2021

Three Essays in Regional Taxation

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Digital Object Identifier: https://doi.org/10.13023/etd.2021.298

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Three Essays in Regional Taxation

DISSERTATION

A dissertation submitted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in the College of Business and Economics at the University of Kentucky

By
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ABSTRACT OF DISSERTATION

Three Essays in Regional Taxation

This dissertation looks at the role that geographically limited taxation and the behavioral response of economic agents to different forms of regional taxation. While geographic taxation is typically thought to be confined to state and local taxes, federal tax policy can also create designated areas that receive preferential tax treatment. In this dissertation, both types of regional taxation are examined and are unified under common themes such as tax mobility and the definition of the tax base.

In chapter 2, I examine the important effect of how the location of high income earners employment is influenced by taxes in the state of employment. Previous literature has focused on the residency decision of high income earners, but that is not the only margin that matters, as in both the United States and the European Union taxes are instead due where income is earned. Using the universe of PGA tour participants from 1970-2018, I estimate the participation response to golfers with respect to a change in their effective tax rate. In the baseline specification, I find a tax participation elasticity of 0.32 and for those in the top 25 percent of prior year earnings, consistent with the superstar effect, find a larger elasticity of .82. I also find that federal taxes play a minimal role in the tax response of golfers and the primary effect is driven by changes in state taxes, suggesting that golfers reallocate their income away from low tax states towards higher tax states instead of a true labor supply response.

In Chapter 3, I look at an important distinction in tax policy, whether tax base changes are different than an equivalent change in the tax rate. This is in the context of a gradually narrowing consumption tax base that has largely left both physical and digital services out of the tax base. Using variation in food tax inclusion and the food tax rate combined with data from the Nielsen Corporation on the retail sale of food, I find that a one percent increase in the gross price of food decreases sales in border areas by less than one percent using a border pair identification strategy in both a static and dynamic setting. In addition I estimate the effect of removing food from the tax base using removals from West Virginia and South Carolina and find no additional effects beyond an equivalent rate reduction, suggesting among this very specific population, there is no meaningful difference between the two. This is largely suggestive that broadening the tax base would provide large gains in revenue with little loss in efficiency.

Chapter 4 explores whether place-based policies, which are geographically defined programs that provide special benefits to particularly poor neighborhoods lead to persistent effects once the programs expire. This question is important as the United States as well as other countries continue to invest in place-based policies without knowing the long run impacts. Using restricted access American Community Survey and the expiration of Renewal Communities relative to Empowerment

Zones, I find little evidence for a permanent improvement within Renewal Communities, with RC employment, income, and rent decreasing relative to the continuing Empowerment Zones. However, I do find that for those individuals who live and work in the zone, there is no subsequent decline in employment or income, suggesting that there is heterogeneity in persistence. Given that the newest iteration of place-based policies in the United States, Opportunity Zones, largely shares the same benefit structures as Renewal Communities it is unlikely that we will see lasting benefits from it's implementation.

KEYWORDS: Tax Policy, Tax Mobility, Superstars, Tax Base, Place-based Policies, Optimal Tax

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Date: July 29, 2021

Three Essays in Regional Taxation

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To Sarah and Laurence

ACKNOWLEDGMENTS

First, I would like to acknowledge my advisor, David R. Agrawal, who without his excellent guidance and advice I would not be half the economist I am today. I also would like to thank committee members James Ziliak, William Hoyt, and David Hulse along with the outside examiner Caroline Weber. I additionally want to thank James Ziliak and Chris Bollinger for providing me with the opportunity to be a research assistant with them, for which I am extremely grateful. I learned much from both of you. Additionally I would like to thank Nicholas Moellman, Cody Vaughn, and Zishen Ye for being great office mates over the past few years and to Chase Coleman for effectively being by virtual office mate during the COVID-19 epidemic. I have learned much from all of you over the years. Additionally I would like to thank my great colleagues in the Martin School Jawad Shah, Himawan Saputro, and Partomuan Juniult for valuable feedback and interesting conversations about tax policy and rules in different countries. I would also like to thank Sharokh Towfighi from my time as an undergraduate for inspiring me to pursue a PhD in economics, without your encouragement I am not sure I would have ended up applying for PhD programs. Finally I would like to thank my wife, Sarah, for all her support and encouragement over the last five years and dealing with many nights alone while I worked on various projects and for agreeing to move to the United Kingdom with me for my first job. I couldn't imagine a person who I would rather spend my life with.

Additionally, for chapter 2, I received valuable feedback from Dan Black, Chris Bollinger, Daniel Feenberg, Dirk Foremny, Benjamin Glass, Ulrich Glogowsky, Lucas Goodman, William Hoyt, David Hulse, Niels Johannesen, Clara Martínez-Toledano, Robert McClelland, Jakob Miethe, Sebastian Siegloch, Kirk Stark, Cullen Wallace, Caroline Weber, Quinton White, David Wildasin, James Ziliak, and Eric Zwick as well as conference/seminar participants at the National Tax Association Annual Conference, Southern Economic Association, Symposium of Public Economics at Osaka University, University of Exeter, University of Kentucky, the U.S. Economic Research Service, and the U.S Department of Treasury Office of Tax Analysis. For chapter 3, I received valuable feedback from David Agrawal, William Hoyt, David Hulse, Beau Sauley, and James Ziliak. Finally for chapter 4, I received meaningful input from David Agrawal, Jesse Gregory, William Hoyt, David Hulse, Caroline Weber, Owen Zidar, and James Ziliak and invaluable help from Census Administrators Jacob Cronin, Bryce Hannibal, and Charles Hokayem.

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[1]Calculated based on data from The Nielsen Company (US), LLC and marketing databases provided by the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are those of the researchers and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

[2]Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2376. (CBDRB-FY21-P2376-R9059)

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Chapter 1 Introduction

Regional taxation is a broad term that refers to taxes that are limited to a geographically defined area. In the United States, due to the large role of decentralized government, this primarily takes the form of state and local taxes to finance subnational public goods and in some cases limited forms of redistribution. When thinking about the optimal tax policy for state or local government, many behavioral responses of individuals, firms, and even other governments need to be taken into account when determining which policies will maximize welfare. For example, if there isa increase in the income tax rate for a given state, this may cause some more mobile individuals to move out of the state to a state with a lower tax rate, it also may make moving into the state less attractive and may make it harder for businesses to attract a talented workforce, and it also could cause local governments both in the state and in neighboring states to change their tax code to offset the externality from the state government. Better understanding the behavioral responses from subnational tax policy helps better design the tax system to maximize both welfare and revenue.

Regional taxation isn't limited to subnational governments as national governments have often created special economic areas, typically referred to as place-based policies, that are limited in geography. For example, Mexico has a special value added tax rate and income tax rate near the United States border and China has special economic zones where businesses receive targeted tax credits and differential business and trade rules. Much like subnational taxation, there are important questions regarding benefits and efficiency of place-based policies, especially regarding whether they create new activity or simply shift businesses and employees from outside areas.

The following chapters will address both state and local regional taxation and national programs that create geographically targeted tax incentives. While subnational taxation and federally created place-based policies differ in many respects there are common issues, such as taxpayer mobility and alterations of the tax base,that are of fundamental importance which unifies the different chapters together.

The literature on tax induced mobility has largely focused on the physical location of economic agents, such as firms or individuals. While the physical location is an important factor in the determinants of tax liability, it is importantly not the only factor that matters. For example, in a state that has a sales only corporate tax apportionment a firm could have a physical presence in a state, have zero sales in the state, and have no state tax liability besides property taxes. This extreme example highlights that it is not just the physical location of the agents that matters, but the economic activity that creates the tax liability. This dissertation uses a wide breadth of geographic information to properly determine both the location of agents, as well as the location of activity, to properly identify the tax treatment and movement of earnings, employment, and other economic outcomes.

In a related area, the reallocation of economic activity across a geographic or characteristic boundary to a tax preferred side is often thought to be welfare reducing as it primarily consists of tax avoidance. This is clearly the case in some instances,such as cross border shopping or shifting income in response to a statutory tax change,but this need not be the case. For example, if the reallocation of business activity towards the tax preferred side creates agglomeration economies, where firms are more productive due to the presence of other firms nearby, this could be welfare improving,as it subsidies the positive externality that adjacent firms create. Agglomeration economies can be present for even small geographic areas for certain industries, such as retail, so even small geographically targeted areas can lead to potential benefits.In Chapter 4, I investigate whether programs that cause the reallocation of economic activity ultimately lead to long run agglomeration effects once the programs expire.

A final unifying theme in this dissertation highlights components of the tax base and how the tax base is defined are important for regional taxation. States and national governments define their personal income tax base to include all income earned in the state, whether by residents or nonresidents, outside a handful of reciprocity agreements. State and local governments also have substantial flexibility in choosing what to include in their retail sales tax base and those decisions have important consequences for consumer and producer behavior and revenue. Finally, creating tax free investments in low income areas can be viewed as a way of narrowing the tax base to address concerns about equity and economic development. Subsequent chapters will address each of these issues thoroughly and why these issues might matter economically.

Chapter 2 focuses on how employment decisions of high income earners are influenced by taxes in not just the state of residence but the state of employment.When highly mobile individuals, such as top athletes or consultants, are able to freely choose where to both live and work the decision on where to work depends upon the prevailing tax rate in the residence state and the employment state. Because dataon employment location and residence is incredibly limited, I use data on PGA Tour golfers from 1970-2018 to estimate the effect of changes in their expected tax liability on their participation decision. I find robust evidence that golfers do respond to taxes estimating a baseline elasticity of 0.32 for the whole population of golfers. For those with higher abilities, consistent with the superstar effect, I find a stronger effect of taxes, with those at the top 25 percent of prior year income having an elasticity of 0.82. Consistent with a reallocation of labor across taxing jurisdictions, I also find that golfers respond most strongly state taxes and the effect is concentrated among golfers in low tax states, suggesting that golfers are reallocating their labor towards low tax states across taxing jurisdictions.

The results in chapter 2 suggest that the behavioral effect of taxes on nonresidents is important when thinking about optimal tax policy. Not accounting for the response of nonresident earners can lead to bias estimates in the elasticity of taxable income and when nonresident income is large, perhaps looking specifically at an entertainer tax or telecommuting tax, can underestimate the ETI in response to a tax change. Thus states should think about how their residents and nonresidents respond, and respond separately, to any changes in income tax rates.

Chapter 3 examines an important distinction in the tax literature, whether tax rate changes are treated differently than changes in the tax base. The setting for this chapter is with the retail sales tax utilized by state governments in the United States which has gradually narrowed over time due to technological change and shifting consumption patters. Using a common exemption from the tax base in food and comprehensive data from the Retail Scanner Data Set from the Nielsen Corporation and a border pair strategy. I find that a 1 percent increase in the gross tax rate of food decreases sales in border areas by less than 1 percent in both a static and dynamic setting. In addition, I find no distinct effect of the removal of food from the tax base beyond what a rate reduction would suggest thus suggesting minimal distinction between the two.

The results here strongly suggest that tax base exemptions in the consumption tax are costly. While the consumption tax base has gradually narrowed over time,the rate has subsequently increased. The future of tax policy in the retail sales tax should focus on expanding the tax base to include both physical and online services instead of further rate increases. While food has some substantial equity concerns regarding its taxation, other exempt categories are less regressive and would similarly cause small efficiency loss for large revenue gains.

