THREE ESSAYS ON HETEROGENEITY IN PUBLIC FINANCE

AN ABSTRACT

SUBMITTED ON THE 15TH DAY OF MARCH 2016

TO THE DEPARTMENT OF ECONOMICS

IN FULFILLMENT OF THE REQUIREMENTS

OF THE SCHOOL OF LIBERAL ARTS

OF TULANE UNIVERSITY

FOR THE DEGREE OF

DOCTOR OF PHILOSOPHY

BY

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This dissertation explores how heterogeneity in demand-side characteristics influences the findings of established models in public economics. The first chapter examines the distributional pattern of commute length responses to fuel price shifts. The existing literature assumes mobility adjustments are constant across all households: practical violations of that assumption may affect reported findings. This research explains sources of response differences and tests for their presence across measures of income, homeownership status, and commuting type. The results suggest that increased motor vehicle taxes reduce commuting distances, but with a more pronounced decline among households with high-incomes. Homeownership status is a key driver of the distributional pattern, as owner responses are similar across income levels, while the shift in commuting increases with income among renters. These findings indicate that accounting for household mobility may change existing distributional estimates.

The second chapter studies how heterogeneity in agglomeration and tax differentials affects residential mobility patterns across industries. Interstate mobility can limit states' ability to choose their desired tax policies. The forces of agglomeration, however, may allow states more leeway in setting tax rates. This research examines the residential location decisions of professional racecar drivers and golfers, which have similar industry characteristics but different levels of agglomeration. The findings show that tax preferences are a powerful determinant of golfer residential patterns, while agglomeration mitigates much of this effect among racecar drivers. These results highlight the need to better understand how competition and agglomeration interact when formulating tax policy.

The final chapter examines how heterogeneity in accrual and disbursement mechanisms – methods of spending and saving – affect the performance of rainy day funds, contingency funds intended to aid governments during a financing shortfall. There is significant variation in the way rainy day funds are structured across states. The analysis provides definitive evidence that volatility-based mechanisms improve the alignment of fund changes and economic performance, and mixed evidence of an effect on the magnitude of fund changes. Volatility-based mechanisms have a greater effect in states with more relaxed budget-balancing requirements, suggesting that a substitution effect exists with such restrictions. This demonstrates the potential efficiency gains available from structural reform.

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INTRODUCTION

This dissertation examines how heterogeneity in demand-side characteristics influences public finance outcomes in markets for transportation and housing, in residential decisions across industries, and in budget stabilization fund performance. Public economics seeks to develop predictive theories that are closely aligned with actual performance. This research offers insight into how existing, generalizable models can be tailored the unique settings and needs of different jurisdictions and levels of government.

The first chapter researches the effect of fuel price changes on household mobility. Distributional models on the demand for transportation taxes typically include an assumption of constant migration across households: discrepancies in the mobility response to tax changes may significantly alter incidence estimates. The analysis finds that migration responses to motor fuel price changes vary across measures of household income, commuting preferences, and homeownership. The results suggest incorporation of mobility responses into distributional models will alter estimates of the transportation tax burden.

The second chapter examines the influence of tax differentials and agglomeration on residential outcomes of professional athletes. Interstate mobility introduces the possibility of competition for tax bases, while the forces of agglomeration may offset the influence of competition. This paper analyzes the residential determinants of racecar drivers, who are subject to agglomeration effects, and golfers, who are not. The findings indicate that, contrary to existing research's presentation of tax differentials and agglomeration as an all-or-nothing proposition, outcomes may be highly dependent on industry characteristics.

The final chapter analyzes the effect of accrual and disbursement mechanisms on the efficiency of budget stabilization funds. Budget stabilization funds are savings accounts that assist governments in poor economic periods: much of the research has stressed the importance of adequate deposit levels in affecting outcomes. However, the structure of accrual and disbursement mechanisms may also influence fund performance. The research finds that funds linked to economic measures perform better than funds tied to legislative activity, and that the difference is more pronounced in jurisdictions with loose budget-balancing requirements.

FUEL PRICE SHIFTS AND THE DISTRIBUTIONAL IMPLICATIONS OF MIGRATION

This research tests for heterogeneity in the distributional pattern of commute length responses to fuel price shifts. The existing incidence literature on transportation policy offers a thorough examination of consumption behavior, yet typically assumes that mobility adjustments are constant across all households. This work discusses the potential ramifications for a non-uniform commuting response on existing distributional estimates, explains where response differences may be found, and tests for their presence across measures of income, homeownership status, and commuting type. The results suggest that increased motor vehicle taxes generally reduce commuting distances, but that the decline is more pronounced among households with high-incomes. Homeownership status is a key driver of the distributional pattern, as owner responses are similar across income levels, while the shift in commuting increases with income among renters. These findings indicate that accounting for household mobility may change distributional estimates of motor vehicle policies.

I. INTRODUCTION

Measuring the distributional burden of motor vehicle taxes has long been of interest to researchers and policymakers. The size of the tax base – among major spending categories, average transportation expenditures in the U.S. are exceeded only by spending on housing – ensures that patterns of motor vehicle tax incidence could make a notable contribution to the distributional trends of the tax system at large (Energy Information

Administration 2015). Existing research on motor vehicle tax incidence provides a comprehensive analysis of consumption responses to price shifts. It also accounts for a number of factors that increase estimate precision, including forward-looking observation of household ability to pay and vehicle purchasing effects. However, this research typically relies on the assumption that household migration patterns are insensitive to changes in the transportation market. The validity of this assumption and its effect on distributional estimates is understudied.

This study examines how fuel price shifts affect the distribution of commuting length, defined as the length of travel between the residence and employment center, across various types of households. The primary response to changes in the motor vehicle market is to alter consumption levels (e.g. changing the amount of fuel consumed in response to a change in fuel prices). Yet a household's ability to adjust its short-term driving patterns may be constrained by the home-labor commute. Therefore, families may choose to alter their commuting patterns in order to reduce their motor vehicle consumption, either through residential migration, a change in employment location, or a combination thereof. Evidence of this behavior is observed in Devereux, Lockwood and Redoano (2007), who found that shifts in excise tax rates on gasoline produced evidence of changes in the rates of interstate household migration.

Understanding the discrepancies in commuting responses across subsets of the population has the potential to improve the accuracy of distributional estimates. Certain households may respond disproportionately to transportation price increases through relocation of residence or employment. In that case, static incidence estimates may overstate their motor vehicle tax burden, thereby understating the effect on types of households that do not respond. Economic intuition supports the notion that commuting responses to fuel price shifts could depend on the income, family composition, homeownership status, and the household mode of commuting. Transportation policies are used to address a wide range of issues, including infrastructure spending, residential patterns, and the effects of climate change. Understanding their incidence may enable legislators to effectively implement such incentives while achieving the desired distributional outcomes.

The empirical work presented in this analysis uses time-series data from the American Housing Survey to test for differences in the commuting responses across households. Metropolitan-level fixed effects are employed to address concerns with heterogeneity in spatial and demographic characteristics across areas, and a time trend is included to account for shifts in residential and commuting patterns over time. Migration outcomes are measured in terms of the distance spent driving to work to account for relocation of both the residence and place of employment.

The distributional analysis uses a series of stratifications across metrics both of interest to policymakers and predictive of changes in behavior, including income, homeownership status and residential mobility. Sensitivity analysis is included to test for the effect of construction choices in the fuel price index and of householder age on distributional outcomes, and to test for some of the limitations inherent to the lack of a natural source of identification in the dataset.

The results indicate that fuel price changes produce commuting shifts that vary across household characteristics. While increased fuel costs generally reduce commuting distances for households, the decrease is much more pronounced among households in the middle and upper portions of the income distribution than it is for lower-income families. Homeownership status also has a significant effect on the income distribution of commuting responses. While the mobility response is relatively constant across income levels for homeowners, high-income renters have a much stronger commuting response to fuel price increases than low-income renters.

In general, these findings indicate that households do not engage in commuting responses uniformly, and suggests that movement away from the constant migration assumption could improve static distributional analyses of transportation taxes. Taken together, the results presented suggest that while fuel taxes may ably function as a source of infrastructure funding and combat climate change and urban sprawl, their burden on lower-income households may be greater than what is implied by the existing literature.

II. BACKGROUND

Despite rising vehicle costs and environmental concerns, the portion of U.S. household resources devoted to motor vehicle consumption is significant and rising. Americans drove over three million miles in 2014, a 1.5 percent increase over the previous year (Federal Highway Administration 2015). The cost of fueling that travel was $600 billion, roughly equivalent to the gross domestic product of Argentina (World Bank 2015 and Energy Information Administration 2015).

Government regulation of the motor fuel market is implemented through the imposition of excise taxes on motor fuels and manufacture-level restrictions on fuel efficiency. These policies intend to compensate the public for the social externalities created by driving, which include pollution, damage to highways, labor hours lost to traffic congestion, accident-induced injuries and fatalities, and contribution of fuel emissions to climate change. Excise taxes on motor fuel are imposed as specific taxes, levied at a set (nominal) dollar amount per unit consumed. In 2012, the nationwide effective excise tax rates on gasoline and diesel fuel were 48.9 cents per gallon and 54.1 cents per gallon respectively, and generated roughly $95 billion in revenue (Congressional Budget Office 2014). Fuel efficiency restrictions are imposed at the manufacturer level through Corporate Average Fuel Efficiency (CAFÉ) standards, and increased by 15 percent from 1998 to 2011 (Congressional Budget Office 2014).

Recent scholarship suggests that these policies may not be the most efficient form of addressing driving-related externalities, and instead call for the implementation of congestion pricing schemes. It also identifies the current system as inadequate in covering the costs associated with driving, as federal highway expenditures have exceeded the revenues designed to fund such spending in all but one year since 2000 (Congressional Budget Office 2014).

There are a number of studies that measure the welfare effects of motor vehicle taxes. Several studies (Poterba 1991, Metcalf 1993, Chernick and Reschovsky 1997) examine the incidence of taxes on motor fuel as part of an effort to find an optimal measure for a household's ability to pay such levies. That research finds that the fuel tax burden on high-income households increased when consumption and lifetime income measures were used in place of annual income data. Later studies (West 2004, Bento, Goulder, Jacobsen and Von Haefen 2009, and others) improve transportation-specific distributional estimates. Notably, these studies account for the endogeneity of vehicle stock and vehicle miles traveled decisions, as while an increase in vehicles owned is likely to lead to more driving, an exogenous increase in the demand for vehicle miles will in turn lead to greater vehicle purchases. The literature offers a detailed account of the incidence of changes in fuel prices on fuel consumed, yet it typically employs cross-sectional observations and ignores the effects that mobility may have on welfare.

Mobility effects are omitted in standard incidence analysis of transportation taxes, as the burden of an increase in fuel taxes is measured by the additional payments households make. This effectively uses the "envelope theorem" as rationale to discard the effects of additional responses (e.g. changes in residence) so long as households are initially maximizing. However, large changes in prices or taxes that induce major shifts in behavior may also have important consumption effects that should be accounted for in assessing welfare. To the extent that changes in fuel taxes induce mobility in some households, this effect should be captured in subsequent distributional analysis.

In addition, it is not always the case that consumers are initially maximizing. In an example that illustrates this basic point, Gruber and Koszegi (2004) show that distributional effects of taxes on tobacco varied sharply when consumers are allowed to be time-inconsistent in their addiction. A related line of thinking is developed in the new Keynesian tradition by Akerlof and Yellen (1985), who demonstrate the difference in welfare results in a model of "near rationality" when some agents do not maximize. Moreover, Mankiw (1985) show that a similar phenomenon occurs in a model with monopoly and menu costs.

There is reason to believe that the commuting pattern shifts generate significant changes in relationship between motor vehicle taxes and commuting patterns that need to be factored into a comprehensive analysis of welfare. Commuting trips were estimated to account for 28 percent of the total miles driven in 2009 (National Household

Transportation Survey 2009). The possibility that some households but not others could move in response to fuel price changes may have significant effects on distributional outcomes. Such findings may lead to changes in the willingness of policymakers to implement (or increase) fuel taxes, and thus affect efforts to fund transportation programs, combat urban sprawl, and discourage the causes of climate change.

A theoretical link between transportation costs and household mobility can be found in Alonso (1964), Mills (1967), and Muth (1969), whose work combines to form the Alonso-Muth-Mills (AMM) Model. The model is a widely used theory of urban spatial structure that captures how differences in wealth and preferences influence the optimal distance between the household residence and worksite. Households maximize consumption of residential land and a composite commodity subject to their budget constraint. Neighborhood selection is thus dependent on the relationship between wages, commuting costs (which proportionally increase with the distance from the employment center) and the price per unit of land (which is a decreasing function over the same metric).

The model produces a straightforward relationship between fuel costs and commuting distance. As travel becomes more expensive, residences will move closer to the workplace, thereby creating a more compact city with reduced commuting and fuel use. In general, households with lower incomes will live closer to the city if the ratio of their commuting costs to land area consumed is greater than that of the general population. [1]

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[1] A more thorough examination of this version of the AMM model may be found in Glaeser and Kahn (2004).

All models in this tradition imply that if fuel prices change there will be immediate changes in residential location patterns. In practice, however, there are substantial fixed costs of moving, so that small changes in fuel prices will not induce movements in location. Larger changes in fuel prices may induce locational shifts, but the presence and intensity of such responses may depend upon household characteristics such as income, homeownership type, and family size.

This is the first study that tests for the validity of the implicit constant migration assumption in analyses of motor vehicle tax incidence. There are a few ways that commuting shifts induced by motor vehicle market changes may differ across households. Relocating either the place of residence or of employment imposes two general types of costs on the household: monetary (e.g. moving costs, loss of income while switching jobs) and social costs (e.g. changing school districts, loss of workplace familiarity).

Intuition suggests that high-income households are more capable of absorbing the monetary costs associated with relocation, since such expenses likely represent a smaller percentage of their overall budget. One would therefore expect more commuting responsiveness from higher-income households – and indicate that static distributional estimates overestimate the burden of motor vehicle tax increases on that population. Meanwhile, the social costs of moving would seem to be greatest among households with more family members (and thus more neighborhood connectivity) and with longer tenures in the residence or place of employment. Such a finding may make large and older households less likely to change their commuting patterns in response to a change in motor vehicle prices, and thus absorb more of the burden from a tax increase than suggested in static estimates.

III. EMPIRICAL APPROACH

Theoretical Framework

Urban spatial theory provides a basic intuition for how migration responses to fuel price changes vary across households. In their analysis of urban sprawl and growth in the post-war era, Glaeser and Kahn (2004) construct a version of the AMM Model with a reservation location equation that connects commuting distance (*d*), wages (*w*), and commuting costs (*p*), expressed in equation (1). In this setup, the commute distance is an increasing function of earnings and their relative influence of on utility (ϕ_1) and a decreasing function of commuting costs and their utility effect (ϕ_2).

(1) $d = \phi_1 (w/p) + \phi_2$

(2) $\delta^2 d/\delta wp = -\phi_1/p^2$

Equation (2) expresses the second partial derivative of equation (1) with respect to wages and commuting costs. This equation shows how migration responses to fuel price shifts would change across income levels in the AMM Model. Recall that a general finding of the AMM Model is that increases in fuel costs lead to decreases in the size of an urban area. Equation (2) implies that such a rise in fuel costs will produce a stronger migration response among households with more wages, and thus shift a larger portion of the fuel tax burden to the remainder of the population. Equation (2) also indicates that the difference in response behaviors will decrease as fuel prices increase: this is because higher transportation costs will reduce the initial size of the area, thus leaving all households with a smaller range of migration.

In practice, there are a few ways that such a simplified model may misconstrue the actual relationship between transport cost-induced migration and household earnings. The above model assumes that moving (and thus changing the commuting distance) is costless. In practice, migration of either the residence or the place of employment produces both financial and social costs that could affect household behavior. Some of the financial costs associated with migration are constant across all households (e.g. hiring a moving truck or moving company when changing residences). Such costs lead to more concentrated movement in higher-income households, as they may be better able to absorb those short-term fixed reductions in net earnings in exchange for greater long-term utility.

Conversely, other financial costs associated with moving are dependent on household value and location. Equations (1) and (2) implicitly assume a constant homeownership rate regardless of income. However, higher-earning households are likely to exhibit greater take-up of homeownership, while lower-income households show greater preference for home rentals. Moreover, rentals may be more common in urban areas than in suburban locations.

The distinction between renting and owning a home may also change the amount of fixed costs associated with moving. Mortgages and real estate charges associated with residential sales, incurred by homeowners but not with renters, may prove costly. The relatively short time period of rental agreements could also induce more movement among renters relative to the homeowner population, which would indicate an increased ability to migrate among lower-earning observations.

The social costs of moving could be correlated with household demographics in ways that influence migration behavior. The social ties to a residence or job, for example, are likely to increase with the time spent in those locations. Such a relationship may make older households, who are also likely to earn more wages than the average population, less likely to move in response to a fuel price increase. Increases in family size may also add to a reluctance to migrate, as factors like school enrollment and balancing multiple labor commutes may offer value to foregoing a switch in residences or employment.

The model above assumes that transportation costs per mile are uniform for the entire population. In practice, such costs vary both within the motor vehicle market (through discrepancies in the fuel efficiency of certain vehicles) and between motor vehicles and other methods of transportation. In particular, households that use alternative transportation methods such as trains, buses, bicycles and walking reduce their connection between transportation costs and the price of fuel (in the latter two cases, to zero). Therefore, households that rely on those transportation means are likely to exhibit much lower migration responses to fuel changes than the population at-large.

Traditional urban spatial models assume that all employment is located in the center of the city in order to make the land price function endogenous and consistent (in this case, based on distance from the city center). In practice, employment opportunities are ubiquitous across a metropolitan area, though they may be more concentrated in dense and centrally located regions. Imperfect concentration of employment and other factors all contribute to the relationship between land pricing and distance from the central city district varying greatly across urban areas. Such a variable “rent gradient” affects the housing options available for a given budget allocation. For instance, low-income households may find it easier to move closer to the city that exhibits more urban sprawl (such as Houston) than they would in one with a more compact spatial structure (such as New York City). This issue may also lead to undervaluing of outlying areas and overvaluing of more urban locations, as the presence of even limited employment opportunities in suburban areas would add value to their residences.

Finally, measuring fuel price commute responses are complicated by the unique qualities of employment and residential patterns in each geographic area. For example, the standard approach models the residential choice as a function of features of one city. This requires adjustments must be made for residences that lie in close proximity to more than one urban area. Henderson and Mitra (1995) and others construct a polycentric model that surrounded the main urban area with endogenous ‘edge cities’. This model retains some of the core principles present in the one city setup – residential choices are still a function of the tradeoff between commuting distance and land pricing – but additional considerations of developers and a more complex pricing scheme makes the policy directives of such a model less clear.

Regression Analysis

The basis for all subsequent empirical analysis is described in equation (3). The migration variable adopted in this research is the average one-way commute, in miles, of the household laborers (*d*). Such a measure is easier to identify than mobility metrics used in other studies (such as the relationship to the “center of the city,” which may be difficult to define). It also controls for the dispersed employment issue that arose in equations (1) and (2), as commuting distances are not affected by the location of the employment center. Finally, use of commute length as the outcome measure captures individual responses made through a change in both household and employment location.

$$(3)\ d_{ijt}= \alpha_0+ \alpha_1\ln(p_{jt})+ \alpha_2\ln(w_{ijt})+ \alpha_3 t_j+ \varepsilon_{ijt}$$

Note that the commute length response to a fuel change is not equivalent to the total change in total household mobility. Such a measurement does not capture the migration response of non-employed households, which are much more likely to be aged or low-income than the general population.[2] Fuel prices increases also raise the cost of commuting to services and leisure activities, which may induce migration responses with different distributional trends than those presented in this research.

The regression analysis indexes observations by household (*i*), time period (*t*), and metropolitan area (*j*). This approach adopts a fixed effects approach, where changes are only measured within a metropolitan area *j* and then compiled across the entire sample. The fixed effects methodology accounts for the city-specific factors than can influence migration decisions such as the presence of nearby cities, geographic features, and changes in the rent gradient across metropolitan areas. The time factor is also included in the independent variable set of each stage in order to capture ongoing changes in urbanization patterns and vehicle usage across the entire sample. All regressions cluster observations at the metropolitan level to account for potential correlation between data points.

[2] Measuring the response of aged households in particular would also pose challenges to accurately observing household ability to pay, as current income observations may not provide an accurate representation of household well-being.

The fuel price index (*p*) and an income measure (*w*) are the key metrics of interest in the dependent variable set. The fuel price index will be compiled using monthly data for the three years prior to the survey date, with the weight given to each observation doubling every three months closer to the time of the interview. If consistent with the theory presented in the traditional models, increases in fuel prices raise commuting costs and result in households moving closer to their employment centers. Income will be measured as the sum of all household incomes over the previous year, with an alternative to this measure explored in the sensitivity analysis.

The equation implements a specification that is broadly consistent with approaches taken by Glaeser et. al. (2008) and Song and Zenou (2006). The independent variables that are log transformed are the fuel price index and household income measures. Ramsey RESET specification tests were conducted between this specification and one with the dependent variable also log transformed, with the chosen specification coming out more favorably. Taking the natural log of the fuel price variable ensures that differences in the distance base do not affect the magnitude of the resulting coefficient. Analysis of the results will at times exploit this feature of the model to measure the implied effect of a ten percent increase in fuel prices on commuting distance, taken by multiplying the coefficient by *ln(1.1)*.

While equation (3) accounts for a number of the obstacles to estimation discussed in the theory, it does not provide an avenue for analyzing the distributional effects of fuel responses across households. In order to achieve this, the empirical analysis includes a series of stratifications that provide insight into how such a response varies by income and other metrics. These stratifications will lead to quintile-specific quasi-elasticities for the commuting response across income measures, which indicate how migration responses to fuel policy changes may shift across households.

Additional stratifications will be implemented to both gain a better understanding how certain variables affect migration and to control for potential sources of endogeneity. In order to address the complications presented with the relationship between fuel prices, commute mode and availability and commuting length, all regressions stratify households into three categories: those that commute with a motor vehicle and which have at least as many vehicles as adults, motor vehicle commuters with fewer vehicles than adults, and households that commute with alternative measures of transportation. Other transportation incidence studies, such as West (2004), control for endogeneity of vehicle stock choices and the demand for fuel. Since this analysis uses commute length and not vehicle miles travelled as the outcome measure, the endogeneity concern is not expressed strictly household vehicle stock, but with two similar metrics.

The first is whether the number of vehicles in the household is at least as great as the number of adults present.[3] For example, if a fuel price increase leads to change in a two-adult household from owning two vehicles to one may influence decisions of optimal commute length. On the other hand, if the same household instead increases the vehicles owned from two vehicles to three commuting choices are not likely to be affected, as for each adult the option to commute with a motor vehicle is unchanged. The second factor is the availability of alternative means of transportation. Fuel price changes may lead to

[3] For the purposes of this research, adults mean either (a) the householder or spouse; or (b) a full-time laborer aged 16 or older. This definition is intended to exclude dependents still in school, whose vehicle status we assume does not affect the commuting decisions of the household. Adults are used in place of laborers in order to ensure that transportation demands of non-laborer adults are accounted for.

shifts in the coverage of public transportation, which may change the commuting habits of households disproportionately depending on the commuting strategy employed.

The empirical analysis also implements stratifications across indicator variables for homeownership and recent residential migration. Stratification across these variables will offer insight into whether the financial and social costs associated with moving have an effect on the practical decisions of households to migrate. Consistent with the earlier discussions of those variables, one would expect that observations that own their home and that have lived in a location for a longer period of time will be less likely to undergo fuel price-induced mobility.

