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

[Job Market Paper – Preliminary and Incomplete]

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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. This implies that if more low-skill urban dwellers owned cars, gaps in employment rates would shrink. 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 prior approval laws increase 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 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 this conjecture 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 intra-household allocations of time and car access [so what? How is this a limitation?]. 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 tend 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 a family owns a car, regulation generates a two-percentage-point increase in second-car ownership rates. This effect is concentrated among married parents of young children.

Although the CE Survey is the largest 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, so I utilize data from the March Current Population Survey (CPS) 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, the association between rate regulation and employment is strongest among married couples with children in the CPS.

Interestingly, I find that the ownership of the second car in the household has divergent effects on labor supply within these households. I find that the second car increases the father's probability of employment, while it decreases the employment of mothers. This result stands in contrast to the previous literature on urban labor markets, which uniformly predicts that easing spatial frictions will increase labor supply.

Since the effect is driven by parents of young children, one potential explanation of this result 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 related to family activities, especially for child care purposes, and this disparity 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, effectively separating minorities from fast-growing employment opportunities in the suburbs. In the mid-1990s an offshoot of this work called the "automobile mismatch" claimed that insufficient access to a private automobile is also an important spatial barrier to employment, especially for minorities.

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. I contribute to this subset of the literature in the following 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 with potentially limited generalizability. 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 focused only on individual employment and primary car ownership, presuming that the main effects of reducing the costs of car ownership will be to reduce the prevalence of carlessness and to increase labor supply. The price elasticity of cars is greater at the second car than at the first, however, and the proportion of households without a car is small and relatively stable over time, as shown in Figure 1. The labor supply of secondary earners is also more elastic than that of primary earners.

This paper's focus on how the second automobile impacts intra-household allocation of time is unique in this literature. Since multiple-car ownership and secondary labor supply are both more elastic to price, policies that change the costs of owning cars (e.g. environmental policies, infrastructure investment, tort reform, *etc*.) will have their largest effect on these margins. Cars are generally shared

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

within households, but not equally across members; the second car reduces spatial frictions for secondary earners by more than the first car does. Since secondary earners in low-income households can account for a significant proportion of their households' wage earnings (Cattan, 1998), policies affecting secondary car ownership may be particularly important to the well-being of families in poverty. Even if one is only interested in primary labor force participation, estimates can be biased in specifications neglecting the simultaneity of intra-household allocations of car access and time.

The literature's focus on how vehicles lower the spatial barriers to employment overlooks the usefulness of vehicles in home production, and thereby misses an avenue through which car ownership can alter 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 on secondary cars and secondary earners, a consideration of household production may be crucial in understanding the effects of those policies.

The rest of the paper is organized as follows. The following Section II provides some background on the previous literature concerning transportation barrier 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 rates improve access to employment, then in reduced-form models we should see an increase in employment in states after cost-reducing auto insurance legislation is passed. Section IV presents the results of the analysis. Section V discusses other sources of exogenous variation, some of which have been previously 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.

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). Kain (1968) documents the shift in the location of manufacturing establishments, but low-skill labor demand has shifted from manufacturing to the service and retail sectors, which have grown faster in the suburbs than in central cities (e.g. Stoll *et al.*, 2000). A large literature developed that is dedicated to testing this more generalized spatial mismatch hypothesis, and more generally whether spatial frictions can help explain persistently high unemployment in US central cities.

From the beginning authors in this literature noted that spatial separations must be mediated by mode of transportation—the implied mechanisms regard distance as relevant only insofar as it affects time, whether spent searching or commuting. It was not until the mid-1990s, though, after consensus had not been reached regarding the relevance of housing discrimination, that authors began to focus on 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.

Given the dearth of clear evidence regarding the special mismatch hypothesis, the academic discussion has moved towards an analysis of mode choice. The offshoot "automobile mismatch"[2] hypothesis emphasizes that the low densities of suburbs imply every residence is far from every

[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.

workplace; no matter where one lives in a metropolitan area, a car is essential for finding and keeping a job.

Anecdotal evidence supports the idea that cars are necessary whether a family lives in the suburbs or in the city. As part of the Moving to Opportunity (MTO) demonstration program, public housing resident 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 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, as 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 such vehicles.[7]

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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

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. Glaeser, Kahn, and Rappaport (2008) provide evidence from the 1980, 1990, and 2000 Censuses that new mass transit stations induce 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 documented strong, positive, robust correlations between employment and car ownership, showing that those who own cars are much more likely to be employed. Figure 2 demonstrates that for single mothers with less than a college degree, the time-series of car ownership and employment are highly correlated. Interpreting this relationship is difficult 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. This possibility is likely reflected in Figure 2 in that the enactment of welfare reform in the mid-1990s 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

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California had previously implemented similar programs (http://www.cars.gov/files/official-information/CARS-Report-to-Congress.pdf).