Chapter 4 studies a novel response to the widespread use of place-based policies in the last 30 years of United States local economic development programs in how they persist once the benefits expire. While these programs, and specifically Empowerment Zones, have substantial and measurable benefits for zone residents in terms of employment outcomes in earnings, little is known about whether these benefits persist once the programs expire. I utilize the expiration of the Renewal Communities, similarly designed to Empowerment Zones, relative to the Empowerment Zone program in 2009 to estimate the effect of the repeal on individual and tract level outcomes using a difference and difference approach. I ultimately find that the repeal leads to a statistically significant decrease in employment, earnings, and rent for residents of Renewal Communities and an overall fall in employment within Renewal Communities. While one of the primary benefits of both Renewal Communities and Empowerment Zones is a employment credit for residents in the area to employers in the area, I find no evidence of a decline among workers who live and work in the area suggesting that there is heterogeneity in persistence for different groups.

These results provide evidence on the short to medium run impacts of ending a place-based policy. Understanding precisely how program benefits map into long rum improvements help policymakers better understand how to design more beneficial programs and understand the long run effects of existing programs like Opportunity Zones. It also provides some evidence into whether or not targeted tax incentives can lead to long run agglomeration economies, with some inconclusive results.

Chapter 2 The Effect of Taxes on Where Superstars Work

Taxpayers around the world are increasingly "globalized." Due to economic integration across jurisdictions, individuals now—more than ever—earn income from foreign capital and derive labor income from nonresident jurisdictions. However, the dramatic rise of foreign sourced income, whether derived from offshore capital or from inter-jurisdictional labor supply, poses new challenges for governments seeking to collect tax revenue. In particular, the globalized nature of taxpayers limits the effectiveness of information reporting, providing new and creative ways to avoid taxes.

Fiscal systems around the world raise a disproportionate share of tax revenue from superstars (Scheuer and Werning, 2016; Scheuer and Slemrod, 2020) and as a result, many jurisdictions have proposed tax reforms aimed at raising more revenue from these individuals. But, effectively taxing superstars raises important policy challenges. A critical feature of high-income earners—such as consultants, entertainers, athletes, rental property owners, business owners and the self-employed—is that they often have employment contracts in many different states earning *nonresident* income, which is taxable by the state of employment.[1] Thus, they may live and work in different locations.

States raise 7.5% of personal income tax revenue from nonresidents, with nonresident employees contributing as much as 15% of revenue. In New York, this revenue represents more than six times both cigarette tax revenue and estate tax revenue. Due to the importance of superstars for fiscal systems, even states with little or no cross-border commuters, such as Hawaii, raise 7% of revenue from nonresident income taxes. Even after removing nonresident commuters, states collect over 13 billion dollars or

[1] Against the recent rise in pass-through income (Smith et al. 2019), many sole-proprietors, partnerships, or S-corporation owners earn income in multiple states.

4% of revenue from nonresidents,[2] likely from superstars able to work in non-adjacent states.

Following the COVID-19 pandemic, taxation of nonresident income will likely rise in importance. As many companies allow their employees to telecommute, companies are less confined to hiring individuals within a short commute. Telecommuting will further break any link between the place of residence and place of work. Thus, nonresident income will become increasingly important, just like foreign-sourced income has increasingly posed new challenges to the international tax system.

As taxes in the U.S. are predominantly employment-based, taxes are due to where the income is *earned*.[3] For an individual with employment opportunities in other states, moving residences will reduce tax liabilities *only if* the initial state of residence tax rate is higher than in the employment state. Therefore, even *without* changing her state of residence, high taxes in the nonresident state may deter the individual from accepting an employment contract in that state. Thus, at the margin, taxes should matter for the location of where to *work* as well. Although a large literature studies the mobility of residences across jurisdictions (Kleven et al., 2020),[4] empirical work on the effect of taxation on the location of employment and taxation of nonresidents is virtually nonexistent. Yet, both elasticities matter to measure the efficiency costs of taxation. We present a conceptual model that highlights why both the elasticities of residence versus employment matter and conditions under which the employment mobility responses are not captured in existing empirical estimates of the elasticity of taxable income.

[2]This is more than twice as much revenue that is raised from taxes on alcohol, approximately equal to revenue from tobacco products, and approximately 1/3 of state corporate tax revenue.

[3]Internationally, taxation of nonresidents are governed by bilateral tax treaties. Nonresident workers may be taxed in the country of residence or employment.

[4]See Agersnap et al. (2020); Agrawal and Foremny (2019); Akcigit et al. (2016); Bakija and Slemrod (2004); Gordon and Cullen (2012); Kleven et al. (2013); Moretti and Wilson (2017); Muñoz (2020); Schmidheiny and Slotwinski (2018); Young and Varner (2011); Young et al. (2016); Lehmann et al. (2014); Kleven et al. (2014); Milligan and Smart (2019).

We provide previously elusive evidence on the effect of nonresident taxes on the location of employment. Studying the effect of taxes on the location of where income is earned has been hindered by a lack of data. Most publicly available datasets do not contain contractual information on the location of work for *all* contracts. Moreover, Internal Revenue Service (IRS) Form 1040 data aggregates all contracts and income. The use of IRS data would require obtaining all W-2/1099 forms for each contract to determine the employment location. These administrative data do not contain the full menu of choices that the high-income individual chooses from; in other words, the researcher does not necessarily know the states from which employment contracts were declined.

Just as Kleven et al. (2013) inspired a literature on international residential mobility using the example of football players, and much of the following literature has focused on nice sectors, we make progress on studying the effect of taxes on the location of employment by focusing on the specific labor market for professional golfers. First, extensive data on golfers is publicly available, including: their residential decision, their "ability" levels, the prizes they win, and most critically, the decision of which states they do/don't play. Critical to our analysis, we know the full slate of options facing professional golfers: we see both the tournaments that they play in *and* we know the tournaments that they decline. Using publicly available sources, we scrape data on the careers of all professional golfers from 1970 to the present. We then exploit state-level tax rate changes to identify the effect of taxes on employment location decisions. Although focusing on a specific occupation, we show to the best of our ability, that demand-side pass-through plays a limited role, allowing for possible generalizations of our estimates and methods.

Furthermore, tax rules on golfers generalize to most other occupations. As golfers are self-employed, the rules applying to them are consistent with most occupations: prize earnings are taxed in the state where the tournament is located and additional taxes on these earnings are only due to the residential state if it is higher-tax. Indeed, *most* golfers live in zero-tax or low-tax states so that the only tax rate relevant for their weekly labor supply, is the tax rate in the state of employment.[5] These taxing rules are not just a U.S. curiosity – in the European Union income earned by athletes can be taxed in the country where the money was earned – whether or not the athlete has a fixed base in the country is irrelevant. We exploit this unique institutional feature to identify the effect of taxes on employment location decisions and the resulting high-frequency labor supply of athletes. The decision where to work each week is the player's choice and data on the *weekly* employment decisions is readily available. Top golfers also face state top marginal tax rates that may vary between zero and thirteen percentage points.

The fact that golfers make labor supply decisions on a weekly basis also provides a unique opportunity to contribute to the labor supply literature (Blundell and MaCurdy, 1999; Keane, 2011). Taxes provide an exogenous source of variation that allow us to estimate the *high-frequency* labor supply elasticity – the decision to work or not on a weekly basis.[6] Such an elasticity may be different than standard labor supply estimates due to frictions from labor supply contracts, but is increasingly relevant in the gig economy, where platforms (Uber, Instacart) allow workers[7] to make daily decisions on when and how much to work. The lack of empirical evidence on high-frequency labor supply represents an important gap in the literature given recent technologies that give workers more flexibility on when/where to work.

[5]Although most golfers reside in low-tax states, some golfers (e.g., Phil Mickelson) reside in high tax states. This provides a unique opportunity to identify the effect of taxes. For golfers residing in low-tax states, the employment decisions will be made based on the tax rate in the employment state. For golfers residing in high-tax states, taxes in the state of employment will not matter, and these golfers will make their decisions based on the residence rate.

[6]Similar to Oettinger (1999), Farber (2005), Thakral and Tô (2020) and Chen et al. (2019b), except with higher income earners

[7]Uber drivers supply elasticities are very elastic, with a median elasticity of 1.92 (Chen et al., 2019b)

Given the set of tournaments is fixed, we know the full set of work alternatives facing each golfer. However, golfers do not earn salary income, but rather earn prize money contingent on their weekly performance. Given golfers decide to participate based on their *expected* earnings, we combine this comprehensive data on weekly labor supply, with various measures of expected earnings for participants and nonparticipants based on quality indexes. We then use a grouping estimator to circumvent endogeneity concerns and we simulate participation tax rates for each tournament. The model is identified by year over year changes within golfer and within tournament. We flexibly control for age to account for participation preferences of young versus old golfers. Any threat to identification arises from contemporaneous unobservable changes that are correlated with state tax rates and the participation decisions of golfers within an age cohort.

A one percent increase in the net-of-participation tax rate (Immervoll et al., 2007) raises the baseline participation rate in a tournament by 0.135 percentage points. The average net-of-participation tax rate is approximately 62%, so a one percent change is a 0.62 percentage point change. Given the average participation rate is around 43%, this implies an extensive margin elasticity of approximately 0.32. However, those at higher levels of earnings having elasticities closer to unity. This elasticity differs from standard extensive labor supply elasticities because it is affected by spatial variation and differs from residential migration elasticities because the decision to change employment states can be made at a high-frequency. Nonetheless, this elasticity is smaller than the residential response of football players in Kleven et al. (2013) and many other tax-induced migration studies, perhaps due to the fact that the residential response is an all-or-nothing decision or because many golfers need to qualify for elite tournaments. But, it is larger than the standard intensive margin labor supply response of Moffitt and Wilhelm (2000). While many things matter, at the margin, taxes appear to be an important factor.

Relating our elasticities back to the literature on superstar effects, we document realized earnings is convex in player ability. Second, what the superstar models predicts is an increasing *earnings* elasticity (Scheuer and Werning, 2016), but the results in the prior paragraph suggest that the participation elasticity is increasing in ability. We show that higher earners have a higher participation elasticity, but this likely underestimates the rise in the earnings elasticity due to the superstar phenomenon because the tournaments that these high earners typically participate in come with higher prizes. We present evidence that this is the case and and thus our participation elasticities likely underestimate the effect of superstar phenomenon on increasing earning elasticities.

Returning to the policy debate, high-income earners in tax employment-based federations such as the U.S. and supranational institutions such as the E.U., have potentially two margins of response: the conventional relocation of residence and employment responses that shift jobs to low-tax states. Although often assumed to be the same, many high-income professionals make *separate* decisions on each of these margins. Thus, studies that find no migration responses when looking at residential locations may miss important mobility changes in employment. High responses on both margins may limit the ability of states to engage in progressive redistribution. Finally, the relative elasticity of employment and residence determine whether it is preferable to tax income under the residential principle or the source principle, a topic that is increasingly relevant as teleworking allows workers to optimize their place of employment and place of work separately.

2.1 Institutional Details

2.1.1 The PGA Tour

The Professional Golf Association (PGA) runs U.S.-based golf tournaments. The PGA Tour consists of two different tournament eligibility rules: most events are “open” tournaments that all PGA Tour members are eligible to participate in and a handful of “invitationals” that can limit participation. The PGA Tour is run concurrently with a lower-level tournaments for golfers that have not qualified for the PGA Tour. In order to consistently participate in the PGA Tour, one has to earn a PGA Tour card. Since 2013, these cards are automatically awarded to the top 125 points finishers from the previous year and any tournament winners.[8]

Each tournament has a total “purse” that golfers earn income from. Halfway through each four-day tournament, golfers with the worst scores – and thus significantly far away from winning – are “cut”. While the number of cut golfers varies, these approximately fifty percent of players earn no income in the tournament. The remainder of the golfers that are not cut, all earn income that is monotonically increasing in their position on the leaderboard. The prizes for winners and golfers at the very top of the leaderboard are much higher than for lower-placing golfers. Prizes at the bottom of the leaderboard may be a few thousand dollars, while prizes for winning can be several million dollars. Thus, golfers face income risk.