The lack of a natural experiment to identify the relationship between fuel prices and commute length incidence warrants caution when using this research to make statements about causal inference. Due to the use of an approach that uses time-series data and metropolitan-level fixed effects, unobserved variables that change over the course of the sample and which affect the incidence of commuting patterns have the potential to affect the findings presented here. For instance, if increased awareness of global warming caused upper-income households to disproportionately reduce their driving consumption as fuel prices increased, than this research would risk attributing such behavior to price shifts. Future research with data conducted in a more controlled setting may have the potential to improve on this limitation.

There are a couple of other factors that are not accounted for in this estimation process. This research does not include information on land and housing prices, which may be associated with both fuel prices and commuting responses (e.g. increases in fuel prices may reduce the price of land in suburban spaces and change migration patterns there). The exclusion of housing prices prevents direct measurement of any changes in the rental gradient across given areas. The effect of this omission depends on the spread and intensity of changes across cities. “Urban renewal” processes may lead to an overstatement of high-income responses if they coincide with fuel price increases, while an increase in the appeal of suburban spaces would have the opposite effect.

Furthermore, this analysis is restricted from measuring any effects of changes in the take-up of commuting types on the distribution of commute length responses. While general take-up of the different commute mode options (motor vehicles travel with at least as many vehicles as adults, with fewer vehicles than adults, and alternative means of transportation) held constant across all observations and years, city-specific changes may lead to bias in the response metric. For instance, if improvements in the public transit of a city lead to increased usage from high-income groups over the course of the sample, failure to observe such a pattern will therefore lead to overestimation of their commute response in this research.

IV. DATA

The primary data source for this research is the American Household Survey (AHS). The AHS provides meticulous information on U.S. housing characteristics, and is run as two separate surveys. The Metropolitan Survey observes the housing market in particular urban areas (with a rotating set of markets observed in each round). Meanwhile, the National Survey provides a nationally representative housing sample, and includes special breakout questions for six of the largest U.S. markets – New York, Los Angeles, Chicago, Philadelphia, Detroit, and Northern New Jersey.

This research draws from observations in the Metropolitan Survey and in the large market breakout of the National Surveys from 1997 through 2009.[4] Both of these samples are designed be representative at the metropolitan level, and define all variables used in this analysis identically. The AHS also offers detailed information on neighborhood features, time and distance spent commuting to work for each household laborer, and migration and employment status data for the previous two years, which allows for detailed analysis on the commuting response to fuel price changes.

Historical data on fuel prices is taken from the Energy Information Administration (EIA), which offers historical price information by state and month for the duration of the AHS time period. Because U.S. taxes on motor fuel are constructed as specific taxes, an increase in the tax on gasoline may be modeled as having an identical consumption effect as an equivalent price increase induced from a change in supply.[5] This symmetry allows for use of fuel price changes as a proxy for changes in tax policy, which offers much more variation and thus increased identification of the relationship of interest. This dataset will not capture within-state price variation, which limits the ability of this research to understand intrastate migration.[6] However, past research has found intrastate gas price variation to be relatively small, with a standard deviation of roughly

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[4] Observations from nine survey years in this time period were used, and include all odd-numbered years from 1997 through 2009, as well as 2002 and 2004.

[5] This assumes that the burden of the fuel tax is borne entirely by consumers, and that a change fuel tax policy would not cause different behavioral responses than supply-side pricing shifts. Research by Alm, Sennoga and Skidmore (2005) produces empirical work that is largely consistent with this assumption, particularly in urban areas.

[6] EIA fuel price information is the most precise dataset available for the purposes of this research. Use of more localized data from crowdsourcing sites was explored, but improvements in the locational knowledge were not deemed to justify subsequent restrictions in the empirical sample.

four cents per gallon (West 2004). General price index information from the Bureau of Labor Statistics is used to fulfill the data requirements of this research. The final sample consists of over 188,000 households, with an average of about 21,000 observations per sample year.

V. RESULTS

Summary Statistics

Table 1 displays household summary statistics broken out by income quintile.[7] Income is skewed toward the upper end of the distribution, with the average of the entire sample taking on an annual value around $20,000 more than the average of the third quintile. The table also shows a positive correlation between income and suburbanization, with households in the bottom income quintile being almost twice as likely to reside in an urban area as households in the top income quintile.[8] The number of motor vehicles and the length of the average commute length increase with income in the sample, which is consistent with more of the population living in less dense locations. Commute mode choices are relatively constant across the income distribution, with the exception of the lower income quintile, whose workers are far more likely than the rest of the sample to use alternative methods of transportation to a motor vehicle.

The summary statistics are parsed differently in Table 2, in this case by quintiles measuring the average household distance from home to work. Average income is

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[7] All income sorting is separated by survey year and metropolitan area. This method ensures that the income of, say, a family surveyed in Kansas City in 2003 is not compared with that of a family living in New York in 2003, or with a family in Kansas City in 1999.

[8] This research (and that of the AHS) uses Bureau of Labor Statistics definitions of urban, suburban, and rural spaces when making such classifications.

highest among households with the longest commute, while the income differentials in the other four quintiles are relatively small. A similar trend may be found with the probability of suburban location, as the closest three distance categories have noticeably higher levels of urban households than the furthest two commuting quintiles. Even though the sample is comprised entirely of non-rural households, only workers with the shortest commutes exhibit strong take-up of alternative methods of transportation. About thirty-five percent of the households in the closest quintile used such methods (which included bus, train, bike, and foot travel), while less than ten percent of all other groups did so. Tellingly, alternative transportation use was below ten percent even among the second-closest commuting quintile, which had an average commuting distance of just over five miles. This result speaks to the ubiquity of the motor vehicle in the United States. Householder age and ethnic heterogeneity do not exhibit any noticeable trends across distance categories.

Table 3 presents averages of the same variables broken out by categories that may influence household commuting responses. As expected, there is a substantial difference between the average profile of households that rent their dwelling and that of households that own their home. Homeowners have an average income more than twice that of renters, while renters almost twice as likely to live in an urban area and to commute to work using an alternative to motor vehicles than homeowners. A discrepancy is also present in commuting distance; homeowners commute on average about two miles further (each way) than their renter counterparts. Urban and suburban dwellers exhibit differences similar to those of renters and owners, with urban dwellers earning lower income, facing shorter commutes, and being more than twice as likely to commute without a motor vehicle as suburban residents. The similarities between the split across homeownership and urban status categories are consistent with the fact that the majority of rental properties are located in urban areas.

Table 3 also shows summary figures broken out by the commuting method. Average income, age, and education levels are nearly identical for 'Motor Vehicle' and 'Other' commuters.[9] However, households that use a motor vehicle to commute faced a commute nearly twice as long as households using alternative methods of transportation. Finally, transportation choices other than commuting are examined through summary statistics broken out by level of vehicle ownership. Notably, average household income for units owning two vehicles nearly doubles that of one-vehicle households, representing by far the largest income jump across ownership categories. More than two-thirds of households that did not own a motor vehicle lived in urban areas. Despite the lack of vehicle ownership, nearly half of these households used a motor vehicle to commute to work (which is possible either through renting a vehicle or by carpooling with other vehicle-owners), and faced a commute more than 70 percent as long as households with three or more owned vehicles.

Table 4 shows average fuel prices across sample years and regional areas. (Prices are in real 2001 dollars to account for inflationary effects.) The results show some variation across geographic areas – in a given year, average prices may be as much as 25 percent higher in the West than in the Midwest, for instance. However, the more notable variation occurs across time, as in every region real prices increased by more than 100 percent from 1997 to 2009.

[9] Households with commuters of both types were considered 'Other' units for all empirical analysis.

Tables 1 through 4 present the mean characteristics of the sample. However, as this research is focused on the incidence of changes in the fuel market, it is useful to examine some of the distributional patterns exhibited by the observations. Figures 1 through 7 display density histograms of commuting distance, with commutes sorted into five-mile increments. Figure 1 presents the commuting density of the entire sample. The results show that commuting distances can be sorted into three roughly equal categories: commutes of below five miles, which comprise roughly 30 percent of the total; commutes of between 5 and 15 miles, which represent about 40 percent of total commutes; and commutes of greater than 15 miles, which represent the remaining 30 percent.

Figure 2 confines commutes to those observed in the first year (1997) and last year (2009) of the sample window. This figure provides compelling evidence of ongoing sprawl in metropolitan areas. Households in the 2009 survey were about five percent less likely than those in the 1997 survey to engage in commutes of under ten miles, and therefore equally more likely to undertake longer commutes.

Figure 3 produces the commuting density plots of the households in the highest and lowest income quintiles. Consistent with the information presented in Table 1, this figure shows that low-income quintile households are more than ten percent more likely to have a commute length of less than ten miles than high-income households. However, the figure also shows that as the commuting distance increases, the relative difference between high- and low-income frequencies grows. For instance, while each group is almost exactly as likely to engage in a commute of between ten and fifteen miles, high-income households are more than twice as likely as low-income households to undertake a commute between 25 and 30 miles each way.

While Figure 3 shows that commuting distances increase with income, Figure 4 and Figure 5 indicate the gap in commuting length between high- and low-income workers has decreased in recent years. Figure 4 shows the commuting densities for the lowest income quintile in 1997 and 2009. In 2009, households were eight percent less likely to engage in commutes of under five miles, and more likely to undergo every five-mile commuting category above ten miles than they were in 1997.

Meanwhile, Figure 5 shows that commuting trends of high-income households over the same time period were mostly constant, showing a nearly identical tendency to engage in short and long commutes across the twelve-year window. Independent of other market forces, it could indicate that low-income households were moving further away from work – perhaps, as asserted by Brueckner (1997) and others, to obtain the public service improvements available in suburban areas. However, if increased fuel prices made shorter commutes more attractive, or if an increase in high-income urban dwellings drove up home prices, it may suggest that this migration reduced welfare levels of the low-income population.

Figure 6 shows the commuting density plots of individuals in the Northeast and the West regions of the U.S. respectively. This figure shows that individuals in the West undergo commutes of consistently longer length than their Northeastern counterparts, which is consistent with the population density patterns and geographic constraints faced in each region. These patterns highlight the importance of controlling for geography in regression analysis of household spatial responses.

Finally, Figure 7 displays the commuting trends of households that rent their residence and households that own their home. The renting population is more likely to engage in commutes of less than ten miles and less likely to engage in longer commutes than homeowner households.

Regression Results

Table 5 presents the results of regressions performed on the entire sample of AHS households. The left-most column shows the results of a regression run on the entire sample, with no commute mode stratification. The remaining three columns display the regression results when the sample is divided by commute choice and car availability. The second column displays the results for households that commute by motor vehicle with at least as many motor vehicles as adults, thereby giving each adult maximum agency over how to achieve transportation needs. The third column returns the results for households that include a motor vehicle commuter, but with fewer vehicles than adults, which may make these households more sensitive to commute price changes than households with more vehicles. Finally, the last column shows the results for “Other” households that exclusively commute by means other than a motor vehicle (or who work from home). The direct effect of a change in fuel prices on this population would seem to be much smaller than in other households.

Each of the specifications in Table 5 that are run on households that commute with a motor vehicle return a negative correlation coefficient for the fuel price index that is statistically significantly different from zero. This finding is consistent with the theory that consumers respond to the net wage decrease triggered a rise in fuel prices through moving closer to their workplace and thereby reducing their commuting costs. The magnitude of each coefficient differs by the vehicle availability of the household.

The coefficients indicate that a ten percent increase in the fuel price index triggers a commute reduction of 0.06 miles (or roughly 0.5 percent of the mean commute) among households with at least a vehicle per adult, but a 0.18 mile (1.5 percent) decline in households with fewer vehicles than adults.[10] This suggests that households with less vehicle availability may be more sensitive to changes in the price of transportation, and confirms the importance of accounting for the interaction between vehicle availability and commute length. Across the whole sample, a ten percent increase in the fuel index decreases commute length by 0.08 miles, which supports the notion that transportation policies may have substantial power in mitigating the effects of urban sprawl.

Unlike with other households, observations that do not commute with a motor vehicle do not have a statistically meaningful relationship with the fuel price index in these regressions. This finding supports the notion that motor fuel costs have little to no impact on the net transportation costs of households commuting without a motor vehicle. Furthermore, the commuting distance of these households experienced no noticeable change across the observed time period. This result may speak to the idea "Other" commuters, shown to have much shorter commutes than the general population, are insensitive to the forces behind urban sprawl, as developments like suburban road and vehicle improvements have no impact on the efficacy or cost of, for instance, the commute with light rail.

[10] The fuel prices and commuting length relationship is stated in terms of the mileage response to a ten percent increase in fuel prices in order to offer a sense of magnitude to all findings. As the average household commute is just over 13 miles, a reduction in commute length of -0.13 miles to a ten percent fuel price increase would represent a price elasticity of commute length of -0.10.

All of the regressions except for those run on non-vehicle commuters exhibit a positive time trend, which is consistent with the trends shown in the summary figures. The regressions also support the notion that the presence of children has a positive effect on commute length, pushing the average distance between 0.37 miles further than it otherwise would have been across all households. This is consistent with the argument that suburban spaces have improved access to public services, as households with children engage in increased use of public schools, health care, and other public goods provided by governments. A similar relationship is reported in the whole sample regression for the presence of multiple adults, suggesting that commute lengths increase when residential choices must deal with the demands of multiple actors.

In all three specifications the linear logged income variable takes on a negative value, while the log of squared income is positive. This result suggests that increased earnings generally reduce commute lengths, but that magnitude the effect decreases as the upper end of the income distribution approaches. Such a finding suggests a complex relationship between income and commuting, and will be investigated further in regressions with stratifications across household income.

The results found in Table 5 offer insight into how fuel prices impact the commute length of all households, and are illustrative of the importance of other factors in determining the magnitude and size of such an impact. However, it does not reveal anything about the income incidence of such an effect, as Table 4 does not stratify across such a measure. The remaining regressions stratify across household income to understand the distributional patterns of migration.

The results of Table 6 show significant variation in the motivating factors of changes to commuting patterns. Most importantly for this analysis, the effect of fuel prices on commuting behavior is heterogeneous across income groups. The relationship between the fuel price index and commute length generally becomes more negative as household income increases, producing positive values for the lowest quintile and negative values for all other categories in the whole sample regression, although the result is only statistically significant among the middle three income groups. Since a fuel price increase causes commuting costs to rise, a negative coefficient value indicates that a given household responds by moving closer to work and reducing commuting costs. This suggests that positive utility responses to fuel price changes are undertaken predominantly by the middle and upper income classes, and may emerge in response to a powerful effect of the fixed costs associated with moving.

Further stratification by commute mode in Table 6 illustrates how the fuel price index changes with the stock and use of household vehicles. The relationship between fuel prices and commute length among households with matching vehicles to adults mirrors the trend of the whole sample, albeit in a more muted fashion, as only households in the middle income category have a statistically significant fuel price coefficient. This is also true in vehicle commuters with fewer adults than vehicles, although the magnitudes of the fuel price coefficient are greater in the middle and upper quintiles. The results of the middle-income households in this category suggest that a ten percent fuel index increase would lead to a commuting reduction of 0.47 miles, more than double that of middle-income households that were “sufficient vehicle” car commuters and the whole sample.

Meanwhile, the only statistically significant fuel-commute relationship for non-vehicle commuters occurs through a positive effect on the lowest income category. This result indicates that among these households, an increase in fuel prices increased their average distance to work by 0.24 miles. One explanation for this result consistent with urban theory is that increased transportation prices made rental properties closer to employment centers more valuable and thus too expensive for low-income individuals living in these locations. Looking across commute mode types, white households exhibited increased commute length when traveling with a motor vehicle and decreased lengths when commuting by other means. Such a result is suggestive of a racial disparity in the commuting behavior of households, and broadly supports the findings of Raphael et. al. (2001) and others. These regressions offer an understanding of the income incidence of migration-based fuel price responses.

Tables 7 and 8 report the results of further stratifications, in this case across measures of income, commute mode, and homeownership status. Renters are believed to face lower fixed costs associated with migration, since their living arrangements typically come with fixed end dates. On the other hand, for homeowners fuel prices may change not only the commuting costs associated with travel to employment, but also their wealth stock, as subsequent changes in land pricing could have an effect on the value of their home. Therefore, if owners believe that movement in fuel prices is indicative of a long-term trend, they may have increased incentive to relocate before such adjustments are made to the housing market.

The results of these regressions suggest that homeownership status has a notable influence on how households factor fuel prices into their commuting responses. Among homeowners, the spatial response to fuel price changes is relatively constant across income levels. In Table 7, for instance, the relationship between the fuel price index and commute length among the whole sample produces a coefficient statistically different from zero in only the middle three income quintiles (negative in each case). The magnitudes across all quintiles range only from -0.84 to -2.05, a smaller discrepancy than what was reported in earlier tables.

Meanwhile, the spatial response to changes for renters varies significantly across income categories. For households in the lowest income quintile, a rise in the fuel price index causes an increase in commuting distance that is statistically significant for the whole sample regressions as well as those run on the "low vehicle" and other commuting categories. However, as income increases, the relationship between fuel prices and commuting length becomes negative, with the strongest relationships reported in the middle and upper income categories.

For the whole sample regressions, the coefficients indicate a commuting length response to a ten percent fuel index ranging from a 0.15 mile-increase for the lowest quintile to a 0.27 mile-decrease for the middle quintile. This dichotomy alludes to homeownership decisions being a significant determinant in the incidence of commuting responses. One possible reason for this difference may be that there are high, non-monetary costs associated with homeownership – which could range from the additional time spent with real estate agents to the social cost of changing neighborhoods. One would expect such responses to be relatively constant across the income level of the homeowner, and therefore would not anticipate a significant income trend in the regressions that appear in Table 76.

These regressions also produced a few noteworthy differences across commute mode categories. While the whole sample regression run on renters produced significant evidence of a positive relationship between fuel prices and commute length, the strength of that relationship varied significantly across commute type and vehicle availability. Among households with at least as many vehicles as adults, the coefficient was not significantly different from zero. Meanwhile, alternative commuters and “low vehicle” car commuters reported significant positive coefficients, with a ten percent increase in the index increasing commute length in those groups by 0.26 miles and 0.60 miles respectively. The magnitude of the latter finding is particularly large relative to earlier results, and supports earlier evidence that low-income groups see their commute lengths increase when the cost of transportation rises for the general population.

Finally, the results for non-vehicle commuting renters in Table 8 indicates a large difference in the effect of fuel prices on commuting length at the tails of the income spectrum. While low-income households in this group are predicted to respond to the ten percent price increase by adding 0.26 miles to their commute, the regressions expect high-income renters who commute without a motor vehicle to respond to the same increase with a decline of 1.19 miles in their commute length. This result suggests that the motivations behind the homeownership and residential selection may be quite different across these two populations,

This research also stratifies across residential migration status in order to help determine why the response to fuel prices by commuting type changed for high-income and low-income households. A worker’s distance to work can change for three reasons: because she changed her residence (but not her employment), because she changed her employment location (but not her residence), or because both the residence and the employment location changed. Although the American Housing Survey does not include records on employment location and switching, it does have a variable indicating whether or not the family unit changed residences in the past two years.

The sample is further stratified through the recent migration variable to examine how the behavior of “recent movers” (those who reported a move in the last two years) differs from that of “continuous dwellers” (those who lived in the same home for the past two years). Differences in spatial response patterns across groups may therefore provide useful information about how households change residences or jobs in response to an increase in fuel prices.

Tables 9 and 10 display regressions that stratify the sample by income level commute type, and migration status. As with all of the regressions presented thus far, no single stratification in these tables produces a set of fuel price coefficients that are statistically significant from zero in every income quintile. However, there are a few differences to note across these results. Among continuous dwellers, the statistically significant fuel price coefficients are both exclusively negative and in the middle and upper income quintiles. This result confirms the income trend highlighted earlier, and provides evidence that households are able to respond to increases in fuel prices through means other than residential relocation, either through employment switching, teleworking, or other strategies. In contrast, significant fuel price coefficients for recent movers both occur in the lowest quintile (where they are positive) and in the middle three income quintiles (where they are negative).

As was the case in the previous regressions, Tables 9 and 10 reveal significant intra-category differences across income levels. The difference in responses across income groups was especially stark among recent movers with that either commuted without a motor vehicle or which had fewer vehicles than adults. For instance, recent movers in the latter category and the middle income level are projected to respond to a ten percent fuel price increase with a 0.61 mile decrease in commute length: meanwhile, households in the lowest income category would be expected to add 0.37 miles to their commute based on the same change in fuel prices. These findings highlight the importance of both commute mode and mobility in generating incidence patterns of transportation taxes across income groups.

Sensitivity Analysis

The empirical work thus far has presented evidence of patterns in the spatial response of households to fuel prices across income, homeownership, commuting type, and demographics. However, given the potential barriers to consistent estimation discussed earlier, there is value in understanding how the results change when a few alternative specification methods are employed.

The first part of the sensitivity analysis replaces the “current” fuel price index used in the standard analysis with a “future” price index that is compiled in the same way, but which uses data from two years after the commuting data was recorded. This specification offers a placebo test for the effect of fuel prices on commuting values. If future fuel prices produce a significant relationship with current commuting distances, it may indicate modeling issues with the fuel price and commute structure in the previous tables, through endogeneity, omitted variable bias, or other means. Table 11 shows the results of the placebo regressions. The results show no significant findings between future fuel prices and commute length, indicating that households do not have foresight in future price shifts.

The next portion of the sensitivity analysis explores changing the way that the fuel price index is compiled. In the main set of regressions a fuel price index was used that included monthly fuel price data from the three years prior to the interview date, with the weight of each data point doubling in three month increments as the data grew closer to the survey time. Table 12 presents the results of the same regressions performed in Table 5, but now with a fuel price index that only uses data from only one year before the interview date. This is designed to test if main regression results were in fact driven by more dated price information, whose effect on migration decisions is less clear than recent pricing shifts.[11]

The results in Table 12 ease concerns about the effect of changes in the fuel price index on the results of interest, as its findings are markedly similar to those in Table 5. As with the earlier work, the fuel price index produced a significant and negative relationship for commute length of the entire sample, with a strong negative effect among "low vehicle" car commuters, a weaker link with "sufficient vehicle" commuters, and no evidence of a relationship for households the employ alternative commuting tactics. The coefficient for the entire sample indicates that a ten percent increase in the fuel index would produce a decline in commuting by 0.10 miles, statistically indistinguishable from

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[11] The alternative index also uses a weighting scheme that doubles the weight of each data point every three months approaching the survey date. Additional specifications that lengthened the weighting period to six months and which weighted all data points equally were also tested, and returned results similar to those presented in Table 9.

the 0.08-mile decline reported in Table 5. Commute lengths also increased with time for vehicle-commuting households only, as was the case in previous work.

The third sensitivity test examines how the results change when an adjustment is made to the income ranking mechanism. Past work by Poterba (1991) and others have argued that using annual an annual income metric to measure the progressivity of a tax system (as is done here) will overstate the tax burden of the system on low-income households if the goal is to measure lifetime ability to pay, since the income of students, other younger workers early in their careers, and retired households who previously earned high-incomes at the bottom of the income distribution.

