The Effect of Car Ownership on Employment: Evidence from State Insurance Rate Regulation

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ABSTRACT

Various economic theories suggest that one reason for low rates of employment among low-skill, inner-city residents is that they are spatially separated from jobs that have moved out to the suburbs. To test this, I exploit variation in state "prior approval" insurance rate regulation which has been shown to suppress auto insurance prices, thereby decreasing the cost of owning a car. I find that rate regulation increases the proportion of multi-car households among married couples with children. In those households, I find that the additional car in the household encourages mothers to decrease their labor supply while their husbands increase their labor supply. One possible explanation of this result is that second cars are stronger complements to time spent in home production (and especially childrearing) than they are to time spent in the labor market.

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I. Introduction

A commonly cited barrier to employment among the urban poor is a lack of reliable transportation. Previous attempts to test for this barrier have either focused on a small portion of the population (raising concerns regarding external validity) or have used methods that are subject to reverse causality (raising concerns about internal validity). Prior work has also ignored the role of simultaneous intra-household allocations of time and car access. In this paper I address those limitations and measure the effect of household car ownership on the employment of its members by exploiting a previously overlooked source of exogenous variation in the cost of car ownership: changes in state-level insurance rate regulation.

Studies of insurance regulation show that insurance rates have tended to be lower in states that require insurers to obtain "prior approval" from the state insurance commissioner before implementing rate changes. Using Consumer Expenditure Interview (CE) Survey data from the period 1984-2006, I find that while rate-suppressing insurance regulation has no impact on whether families own a car, regulation generates a two-percentage-point increase in multi-car ownership rates. This effect is concentrated among married parents of young children.

Although the CE Survey is the largest annual survey of car ownership available over a long period of time, it is still too small to identify a reduced-form relationship between rate regulation and labor supply. The March Current Population Survey (CPS) is substantially larger, but it does not measure car ownership. I resolve these data limitations by combining data from both surveys to construct a two-sample instrumental variables (2SIV) estimate of the effect of car ownership on labor supply.

Just as the effect of rate regulation on car ownership is driven by married couples with children in the CE Survey, I find that in the CPS data the association between rate regulation and employment is also strongest among married couples with children. Interestingly, the ownership of the second car in the household has divergent effects on labor supply within these households. The second car increases the father's probability of employment, while it decreases the employment of mothers. This latter result stands in contrast to the previous literature on urban labor markets, which uniformly predicts that easing spatial frictions will increase labor supply.

One potential explanation for these results is that cars are not only useful for getting to work, but they also increase the productivity of time spent in household production. As I demonstrate below, mothers are disproportionately responsible for family-related vehicle trips, especially for child care purposes, and this disparate responsibility is larger in families that own a second car.

This paper is part of a larger literature linking transportation to job market success. This literature began with Kain's (1968) seminal work on the "spatial mismatch hypothesis," which argued that persistent inner-city unemployment is a result of racial discrimination in housing markets which

separates minorities from fast-growing job opportunities in the suburbs. In the mid-1990s an offshoot of this work called the "automobile mismatch" hypothesis claimed that insufficient access to a private automobile is also an important spatial barrier to employment.

It is easy to motivate the automobile mismatch hypothesis in that employment rates are much lower for those who do not own a car. Some proponents of this hypothesis have called for programs to subsidize car ownership in order to increase labor supply.[1] Despite these claims, the basic correlation that motivates the hypothesis is potentially contaminated by reverse causation: employed individuals have more income and are thus more able to afford a car than the unemployed.

For this reason, a subset of the automobile mismatch literature examines whether exogenous changes in the cost of car ownership also alter employment. This approach enhances the internal validity of estimates of the effect of vehicle ownership on labor supply, but it also makes the estimates more relevant to policy choices. Many policies change the cost of car ownership, from emissions controls to wheel taxes. If cars are important for connecting workers to jobs, such policies have unintended consequences in low-skill labor markets. I contribute to this subset of the literature in two ways.

First, given the difficulty in finding exogenous variation in car ownership to identify the models, many studies are forced to restrict their analysis to case studies, generating results with questionable applicability in a more general setting. I address those concerns and measure the effect of car ownership on employment in a nationally representative sample by exploiting a previously overlooked source of exogenous variation in the cost of car ownership: changes in state-level insurance rate regulation.

Second, previous studies have neglected the simultaneity of household decision making. Both cars and wage earnings are often shared among household members, so regressing individual employment on exogenous variation in individual car ownership yields estimates that might not support their interpretation as a partial treatment effect. An individual's car ownership may discourage other household members from buying their own car, while allowing them to use it to commute or search for jobs. If wage earnings are shared, labor supply responses of household members will also be co-determined. An estimate of the individual response to an exogenously assigned vehicle must then be interpreted as the unconditional, total effect after these internal adjustments. For example, a car may affect its owner's labor supply even if the owner hardly uses the automobile herself. If members of the same household are also included in the study as independent observations, the estimate is even further afield from its typical interpretation as measuring the effect of car ownership on car owners. Prior work

[1] These include Smeeding (1993), Ong (1996), O'Regan and Quigley (1998), Ong and Blumenberg (1998), and Raphael and Stoll (2001), among others.

has considered neither secondary car ownership nor the labor supply of secondary wage earners, such that parameters have been either misestimated or misinterpreted, depending on one's point of view.

Although primary car ownership and the labor supply of primary earners are certainly important outcomes, the results of this paper imply that they are fairly unresponsive to policy choices. The decision to buy a second car, on the other hand, is more sensitive to policy. Cars are generally shared unequally across household members; the second car is used disproportionately by secondary earners. Since secondary earners often account for a significant proportion of a low-income households' wage earnings (Cattan, 1998), secondary car ownership and secondary earners might be the mechanisms through which policies affecting ownership costs have their largest effects on poor families' well-being.

Previous studies have only considered how vehicles increase access to jobs, overlooking the usefulness of vehicles in home production and thereby missing an avenue through which car ownership can actually lower labor supply. Much of home production is now accomplished outside the home, so a private vehicle can dramatically increase the productivity of time spent in household production, encouraging exit from the workforce. If policies affecting car ownership have their largest effects through secondary cars and secondary earners, then considering household production may be crucial in understanding the effects of those policies. This paper is the first to test for the impact of the second automobile on the intra-household allocation of time.

The rest of the paper is organized as follows. The following Section II provides some background on the previous literature concerning transportation barriers to work, as well as the relevance of multiple-car ownership for household time allocation. Section III describes the data used in this study, and proposes a new source of exogenous variation in the cost of car ownership. State auto insurance rate regulation lowers the cost of car ownership by suppressing insurance premiums. If car ownership improves access to employment, then in reduced-form models we should see an increase in employment after cost-reducing legislation is passed. Section IV presents the results of the analysis. Section V discusses other sources of exogenous variation, some of which have been used in previous studies on this question. I find that they are unfortunately too weak to be useful as instruments in the context of the CE Survey. Section VI concludes.

II. Background

II.A. Previous Literature Linking Transportation to Work

A commonly cited barrier to employment among the urban poor is a lack of reliable transportation. The argument suggests that as metropolitan areas continue to sprawl outward, inner-city residents find themselves more spatially isolated from high-growth areas because the transit systems they

rely on are increasingly unable to connect inner-city low-skill labor with vacancies scattered throughout low-density suburbia.

This is not a new concern. Kain (1968) was the first to propose this "spatial mismatch hypothesis" which suggested that a major explanation for low rates of employment among low-skill, inner-city black residents in Chicago and Detroit is that racial discrimination in housing markets restricted them from changing residential location to match the outward movement in the spatial distribution of low-skill labor demand from the central cities to the suburbs. Kain (1968) documented the shift in the location of manufacturing establishments, but low-skill labor demand has shifted from manufacturing to the service sector, which has grown faster in the suburbs than in central cities (e.g. Stoll et al., 2000). Since Kain's seminal work, the hypothesis has been generalized to attribute residential concentration of Hispanics as well as blacks to housing discrimination in US cities at large rather than just Chicago and Detroit (e.g. Raphael and Stoll, 2001). A large literature developed that is dedicated to testing this more generalized spatial mismatch hypothesis suggesting that geographic differences between residential location and areas of rapid job growth can help explain persistently high unemployment in U.S. central cities.

From the beginning of this literature, Kain and other authors noted the importance of transportation in that spatial separations are only relevant insofar as they affect the amount of time spent commuting or searching for work. It was not until the mid-1990s, though, that authors began to focus on transportation mode as a distinct explanation. Raphael and Stoll (2001) document wide disparities in car ownership across racial and ethnic groups, comparable in magnitude to gaps in home ownership rates. Taylor and Ong (1995) note that commuting distances were similar across races, compared to the wide dispersion in commuting times associated with differences in transport mode.

After four decades of study, no consensus has yet been reached regarding the relevance of housing discrimination, and the academic discussion has moved increasingly towards an analysis of mode choice. The offshoot "automobile mismatch"[2] hypothesis emphasizes that the low densities of suburbs imply that no matter where one lives in a metropolitan area, a car is essential for finding and keeping a job.