[7] Anecdotally, recent news reports claim that prices for used cars have jumped as much as 30% year-on-year in 2010, especially among low-mileage trucks and SUVs. A portion of this increase is probably due to the recession's effect on household income, as used cars are inferior goods.

[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.

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 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. Other papers exploit plausibly exogenous state-by-state slackening of the 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) use panel data from the 1991, 1992, and 1993 SIPP to estimate the employment effects of moving to car ownership. They showed in a difference-in-difference framework that the correlation between car ownership and employment was strongest for Blacks, moderate for Latinos, and weakest for Whites, which mirrors the relative spatial isolation of these groups. They also demonstrated that the correlation was strongest in cities with the highest indices of segregation. They concluded that differences in 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 had lower insurance premiums.

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 Conceptual 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 identical between household members.

Suppose that the household pays a fixed time cost for each member that works, in order to represent time lost commuting. If both members work, the household pays a higher 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, and for the other to avoid the commuting cost by withdrawing from the labor market. For any given set of preferences, higher commuting costs have an unambiguously non-positive effect on the extensive margin of labor supply (although for the remaining worker the effect on hours is ambiguous).

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 key implication is this: If cars' elasticity of substitution with 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 I know of 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 Empirical Facts

Women's access to reliable transportation may increase more with a family's second car than its first. Noble (2004) measures the probabilities that each spouse will report being a "main driver" of a household vehicle, and finds that the difference between one- and two-car households in wives' drivership is 62 percentage points, an increase approximately equal to that between zero- and one-car households for husbands' drivership.

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.

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[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 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).

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 only**.** 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 and I report the ratio of these two values and its standard error, which is calculated using the delta method.

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 while the second fact is that the increased mobility afforded by the second car is associated with differential responsibility for home production, and in particular differential responsibility for 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.

Previous literature studying the impacts of these regulatory regimes has focused on one variable in particular as a reliable measure of regulatory strictness. Whether or not a state requires each insurer to obtain "prior approval" from the state insurance commissioner before changing its rate structure monotonically indicates the intensity of state regulation (Harrington, 2002). Szewczyk and Varma (1990) show California's 1988 passage of a referendum enacting rate regulation was associated with significant adverse effects on the share prices of firms with substantial exposure to the California market. Harrington (1992) argues that state-specific, illiquid investments by insurers can be appropriated by state insurance commissioners, such that exit by insurers will be too slow to deter rate suppression. D'Arcy (2002) finds that "prior approval" is associated with larger assigned risk markets and more insurer insolvencies. Litan (2001) surveys these and other analyses, concluding that the preponderance of the evidence supports the hypothesis that "prior approval" laws suppress rates below their competitive levels.

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 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.[13]

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.

[13] I further discuss no-fault laws and other potential sources of variation in Section VI.

III.B. Two-Sample IV Method

The goal of the 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 I do have 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.[14]

I can, however, control for any variables which are constant for all households within each state over the time period by adding state fixed effects. This includes fixed attributes like the climate and topography of the state, as well as the within-state averages of variables which do change over the period, like average total highway lane-miles built or the typical political environment. I similarly control for any unobserved macroeconomic shock within a given year, to the extent that it affects all states equally, using year fixed effects.

Simultaneity bias, or reverse causality, is another potential problem. For example, having a job provides one the income and access to credit necessary for buying and maintaining an automobile. These

[14] 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.

problems are essentially impossible to solve by adding covariates to the model. 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 which is plausibly uncorrelated with the error.

Rate regulation laws may reduce the costs of car ownership, thus increasing ownership rates in states that enact such laws. If rate regulation is to be a good instrument for identifying the effect of car ownership on employment, it must only affect employment through car ownership itself. Since lower prices increase the purchasing power of nominal wages, regulation may have an independent effect by increasing real wages. Wage increases have an unambiguously positive effect on LFP in most models. Motor vehicle insurance, however, represents 2.49 percent of the average household's expenditures,[15] so the average regulation-induced 11.23 percent decrease in annual premiums represents a 0.28 percent increase in real wages. Such a small increase in overall price levels is unlikely to explain the observed results.