While the sample was confined to families with householders under 65 years of age in order to measure the behavior of 'working-age' households only, the AHS does not provide the annual consumption or lifetime income measures suggested by the literature in order to more accurately gauge progressivity. In order to test the effect of this specification choice, this research adopts an alternative specification where households are again ranked by income, but only within the following age categories of the householder: under 35 years of age, between 35 and 45 years old, between 45 and 55 years of age, between 55 and 65 years of age, and over 65 years old. This step is designed to help control for the life-cycle effects of the household in generating the income measure.

Table 13 presents the reported magnitude and significance level of the fuel price coefficient in non-stratified, whole sample regressions broken out by income quintile and age category. The results are largely consistent with what was presented in earlier work, as upward shifts fuel prices increasingly pull households closer to work as their income rises. Evidence of this relationship is strongest among middle-income quintiles: notably, there is no clear sign of a discrepancy in the “pull” effect across age categories. As previous work by West (2004) and others indicated that the distribution of the vehicle and miles traveled response was borne most by middle-income families, such a result suggests that the total fuel price response is more balanced across household income.

Notably, the observations with a householder above aged 65 displayed a strong “push” effect in the bottom quintile, with a ten percent fuel price index increasing average commute length by 0.34 miles. Such a result may be influenced by labor force participation changes, which are highly variable among this age group. In general, access to improved measures of household ability to pay would help to place the results generated in this research in better context.

The regression analysis presents a complex relationship between commuting responses to fuel price shifts and household characteristics. However, there were a few consistent findings that may be used to enhance public understanding of how mobility responses could impact existing distributional estimates of fuel taxes. While the differences were only sometimes significant, each set of specifications that stratified by household income returned a fuel price coefficient that grew stronger (more negative) as income increased, suggesting that higher-income households move closer in response to price increases than the general population.

Additionally, stratification by homeownership type revealed that this result was driven by the behavior of renting households, while owner households exhibited no noticeable trend in response rates across income levels. The results also offer consistent evidence of urban poverty trap among racial minorities, as white, low-income households consistently undertook longer commutes than other low-income families. Further examination of this result revealed a relationship with commute type, as white low-income households lived closer to work if they did not commute with a motor vehicle but further away from work if they were motor vehicle commuters.

VI. DISCUSSION

This research establishes the distributional impact of migratory responses to shifts in fuel tax policy. While existing estimates of motor vehicle policy incidence have become increasingly precise in their measurement of transportation consumption responses, such studies almost always assume that residential or employment relocation responses are constant across the population. However, theory on urban spatial structure has shown that such a mobility response is not likely to be uniform across measures of household wealth, size and age. Given the priority placed upon tax equity and economic diversity in public finance, such results may have significant ramifications on the evaluation of motor vehicle taxes as a policy tool.

From a general standpoint, the empirical results show that consumers respond to increases in fuel prices by reducing their commuting length. These findings also indicate that shifts commuting length as a response to shifts in motor vehicle taxes change across measures of household income. Given a rise in fuel prices, households with higher income values reported an average spatial response that generally reduced transportation costs. Meanwhile, families in the lowest income category reported increases their commute length in response to a rise in fuel prices, compounding the additional transportation expenses incurred for this group. Further research on the distribution of these policies that used consumption or lifetime income measures to rank households would identify how the use of annual income in this analysis affects the findings: in absence of this work, the previous literature would indicate that the results would represent a lower-bound on the progressivity of transportation levies.

Increased stratification work indicates that the disparity in commute response is related to homeownership status. The adjustment of homeowners was welfare-improving and of similar magnitude across levels of income, while for the home-renting population fuel price increases had a "push" effect on commuting distances for low-income families and a "pull" effect on commuting lengths for high-income housing units, particularly among families that rent their homes. This result affirms the importance of fixed moving costs in migration decisions predicted by urban spatial theory.

The empirical work also offers analysis on the effect of changes in transportation costs on the commuting length of households often identified as being part of the 'urban poverty trap,' which is chiefly defined by low-income levels and a lack of home and vehicle ownership. The results show that the "push" effect on low-income households is particularly strong among families that rent their home and use methods other than a motor vehicle to commute to work. Such a result indicates that motor vehicle taxes intensify urban poverty trap, and suggest that public transportation or rental vouchers may be useful in offsetting the regressive effects of such policies.

Table 1: Summary Statistics by Income Quintile

Variable	*Q1 (Low)*	*Q2*	*Q3*	*Q4*	*Q5*	*Total*
Household Income	$14,408	$33,912	$54,520	$83,033	$185,898	$74,752
Age of Household Head	45.5	44.7	45.0	45.3	47.1	45.5
Urban Households (%)	44.9	38.0	32.1	27.0	23.3	33.0
Households with Education > High School (%)	48.3	58.4	66.5	74.7	85.0	66.7
White Households (%)	45.9	51.4	57.8	64.3	71.4	58.2
Households Commuting with Motor Vehicle (%)	82.9	87.5	89.5	90.6	89.6	88.0
Number of Vehicles	1.34	1.69	2.01	2.35	2.61	2.00
One Way Commute Length (mi)	11.70	12.43	13.10	14.05	13.99	13.06
N	*36,171*	*36,989*	*37,115*	*37,121*	*37,220*	*184,616*

NOTE: Sample draws from 1997-2009 American Housing Surveys. All reported numbers are mean values unless stated otherwise. Household income is expressed in real 2001 dollars. Households labeled as 'white' exclude those who identify themselves as Hispanic. Urban definition taken from those in the AHS and U.S. Census Bureau.

Table 2: Summary Statistics by Distance to Work Quintile						
Variable	*Q1 (Short)*	*Q2*	*Q3*	*Q4*	*Q5*	*Total*
Household Income	$73,092	$71,500	$72,214	$75,976	$80,767	$74,752
Age of Household Head	45.6	45.6	45.7	45.5	45.2	45.5
Urban Households (%)	36.0	37.7	35.1	30.8	25.9	33.0
Households with Education > High School (%)	65.0	65.8	66.7	67.4	68.3	66.7
White Households (%)	60.5	57.1	56.4	57.4	60.1	58.2
Households Commuting with Motor Vehicle (%)	69.4	90.3	91.9	92.6	94.1	88
Number of Vehicles	1.9	1.95	1.98	2.04	2.13	2
One Way Commute Length (mi)	1.52	5.2	9.68	15.83	31.78	13.06
N	*34,006*	*36,921*	*38,226*	*37,648*	*37,815*	*184,616*

NOTE: Sample draws from 1997-2009 American Housing Surveys. All reported numbers are mean values unless stated otherwise. Household income is expressed in real 2001 dollars. Households labeled as 'white' exclude those who identify themselves as Hispanic. Urban definition taken from those in the AHS and U.S. Census Bureau.

Table 3: Summary Statistics, Various Categories

	Homeownership		Urban Status		Commute Type	
Variable	*Rent*	*Owners*	*Urban*	*Suburban*	*Vehicle*	*Other*
Household Income	$41,944	$91,793	$63,161	$80,461	$74,906	$73,624
Age of Household Head	39.1	48.7	43.8	46.3	45.6	44.6
Urban Households (%)	47.0	25.7	100	0	30.0	55.2
Households with Education > High School (%)	59.6	70.3	64.3	67.8	66.4	68.6
White Households (%)	43.1	66.1	43.5	65.5	59.4	49.5
Households Commuting with Motor Vehicle (%)	82.0	91.2	80.0	92.0	100	0
Number of Vehicles	1.47	2.28	1.69	2.15	2.09	1.36
Commute Dist. (mi)	11.80	13.72	11.45	13.86	13.88	7.02
N	*63,110*	*121,506*	*60,924*	*123,692*	*162,521*	*22,095*

	Vehicles Owned				Total
Variable	*0*	*1*	*2*	*3+*	
Household Income	$34,089	$45,847	$82,886	$101,584	$74,752
Age of Household Head	48.1	45.7	44.2	46.8	45.5
Urban Households (%)	67.2	41.2	28.0	24.3	33.0
Households with Education > High School (%)	48.3	63.4	70.8	68.0	66.7
White Households (%)	35.3	54.7	62.3	61.0	58.2
Households Commuting with Motor Vehicle (%)	47.9	86.2	92.1	93.2	88.0
Commute Dist. (mi)	10.15	12.03	13.65	13.91	13.06
N	*11,535*	*49,623*	*75,367*	*48,091*	*184,616*

NOTE: Sample draws from 1997-2009 American Housing Surveys. All reported numbers are mean values unless stated otherwise. Household income is expressed in real 2001 dollars. Households labeled as 'white' exclude those who identify themselves as Hispanic. Urban definition taken from those in the AHS and U.S. Census Bureau.

Table 4: Real Fuel Prices by Region and Year				
	1997	2003	2009	All Years
Northeast	$0.867	$1.121	$1.919	$1.342
Midwest	$0.817	$0.960	$1.460	$1.205
South	$0.823	$1.055	$1.849	$1.283
West	$0.901	$1.247	$1.973	$1.399
Total	$0.861	$1.153	$1.938	$1.283

Note: Figures are in 2001 dollars.

FIGURE 1

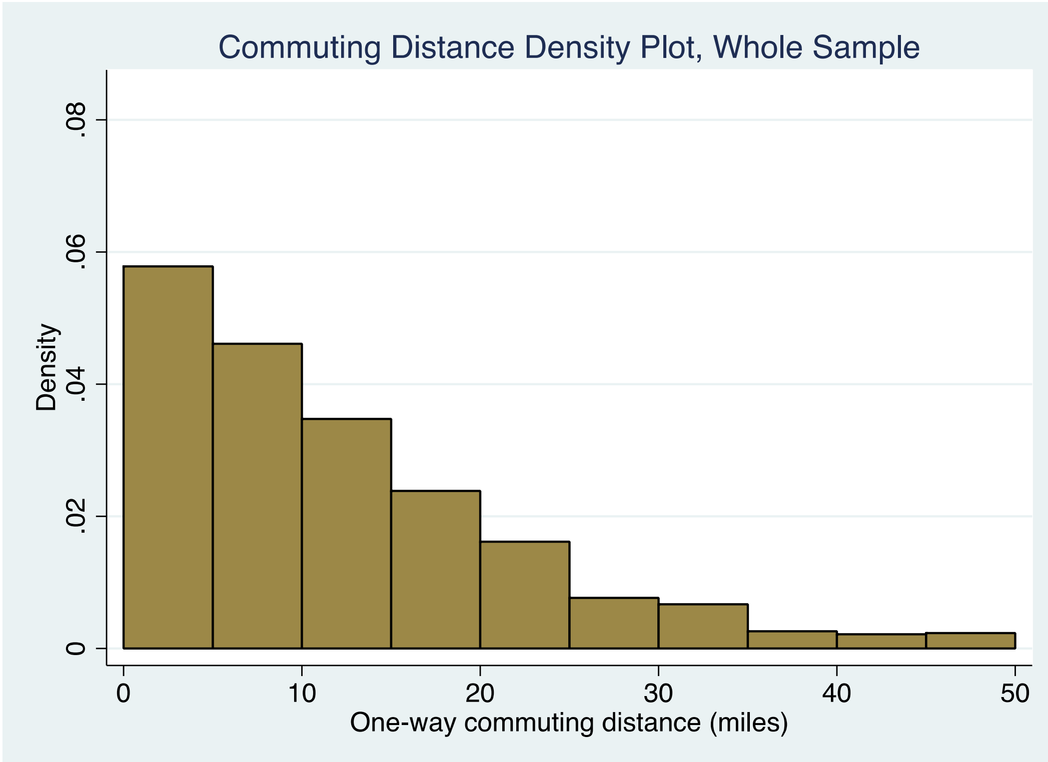
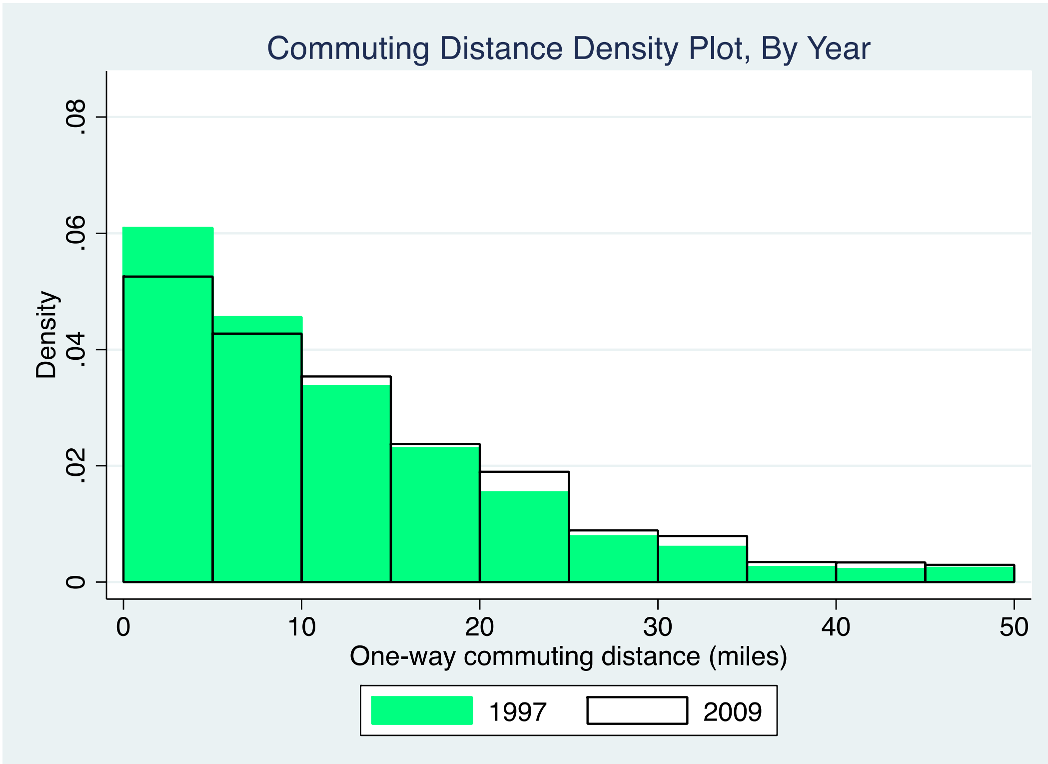

Commuting Distance Density Plot, Whole Sample
Density
0
.02
.04
.06
.08
0
10
20
30
40
50
One-way commuting distance (miles)

FIGURE 2



Commuting Distance Density Plot, By Year
Density
0
.02
.04
.06
.08
0
10
20
30
40
50
One-way commuting distance (miles)
1997
2009

FIGURE 3

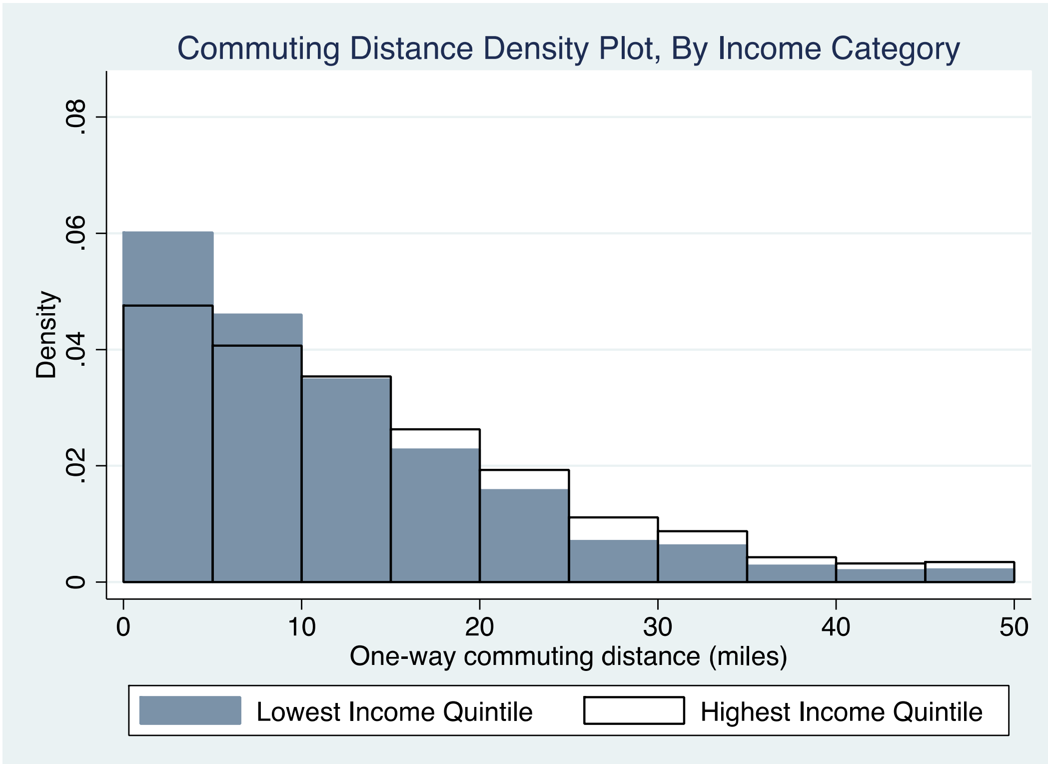

Commuting Distance Density Plot, By Income Category
Density
0
.02
.04
.06
.08
0
10
20
30
40
50
One-way commuting distance (miles)
Lowest Income Quintile
Highest Income Quintile

FIGURE 4

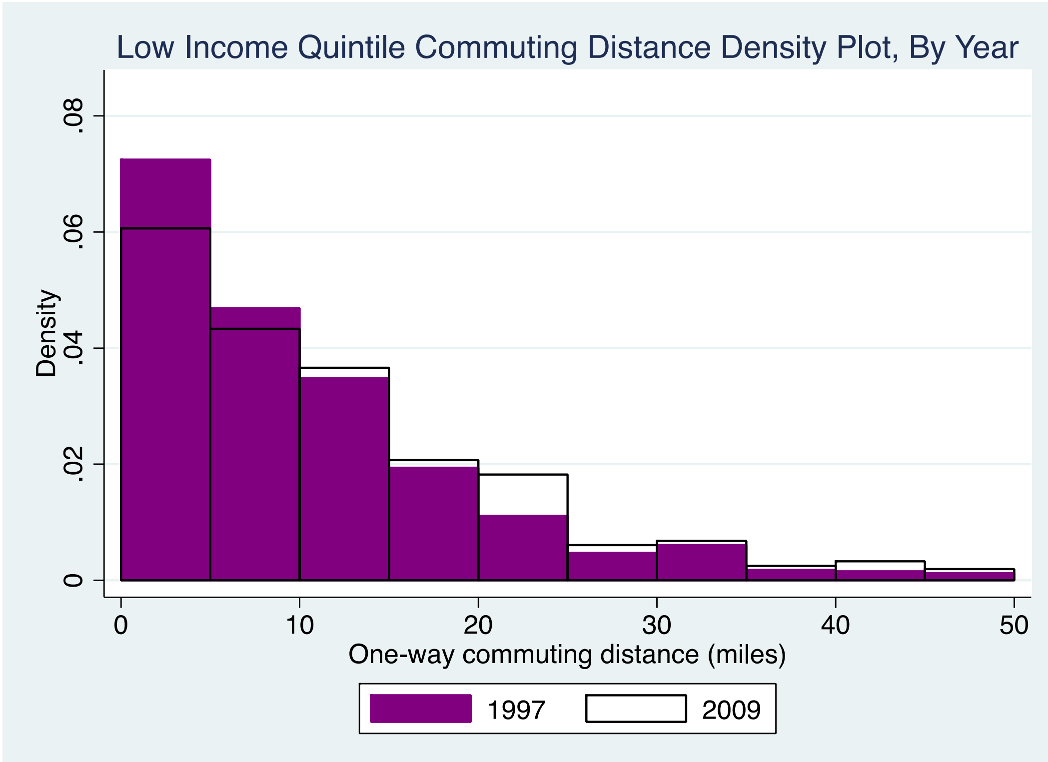

Low Income Quintile Commuting Distance Density Plot, By Year
Density
0
.02
.04
.06
.08
0
10
20
30
40
50
One-way commuting distance (miles)
1997
2009

FIGURE 5

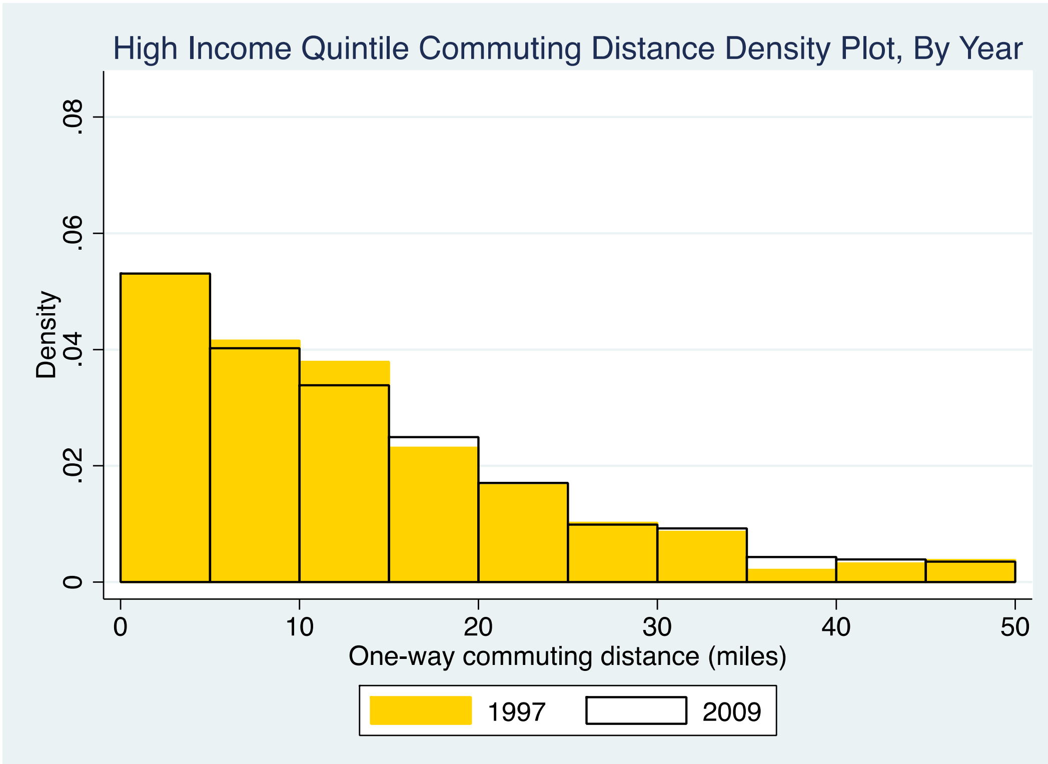
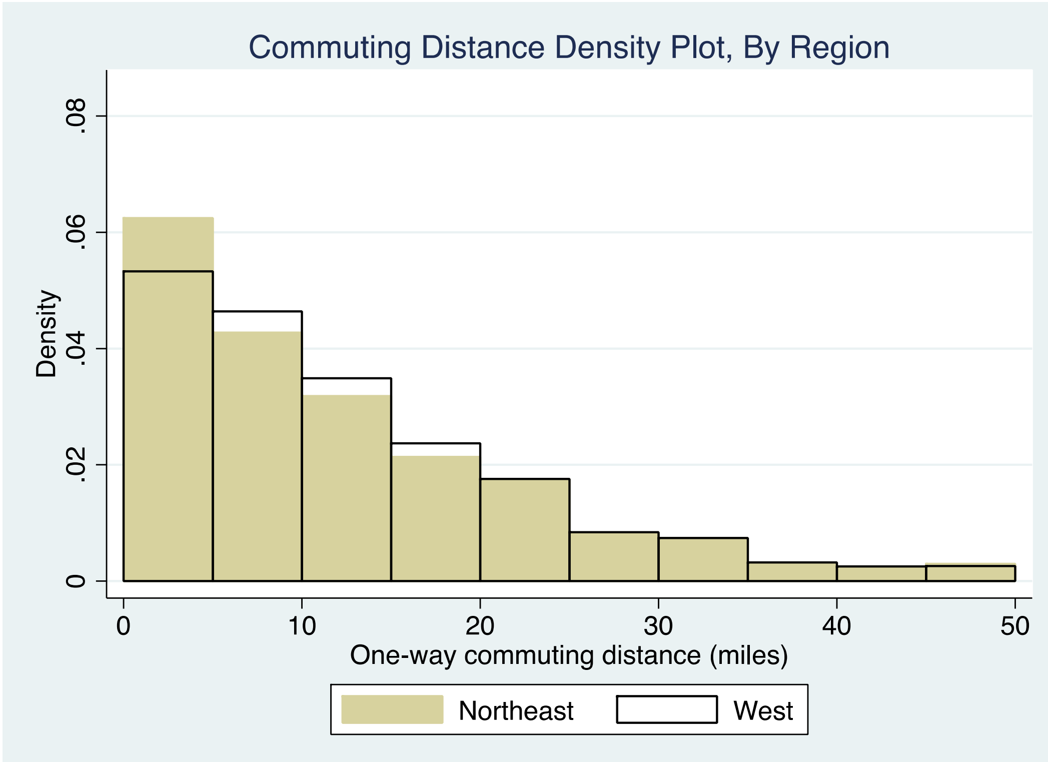

