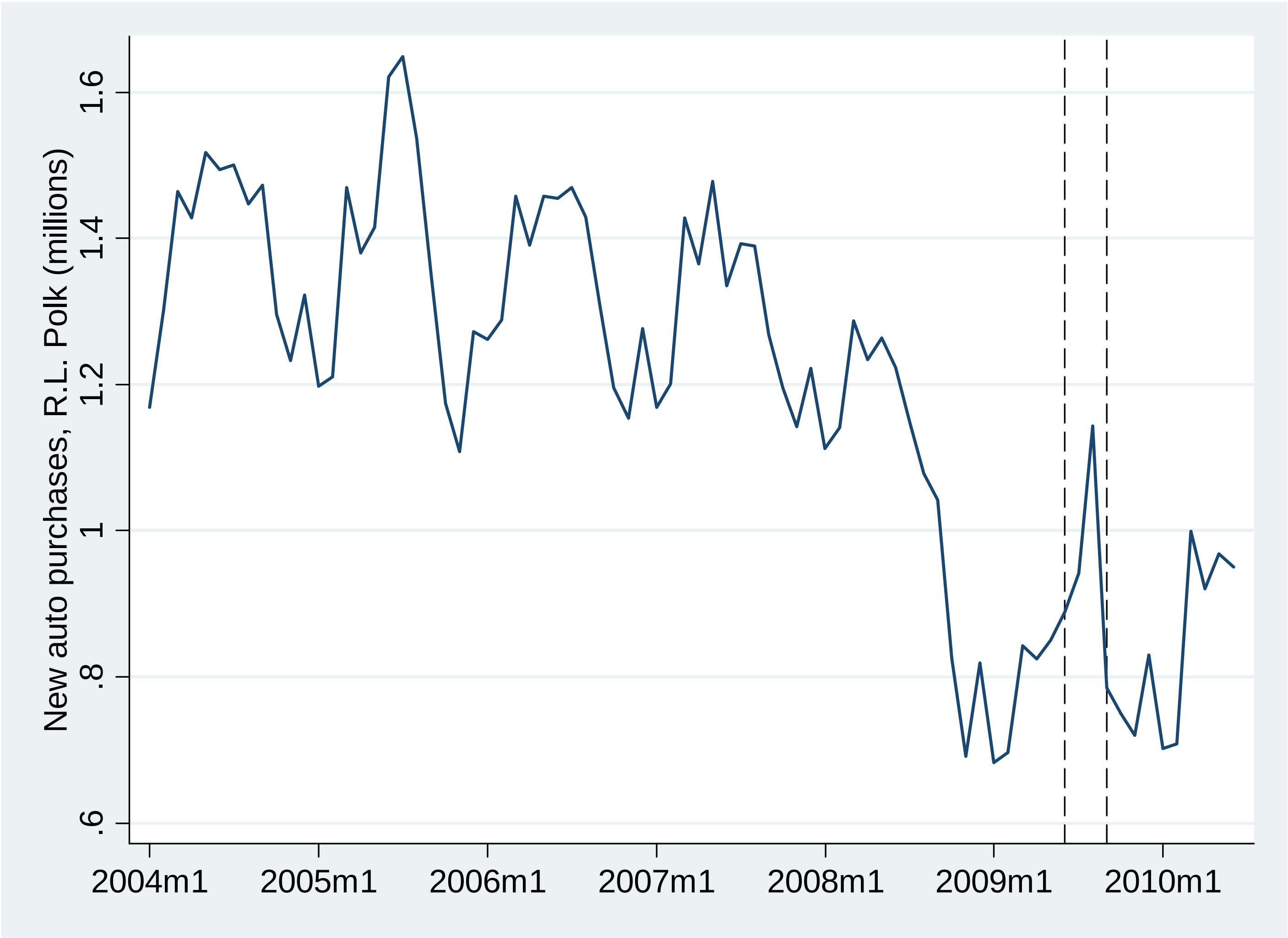

Figure 2
Purchases under CARS Program across States

This figure shades states by the number of autos purchased under the CARS program scaled by 2004 auto purchases. States with a darker shading have a higher number of cars sold under the CARS program.