Political endogeneity is another concern of this type. For example, states with unusually high rates of unemployment may be more likely to enact a populist legislature willing to expand the scope of government intervention in markets. If that state's unemployment time series has mean-reversion, then such a political mechanism could yield spurious results. Political endogeneity could also occur if rate regulation is enacted in response to particularly high insurance premiums, which themselves may have been a result of a booming state economy (with high levels of employment, car ownership, vehicle miles traveled, congestion, medical and repair prices, *etc*.)

Very few datasets contain each of the three variables (car ownership, employment status, and state-year identifiers defining rate regulation) required for IV estimation. As mentioned above, the SIPP is one of the few that do, but it covers a limited period of time. The CE Survey also measures all three, but even across twenty years its sample is too small to detect an employment effect.

To solve 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

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

[15] In the December 2009 Consumer Price Index for Urban Consumers, the weight on motor vehicle insurance is 2.492 percent. ftp://ftp.bls.gov/pub/special.requests/cpi/cpiri2009.txt

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

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

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

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.[16]

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.[17] 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

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

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

2006.[18] 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 in this way. Table 11 shows the remaining observations in each state-year cell.

Cars and market wage earnings are often shared within each household,[19] 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 focus on households consisting only of married couples living alone or with only their own children.[20]

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 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.

There are a few possibilities for reporting bias in the CE Survey. For example, when premiums increase, many poor households may let their insurance coverage lapse and drive uninsured. Since this is illegal in most states, these households may be reluctant to report that they own an automobile but will have no such reluctance reporting their employment status. This effect will upwardly bias (in magnitude)

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[18] 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.

[19] Indeed, the BLS defines a "consumer unit" as a set of individuals sharing a substantial proportion of household expenditures.

[20] "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.

estimates of the effect of premiums on car ownership, and IV regressions of employment on car ownership will be biased downward.

The Integrated Public Use Microdata Series - March Annual Demographic File and Income Supplement of the Current Population Survey consists of 48 years (of which I use 23) of the Current Population Survey (CPS), with its variable definitions harmonized across years. The CPS is collected monthly by the Census Bureau and the Bureau of Labor Statistics, but the March Supplement is only collected each March. Table 3 shows sample means for both the CE Survey and the CPS. Note that most of the variables measured in both datasets have similar means.

The variable defining rate regulation laws in each state-year is drawn from the appendix of Grace and Phillips (2008), who extend Harrington (2002). They categorize laws into eight categories of regulatory strictness. Following Harrington (2002) I separate those 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.

IV. Results: The Effect of Car Ownership on Employment

The incentives created by prior approval laws for households to buy their first car (or sell their last, in the case of repeals) are too weak for prior approval to be a suitable instrument on the zero- to one-car margin. Table 4 demonstrates that households appear unwilling or unable to buy their first car upon the enactment of prior approval legislation. Across all groups, no first-stage estimate is statistically significant.

This result is consistent with a few plausible explanations. One explanation is that cars are a "lumpy" investment in that there are increasing returns to expenditures on a car at low levels of expenditure, in part because there are sizable fixed costs to car ownership. Another possibility is that there may be sizable transaction costs associated with changing one's level of consumption of cars (as in Chetty and Szeidl, 2007). 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 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. Table 5, however, shows that married couples living only with their own children buy an additional car when rate regulation goes into

effect. The first entry of the first 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.

Rate regulation affects employment through its effect on car ownership. One might expect that this effect is heterogeneous among different demographic subgroups, but the surprising result is that the effect of rate regulation (through car ownership) is *opposite* for members of the *same household*. Following along the first row, we find that rate regulation is associated with a 1.5 percentage point increase in male labor supply and a 1.7 percentage point reduction in female labor supply. Without taking these offsetting intra-household adjustments into account, the effects of car ownership on employment is obscured.

The 2SIV estimate indicates that a second car increases a husband's labor supply by 62 percentage points. Given the sample mean, this implies a probability of employment well over 100 percent. The corresponding effect of a second car on female labor supply is to reduce it by 70 percentage points. These impossibly large estimates may be an artifact of the linear probability model specification, or they may represent a deeper problem with the analysis.

The 2SIV results for women are consistently negative across suggest that car ownership reduces their labor supply, but the adjusted standard errors are large enough that the null hypothesis cannot be rejected.