In our sample, the average number of PGA Tour golf tournaments per year is 34 with the average golfer participating in roughly 40 percent of tournaments. Golfers may choose not to participate in a tournament for a variety of reasons, including rest, overlap with a prominent European tour event, injury, personal course preferences, purse sizes, or state taxes. While there are reason to not participate in a tournament, there are also ample reasons why one may choose to participate when they would otherwise prefer not to, such as sponsorship requirements, Ryder Cup points, and being on the margin for earning a tour card.

[8] A more detailed description of PGA Tour rules can be found in Appendix A.1.

2.1.2 Tax Setting

Golfers are considered self-employed individuals[9] and have *always* been liable for the paying taxes on prizes to the state of employment and, if applicable, to their state of residence. Unlike prize income that has a clear location of employment, depending on the nature of the contract, endorsement income is apportioned by either the fraction of working days in a state, by the fraction of labor income earned in that state, or where the endorsement activity takes place (Donley, 1997). Endorsement income is not unobserved, but golfers will sign endorsement contracts to apportion as much as possible to their state of residence, which is usually low-tax, in order to minimize tax burdens. Our empirical model assumes that all endorsement income is apportioned to the state of residence. Given a golfers' prize income is taxed in the state of employment, each golfer must file an income tax in every state they participate in, whether or not they have positive earnings.

In order to avoid double-taxation, taxes paid in nonresident states are generally credited on resident state tax returns.[10] Additional taxes on earnings are only due to the resident state if it levies a higher tax rate. Thus, the *maximum* tax rate between the employment and resident states determines the effective tax rate faced by the athlete. Most golfers, with the notable exception of Phil Mickelson, live in zero-tax or low-tax states (Driessen and Sheffrin, 2017). Golfers will need to file nonresident tax returns in many states. For example, Patrick Rodgers, a typical golfer, competed in 29 events across 17 states plus international tournaments in 2016 (Finch, 2017). Golfers are allowed to deduct expenses, which are unobservable, from their tax returns. Generally, expenses such as air travel or payments to caddies,

[9] Golfers could also incorporate themselves and hire themselves as an employee, but this is not common.

[10] One notable exception is Illinois, which has special rules for athletes, that prohibit golfers from receiving a credit against nonresident income taxes.

are allocated to the tournament state as they are directly related to the “cost of business” in the state. This also means that golfers must file taxes in the state of every tournament they participate in regardless of tournament earnings. Some states have reciprocity agreements (Agrawal and Hoyt, 2018) in which the state of employment agrees to not claim the income of workers from their resident state. However, reciprocity agreements only apply to income earned from an employer, not self employed income, and therefore are of little concern in this context.

States also differ in how they apportion nonresident income, with two specific methods of calculating income tax liability. The first method works as expected, where nonresidents are taxed based upon income earned in the state. For example, if an individual living in zero-tax Florida earns \$100,000 in Alabama, his income tax liability would be based upon the \$100,000 dollars earned (and taxed) in Alabama. In this case, earnings in other states are irrelevant. The second method is based upon the fraction of income earned in the state relative to total federal income. In this case, if an individual lives in Florida and earns \$300,000 in Florida and \$100,000 in New Jersey, his total tax liability in New Jersey would be 25 percent of the taxes that would be owed if all \$400,000 were taxed in New Jersey. In the presence of progressive taxation, the tax liability is higher in the latter apportionment method. To see this, let the tax schedule in state s be given by $T_s(y_s)$. Then, taxes due to Alabama are $T_A(100,000)$ and taxes due to New Jersey are $0.25T_N(400,000)$. If the tax schedules in Alabama and New Jersey are progressive and identical, then $T_A(100,000) < 0.25T_N(400,000)$.

While it is important to discuss details specifically relating to the tax situation of golfers to understand the tax landscape they face, the policy relevance is with non-athletes who are able to choose where to work. With minor exceptions, the rules discussed above (sourcing of income to the employment state, credits, expense deductions, and apportionment) apply to self-employed individuals with income from multiple states, cross-border commuters, individuals with rental income in multiple states, and even professors with consulting income from multiple states. Individuals affected by these rules also include other higher income populations such as entertainers, artists, and consultants, but also lower to middle income occupations in multistate MSAs (Chicago, New York City, etc.) who may face similar high frequency decisions. Therefore, for most individuals, the tax rules are in line with golfers, which strengthens our decision to focus on golfers, where the full menu of employment choices is known to us.

The rules discussed above are not just a curiosity of the U.S. tax system. Although international taxing rules depend on bilateral tax treaties, these treaties within the European Union are often quite similar. Thus, as a general rule athletes in the European Union are taxed based on where the athletic event takes place. More generally, in the European Union, income related to real estate is usually taxed in the source country and for consulting work, it often depends similar to the athletes case based on where the work is performed.

2.2 Conceptual Framework

In this section, we highlight the conceptual issues of how the employment location elasticity differs from the residence elasticity. As telecommuting fundamentally changes the ability of individuals to optimize employment and residence separately, the conceptual differences in the elasticities will become even more important following COVID-19.

To highlight the differences, consider how nonresident and resident income influence state income tax revenue. State personal income tax revenue in the employment state can defined as

$$R(\tau) = \tau N_R z_R + \tau N_{NR} z_{NR} \tag{2.1}$$

where $N_g,\ g \in \{R, NR\}$ is the number of residents (R) working in the state and the number of nonresidents working in the state (NR). Let z_g be the taxable income of the average resident and the average nonresident, respectively. Consider a single bracket tax system where τ denotes the tax rate paid to the state of employment. The analysis could be extended to a progressive tax $T(z_g)$, but the flat tax allows us to focus on the key conceptual differences in elasticities.[11]

Define the taxable income elasticities as $\eta_g = \frac{(1-\tau)\, dz_g}{d(1-\tau)\, z_g}$ and define the mobility elasticities as $\epsilon_g = \frac{(1-\tau)\, dN_g}{d(1-\tau)\, N_g}$. The elasticity of taxable income (ETI) captures how individuals adjust their taxable income via labor supply (hours) responses and other avoidance strategies. The mobility elasticities capture the extensive margin response of where to live (for residents) or work (for nonresidents). As we will discuss subsequently, the ETI is usually estimated by excluding individuals who change states, which implies that the intensive margin response will not then capture the shift of employment/residence from one state to another. Then, we totally differentiate R to obtain:

$$dR = \frac{R}{\tau} d\tau - \eta_R \frac{\tau}{1-\tau} z_R N_R d\tau - \eta_{NR} \frac{\tau}{1-\tau} z_{NR} N_{NR} d\tau - \epsilon_R \frac{\tau}{1-\tau} z_R N_R d\tau - \epsilon_{NR} \frac{\tau}{1-\tau} z_{NR} N_{NR} d\tau \tag{2.2}$$

We can further manipulate this expression by defining s_g as the share of state income earned by group g. For nonresidents, this is $s_{NR} = N_{NR} z_{NR} / (z_{NR} N_{NR} + z_R N_R)$, and we also have $s_R = 1 - s_{NR}$. Finally, let the percent change of any variable $x$be defined by $\hat{x} = \frac{dx}{x}$. Then dividing (2.2) by total tax revenue, R, yields:

$$\widehat{R} = \widehat{\tau} + [\eta_R(1 - s_{NR}) + \eta_{NR} s_{NR}]\, \widehat{1-\tau} + [\epsilon_R(1 - s_{NR}) + \epsilon_{NR} s_{NR}]\, \widehat{1-\tau}. \tag{2.3}$$

[11] Alternatively, we could define z_g as the revenue raised in the top tax bracket. Then, if the tax rate was progressive and given we allow for income differences across residents and nonresidents, our analysis can easily be interpreted as the effect of the top marginal tax rate on the tax revenue raised from the top tax bracket. For superstars, the revenue raised in the top bracket, makes the marginal tax rate a reasonable approximation for the average tax rate (Kleven et al., 2013).

This equation shows the percent change in tax revenue as a result of a small perturbation in the tax rate. The change in tax revenue can be decomposed into several parts. The first term says that revenues increase proportionally to the percent increase in taxes. This is the standard "mechanical effect" of higher taxes, ignoring behavioral responses. The second term is the percent change in tax revenue resulting from behavioral responses to taxable income. This term is the weighted average of taxable income elasticities across residents and nonresidents. Note that for a tax increase, $\widehat{1-\tau}$ is negative, so this terms lowers tax revenues. Finally, the third term is the decline in tax revenue resulting from a shifting of residential and employment locations. This term is a weighted average of the residential relocation, ϵ_R, and the employment relocation, ϵ_{NR}, responses.

Critically, notice that if the researcher has access to administrative tax data on taxable income of both residents and non residents from a single jurisdiction (state), then the researcher can estimate a single taxable income elasticity – using a balanced sample of taxpayers – where $\eta = \frac{(1-\tau)\,dz}{d(1-\tau)\,z}$ where z is the weighted average of income across residents and nonresidents that appear in the data in both periods. Simplifying yields:

$$\widehat{R} = \widehat{\tau} + [\eta + \epsilon_R(1 - s_{NR}) + \epsilon_{NR}s_{NR}]\,\widehat{1-\tau}. \tag{2.4}$$

However, using these administrative data, the researcher cannot include individuals that only appear in the dataset for one period to estimate the taxable income response. Furthermore, the researcher cannot estimate participation decisions unless they know why the taxpayer is leaving the data. In other words, the researcher would need to know if the individual appeared in the data for only one period because of a relocation of residence/job, because of a death, or just because of losing contact with the tax administration system due to nonfiling.

Then, researchers could assume ϵ_R based on current mobility estimates in the literature derived from-multi-jurisdictional administrative data. But, we still do not know ϵ_{NR}. Critically, notice that one cannot argue that someone who stops working in the nonresident state will be captured in the taxable income response because many studies drop individuals with multi-state income to avoid complex changes in taxes discussed in this paper. This would not be a problem if one could assume $\epsilon_{NR} = \epsilon_R$, but given home biases, costs of changing jobs not equaling the cost of moving, this is unlikely to be true. Thus, our conceptual model suggests the critical need to estimate ϵ_{NR} when nonresident income is an important source of state tax revenue.

Finally, data indicates that the share of state tax revenue raised nonresident income, s_{NR}, is important. In Table 2.1 we show the fraction of income tax revenue from nonresidents. To show that this share is not driven by workings living in cross-border urban areas, we also report the fraction of income tax revenue from nonresidents adjusted after removing cross-border commuters. In order to make this adjustment, we calculate the fraction of total income earned by cross border commuters relative to all income earned in the state from the 2018 American Community Survey. Assuming the share of income is similar to the share of taxes, we then subtract the fraction the cross border commuter share of total income from the share of nonresident income taxes in order to isolate the fraction of tax revenue from nonresidents who are not commuters. This adjusted estimate ranges from about 1% to 7%, or about 4% of revenue across all states. These numbers are not small: they represents around half of all nonresident income tax collections and (extrapolating to include states not in our data) around 13 billion dollars in total state income tax revenue.