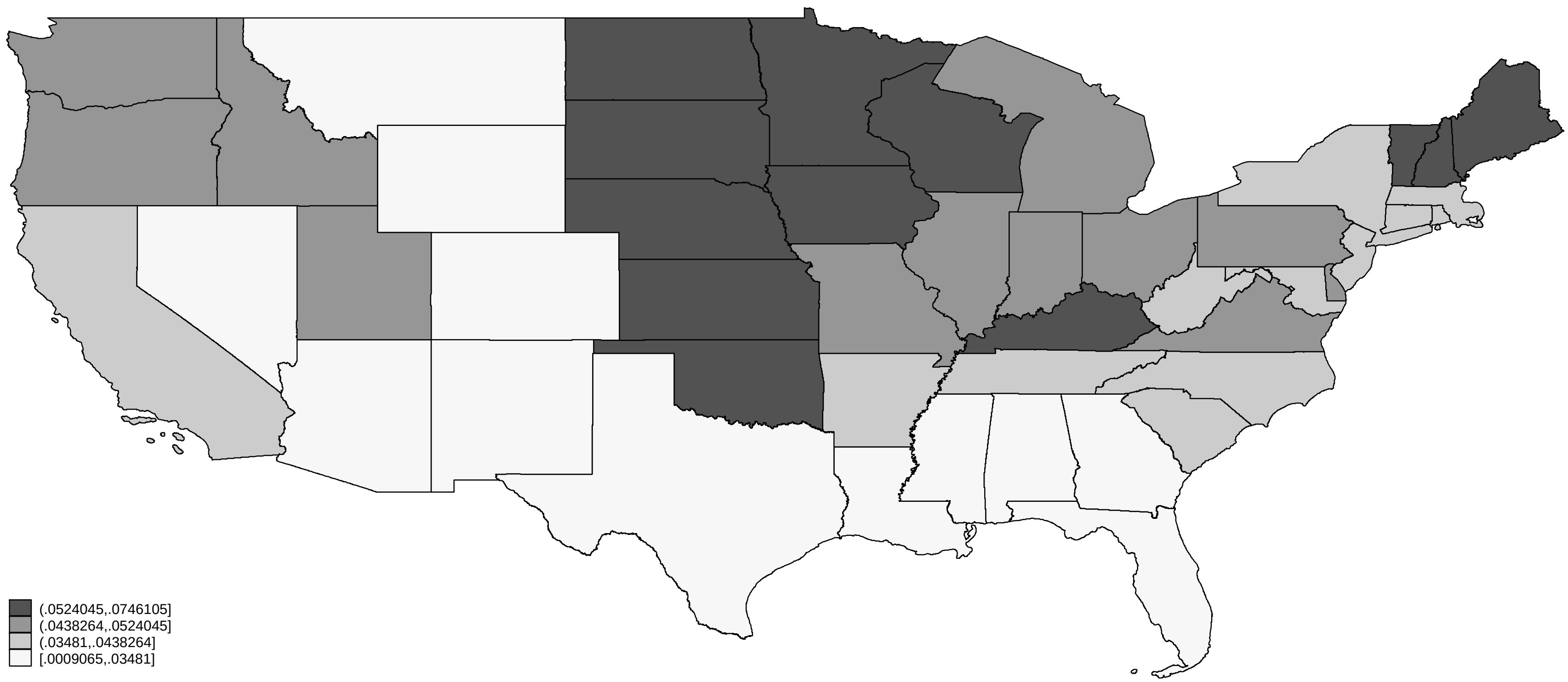

Figure 3
CARS Auto Purchases and Ex Ante Exposure—State Level

This figure plots the relation between our measure of qualifying clunkers and actual purchases made under the CARS program at the state level. Both variables are scaled by 2004 auto purchases in the state.

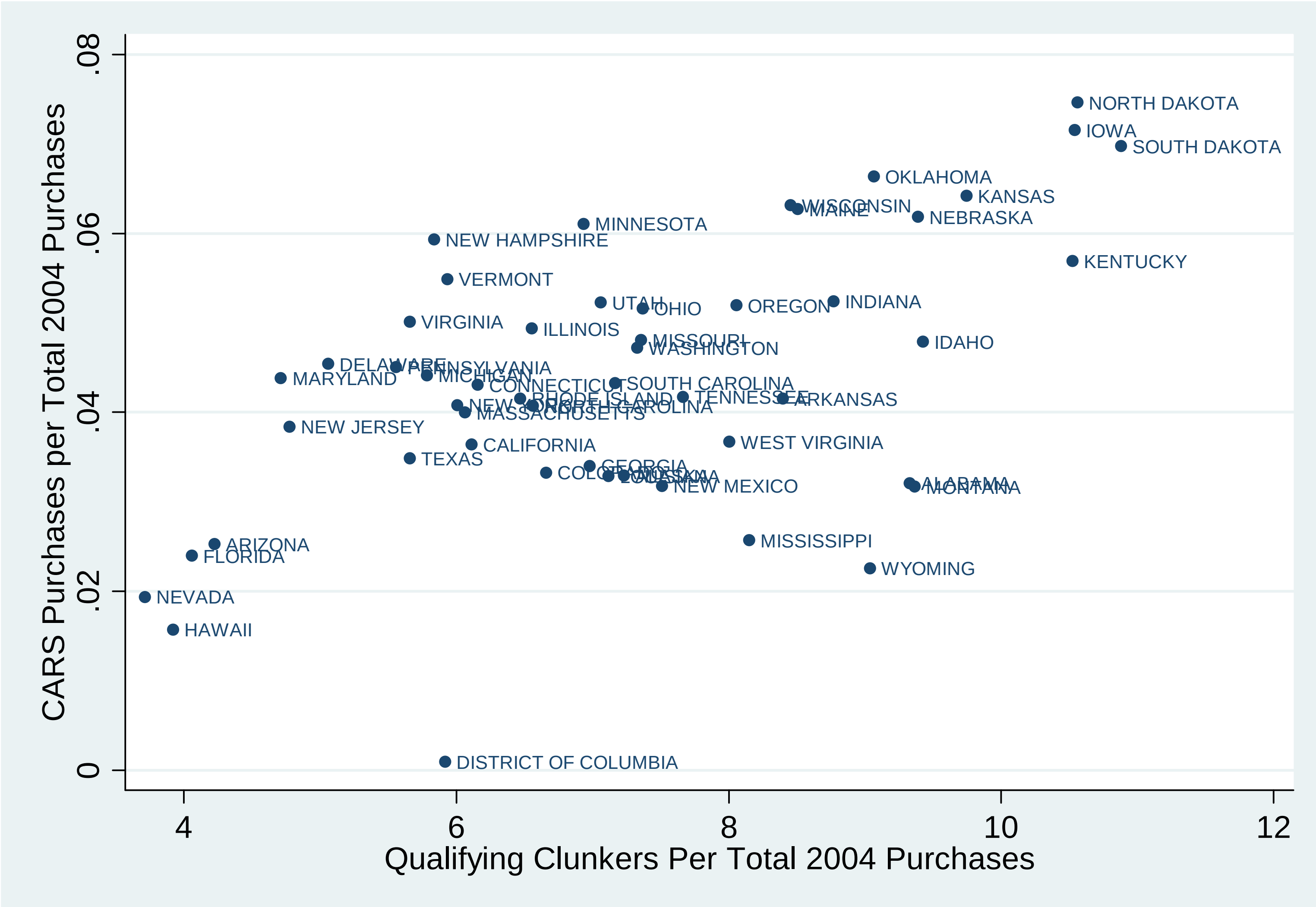

Figure 4
Auto Purchases for High and Low CARS Exposure Cities

High and low CARS exposure cities are the top and bottom quintile cities based on the ratio of qualifying clunkers as of the summer of 2008 to total auto purchases in 2004. The quintiles are formed using weights that ensure that each quintile includes the same number of total 2004 auto purchases. The top panel shows purchases by month scaled by 2008 average monthly purchases. The bottom panel shows the cumulative difference in the ratios between high and low CARS exposure cities.