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[2] Automobile mismatch is also sometimes known as modal mismatch or transportation mismatch. Some authors regard the automobile mismatch concept as a subset of the spatial mismatch hypothesis, interpreting the spatial mismatch hypothesis to be the idea that commuting costs cause unfavorable labor market outcomes. Blumenberg and Manville (2004) and Grengs (2010) provide extensive surveys, both with this view. Taylor and Ong (1995) coin the phrase "automobile mismatch", and they regard it to be mutually exclusive of the spatial mismatch hypothesis, finding that the commuting patterns of blacks and Hispanics in segregated neighborhoods are similar to those of suburban whites, conditional on car ownership. In this paper I adopt the moderate view of Raphael and Rice (2002), among others, treating the two conjectures as independent apart from their mutual concern with spatial frictions in urban labor markets.

Anecdotal evidence supports the idea that cars are necessary for employment whether a family lives in the suburbs or in the city. As part of the Moving to Opportunity (MTO) demonstration program, public housing residents were experimentally relocated to lower-poverty area. Evaluations find no impact on the employment levels of experimental households (Kling et al., 2005; Kling, Liebman, and Katz, 2007).[3] In subsequent interviews with relocated households, lack of personal transportation, even after a move, is a commonly cited impediment to employment (Turney et al., 2006):[4]

> "Terry, a 33-year-old experimental, discusses how transportation issues often result in her being late to her job as a school nurse at an elementary school in Baltimore. 'The bus driver, she was late one day and then the next day she didn't come at all. … I am at the point where I am ready to buy a car,' she says, but gets depressed because she cannot afford car insurance."

Many policies change the cost of driving; if car ownership is a key missing ingredient to economic success, such policies may have unintended effects on labor markets. For example, many means-tested transfer programs assess personal automobile assets when determining eligibility, including TANF and SNAP[5] (Sullivan, 2006; Bansak, Mattson, and Rice, 2010; Baum, 2009; Super and Dean, 2001), possibly decreasing the incentive to own a car and hence, reducing employment prospects. Likewise, emissions regulations, fuel efficiency requirements, and gasoline formulation standards all make owning an older used car more expensive. Even the government's recent involvement in the auto industry itself can have effects on car ownership; the "Cash for Clunkers"[6] program may have increased the prices of used cars by requiring that cars traded in for credit be permanently disabled, reducing the supply of used vehicles.[7]

The most direct policy intervention, perhaps, is in the form of subsidies for highways and transit, which change the relative prices faced by households choosing between private and public transportation.

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[3] Quigley and Raphael (2008) reassess this assessment. Estimating a structural model of spatial mismatch for comparison, they argue the experimental variation in neighborhood characteristics effected by MTO was too small to generate enough statistical power to reject small or moderate effects on employment levels.

[4] The interviews were conducted in Baltimore, however, and the authors report that the health care jobs in which half the employed experimentals reported working were more likely to be located in the city of Baltimore than in the surrounding counties.

[5] Temporary Assistance to Needy Families (TANF) was the replacement for the Aid to Families with Dependent Children (AFDC) welfare program. Supplemental Nutrition Assistance Program (SNAP) is the new name for the Food Stamps Program.

[6] "Cash for Clunkers" was renamed the Car Allowance Rebate System. From July 27 to August 25, 2009, vehicles under 25 years old getting <18 miles per gallon (or heavy trucks of any fuel economy older than 2001) could be traded in for scrap value and a $3,500-$4,500 voucher towards a new vehicle with a base price under $45,000 and with a minimum fuel economy (22 mpg for passenger automobiles). 677,842 vehicles were scrapped and $2.85 billion was paid in credits. Other countries as well as Texas and California had previously implemented similar programs (http://www.cars.gov/files/official-information/CARS-Report-to-Congress.pdf).

[7] Anecdotally, the Consumer Price Index reports that used car prices in September 2010 were 12.9 percent higher than those of September 2009, the largest jump of any expenditure category. A portion of this increase is probably due to the recession's effect on household income, as used cars are inferior goods.

Glaeser, Kahn, and Rappaport (2008) provide evidence from the 1980, 1990, and 2000 Censuses that the opening of a new light rail station induces the relocation of low-income, low-skill residents to its neighborhood.[8] This suggests that for many poor households, the cost of relocating can be lower than the cost of car ownership, so disparities in car ownership are an important source of the residential segregation observed by Kain and others. Holzer, Quigley, and Raphael (2003) document that when the heavy rail system was expanded east of Oakland to high-growth, predominantly white suburbs, firms located near new stations soon increased their hiring of minorities. These results can be interpreted as evidence against the residential location choice frictions required by the spatial mismatch hypothesis in favor of the importance of disparities in car ownership rates for explaining the persistent unemployment of urban, low-skilled workers.

Early studies have shown that those who own cars are much more likely to be employed. Interpreting this relationship is difficult, though, because car owners are not randomly selected in the population. Employed individuals have more income and are thus more able to afford a car, so the correlation between the two variables may be due to causation in the opposite direction—i.e., perhaps employment allows one to buy a car. As an illustration, Figure 1 demonstrates that among single mothers with less than a college degree, the time series of car ownership and employment are highly correlated; this can be explained, however, by the enactment of welfare reform in the mid-1990s, which increased work for low-educated single mothers. Alternatively, a third unobserved variable could affect both car ownership and employment in the same direction, leading to a spurious correlation. For example, documentation of legal immigration status may help one both in buying and financing a car and in obtaining employment.

Policy tools for ameliorating spatial mismatch can vary along three dimensions: community development (moving jobs to inner city), residential mobility (relocating low-skill workers out to jobs), and transportation programs (decreasing the reverse commuting costs of inner-city workers). The third of these can be split further divided between mass transit subsidies and subsidies for car-centered development. Connecting workers to jobs has long been a goal of transit, but many authors have claimed that the nature of sprawl requires personal transportation. On the basis of ordinary least squares (OLS) estimates, several authors have called for subsidies for car ownership among the poor (Ong, 1996; Ong and Blumenberg, 1998; O'Regan and Quigley, 1998). Smeeding (1993) suggests "car stamps," vouchers that recipients can put toward the price of a car. TANF regulations explicitly allow for local authorities to

[8] Turney et al. (2006) also report that many MTO experimental interviewed soon moved out of their restricted, low-poverty, placement neighborhoods into poorer neighborhoods in order to be closer to bus and train lines that ran more frequently.

use TANF funds for "Wheels to Work" programs, and such programs are now operating in a majority of states (Goldberg, 2001).

Several studies focus on the causes and consequences of car ownership for welfare recipients, as the shift from AFDC to TANF put greater emphasis on increasing recipients' labor force participation. In a survey of TANF recipients in Los Angeles County, Ong (2002) finds that insurance premiums vary substantially across geographic regions. He then demonstrates that car ownership is lower and joblessness higher in high premium areas. Gurley and Bruce (2005) analyze the Family Assistance Longitudinal Survey of welfare recipients across Tennessee. The panel data allows individual fixed effects and for the authors to distinguish between prior car ownership and subsequent transitions into employment. Gurley and Bruce find that gaining car access is associated with a 21 percentage point increase in the probability of gaining employment and leaving welfare, but as they note, this approach does not completely eliminate simultaneity concerns.

Other papers exploit plausibly exogenous state-by-state slackening of vehicle asset tests in the AFDC and TANF welfare programs, a strategy that potentially identifies a treatment effect of car ownership. Sullivan (2006) finds in the Survey of Income and Program Participation (SIPP) that vehicle asset exemptions had a measurable effect on the probability of welfare recipients owning a car, and Bansak, Mattson, and Rice (2010) find little evidence that it increased their probability of employment. Baum (2009) uses the same methods in the National Longitudinal Survey of Youth to identify a positive effect of car ownership on labor supply.

Raphael and Stoll (2001) employ the 1991, 1992, and 1993 SIPP to estimate the employment effects of moving to car ownership. They show in a difference-in-difference framework that the correlation between car ownership and employment is strongest for blacks, moderate for Latinos, and weakest for whites, which mirrors the relative spatial isolation of these groups. They also demonstrate that these differences in correlations are wider in cities with more severe segregation. They concluded that differential car ownership rates may explain differences in employment rates across racial and ethnic groups.

Raphael and Rice (2002) is the only national study (beyond those aforementioned restricted to welfare recipients) that attempted to isolate the impact of car ownership on employment using plausibly exogenous variation in car ownership. The authors documented that states with lower insurance premiums had higher rates of car ownership and higher employment rates, suggesting a causal relationship between car ownership and employment. Unfortunately that paper utilized only cross-state variation in premiums and car ownership rates to identify the model, possibly subjecting the model to an omitted variable bias: states with high car ownership rates have lower insurance premiums. I show in Section V that the results of Raphael and Rice (2002) are not robust to the inclusion of state fixed effects.