2.3 Data

2.3.1 Data Sources

We scrape data on the universe of U.S.-based PGA Tour sponsored golf tournaments between 1970-2018 from online sources, mainly golfstats.com. Tournament-specific data includes information about golfer participation, tournament prizes, placements, round-by-round scoring, and golfer nationality. The tournament information also contains the location and day of the final round. While this provides a detailed snapshot of an individual golfer's performance in a given tournament, in order to properly calculate the participation tax rates, additional golfer information is needed on residence. Fortunately, golfstats.com also contains biographical information that includes golfer residence, birthday, birthplace, and a career biography.[12] The data then allows us to observe golfers across time, including both the state of residence and the state where income is earned, giving us the necessary information to determine taxes.[13]

In order to properly asses the tax burden, the sample is restricted to U.S. nationals. This is done to limit issues with complex international tax treaties and participation with European tours. Golfers also must participate in at least one tournament in a given year to be counted in the panel for that year, so if a golfer is injured or does not play in 2005 but participates in some tournaments in 2004 and 2006, they are unobserved for 2005 and observed for 2004 and 2006. Thus our unit of observation is golfer×tournament×year when the golfer participates in any tournament.

If golfers disproportionately live in high-tax states, the employment-state tax rate would never matter. Some individual sports have large agglomeration effects on residence (Driessen and Sheffrin, 2017) which could limit spatial variation in taxes. The distribution of the location of golfers/tournaments is provided in Figure A.2. Overall, the concentration of golfer residence is in warm, sunny, and *low-tax* places

[12]Residence is observed only once at the time of scraping. Thus, we assume the golfer lives in the same state throughout his professional career. Given most golfers live in low-tax states, this is not a major concern. The results are robust to using current residence, or at the opposite extreme, birthplace.

[13]An overview of the data available is in Figure A.1.

such as Florida, Tennessee, and Texas, which implies the employment tax rate will be especially salient for golfers. There are still a few higher tax states (California, Arizona, North Carolina) that have some golfer populations, but on average, golfers in these states are lower quality golfers. Furthermore, PGA Tour tournaments are spread across 29 states during the sample period, giving substantial variation in the tax rates in the employment states.

To justify the focus on golfers and not relocation of tournaments, note that tournaments rarely change locations. In our sample of thousands of tournament-year observations, tournaments change locations roughly 25 times (excluding the one tournament that moves each year). Moreover, due to rigid pressures from tournament venues, these changes are uncorrelated with taxes: the average tax difference between new and old state is –0.00008 percentage points. All results are robust to dropping these tournaments.

2.3.2 Data Construction: Taxes and Abilities

Although golfers face the same tax rules as most other occupations, a difference from other occupations is that golfers make decisions on the basis of their expected income rather than observed wages. As in Kleven et al. (2013), expected income for each tournament is created by constructing a quality index (ability measure) and then applying a grouping estimator (Blundell et al., 1998), which is a cell average of earnings in a group of tournament×year×quality. We follow the data driven process of Lubotsky and Wittenberg (2006) to construct the quality index.[14] The quality index is constructed for each year using golfer age, the previous year's placements and earnings, and the prior year's number of tournaments. Although using exogenous and lagged characteristics, as shown in Figure A.3, the index is strongly correlated

[14] A more detailed explanation of the Lubotsky and Wittenberg (2006) index is in Appendix A.2.

with current-year performance. Then for each year, we group golfers by deciles of quality and assign the mean value of earnings in a tournament for the golfers in that decile who participate. This directly leads to a grouping estimator of event earnings and taxes which should abstract away from concerns regarding individual-specific estimates of earnings and tax rates. Additionally, the appendix provides estimation using a similar grouping method but instead of the index, using predicted values of earnings based on similar characteristics. The results are robust.

Next, the decision to work in a state is not based on an average tax rate, but rater on the taxed paid on the additional earnings from participation. In order to better model the tax decision to participate at a high-frequency, we adapt the participation tax rate (PTR) used in Immervoll et al. (2007) and Laroque (2005). The PTR is a measure of the average tax rate from participating in the labor market, holding all other income constant. In our situation, the PTR corresponds to a golfer choosing to participate or not in a particular tournament. To do this, we construct total expected income $\mathrm{I}_{\mathrm{ity}}$ for individual i in tournament t in year y, conditional on participating ($P_{ity} = 1$) or not ($P_{ity} = 0$) in a particular tournament. The latter of these incomes is the sum of tournament-specific expected earnings from all *other* tournaments the golfer participates in excluding tournament t; while the former is the sum of all expected earnings including tournament t.[15] Then, the PTR is:

$$\mathrm{PTR}_{isty} = \frac{\mathrm{T}_{\mathrm{sy}}[\mathbb{E}(\mathrm{I}_{\mathrm{ity}}|P_{ity} = 1)] - \mathrm{T}_{\mathrm{sy}}[\mathbb{E}(\mathrm{I}_{\mathrm{ity}}|\mathrm{P}_{\mathrm{ity}} = 0)]}{\mathbb{E}(\mathrm{I}_{\mathrm{ity}}|P_{ity} = 1) - \mathbb{E}(\mathrm{I}_{\mathrm{ity}}|P_{ity} = 0)} \tag{2.5}$$

where T_{sy} represents the tax function of state s. The relevant tax function is either the state of residence or the state of employment, whichever rate is higher. Unlike other high-income earners that know their income from participating, golfers have

[15] Kleven et al. (2013) estimate ability and then use group averages to construct tax rates. Because, as in Kleven et al. (2013), the group average is not estimated, this does not generate uncertainty that needs to be corrected in standard errors.[16] See appendix A.3 for details

uncertainty over their earnings from participating. Then, $\mathbb{E}(\mathrm{I}_{\mathrm{ity}}|P_{ity} = 1)$ represents expected total income from individual i participating in tournament t, which is the mean value of earnings from the golfer's quality decile estimated in the prior step. The expectation is constructed based upon their *expected* earnings from all the tournaments the golfer has decides to participate in, allowing participation in only the tournament t to vary.[17] Then, $\mathbb{E}(\mathrm{I}_{\mathrm{ity}}|P_{ity} = 0)$ represents expected income if the golfer i chooses not to participate in tournament t, but holds all other tournament participation fixed. This makes the denominator the expected winnings from the tournament. In this way, the participation tax rate corresponds to the difference in taxes from participating versus not participating, relative to the expected increase in income from participating in a given tournament. This implicitly means that all other decisions in regards to tournament participation are held constant, including both past and future tournaments in a given year. Appendix A.3 provides specific examples.

Taxes are estimated using NBER TAXSIM (Feenberg and Coutts, 1993) for the home state and the state that the tournament is held, accounting for state differences in the apportionment of nonresident income. Given that TAXSIM does not currently provide a way estimate taxes for nonresident income, we construct the appropriate apportionment and effective tax rates by hand. This requires us to account for the taxes owed on earnings in the resident state from a given tournament, the earnings in the state of the tournament, and the specific apportionment structure of the state explained previously. A thorough explanation of the work required is in Appendix A.3.

[17]To construct the PTR, we assume golfers decide the set of tournaments they will participate in at the start of the year. Although a debatable assumption, we will provide empirical evidence consistent with this assumption.

2.4 Preliminary Evidence on Superstar Phenomenon

As shown theoretically in Scheuer and Werning (2016), in the presence of superstar effects, earnings are convex in ability. Intuitively, one explanation is that higher ability individuals get matched to better firms, which then leads to the convexity of the earnings schedule. Critically, superstar phenomenon then increases earnings elasticities relative to measures that omit superstar effects. As initial evidence on the convexity of earnings for golfers, we present some descriptive statistics of the earnings distribution.

Figure 2.1 shows the dollar value of prizes offered for each golfer placement in a tournament. We show a lower quality tournament such as the Sony Open, a medium quality tournament such as the Travelers Championship, and an elite tournament such as the Players Championship. The final figure shows an average across all tournament. If a golfer is cut, they receive no prize, and so placements below (approximately) eighty have a tournament earnings of zero. As can be seen, the prize distribution is extremely convex in golfer rank and this convexity has increased over time.

Is there a clear relationship between prizes and tournament quality tiers? The highest prize tournaments are often the most well-regarded tournaments, and so we use the size of the total purse to rank tournaments.[18] Comparing Panel (a) for the total purse and Panel (b) for the top prize, indicates ranking of tournaments by purse size maps almost one-to-one of ranking by the top prize. There are a few exceptions in the tails, but generally speaking, the relationship between the prizes in these quality tiers is linear– with a relatively small slope parameter—and not convex. Although the increase in prizes by tournament quality is (generally) linear, this increase in prizes may amplify superstar effects if the tournaments that high ability golfers typically

[18]Other factors such as the time in the schedule and the venue may matter. Survey evidence by golfers ranking tournaments indicate the golfer rankings almost perfectly match the size of the purse.

participate in come with much higher prizes. Figure 2.3 shows that elite golfers are less likely to participate in tournaments with small purses and more likely to participate in tournaments with large purses. In particular, the participation rate for the top ability golfers is 10 percentage points higher for the top 20% of tournaments. Critically, as shown in the prior figure, these top 20% of tournaments have top prizes that are approximately $500,000 more than lower ranked tournaments.

Given the linearly increasing purse size across tournament rank, but the convexity of prizes within tournaments, are golfer earnings convex in ability? To show this, Figure 2.4 sorts golfers into ten deciles based on our index of ability. We then plot the observed mean total earnings in each decile and the 90th percentile of earnings in each decile. These graphs show a convex relationship – with the convexity being more pronounced when focusing on top earnings in each decile. Panel 2.4c then shows the standard deviation within ability partitions, which provides an estimate of how much income risk golfers face and how this is skewed over the distribution. Then, in panel 2.4d, we rank golfers based on earnings and again show the convex relations consistent with superstar effects. Overall, the sharp increase in golfers earnings is due to: prizes being convex, high ability golfers being more likely to win, and excellent golfers selecting into better tournaments. In our view, because the increase in prizes across tournaments is basically linear, the latter effect is secondary to the other two.

2.5 State Case Studies

As initial evidence, we present event studies for the two largest tax changes in our sample. First, we study Connecticut's adoption of a (4.5% flat) income tax. Second, we study passage of Proposition 30 in California, which substantially raised marginal tax rates on income above $250,000 starting in 2012. Also notable, California features multiple tournaments, while Connecticut only hosts one. Because these tax reforms affect two different populations, we estimate the following model separately for each state

$$P_{isty} = \sum_{e=-5}^{-2} \theta_e \cdot 1(y - y^* = e) + \sum_{e=0}^{3} \pi_e \cdot 1(y - y^* = e) + \mu_i + \varepsilon_{isty}, \tag{2.6}$$

where P_{isty} is an indicator variable for participation for golfer i, under state tax system s, in tournament t, of year y and where μ_i are golfer fixed effects.

For California, the time of the event is $y^* = 2012$ and for Connecticut $y^* = 1992$. The omitted year is the year prior to the event.[19] Then, the θ_e's represent the evolution of golfer participation before the tax reform relative to the omitted period and the π_e's represent the post tax reform evolution of golfer participation. For Connecticut, because this was the introduction of a flat tax, this event study is estimated on all golfers with positive realized earnings. For California, the tax solely affected high income earners, so we restrict the sample to those in the top 25th percentile in previous year earnings.

The results of these case studies for both states are presented in Figures 2.5a and 2.5b. Both figures show no discernible pre-trends. Then, in the year following the reform, participation in the state's tournaments decline substantially. In particular, by four years after the reform, the participation rate for the tournaments decline by 10 to 20 percent. Noticeably, the possibility of dynamic treatment effects, suggests that when pooling events, we will need to carefully select the comparison group. The case studies thus initial visual evidence of how tax increases at the state level discourage participation in the tournaments in the state. While these case studies are informative, we now turn to a more comprehensive event study approach that

[19]While for California, the tax was passed after all tournaments had already happened in that state, it was known well in advance that multiple tax increases for high income earners were on the ballot. It is likely that golfers and their accountants were well aware of the tax changes before they were passed.

systematically incorporates multiple events and events of different sizes across the full population of golfers.