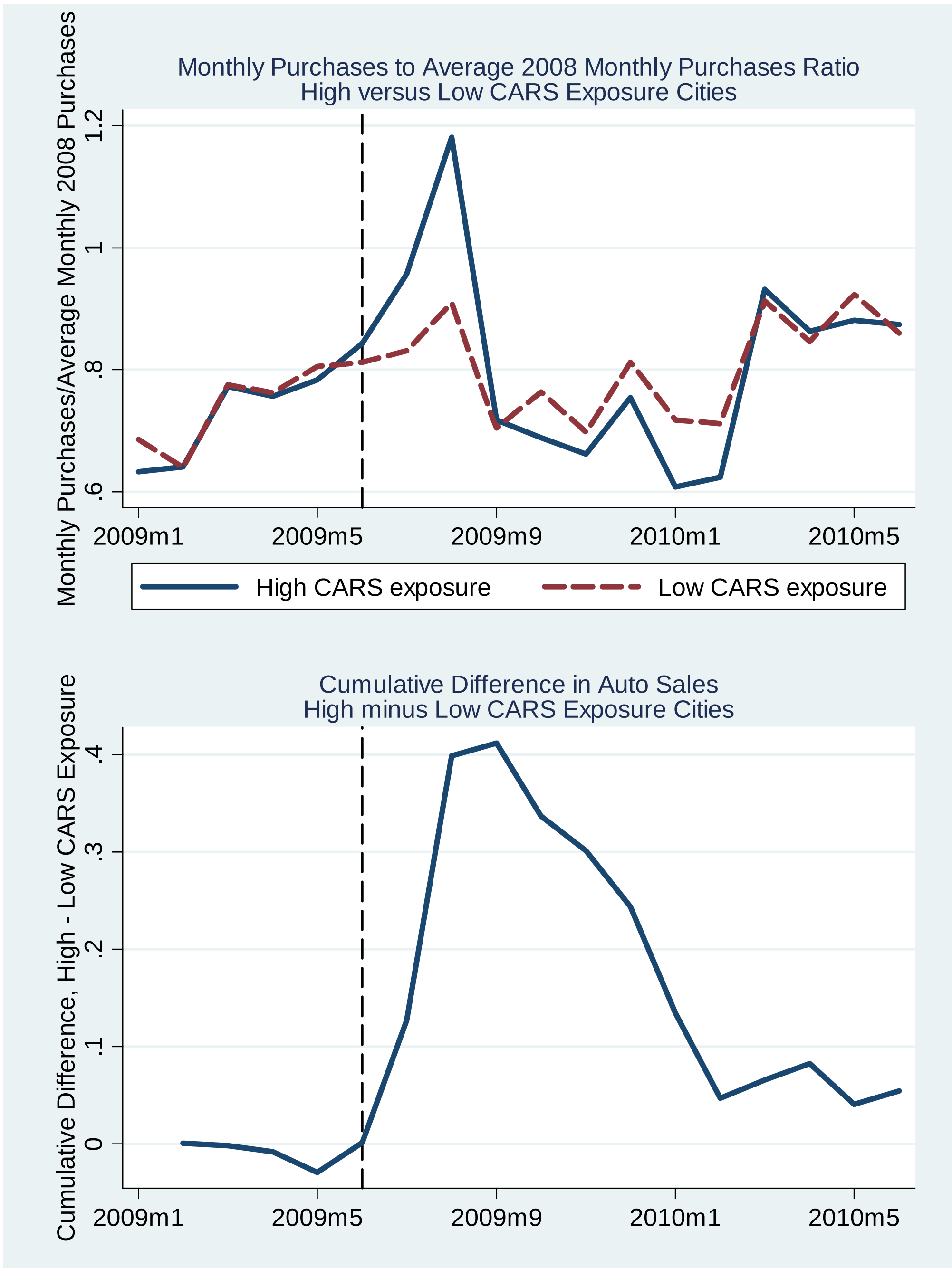

Figure 5
CARS Program Impact Coefficients

The top panel plots the coefficients β^m of the following regression for each month *m* from July 2009 to June 2010:

$$\frac{Autopurchases_{cm}}{Autopurchases_{c0}} = \alpha^m + \beta^m * CARS\ Exposure_c + \Gamma^m * Controls_c + \varepsilon_{cm}$$

where the left hand side variable is auto purchases in a given city in a given month scaled by the average monthly purchases in the 12 months prior to the CARS program. The variable *CARS Exposure* is the ratio of clunkers to 2004 total auto purchases scaled by the standard deviation of the same variable across cities to ease interpretation of the coefficients. The control variables include the natural logarithm of median household income, the unemployment rate, the change in the unemployment rate from 2006 to 2009, the change in the debt to income ratio from 2002 to 2006, the household default rate, the change in the household default rate from 2006 to 2009, the credit card utilization rate, the natural logarithm of median house price, house price growth from 2006 to 2009, the fraction of the city that is urban, and the natural logarithm of total population. The bottom panel represents the cumulative version of the top figure:

$$\frac{CumulativeAutopurchases_{cm}}{Autopurchases_{c0}} = \alpha^m + \beta^m * CARS\ Exposure_c + \Gamma^m * Controls_c + \varepsilon_{cm}$$

where the left hand side variable is cumulative auto purchases from July 2009 until each month *m*. All specifications are weighted by total population of the city.