II.B. Cars and Home Production

More than 80 percent of all vehicle trips taken are for non-work purposes, but most of the literature on car ownership (and especially its effect on labor supply) has focused on the impact of car ownership for commuting. Such a narrow focus on journey to work misses the important role of the automobile in home production. Expanding the model of time allocation to include home production changes the prediction of the impact of a decline in the cost of car ownership on labor force participation from being unambiguously positive to being ambiguous. The sign of the effect instead depends on how much car access reduces the fixed time cost of going to work compared to how much it increases the marginal productivity of time spent in home production.

II.B.1 Motivating Framework

A unitary model of household decision making with identical workers and diminishing marginal utility in consumption and leisure will imply that the optimal time allocation is the same for each household member. Suppose instead that the household pays a fixed time cost in commuting for each member that works. If both members work, the household pays a higher fixed cost than if one household member works. This fixed cost creates a non-convexity in the household's budget set such that for some preferences, it is optimal for one worker to incur the commuting cost and work outside the home while the other avoids the commuting cost by withdrawing from the labor market. For any given set of preferences, higher commuting costs have an unambiguously negative effect on the extensive margin of labor supply; as commuting costs increase, households go from two workers to one worker to none.

As is typical in home production models, suppose that the final consumption good is produced by combining two intermediate goods: market wage earnings and home production. The first of these intermediate goods is income collected from time spent in the labor market. The other intermediate good is produced with time spent in home production, combined with household capital inputs like housing, appliances, tools, and cars.

Mode choice can be introduced by allowing the household to exchange some of the market intermediate good to buy a car, which enters into the household's production function in two ways. The first way is that is decreases the fixed time cost of labor force participation. The second way is that it increases the marginal productivity of time spent in home production. A change in the price of cars affects the optimal time allocation through both channels, but the sign of that impact is ambiguous.[9] The

[9] As I document below, home production is now increasingly performed outside the home. Buying groceries, picking up children from school, shopping, transporting children to doctors or activities, dining at restaurants, *etc.*, are all non-market activities which are often easier with a private automobile. Splitting time spent in home production into two subcategories (inside home and away from home) yields an unambiguous, testable hypothesis

key implication is that if the elasticity of substitution between cars and time spent in home production is sufficiently small, and the factor by which cars reduce commuting time is also small, then lower car ownership costs can increase the specialization of workers within a household.[10]

This view of car ownership fits into a large and growing literature on household time allocation and the household production function.[11] Some of these studies model capital inputs to the household production function, but almost all of these assume capital is a substitute for time spent in home production.[12] The only paper that explicitly allows for capital inputs to be complementary to home production is Baxter and Rotz (2009). They examine the differential expenditure patterns of one- and two-earner married couples to identify which roles different goods play in the household production function. The authors note that *a priori* the theoretical effect of labor supply on car ownership is ambiguous since the elasticity of substitution is unknown.

II.B.2 Stylized Facts: The Automobile as an Input to Home Production

Table 1 illustrates a number of key points about the importance of cars as an input to both labor supply and home production. This table reports results from the 2001 National Household Travel Survey (NHTS). Conducted periodically by the U.S. Department of Transportation over the past 40 years, the NHTS is the "nation's inventory of personal travel." Survey respondents provide data on all trips taken in one 24-hour period in 2001, including the purpose of the trip, mode, time, place, and distance. If the trip occurs in a personal automobile, data is also collected about all the occupants and vehicle characteristics. Data is collected from 69,817 households and 160,758 people. I report results in Table 1 for married couples living alone or with their own children**.** Each column reports the mean number of trips by car per day. In separate columns I generate results for three subsamples: all families, families with one car, and families with two cars. For all subsamples I report separate estimates for husbands and wives as well as the ratio of these two values and its standard error.[13]

The numbers in the table generate the two key stylized facts about car travel outlined in the previous section. The first fact is that a second car is correlated with wives' increased mobility. The second fact is that the increased mobility afforded by the second car is associated with differential responsibility across genders for home production, and in particular differential responsibility for

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that a decrease in the price of a second car should increase the home production done outside the home. Data availability prevents me from testing that hypothesis in this paper.

[10] This also depends on home goods being substitutes for market goods in the production of the final consumption good (Jones, Manuelli, and McGrattan, 2003).

[11] Aguiar and Hurst (2007) provide a survey of this literature.

[12] Greenwood, Seshadri, and Yorukoglu (2005); Coen-Pirani, Leon, and Lugauer (2010); Cowan (1983).

[13] Standard errors on the ratio are calculated by the delta method.

childrearing. When no children are present, an extra car has no impact on the number of trips taken, whether by men or by women.

In Panel A of Table 1, I report average trips for families with and without children. Wives take 12% more trips in multi-car families than in single-car, whereas husbands' trips are unchanged. In one-car families without children males take 8 percent more trips than females, but in one-car families with children there is no difference between genders. The basic results are unchanged in two-car families without children, but note that in two-car families with children the number of trips for women increases considerably to 5.4 trips per day and men are making 11 percent fewer trips per day, a difference that is statistically significant at conventional levels.

In Panel B, I report the average number of car trips per day by purpose. I report results for three broad purposes: driving to and from work, for family or personal reasons, and for serving particular passengers in the car. This last group is a subset of family/personal reasons trips, and it includes trips like taking children to soccer practice, doctor's appointments, or picking children up from school. Not surprisingly, men are taking more car trips for work purposes in all family types and in one- and two-car families.

Among one-car families without children, there is no difference between husbands and wives in the number of trips made for household care. However, this changes considerably by adding children or a second car. In families with children and one car, husbands make 21 percent fewer trips for family/personal reasons. In two-car families, husbands make 8 percent fewer trips without children in the household but 34 percent fewer trips in households with kids.

Note that moving from one- to two-car families, wives without children are actually driving slightly less (1.76 versus 1.70 trips per day). In contrast, wives with children are making 14 percent more trips in households with a second car than wives in households with only one car. The second car only makes a difference if children are in the house.

A large fraction of the family trips taken by both husbands and wives are serving a passenger in the car. If no children are present, husbands and wives make similar numbers of these trips. With children, however, wives are making many more of these trips. In households with both a second car and children, wives serve as chauffeurs at double the rate of their husbands.

The importance of the second car for married mothers is most easily demonstrated in Panel C, where I report estimates by the labor force status of the wife and by whether children are present in the car. In this group of results, I include only households with children and in which the husband is employed. In families where both parents work, the numbers of trips without children in the car are very similar for both one- and two-car families. Notice however that in both one- and two-car families, men take about 40 percent fewer trips with children in the car than women take. For working mothers, the

addition of the second car is associated with a 16 percent increase in the number of trips with children (1.41 versus 1.22). In households where the mother does not work, the addition of the second car is associated with a 30 percent increase in trips with children (2.21 versus 1.70). In households where both parents work, a second car shifts both men's and women's trips toward children.

These results in Table 1 show that the positive association between multi-car ownership and women's travel is much stronger when children are in the house. This interaction suggests that a second car may be a complement to home production and may increase specialization in the household division of labor. A decline in the cost of car ownership can reduce the cost of home production and encourage exit from the workforce.

III. Methods and Data

In this section I examine the impact of car ownership on labor supply using arguably exogenous variation in car ownership generated by state regulation of insurance rates. As I outline below, the primary data set for car ownership is the Consumer Expenditure Interview Survey (CE Survey). This sample has a number of distinct advantages, but it is a relatively small data set compared to many others, and the fundamental cost of any two-step estimation procedure is a large reduction in precision. As a result, I employ the two-sample instrumental variables method developed by Angrist and Krueger (1992, 1995) to augment the CE Survey with a much larger sample from the March Current Population Survey (CPS).

III.A. Background on Auto Insurance Rate Regulation

Every state has an elected or appointed insurance commissioner whose job is to oversee regulation of the insurance industry in that state. This devolution of regulation to the state level is the result of the McCarron-Ferguson Act of 1945, which protected insurance cartels ("rating bureaus") from anti-trust enforcement in exchange for increased regulation of the industry by the states. Over time states diverged substantially in their chosen forms of regulation, ranging from direct, explicit price setting to near-total deregulation.

Although these regulatory systems differ on many dimensions, previous literature studying the impacts of these regulatory regimes has coalesced around "prior approval" laws as a reliably monotonic

indicator of the intensity of state regulation.[14] "Prior approval" laws require each insurer to obtain "prior approval" from the state insurance commissioner before implementing any change in its rate structure.[15]

Over time states have both enacted and repealed rate regulating legislation, allowing researchers to test for its impact on prices. Consensus has emerged that, at least since the 1970s regulation has restricted automobile insurance prices and insurer profits below their competitive levels (e.g., Harrington, 1984 and 1987; Grabowski, Viscusi, and Evans, 1989; Harrington, 2002).[16]

Table 2 shows the states in my sample that enact or repeal such rate-regulating legislation during the sample period. These states are located in every area of the country, and the law changes are similarly scattered across time periods. Some states enact "prior approval" regulation while other states deregulate, while still others have multiple regime changes. There does not seem to be any pattern to which states change their regulatory regime in which years.