2.6 Stacked Event Studies

In the context of state tax changes, the tax changes affect different income groups, often feature multiple tax changes in close succession, and affect the participation tax rates of golfer's differently depending on whether the employment state tax rate is lower/higher than the residence tax rate. Furthermore, it has recently been shown that the two-way fixed effect design with variation in treatment timing may produced biased treatment effects (Goodman-Bacon, 2021) by comparing early adopting and late adopting states.[20] We modify standard event study designs to address these important features. Our modifications draw inspiration from Moretti and Wilson (2017) and Cengiz et al. (2019).

In order to focus the event studies, we follow Moretti and Wilson (2017) in defining treatment and defining tax changes. A golfer's tax rate will change almost every year because a golfer's predicted expectation of earnings can change each year and because state tax codes change at least one element of the tax code (tax rate, tax base, exemption or deduction) frequently. For this reason, first, we focus on tax changes of at least one percentage point in absolute value – which allows us to define clear "major" state-level tax reforms.[21] Second, we drop golfers that change quality deciles by more than two quality deciles in the event year. By focusing on golfers that do not significantly change quality deciles, combined with the large tax increase restriction, we isolate statutory tax changes rather than changes due to changes in

[20]While there are numerous ways to solve this, we follow the stacked event study design of Cengiz et al. (2019) and Baker et al. (2021).

[21]Small changes are not a problem for our main models because, in those models, we exploit information on the magnitude of the change. But, tiny tax changes add significant noise in models with dummy variables, and make it impossible to create clear events with natural treatment and comparison groups.

expected income. Then, a golfer is treated if his PTR in a given state-year increases by 1 percentage point (or greater) and if the golfer does not change quality by more than two deciles. Thus, golfers who do not have a PTR increase of 1 percentage point or greater in the given state-year are in the comparison group. Critically, notice that treatment status is defined using an individual level PTR, rather than a state-specific tax rate, such as a specific tax rate. Employment state tax rates may not affect individuals living in high-tax states and may affect high and low-income individuals differently depending on whether the reform was to a top marginal tax rate or another element of the tax schedule. Our treatment and comparison definition accounts for this.

Next, we follow Moretti and Wilson (2017) to construct "clean" treatment events. As noted previously, states may change their tax code incrementally over several year. In order to create a clean pre-treatment period free of other tax major tax changes, we eliminate all treated units that have a tax change of 1 percentage point (or greater) in absolute value in the prior 4 years. In this way, a major tax event is defined as the *first* year of the series of major reforms and prior tax changes do not substantially affect pre-trends. Second, following Moretti and Wilson (2017), we focus on permanent tax increases. This means that we focus on tax changes that are not reversed for at least the next five years. To do this, any major initial tax increase cannot be followed in a major tax decrease of one percentage point or more in the following 4 years. However, tax increases are allowed to follow the first tax increase, as many states choose to slowly increases taxes over time instead of a one period jump. In this way, we focus on permanent tax changes.

In order to produce unbiased estimates, the comparison group for each major event must also be "clean" of major reforms. Clean comparisons are those individual-state pairs that do not have a nontrivial state tax change in the entire eight-year panel around the treatment event. To generate these, we create separate comparison groups for *each* event by using golfer-state pairs that do not have a more than 1 percentage point change in the event year or in *any* of the 4 years before or after the event in the given state-year.[22] We follow this procedure for each major treatment event – constructing a set of clean comparisons for each treatment. Let each major treatment and comparison grouping be indexed j. Following, Cengiz et al. (2019), each grouping j containing a treatment event along with its clean controls are then stacked over each other.

We then estimate the following equation:

$$Y_{istyj} = \sum_{e=-4}^{-2} \theta_e \cdot D_j \cdot 1(y - y_j^* = e) + \sum_{e=0}^{4} \pi_e \cdot D_j \cdot 1(y - y_j^* = e) + \beta_1 \ln(w_{dty}^{\mathbb{E}}) + \sigma_a + \delta_{ty} + \psi_{ij} + \omega_{yj} + \varepsilon_{istyj}, \tag{2.7}$$

where Y_{istyj} represents the outcome of interest for golfer i, in state s, in tournament t, in year y, and for event j. We consider the golfer participation decision and, to show a clean first stage, the golfer tax rate. The treatment indicator, D_j, takes on the value of 1 if the observation is defined as treated (experiencing a major tax increase), and zero if in the comparison group. The θ_e coefficients represent the relative difference in outcomes between treated units and the event-specific comparison group prior to the tax increase, while π_e represent the relative difference between treatment and comparison group after the tax increase. All effects are relative to the year prior to the major tax reform. The only difference between this design and the standard event-study estimator is that we saturate the unit and time fixed effects with indicators for each specific event, yielding individual by event fixed effects (ψ_{ij}) and year by event fixed effects (ω_{yj}). Given the stacked nature of the design, identification comes from comparing the treatment group with each event-specific clean compression group,

[22]Treated golfers that play in another state are excluded from the comparison group, even if they experience no tax changes in the other states. In other words if a golfer experiences a major treated event in California, no other states of that golfer are used as a comparison. Intuitively, those states are contaminated by possible shifting of where the golfer plays.

rather than comparing early and late adopters as in the standard two-way fixed effect design. By doing this, we ensure ensure the θ_e and π_e are identified only from variation within each group j, making the results equivalent to a contemporaneous event study, where all the events occur simultaneously (Baker et al., 2021).

We have three sets of controls in addition to the fixed effects. First, age fixed effects control for any common career dynamics. Second, as in a standard labor supply equation, we control for expected earnings, $\ln(w^{\mathbb{E}}_{dty})$ in decile d. Because expected earnings are exogenous, they can reasonably viewed as exogenous. Finally, we include tournament by year fixed effects (δ_{ty}) to control for time varying tournament characteristics that are unrelated to tournament state tax liability, such as weather or course conditions. Importantly, because treatment is defined at the individual and year level and not the tournament year level, there is no explicit need to control for event specific tournament by year fixed effects to ensure the stacked difference-in-difference is well identified. Standard errors are clustered two-ways: the golfer level and the state level.

As a robustness check to studying only tax increase, we verify robustness of our results by including both tax increases and tax decreases following the same rules as before, except with the reverse convention for tax decreases.[23]. Practically, we redefine D_j such that it equals 1 if the observation experiences a major tax increase, -1 if it experiences a major tax decrease, and zero if in the comparison group. Such a specification imposes that the response to tax increases is equal in magnitude, but opposite in sign, to tax decreases.

Before presenting results of the participation responses, we show clean "first stages" with respect to how major tax reforms affect the participation tax rate. First, we show that tax changes translate into increases in the PTR. Second, because the

[23]For example, for a tax decrease event, we only allow further decreases in the following periods and do not allow for offsetting increases. This is identical to Moretti and Wilson (2017)

PTR depends on expected earnings, we show how this varies across golfer abilities. Finally, although the relevant tax rate for the golfer's participation decision is the PTR, the policy parameter is the statutory tax rate. These statutory changes consist of changes in marginal tax rates, brackets, and deductions or exemptions, but can be summarized in a single policy-relevant average tax rate. Given policymakers cannot directly change the PTR because it depends, among other things, on the residential location of the golfer, we show how changes in the PTR map to changes in the average tax rate on income in a state.

2.6.1 Results

We proceed by first showing a clean first stage where the timing of the tax change translates to a change in PTR. Figure 2.6 shows the results of estimating (2.7) where Y_{istyj} is the participation tax rate (in percentage points). Keeping in mind that we focus on events greater than one percentage points, each major tax change raises the PTR by about 1.7 percentage points. Interestingly, the mean increase for all golfers is very similar to the increase for golfers in the top 25%. The reason for this is that in the event studies, we focus on state tax changes, which may be less targeted than many federal reforms. Thus, the limited heterogeneity in the impact of taxes on PTR will not bias the average estimate toward that of players with high ability. The figures also show how the average tax rate on income in the source state increases following the increase in the PTR. The average tax rate increases by more than half, and by only a slightly larger amount at the top of the distribution. The ATR in the employment state rises by less because it excludes changes in the residence state that influence the PTR and because of the limited progressivity in the tax code. Finally, although we have focused on permanent tax increase, there is some reversion back to zero and some initial downward trends in the pre-reform period. These trends are driven by the fact that the PTR depends on expected earnings. Although we restrict the sample to individuals that do not move more than two deciles, there is some mild mean reversion among golfers that remain in our sample – and we cannot focus on golfers that never change deciles over a ten year period as the sample would become too thin.

Figure 2.7 estimates (2.7) where Y_{istyj} is now whether the golfer participates in the tournament or not. We show all golfers with positive earnings and golfers in the top 25% separately. Panel (a) and (b) focus on tax increases, while panel (c) and (d) pool both increases and decreases. Interestingly, the effect of tax increases are larger than tax decreases, suggesting that eliminating a tournament that was played in the past is easier than adding a tournament that one did not play previously. Adding a tournament may come with added costs, such as a lack of familiarity with whether the course or a lack of practice on the course making it less appealing. With respect to heterogeneity by golfer earnings, we see little heterogeneity in the participation response. This is not to say that the response to all tax changes is identical. Given these figures focus on major reforms, the reforms are highly salient to both high quality and low quality golfers, representing a non-trivial tax change for both sets of golfers. For smaller tax changes, small tax changes may be a large dollar change in taxes for superstar golfers, but may be meaningless for low income-golfers. In this way, golfers may still be heterogeneous with respect to smaller tax changes and the participation elasticity may differ over the income distribution. Finally, given the change in the PTR is similar in percentage points across golfer abilities, the percent change in the net-of-participation rate will be smaller due to progressivity. A smaller percent change in the net-of-PTR corresponds to a higher elasticity even if the change in the participation rate is equal across groups.

Finally, Figure 2.8 shows the results separately for gofers living in low-tax states and high-tax states. A golfer in a high-tax state is defined as a golfer who has zero expected employment state tax liability from participating in an event, and thus any change in their PTR would come from tax changes in their resident state and should minimally alter the spatial distribution of his participation decision. Furthermore, one concern with the prior event studies is that, wage incidence of the taxes as a result of prizes adjusting to taxes may attenuate our result. Golfers living in high-tax states find tax increases in the source state irrelevant because they pay the higher tax rate of source and residence state. But, if there is wage incidence, a high-tax state golfer will be affected by the change in the prize schedule in the same manner a low-tax state golfer will be.[24] Thus, this figure shows the setup for a possible triple difference where high-tax golfers are then used to remove any equilibrium effect from wage incidence. The event studies indicate that high-tax golfers are almost unaffected, suggesting any wage incidence effects are small.[25]

Although visually appealing, as discussed in the previous section, the event studies do not easily facilitate estimation of an extensive margin elasticity with respect to the participation tax rate. Thus, we proceed with a specification that exploits all variation in tax changes.

2.7 Baseline Regressions

While the prior event studies provide clear and transparent visual results, they come at a cost of not using all of the tax variation across states. In particular, they exploit only large tax changes. To exploit all tax changes and to obtain an elasticity of labor supply, we now turn to a panel data design that regresses golfer participation in a tournament in state s on a measure of expected income, taxes, controls and fixed effects.

[24] This assumes that the wage incidence of a change in taxes in the employment state are similar to the wage incidence from a tax change in the residence state.

[25] Note that Panel (a) is different from the prior figure because golfers in high-tax states are excluded from both the treatment and comparison group.