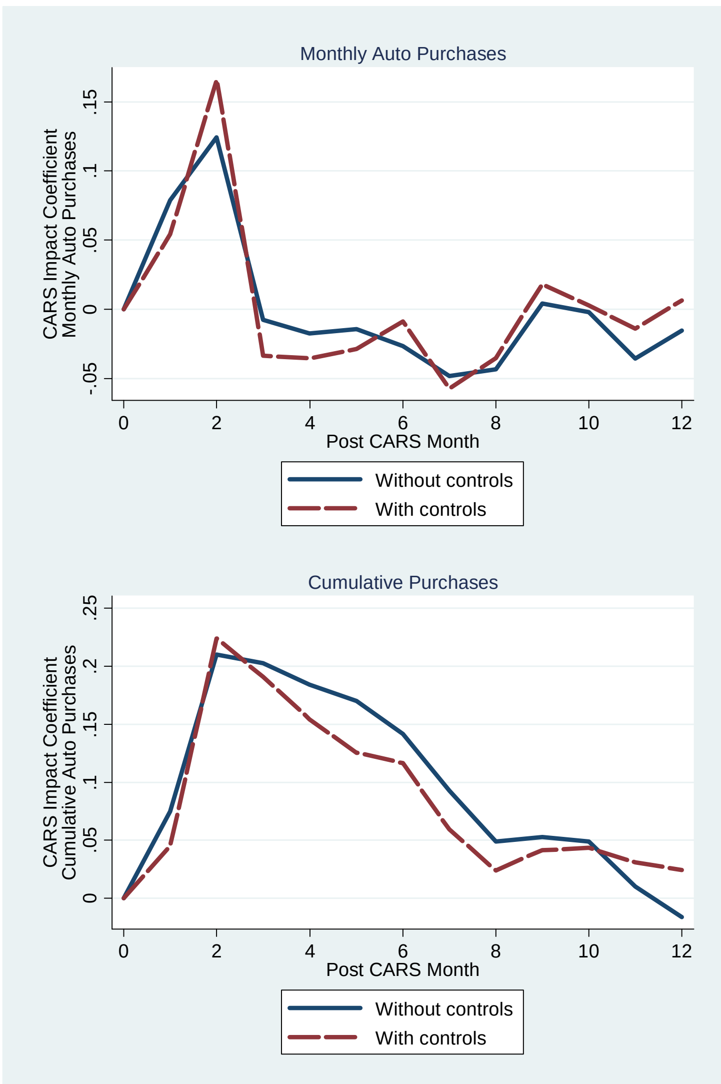

Figure 6
Placebo Tests

This figure presents a series of placebo tests for the cumulative impact coefficients. In the top panel, we examine the cumulative effect for placebo experiments for every other month in our sample that does not enter into our treatment period. Each series of dots represents the cumulative difference in auto purchases for high CARS exposure cities in months where there is no CARS program. The solid blue line is the true CARS program that began in July 2009. The middle panel is identical except that the solid blue line begins in September 2009 to highlight the program reversal. The bottom panel plots only placebo tests that begin in July of each respective year. All tests include control variables for the logarithm of median household income, the unemployment rate, the default rate, the credit card utilization rate, the logarithm of median house price, the fraction of the city that is urban, and the natural logarithm of population.

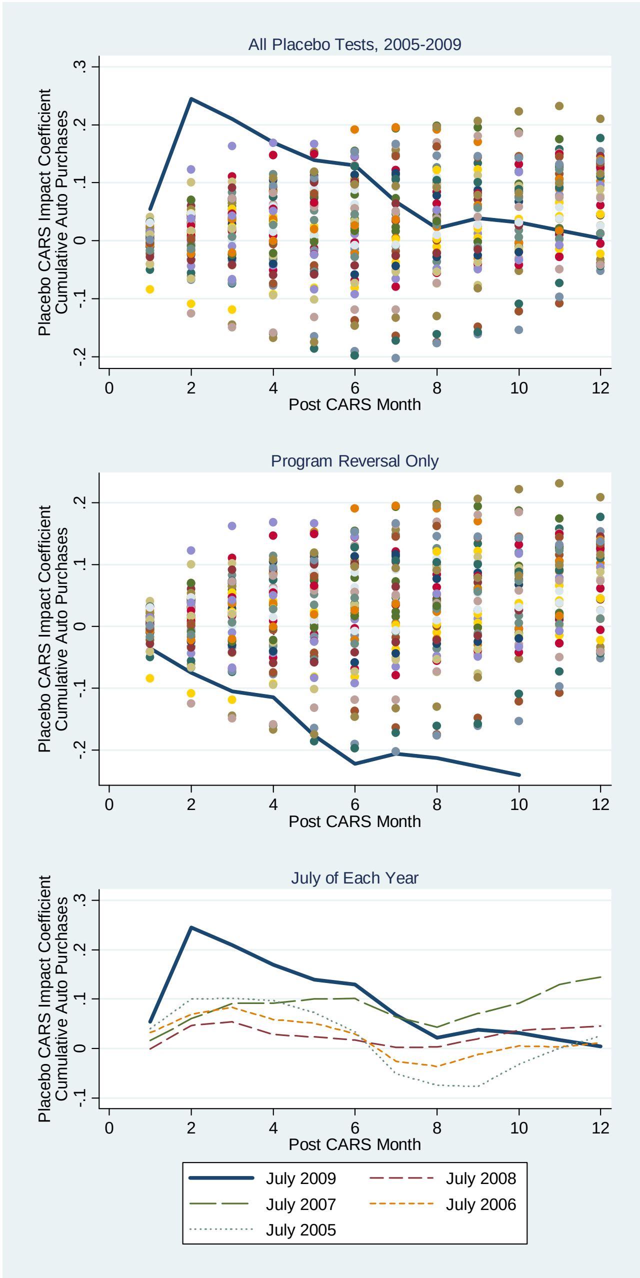

Figure 7

An Alternative Shock: The Mortgage Default Crisis and Household Leverage

This figure presents evidence of a shock—the mortgage default crisis—that had a long-run effect on auto purchases. We split cities into quintiles based on the increase in the debt to income ratio from 2002 to 2006, and the figure plots auto purchases for the highest and lowest quintile of the distribution. The quintiles were weighted by total auto purchases for 2004. The top panel shows purchases by month scaled by 2004 average monthly purchases. The bottom panel shows the cumulative difference in the ratios between high and low debt to income exposure cities.