High Income Quintile Commuting Distance Density Plot, By Year
Density
0
.02
.04
.06
.08
0
10
20
30
40
50
One-way commuting distance (miles)
1997
2009

FIGURE 6



Commuting Distance Density Plot, By Region
Density
0
.02
.04
.06
.08
0
10
20
30
40
50
One-way commuting distance (miles)
Northeast
West

FIGURE 7

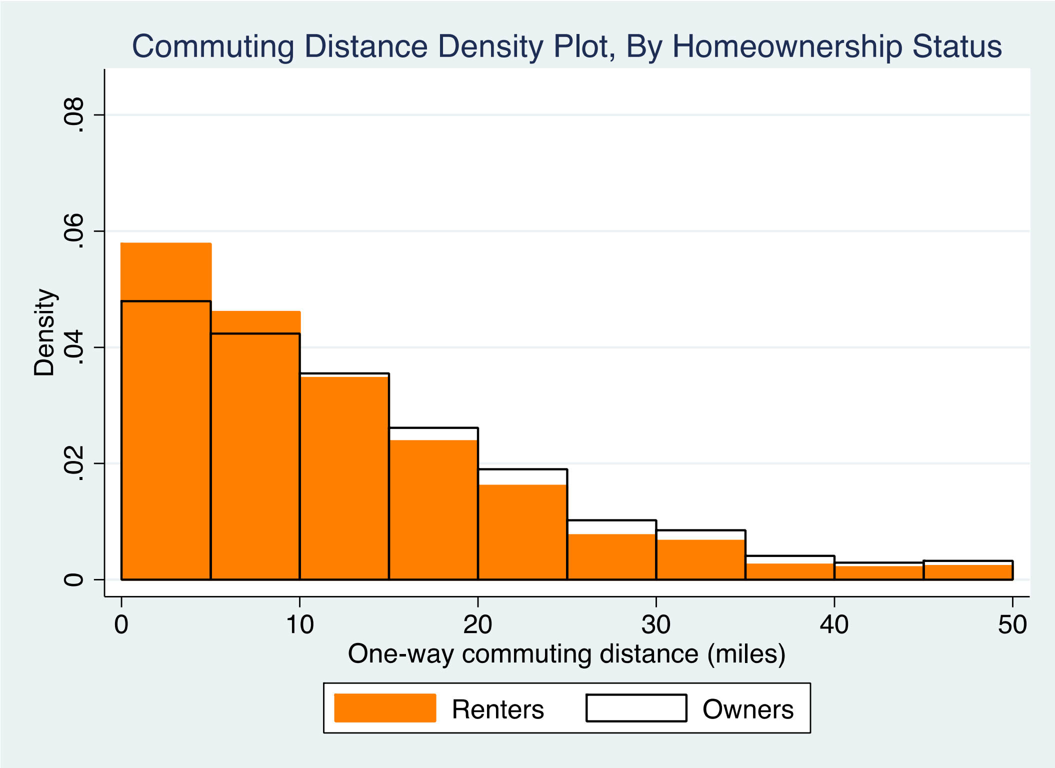

Commuting Distance Density Plot, By Homeownership Status
Density
0
.02
.04
.06
.08
0
10
20
30
40
50
One-way commuting distance (miles)
Renters
Owners

Table 5: Commuting Distance Regressions, Whole Sample

Dependent variable: One-way commuting distance (miles)

	Whole Sample	Car Commuters, Adults >= Cars	Car Commuters, Adults < Cars	Other
D(# of Adults > 1)	0.490	0.430	0.372	0.570
standard error	*0.07*	*0.09*	*0.19*	*0.14*
	***	***		***
D(# of Children>0)	0.373	0.308	0.081	0.052
standard error	*0.06*	*0.07*	*0.16*	*0.13*
	***	***		
Ln(Income)	-0.644	-1.044	0.460	0.329
standard error	*0.16*	*0.20*	*0.36*	*0.26*
	***	***		
Ln($Income^2$)	0.063	0.082	0.007	-0.016
standard error	*0.01*	*0.01*	*0.02*	*0.01*
	***	***		
Ln(Fuel Price Index)	-0.852	-0.636	-1.865	0.364
standard error	*0.22*	*0.26*	*0.61*	*0.45*
	***	*	**	
D(White Head of Household)	0.372	0.412	1.050	-2.033
standard error	*0.07*	*0.08*	*0.18*	*0.14*
	***	***	***	***
Time (years)	0.094	0.136	0.158	-0.022
standard error	*0.02*	*0.03*	*0.07*	*0.05*
	***	***	*	
Constant	10.721	13.556	5.891	2.155
standard error	*0.83*	*1.04*	*1.92*	*1.35*
	***	***	**	
Adjusted R^2	0.0148	0.0138	0.0194	0.088
N	182,225	129,277	30,951	21,997

* P(|t|)<0.05, **P(|t|)<0.01, *** P(|t|)<0.001

Table 6: Stratifications by Income Quintile and Commute Mode - Selected Variables

	Whole Sample					Car Commuters, Cars >= Adults				
	Q1 (Low)	Q2	Q3	Q4	Q5	Q1	Q2	Q3	Q4	Q5
Ln(Fuel Price Index)	0.691	-0.981	-2.400	-1.444	-0.990	0.504	-1.036	-2.116	-0.961	-0.637
standard error	*0.47*	*0.46*	*0.48*	*0.51*	*0.52*	*0.57*	*0.53*	*0.57*	*0.60*	*0.60*
		*	***	**				***		
Time (years)	0.040	0.127	0.217	0.139	0.099	0.134	0.197	0.253	0.147	0.147
standard error	*0.05*	*0.05*	*0.05*	*0.06*	*0.06*	*0.06*	*0.06*	*0.06*	*0.07*	*0.06*
		*	***	*		*	**	***	*	*
Adj. R^2	0.0142	0.0103	0.0139	0.0141	0.0144	0.0121	0.0086	0.0131	0.0151	0.0143
N	33,780	36,989	37,115	37,121	37,220	24,356	27,349	26,907	26,154	24,511
	Car Commuters, Cars < Adults					Other				
	Q1	Q2	Q3	Q4	Q5	Q1	Q2	Q3	Q4	Q5
Ln(Fuel Price Index)	0.951	-1.020	-4.880	-2.686	-1.928	2.497	0.146	1.603	-1.605	-1.473
standard error	*1.83*	*1.55*	*1.23*	*1.24*	*1.22*	*0.76*	*0.97*	*1.08*	*1.23*	*1.23*
			***	*		**				
Time (years)	-0.051	0.102	0.490	0.206	0.230	-0.251	-0.029	-0.232	0.148	0.027
standard error	*0.20*	*0.17*	*0.14*	*0.14*	*0.14*	*0.08*	*0.10*	*0.12*	*0.14*	*0.14*
			***			**		*		
Adj. R^2	0.0208	0.0270	0.0238	0.0179	0.0209	0.0382	0.0578	0.0967	0.1297	0.1736
N	3,474	4,970	6,260	7,452	8,795	5,950	4,670	3,948	3,515	3,914

* P(|t|)<0.05, **P(|t|)<0.01, *** P(|t|)<0.001

Table 7: Stratifications by Income Quintile and Homeowner Status - "Whole Sample," "Car Commuters, Cars>=Adults"

	Whole Sample, Owners					Whole Sample, Renters				
	Q1 (Low)	Q2	Q3	Q4	Q5	Q1 (Low)	Q2	Q3	Q4	Q5
Ln(Fuel Price Index)	-0.995	-1.389	-2.050	-1.202	-0.842	1.549	-0.603	-2.811	-1.815	-2.278
standard error	*0.87*	*0.69*	*0.60*	*0.57*	*0.54*	*0.56*	*0.62*	*0.80*	*1.16*	*1.79*
		*	***	*		**		***		
Time (years)	0.170	0.231	0.188	0.113	0.087	-0.032	0.046	0.268	0.241	0.217
standard error	*0.10*	*0.08*	*0.07*	*0.06*	*0.06*	*0.06*	*0.07*	*0.09*	*0.12*	*0.19*
		**	**					**		
Adjusted R^2	0.0135	0.0151	0.0167	0.0175	0.0152	0.0164	0.0085	0.0138	0.0111	0.0361
N	11,499	18,416	25,143	31,004	34,345	22,281	18,573	11,972	6,117	2,875
	Car Commuters, Cars >= Adults, Owners					Car Commuters, Cars >= Adults, Renters				
	Q1 (Low)	Q2	Q3	Q4	Q5	Q1 (Low)	Q2	Q3	Q4	Q5
Ln(Fuel Price Index)	-0.574	-1.588	-1.511	-0.723	-0.490	1.094	-0.571	-3.042	-1.063	-1.331
standard error	*0.99*	*0.79*	*0.70*	*0.68*	*0.63*	*0.69*	*0.71*	*0.94*	*1.35*	*2.11*
		*	*					**		
Time (years)	0.230	0.351	0.195	0.132	0.126	0.077	0.069	0.346	0.163	0.233
standard error	*0.11*	*0.09*	*0.08*	*0.07*	*0.07*	*0.07*	*0.08*	*0.11*	*0.14*	*0.23*
	*	***	*					**		
Adjusted R^2	0.0152	0.0142	0.0174	0.0195	0.0148	0.0137	0.0068	0.0126	0.0114	0.0316
N	8,410	13,498	17,997	21,613	22,541	15,946	13,851	8,910	4,541	1,970

* P(|t|)<0.05, **P(|t|)<0.01, *** P(|t|)<0.001

Table 8: Stratifications by Income Quintile and Homeowner Status - "Car Commuters, Cars<Adults," "Other"

	Car Commuters, Cars < Adults, Owners					Car Commuters, Cars < Adults, Renters				
	Q1 (Low)	Q2	Q3	Q4	Q5	Q1 (Low)	Q2	Q3	Q4	Q5
Ln(Fuel Price Index)	-3.429	-1.854	-4.928	-2.260	-2.060	6.247	0.506	-3.245	-9.558	0.175
standard error	*2.56*	*1.93*	*1.37*	*1.28*	*1.25*	*2.54*	*2.66*	*2.75*	*5.52*	*5.83*
			***			*				
Time (years)	0.251	0.190	0.507	0.146	0.254	-0.463	-0.016	0.238	0.939	-0.222
standard error	*0.28*	*0.20*	*0.15*	*0.15*	*0.14*	*0.27*	*0.32*	*0.31*	*0.66*	*0.69*
			***							
Adjusted R^2	0.0280	0.0332	0.0265	0.0202	0.0213	0.0416	0.0268	0.0501	0.0609	0.0863
N	1,919	3,359	5,042	6,841	8,453	1,555	1,611	1,218	611	342
	Other, Owners					Other, Renters				
	Q1 (Low)	Q2	Q3	Q4	Q5	Q1 (Low)	Q2	Q3	Q4	Q5
Ln(Fuel Price Index)	1.311	1.215	3.027	-1.535	0.321	2.780	-0.606	0.064	-1.591	-12.488
standard error	*1.90*	*1.96*	*1.65*	*1.52*	*1.34*	*0.84*	*1.08*	*1.28*	*1.96*	*3.00*
						***				***
Time (years)	-0.210	-0.298	-0.477	0.086	-0.153	-0.260	0.100	0.001	0.345	1.223
standard error	*0.23*	*0.21*	*0.18*	*0.18*	*0.15*	*0.09*	*0.11*	*0.10*	*0.21*	*0.30*
			**			**				***
Adjusted R^2	0.0903	0.1059	0.1322	0.1718	0.2151	0.0358	0.0455	0.084	0.1044	0.1172
N	1,170	1,559	2,104	2,550	3,351	4,780	3,111	1,844	965	563

* P(|t|)<0.05, **P(|t|)<0.01, *** P(|t|)<0.001

Table 9: Stratifications by Income Quintile and Migration Status - "Whole Sample," "Car Commuters, Cars>=Adults"

	Whole Sample, Home Tenure >= 2 years					Whole Sample, Home Tenure < 2 years				
	Q1 (Low)	Q2	Q3	Q4	Q5	Q1 (Low)	Q2	Q3	Q4	Q5
Ln(Fuel Price Index)	-0.131	-0.654	-2.399	-1.058	-1.334	1.385	-1.323	-2.542	-2.530	-0.635
standard error	*0.69*	*0.66*	*0.62*	*0.62*	*0.64*	*0.66*	*0.66*	*0.78*	*0.94*	*0.93*
			***		*	*	*	**	**	
Time (years)	0.134	0.066	0.237	0.079	0.112	-0.052	0.181	0.183	0.279	0.090
standard error	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.07*	*0.08*	*0.09*	*0.11*	*0.12*
	*		***			*	*	*		
Adjusted R^2	0.0241	0.0167	0.0158	0.0174	0.0151	0.0108	0.0087	0.0145	0.0144	0.0166
N	11,814	14,789	16,978	19,038	20,617	21,966	22,200	20,137	18,083	16,603
	Car Commuters, Cars >= Adults, Home Tenure >= 2 years					Car Commuters, Cars >= Adults, Home Tenure < 2 years				
	Q1 (Low)	Q2	Q3	Q4	Q5	Q1 (Low)	Q2	Q3	Q4	Q5
Ln(Fuel Price Index)	-0.036	-0.292	-2.192	-0.077	-1.209	0.977	-1.697	-2.279	-2.554	0.169
standard error	*0.90*	*0.77*	*0.74*	*0.77*	*0.77*	*0.78*	*0.75*	*0.89*	*1.09*	*1.06*
			**				*	*	*	
Time (years)	0.224	0.123	0.264	0.071	0.171	0.057	0.258	0.248	0.279	0.119
standard error	*0.08*	*0.08*	*0.08*	*0.08*	*0.08*	*0.09*	*0.09*	*0.11*	*0.13*	*0.13*
	**		**		*		**	*	*	
Adjusted R^2	0.0244	0.0152	0.0157	0.0188	0.016	0.0092	0.0079	0.0139	0.0165	0.0183
N	7,872	10,368	11,909	13,103	13,398	16,484	16,981	14,998	13,051	11,113

* P(|t|)<0.05, **P(|t|)<0.01, *** P(|t|)<0.001

Table 10: Stratifications by Income Quintile and Migration Status - "Car Commuters, Cars<Adults," "Other"										
	Car Commuters, Cars < Adults, Home Tenure >= 2 years					Car Commuters, Cars < Adults, Home Tenure < 2 years				
	Q1 (Low)	Q2	Q3	Q4	Q5	Q1 (Low)	Q2	Q3	Q4	Q5
Ln(Fuel Price Index)	-1.780	0.233	-3.674	-1.869	-0.941	3.534	-2.751	-6.414	-5.325	-5.517
standard error	*2.44*	*2.12*	*1.54*	*1.46*	*1.38*	*2.70*	*2.50*	*2.23*	*2.50*	*2.87*
			*					**	*	
Time (years)	0.018	-0.127	0.465	0.005	0.124	-0.146	0.391	0.456	0.744	0.581
standard error	*0.26*	*0.21*	*0.16*	*0.17*	*0.16*	*0.29*	*0.29*	*0.28*	*0.31*	*0.37*
			**						*	
Adjusted R^2	0.0366	0.043	0.0356	0.0304	0.0241	0.0245	0.0277	0.0264	0.02	0.025
N	1,385	2,179	2,983	3,947	4,893	2,089	2,791	3,277	3,505	3,902
	Other, Home Tenure >= 2 years					Other, Home Tenure < 2 years				
	Q1 (Low)	Q2	Q3	Q4	Q5	Q1 (Low)	Q2	Q3	Q4	Q5
Ln(Fuel Price Index)	1.314	-1.491	0.301	-3.167	-1.523	3.921	2.358	3.946	2.040	-1.667
standard error	*1.12*	*1.41*	*1.43*	*1.47*	*1.55*	*1.02*	*1.32*	*1.74*	*2.29*	*2.19*
				*		***		*		
Time (years)	-0.078	0.102	-0.081	0.258	0.012	-0.464	-0.220	-0.521	-0.148	0.073
standard error	*0.12*	*0.14*	*0.15*	*0.17*	*0.17*	*0.11*	*0.14*	*0.20*	*0.26*	*0.29*
						***		**		
Adjusted R^2	0.0458	0.0652	0.0888	0.1486	0.1789	0.0375	0.0548	0.122	0.118	0.1579
N	2,557	2,242	2,086	1,988	2,326	3,393	2,428	1,862	1,527	1,588
* P(|t|)<0.05, **P(|t|)<0.01, *** P(|t|)<0.001										

Table 11: Placebo Regressions (Future Fuel Prices)

	Whole Sample					Car Commuters, Cars >= Adults				
	Q1 (Low)	Q2	Q3	Q4	Q5	Q1	Q2	Q3	Q4	Q5
Ln(Fuel Price Index)	-2.158	0.060	0.446	0.078	0.870	-2.534	0.475	0.020	0.004	0.336
standard error	*1.71*	*0.74*	*0.77*	*0.89*	*0.83*	*1.97*	*2.76*	*0.95*	*1.10*	*1.01*
Time (years)	0.354	0.090	0.007	0.057	-0.003	0.415	0.102	0.054	0.115	0.073
standard error	*0.07*	*0.08*	*0.08*	*0.09*	*0.08*	*0.10*	*0.27*	*0.10*	*0.12*	*0.10*
	***					***				
Adj. R^2	0.023	0.013	0.012	0.012	0.010	0.018	0.034	0.011	0.013	0.011
N	15,493	16,296	16,401	16,320	16,450	10,186	1,854	11,637	11,487	10,777
	Car Commuters, Cars < Adults					Other				
	Q1	Q2	Q3	Q4	Q5	Q1	Q2	Q3	Q4	Q5
Ln(Fuel Price Index)	-4.102	0.475	0.659	-1.694	0.056	-1.209	-0.675	1.867	1.762	2.576
standard error	*2.93*	*2.76*	*2.32*	*2.14*	*1.97*	*0.97*	*1.32*	*1.37*	*1.78*	*1.85*
Time (years)	0.471	0.102	0.073	0.162	0.085	0.180	0.092	-0.084	-0.270	-0.222
standard error	*0.30*	*0.27*	*0.23*	*0.22*	*0.19*	*0.10*	*0.14*	*0.14*	*0.19*	*0.18*
Adj. R^2	0.029	0.034	0.020	0.021	0.016	0.033	0.053	0.075	0.114	0.155
N	1,356	1,854	2,359	2,868	3,423	3,951	2,861	2,405	1,965	2,250

* P(|t|)<0.05, **P(|t|)<0.01, *** P(|t|)<0.001

Table 12: Commuting Distance Regressions, Whole Sample, Alternative Fuel Index

	Whole Sample	Car Commuters, Adults > Cars	Car Commuters, Adults <= Cars	Other
D(# of Adults > 1)	0.495	0.434	0.385	0.569
standard error	*0.07*	*0.09*	*0.19*	*0.14*
	***	***	*	***
D(# of Children>0)	0.374	0.307	0.084	0.053
standard error	*0.06*	*0.07*	*0.16*	*0.13*
	***	***		
Ln(Income)	-0.642	-1.042	0.465	0.327
standard error	*0.16*	*0.20*	*0.36*	*0.26*
	***	***		
Ln($Income^2$)	0.063	0.082	0.007	-0.016
standard error	*0.01*	*0.01*	*0.02*	*0.01*
	***	***		
Ln(Fuel Price Index)	-1.032	-0.876	-1.561	0.636
standard error	*0.25*	*0.30*	*0.72*	*0.52*
	***	**	*	
D(White Head of Household)	0.367	0.406	1.067	-2.028
standard error	*0.07*	*0.08*	*0.18*	*0.14*
	***	***	***	***
Time (years)	0.115	0.163	0.133	-0.051
standard error	*0.03*	*0.03*	*0.08*	*0.06*
	***	***		
Constant	10.657	13.453	6.102	2.280
standard error	*0.83*	*1.04*	*1.92*	*1.35*
	***	***	**	
Adjusted R^2	0.0148	0.0138	0.0193	0.0881
N	182,225	129,277	30,951	21,997

* P(|t|)<0.05, **P(|t|)<0.01, *** P(|t|)<0.001

Table 13: Sensitivity Analysis, Alternative Income Sorting

ln(Fuel Price Index) Coefficients, Whole Sample with No Commute Mode Stratification

Householder Age	Quintile 1 (Low)	Quintile 2	Quintile 3	Quintile 4	Quintile 5
Under 35	-0.641	-1.293	-1.070	-2.510*	-0.364
35-45	1.235	-2.438*	-2.289	-2.132	0.282
45-55	0.132	-1.425	-2.629*	-2.445	-2.318
55-65	0.370	-0.127	3.632*	-2.055	-3.360*
Over 65	3.612***	-0.310	1.357	-1.917*	-0.550

* P(|t|)<0.05, **P(|t|)<0.01, *** P(|t|)<0.001

AGGLOMERATION, TAX DIFFERENTIALS, AND THE MOBILITY OF PROFESSIONAL ATHLETES

Interstate mobility can limit states' ability to choose their desired tax policies. The forces of agglomeration, however, may allow states more leeway in setting tax rates. Moreover, mobility and agglomeration effects are not uniform for all individuals within a state, and can vary significantly across different groups. This research explores such heterogeneity by examining the residential location decisions of professional racecar drivers and golfers, which have similar industry characteristics but different levels of agglomeration. The findings show that, consistent with the theory presented, tax preferences are a powerful determinant of golfer residential patterns, while agglomeration mitigates much of this effect among racecar drivers. These findings highlight the need to better understand how competition and agglomeration interact when formulating tax policy.

I. INTRODUCTION

States and localities typically feel constrained in setting their tax policies by the real possibility of interstate mobility by individuals and businesses. In policy debates, commentators and analysts typically compare top marginal tax rates between different jurisdictions to assess whether raising a tax rate will make a state "uncompetitive." More sophisticated analyses will also include the benefits provided to residents through taxation as a mitigating factor. Nonetheless, for very high earners, whose tax payments will typically exceed a normal provision of government benefits, the emphasis is placed on comparing the tax differentials.