Table 5 also presents results for several subgroups. The effects appear to be widespread and robust—it does not appear that one subgroup is driving the car ownership estimate while another group is pushing the employment numbers. Although the 2SIV estimates for women in most subgroups are lacking enough power to reject the null hypothesis at conventional confidence levels, they are consistently negative.

Strikingly, the strongest results are among married couples with children under 16, as shown in Table 6. 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.

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. 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.[21]

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) examines 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.

The last four Columns (4) – (7) follow the method of Raphael and Rice (2002), the only previous national study that addresses endogeneity. 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.[22]

VI. Conclusion

This study examines whether increased access to reliable transportation has an impact on the labor supply of low-income, urban households. In particular, I focus on the potential implications of policies altering the cost of driving for labor-market outcomes 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 choices.

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[21] 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.

[22] 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.

A concern with previous attempts 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 reservations 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 unobserved permanent geographic differences could cause this method to yield a spurious estimate.

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, Manueli, and McGrattan, 2003), most of this literature regards such capital as a substitute for household labor, as in the form of increased availability of household appliances. Automobiles differ from most other types of household capital in that they also decrease the fixed costs of labor market participation, but also in that it appears to increase the marginal productivity of labor inputs to household production.

Cowan (1983), an early entrant into the household capital literature, documents the rapid change in the early 20th century from home production of food, clothing, and health care to consumption of market

substitutes for those goods; and from door-to-door home delivery of market goods that were close substitutes for home goods (e.g. milkmen, mail order, doctor house calls) to self-service, centralized distribution (e.g. department stores, supermarkets). Over time, automobiles became more essential for intra-urban travel (Kahn and Glaeser, 2003), and "home production" moved increasingly outside the home itself. Cowan concludes that automobiles probably increased the burden of housework on American women, anticipating the above 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."

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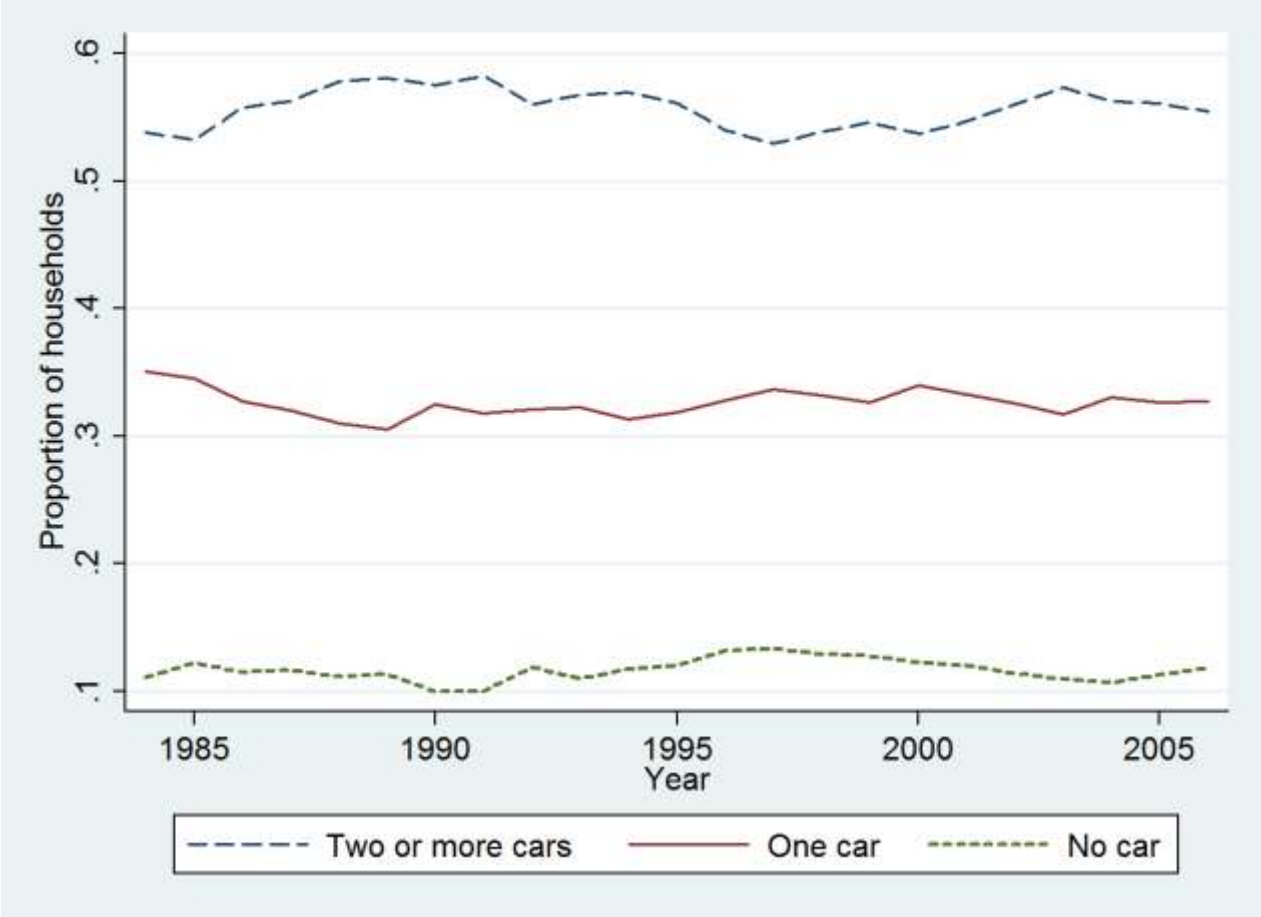
Appendix A. Observations by year, state