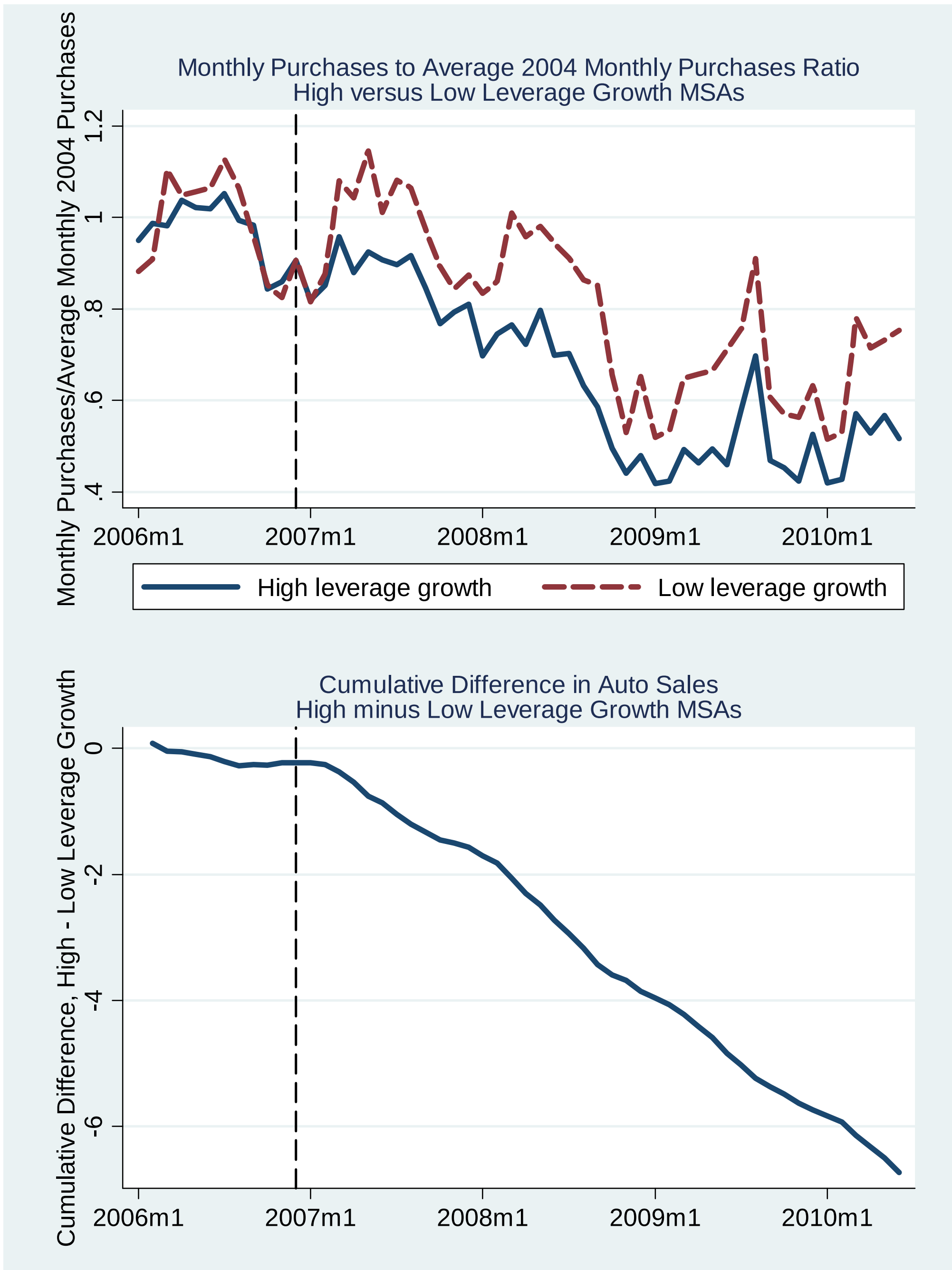

Figure 8
Other Measures of Economic Activity

This figure presents employment, household default, and house price patterns for high and low CARS exposure cities. High and low CARS exposure cities are the top and bottom quintile cities based on the ratio of qualifying clunkers as of the summer of 2008 to total auto purchases in 2004. The quintiles are formed using weights that ensure that each quintile includes the same number of total 2004 auto purchases. The dotted line marks the beginning of the CARS program.