But what if the ability to earn high incomes depends on the presence of other similarly skilled individuals within the state or access to specialized industry know-how? In this case, we have an example of an agglomeration economy: the positive spillover benefits an economic actor receives from being in close proximity to other similarly situated actors. With positive spillovers, an individual can no longer simply compare top marginal tax rates across areas to determine the locale with the highest after-tax income; it is important to take into account that pre-tax incomes will typically be higher in the agglomeration setting. Agglomeration helps to explain the persistence of the attraction to Silicon Valley in the tech industry, despite high state and local tax rates. Efforts to recreate similar industry dynamics (and subsequent increases in job growth and tax revenues) have included Silicon Snowbank (in the Minneapolis-St. Paul region), Silicon Bog (in Limerick, Ireland) and Media Valley (in Inchon, South Korea), to varying degrees of success (Hospers, Desrochers and Sautet 2009).

This paper explores how the effects of agglomeration and tax differentials vary across markets through the examination of the determinants of interstate mobility among golfers and racecar drivers. Professional golfing and racecar driving possess a similar structure and set of incentives for participants. Both sports use a tour system, with weekly events spread across the country in a manner that requires near-constant travel during the season. Athletes in each sport earn income through a combination of event performance, endorsement income, and other sports-related contracts. Furthermore, both classes of athletes are subject to similar state income tax regimes: state source taxation on earnings from specific performances and residual taxation on their income in their home state. However, while golf is a relatively individual enterprise, requiring only a minimal level of coaching and access to practice facilities that can be found almost anywhere in the country, racecar driving is done with a team and has mechanical requirements that increase the value of agglomeration.

The empirical evidence demonstrates significant variation in the mobility decisions of athletes across industries. Athletes in both sports are attracted to areas with low tax rates and warm climates. Among golfers, these preferences lead to significant migration toward tax-advantaged states in the South and East. Meanwhile, a powerful agglomeration factor – borne out of both increased advantages to firm concentration, an initial advantage in resources, and fortuitous circumstances – contributes to large racecar driver migration to North Carolina. This residential pattern endures despite the imposition of tax rates that are higher than the nationwide average and the presence of nearby states with more attractive tax policies.

As the findings suggest, the conflicting forces of agglomeration and tax competition need not produce a universal, all-or-nothing outcome. In practice, the mobility rates and responses to changes in taxation can vary widely across industries or professions due to differences in agglomeration levels. This idea helps to explain why, for instance, retirees may often leave New York for Florida in greater numbers than investment bankers working on Wall Street, who benefit from the knowledge rich environment available in their industry. The results demonstrate the importance of information about the effects of agglomeration as well as tax rates when assessing the pressures associated with potential interstate mobility.

II. BACKGROUND

Agglomeration

An agglomeration economy is defined as a set of industries where the participating firms benefit from locating in close proximity to one another. Duranton and Puga (2003) identify three potential sources for agglomeration economies. The first channel is the sharing mechanism, which characterizes gains related to the combined use of facilities, input resources, or the pooling of risk among firms. The second channel, the matching mechanism, describes improvements in human or capital resources that firms acquire because of the proximity to other firms within the industry (for instance, the increased access to talent that a financial firm receives by locating on Wall Street). The third channel is a learning mechanism that highlights any gains made in knowledge diffusion or accumulation caused by agglomeration. If the agglomerative returns are sufficiently large, it is possible for a rationally behaving firm to choose to locate in a high-tax region with other firms in the same industry.

Agglomeration, therefore, may counteract some of potential adverse effects of tax differentials. Baldwin and Krugman (2004) study the market and tax outcomes in the European Union and find that the introduction of agglomeration reverses the outcomes of a standard tax competition model. In their analysis, the presence of multiple firms in an agglomeration economy creates an 'agglomeration rent' that can be exploited by governments through higher tax rates, as the advantages to a firm of locating in an area with an industry presence may increase the firm's tolerance for taxation. Brulhart, Jametti and Schmidheiny (2007) examine the interaction between corporate tax differentials and agglomeration rents in Switzerland, and find that agglomerative forces neutralize the tax advantages across local municipalities. These findings mirror the analysis of Borck and Pfluger (2006), who find that a mobile factor that completely agglomerates in one region of a tax competition model may incur a locally taxable agglomeration rent.

A number of factors suggest that the forces of agglomeration have played a significant role in both initially drawing the racecar industry to North Carolina and keeping it there over time, despite the presence of a state income tax levied at the relatively high rate of 7.75 percent for those in the top income bracket (National Bureau of Economic Research 2013). [12] Historically, the agglomerative draw of racing to North Carolina was due to a combination of factors. In the early part of the 20th century, much of the advanced technology in automobile development was located in the U.S. southeast, in part due to the presence of large bootlegging firms operating in response to the federal prohibition of alcohol. When a ban on stock car racing was rescinded at the end of World War II, North Carolina emerged as one of the few southern states that permitted the inclusion of former bootleggers into racing events, which was a critical component in the initial attraction of racecar driving to the state (Mitchelson and Alderman 2011).

Later it was discovered that piedmont, a clay produced in abundance in North Carolina, proved to be an ideal surface for racing, resulting in the construction of a number of facilities in the state that were suitable for a professional tour. Consequently, when the National Association for Stock Car Auto Racing (NASCAR) series was initially founded, 30 of the 52 sites used for events were located in the state (McKim 2010). The

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[12] The reported tax rate was effective through the 2013 calendar year. Subsequent changes in the law eliminated the use of multiple tax brackets and reduced the North Carolina state income tax rate to 5.75 percent in 2014.

reliance on North Carolina racing tracks led a number of major racecar teams to locate their building facilities in the state.

Today over ninety percent of racing teams operating in NASCAR operate in North Carolina, despite the fact that event presence in the state has dropped significantly since the tour was initially founded. Table 14 shows the distribution of racing and golfing events across states. North Carolina only held four of the 69 NASCAR races held in 2013, which was fewer than totals in the nearby states of Florida and Virginia. Despite this reduction, there is evidence that agglomeration forces in North Carolina have become both stronger and more localized in recent years. Mitchelson and Alderman (2011) reported that over 60 percent of all racing employment was located in a 400 square mile area of land in central North Carolina. The persistence of the racing industry in North Carolina illustrates the power of hysteresis and path-dependency for agglomerative industries.

The incentives for agglomeration for racing team owners draw from all three of the channels discussed above. Incentives for sharing include the common use of training racing track facilities and of equipment used in the construction of the vehicles. A geographically concentrated racing industry also allows for matching, as it increases the chance that racing organizations and laborers will find one another. Finally, co-location offers increased potential for learning: as organizations develop new technologies and designs that improve racing outcomes, the increased rates of information exchange and communication that are a consequence of a concentrated expedite the spread of innovation.

The concentration of teams in North Carolina is a major motivating factor in the residential decisions of drivers. Tour events are a near constant presence on weekends from February through November, and drivers typically spend Tuesdays and Wednesdays consulting with team members in making changes to their vehicles to optimize them for the upcoming event. There is no absolute requirement to live in the same state as your team—a majority of NASCAR teams that hire more than one driver employ racers that live in more than one state. Thus, a driver is not physically tied to a team, as a foreman might be to a factory. Nonetheless, the appeal of being able to make pre-race adjustments in person is a powerful lure for taking residence in North Carolina.[13]

The agglomerative pull for golfers is not nearly as strong as it is for their racing counterparts. Unlike racing tracks, golfing is ubiquitous in the United States, with more than 29 million registered golfers across the country in 2013. Professional golf is also far more of an individual enterprise than racing. Whereas a racecar driver usually has a crew and may also be part of a team, golfing is far more of an individual endeavor. Professionals typically only employ a caddy, and on some occasions, a swing coach (Svrluga 2012). Each of these may only be needed on location during an event. Moreover, while dry, warm climates are viewed as most suitable for golfing activities, attractive golfing destinations may be found in a variety of climates. Locations in Indiana, Michigan, New York, Ohio, Oregon, and Washington each appeared in the top twelve of Golf Magazine's list of most attractive golf cities (Isom 2014). Table 14 shows that, as with racecar driving, golfing events are held in a variety of locations, and thus offers little benefit to locating in a particular region. However, since two of the most

[13] Only two of the top twenty drivers ranked by earnings live outside of North Carolina.

prominent states on the tour do not impose an income tax on their residents (Florida and Texas), golfers may have more of an incentive to locate in these areas.

Taxation

State governments typically draw the majority of their tax revenue through levies on individual income and consumption. There is significant variation in the individual income tax rates imposed by U.S. states. Eight states did not charge any tax on comprehensive personal income in 2013 (Alaska, Florida, Nevada, New Hampshire, South Dakota, Tennessee, Texas and Washington[14]). At the other end of the spectrum, the tax rate imposed on the highest earners in California was 13.3 percent, inclusive of the "millionaire surcharge" (National Bureau of Economic Research 2013).

The income of athletes can be broadly sorted into earnings directly attributable to the tour and income generated from outside sources. Income earned by an athlete at an event is taxed in the state where the event is held (this is known as source taxation). Most states assess tax rates on that income measured by the athlete's worldwide income, which often results in these earnings being taxed at the maximum income rate. However, the state where the athlete resides will usually credit any taxes paid in other states when assessing their own taxes up to the tax rate of the resident state.[15] With this framework, the tax rate imposed by the athlete's home state is equivalent to the minimum tax rate that the athlete will pay on any tour income.

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[14] Although Tennessee and New Hampshire do not impose taxes on income, they do tax income generated from interest and dividends.

[15] A few states, such as Louisiana, credited <u>all</u> taxes paid to other states. However, Louisiana changed its law to conform to practices in other states in 2015.

For example, consider two athletes: Athlete A, who lives in a state with no income tax, and Athlete B, who lives in a state with a flat income tax of seven percent. Suppose that each athlete earns tour income taxed at source at a rate of three percent. Since Athlete A pays no income tax in his home state, his equivalent income tax rate is the three percent charged to him from each "event host" state. However, the seven percent home state tax rate for Athlete B means that he will pay the three percent tax rate to the event host state, as well as a four percent tax to his home state. Note that if the "host state" tax rate charged to the athletes was greater than seven percent, then the taxes paid by each athlete on the host state earnings would be equivalent. Therefore, the tax rate of an athlete's home state serves as the minimum tax rate that will be paid on all tour income. The lower this tax rate is, the more there is to be gained from earnings from events in low- or no-tax states.[16]

Whereas the tour earnings generated by golfers are derived from their event performances, racecar drivers are typically paid an annual salary by the racing team in addition to receiving a percentage of the proceeds from their event winnings.[17] Earnings from salary are viewed from a tax perspective as being earned in the driver's home state. All else equal, this indicates that the tax policies of a driver's home state would have a larger effect on net earnings than they would for a golfer with an equivalent gross income.

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[16] Some pairs of U.S. states have entered into reciprocity agreements, which allow residents of one state to request exemptions from tax withholding on income earned in another state and has the home state tax this income. These reciprocity agreements, which exist mostly among Midwestern and Mid-Atlantic states, have been factored into subsequent tax calculations in the regression analysis.

[17] The balance between the percentage of winnings and salary earned varies by team, and in some cases, by athlete.

Not all tour income is directly attributable to specific activities outside an athlete's home state. The tax rate of an athlete's home state determines the taxes paid on any income that is not directly attributable to activity in other states. This category captures a wide range of income including earnings from personal commercial endorsements, income from other business activities (such as course construction fees for golfers), and appearance fees for personal events. This outside income for drivers and golfers can be quite substantial, particularly among the most skilled athletes. [18]

Table 15 presents the income generated from golf event earnings and from other activities for the top earners on the Professional Golfers' Association (PGA) Tour in 2013 (Sirak 2014). Earnings from outside income dwarf income generated from the tour for the most lucrative athletes. Tiger Woods and Phil Mickelson together earned $116 million in outside earnings in 2013, as opposed to only $19 million from tour performances. Since this income is taxed by the individual's state of residence, the after-tax incomes of these athletes are particularly sensitive to state income tax policies. Note that endorsement earnings mostly flow to a small number of athletes; aside from Woods and Mickelson, the average income generated outside the tour for the other elite golfers in Table 15 was "only" around $4.4 million.

Less information is available on the outside income of racecar drivers, with details on only a small number of top earners reported to the public. Endorsement earnings by driver rank appear to follow a similar skewed pattern as they do in golf, with top drivers taking the majority of the available opportunities, while less-established

[18] At the federal level, the Internal Revenue Service has at times has sourced some fraction of endorsement income to the United States when the endorsement. Contracts require substantial participation in events in the United States (Ambord 2013). To our knowledge, no states have taken a similar approach.

athletes generate significantly less endorsement activity (Pockrass 2012). Dale Earnhardt Jr., the top earner in NASCAR, received $11 million in 2013 endorsement income, while the remainder of the drivers in the top ten of total earnings averaged “only” $3.6 million in income generated outside of NASCAR events.

With the existing source-resident tax system, the incentives for an individual to live in a tax-advantaged state are dependent upon his or her income level, the structure and composition of earnings, and the pattern of tax rates across states. Table 16 presents the authors’ estimates for the total state income tax liability of golfers and racecar drivers across different skill levels. Taxes paid on tour earnings were determined by calculating an effective tax rate on event earnings outside the state (assumed to be a function of the number of tournaments, with equal earnings in each event), and then allowing a credit for these payments in the resident state up to the liability due from the resident state tax. Salary and endorsement tax payments were simply a function of the resident income top tax rate. We calculate a net tax burden, after allowing for federal deductibility at a 40 percent rate. Note that because elite performers in both sports tend to earn dramatically more in endorsements (taxed exclusively by the home state) than their less-skilled counterparts, the incentive for these athletes to move to states with no income tax exceeds that of other athletes both as a proportion of total earnings and in nominal terms.

An elite golfer with $5 million in total earnings would save about $335,000 by moving from California to Florida, while a below-average golfer earning $750,000 would save only about $39,000 from such a move. Since racecar drivers tend to live either in North Carolina or in tax-advantaged states, only North Carolina and Florida were used as examples for racecar drivers. Moving from North Carolina to Florida would save an elite driver (with $5 million in earnings) about $207,000, while a below-average driver ($750,000 in earnings) would save about $28,000. While these calculations are just meant to be illustrative, they demonstrate an important point. Tax incentives for elite athletes, with income derived primarily from endorsements, differs from other professional athletes who have a larger fraction of their income taxed in source states.

A number of studies find evidence of a significant relationship between taxes and migratory patterns. Kleven, Landais and Saez (2010) find that top income tax rates had strong mobility effects on elite European footballers and report indications of sorting effects of high-performance players in low-tax countries. Moretti and Wilson (2013) explore the effect of tax policy on the migration of star scientists and find that state tax rates and credits induce movement among that population, with credits having a stronger 'pull effect' (the policy in the destination state) and tax rates having a strong 'push effect' (the policy in the state of origin). Alm, Kaempfer and Sennoga (2011) study how income taxes affect free-agent movement among baseball players, and find that income tax increases force teams to pay players higher salaries as compensation for the reduction in net wages.

Other studies have found a weaker link between migration and tax differentials. Dahl and Sorenson (2009) apply a conditional logit model to individual-level data to test for the pull of tax preferences among entrepreneurs in Europe and find that social factors had a much stronger appeal than tax differentials. Young and Varner (2011) employ a similar methodology to aggregate-level data examining the migratory response to the imposition of a 'millionaire surcharge' in New Jersey, and find that the policy induced little migration responsiveness among top earners. Coomes and Hoyt (2008) examine how tax policy influenced residential choices in multistate metropolitan areas, and find that income tax rates play a significant role only when the filing state based taxes on employment and not residence. Chen and Rosenthal (2008) conduct an empirical analysis of interstate mobility across the life cycle, and find that regardless of marital status, young educated individuals migrate toward high-quality business environments, while married couples migrate towards high-quality consumer amenity areas, such as low taxes, irrespective of income. None of this work, however, probes the interactions between taxes and agglomeration.

III. EMPIRICAL FRAMEWORK

In order to understand how the variables of interest affect mobility outcomes, the empirical approach examines the residential choices of golfers and racers. The analysis adopts two different specifications that draw from other research that has focused on agglomeration and on tax differentials. The first model uses a probit framework to examine the probability of moving to a state with a lower income tax rate than the birth rate. The second model uses a conditional logit framework, which allows us to isolate the effects of tax variables and agglomeration.

Both of these specifications illuminate the role that tax and agglomeration incentives play in athlete mobility. Each specification employs variables that capture tax policy, agglomeration measures or sport-specific differences, weather, distance, and individual preferences. This analysis seeks to more precisely understand the balance of factors that determine residential outcomes in each industry. The pull of agglomeration for racecar drivers is strong, but how does that relate to the draw of agglomeration in golfing? Similarly, what sort of differential exists in the attractiveness of tax policies and weather across sports? Identifying the nature of the difference between these forces may offer insight into how easily residential outcomes can be influenced by changes in public policy.

The first specification is represented in equation (1) below. These regressions use a probit specification and tests for the determinants of “tax improving migration,” which we define as an athlete residing in a state with a lower income tax rate ($Inc\ Tax_R$) than the state of his or her birth ($Inc\ Tax_B$). Mobility to a state with more advantageous tax policies represents the precise type of migration of interest to the tax differential literature, since states that adjust their tax policies to increase their “competitiveness” do so in part to attract the sort of migration represented in this specification. This outcome measure implicitly values the characteristics of an athlete’s birth state, as ensuing empirical work shows that these factors may affect migration patterns.[19]

$$(1)\ \Pr(\text{Inc Tax}_R < \text{Inc Tax}_B) = F[\text{Age}, \ln(\text{Inc}), \ln(\text{Low Inc Tax Dist}_B), \text{Temp}_B]$$

The independent variable of interest is the natural log of athlete’s total income, including earnings from outside activities [*ln(Inc)*], which tests for the relative effect of individual earnings on mobility outcomes. As demonstrated in Table 16, the value of tax-advantaged residences increases, in some cases non-linearly, with athlete income. Therefore, we would expect athletes with greater earnings to have increased residential selection rates of states with low taxes. However, if there was an agglomeration presence in a particular industry crowding out the advantages offered by states with no income tax,

[19] In addition to tests for tax-improving migration, sensitivity analysis was conducted where the dependent variable was the likelihood of living in a state with no income tax (thus removing the effect of birth characteristics). Those regressions produced similar results to the analysis presented in this research.

such a factor may also eliminate the relationship between income and the residential tax status. A log measure of this variable is adopted to ensure that the results are not biased by observations towards the tails of the earnings spectrum.

The analysis also includes measures reflecting position in the life-cycle (*Age*), location [$ln(Low\ Inc\ Tax\ Dist_B)$], and climate ($Temp_B$). Specifically, an age variable captures the effect of the athlete's position in the labor cycle on migration choices. The log of the distance between the birth state and the nearest state that does not impose an income tax is included to account for the effect of moving costs. One would expect the likelihood of migrating to such states to decrease with the distance it takes to move to a state, since moving and social costs are likely to increase with such a metric. [20] Finally, birth state temperature is included to account for the effect of climate on athlete mobility choices. We conduct our analysis stratified by sport and with pooled specifications in order to understand more fully how migration patterns change across golfing and racing.[21]

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[20] While the monetary costs associated with moving are relatively small when compared to lifetime income of high earners, the moving costs variable can also be thought of as capturing the non-monetary benefits of living close to home – including proximity to family and friends and other neighborhood connectivity measures. This explains the persistence of athlete residences (and that of the general population) in their home states regardless of their agglomeration presence and tax policies.

[21] The results presented here use only income tax data to measure the tax attractiveness of states, as both tour income and endorsement income are subject to income taxation. However, one may also wish to consider effective tax rate information if it is believed that athletes also respond to other types of state taxation (sales taxes, corporate income taxes, etc.). Without individual tax return data, it is not possible to calculate the effective tax rate for each athlete. However, sensitivity analysis was conducted that replaced the income tax information with effective tax rate information for the typical household of three people with annual earnings of $150,000, the highest earnings available (Gandhi 2013). Those results are similar to what is presented here, largely because of the significant overlap between income and effective tax levels across locales.

The second set of models, displayed in equation (2), adopts a conditional logit specification consistent with the research of Dahl and Sorenson (2009) and other agglomeration papers.[22] The dependent variable in this framework is the likelihood of taking up residence in state *j* given a birth state of *i*. The outcome of each individual is represented by 50 outcomes in the dataset: one positive outcome (for the actual state of residence) and 49 negative outcomes (representing states where the athlete did not choose to live).

$$(2)\ \Pr(R=i|B=j) = F[(\text{Inc Tax}_j - \text{Inc Tax}_i), \ln(\text{Dist}_{ij}), (\text{Temp}_j\text{-Temp}_i), \text{Athlete}_i]$$

The conditional logit model differs from more traditional multinomial logit models in that decisions are identified as the product of differences across alternatives – which in this case are the states available to reside in – instead of across industry-specific factors. Athlete-specific identifiers may be included as case-specific measures, which are allowed to take on different parameter values for each alternative. However, given the relatively small size of our sample relative to the large number of choices (particular for racing), we were not able to implement that strategy empirically. Thus, we cannot test for the differential effects of migration by income in this framework.

While the conditional logit approach does not allow for a traditional incorporation of birth state characteristics into these regressions, we incorporate the effects of birth by defining each regressor through the difference between the measure in the birth state and that in the state of residence. The independent variable set includes the difference in income tax rates, average temperature, and distance between states *i* and *j*. As we show in

[22] This model is also referred to in the literature as a "McFadden choice" model.

the summary statistics below in Table 17, birth state factors appear to be an important component of migration outcomes.

Importantly, this specification allows us to include an explicit measure of agglomeration. This is established through a measure of the number of other athletes in the industry who reside in state *j*: regressions with both a linear and logged measure of athlete presence ($Athlete_i$) are included in this specification set. The inclusion of this metric changes the expected outcomes of key variables across sports. In our other specification, sport indicator and income variables served as a proxy for the combined effect of agglomeration and tax preferences. The inferences about the effects of agglomeration and taxation were based on differences in results for each sport. In this specification, agglomeration and tax preferences are accounted for separately: therefore, we would expect similar results for the income tax variable but notable differences in the agglomeration measure across industries.

IV. DATA CONSTRUCTION AND DESCRIPTIVE ANALYSIS

Event Income and Mobility Measures

This research uses data on athletes participating in the NASCAR and the PGA tours, the largest and wealthiest American sports organizations in racecar driving and golfing respectively. Constructing a dataset suitable for the desired empirical analysis required several data sources and a few simplifying assumptions. The data includes any athlete that participated in at least one 2013 event. For golfers, events include those from the PGA Tour, the Web.Com Tour (which typically includes younger golfers who have not yet qualified for the PGA Tour), and the Senior PGA Tour (which is open to all PGA-qualified golfers over 50 years of age). For racecar drivers, events include the NASCAR

Sprint Cup (the most popular of the tours, equivalent to the PGA Tour for golfers) and the NASCAR Nationwide Cup (the development series for younger racers). Earnings information is taken exclusively from the official websites of the PGA and NASCAR; racecar income includes driver salaries from team memberships. Roughly 550 golfers and 150 racecar drivers meet the above criteria.