There are other aspects of state insurance laws that can potentially be used as variation in the cost of car ownership. One is whether a state has a no-fault insurance system. Under no-fault, drivers and their passengers are covered by the driver's own insurance regardless of who is at fault, and drivers have limited ability to recover damages from other insured drivers. Previous research[17] has demonstrated that no-fault insurance changes the cost of insurance, but few states changed their compensation regimes over the sample period, and preliminary investigations for this paper found that no-fault laws did not generate enough variation in car ownership to identify the first stage.[18]

Rate regulation laws, on the other hand, may be more promising instruments. Although we cannot rule out the possibility of the enactment of such laws depending on the macroeconomic condition of the state, the inclusion of time effects would remove all but idiosyncratic shocks to the state, not shared by the rest of the states in the sample for that year. Using premiums data from the National Association of Insurance Commissioners and data on prior approval regulation, and controlling for household characteristics and state and year effects, I find that prior approval rate regulation is associated with a decrease in average annual premiums[19] of $84.46 per year, or 11.8 percent.[20] This suggests that rate regulation is in fact affecting car ownership rates through the presumed mechanism.

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[14] This dichotomous distinction was originated by Hartman (1970).
[15] Many of these studies also note that since such price controls are associated with reduced availability of insurance in the private market, the size of the residual shared market can also be used as a continuous index of regulatory strictness (e.g. Grabowski, Viscusi, and Evans, 1989). I investigate this variation in Section VI.
[16] Litan (2001) provides a succinct survey of these and other analyses.
[17] Schmit and Yeh (2002) survey this literature.
[18] I further discuss no-fault laws and other potential sources of variation in Section VI.
[19] Like Raphael and Rice (2002) I use average premiums expenditures (the ratio of premiums collected to car-years insured) rather than inverse loss costs (premiums divided by payouts) as the relevant measure of price because it seems more likely that the absolute dollar price paid for a policy is a more relevant variable for poor households than a per-unit cost scaled by the expected payout. Liability premiums would be an even better measure, and

III.B. Two-Sample IV Method

The goal of this paper is to examine the impact of car ownership on labor supply. As I outline below, the data are repeated cross sections that vary over time and states, and the unit of observation is a household. Therefore, the basic equation of interest can be described by a linear probability model of the form

$$(1) \qquad E_{hst} = O_{hst}\beta_1 + \mathbf{X}_{hst}\beta_2 + u_{1s} + v_{1t} + \varepsilon_{1hst} \ ,$$

where E_{hst} is an indicator for the employment status of the head (or spouse, in some specifications) of household h in state s responding in year t, O_{hst} is an indicator for whether the household owns a car, $\mathbf{X}_{hst}$ is a vector of household characteristics (some of which are themselves member characteristics, such as education of the head or age of oldest child). The three-part error structure captures fixed state (u_{1s}) and year effects (v_{1t}) plus an idiosyncratic error (ε_{1hst}).

OLS estimates of equation (1) are unlikely to produce consistent estimates of the impact of car ownership on labor supply. One problem is omitted variable bias, in that some characteristics correlated with both O_{hst} and E_{hst} are unobserved and thus omitted from $\mathbf{X}_{hst}$.

Some of these omitted variables are obvious. For example, health and physical conditions such as poor eyesight may make it difficult for an individual to obtain a driver's license and to work. Most nationally representative data sets have limited ability to measure such covariates, and although the CE is a panel data set, the short time frame of the panel means there is not enough variation in car ownership within the panel to exploit this dimension of the data.[21]

State fixed effects control for any variables that are constant for all households within each state over the time period by including state fixed effects. This includes fixed attributes like the climate and topography of the state, the public transportation system, the highway system, etc. I similarly control for any unobserved macroeconomic shock within a given year, to the extent that it affects all states equally, using year effects.

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although I explore this variable in Section VI, data is not as widely available for liability premiums as for average premiums. Still, the two variables are highly correlated.

[20] State average premiums are matched to households in the CE Survey, in order to be able to control for changes in the composition of households in a state. Sample weights are used to adjust for sample stratification. The *p*-value on the estimate is equal to .010, with standard errors clustered by state. Results are available by request.

[21] I have at most four observations for each household. Each household is interviewed for consecutive five quarters then rotated out, but the first interview is intended to set a baseline and is not reported. Many households are interviewed for fewer than four quarters if they decline to continue to participate, if they move to a new residence without informing the BLS, or if they leave the sample for any other reason.

Simultaneity bias or reverse causality is, however, the most likely problem with an OLS model. Having a job provides one the income and access to credit necessary for buying and maintaining an automobile. The only way to isolate the causal effect of car ownership on employment is to either manipulate car ownership directly or to isolate some variation in car ownership that is plausibly uncorrelated with the error in the employment equation.

Very few datasets contain each of the three variables (car ownership, employment status, and state-year identifiers defining rate regulation) required for IV estimation. The SIPP is one of the few that do, and many of the aforementioned papers use it for this reason, but it covers a limited period of time. The CE Survey also measures all three variables across three decades, but the sample is still too small to detect an employment effect.

To overcome this data availability limitation, I use the two-sample instrumental variables strategy (2SIV) first developed by Angrist and Krueger (1992, 1995). The idea is to use one dataset to estimate the first-stage effect of the instrument on the endogenous regressor, and use another dataset to estimate the reduced-form effect of the instrument on the outcome of interest, which is assumed to work only through the instrumented endogenous covariate. In this case, the first-stage equation is of the form

$$(2) \qquad O_{hst} = R_{st}\,\pi_1 + \mathbf{X}_{hst}\,\pi_2 + u_{2s} + v_{2t} + \varepsilon_{2hst},$$

where all variables are defined as above and R_{st} is a dummy variable that equals unity if state s in year t has prior approval legislation and zero otherwise. This equation will be estimated with data from the CE Survey. The reduced form equation is defined as

$$(3) \qquad E_{hst} = R_{st}\,\theta_1 + \mathbf{X}_{hst}\,\theta_2 + u_{3s} + v_{3t} + \varepsilon_{3hst},$$

and the equation will be estimated with data from the March Current Population Survey. Finally, since the model is exactly identified, the two-sample instrumental variables estimate is simply the ratio of the reduced form estimate to that of the first-stage on the instrument R_{st}, or

$$(4) \qquad \hat{\beta}_1 = \hat{\theta}_1 / \hat{\pi}_1.$$

I derive standard errors for the 2SIV estimate by using a delta method technique developed by Dee and Evans (2003).[22]

III.C. Data

The CE Survey is a nationally representative, rotating panel survey administered quarterly by the Bureau of Labor Statistics. Its main purpose is to provide the consumption bundle over which the Consumer Price Index is computed to measure inflation. Each of approximately 7,600 addresses of "consumer units", defined broadly as individuals who pool their incomes and make expenditure decisions jointly, are interviewed for five consecutive quarters and then replaced. The first of the five surveys is a reference survey so that new purchases are assigned to the correct quarter.[23]

The CE Survey data include expenditures that respondents could be expected to recall for three months or more, household assets, and demographic characteristics of household members. Although each household is interviewed on several occasions, I treat each year as a repeated cross section of households. Each observation represents one household's response in one quarter, so the number of observations per household is equal to the number of interview responses.[24] Since observations for each household are likely to be highly correlated across time, unadjusted OLS standard errors would under-estimate the variance of the distribution of the coefficient estimates. Accordingly, I adjust the standard errors to allow for arbitrary correlation across observations within each state.

I combine data from survey years 1984 to 2006, generating panels of repeated cross sections that vary across consumer units, states, and years. I account for sample frame changes in 1986, 1996, and 2006.[25] Sampling weights were used in all regressions, although all of the results in this paper are qualitatively similar when they are not used.

The CE Survey recodes state identifiers for some observations in order to protect respondent anonymity. I drop any household with a recoded state identifier. In any state-year cell where recoded state identifiers are not specifically flagged as such, all observations are dropped. About a quarter of the sample (136,036 households out of 553,749) is dropped for this reason.

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[22] Assuming zero covariance between estimates for $\hat{\pi}_1$ and $\hat{\theta}_1$ and using the delta method, one can show that the squared t-statistic on the 2SLS estimate in equation (4) is approximately equal to $t(\hat{\beta}_2)^2=\{[1/t(\hat{\pi}_1)]^2+[1/t(\hat{\theta}_1)]^2\}^{-1}$.

[23] Excellent descriptions of the CE Survey can be found in Meyer and Sullivan (2007), (2004), and (2003).

[24] Typically each household provides four responses, but a sizable fraction of households do not complete all four surveys (after the initial baseline survey).

[25] Specifically, the first quarter of each year's survey overlaps with the fifth quarter released in the previous year. For example, the data for 1992 includes the four quarters of 1992 in addition to the first quarter of 1993. Usually these overlapped quarters are identical, but in years in which the sampling frame was changed, the two quarters can include different observations. I solve this by extracting all quarters (including overlapping first quarters) and removing duplicated observations.