Given that our data is an individual-tournament-year panel, we could estimate:

$$P_{isty} = \beta_0 \ln(1 - \text{PTR}_{isty}) + \beta_1 \ln w^{\mathbb{E}}_{dty} + \mu_i + \delta_{ty} + \sigma_a + u_{isty} \tag{2.8}$$

where P_{isty} is an indicator variable for participation for golfer i, under state tax system s, in tournament t of year y, $(1-\text{PTR}_{isty})$ is the net-of-PTR rate, $w^{\mathbb{E}}_{dty}$ represents the group expected earnings for decile d, and μ_i, δ_{ty}, and σ_a are golfer, tournament by year (e.g., week), and age fixed effects. The covariates have the same justification as in the event study design. Because expected earnings are constructed using a grouping estimator and not an individual prediction, they can reasonably viewed as exogenous. However, this specification is problematic due to the correlation between participation in past and future tournaments and the PTR. Given the PTR is constructed using participation across the entire year, it would violate the strict exogeneity assumption (i.e., $\mathbb{E}(u_{isty}|\text{PTR}_{is1y}, ...\text{PTR}_{isTy} = 0)$). In order to overcome this, while also making the identifying variation as clear as possible, we use year-over-year differences within a tournament where the identifying variation comes from year differences in the same tournament (eg., Safeway 2018 - Safeway 2017). If tournament quality is generally constant over time, the primary source of identification would come from changes in the tax rate and variation in golfer quality. Thus, we estimate the following model:

$$\Delta P_{isty} = \beta_0 \Delta \ln(1 - \text{PTR}_{isty}) + \beta_1 \Delta \ln w^{\mathbb{E}}_{dty} + \delta_{ty} + \sigma_a + \Delta u_{isty} \tag{2.9}$$

where the Δ operator represents the tournament-year difference.[26] Unlike the literature on tax-induce migration, to identify the pure tax elasticity, we don't need to control for endogenous public services given nonresidents consume minimal public services.

[26] If there is measurement error, the long difference rather than a weekly difference helps minimize these concerns (Griliches and Hausman, 1986).

While this is a clear improvement over the fixed effects specification, some concerns could remain. Because the panel spans the entire career earnings of golfers, one may worry about differences in growth as golfers are heterogeneous in career trajectories due to unobserved differences in skill over time. This could lead to non-stationarity in differences (Lillard and Weiss, 1979) as well as the nonlinear growth in golfer earnings due to the increasing popularity and marketability of the sport. In order to robustly control for heterogeneous growth rates, we estimate (2.9) with a linear trend in differences, a quadratic trend, and preferably a double-differenced version (Kniesner and Ziliak, 2015):

$$\Delta^2 P_{isty} = \beta_0 \Delta^2 \ln(1 - \mathrm{PTR}_{isty}) + \beta_1 \Delta^2 \ln w^{\mathbb{E}}_{dty} + \delta_{ty} + \sigma_a + \Delta^2 u_{isty} \tag{2.10}$$

where $\Delta^2 = \Delta_t - \Delta_{t-1}$, the difference-in-difference operator.

How does our specification studying employment decisions across space differ from a standard extensive labor supply estimating equation? A standard (single jurisdiction) structural extensive margin specification following Saez (2002) would be:

$$P_{iy} = \beta \ln\left[I_{iy}(1 - ATR_{iy})\right] + X_{iy}\alpha + \varepsilon_{iy} \tag{2.11}$$

where P_{iy} is an indicator variable for workforce participation, I_{iy} is the wage bill, and $1 - ATR_{iy}$ is the net-of-average tax rate, which can be observed or estimated. However, this paper does not look at a classical extensive margin response regarding the decision to work or not. Instead, a golfer's decision is whether to participate in one additional tournament each week. Thus, the overall average tax rate will mismeasure the additional taxes paid from participation because golfers only care about the average tax rate on the *additional* income earned from participation. For this reason, the PTR is relevant for the golfer's decision, but not for policy recommendations.

Conceptually, the policy parameter is not the elasticity with respect to the PTR, but rather the elasticity with respect to statutory components of the tax code. Statutory changes in the tax code, due to changes in marginal tax rates, brackets, exemptions, or deductions can be summarized in the average tax rate. Thus, we estimate a variant of (2.9), which yields the effect of tax rate changes from the simulate average tax rate in the employment state, ATR, on the PTR:

$$\Delta \ln(1 - \text{PTR}_{isty}) = \beta_0 \Delta \ln(1 - \text{ATR}_{isty}) + \beta_1 \Delta \ln w^{\mathbb{E}}_{dty} + \delta_{ty} + \sigma_a + \Delta u_{isty}. \quad (2.12)$$

This specification maps the PTR—which depends on taxes in the residence and employment state on the additional earnings from participation—to changes in the average tax rate in the state of employment.

To study the effects of policy changes, we can transform our elasticities with respect to the PTR by multiplying by our estimates in (2.12) to obtain:

$$\epsilon_{1-ATR} = \epsilon_{1-PTR} \cdot \frac{d\ln(1 - \text{PTR})}{d\ln(1 - \text{ATR})}. \quad (2.13)$$

The transformation yields the policy-relevant elasticity.

2.7.1 Baseline Results

The results estimating (2.9) are presented in Table 2.2. Overall, a one percent increase in the net-of-PTR increases the participation of golfers by 0.135 percentage points.[27] We convert each coefficient to an elasticity using the average participation rate for the estimating sample. Given, on average, 42 percent of golfers participate in a tournament, the implied participation elasticity is 0.32. Such an elasticity is larger than standard intensive margin elasticities for high income earners in labor economics

[27] Given the mean net-of-tax rate, a one percent increase implies a 0.62 percentage point change in the PTR.

(Moffitt and Wilhelm, 2000), but smaller than many residential mobility elasticities (Kleven et al., 2020).[28]

As noted previously, the policy relevant elasticity is not with respect the PTR, but the average tax rate in the employment state. To study the effects of policy changes in a particular employment state, we can transform our elasticities using(2.13). As indicated in Table 2.2, the elasticity with respect to the golfer's average tax rate in the employment state is approximately 50% of ϵ_{1-PTR}, yielding an elasticity of 0.16. Intuitively, the average tax rate is less than or equal to the participation tax rate, so that $1 - PTR \leq 1 - ATR$.[29] Then a one percent change in the net of average tax rate, induces a smaller percent change in the net of participation tax rate, which makes the elasticity for the ATR smaller.

The remainder of the table explores heterogeneity by golfer characteristics. Column (2) drops golfers who do not have any earnings in a year, which are on average, lower-quality golfers. Because golfers who do not make a cut may have a different incentives for playing golf, these golfers are excluded from the sample in column (3). Overall the results are fairly consistent, although dropping low-performing golfers who miss the cut yields higher elasticities. This provides suggestive evidence that top golfers drive the effects. We also cut our sample based upon yearly earnings in year $y - 1$.[30] Current earnings may be endogenous to the decision to participate; as a solution we use lagged earnings for this heterogeneity exercise. In addition, current year earnings may be a poor approximation for what a golfer would *expect* to earn due to hot hand effects (Livingston, 2012) – a stochastic shock in performance that lies

[28]It could also be useful to compare these elasticities to other extensive margin elasticities. Unfortunately, most elasticities look at the extensive margin for single mothers. Those estimates seem to be around 0.8 (Meyer and Rosenbaum, 2001).

[29]The inequality is not strict because if taxes are flat, the PTR equals the ATR. Moreover, as income goes to infinity, the ATR converges to the PTR.

[30]Table A.1 show that the relationship still holds cutting on longer lagged or contemporaneous earnings, although with smaller elasticities the more current are earnings.

outside of career norms – or golfers inability to forecast future performance. Columns (4) and (5) focus on high-income or superstar golfers. These golfers presumably have the most flexibility at changing tournaments and, due to progressive taxes, are likely to realize the largest tax differentials across states. Indeed, those golfers in the top 25% of the earnings distribution (median earnings $1.07 million in 2018) have elasticities, 0.81, that are more than twice as large.[31] Moreover, higher up in the income distribution, a one percent increase in the net-of-ATR has a larger percent increase on the net-of-PTR. Intuitively, as income goes to infinity changes in the ATR translate one-for-one into changes in the PTR. Thus, column 5 indicates that after applying (2.13), the elasticity with respect to the net-of-ATR is $0.727 \times 0.812 = 0.590$.

To examine heterogeneity more flexibly, we estimate (2.9) by interacting the model with decile indicators of earnings or quality from our index. The first set of decile indicators are constructed using the actual earnings in $y-1$. The latter set uses our quality index to construct indicators of ability. Golfers are grouped into deciles each year. The results of these estimates are presented in Figure 2.9. First, taxes have very little effect at the lower end of the income distribution. This result is consistent with these golfers needing to play in tournaments to retain their PGA Tour cards and these golfers having less salient tax differentials across states. Both figures show a clear, positive relationship between earnings and taxes after the sixth decile. The median income in the 10th decile is $850,000.[32] At the 10th decile, the implied elasticity is roughly 2.54, suggesting a strong response to taxes at superstar income levels. Identifying effects only at the upper part of the income distribution, consistent with theory, strengthens the case for identification. Furthermore, the in-

[31] Visualizations of these regressions—in the form of binned scatter plots—are shown in Figure A.4.

[32] These bins are constructed by year, so the 10th decile in 1980 has a median income of $340,000 while 10th decile in 2018 has a median income of $1,400,000.

creasing elasticity over golfer ability is consistent with superstar effects raising the earning elasticity as ability increases.

Figure 2.10 shows how the PTR changes with respect to the ATR over the ability distribution. To do this, we estimate (2.12) by interacting the model with decile indicators of earnings or quality from our index. Figure 2.10 presents estimates β_0 by decile. As can bee seen, the mean effect is approximately 0.5, but the effects are larger at the top of the income distribution. Thus, elasticities with respect to the net-of-PTR are closer to the elasticities with respect to the net-of-ATR, the higher the golfer is in the income distribution.

In Table 2.2, the effect of expected earnings is smaller than the effect of taxes. First, major tax reforms, such as California's millionaire tax, may be very salient to superstars that regularly seek the advice of tax accountants. Moreover, prizes may grow slowly over time relative to discrete tax changes. Second, expected prizes may be highly uncertain to the golfer, and although this makes taxes uncertain, the tax function T_{sy} is known. Then, if there is measurement error in expected earnings (Keane, 2011), the magnitude of the attenuation bias will depend on the signal-to-noise ratio. Signal-to-noise is likely smaller for $w^{\mathbb{E}}_{dty}$ than for $1-\mathrm{PTR}_{isty}$. Intuitively, any measurement error in the tax term is mitigated by the division in (2.5). To see this, note that if tax systems are flat, then $1-\mathrm{PTR}_{isty}$ contains *no* measurement error as the division perfectly cancels income from the expression and simply returns the flat tax rate. In the case of progressive taxes, such perfect cancellation will not eliminate measurement error, but the division will mitigate any noise in the participation tax rate. For this reason, we expect the attenuation bias on taxes to be minimal compared to the earnings term.

To shed additional light on the differences of the coefficients, we remove individual fixed effects from the data when constructing the group (decile) means. Separately for each decile and year, we regress realized earnings on individual fixed effects and tournament fixed effects. Then, rather than use realized earnings to construct our expectation, we use the coefficient on the tournament fixed effects as an estimate of earnings in the tournament for each decile. The results, presented in Table A.2 show similar results, however, doing this allows us to interpret the wage term as the expected value of earnings in that decile. Then, the divergence between the expected wage and tax coefficients can be due to two factors. First, people are not good at forming expectations over earnings (i.e., player expectations are only loosely correlated with the accurate expectation we have created). Such errors in the golfers expectations could be a result of the player being overly optimistic/pessimistic. Second, golfers may not have enough information to form these expectations well. In other words, quality and ranking only loosely allow the golfer to form an expectation of earnings. Under such a view, the wage information is not very salient and so golfers do not pay as much attention to earnings as they do to taxes.