Records of the state of birth and of the primary residence are included for all athletes. As with tour income the official tour websites provided near-universal coverage of birth state information, and also have some data on primary residences. When primary residence information was unavailable from the tour website, we rely on information from sports websites run by sports services from ESPN, Yahoo, Golf.com, and Racing Reference—if these sites were not conclusive, we draw from athlete profiles from newspapers and magazines to identify a place of residence. In a few instances (for fourteen golfers and three racecar drivers), multiple home locations were identified and we were not able to identify a site of primary residence: these observations are dropped from our dataset to eliminate any ensuing uncertainty in our regression results.[23]

Endorsement Income

Data on actual 2013 endorsement income is publicly available for only 31 golfers (Sirak 2014) and ten racecar drivers (Forbes 2014). However, given the strong tax implications of this income source discussed above, we opt to generate estimates of endorsement income for the remainder of the population. Using the available data, we first identify endorsement income patterns by regressing the log of an athlete's endorsement income

[23] Specifications were also run where these athletes were included with their primary residence was the one in the lowest tax state, implying that such residences were leveraged for tax purposes. The ensuing results did not differ significantly from those presented in this analysis.

on their tour income, the age, and the square of their age.[24] The coefficients are then used to generate endorsement estimates for the remaining athletes: a minimum value of zero was imposed on these estimates of zero (to ensure all values were non-negative), and set a maximum for these estimates at a value equal to the lowest endorsement earnings of the actual data available for each sport.[25]

Foreign Athletes

Athletes either born or living abroad pose a special issue for this research. Some foreign-born athletes moved to the United States and established residence, while others have maintained foreign residences. For example, Sergio Garcia lists his residences as Spain and Switzerland, but maintains his tax home in Switzerland.[26] Foreign-born golfers who do not become U.S. citizens or green card holders can avoid becoming a resident of the United States for tax purposes—and subject to taxation on their worldwide income by the United States—by limiting their time in the United States to avoid the "substantial presence test" or by establishing a "closer connection" to a foreign country.[27] The decision to become a resident of the U.S. is endogenous, but there is no additional information to predict the decision of a foreign golfer to reside in the United States. The

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[24] This specification returned an Adjusted R^2 value of 0.15. Specifications with linear, cubic and quartic age functions were also tested and did not return significantly different results.

[25] Specifications with maximum estimated values of 83.3, 75.0, and 66.7 percent of lowest actual endorsement earnings were also tested, again with no significant variation in the ensuing regression results from those presented below.

[26] A recent Tax Court decision discusses his tax situation. See Ambord (2013) for details.

[27] See Internal Revenue Service (2012) for a discussion of these concepts. A golfer could avoid triggering the substantial presence test by spending less than 122 days each year during a three-year period.

empirical work avoids this endogeneity by eliminating all golfers either born or currently living abroad.

Final Sample Mobility Statistics

The final sample consisted of 452 golfers and 142 racecar drivers. The average earnings across sports were remarkably similar, with mean income for golfers equal to $1.47 million while income for racecar drivers was $1.38 million. The only notable demographic difference between the sports was in the average age, as the mean value for golfers (39.9 years) was seven years higher than for racecar drivers (32.9 years): this is explained largely by the existence of a Senior Tour for golfing with no age-equivalent series in racecar driving.

Summary data on state residential information of golfers and racecar drivers reveals significant differences across sports and with the general public. Golfers are more than twice as likely as the average U.S. citizen to reside in a state that does not impose an income tax (45 percent to 20 percent), while racecar drivers are less than half as likely as the general population to live in such states. The data show that roughly three quarters of drivers on the NASCAR tour live in North Carolina, a state with only three percent of the general U.S. population. Both golfers and racecar drivers are less likely to live in California than the average resident. This may be due to high state tax rates and low agglomerative draw for these industries in California, which can offset the advantages of warm weather and potential endorsement opportunities present in the state.

Table 17 combines information about tax rates with average temperature data to gain an overall picture of mobility for these two important factors. States are divided into those with cold, mild, and hot temperatures. Migration among golfers appears to be concerned with tax differentials and climate in roughly equal proportion: high-tax and cold-weather states experience high levels of emigration and little immigration, while low-tax and hot-weather states incur strong positive migration flows. For racing, migration is driven almost exclusively by the category that includes North Carolina, although other types of net migration results are a bit higher for lower tax states than for higher tax categories.

Table 17 produces a “stickiness” of mobility of professional athletes, as in the sample 48 percent of golfers and racecar drivers live in states with the same temperature and tax policy designations as the state in which they were born. Further inspection shows that this pattern is driven almost entirely by non-migrants, as 46 percent of all athletes had the same residence and birth states. This finding helps to confirm the significance of non-monetary moving costs discussed above, as even athletes with much to gain from the exploitation of tax differentials choose disproportionately to remain in their home state. When the analysis is confined to athletes that change residence, golfers have strong migratory tendencies toward the southern and mountain regions, with the South Atlantic region (which includes North Carolina and Florida) representing a particularly attractive destination. Among drivers, migration is undertaken almost exclusively to the South Atlantic, which was expected given the residential tendency towards North Carolina mentioned earlier.

V. ECONOMETRIC ANALYSIS

The results for the first set of probit models are split into Tables 18 and 19. Table 18 shows the results for the golfing population (columns 1 through 3) and racing population (columns 4 through 6), while Table 19 displays the results of the pooled sample regressions. The results are suggestive of a stark contrast in the mobility decisions of golfers and racers that belies the similarities in the setup across sports.

Table 18 produces some evidence of a positive relationship among golfers between income and the likelihood of moving to a tax-advantaged state, as the logged income measure returns values significantly different from zero and in the direction predicted by theory in two of the three stratified regressions. This is consistent with the predictions offered in the discussion, and indicates that as golfers generate more income (and thus have more to gain from exploiting tax differentials), their mobility rate to tax-advantaged areas increases.

Unlike the golfing cohort, income for the racing population has no effect on the likelihood of migrating to a tax-improved location. In fact, of the three regressions the only result with an income coefficient significantly different from zero produces a negative sign, which runs contrary to the predictions generated by Table 16. Since the incentive to move to a tax-advantaged location increases with income, the lack of this relationship among the racing cohort may be taken as a sign that another factor (such as agglomeration) is affecting migration patterns of the industry as a whole.

Climate appears to have an important effect on golfers consistent with the results in the mobility tables. Increasing the temperature of a golfer's birth state is found to decrease their likelihood of moving to a low tax state. This result is consistent with the story that golfers prefer both low taxes and warm weather, and meeting the athlete's climate preferences makes them less likely to move (as migration would solely be for tax purposes). Meanwhile, the regression results for racers display no sign of an influence on weather in determining migration outcomes.

The variable measuring distance to locations without an income tax is surprisingly found to have a significant, positive relationship with the likelihood of tax-improving migration among golfers, but no relationship among the racing subsample. Such a result reflects the difficulties of using distance measures. The distance measured between two states also predicts the amount of overlap each location will share in weather behavior and they may indirectly imply that climate is very important for golfers. The variable measuring distance to a state with both no income tax and warm weather produce no significant results for golfers but a positive relationship among the racing cohort. Given the general migration pattern of racers, the result for the racing cohort may suggest that both warm weather and low taxes are needed in order for a racecar driver to resist the agglomerative draw of North Carolina.

The results of the pooled regressions as presented in Table 19 largely confirm the findings in the stratified samples. The pooled regressions again produce a significant dichotomy in the effect of income on the migration decisions of golfers and racecar drivers. The interaction term between income and golfers is positive and significant in each of the six specifications, but there is no significant effect on income by itself. This provides stark evidence that income seems to matter only for golfers. The result is especially strong when the temperature effect was split into sport-specific variables, as the income effect was roughly four times as strong in these regressions as it was in specifications with only a pooled temperature regressor. Meanwhile, for racecar drivers any indication of a relationship between income and migration patterns suggested that more elite performers are less likely to engage in tax-improving migration. As with the stratified sample, such a counterintuitive result is suggestive of a strong agglomeration presence in the industry.

Other results in Table 19 demonstrate the importance of factors other than taxes and agglomeration in migration choices. As in the previous table, age returns a strong, positive relationship with tax-improving mobility in all specifications, indicating that athletes of all types may place increased value in states with lower tax rates as they get older. The distance metrics again reveal a strong positive relationship among golfers for the "tax only" variable and with racers for the "tax and weather" factor. Unlike in Table 18, the one regression with sport-specific distance metrics to the nearest warm-weather, no-tax state produces a significant and negative relationship for the golfing cohort. [28]

The results of the conditional logit regressions are presented in Table 20, and help to place the previous findings in context by differentiating between the effects of tax preferences and agglomeration. The results for the income tax rate differential parameter indicate that both golfers and racecar drivers are attracted to states with low tax rates. Holding other variables constant, a one percentage point decrease in the income tax rate of a state increases the odds ratio of living in a given state (which is the probability of living in a state divided by the probability of not living in that state) by about five percent for golfers and roughly ten percent for golfers. For example, take a situation where an athlete living in the West is contemplating the relative appeal of living in Tennessee, a state with no income tax, and Georgia, a state with a top income tax rate of six percent.

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[28] This result warrants further exploration, but could confirm an increased need to leave the birth state among golfers who are born in colder climates (which would thus be further away from such states). Such a relationship is supported by evidence of a negative relationship between tax-improving migration and birth temperature, which is also present among racecar drivers.

(For this example, the distance to each state, temperature, and athlete presence are roughly equal.)

These results suggest that the difference in tax rates would lead Tennessee's odds ratio to increase by 0.30 points relative to that of Georgia for golfers, while the difference would be 0.60 points for a racecar driver. Note that racers actually have a stronger tax effect in these regressions, which was not identified in earlier specifications that did not separate income and tax preferences. However, recall that because of sample size we cannot explore the important interaction between athlete income and tax differentials in this specification.

In this specification it is the difference in athlete clustering across states is the primary determinant of the different migration outcomes across sports. The athlete factor—our direct measure of agglomeration—produced positive and statistically meaningful results for both golfing and racing. The coefficients are larger for racing than for golfing. Taken in combination with the strong athlete presence in North Carolina athlete presence explains why racers migrate overwhelmingly to a high-tax state while golfers generally prefer low tax locations.

Using the linear share measure of athlete presence, the agglomerative draw of North Carolina is equivalent to a state with identical distance and temperature values but no athlete presence decreasing its income tax rate by more than 50 percentage points: the comparative example for the most popular state among golfers (Florida) is equivalent to an identical state with no athlete presence reducing its income tax rate by less than half of that amount (around 25 percentage points). The logged variable produces a similar outcome, as the roughly fifty percent difference in the magnitude of the agglomeration factor across sports combined with the dominant racer residential presence of North Carolina makes the agglomeration draw in that state much more powerful among racecar drivers than any state for the golfing cohort.

These results speak to the power of hysteresis on migration outcomes. While the magnitude of each effect is different in each industry, the models in Table 20 suggest that athletes in both sports are drawn to places with low taxes and an industry presence. That these forces combined to produce high levels of agglomeration in racing and not golfing may owe much to the heavy historical presence of racing in North Carolina and the lack of a comparable locale in golf.[29] Model specifications that do not include the agglomeration factor again show a relationship with tax rate differentials that is positive among golfers and negative with racecar drivers. This result would seem to confirm agglomeration as the omitted factor that led to the counterintuitive income result among racers found in the first set of specifications.

The distance and temperature results are consistent with our prior findings. Increased proximity to an athlete's birth state raises the likelihood of being the state of resident for both sports: this is consistent with the stickiness of birth regions shown in Table 17. Warmer climates also proved attractive to both golfers and racecar drivers.

VI. DISCUSSION

This research examines the differences in the effects that agglomeration can have on the determinants of state residence choices of golfers and racecar drivers. The raw data

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[29] Hysteresis may explain why the patterns of residence and tax policy among NASCAR drivers appear to differ from drivers in the European Formula 1 series, who seem, like PGA golfers, more likely to seek tax-preferenced locations than the general public (Brown 2014). These findings confirm the importance of industry-specific knowledge when predicting migration outcomes related to agglomeration and tax policy.

indicates sharp differences in residential locations between the two sports, with the majority of drivers located in a single state with relatively high income taxes, while golfers were more than twice as likely as the average citizen to live in states with no income tax. After including a range of explanatory variables, the first regression results confirm differential behavior across these two sports, consistent with our theory. Golfer mobility decisions appear to be driven solely by low tax rates and warm weather. While racecar drivers also find warm climates and low taxes appealing, the draw of those factors is negated by a strong agglomeration presence in the industry, leading to near-universal mobility toward a state with a warm climate but also a high set of tax rates. The conditional logit analysis suggests that tax rate differentials and weather affected both groups, but racing decisions are overwhelmingly affected by agglomeration.

This work suggests that a simplistic picture of tax differentials and interstate mobility may be misleading. The structure of compensation within industries and the presence of agglomeration effects are likely to be important determinants of interstate mobility, along with tax rates and other fiscal variables. Ultimately, the differences found across golf and racecar driving, industries with much in common in terms of travel and earnings structures, highlights the need for carefully assessing the impact of taxes on individual and firm decisions.[30] High taxes may cause some subgroups to leave a state,

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[30] These results also offer insight into the possible effectiveness of highly targeted tax policies such as the "athlete tax" (Locker 2013) across industries. The willingness of racecar drivers to locate in a high-tax state and forgo the significant earnings increases that would accompany tax-advantaged migration suggests that policies like the "athlete tax" would not serve as a migration deterrent in areas that already have an agglomeration presence in a particular industry. On the other hand, the imposition of such a tax on industries like professional golf may serve as a driver behind significant state emigration, demonstrating the importance of industry knowledge before creating such targeted policies.

but other subgroups may remain to take advantage of agglomeration economies. Tax policies should take these factors into account.

Table 14: Location of NASCAR and PGA Tournaments By State*

PGA TOUR (Golf)							
State	Events	State	Events	State	Events	State	Events
Florida	8	Ohio	3	Nebraska	2	Minnesota	1
California	7	Pennsyl-vania	3	New York	2	Missouri	1
Texas	7	Arizona	2	South Carolina	2	Nevada	1
Georgia	6	Connecticut	2	Tennessee	2	New Jersey	1
North Carolina	6	Indiana	2	Alabama	1	Utah	1
Hawaii	3	Kansas	2	Idaho	1	Washington	1
Illinois	3	Louisiana	2	Iowa	1	West Virginia	1
Maryland	3	Mississippi	2	Massachu-setts	1		

NASCAR TOUR (Racing)							
State	Events	State	Event s	State	Event s	State	Event s
Florida	6	Alabama	3	Michigan	3	New York	2
Virginia	6	California	3	New Hampshire	3	Indiana	2
Arizona	4	Illinois	3	Texas	3	Pennsyl-vania	2
Delaware	4	Indiana	3	Georgia	2	South Carolina	2
North Carolina	4	Kansas	3	Iowa	2	Ohio	1
Tennessee	4	Kentucky	3	Nevada	2	Wisconsin	1

**Racing exhibitions include events from Sprint and Nationwide Tours. Golfing exhibitions Include events from PGA, Senior, and Web.Com Tours.*

Table 15: 2013 Tour and Outside Income for Top American PGA Earners (millions)

Athlete	Age	Residence	Top Income Tax Rate	Golf Income	Outside Income
Tiger Woods	38	FL	0	$12.1	$71.0
Phil Mickelson	44	CA	13.3	$7.0	$45.0
Henrik Stenson	38	FL	0	$18.6	$2.9
Ernie Els	44	FL	0	$2.2	$12.5
Matt Kuchar	36	GA	6	$7.1	$3.8
Luke Donald	36	IL	5	$2.9	$7.0
Steve Stricker	47	WI	7.75	$6.6	$3.0
Graeme McDowell	35	FL	0	$4.5	$5.0
Lee Westwood	41	FL	0	$2.9	$6.5
Jordan Spieth	21	TX	0	$4.7	$4.5
Dustin Johnson	30	SC	7	$4.6	$4.5
Jim Furyk	44	FL	0	$3.5	$5.3
Keegan Bradley	28	FL	0	$4.2	$4.5
Fred Couples	55	CA	13.3	$2.3	$6.0
Ian Poulter	38	FL	0	$4.2	$4.0
Zach Johnson	38	GA	6	$5.2	$3.0
Brandt Snedeker	33	TN	0	$5.7	$2.5
K.J. Choi	44	TX	0	$1.3	$6.5
Jason Day	26	OH	5.925	$5.2	$2.5
Webb Simpson	29	NC	7.75	$4.7	$3.0
Jason Dufner	37	AL	5	$3.7	$3.5
Hunter Mahan	32	TX	0	$3.3	$3.8
Davis Love III	50	GA	6	$0.4	$6.5
Rickie Fowler	25	FL	0	$2.2	$4.5
Bubba Watson	35	FL	0	$2.2	$4.2
Tom Watson	65	KS	4.9	$0.3	$6.0
Camilo Villegas	32	FL	0	$0.9	$5.0
Harris English	25	GA	6	$3.7	$2.0
Bill Haas	32	SC	7	$4.0	$1.5
Kenny Perry	54	KY	6	$3.4	$2.0
Billy Horschel	27	FL	0	$4.1	$1.3

Table 16: Tax Savings Examples		
PGA TOUR		
Below-average performer: Tour Earnings of $500K, Endorsements of $250K		
State of Residence	Total State Taxes Paid	State Taxes Paid After Federal Deductibility
California	$92,250	$55,904
Arizona	$45,317	$27,462
Florida	$27,190	$16,477
Average performer: Tour Earnings of $1M, Endorsements of $500K		
State of Residence	Total State Taxes Paid	State Taxes Paid After Federal Deductibility
California	$199,500	$120,897
Arizona	$91,997	$55,750
Florida	$55,744	$33,781
Elite performer: Tour Earnings of $2M, Endorsements of $3M		
State of Residence	Total State Taxes Paid	State Taxes Paid After Federal Deductibility
California	$665,000	$402,990
Arizona	$274,794	$166,525
Florida	$111,488	$67,562
NASCAR TOUR		
Below-average performer: Tour Earnings of $250K Salary & Endorsements of $500K		
State of Residence	Total State Taxes Paid	State Taxes Paid After Federal Deductibility
North Carolina	$58,571	$35,494
Florida	$12,273	$7,438
Average performer: Tour Earnings of $500K, Salary & Endorsements of $1M		
State of Residence	Total State Taxes Paid	State Taxes Paid After Federal Deductibility
North Carolina	$117,313	$71,092
Florida	$24,717	$14,979
Elite performer: Tour Earnings of $1M, Salary & Endorsements of $4M		
State of Residence	Total State Taxes Paid	State Taxes Paid After Federal Deductibility
North Carolina	$389,626	$236,113
Florida	$49,434	$29,957

Taxes paid on tour earnings were determined by calculating an effective tax rate on event earnings outside the state (assumed to be a function of the number of tournaments, with equal earnings in each event); and then allowing a credit for these payments in the resident state up to the liability due from the resident state tax. Salary and endorsement tax payments were simply a function of the resident income tax rate. We calculate net savings after federal deductibility.

Table 17: Migration By Effective Tax Rate and Average Temperature

Rows: Birth Tax Rate, Temperature. Columns: Residence Tax Rate, Temperature.

PGA TOUR

	Low, cold	Low, mild	Low, hot	Mod., cold	Mod., mild	Mod., hot	High, cold	High, mild	High, hot
Low, cold	12	2	16	0	0	1	0	0	3
Low, mild	0	13	15	0	0	6	0	0	0
Low, hot	3	2	80	0	1	10	0	0	4
Moderate, cold	0	0	7	3	0	0	1	0	0
Moderate, mild	2	3	27	0	16	5	0	0	6
Moderate, hot	0	1	19	0	1	36	1	0	6
High, cold	2	0	20	0	3	1	9	0	0
High, mild	1	0	11	0	1	0	0	2	2
High, hot	2	4	27	0	2	5	0	1	57

NASCAR TOUR

	Low, cold	Low, mild	Low, hot	Mod., cold	Mod., mild	Mod., hot	High, cold	High, mild	High, hot
Low tax, cold	0	0	0	0	0	0	0	0	6
Low, mild	0	4	0	0	0	0	0	0	10
Low, hot	0	0	9	0	0	0	0	0	15
Moderate, cold	0	0	0	0	0	0	0	0	0
Moderate, mild	0	0	1	0	5	0	0	0	12
Moderate, hot	0	0	0	0	0	5	0	0	17
High, cold	1	0	0	0	0	1	1	0	11
High, mild	0	0	0	0	0	0	0	0	4
High, hot	0	1	3	0	0	0	0	0	36

Table 18: Tax Mobility Regressions

Dep. Var.	P(Residence Income Tax < Birth Income Tax)					
	PGA			NASCAR		
Regressor	(1)	(2)	(3)	(4)	(5)	(6)
Age	0.014	0.012	0.013	0.015	0.015	0.013
standard error	*0.005* **	*0.006* *	*0.006* *	*0.011*	*0.011*	*0.012*
ln(Income) [A]	0.055	0.065	0.049	-0.070	-0.065	-0.173
standard error	*0.026* *	*0.029* *	*0.028*	*0.059*	*0.063*	*0.086* *
ln(No Tax Distance) [B]		0.206			0.019	
standard error		*0.030* ***			*0.040*	
ln(No Tax, High Temp. Distance) [C]			-0.035			1.494
standard error			*0.103*			*0.286* ***
Birth State Temperature	-0.054	-0.036	-0.022	-0.014	0.011	0.044
standard error	*0.008* ***	*0.011* ***	*0.011* *	*0.015*	*0.022*	*0.025*
Constant	1.541	0.451	0.117	0.346	-1.007	-4.965
standard error	*0.604*	*0.705*	*0.780*	*1.281*	*1.624*	*1.932* *
Observations	452	452	452	142	142	142

* p<0.05, ** p<0.01, *** p<0.001: z-scores in parentheses

Table 19: Pooled Tax Mobility Regressions						
Dep. Var.	P(Residence Income Tax < Birth Income Tax)					
Regressor	(1)	(2)	(3)	(4)	(5)	(6)
Age	0.014	0.012	0.012	0.014	0.013	0.013
standard error	*0.005*	*0.005*	*0.005*	*0.004*	*0.005*	*0.005*
	**	*	*	**	*	**
ln(Income) [A]	-0.002	-0.004	-0.017	-0.095	-0.088	-0.228
standard error	*0.020*	*0.025*	*0.025*	*0.046*	*0.051*	*0.078*
				*		**
ln(Income)*PGA [A]	0.043	0.053	0.052	0.154	0.158	0.289
standard error	*0.011*	*0.012*	*0.011*	*0.048*	*0.055*	*0.080*
	***	***	***	**	**	***
ln(No Tax Distance) [B]		0.161			0.018	
standard error		*0.022*			*0.038*	
		***				
ln(No Tax Distance)*PGA [B]					0.190	
standard error					*0.051*	
					***	
ln(No Tax, High Temp. Distance) [C]			0.203			1.230
standard error			*0.093*			*0.294*
			*			***
ln(No Tax, High Temp. Distance)*PGA [C]						-1.204
standard error						*0.313*
						***
Birth State Temperature	-0.047	-0.027	-0.009	-0.026	-0.004	-0.009
standard error	*0.007*	*0.010*	*0.010*	*0.011*	*0.015*	*0.015*
	***	**		*		
Birth State Temperature*PGA				-0.026	-0.030	-0.003
standard error				*0.011*	*0.013*	*0.012*
				*	*	
Constant	1.330	0.164	-0.945	1.335	0.183	-0.726
standard error	*0.538*	*0.631*	*0.700*	*0.543*	*0.631*	*0.705*
	*			*		
Observations	594	594	594	594	594	594
* p<0.05, ** p<0.01, *** p<0.001: z-scores in parentheses						

Table 20: Conditional Logit Regressions

Dep. Var.	Likelihood of living in a given state					
	PGA			NASCAR		
Regressor	(1)	(2)	(3)	(4)	(5)	(6)
Income Tax Rate Difference	-0.047	-0.052	-0.120	-0.079	-0.114	0.162
standard error	*0.019*	*0.015*	*0.016*	*0.035*	*0.033*	*0.023*
	*	***	***	*	***	***
ln(Distance From Birth State) [A]	-1.307	-1.235	-1.381	-1.371	-1.195	0.880
standard error	*0.058*	*0.062*	*0.060*	*0.122*	*0.128*	*0.087*
	***	***	***	***	***	***
Temperature Difference	0.089	0.023	0.154	0.103	-0.045	0.118
standard error	*0.011*	*0.022*	*0.009*	*0.022*	*0.025*	*0.007*
	***			***		
Athlete Presence (linear share)	0.061			0.072		
standard error	*0.008*			*0.004*		
	***			***		
Athlete Presence (logged share)		0.813			1.264	
standard error		*0.126*			*0.092*	
		***			***	
Observations	22,600	22,600	22,600	7,100	7,100	7,100
* p<0.05, ** p<0.01, *** p<0.001: z-scores in parentheses						

DOES REDUCING LEGISLATIVE DISCRETION IMPROVE RAINY DAY FUND OUTCOMES?