Cars and market wage earnings are often shared within each household,[26] so there is potential for simultaneous determination of the labor supply of the primary and secondary earners within each household. To capture these intra-household dynamics I treat the household as the unit of decision-making, rather than the individual. Each observation represents one interview completed by one household. When examining the decisions of married couples, I restrict the sample to households consisting only of married couples living alone or with only their own children.[27]

I count an individual as employed if he or she has been working for pay in the past twelve months. I count personal vehicles for the purposes of household car ownership as cars, trucks, minivans, vans, or sport-utility vehicles: motorcycles and mopeds do not count. Race and ethnicity variables assign "white" and "black" categories as non-Hispanic white and non-Hispanic black. Education variables have slight changes in the CE Survey at 1996. Before 1996, "less than high school" is defined as anyone attending 11 years of school or less; "high school graduates" are anyone who has attended the 12th grade through three years of college; and "college graduates" are those who have attended four or more years of college. From 1996 onward, those who attended 12th grade but did not receive a diploma are categorized as "less than high school"; "high school graduates" include respondents with some college education but no degree and anyone with an Associate's degree, and "college graduates" are holders of Bachelor's degrees.

The reduced-form model is estimated using data from the Integrated Public-Use Microdata Series (IPUMS) March Annual Demographic File and Income Supplement of the Current Population Survey (CPS). The CPS is a household-based survey collected monthly by the Census Bureau and the Bureau of Labor Statistics and it provides data on basic labor market outcomes such as the monthly unemployment rate. Each March, CPS respondents also complete the Annual Demographic File and Income Supplement which asks detailed questions about employment in the previous year as well as questions on current health insurance status. The IPUMS versions of the March CPS are harmonized over time so that variables are defined in a consistent way. From the March CPS, I extract a sample that includes the same year and state groups available in the CE Survey.

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[26] Indeed, the BLS defines a "consumer unit" as a set of individuals sharing a substantial proportion of household expenditures. Also, preliminary estimates for this paper showed that single-person households were no more likely to purchase an additional car, as one would expect since an individual can only use one car at a time. Unmarried multi-person households also did not buy additional cars. This may be explained in that cars have high operating costs that can vary significantly by the intensity of usage. It is difficult for a car owner to monitor the intensity of usage of other drivers borrowing her car, so households may be reluctant to share a car for an extended period without the asset pooling provided by marriage.

[27] "Own children" include stepchildren and adopted children. Earlier versions of this paper did not include this restriction, and the resulting estimates were similar to those produced here, though with smaller confidence intervals. Those results are available from the author by request.

Table 3 shows sample means for both the CE Survey and the CPS. The CPS sample is three times larger than the CE Survey. Both feature similar means for employment and age, although fewer hours of work are reported in the CPS than in the CE Survey. State-defined variables like gas taxes and average insurance premiums are almost identical since the CPS is restricted to the same states and years available in the CE Survey. The racial and ethnic distributions match up well between the data sets, but the CPS contains fewer college graduates.

The variable defining rate regulation laws in each state-year is drawn from the appendix of Grace and Phillips (2008), who extend the earlier work of Harrington (2002). They group laws into eight categories of regulatory strictness. Following Harrington (2002) and most other studies of rate regulation (e.g., Weiss, Tennyson, and Regan, 2010), I separate states into two categories: "prior approval" and "competitive rating". Prior approval laws range from the state explicitly setting insurance rates to insurers at least needing the state insurance commissioner to have an option of disallowing rate changes for some period between when the rates are filed with the state and when they are allowed to be used. Competitive rating regimes, on the other hand, require insurers to file rate changes but then allow insurers to use those rates without getting the approval of the state. As noted above in Section III.A., there is a general consensus in the literature on insurance regulation that prior approval laws suppress insurance premiums, and in my own calculations I find that regulation reduces premiums by 11.8 percent on average.

IV. Results: The Effect of Car Ownership on Employment

Table 4 reports first-stage estimates from the CE Survey. In the first two columns, I report estimates of the impact of rate regulation on whether a family owns any car while in the last two columns, I examine the movement from one to two cars. For each set of results, I report estimates for families with and without children. In separate rows, I generate estimates for the entire sample, and then separate estimates for households based on the age of the husband.

The results in the first two columns of Table 4 indicate that incentives created by prior approval laws for households to buy their first car (or sell their last car, in the case of repeals) are too weak for prior approval to be a suitable instrument on the zero- to one-car margin. In all regressions, none of the first-stage results are statistically significant and most of the estimates are qualitatively small in magnitude. As one example, in families without children present, prior approval regulation is estimated to reduce first-car ownership rates by one percentage point, with a standard error that is the same value.

This result is consistent with a few plausible explanations. First, a large part of the macroeconomic literature on habit formation and durable goods consumption has noted the rarity with which households change adjust their consumption of automobiles, conjecturing that cars are a "lumpy"

investment subject to adjustment costs, perhaps due to the well-known "lemons" problem in used-car markets.[28] There are also several non-cost barriers to owning and operating a vehicle, including getting and keeping a license. If one is prevented from driving by physical limitations or legal prohibitions, marginal changes in the cost of car ownership are unlikely to have any effect. Whatever the reason, it appears that few households are sufficiently close to the margin of car ownership to be induced by mildly cost-reducing policies to change their car ownership status.

Married couples living alone are also shown in column (3) of Table 4 to be just as unresponsive regarding their decision to buy a second car as they are in the decision to buy a first car. In contrast, column (4) of Table 4 shows that married couples living with their own children are more likely to buy an additional car when rate regulation goes into effect. The first entry of the last column indicates that a "prior approval" rate regulatory regime in a given state and year is associated with a 2.4 percentage point increase in the proportion of households owning two or more cars.

Given that prior approval legislation only changes second car ownership rates for married couples with children, I focus on this group when constructing reduced-form and 2SIV estimates. These results are presented in Table 5. The first column of Table 5 presents the first-stage results from the CE Survey and the first three entries of the first column repeat the first stage results from column (4) of Table 4. Each subsequent row of the table represents a different subsample, based on the age of the husband, age of the wife, the relative ages of the spouses, and the age of the oldest child. For each specification I report the coefficient estimate, its standard error, and the sample size. In column (1) we see that the rise in second-car ownership is broadly shared across groups. The only subsample where the first-stage regression is statistically insignificant at a p-value of 0.10 or higher is for families where the oldest child is over 15 years of age.

Columns (2) and (3) of Table 5 report the reduced-form effect of rate regulation on employment for husbands and their wives, respectively. These results are generated from the March CPS sample. Although one might expect the impact of rate regulation (through car ownership) to be heterogeneous among various demographic subgroups, the surprising result is that this effect is not just heterogeneous but *opposite* for members of the *same household*. Among all married couples with children, rate regulation generates a 1.5 percentage point increase in the probability that males work but there is an equally large reduction in the probability that wives work. The result for males is statistically significant at a p-value of 0.05 but the female result at only a value of 0.10.

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[28] Most recently Chetty and Szeidl (2007) show this in the 1990-1999 CE Survey, building on earlier work including Attanasio (2003), Eberly (1994), Bar-Ilan and Blinder (1992), and Bernanke (1985). These lumpy adjustment costs are also present for households on the margin most of owning two cars, but some may be less severe (e.g., they may have an established relationship with a dealer, or have lower costs in assessing vehicle quality through the experience of owning one car).

Columns (4) and (5) show the 2SIV estimates of the effect of multi-car ownership on husbands' and wives' employment. These are estimated simply by dividing the reduced-form by the first-stage estimate. In the first row of these columns, we see that the presence of the second car in the household is estimated to increase employment of husband by 62 percentage points, but to *reduce* employment among wives by 69 percentage points. The standard errors on the 2SIV estimates are very large, such that hardly any information can be obtained regarding the magnitude of the effect of car ownership on employment. This is not an overwhelming concern because in any case the estimated magnitudes must depend on distributional assumptions of the particular specification. The sign of the estimates, though, is generally more dependably estimated. Across all subgroups, the effects on male and female labor supply maintain their opposite signs, but the 2SIV specification often lacks enough power to reject the null hypothesis at conventional confidence levels. Among married couples with young children, though, we observe that the second car has a positive effect on the husband's labor supply and a negative effect on the wife's labor supply, a result that is statistically significant at the usual standards.

In the second and third rows, I separate the sample based on the age of the husband. We see that the effect on employment is substantially reduced for older households. The fourth and fifth rows verify that this is not merely a function of the husband's age, but holds for the age of the wife as well. The sixth and seventh rows compare households by the relative ages of the spouses. This can be thought of as a proxy for bargaining power within the household. We see that effects hardly vary by the relative ages of the spouses.