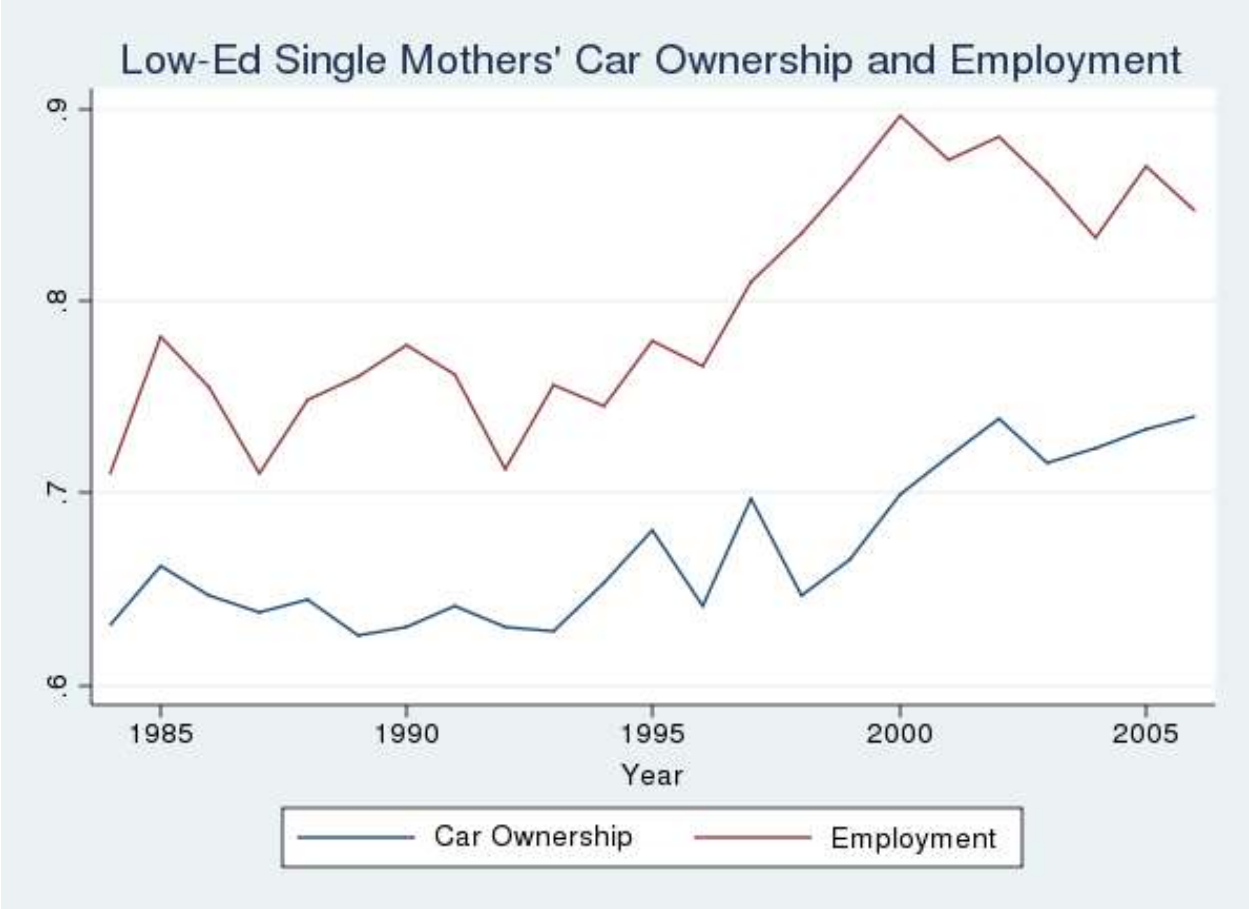
[Table 9]