To verify we are identifying a spatial reallocation (Fajgelbaum et al., 2018) and not a decline in the number of total tournaments played, we compare model specifications with only state taxes and only federal taxes. Table 2.3 presents the results using only the state rates or only the federal tax rates. To obtain elasticities with respect to state tax rates, we scale the coefficients such that the elasticities comparable to the mean tax change in the main tables. The elasticities are similar. We compare this to estimates using federal tax changes only. To do this, we use golfers residing in zero-tax states and apportion all income to the state of residence.[33] This specification does not have any spatial variation and utilizes only temporal variation. The estimated elasticities are approximately zero. Given that federal taxes do not reduce the number of times a golfer participates, our strong state tax results suggests that state taxes spatially distort employment rather than reduce the

[33] We use these golfers to eliminate any effects of deductibility of state and local taxes.

number of tournaments played. Thus, we conclude that the mobility response in our theoretical model drives the results, rather than a taxable income response.

We can also exploit a unique feature of U.S. tax law. When individuals live in a high-tax state, additional taxes will always be due to the state of residence, implying that the low-tax employment state taxes are irrelevant. Theoretically, golfers living in high-tax states should not have incentives to spatially distort their contracts. Table 2.4 shows that when focusing on golfers residing in the highest tax states, taxes appear to be an unimportant factor for where to work.

All of the prior results do not adjust for any wage incidence effects on prize amounts outside of our measure for expected earnings. If expected earnings *completely* capture the wage incidence of progressive taxes, then our estimates can be interpreted as pure labor supply elasticities rather than equilibrium concepts. However, for equivalent tax changes, if there is differential incidence based on different placements (within a decile), then expected earnings would not fully account for incidence. Assuming that any tournament level wage incidence would largely be targeted at attracting the very top earners, elasticities for the full population would not be substantially effected, but elasticities at the top would. More generally, any wage incidence effect above our measure of expected earnings would dampen the impact of taxes on participation as the PGA Tour would effectively be paying the tax increases for the golfers. As we still see fairly strong results, this implies that any wage incidence does not fully compensate golfers for participating in high tax states. In order to interpret the estimated coefficient as a labor supply elasticity, however, we must assume these wage incidence effects are small. Nonetheless, we realize that the incidence of these taxes may be extremely heterogeneous across golfers, across tournaments given varying objectives of tournaments, and across prize categories. Estimating incidence effects is, therefore, extremely challenging because taxes are progressive, expectations rather than prizes matter for participation, and tournaments may have varying objectives over which golfer to attract. If taxes significantly impact tournament prizes then, we estimate an equilibrium effect, not an elasticity of labor supply. We will subsequently explore a triple difference design that addresses this issue.

To address this issue, we subsequently add another layer of differencing that exploits golfers residing in high-tax states. For these golfers, employment-state taxes do not matter as shown in Table 2.4. But, outside of the direct effect of taxes, any incidence of taxes on prizes affects golfers living in high-tax states in a similar way as golfers living in low-tax states. All else equal, higher pre-tax prizes make a tournament more lucrative. This additional difference then allows us to purge out any incidence effects assuming that the incidence effects in response to home state tax changes are similar to those of employment state tax changes. While relying on assumptions, this provides another test of if our expected earnings term account for incidence, and allows us to interpret the estimated coefficient as a labor supply elasticity.

Table 2.5 shows the triple difference estimates. To construct these we interact the net-of-PTR with an indicator for whether the golfer lives in a low-tax state. A low-tax state golfer is a golfer that has a positive expected tax liability in the state of employment from participating in a tournament. Then, the coefficient on the non-interacted PTR term shows the effect of tax changes for golfers living in high-tax states. The interaction term is the *additional* effect for golfers living in low-tax states; the total effect is given by the marginal effect. Interpreting only the interaction term allows us to difference out any incidence effects that affect both sets of golfers similarly. As can be seen, the total effects are almost identical to this differenced version. This suggests that our measure of expected earnings accounts for most of the effect of any wage incidence and that there is little additional incidence differentiation within deciles. For golfers with positive earnings, the elasticity prior to differencing is 0.628, but after using high-tax state golfers to difference out any wage incidence, the elasticity is 0.648. This implies that expected earnings mostly capture the wage incidence of progressive taxes, and so our estimates can be interpreted as pure labor supply elasticities rather than equilibrium concepts.

2.7.2 Robustness

As discussed previously, this panel spans the entire career of many golfers, which raises concerns about differences in growth paths as golfers are heterogeneous in career trajectories due to unobserved skill differences over time. The results of (2.10) are presented in Table 2.6. Controlling for heterogeneity in the growth term leaves the coefficient estimates and elasticity results generally unchanged. This suggests that the results are not being driven by golfer-specific differences in earnings profiles or participation nor by growth in prizes over time. The table focuses on all golfers with non-zero income, but results for top golfers are also unchanged.

To verify the results are robust to the few tournaments that change locations and any entering tournaments, when utilizing only tournaments that stay in the same state and exist forever, we obtain elasticity estimates of 0.226 for individuals with positive earnings and 0.521 for high income earners. Thus, tournaments do not appear to be responsive to taxes, likely in part as a result of rigidity from local sponsors and agreements with elite courses.

Next, we construct an alternative measure of predicted earnings that does not rely on the creation of a quality index. To do this we use a fractional probit model to observed earnings based on golfer characteristics, where earnings are to be between zero and one, where zero corresponds to missing the cut and one corresponds to the top prize of of the tournament. We then use the estimates from this model to calculate fitted earnings for participants and non-participants. We then divide predicted earnings into deciles and assign the cell means for all golfers. As as discussed in Appendix A.2, all the results are robust to this alternative grouping estimator. These can be seen in Tables A.3 and along with Figure A.5.

Golfers are also exposed to income risk that our focus on expected earnings may not capture. To address this, we calculate the standard deviation of expected income in a decile and control for it in the regressions. As a result, in these specifications, the coefficients on the net-of-PTR can be interpreted as the effect of the net-of-PTR, after partialing out any changes in income risk as measured by changes in the standard deviation of expected earnings. Table A.4 shows that the elasticities increase slightly.

In constructing the participation tax on one particular tournament, we hold fixed a golfer's decision to participate in other tournaments in the state, using the expected income for each of those tournaments. If participation decisions are made sequentially throughout the year, the decision to participate may depend on the realization of income shocks in that state so far. Table A.5 shows this does not appear to be a major concern. In particular, for states with only one tournament, then the sequence of tournaments in that state should not matter. The coefficients in these states are quite large and appear to drive our results. States with more than one tournament have much smaller effects – but given only a few states are in this category, confidence intervals are large.[34] Instead, what we want to highlight is that the coefficient estimate for the first tournament in these popular states is almost identical to the coefficient for subsequent tournaments in the state. This provides suggestive evidence that the order of tournaments in a state does not matter.

Finally, golfers should be less responsive to taxes for prestigious tournaments. Although we have eliminated Majors from the analysis, other tournaments may allow golfers to quickly move up the rankings or to qualify for honors. Using the total purse

[34]States with multiple tournaments likely have smaller effects because the tournaments located in popular states for golf, are likely higher quality. For this reason, we caution against comparing the effects in states with one tournament versus states in multiple tournaments.

each year as a proxy for quality, we partition the approximately 35 tournaments each year into quartiles. Table A.6 shows that golfers are less responsive to taxes at prominent tournaments. The elasticity generally falls off monotonically on the basis of tournament quality, and as expected, top golfers are not tax-responsive to the best tournaments.

2.7.3 Interpretation

It is often assumed that governments maximize revenue from nonresident top-earners. Our setting provides a clear example: given athletes are nonresidents, it is highly credible that the goal of taxes on out-of-state workers is to maximize revenue. Unlike many countries, state governments do not levy preferential rates on nonresidents. Assuming our estimates are applicable outside of the golf setting (so that the total number of working players each week is not fixed), our estimates shed light on whether state tax increases raise revenue from nonresidents *if* states could levy differentiated tax rates.

Following Agrawal et al. (2020), the change in tax revenue for a change in the (average) tax rate on nonresidents is positive if the net-of-tax rate is greater than the elasticity times the tax rate. To see this, let ATR_s denote the average tax rate on golfers in state s and let $B_s(ATR_s)$ denote the golfer income tax base. Then, differentiating with respect to the average tax rate:

$$\frac{d\left(ATR_s B_s\right)}{dATR_s} \propto 1 - \frac{ATR_s}{1-ATR_s}\epsilon_{1-ATR_s}. \tag{2.14}$$

To apply this formula, we assume participation responses are the only behavioral changes to the tax base. Then, we take the largest possible elasticity (for the top decile of players) with respect to the participation rate—2.54—and adjust it using equation (2.13), which yields $2.54 \times 0.674 = 1.712$. Using the Using the highest *average* tax rate (12.2%) over any state, implies that (2.14) evaluates to $0.878 > 0.122 \times 1.712$. The implication is clear: states are well to the left of the peak of the Laffer curve for taxing nonresident superstars. Obviously, this is a partial equilibrium analysis.

2.8 Conclusion

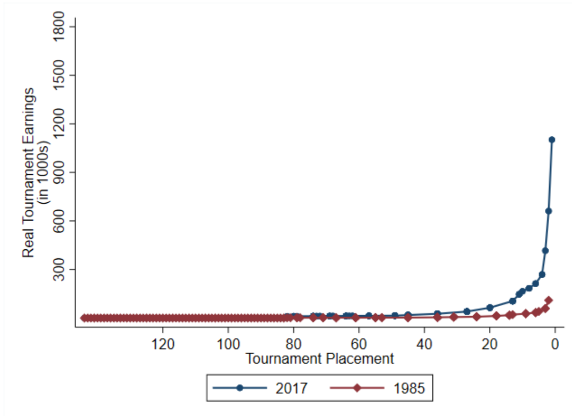
High income earners are responsive to income taxation via taxable income (Saez et al. 2012) and residential location responses (Kleven et al. 2020). We document a novel behavioral response for top earners: adjustments of the location of employment contracts, independent of residential relocation. We estimate a high-frequency labor supply participation response for superstars that is increasingly relevant in the digital economy. Using the variation in the location of professional athlete events and changes in state tax systems over time, we show that high-income superstars are more more likely to play in with lower state tax rates. The place of employment is responsive to taxes because the U.S. tax system, like other decentralized federations, sources earnings (strictly) to the place of employment if the home-state tax rate is lower than the tax rate in the state of work. The location of employment is likely to be tax-sensitive for many other high-income occupations: management consultants, rental property owners, pass-through businesses, artists, and self-employed individuals that travel frequently.

Whether the elasticities for golfers is larger or smaller than other occupations is unclear. On the one hand,golfers are likely to be a particularly responsive component of the labor market: they do not need to play every week, they have ample choice in the states they may play, and they are likely to have professional tax accountants advising them. Golfers are also extremely likely to live in low-tax states, which implies that the effective tax rate for participation will often be the rate in the employment state. If so, our estimates represent an upper bound on the participation elasticity, implying a lower bound on top marginal tax rates for nonresidents. On the other hand, the rigid structure of PGA Tour qualifications, the desire to qualify for elite tournaments, and sponsors potentially requiring participation in particular events may make professional golfers less responsive than other segments of the population. Finally, demand side factors may shape the elasticity. If tournament demand for golfers is different than firm demand for out-of-state workers, then researchers should be cautious at generalizing our estimates to other settings. Although these issues raise questions about external validity, the existing literature on residential relocations generally focuses on somewhat niche sectors (football players, scientists, top income earners) and it is clear why we also need to do so to overcome data limitations and to study a novel and important mechanism.