Evidence from the NHTS presented above in Section II suggests that spousal gaps in travel are larger among families with a second car, and that difference is itself stronger among families with children. Further, much of this effect was concentrated in "serve passenger" trips and in trips with children as passengers, suggesting that shuttling children to various activities and appointments may represent a sizable portion of the usefulness of a second car. In that case, we should expect to see a larger effect on labor supply in families with children who are too young to drive.

The eighth and ninth rows explore this possibility, splitting the sample by the age of the oldest child. The effect of a second car on the mother's labor supply is concentrated in families with young children under the age of 16.[29] This suggests that the gains from increased specialization may be partially due to the ability to shuttle children to various activities. Once children can drive themselves (or the oldest child can partially assume that role), the labor market effects of a second car are substantially diminished.

[29] In most U.S. states the minimum legal driving age is 16.

V. Alternate Specifications

The labor market effects implications of car ownership have been difficult to gauge, at least partially because many events or factors which influence an individual's decision to purchase a car also affect an individual's decision to work, so it is difficult to find a convincingly exogenous source of variation in car ownership. For example, many states have reasonably strict emissions testing laws that impose significant costs on owners of old, inexpensive cars. This may seem like a potential candidate for exogenous variation in car ownership, especially since owners of such vehicles are likely to be marginal car owners. Unfortunately state or local emissions testing laws are often a portion of a package of air quality laws. These simultaneous policies can change the industrial composition of the area and impact the skill distribution of labor demand.[30]

Other potential instruments, though, may plausibly satisfy the exclusion restriction but fail to have any first-stage effect on car ownership. Tables 7 and 8 present first-stage results from several plausibly exogenous determinants of car ownership, as estimated in the CE Survey with state and year fixed effects and individual covariates. Columns (1) and (2) examine the role of compulsory insurance and no-fault laws, respectively, using data from Cohen and Dehejia (2004).

Column (3) exploits variation in the proportion of car-years insured in the "residual" or "assigned risk" markets for drivers who have been rejected at least twice on the private "voluntary" market. Claims in this assigned risk market are shared by all insurers operating in the state, and its size has been consistently found to vary positively with regulatory strictness (e.g. Grabowski, Viscusi, and Evans, 1989). In that sense, it can be thought of as a continuous, ordinal index of the degree of regulatory intervention, potentially providing more variation both across and within states.

In Table 7 the last four columns (4) – (7) follow the method of Raphael and Rice (2002), the only previous study that addresses the endogeneity of car ownership with a nationally representative sample. Gas taxes are obtained from the Department of Energy's *Petroleum Marketing Monthly*, and measures of average premium expenditures are obtained from the National Association of Insurance Commissioners. Unfortunately, I find no evidence of a first-stage effect on car ownership at either the extensive or intensive margins. Together, these suggest an extremely low price elasticity of demand for vehicles.[31] Raphael and Rice use a cross section of states, and in preliminary analyses for this paper I found that without state fixed effects, results emerge that are qualitatively similar to those of Raphael and Rice. This sensitivity to the inclusion of state panel controls suggests that the estimates are entirely driven by

[30] Henderson (1996), Becker and Henderson (2000), and Greenstone (2002) are examples of relatively recent work documenting the effect of the Clean Air Act on industrial composition.

[31] Other possible instruments which I have investigated but do not report here include personal property "wheel" taxes, presence of a 16-year-old age-eligible driver (compared to households with 15-year-olds), and graduated driver licensing laws among households with teenaged children.

permanent cross-state differences, which raises the concern that unobserved permanent geographic differences could cause this method to yield a spurious estimate.

VI. Conclusion

This study examines whether increased access to a private vehicle impacts labor supply decisions of low-income, urban households. In particular, I focus on the potential implications of policies that alter the cost of owning an automobile, both for the decision to buy a second car and for the decision of secondary earners to participate in the labor market. These secondary, intensive, intra-household margins have been previously overlooked in the related literature, but I argue that they are important because these decisions are especially sensitive to policy.

A concern with previous studies using variation in the cost of car ownership to measure the impact of car access on economic self-sufficiency is that costs are likely to be higher in areas with booming local labor markets. In light of these concerns I assess one prominent variant of this approach, which uses variation in the premiums paid for auto insurance. I find that this approach is almost entirely driven by permanent cross-state differences, which raises the concern that such unobserved permanent geographic differences could cause this method to yield spurious estimates.

To address this concern I explain some of these cross-state differences in premiums as a function of insurance price regulation imposed by state governments. Eleven states substantially change the strictness of their regulatory regimes in the study period, and I exploit these changes to isolate plausibly exogenous variation in car ownership rates. In particular, I find that the proportion of households owning two or more vehicles increases in states and years in which insurers are required to submit any proposed rate changes to the state insurance commissioner for approval before instituting them in the market. This result has potentially important policy implications in itself, as these laws are generally not directly intended to change car ownership rates.

I find in the reduced form that these stricter regulatory regimes are also associated with an increase in men's labor supply and a decrease in women's labor supply. Since insurance regulatory regimes could only affect employment through its effect on car ownership, I interpret the ratio of these estimates as the effect of car ownership on labor supply.

When I estimate these relationships separately for different subsamples, I find that one group in particular is driving these results. Married couples with children are the only subgroup that changes its level of car ownership. They are also the only group with the aforementioned labor market responses. This suggests that childrearing may be an important component in understanding the divergent labor market responses within a household. I propose that one possible explanation consistent with the

observed relationships is that a second car substantially increases the productivity of non-market production, particularly non-market production associated with childrearing.

Although there is a growing various literature exploring the role of household capital inputs (e.g. Coen-Pirani, Leon, and Lugauer, 2008; Greenwood, Seshadri, and Yorukoglu, 2003; Jones, Manuelli, and McGrattan, 2003), most of this literature regards such capital as a substitute for household labor, like household appliances. The results above suggest that automobiles differ from most other types of household capital like toasters and microwave ovens not just in the obvious dimension that cars decrease the fixed time costs of labor market participation, but also in that they increase the marginal productivity of labor inputs to household production.

Cowan (1983), an early entrant into the household capital literature, suggests that the decreasing cost of automobile ownership in the early 20th century contributed to a rapid change in the predominant forms of distribution of goods and services. Home production of food, clothing, and health care was quickly replaced by door-to-door home delivery of market substitutes for those goods, and then later by the self-service, centralized distribution most common today.[32] As automobiles became more essential for intra-urban travel (Kahn and Glaeser, 2003), "home production" moved increasingly outside the home itself. Cowan concludes that automobiles probably increased the burden of housework on American women, anticipating this paper's results: "The automobile had become, to the American housewife of the middle classes, what the cast-iron stove in the kitchen would have been to her counterpart of 1850—the vehicle through which she did much of her most significant work, and the work locale where she could most often be found."

[32] That is, at the beginning of the 20th century, food, clothing, and health care was mostly produced at home. This production was replaced by milkmen, mail order, and doctor house calls. Currently these goods are usually distributed at supermarkets, department stores, and clinics, which require more extensive travel on the part of consumers.

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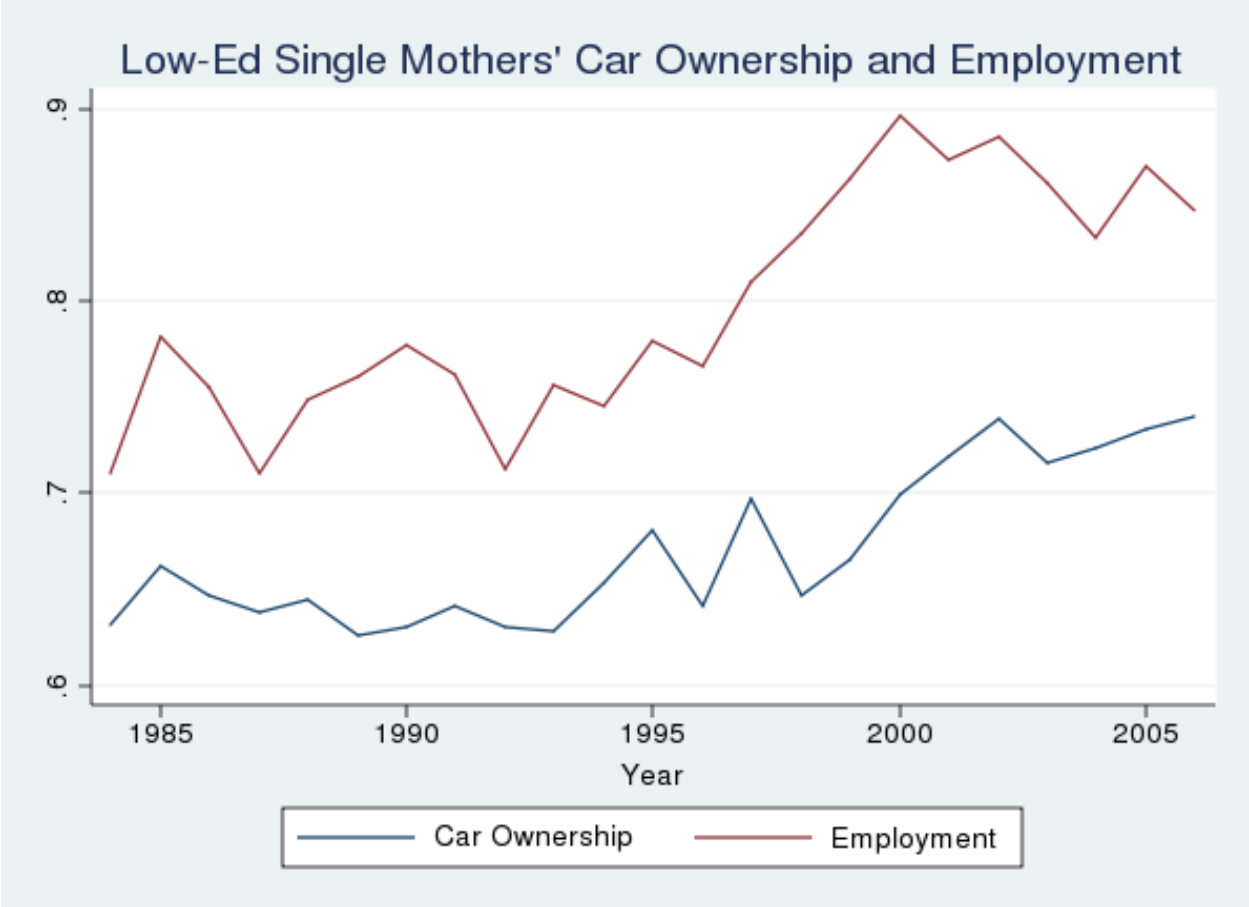
Figure 1
Car Ownership and Employment among Single Mothers without a College Degree