Figure 1
Proportions of households owning zero, one, or multiple cars



CE Survey, 1984-2006. Married couples living alone or with own children only. Sample weights are applied to account for stratified sampling.

Figure 2
Car ownership and employment among single mothers without a college degree



CE Survey, 1984-2006. Single mothers with own children only, high school diploma or less. 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 Obs. 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

Variable (Individual characteristics are husband's)	***CPS***	***CE Survey***
Employed	0.925	0.980
Wife employed	0.734	0.760
Usual hours per week	41.034	--
Wage earnings, $/year	$9,304.381	--
Owns at least one car	--	0.950
Owns two or more cars	--	0.748
Full-time student	--	0.018
Hours worked per week	37.610	44.510
Gas tax, 2005 cents/gallon	22.368	22.368
Average insurance expenditures, $/year	$713.833	$714.908
Combined insurance premiums, $/year	$819.974	$819.347
White	0.780	0.772
Black	0.075	0.082
Hispanic	0.102	0.102
Less than HS diploma	0.117	0.107
HS diploma	0.306	0.290
Some college	0.173	0.246
College degree	0.404	0.357
Age	42.709	41.182
Observations	412,315	102,192

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

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

Percentage Point Change in Probability due to Prior Approval
(Standard Error)
[Number of Observations]

	Ownership of One or More Cars			**Ownership of Two or More Cars**	
Sample	**All**	**Without Children**	**With Children**	**Without Children**	**With Children**
All families	-0.0034	-0.0124	-0.0006	0.0094	0.0238**
	(0.0034)	(0.0097)	(0.0004)	(0.0189)	(0.0085)
	[102,192]	[35,851]	[66,341]	[35,851]	[85,819]
Husband is 18-39 years old	-0.0072	-0.0187	-0.0049	0.0136	0.0204*
	(0.0059)	(0.0129)	(0.0054)	(0.0232)	(0.0101)
	[55,200]	[13,533]	[41,667]	[13,533]	[42,825]
Husband is 40-64 years old	-0.0042	-0.0058	0.0132	0.0086	0.0293*
	(0.0030)	(0.0071)	(0.0089)	(0.0225)	(0.0122)
	[46,992]	[22,318]	[24,674]	[22,318]	[42,994]

* $p < 5\%$, ** $p < 1\%$. Standard errors clustered at state level. 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 Results from Instrumenting for Two-Car Ownership's Effect on Household Labor Supply with "Prior Approval" Rate Regulation, among Married Couples with Children

Percentage Point Change in Probability due to Prior Approval
(Standard Error)
[Number of Observations]

		Reduced Form (CPS)		**Two-Sample IV**	
Sample consists of households in which:	**1st Stage (CEX)**	**Husband Employed**	**Wife Employed**	**Husband Employed**	**Wife Employed**
Full sample (i.e. 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]	[192,841]			
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]			

Instrument is 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
Additional First-Stage, Reduced-Form, and 2SIV Results from Instrumenting for Two-Car Ownership's Effect on Household Labor Supply with "Prior Approval" Rate Regulation, among Married Couples with Children