This research examines how heterogeneity in accrual and disbursement mechanisms – methods of spending and saving – affect the performance of rainy day funds, contingency funds intended to aid governments during a financing shortfall. Though some research has encouraged states to tie fund activity to measures of volatility, other factors, including the appeal of flexibility in the budgeting process, contribute to significant heterogeneity in the way rainy day funds are structured across states. There is definitive evidence that volatility-based mechanisms improve the alignment of fund changes and economic performance, and mixed evidence of an effect on the magnitude of fund changes. Further analysis shows that volatility-based mechanisms have a greater effect in states with more relaxed budget-balancing requirements, indicating that other budget rules may serve as substitutes for improving fund outcomes in strict budget-balancing observations. This demonstrates the potential efficiency gains available outside of rainy day fund maximum value modifications.

I. INTRODUCTION

Budget stabilization accounts – contingency accounts that fund public institutions during revenue shortfalls, also known as rainy day funds – are among the most important fiscal safety nets available to state governments. The duration and intensity of business cycles can be unpredictable, and states generally have less power to affect macroeconomic trends than the federal government. The largest revenue sources of state governments, intergovernmental transfers and sales taxes, are driven by consumer spending and federal assistance, which can vary with economic conditions (Tax Policy Center 2015). This means that state institutions can be left with especially low revenues when their constituency is most in need of government assistance. Until recently, rainy day fund utilization rates were widely lower than those recommended by the literature. In the past two decades, increased research on the value of a sufficiently large rainy day fund in times of fiscal stress has contributed to a rise in both the implementation of rainy day funds and the average size of those accounts among state governments.

The efficiency of rainy day fund structures has received considerably less attention. In order to maximize the usefulness of a rainy day fund, it is necessary not just to have a fund large enough to help governments absorb fiscal pressures that accompany economic downturns, but also to have methods of fund accrual and withdrawal that effectively collect money when times are good and spend funds when they are needed. An inefficient fund may exacerbate the budgetary problems of states, even if it is of sufficient size suggested, if revenues are being collected in poor economic conditions or spent in periods of high productivity. Creating an efficiently operating fund may be more difficult than it appears. Though some research has encouraged states to tie fund activity to measures of volatility, defined as mechanisms directly linked to economic conditions, factors including the appeal of flexibility in fund use and a non-automated process behind identifying public needs contribute to significant heterogeneity in the way rainy day funds are structured across states.

This research compares the effectiveness of rainy day funds that use volatility-based accrual and disbursement mechanisms with those that rely on mechanisms based on legislative action. Do certain types of rainy day fund structures more effectively collect revenues in times of economic prosperity and spend down funds in economic downturns? Research that observes the effect of rainy day fund structure on performance is limited. Such studies have traditionally focused exclusively on fund accrual mechanisms, and use data that did not reflect subsequent changes to rainy day fund size and use. This analysis seeks to provide a rigorous empirical test of both accrual and disbursement mechanisms using recently available data.

This research tests for the relationship between volatility mechanisms and performance two ways. A probit analysis is used to measure whether such volatility-based accrual and disbursement mechanisms increase the likelihood that funds increase their balances in good times and draw down balances in poor economic periods. It also adopts a linear regression to test for the effect on magnitude, or whether such funds collect and spend more money, accounting for existing fund ceilings, than other types of accounts. In both parts of the empirical approach, observations are separated into jurisdictions with loose (low-stringency) and strict (high-stringency) budget balancing rules, to see if differences in the pressure to achieve budget performance outcomes affect the influence of volatility measures.

The results indicate volatility-based accrual mechanisms lead to improved alignment with economic performance than other types of funds, and that they also collect a larger percentage of their allowable limit. Stratification results indicate that this finding is largely driven by improved performance in states with low levels of budget stringency, suggesting that such rules can help to compensate for less rigorous budget processes. Evidence on the efficacy of disbursement mechanisms based on volatility and legislative discretion is more limited. These findings emphasize the proper identification of business cycles in determining the efficiency of rainy day funds.

II. BACKGROUND

Budget stabilization funds, or rainy day funds, are a means of reducing government vulnerability to inter-temporal economic changes in the demand and supply for public services. The available balance and use of these funds is designed to be dependent upon the business cycle. Through the accrual of fund revenues in prosperous periods, policymakers provide themselves with an alternative means of financing in leaner periods when balanced-budget obligations would otherwise require drawing additional revenue from sources that may further hamper economic growth (and which may also be very unpopular). These funds have proven particularly useful in recessionary periods, as the pro-cyclical nature of the typical public budget means that governments often experience reduced revenue streams and increased demand for disbursements simultaneously (Gordon and Rueben 2009).

The United States federal government has no general budget-balancing requirement, and thus has no legal need for a rainy day fund. However, every state government in the union except Vermont has some sort of annual balanced-budget requirement. Budget stabilization fund use among the states is widespread, with 46 of those states having active rainy day accounts (Gordon and Rueben 2009). Rainy day funds have grown significantly over the past few decades. Average account values increased from 2.0 percent of annual average total expenditures in 1987 to 5.0 percent in 2012 (McNichol 2013). Moreover, these funds have been used to great effect, as they helped to avert over $20 billion in cuts to services and tax increases in the early 2000s.

Types of Accrual and Disbursement Mechanisms

The accrual and disbursement processes exhibit a tradeoff between funds tied closely to economic conditions and accounts with legislative autonomy. Economic research tends to advocate for funds based on economic activity, arguing that they more consistently align accrual and disbursement patterns with public needs (Pew Charitable Trusts 2014). However, accounts funded through legislative activity may increase their predictability. This could increase reliance on budget stabilization funds, and allow for better long-term planning in situations where a prosperous or recessionary period lasts for multiple years.

Accrual mechanisms based on economic conditions, known heretofore as volatility-based mechanisms, can be structured in a few ways. This research identifies four types of funds as exhibiting volatility-based accrual mechanisms: funds that use a formula linked to overall revenues, accounts that adopt a formula linked to a specific revenue source, funds that collect with a formula linked to economic conditions, and those that collect based on errors in revenue forecasting.

Deposits made through a formula linked to overall revenue levels typically allows for rainy day funds to track closely with economic performance, as absent major policy changes general revenue levels tend to exhibit similar patterns as the underlying economy. Such a process may call for adjustments when there are major changes to tax policies, or when revenues are generated more from focused sectors of the economy (investment taxes, excise taxes, etc.) in lieu of broad-sourced taxation (personal and corporate income, sales, etc.).

Accruals based on a formula linked to a specific revenue source have traditionally been instituted using receipts unique to a particular jurisdiction, such as oil and gas exports in Alaska. The effectiveness of this mechanism in establishing a well-functioning rainy day fund is a function of both the relative importance of revenue source to total funding, and to the relationship between changes in that source and shifts in the general economy.

Formulas linked to economic conditions offer a similar set of benefits and drawbacks as formulas linked to overall revenues. With economic indicators as the key metric, such formulas ensure that fund accounts move in response to public needs. Additionally, use of formulas protects fund activities from unforeseen changes in the policymaking process.

Accruals that are based on errors in revenue forecasting distribute money to rainy day funds by the amount that actual accruals exceed the predictions of the budget office. These funds exhibit similar characteristics to those based on economic conditions, as a major source of error in public finance projections is an unforeseen change in economic growth. However, when forecasting errors deviate from economic trends, it may lead to account changes that run contrary to recommendations from the literature. For instance, in a case where a recession ends earlier than expected, governments are likely to raise more revenues than forecasted. A fund based on forecasting errors would in that case call for rainy day fund levels to increase, despite economic intuition suggesting that demand for public services would still be above average at the outset of an economic recovery.

There are also several types of rainy day funds that assign rainy day budget authority to legislators. The four types categorized in this research are accruals made through a required fund balance statute, accounts with deposits made through the appropriations process, accruals processed through static deposits (legislator appropriations), and mechanisms based on government surpluses.

Accruals made through a required balance statute are typically achieved through a formula linked to the most recent account levels. This mechanism offers policymakers foresight when activating account funds, as they will be able to understand how those funds will be replenished in advance. However, this process removes any ties with the economic business cycle, and thus requires attention from policymakers to ensure that additional funds are deposited at the appropriate times.

Budget stabilization accounts where deposits are made by appropriation refer to funding through the discretionary funding process. This mechanism offers policymakers more autonomy in maintaining accounts than some of the other alternatives, as it integrates such decisions into the creation of the annual budget. However, as such it also requires significant consensus – normally by the relevant budget and appropriation committees, the general legislature, and the executive – which in some cases may be difficult to reach.

Accruals processed through static (discretionary) legislative deposits further aligns rainy day account decisions with general funding procedures, as it removes any stipulation on where and how funds may be authorized. As with funding by appropriation this process affords policymakers with substantial discretion in determining fund values, which may be useful if the legislative process allows the account to track closely with business cycles, but which relies on consistent, successful passage of budget stabilization legislation.

Accrual mechanisms that are derived from government surpluses devote a certain percentage or maximum nominal value of the amount by which revenues exceed disbursements in a given year to the budget stabilization fund. Government surpluses are typically a strong indicator of a prosperous economic period (Tax Policy Center 2015) which ensures that such a mechanism will likely avoid collecting funds in times where drawing down from rainy day funds is more appropriate. However, evidence (Wagner and Elder 2007) indicates that state governments are structurally imbalanced, with disbursements naturally exceeding revenues under average outside conditions. Such a mechanism therefore risks collecting less frequently (and with lower amounts of revenue) than the optimal rate suggested by economic theory. This mechanism also depends on governments allowing surpluses to go unused, which implicitly prioritizes budget stabilization accruals below short-term demands for government funding that may increase when operating with a net surplus.

Use of volatility-based and legislator-dependent accrual mechanisms is not mutually exclusive. In practice, many governments may choose to combine economic indicators with legislator power to strike a balance between the attractive characteristics offered by each structure. Such funds are typically constructed so that a formula linked to economic performance “triggers” a legislative approval process. The threshold that establishes the trigger and the votes required to approve such a measure can vary significantly across jurisdictions.

Disbursement mechanisms generally exhibit more uniformity than accrual-based structures. The disbursement process can be split up into three categories analogous to those on the accrual side of the ledger: funds that spend through volatility-based, formulaic processes; funds that drawdown at the discretion of government officials; and funds that combine each of the first two processes. As with the accruals process, these differences typically exhibit a tradeoff between accounts that are more rigorous in their tracking of economic conditions and those that provide legislators with the autonomy to modify responses to unique settings as they arise.

There is significant heterogeneity across state governments in the selection of accrual and disbursement processes. While the presence of some variation is not surprising in a large country like the United States, the lack of a majority presence of any type of process is notable. This indicates either that budget stabilization funds are being used for different purposes, although the stated intent is similar across jurisdictions, or that no consensus has been established on what kinds of policies work best.

This is the first study that focuses exclusively on the effect of accrual and disbursement mechanisms on fund performance. Most of the literature has estimated the appropriate magnitude of budget stabilization funds as a function of economic performance and government needs, which has drawn attention to the need for larger accounts (Cornia and Nelson 2003, Navin and Navin 2003, Pew Charitable Trusts 2014). Yet the timing of fund activities is as important a component as the allotment of sufficient resources in constructing budget stabilization funds that serve as a countercyclical safeguard for governments: funds must not only be sufficient, but available if they are to serve their stated intent. These components work independently of one another: that a fund is sufficiently large to help government outcomes says nothing about how money will be collected or used.

This research includes analysis both on all observations and stratifications across a measure of budget balancing stringency. The impact of accrual and disbursement mechanisms may vary according to the stringency of other types of budget balancing rules in public governments. In certain cases, strict versions of budget balancing rules – which include when in the process public budgets must be balanced, how often deficits must be eliminated, and rules on if surpluses are kept or redistributed to the public – may serve as substitutes for any gains that otherwise would result from improvements in accrual and disbursement efficiency.

The empirical analysis adopts a number of controls that help to address a few major hurdles to estimation. Rainy day account values are measured in terms of their legal or estimated "cap," or the maximum value that the fund can take, to address differences in the allowed size across jurisdictions. Analysis is also stratified across groups with different levels of budget stringency, similar to work done by Poterba and Rueben (1999), to account for the effect that other budget rules can have on rainy day fund performance. Finally, a state-specific, time-consistent measure of economic performance is adopted to address heterogeneity in economic performance across observations.

III. DATA

This study uses activity from U.S. state governments over the past 30 years to capture the variation in rainy day fund activity. Information on state rainy day fund expenditures and general budgetary activity are taken from data collected in the Fiscal Survey of States, conducted biannually by the National Association of State Budget Officers (NASBO), from 1977 to 2010. NASBO began recording specific, consistent information about rainy day funds in 1987, thus providing a 24-year sample with which to observe budget stabilization behavior.[31] Rainy day fund information available includes the yearly account balanced (expressed both in dollars and as a percentage of annual government disbursements), account accrual laws and requirements, account ceilings and floors (when applicable), and disbursement rules and corresponding voting requirements. Importantly, the information on other state budget activity is also precise, allowing for the isolation of revenue and expenditure sources described in the previous section.

Observing how fund components influence the efficiency of their activity relies on accurate measurement of state economic conditions. This research relies on state coincident index information produced by the Philadelphia Federal Reserve Bank, for such a measure. The excerpt below summarizes the methodology behind construction of state coincident indices.

> The coincident indexes combine four state-level variables to summarize current economic conditions in a single statistic. The four state-level variables in each coincident index are nonfarm payroll employment, average hours worked in manufacturing, the unemployment rate, and wage and salary disbursements deflated by the consumer price index (U.S. city average). The trend for each state's index is set to the trend of its gross domestic product (GDP), so long-term growth in the state's index matches long-term growth in its GDP.
>
> A dynamic single-factor model is used to create the state indexes. James Stock and Mark Watson developed the basic model for constructing a coincident index for the U.S. Theodore Crone and Alan Clayton-Matthews adapted a basic model for the states. The method involves a system of five major equations: one equation for each input variable and one equation for an underlying (latent) factor that is reflected in each of the indicator (input) variables. The underlying factor represents the state coincident index. The model and the input variables are consistent across the 50 states, so the state indexes are comparable to one another.

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[31] Revenue and expenditure data from 1977 through 1986 is used to provide information about state budget trends.

Use of the coincident index therefore provides a measure that is both consistent across jurisdictions, and through analysis of growth rates, over the course of the sample. The coincident index is used in a similar manner for the rainy day fund research undertaken by Wagner and Elder (2007).

Data on political preferences is drawn from two sources: state-level data for years prior to 2004 is drawn from Dubin (2007), and more recent data is taken from the National Archives. Finally, data on budget stringency (which measures the rigidity of budget-balancing requirements across states) is taken from the annual *Budget Processes in the States* report (also issued by NASBO), which is used to construct a measure of state fiscal stringency.

IV. EMPIRICAL FRAMEWORK

This research employs two approaches to analyze the effect of accrual and disbursement mechanisms on rainy day fund efficiency. The first portion of the analysis seeks to identify if volatility-based mechanisms improve the probability that rainy day funds will increase account levels in prosperous periods and withdraw funds in economic downturns, or providing balance against the business cycle effects of public financing as designed. In this approach, the magnitude of the change in fund balances is ignored, thus excluding any potential confounding factors associated with different ceilings placed on fund values.[32]

$$(1)\ \Pr(\mathrm{Save}_{it}) = \beta_0 + \beta_1 D(\mathrm{Vol}_{it}) + \beta_2 P_{it} + \beta_3 t + \varepsilon_{it}$$

$$(2)\ \Pr(\mathrm{Spend}_{it}) = \Delta_0 + \Delta_1 D(\mathrm{Vol}_{it}) + \Delta_2 P_{it} + \Delta_3 t + \delta_{it}$$

[32] Observations that are at their upper limit in prosperous periods or with no funds in downturns are excluded from the sample. Analysis that included these observations did not significantly alter the findings presented here.

The framework used in this trend analysis for accruals and disbursements is displayed in equations (1) and (2), respectively. For accruals-based regressions, the dependent variable is the likelihood that rainy day fund balances increase in good times [*Pr(Save)*]. For disbursement-based regressions, the outcome variable is the likelihood of decreased rainy day fund balances in below-average economic periods [*Pr(Spend)*]. These equations use a probit methodology to identify how the accrual and disbursement mechanisms of state i in time year t influences the performance of its rainy day fund, subject to a set of controls and a residual component (ε and δ respectively).

Jurisdictions include any state with an active budget stabilization fund in the given time period – the size of the sample was 37 states in 1987 and 46 states in 2010. The analysis is further restricted to ensure that the accruals based sample measures only periods of robust economic activity, and low growth for the disbursement-based sample. Therefore, the accruals regressions [in equation (1)] are confined to observations with coincident index growth in the top two quintiles, while the disbursement regressions [in equation (2)] are restricted to observations in the lowest two quintiles of index growth. [33]

In accordance with heterogeneity in the way rainy day funds are structured, all empirical analysis adopts two types of structural indicators [*D(Vol)*]. The first serves as a dummy for "all volatility-based" accruals and disbursements: this variable only takes on a positive value for funds that exclusively use formulaic accrual or disbursement rules, and will test for the impact of excluding legislators from the process entirely. The second indicator is a "some-volatility based" mechanism test, which also is positive for funds

[33] Average growth rates are state-specific, and are calculated with inflation-adjusted data from 1980 to 2010.

with a hybrid structure. That indicator helps to determine the effect of removing all power over fund management from policymakers.

In addition to variables identifying accrual and disbursement processes, the independent variable set includes controls for political preferences (*P*) and time (*t*). Political control of state governing bodies may affect rainy day efficiency, as the tendency of one party or another to incur deficits may lead to the use of rainy day accounts as less of a counter-cyclical measure and more of a "slush fund." This research uses four types of political measurements to account for such a process: the percentage of legislators identifying as Democratic party members in the state legislature; the party identification of the governor; the combined Democratic representation of both the state legislature and executive; and the presence of a "supermajority" (control of each house of the legislature and the governorship) for either political party. [34] The inclusion of a time metric accounts for the effect of changes in fund efficiency that are due to the increased use of rainy day funds over the past decade rather than changes in the processes that govern such funds.[35]

Given the observations made in the existing literature, one would expect that any distinction between accrual processes would reveal closer alignment between volatility-based funds and economic indicators than other types of accrual mechanisms. Though such a result is most likely in states whose accrual mechanisms rely directly on economic formulas, the immediacy with which recessions affect the major sources of state revenues

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[34] This measure assigns equal weight to the legislature and executive, as most states require coordination from each branch of government when approving a budget.

[35] Sensitivity analysis replaced the continuous time measure with indicator variables that assign observations into two twelve-year periods or three eight-year periods, and produced no significant changes to the results.

– sales taxes are affected directly by consumption changes, and intergovernmental revenues which are typically reduced in recession periods (McNichol 2013) – are also likely to lead to close alignment between economic and fund measures. Similarly, disbursement mechanisms linked to formulas would also be expected provide a closer alignment to economic outcomes than those linked to legislative action.

The other approach used in the empirical analysis tests for the effect of fund structure on the magnitude of changes in account balances. Aligning changes in account levels with economic conditions is necessary but not sufficient to an optimally performing rainy day fund: those changes must also be large enough to make a difference in public finance outcomes. Past underutilization of rainy day accounts would suggest that funds with higher balances would be operating more similarly to what is suggested by the literature than less active accounts.

$$(3)\ \ln[(RDF_{it} - RDF_{i\text{-}1t})/(Ceil_{ic} - RDF_{i\text{-}1t})] = \theta_0 + \theta_1 D(Vol_{it}) + \theta_2 P_{it} + \theta_3 t + \zeta_{it}$$

$$(4)\ \ln[((RDF_{it} - RDF_{i\text{-}1t})/(RDF_{i\text{-}1t})] = \alpha_0 + \alpha_1 D(Vol_{it}) + \alpha_2 P_{it} + \alpha_3 t + \gamma_{it}$$

The framework for the accrual and disbursement magnitude analysis can be found in equations (3) and (4). The equations use natural logs of balance ratios for their dependent variables. The accrual analysis in equation (3) uses the amount of funds collected as a percentage of the funds that were available to gather, given both the previous balance and the maximum amount allowed to be collected in each fund. [36] For the disbursement analysis, the change in balances is taken as a percentage of the previous

[36] For states without a legal ceiling, the value takes on the greater of their own maximum fund account balance over the course of the sample and the 75th percentile maximum of all states without a legal ceiling.

balance, which represents the total amount that was available to be spent. All figures that determine the dependent variables are taken as a percentage of general fund disbursements, to ensure that the size of the total government does not affect the results. Each equation uses the same dependent variable set, sample restrictions, and volatility indicators that were used in the first part of the empirical analysis.

All empirical analysis is performed on three subsamples of the dataset. The first, and largest, is all observations that meet the earlier specified criteria: observations with active funds and robust growth for accruals analysis, and with active funds and poor growth for disbursement regressions. The second and third subsamples will then decompose that group into observations with low and high levels of budget-balancing stringency in their general funds, as defined by the Poterba and Rueben (1999), using data from the *Budget Processes in the States* report from NASBO. This analysis is included to test for the effect that other budget restrictions might have on the effect of implementing different rainy day structures, as suggested by Poterba and Rueben (1999). If budget-balancing requirements lead to the development of large and active rainy day accounts for all types of states, the fund structure may not be as influential in performance among high stringency states as it is in low stringency observations.

V. EMPIRICAL ANALYSIS

Summary Statistics

Table 21 presents the summary statistics for state rainy day fund characteristics and economic indicators across the whole sample, and split into three eight-year periods. In nearly every rainy day fund category there is a notable rise in usage that is consistent with the significant increase in fund reliance that is described elsewhere in the literature. The number of observations without a rainy day fund from 2003 to 2010 is less than half of that value from 1987 to 1994. Similarly, the average fund value rises by about 150 percent from the early period to the latest stage: the rise in fund levels is less pronounced when compared with 1995 to 2002 levels, although the reduced economic growth experienced in the last time period suggests that the difference is larger than indicated by the simple averages.[37]

There is notably less movement in the structure of rainy day accounts. Across all periods, states with rainy day funds tend to be roughly as likely as not to have some volatility-based mechanism included in accrual and disbursement mechanisms, and significantly less likely to have mechanisms exclusively based on volatility. The structures adopted by new observations in later periods are more or less proportional to the composition of fund structure in the earliest period.