CE Survey, 1984-2006. Sample consists of single mothers without a college degree, living only with their own children. Sample weights are applied.

Table 1, Panel A: Average number of trips per day taken by married couples, by presence of children

	All				One car				Two cars			
Mean number of trips per day	**Husb**	**Wife**	**H/W**	**N**	**Husb**	**Wife**	**H/W**	**N**	**Husb**	**Wife**	**H/W**	**N**
All households	4.69	4.92	0.95	15,540	4.63	4.45	1.04	1,121	4.64	4.92	0.94	9,235
	(0.02)	(0.02)	(0.01)		(0.09)	(0.09)	(0.03)		(0.03)	(0.03)	(0.01)	
No children present in family	4.54	4.41	1.03	7,405	4.55	4.21	1.08	592	4.47	4.38	1.02	4,452
	(0.03)	(0.03)	(0.01)		(0.12)	(0.12)	(0.04)		(0.04)	(0.04)	(0.01)	
Children present in family	4.83	5.38	0.90	8,135	4.73	4.72	1.00	529	4.81	5.42	0.89	4,783
	(0.03)	(0.04)	(0.01)		(0.13)	(0.14)	(0.04)		(0.04)	(0.05)	(0.01)	

Means estimated with data from 2001 National Household Travel Survey.

Table 1, Panel B: Average number of trips per day taken by married couples, by purpose of trip and presence of children

	All				One car				Two cars			
Mean number of trips per day	**Husb**	**Wife**	**H/W**	**N**	**Husb**	**Wife**	**H/W**	**N**	**Husb**	**Wife**	**H/W**	**N**
To/from work	0.74	0.52	1.42	15,540	0.63	0.38	1.69	1,121	0.74	0.51	1.44	9,235
	(0.01)	(0.01)	(0.02)		(0.02)	(0.02)	(0.11)		(0.01)	(0.01)	(0.03)	
No children present in family	0.65	0.57	1.16	7,405	0.57	0.41	1.38	592	0.65	0.58	1.12	4,452
	(0.01)	(0.01)	(0.02)		(0.03)	(0.03)	(0.12)		(0.01)	(0.01)	(0.03)	
Children present in family	0.81	0.48	1.70	8,135	0.70	0.33	2.11	529	0.82	0.45	1.83	4,783
	(0.01)	(0.01)	(0.03)		(0.04)	(0.03)	(0.21)		(0.01)	(0.01)	(0.05)	
Family/personal	1.61	2.08	0.77	15,540	1.72	1.93	0.89	1,121	1.58	2.08	0.76	9,235
	(0.01)	(0.02)	(0.01)		(0.06)	(0.06)	(0.04)		(0.02)	(0.02)	(0.01)	
No children present in family	1.61	1.74	0.92	7,405	1.74	1.76	0.99	592	1.56	1.70	0.92	4,452
	(0.02)	(0.02)	(0.02)		(0.07)	(0.08)	(0.06)		(0.03)	(0.03)	(0.02)	
Children present in family	1.61	2.38	0.68	8,135	1.69	2.13	0.79	529	1.60	2.43	0.66	4,783
	(0.02)	(0.02)	(0.01)		(0.08)	(0.09)	(0.05)		(0.03)	(0.03)	(0.01)	
Serve passenger	0.26	0.45	0.57	15,540	0.35	0.39	0.89	1,121	0.27	0.48	0.56	9,235
	(0.01)	(0.01)	(0.02)		(0.03)	(0.03)	(0.09)		(0.01)	(0.01)	(0.02)	
No children present in family	0.14	0.14	0.96	7,405	0.27	0.20	1.32	592	0.13	0.13	0.98	4,452
	(0.01)	(0.01)	(0.06)		(0.03)	(0.03)	(0.23)		(0.01)	(0.01)	(0.08)	
Children present in family	0.37	0.74	0.50	8,135	0.44	0.60	0.72	529	0.40	0.80	0.50	4,783
	(0.01)	(0.01)	(0.01)		(0.04)	(0.05)	(0.09)		(0.01)	(0.02)	(0.02)	

Means estimated in 2001 National Household Travel Survey.

Table 1, Panel C: Average number of trips per day taken by married couples with working husband, living only with own children, split by wives' labor force participation and passengers

	All				One car				Two cars			
Mean number of trips per day	**Husb**	**Wife**	**H/W**	**N**	**Husb**	**Wife**	**H/W**	**N**	**Husb**	**Wife**	**H/W**	**N**
Both husband and wife work	4.72	5.02	0.94	10,969	4.80	4.89	0.98	555	4.67	5.01	0.93	6,497
	(0.03)	(0.03)	(0.01)		(0.13)	(0.13)	(0.04)		(0.03)	(0.04)	(0.01)	
Without children on trip	3.94	3.72	1.06		4.01	3.67	1.09		3.83	3.60	1.06	
	(0.03)	(0.03)	(0.01)		(0.13)	(0.12)	(0.05)		(0.03)	(0.03)	(0.01)	
With children on trip	0.78	1.31	0.60		0.78	1.22	0.64		0.84	1.41	0.60	
	(0.02)	(0.02)	(0.02)		(0.07)	(0.10)	(0.08)		(0.02)	(0.03)	(0.02)	
Husband works, wife does not	4.60	4.80	0.96	3,138	4.46	3.91	1.14	379	4.55	4.86	0.94	1,906
	(0.05)	(0.06)	(0.02)		(0.14)	(0.16)	(0.06)		(0.06)	(0.08)	(0.02)	
Without children on trip	3.67	2.74	1.34		3.27	2.21	1.48		3.61	2.65	1.36	
	(0.05)	(0.05)	(0.03)		(0.13)	(0.14)	(0.11)		(0.06)	(0.07)	(0.04)	
With children on trip	0.93	2.06	0.45		1.20	1.70	0.71		0.93	2.21	0.42	
	(0.03)	(0.05)	(0.02)		(0.11)	(0.12)	(0.08)		(0.04)	(0.06)	(0.02)	

Means estimated in 2001 National Household Travel Survey.

Table 2
Rate Regulation Laws, 1984-2006, from Harrington (2002)

State	Household Observations in CE Survey	Years with Competitive Rating	Years with Prior Approval
California	12,695	1984-1988	1989-2006
Texas	9,623	2004-2006	1984-2003
New York	8,166	1996-2006	1984-1995
Florida	6,921	1984-1986	1987-2006
New Jersey	4,362	2004-2006	1984-2003
Georgia	3,646	1984-1987	1988-2005
Maryland	3,269	1985-1989, 1999-2005	1984, 1990-1998
Louisiana	1,951	2004-2006	1986-2003
Connecticut	1,963	1994-1999	1984-1993, 2000-2006
South Carolina	1,384	1986-1998	1999-2006
Iowa	300	1984-1987	1988-1996

Table 3
Sample Means for Households Observed in CPS and CE Survey, 1984-2006

Variable (Individual characteristics are husband's)	CPS	CE Survey
Employed	0.926	0.979
Wife employed	0.732	0.761
Prior approval rate regulation in effect	0.568	0.551
Owns at least one car	--	0.946
Owns two or more cars	--	0.727
Usual hours worked per week (conditional on employment)	44.398	44.580
Gas tax, 2005 cents/gallon	22.359	22.383
Average insurance expenditures, $/year	$712.299	$713.361
Combined insurance premiums, $/year	$818.354	$817.696
White	0.784	0.775
Black	0.073	0.080
Hispanic	0.101	0.101
Less than HS diploma	0.110	0.095
HS diploma	0.303	0.291
Some college	0.176	0.179
College degree	0.411	0.434
Age	41.093	39.710
Observations	334,315	121,670

Sample restricted to married couples living alone or with own children only.