		Reduced Form (CPS)		Two-Sample IV	
Sample consists of households in which:	**1st Stage (CEX)**	**Husband**	**Wife**	**Husband**	**Wife**
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]			
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 12 to 15 years old	0.065*	0.011**	-0.015	0.166*	-0.228
	(0.025)	(0.003)	(0.008)	(0.078)	(0.155)
	[15,538]	[46,458]			
Oldest child is 6 to 11 years old	0.024*	0.015**	-0.015	0.631*	-0.609
	(0.009)	(0.004)	(0.008)	(0.289)	(0.406)
	[21,755]	[63,314]			
Oldest child is under 6 years old	0.005	0.014**	-0.025**	3.003	-5.358
	(0.012)	(0.003)	(0.008)	(7.877)	(14.137)
	[20,066]	[57,026]			
One child present	0.006	0.013**	-0.012	2.245	-2.007
	(0.011)	(0.004)	(0.006)	(4.202)	(3.859)
	[29,957]	[63,937]			
Two children present	0.041*	0.010**	-0.018	0.244	-0.438
	(0.016)	(0.004)	(0.011)	(0.133)	(0.313)
	[35,650]	[83,545]			
Three or more children present	0.015	0.023**	-0.021	1.548	-1.431
	(0.014)	(0.006)	(0.012)	(1.502)	(1.555)
	[20,212]	[45,359]			
Husband has less than HS education	0.089**	0.026**	-0.007	0.295**	-0.081
	(0.025)	(0.006)	(0.020)	(0.110)	(0.229)
	[9,570]	[23,117]			
Husband has HS diploma	-0.012	0.018**	-0.015	-1.505	1.254
	(0.022)	(0.004)	(0.008)	(2.840)	(2.431)
	[24,989]	[57,220]			
Husband has some college education	0.028	0.024**	-0.026	0.857	-0.950
	(0.030)	(0.005)	(0.011)	(0.938)	(1.091)
	[21,242]	[33,614]			
Husband has college degree	0.025	0.003	-0.013	0.110	-0.533
	(0.018)	(0.003)	(0.007)	(0.163)	0.490
	[13,999]	[43,938]			
Wife has less than HS education	0.096**	0.028**	0.013	0.289	0.14
	(0.017)	(0.007)	(0.016)	(0.086)	(0.173)
	[9,029]	[21,329]			
Wife has HS diploma	-0.013	0.022**	-0.032**	-1.673	2.492
	(0.016)	(0.005)	(0.009)	(2.137)	(3.212)
	[28,155]	[63,299]			
Wife has some college education	0.073**	0.012**	-0.016	0.165	-0.221
	(0.022)	(0.005)	(0.010)	(0.085)	(0.158)
	[24,148]	[35,331]			
Wife has college degree	-0.010	0.003	-0.001	-0.331	0.143
	(0.011)	(0.002)	(0.008)	(0.445)	(0.801)
	[24,487]	[72,882]			
Wife has more educ. than husband	-0.007	0.015**	-0.019	-2.130	2.602
	(0.022)	(0.005)	(0.011)	(6.623)	(8.187)
	[16,194]	[37,486]			
Husband is white	0.020	0.010**	-0.020**	0.515	-0.996
	(0.013)	(0.003)	(0.006)	(0.364)	(0.705)
	[63,950]	[138,888]			
Husband is not white	0.013	0.020**	-0.010	1.619	-0.762
	(0.024)	(0.004)	(0.012)	(3.149)	(1.749)
	[21,869]	[53,953]			

* $p < 5\%$, ** $p < 1\%$. Standard errors clustered at state level. Controls include individual characteristics of husband and wife (dummies for race/ethnicity, education, age), size of family, state and year fixed effects.

Table 7
Potential Instruments with No First-Stage Effect on Car Ownership (One or More Cars)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	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]

Insurance premiums (last 3 columns) are in units of $100 per car-year insured, for legibility.
Each column lists OLS results for one of several variables which might only effect employment through their effects on car ownership. In each group of three entries, top number is OLS estimate of the coefficient on the instrument, second number is (standard error), and third number is [number of observations].

Table 8
Potential Instruments with No First-Stage Effect on Car Ownership (Two or More Cars)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	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]

Insurance premiums (last 3 columns) are in units of $100 per car-year insured, for legibility.
Each column lists OLS results for one of several variables which might only effect employment through their effects on car ownership. In each group of three entries, top number is OLS estimate of the coefficient on the instrument, second number is (standard error), and third number is [number of observations].