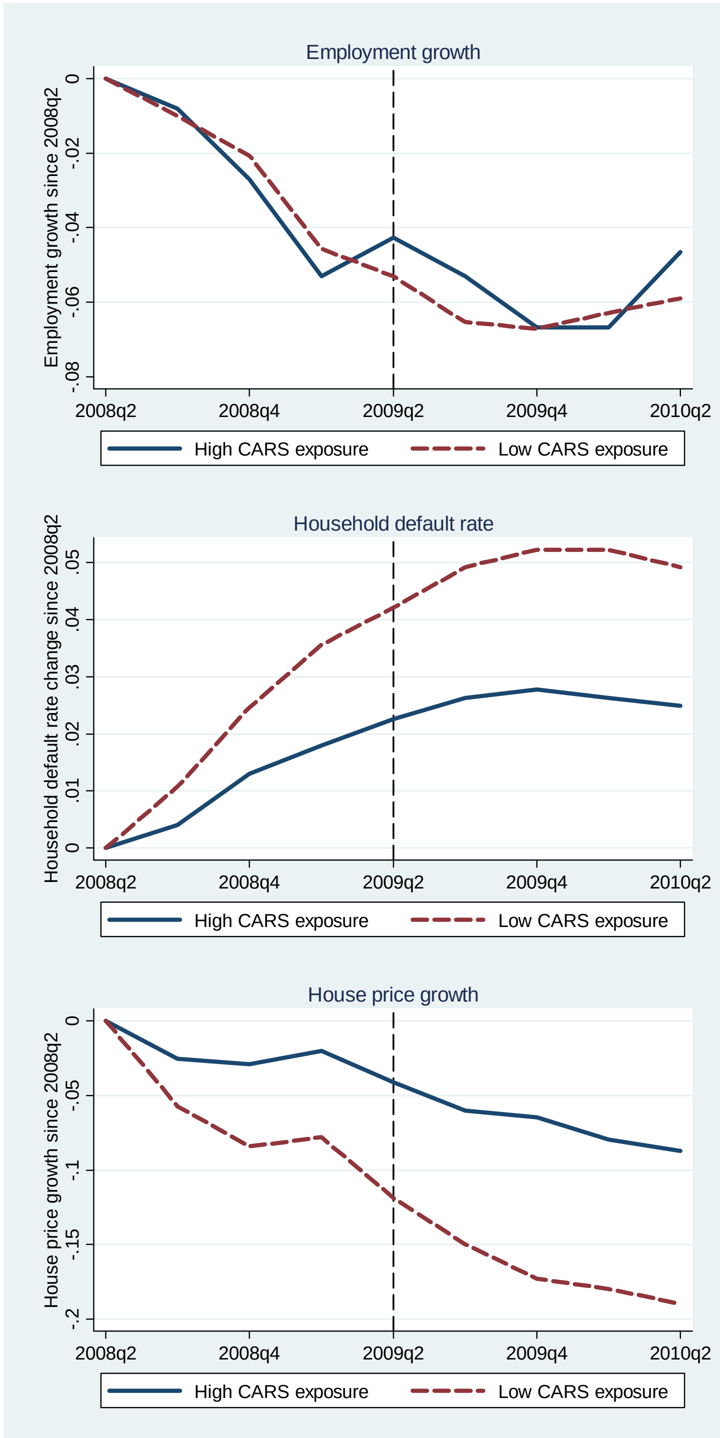

Figure 9
Auto Purchases for High and Low Auto Sector Employment Cities

High and low auto sector employment cities are the top and bottom quintile cities based on fraction of employees that work in the auto sector. The quintiles are formed using weights that ensure that each quintile includes the same number of total 2004 auto purchases. The figure shows purchases by month scaled by 2008 average monthly purchases.

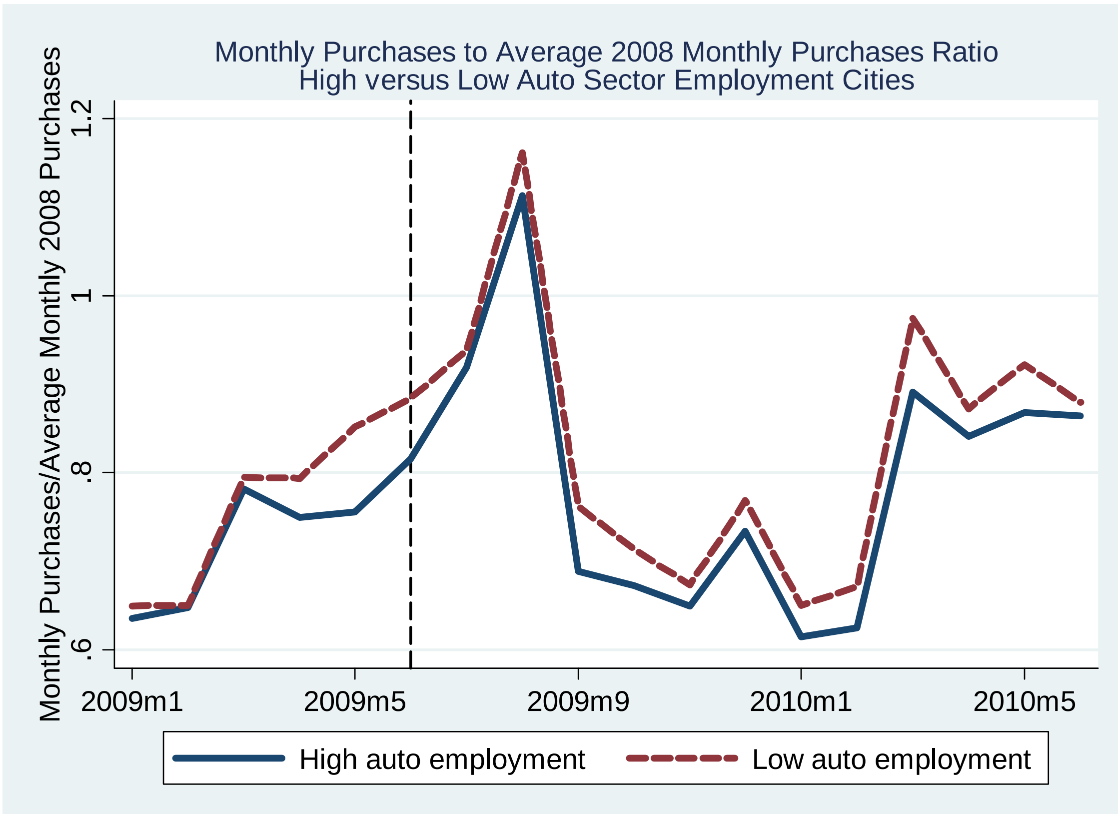

Figure 10
High versus Low Auto Sector Employment Cities:
Other Measures of Economic Activity

This figure presents employment, household default, and house price patterns for high and low auto sector employment cities. High and low auto sector employment cities are the top and bottom quintile cities based on fraction of employees that work in the auto sector. The quintiles are formed using weights that ensure that each quintile includes the same number of total 2004 auto purchases. The dotted line marks the beginning of the CARS program.