The two dependent variables used in the regression analysis are also included in Table 21. The probability of the trend in rainy day fund values matching that of economic conditions (increasing in good times and decreasing in low-growth periods) nearly doubles from the first time period to the second and third. The ceiling-adjusted match between fund values and the economic index also improve over the course of the sample: the difference between the economic and rainy day fund values was cut by nearly than half from first period to the second, and was more than halved again from the second to third eight-year window. This is indicative of a more liberal use of rainy day funds towards the end of the sample period.

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[37] As rainy day funds are a counter-cyclical measure, one would expect to find lower fund values in tougher economic climates.

Other summary information indicates that the rainy day fund characteristics and performance of states with low and high budget stringency are similar. Rainy day fund take-up among low-stringency (85 percent) and high-stringency observations (84 percent) is nearly identical, as is the average fund balance taken as a percentage of the fund's cap (28 percent for low-stringency states and 29 percent for high-stringency states). There is limited evidence that high-stringency observations are more likely than low-stringency states to spend in poor economic times (37 percent versus 32 percent incidence) and collect in prosperous periods (49 percent versus 47 percent). The statistical significance of those discrepancies and their determinants are uncertain.

Figures 8, 9, and 10 illustrate the distribution of accrual mechanisms, disbursement mechanisms, and budget stringency measures across states. Figure 8 highlights the variation in accrual mechanism design in state governments. While there is some suggestion of spatial patterns in accrual mechanisms – for instance, legislator-based accrual mechanisms have a higher concentration in Midwestern states – geography and even neighboring state patterns seem to have very little predictive power. Figure 8 also shows that accrual mechanisms are relatively static within jurisdictions across the sample, as states that changed mechanism designations ("legislator and hybrid" and "hybrid and volatility" observations) are the least populated categories.

Figure 9 displays the disbursement mechanism distribution across state governments. As with accrual mechanisms, geographic regions do not seem to offer predictive power over disbursement mechanism design. As compared with the previous figure, neighboring states do appear to share disbursement mechanisms, though this may be explained in part by even less movement across categories over the course of the sample. Disbursement mechanisms are much less likely than accrual mechanisms to have a hybrid structure. One explanation for the reduced incidence of hybrid mechanisms is that in theory, disbursements are made in times of poor economic performance, where there is greater urgency than the prosperous periods of accrual and thus less room for extended debate that may accompany more complex systems.

Figure 10 displays the results of the budget stringency dummy variable across state observations. States are more likely than not to have high levels of budget stringency, particularly in the Midwestern and Western regions. However, the low-stringency observations tend to be found among more populous states, such as California, Illinois, and New York. As with the accrual and disbursement mechanisms, there was little change in stringency levels within a state over the 24-year sample. This highlights that the variation of interest in this research occurs mostly across states rather than over time.

Regression Analysis

Table 22 presents the results of probit regressions that test for determinants of collecting money in prosperous economic periods – which, for this survey, is defined as observations in the top 40 percent of growth in the coincident index. The results shown in columns (1) and (2), where regressions include all observations in good economic times, are suggestive of a significant effect of accrual structure on fund alignment. In those regressions, the presence of a volatility-based structure increased the likelihood of positive accruals in the rainy day account by 35 to 40 percent. All else equal, that indicates that modifying the account structure from a legislator-based to a volatility-based mechanism would increase the likelihood of saving in such periods from just over 50 percent to around 75 percent. This result supports the assertions in the literature that volatility-based mechanisms improve economic alignment.

Elsewhere in Table 22, columns (3) and (4) show the results of regressions run only in states with relaxed budget-balancing rules, while columns (5) and (6) show the coefficients for high-stringency observations. This stratification is included to test for the effect that other budget-balancing rules could have on the impact of accruals structure. The results indicate that other budget-balancing legislation plays a significant role in the effect that volatility-based accruals have on economic alignment. Among low budget stringency observations, shifting to volatility mechanisms has a significant impact that is three to four times as big as in the larger sample: the coefficients indicate that such a change more than doubles the likelihood of collecting rainy day funds in good times. Meanwhile, the relationship proves to be insignificant among observations with high levels of budget stringency.

One possible explanation for that discrepancy is that states with strict budget-balancing requirements are in need of active and large rainy day funds regardless of how they are structured, while lower stringency observations may have more flexibility in how unforeseen economic downturns are accommodated. Alternatively, volatility-based accrual mechanisms may and other budget processes that are more prominent in states with strict budget rules may serve as substitutes in their effect on fund trends: high-stringency observations may have other legislative processes in place that have a similar influence on rainy day fund levels.

In all of the regressions displayed in Table 22, the likelihood of saving in prosperous periods does not significantly change over time when other factors are accounted for. This suggests that the improvements in accrual alignment presented in the summary statistics are attributable to changes in the structure of budgets over time.

Table 23 tests for the impact of volatility-based disbursement mechanisms on the likelihood that states spend in poor economic periods (the lowest 40 percent of coincident index observations). As compared with the accrual results in Table 22, the relationship between volatility-based mechanisms and the timing of fund drawdowns is decidedly more mixed. Of the pairs of regressions run on the entire set of observations, low-stringency jurisdictions, and high stringency states, each includes a regression with a significant, positive relationship between volatility-based mechanisms and the likelihood of fund drawdown, as well as a regression with no evidence of significant effects.

In the larger sample, the more significant relationship is present when the indicator including jurisdictions with both exclusive volatility mechanisms and hybrid structures are included, while the volatility-only regression returned no evidence of a relationship. That pattern is also present in the regressions performed on high-stringency observations. Meanwhile, the pattern is reversed among low-stringency states: the significance is present among the volatility-only set of structures, and non-existent in the more inclusive indicator.

Unlike in the accrual trend regressions, observations recorded later in the sample period displayed a higher probability of drawing down funds in poor economic times. However, as with earlier regressions, there is little evidence that political representation plays any role in determining the alignment of fund activity and economic performance.

Table 24 presents the linear regression analysis of budget stabilization account structure on the magnitude of rainy day fund accruals. For this set of regressions, the dependent variable is constructed as a percentage of the distance between the prior year's value and the rainy day fund cap. The results for this set of regressions are similar to those presented in Table 23, as they show mixed evidence of a positive volatility-based effect. The strongest evidence for an accrual mechanism effect is present in low-stringency states. However, this relationship is not consistent across each definition of volatility-based accruals, as significance among low-stringency states is only observed with the more inclusive indicator that captures observations with some relationship to volatility.

Among the entire sample of observations with strong economic growth and with high-stringency jurisdictions, the significant regressions are indicative of a volatility-based mechanism increasing accruals by five percent, while in low-stringency states the significant results indicate a larger effect of around 17 percent. However, in each sample there are also regressions with insignificant evidence of a relationship between fund structure and the magnitude of accruals. Although insignificant in the regressions on economic alignment, time produces a positive effect in most of the regressions on accrual values, indicating that all else equal, states collected larger amounts in prosperous times, if not more often.

Table 25 shows the regression results on the magnitude of rainy day fund withdrawals during economic downturns. As the action of interest is disbursements, the funds spent in each observation are divided by the funds available from the previous year. Under that methodology, observations are held harmless for both their previous rainy day fund activity and the fund cap that may influence accrual levels. The results provide no evidence of any relationship between the type of rainy day fund structure and the level of withdrawals in any subsample of the dataset. Such a result indicates that when states are motivated to utilize rainy day accounts, their actions are similar regardless of the way the fund is structured.

VI. CONCLUSION

This research examines the effect that imposing rainy day fund structures that directly respond to changes in economic conditions have on the performance of rainy day accounts. Using state data from the past 30 years, it offers evidence that such mechanisms can, under the right conditions, have a significant impact on account activity, even when factors accounting for time and political preferences are included.

The empirical analysis shows strong, positive effects on the impact of volatility-based accrual mechanisms on the likelihood that funds increase when predicted to by the theory, and that such accruals are higher than they otherwise would be. Stratification across levels of budgetary stringency indicates that much of this result was driven by improvement among states with relaxed budget-balancing rules. Such a result suggests that changes to rainy day account structures may be one way to compensate for less imposing budgetary rules when more active rainy day accounts are desired.

The results for disbursement-based volatility mechanisms produce mixed evidence of a positive relationship between fund drawdowns and poor economic growth. Those results show no clear patterns across levels of budget stringency. While they may be suggestive of volatility-based mechanisms inducing more activity in account management, a lack of evidence may instead indicate that the political pressures to spend in recessionary periods are greater than those to save in more prosperous times. Such a finding could also explain the lack of relationship between fund values and disbursement structures.

Taken together, these findings confirm that while budget stabilization account ceiling regulations are likely to affect their influence, such restrictions are not the only means of increasing rainy day fund efficiency. The rules that govern the way these funds are managed, particularly in their accrual processes, are also an important factor in determining the degree to which such funds smooth consumption across business cycles, as intended.

Table 21: State Profiles, 1987-2010

	1987-1994	1995-2002	2003-2010	Total
States with RDF (%)	74.00	87.76	91.67	84.46
Average RDF Value (% of GF Revs.)	1.81	3.45	4.57	3.28
Average Coincident Index Growth (%)	3.65	3.37	0.80	2.61
State Political Index (1=All Democratic)[1]	0.567	0.437	0.519	0.508
Some Volatility-Based Accruals (%)	47.66	56.25	56.25	53.39
All Volatility-Based Accruals (%)	17.19	19.79	20.83	19.27
Some Volatility-Based Disbursements (%)	35.16	40.63	41.67	39.15
All Volatility-Based Disbursements (%)	15.63	21.35	22.92	19.97
Pr(RDF Trend=Business Cycle Trend)[2] (%)	24.65	43.92	44.6	38.54
(RDF Index - COI Index)[3]	-51.68	-31.67	-14.27	-31.22

[1]Equal weight assigned to political representation in the state legislature and governorship.

[2]A positive value indicates that either rainy day fund values are increasing with an above-average economy, or decreasing with a below-average economy.

[3]Both measures locally scaled from 0 (empty account; lowest growth) to 100 (maximum account; highest growth). Negative values indicate smaller fund levels relative to economic performance.

FIGURE 8

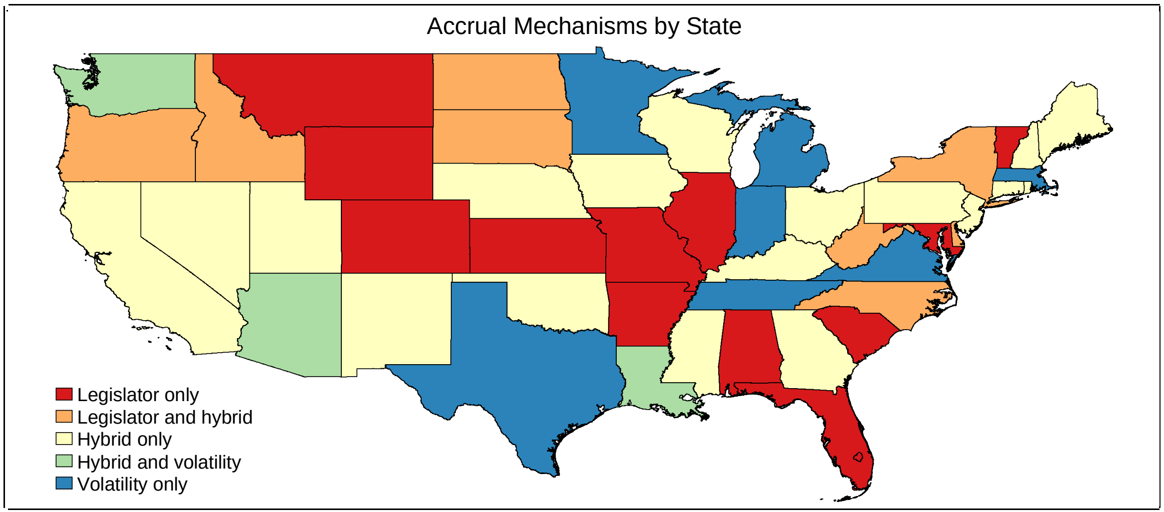
Accrual Mechanisms by State
Legislator only
Legislator and hybrid
Hybrid only
Hybrid and volatility
Volatility only

FIGURE 9

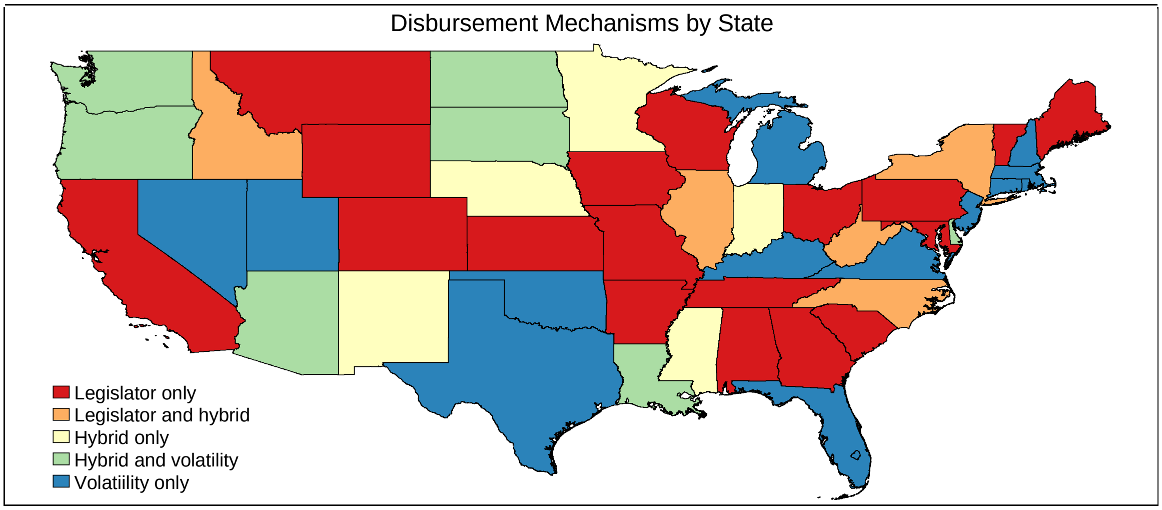
Disbursement Mechanisms by State
Legislator only
Legislator and hybrid
Hybrid only
Hybrid and volatility
Volatiility only

FIGURE 10

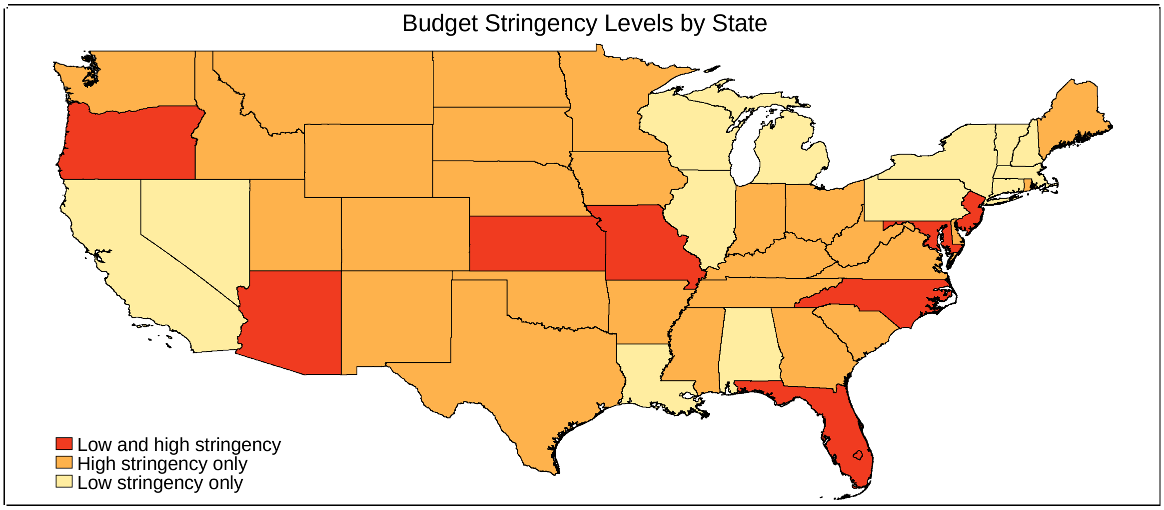
Budget Stringency Levels by State
Low and high stringency
High stringency only
Low stringency only

Table 22: Probit Trend Analysis, Accrual Mechanisms

Dep. Var.	*Pr(Accrual | Economic Index>=60)* [1]					
	All States		*Low Stringency States*		*High Stringency States*	
	(1)	(2)	(3)	(4)	(5)	(6)
D(All Volatility-Based Accrual)	0.355	-	1.341	-	0.146	-
standard error	*0.154* *	-	*0.418* **	-	*0.178*	-
D(Some Volatility-Based Accrual)	-	0.405	-	1.353	-	0.127
standard error	-	*0.128* **	-	*0.293* ***	-	*0.151*
Time	0.001	-0.003	0.017	0.002	-0.006	-0.007
standard error	*0.033*	*0.013*	*0.021*	*0.025*	*0.014*	*0.014*
% Democratic Legislature	0.005	0.005	-0.006	0.02	0.006	0.005
standard error	*0.004*	*0.004*	*0.008*	*0.009* *	*0.005*	*0.005*
D(Democratic Gov.)	0.067	0.100	0.152	-0.033	0.119	0.142
standard error	*0.120*	*0.120*	*0.245*	*0.254*	*0.142*	*0.142*
D(Supermajority)	0.074	0.078	0.035	0.276	0.116	0.111
standard error	*0.117*	*0.118*	*0.250*	*0.258*	*0.138*	*0.139*
Constant	-0.236	-0.463	0.189	-1.983	-0.24	-0.254
standard error	*0.268*	*0.284*	*0.540*	*0.703* **	*0.320*	*0.326*
R^2	0.043	0.042	0.051	0.051	0.046	0.047
Observations	384	384	107	107	277	277

* p<0.05, ** p<0.01, *** p<0.001: z-scores in parentheses

[1] Index uses state-specific coincident measures, scaled from 0-100 and increasing in growth.

Table 23: Probit Trend Analysis, Disbursement Mechanisms						
Dependent Variable:	*Pr(Disbursement | Economic Index<=40)* [1]					
	All States		*Low Stringency States*		*High Stringency States*	
	(1)	(2)	(3)	(4)	(5)	(6)
D(All Volatility-Based Accrual)	0.174	-	0.566	-	0.015	-
standard error	*0.118*	-	*0.224*	-	*0.136*	-
			*			
D(Some Volatility-Based Accrual)	-	0.343	-	0.396	-	0.335
standard error	-	*0.122*	-	*0.225*	-	*0.147*
		**				*
Time	0.023	0.021	0.016	0.012	0.026	0.024
standard error	*0.008*	*0.009*	*0.016*	*0.015*	*0.010*	*0.010*
	**	*			**	*
% Democratic Legislature	-0.001	-0.002	-0.004	-0.004	-0.001	-0.003
standard error	*0.003*	*0.004*	*0.010*	*0.010*	*0.003*	*0.005*
D(Democratic Gov.)	0.12	0.145	0.177	0.127	0.105	0.137
standard error	*0.118*	*0.119*	*0.253*	*0.244*	*0.142*	*0.143*
D(Supermajority)	-0.035	-0.052	0.072	0.099	-0.053	-0.091
standard error	*0.117*	*0.121*	*0.267*	*0.261*	*0.139*	*0.142*
Constant	-0.493	-0.575	-0.426	-0.356	-0.482	-0.604
standard error	*0.266*	*0.266*	*0.600*	*0.593*	*0.311*	*0.311*
		*				
R^2	0.043	0.043	0.047	0.048	0.048	0.048
Observations	422	422	130	130	292	292

* p<0.05, ** p<0.01, *** p<0.001: z-scores in parentheses

[1] Index uses state-specific coincident measures, scaled from 0-100 and increasing in growth.

Table 24: Linear Magnitude Analysis, Accrual Mechanisms

Dependent Variable:	*ln(Pct. of Cap Accrued | Economic Index>=60)* [1]					
	All States		*Low Stringency States*		*High Stringency States*	
	(1)	(2)	(3)	(4)	(5)	(6)
D(All Volatility-Based Accrual)	0.046	-	0.047	-	0.06	-
standard error	*0.023* *	-	*0.039*	-	*0.029* *	-
D(Some Volatility-Based Accrual)	-	0.054	-	0.177	-	0.042
standard error	-	*0.029*	-	*0.082* *	-	*0.035*
Time	0.007	0.007	0.011	0.012	0.006	0.006
standard error	*0.003* **	*0.002* **	*0.003* **	*0.004* **	*0.003*	*0.003*
% Democratic Legislature	0.001	0.001	0.001	0.003	0.001	0.001
standard error	*0.001*	*0.001*	*0.014*	*0.003*	*0.001*	*0.001*
D(Democratic Gov.)	-0.008	-0.003	0.043	0.057	-0.026	-0.016
standard error	*0.022*	*0.025*	*0.047*	*0.053*	*0.025*	*0.024*
D(Supermajority)	0.033	0.032	0.053	0.091	0.028	0.025
standard error	*0.022*	*0.022*	*0.054*	*0.058*	*0.024*	*0.024*
Constant	-0.079	-0.114	-0.076	-0.393	-0.24	-0.254
standard error	*0.051*	*0.061*	*0.141*	*0.254*	*0.320*	*0.326*
R^2	0.046	0.045	0.072	0.099	0.049	0.029
Observations	384	384	107	107	277	277

* p<0.05, ** p<0.01, *** p<0.001: z-scores in parentheses

[1] Index uses state-specific coincident measures, scaled from 0-100 and increasing in growth.

Table 25: Linear Magnitude Analysis, Disbursement Mechanisms

Dependent Variable:	*ln(Pct. of Funds Disbursed | Economic Index>=40)* [1]					
	All States		*Low Stringency States*		*High Stringency States*	
	(1)	(2)	(3)	(4)	(5)	(6)
D(All Volatility-Based Accrual)	0.011	-	-0.003	-	0.02	-
standard error	*0.029*	-	*0.043*	-	*0.037*	-
D(Some Volatility-Based Accrual)	-	0.026	-	0.018	-	0.026
standard error	-	*0.033*	-	*0.050*	-	*0.042*
Time	-0.001	0.007	-0.001	-0.001	-0.002	-0.002
standard error	*0.001*	*0.002* **	*0.003*	*0.003*	*0.003*	*0.002*
% Democratic Legislature	-0.001	-0.001	0.002	0.002	0.001	0.001
standard error	*0.007*	*0.001*	*0.002*	*0.002*	*0.006*	*0.004*
D(Democratic Gov.)	-0.003	-0.002	0.041	0.044	-0.017	-0.014
standard error	*0.033*	*0.011*	*0.061*	*0.062*	*0.036*	*0.036*
D(Supermajority)	0.017	-0.001	-0.017	-0.016	0.011	0.011
standard error	*0.030*	*0.033*	*0.065*	*0.064*	*0.034*	*0.035*
Constant	-0.047	-0.056	-0.197	-0.212	-0.023	-0.026
standard error	*0.063*	*0.063*	*0.139*	*0.145*	*0.077*	*0.076*
R^2	0.002	0.003	0.009	0.01	0.004	0.004
Observations	422	422	130	130	292	292

* p<0.05, ** p<0.01, *** p<0.001: z-scores in parentheses

[1] Index uses state-specific coincident measures, scaled from 0-100 and increasing in growth.

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