[Appendix A] Table 9
Individual observations by year, state in the CE Survey

	84	85	86	87	88	89	90	91	92	93	94	95	96	97	98	99	00	01	02	03	04	05	06
AL	194	154	302	285	262	308	271	296	227	298	282	311	401	385	313	412	376	455	427	436	360	105	
AK		378	93										291	384	383	556	684	743	604	593	633	496	456
AZ	116	125	551	514	131	64	77	79	33	107	131	91	505	555	476	857	894	965	955	1012	1025	976	789
AR			285	260	218	214	206	204	200	260	251	291	79										
CA	2285	2201	2733	2319	1970	1931	1971	2017	1806	2212	2235	2088	2433	2591	2490	3604	4018	3814	3775	3924	3782	479	4078
CO	526	475	603	405	453	383	395	457	436	537	554	562	457	411	441	608	644	766	745	747	735	486	379
CT	111	119	346	298	292	225	272	247	255	339	315	271	348	393	324	536	463	452	381	509	508	448	469
DE													45	53	51	83	67	56	85	86	75	82	
DC			67	87	60	58	63	63	62	52	73	56	7									70	75
FL	830	802	1344	1201	1168	1148	1245	1170	1138	1407	1395	1311	1259	1163	1145	1694	1902	1834	1834	1949	1832	1795	1827
GA	406	385	623	628	513	522	537	609	528	727	783	741	706	706	731	1032	1162	1050	1078	1034	1048	123	
HI	473	489	556	424	412	446	418	256	303	427	436	372	399	401	412	574	619	546	485	525	549	442	493
ID													169	190	197	337	324	342	373	349	341	372	281
IL	848	855	830	783	602	611	645	771	581	706	667	665	882	909	817	1206	1189	1326	1499	1397	1217	1449	1517
IN	288	296	465	411	427	437	395	455	442	495	444	492	388	384	370	467	474	456	491	592	590	104	509
IA	83	46	139	107	81	100	82	71	66	110	114	87	27										
KS	136	152	216	231	202	186	146	170	166	169	165	201	215	155	184	194	222	214	258	204	221	231	130
KY													78	91	64	153	147	148	152	99	126	32	321
LA			250	320	298	280	259	270	284	324	325	283	413	395	410	544	523	472	553	538	465	569	498
ME																						14	22
MD	536	527	676	545	518	600	560	553	504	580	611	567	694	605	584	819	896	828	817	941	958	786	
MA	518	568	784	508	510	572	526	454	415	544	524	494	660	625	621	816	884	834	906	804	801	858	778
MI	564	635	676	563	617	563	530	590	553	711	587	615	806	619	666	848	882	844	882	902	866	108	981
MN	428	481	620	444	405	458	476	443	362	504	454	475	475	393	395	554	617	604	601	590	595	52	
MS																							
MO	531	555	724	642	497	507	542	529	547	641	592	566	521	540	529	770	806	785	842	872	876	798	596
MT																							
NE													130	197	179	282	355	334	295	329	270	319	270
NV																							370
NH			48	52	41	47	50	24	16	56	47	37	11									81	116
NJ	730	723	852	676	638	651	652	514	553	733	663	601	680	700	710	1088	1059	1035	1067	1003	981	1031	1141
NM																							
NY	1522	1557	1959	1505	1462	1425	1502	1518	1304	1574	1568	1611	1776	1414	1321	1748	1893	1599	1774	1745	1613	1672	1771
NC	228	215	305	285	320	272	332	248	239	333	276	237	203	138	160	272	249	210	281	288	267	75	
ND																							
OH	896	988	1385	1339	1037	975	947	904	884	1078	1100	1161	970	869	897	1136	1155	1193	1200	1333	1158	173	870
OK			153	105	97	129	143	71	97	119	109	147	146	148	163	248	247	223	234	257	263	409	101
OR	272	279	352	370	308	344	360	383	327	411	433	399	381	304	374	523	520	559	597	530	466	36	567
PA	1294	1188	1449	1250	1130	1051	1122	1131	977	1206	1242	1192	1079	1151	1076	1715	1751	1622	1687	1834	1653	1717	1786
RI																						151	58
SC			162	144	106	121	90	107	97	155	166	149	306	298	290	395	336	338	358	421	418	680	787
SD																							
TN	309	311	263	167	131	106	119	142	119	141	158	174	128	69	106	133	177	139	107	153	124	36	452
TX	1344	1357	1789	1291	1225	1113	1189	1328	1177	1470	1484	1556	1883	2098	2077	2768	2738	2767	2843	2818	2626	440	2234
UT			225	176	129	87	103	88	112	153	168	162	269	326	317	393	397	397	407	391	451	472	349
VT													95	97	100	200	255	228	195	226	195	50	
VA	359	312	528	439	432	415	390	420	371	444	426	404	626	530	552	768	782	806	930	961	1062	184	1143
WA	407	414	709	506	514	473	584	524	513	714	586	569	490	502	523	656	692	826	732	727	764	903	615
WV	53	61	11																				32
WI	506	431	679	614	596	612	540	576	507	608	573	606	888	710	671	1000	1042	1075	1107	1071	1098	242	
WY																							