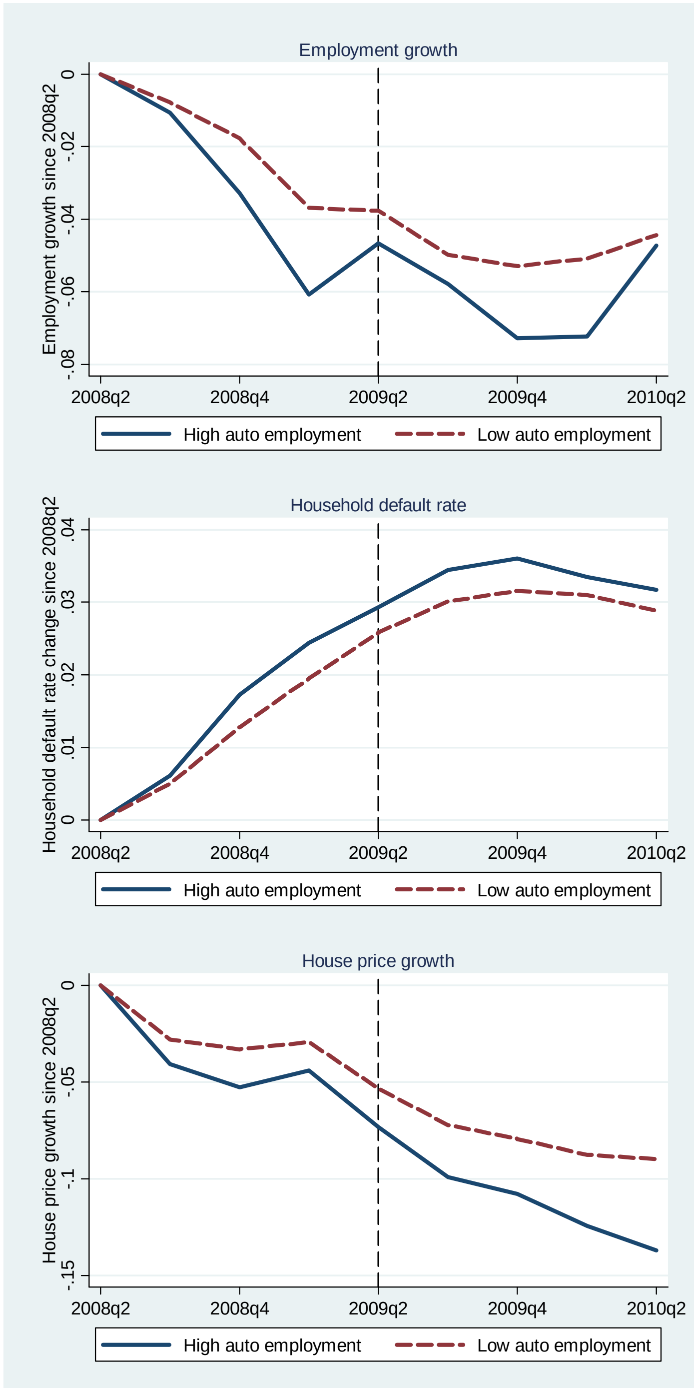

Table 1
Summary Statistics

This tables presents summary statistics for the 957 cities in our sample. Each city represents a Core-Based Statistical Area (CBSA) as defined by the OMB and used by the Census. House price growth data from FHFA is available for only 380 CBSAs in our sample.

	Mean	SD	10^{th}	50^{th}	90^{th}
City-level data					
Clunkers per 2004 auto purchases	9.89	3.35	5.80	9.56	14.62
Clunkers per capita	0.98	0.17	0.78	0.98	1.19
Clunkers per automobile	0.50	0.05	0.44	0.50	0.56
Alternative clunkers per automobile	0.17	0.03	0.12	0.17	0.21
Median household income, census	37.81	8.55	28.50	36.62	48.48
Unemployment rate, 09q2	0.10	0.03	0.06	0.09	0.14
Change in unemployment rate, 06q2 to 09q2	0.05	0.02	0.02	0.04	0.08
Change in debt to income 02 to 06	0.58	0.51	0.09	0.49	1.16
Household default rate, 09q2	0.08	0.03	0.04	0.07	0.11
Change in household default rate, 06q2 to 09q2	0.03	0.03	0.00	0.03	0.07
Credit card utilization rate, 09q2	0.25	0.03	0.21	0.25	0.30
Median house price, census	92.33	51.12	51.29	79.49	142.33
House price growth, 06q2 to 09q2, FHFA	-0.03	0.17	-0.30	0.03	0.11
Fraction of zip codes that are urban	0.34	0.23	0.10	0.29	0.71
Total number of households, census	111.15	284.83	11.43	31.92	239.08
City-monthly level data					
Auto purchases	1188	3570	62	218	2289
Key left-hand side variables					
Purchases to last year average ratio, 08/2009	1.28	0.22	0.98	1.30	1.59
Cumulative purchases to last year average ratio, 08/2009	2.34	0.36	1.88	2.36	2.78
Purchases to last year average ratio, 06/2010	0.96	0.19	0.73	0.97	1.18
Cumulative purchases to last year average ratio, 06/2010	10.72	0.87	9.57	10.82	11.91

Table 2
Determinants of Purchases under the CARS Program at the State Level

The table presents evidence on the best specification to explain purchases under the CARS program at the state level. Standard errors are heteroskedasticity-robust.

Left hand side variable:	CARS purchases per 2004 auto purchases		CARS purchases per capita		CARS purchases per automobiles	
	(1)	(2)	(3)	(4)	(5)	(6)
Clunkers per 2004 auto purchases	0.005**	0.003**				
	(0.001)	(0.001)				
Clunkers per capita			0.006*	0.002		
			(0.002)	(0.002)		
Clunkers per automobile					0.000	-0.001
					(0.003)	(0.003)
Ln(median household income)		0.074**		0.012**		0.005**
		(0.013)		(0.002)		(0.001)
Unemployment rate, 09q2		-0.062		-0.031		-0.009
		(0.198)		(0.023)		(0.013)
Change in unemployment rate, 06q2 to 09q2		0.066		0.031		0.010
		(0.212)		(0.024)		(0.013)
Change in debt to income 02 to 06		0.006		0.001		0.000
		(0.009)		(0.001)		(0.001)
Household default rate, 09q2		-0.062		0.014		0.007
		(0.296)		(0.034)		(0.019)
Change in HH default rate, 06q2 to 09q2		0.040		-0.023		-0.008
		(0.322)		(0.038)		(0.020)
Credit card utilization rate, 09q2		-0.132*		-0.009		-0.005
		(0.063)		(0.008)		(0.005)
Ln(median house price)		-0.028**		-0.003*		-0.001*
		(0.010)		(0.001)		(0.001)
House price growth, 06q2 to 09q2		0.002		-0.001		0.000
		(0.025)		(0.003)		(0.002)
Fraction of zip codes that are urban		-0.032*		-0.004*		-0.003**
		(0.013)		(0.002)		(0.001)
Ln(# households)		0.005**		0.001**		0.000**
		(0.002)		(0.000)		(0.000)
Constant	0.011	-0.463**	0.000	-0.086**	0.003	-0.039**
	(0.007)	(0.113)	(0.002)	(0.015)	(0.002)	(0.008)
N	51	51	51	51	51	51
R^2	0.30	0.68	0.15	0.64	-0.02	0.53

**,* statistically distinct from zero at the 1% and 5% confidence level, respectively

Table 3
Correlation of CARS Exposure with Other Observables at the City Level

Each row in this table reports a univariate regression of the clunkers to 2004 auto purchases ratio on different observable variables and a constant. Standard errors are heteroskedasticity-robust.