Table 4
Linear Probability Estimates of First Stage Relationship, CE Survey Data 1984-2006

Percentage-Point Change in Probability Due to Prior Approval, (Standard Error), [Observations]

	(1)	(2)	(3)	(4)
	Ownership of One or More Cars		**Ownership of Two or More Cars**	
Sample	**Without Children**	**With Children**	**Without Children**	**With Children**
All married couples	-0.0124	-0.0006	0.0094	0.0238**
	(0.0098)	(0.0004)	(0.0189)	(0.0085)
	[35,851]	[85,819]	[35,851]	[85,819]
Husband is 18-39 years old	-0.0185	-0.0049	0.0136	0.0204*
	(0.0127)	(0.0054)	(0.0232)	(0.0101)
	[13,533]	[42,825]	[13,533]	[42,825]
Husband is 40-64 years old	-0.0058	0.0132	0.0086	0.0293*
	(0.0072)	(0.0089)	(0.0225)	(0.0122)
	[22,318]	[42,994]	[22,318]	[42,994]

* $p < 5\%$, ** $p < 1\%$. Statistics reported for each subsample and each specification are the percentage point change in probability due to prior approval, its (standard error), and the [number of observations]. Standard errors clustered at state level. Sample consists of married couples, living alone or only with their own children. Controls include individual characteristics of husband and wife (dummies for race/ethnicity, education, age), state and year fixed effects.

Table 5
First Stage, Reduced Form, and 2SIV Estimates

Percentage-Point Change in Probability Due to Prior Approval, (Standard Error), [Observations]

	(1)	(2)	(3)	(4)	(5)
		Reduced Form (CPS)		**Two-Sample IV**	
Sample	**1st Stage (CE)**	**Husband**	**Wife**	**Husband**	**Wife**
All married couples with children	0.024**	0.015**	-0.017*	0.617*	-0.693
	(0.008)	(0.002)	(0.007)	(0.243)	(0.383)
	[85,819]	[192,841]			
Husband is 18-39 years old	0.020*	0.019**	-0.018*	0.922	-0.885
	(0.010)	(0.003)	(0.007)	(0.481)	(0.559)
	[42,825]	[117,446]			
Husband is 40-64 years old	0.029*	0.007*	-0.010	0.236	-0.356
	(0.012)	(0.003)	(0.006)	(0.135)	(0.258)
	[42,994]	[75,395]			
Wife is 18-39 years old	0.026*	0.019**	-0.017*	0.720*	-0.637
	(0.012)	(0.003)	(0.007)	(0.340)	(0.392)
	[50,564]	[139,364]			
Wife is 40-64 years old	0.025*	0.003	-0.010	0.116	-0.391
	(0.011)	(0.005)	(0.005)	(0.189)	(0.276)
	[35,255]	[53,477]			
Husband is older than wife	0.019*	0.014**	-0.018**	0.763*	-0.936
	(0.008)	(0.004)	(0.007)	(0.368)	(0.541)
	[56,899]	[130,559]			
Wife is at least as old as husband	0.031*	0.013**	-0.015	0.409	-0.471
	(0.015)	(0.003)	(0.009)	(0.215)	(0.361)
	[28,920]	[62,282]			
Oldest child is under 16 years old	0.029**	0.014**	-0.018**	0.466**	-0.620*
	(0.008)	(0.003)	(0.007)	(0.158)	(0.306)
	[57,359]	[166,798]			
Oldest child is over 15 years old	0.015	0.019**	-0.007	1.227	-0.442
	(0.013)	(0.004)	(0.008)	(1.067)	(0.626)
	[28,438]	[26,043]			

* $p < 5\%$, ** $p < 1\%$. Statistics reported for each subsample and each specification are the percentage point change in probability due to prior approval, its (standard error), and the [number of observations]. Instrument is an indicator for whether "prior approval" rate regulation is in effect in a given state-year. Endogenous variable is ownership of two or more cars. Sample is restricted to married couples with children.

Table 6
First-Stage Estimates of Primary Car Ownership (One or More Cars) Regressed on Alternative Instruments
CE Survey Data, 1984-2006

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	**Compulsory Insurance**	**No-Fault**	**Residual Market Share**	**Gas Tax**	**Average Premiums**	**Combined Premiums**	**Liability Premiums**
All married couples	-0.0016	-0.0114	0.0271	0.0003	0.0002	0.0011	0.0002
	(0.0024)	(0.0072)	(0.0225)	(0.0003)	(0.0027)	(0.0023)	(0.0027)
	[121,670]	[121,670]	[121,195]	[121,670]	[107,883]	[99,404]	[59,135]
With children	-0.0043	-0.0101*	0.0155	-0.0004	-0.0013	-0.0007	-0.0036
	(0.0032)	(0.0044)	(0.0234)	(0.0004)	(0.0038)	(0.0035)	(0.0044)
	[85,819]	[85,819]	[85,454]	[85,819]	[75,858]	[69,883]	[41,847]
Without children	0.0056	-0.0131	0.0540	0.0018	0.0030	0.0049	0.0064
	(0.0051)	(0.0172)	(0.0249)	(0.0012)	(0.0033)	(0.0032)	(0.0067)
	[35,851]	[35,851]	[35,741]	[35,851]	[32,025]	[29,521]	[17,288]
Husband 18-39	-0.0110	-0.0065	0.0186	0.0006	0.0022	0.0058	0.0114
	(0.0066)	(0.0111)	(0.0250)	(0.0008)	(0.0033)	(0.0034)	(0.0080)
	[56,358]	[56,358]	[56,124]	[56,358]	[48,838]	[44,635]	[27,858]
Husband 40-64	0.0076	-0.0177*	0.0358	-0.0003	-0.0025	-0.0037	-0.0104
	(0.0069)	(0.0057)	(0.0242)	(0.0006)	(0.0039)	(0.0034)	(0.0066)
	[65,312]	[65,312]	[65,071]	[65,312]	[59,045]	[54,769]	[31,277]

* $p < 5\%$. Gas taxes are in units of cents per gallon, excluding general sales taxes. Insurance premiums (last three columns) are in units of $100 per car-year insured, for legibility. In each group of three entries, top number is the linear probability model first-stage coefficient estimate, second number is (standard error), and third number is [observations].

Table 7
First-Stage Estimates of Secondary Car Ownership (Two or More Cars) Regressed on Alternative Instruments
CE Survey Data, 1984-2006

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample	**Compulsory Insurance**	**No-Fault**	**Residual Market Share**	**Gas Tax**	**Average Premiums**	**Combined Premiums**	**Liability Premiums**
All married couples	-0.0006	-0.0019	0.0458	0.0004	0.0005	0.0006	-0.0057
	(0.0069)	(0.0121)	(0.1270)	(0.0010)	(0.0044)	(0.0041)	(0.0059)
	[121,670]	[121,670]	[121,195]	[121,670]	[107,883]	[99,404]	[59,135]
With children	0.0022	-0.0119	0.0449	0.0008	0.0025	0.0004	-0.0039
	(0.0082)	(0.0175)	(0.1205)	(0.0011)	(0.0043)	(0.0041)	(0.0084)
	[85,819]	[85,819]	[85,454]	[85,819]	[75,858]	[69,883]	[41,847]
Without children	-0.0087	0.0307	0.0729	-0.0007	-0.0015	0.0041	-0.0080
	(0.0165)	(0.0157)	(0.1384)	(0.0016)	(0.0094)	(0.0092)	(0.0112)
	[35,851]	[35,851]	[35,741]	[35,851]	[32,025]	[29,521]	[17,288]
Husband 18-39	-0.0184	-0.0049	0.0274	0.0008	0.0035	0.0054	-0.0037
	(0.0109)	(0.0087)	(0.1010)	(0.0011)	(0.0062)	(0.0059)	(0.0069)
	[56,358]	[56,358]	[56,124]	[56,358]	[48,838]	[44,635]	[27,858]
Husband 40-64	0.0167	0.0021	0.0687	-0.0001	-0.0022	-0.0038	-0.0079
	(0.0107)	(0.0191)	(0.1489)	(0.0013)	(0.0051)	(0.0045)	(0.0087)
	[65,312]	[65,312]	[65,071]	[65,312]	[59,045]	[54,769]	[31,277]

Gas taxes are in units of cents per gallon, excluding general sales taxes. Insurance premiums (last three columns) are in units of $100 per car-year insured, for legibility. In each group of three entries, top number is the linear probability model first-stage coefficient estimate, second number is (standard error), and third number is [observations]. No estimate is significant at the 5% level.