Left hand side variable:	Clunkers per 2004 auto purchases		
Right hand side variable:	Coefficient	R^2	N
Ln(median household income)	-6.460**	0.28	957
	(0.476)		
Unemployment rate, 09q2	12.779	0.01	957
	(7.503)		
Change in unemployment rate, 06q2 to 09q2	-1.520	-0.00	957
	(8.729)		
Change in debt to income 02 to 06	-1.246**	0.06	957
	(0.263)		
Household default rate, 09q2	-20.793**	0.10	957
	(3.845)		
Change in HH default rate, 06q2 to 09q2	-21.117**	0.12	957
	(3.407)		
Credit card utilization rate, 09q2	-12.311**	0.02	957
	(4.769)		
Ln(median house price)	-2.728**	0.23	957
	(0.245)		
House price growth, 06q2 to 09q2	3.039**	0.06	380
	(0.868)		
Fraction of zip codes that are urban	-8.007**	0.56	957
	(0.289)		
Ln(# households)	-1.330**	0.55	957
	(0.087)		

**,* statistically distinct from zero at the 1% and 5% confidence level, respectively

Table 4
The Effect of the CARS Program on Auto Purchases

This table presents regression coefficients that are displayed in Figure 5. Panel A shows the differential pattern in monthly auto purchases for cities with high CARS exposure and Panel B shows the differential pattern in cumulative purchases. The measure of CARS exposure is clunkers scaled by 2004 auto purchases. Each row reports a separate regression. Standard errors are heteroskedasticity-robust.

Panel A
Monthly Auto Purchases

$$\frac{Autopurchases_{cm}}{Autopurchases_{c0}} = \alpha^m + \beta^m * CARS\ Exposure_c + \Gamma^m * Controls_c + \varepsilon_{cm}$$

	Without controls (N = 957)		With controls (N = 380)	
	Coefficient	R^2	Coefficient	R^2
July 2009	0.079**	0.20	0.054**	0.30
	(0.008)		(0.013)	
August 2009	0.124**	0.26	0.166**	0.46
	(0.012)		(0.017)	
September 2009	-0.008	0.00	-0.034	0.10
	(0.011)		(0.019)	
October 2009	-0.017	0.01	-0.035	0.13
	(0.012)		(0.022)	
November 2009	-0.014	0.01	-0.029	0.12
	(0.009)		(0.015)	
December 2009	-0.026**	0.02	-0.009	0.09
	(0.009)		(0.017)	
January 2010	-0.048**	0.08	-0.057**	0.20
	(0.012)		(0.016)	
February 2010	-0.043**	0.09	-0.035**	0.17
	(0.009)		(0.009)	
March 2010	0.004	-0.00	0.018	0.11
	(0.010)		(0.019)	
April 2010	-0.002	-0.00	0.003	0.09
	(0.009)		(0.014)	
May 2010	-0.036**	0.04	-0.014	0.14
	(0.010)		(0.015)	
June 2010	-0.015	0.00	0.007	0.14
	(0.011)		(0.014)	

Panel B
Cumulative Auto Purchases

$$\frac{CumulativeAutopurchases_{cm}}{Autopurchases_{c0}} = \alpha^m + \beta^m * CARS\ Exposure_c + \Gamma^m * Controls_c + \varepsilon_{cm}$$

	Without controls (N = 957)		With controls (N = 380)	
	Coefficient	R^2	Coefficient	R^2
August 2009	0.210**	0.22	0.224**	0.33
	(0.018)		(0.025)	
June 2010	-0.016	0.00	0.024	0.14
	(0.053)		(0.090)	

**,* statistically distinct from zero at the 1% and 5% confidence level, respectively

Table 5
Other Economic Outcomes High and Low CARS Exposure Cities

This table presents regressions of economic outcomes on our measure of exposure to the CARS program, which is the number of clunkers in a city scaled by 2004 auto purchases. Each outcome variable is constructed by first calculating the growth rates from June 2008 to June 2009 and June 2009 to June 2010, and then differencing. Each outcome variable therefore represents the change in the growth rates (or change in the change in the case of household default rates). Standard errors are heteroskedasticity robust.

	(1) Employment growth$_{09,10}$ – Employment growth$_{08,09}$	(2) Default rate change$_{09,10}$ – Default rate change$_{08,09}$	(3) House price growth$_{09,10}$ – House price growth$_{08,09}$
CARS exposure	-0.0002	0.002**	-0.007**
	(0.0003)	(0.0003)	(0.001)
Constant	0.036**	-0.039**	0.055**
	(0.002)	(0.002)	(0.009)
N	957	957	380
R^2	0.00	0.06	0.05

**,* statistically distinct from zero at the 1% and 5% confidence level, respectively

Table 6
Economic Outcomes in High Auto Employment Cities

This table presents regressions of economic outcomes on our measure of high auto employment cities, which is the fraction of all employees in the city that work in the auto industry. Each outcome variable is constructed by first calculating the growth rates from June 2008 to June 2009 and June 2009 to June 2010, and then differencing. Each outcome variable therefore represents the change in the growth rates (or change in the change in the case of household default rates). Standard errors are heteroskedasticity robust.

	(1) Employment growth$_{09,10}$ – Employment growth$_{08,09}$	(2) Default rate change$_{09,10}$ – Default rate change$_{08,09}$	(3) House price growth$_{09,10}$ – House price growth$_{08,09}$
Auto employment share	0.598**	-0.024	-0.095
	(0.061)	(0.057)	(0.246)
Constant	0.016**	-0.024**	0.016*
	(0.002)	(0.002)	(0.008)
N	957	957	380
R^2	0.04	0.00	0.00

**,* statistically distinct from zero at the 1% and 5% confidence level, respectively