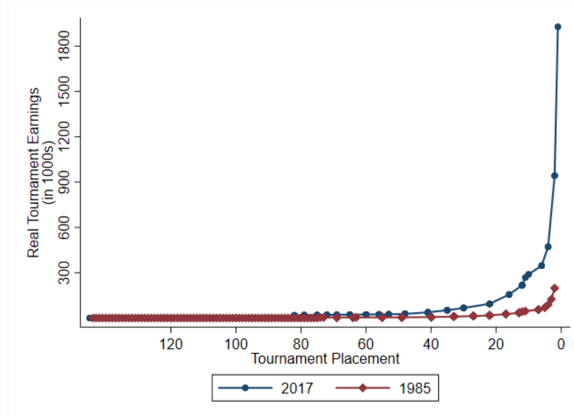
As the residence mobility literature has begun to realize how to move beyond these niche sectors, our paper provides a starting point to focus on how taxes affect the location of employment (as opposed to residence)—a distinction that will likely be increasingly important if the pandemic has a lasting effect on remote work. Thus, we hope our paper will spark an empirical literature on the tax-induced location of *employment* for high-income earners, much like Kleven et al. (2013)'s study of football players triggered a literature on the effect of taxes on international *residential* mobility. Researchers could study the work location decisions of consultants, pass-through entities, rental property owners and low-income workers in multi-state MSAs. Follow-up studies using administrative data could be possible, if researchers are able to obtain job-specific W-2 and 1099 forms. Unlike this study, follow-up studies will grapple with the lack of information on *declined* offers, but our methodological approach to deriving tax rates on nonresident income provides a useful starting point.

In the presence of decentralized taxation, governments must choose where to tax income. The U.S. applies both the source and residence principle, which effectively sources earnings (strictly) to the place of employment if the home-state tax rate is lower than the tax rate in the state of work. Intuitively, governments seek to allocate taxing rights on the basis of the more inelastic factor. Our results, taken in conjunction with prior mobility studies, imply that for high-income earners, both the elasticity of the location of employment and residence are tax-responsive.

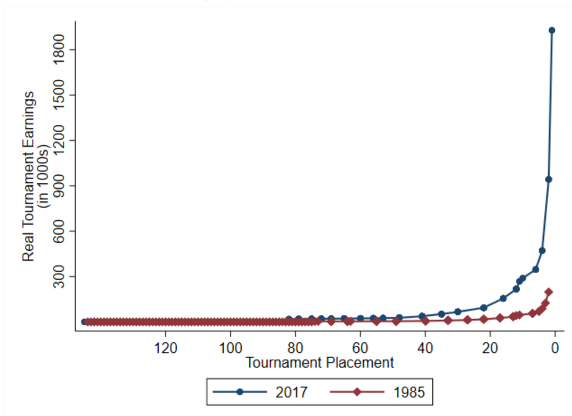
Figure 2.1: Earnings by Placement for Select Golf Tournaments



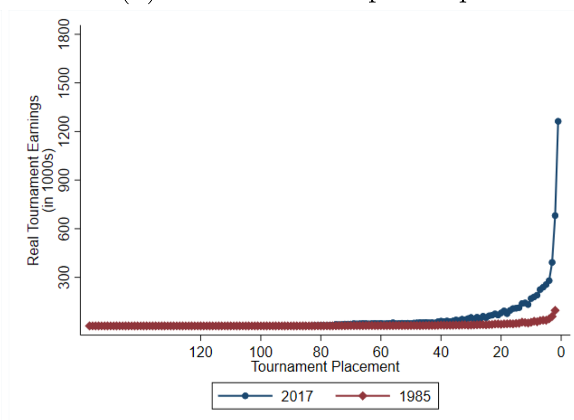
(a) Sony Open



(b) Travelers Championship



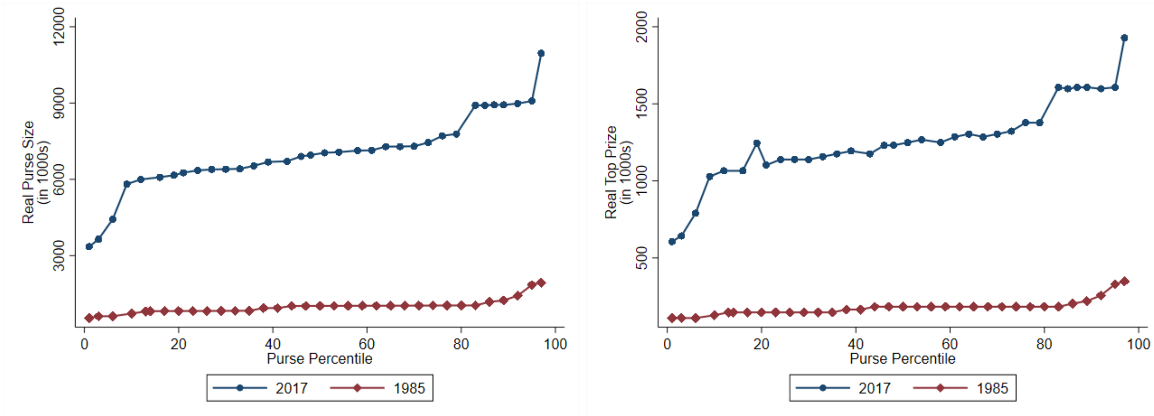
(c) Players Championship



(d) Average of All Tournaments

This figure shows tournament prizes based on golfer placement in the tournament. Golfers that are "cut" receive no prizes, and the number of cut golfers (usually below rank 80) are thus filled in with earnings of zero. The blue (circle) line shows golfer earnings in 2017, while the red (diamond) line shows golfer earnings in 1985. Panel (a) shows the prizes for a low ranked tournament, Panel (b) shows the prizes for a middle ranked tournament, while Panel (c) shows prizes for an elite tournament. Finally, Panel (d) averages the prizes at each rank for all tournaments in the year.

Figure 2.2: Tournament Payouts by the Size of the Purse

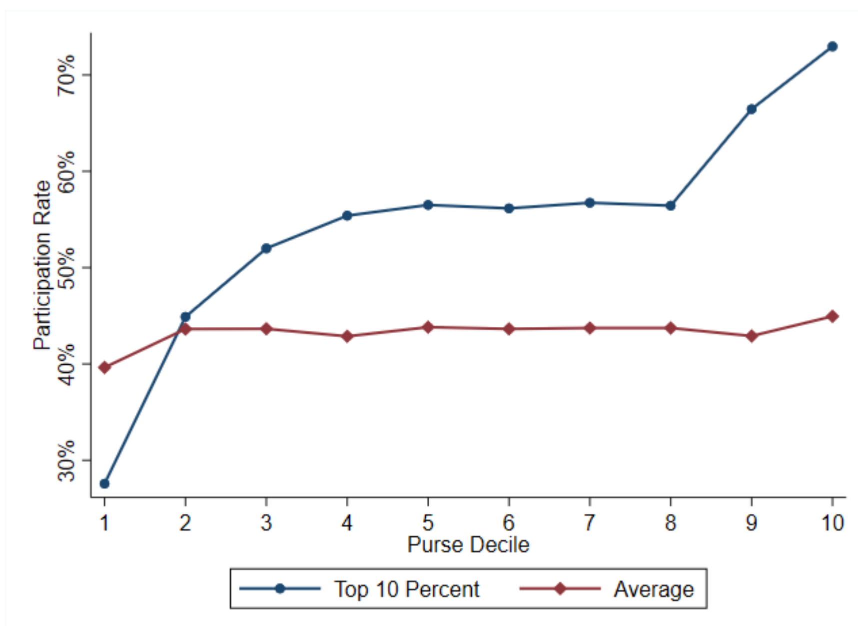


(a) Total Purse

(b) Top Prize

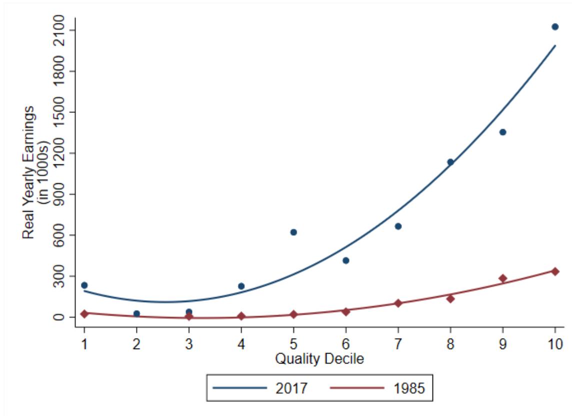
This figure shows tournament payouts based on a tournament's ranking. Tournaments are ranked using the total purse size; survey evidence among golfers indicates that purse sizes are highly correlated with tournament rank. The blue (circle) line shows golfer earnings in 2017, while the red (diamond) line shows golfer earnings in 1985. Panel (a) shows the total purse, while Panel (b) shows the top prize in the tournament.

Figure 2.3: Participation Rate by Purse Decile: Top Golfers

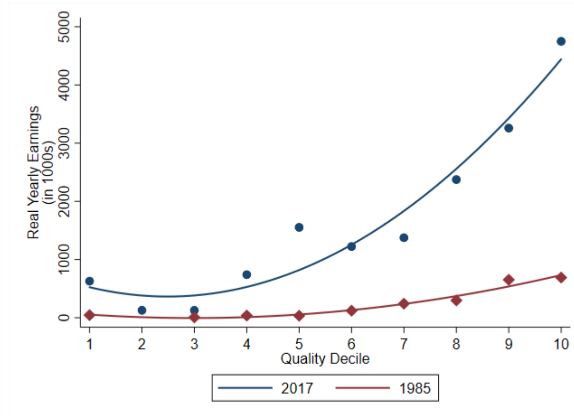


This figure shows the participation rate of golfers. For each year, we rank tournaments by the size of the purse, with the tenth decile having the largest purse. We also use our measure of golfer ability to partition top golfers from the average golfer. Golfers and tournaments are ranked each year and thus may change deciles over time. The red (diamond) line shows the mean participation rate across all golfers by purse decile. The blue (circle) line, shows the participation rate of golfers in the top 10% based on ability.

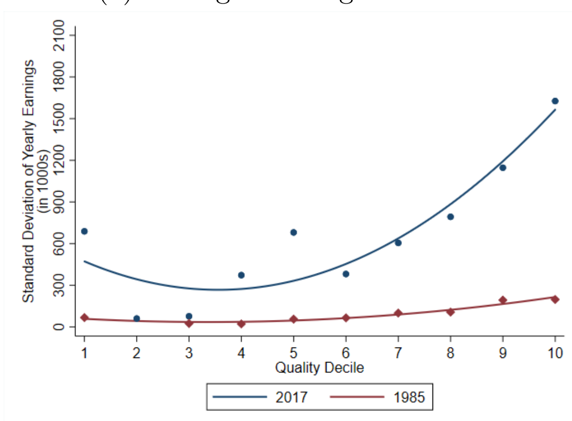
Figure 2.4: Yearly Earnings by Quality Decile



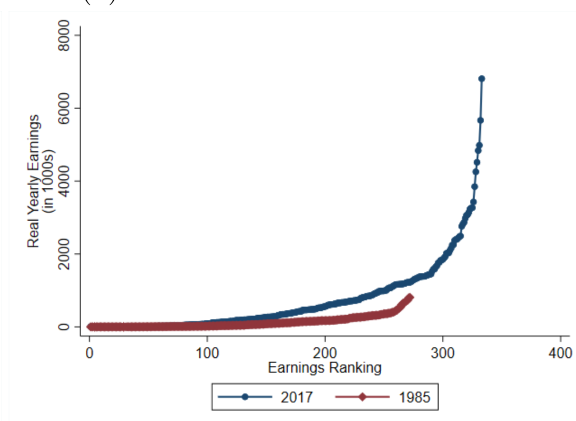
(a) Average Earnings in Decile



(b) 90th Percentile in Each Decile



(c) Standard Deviation by Decile

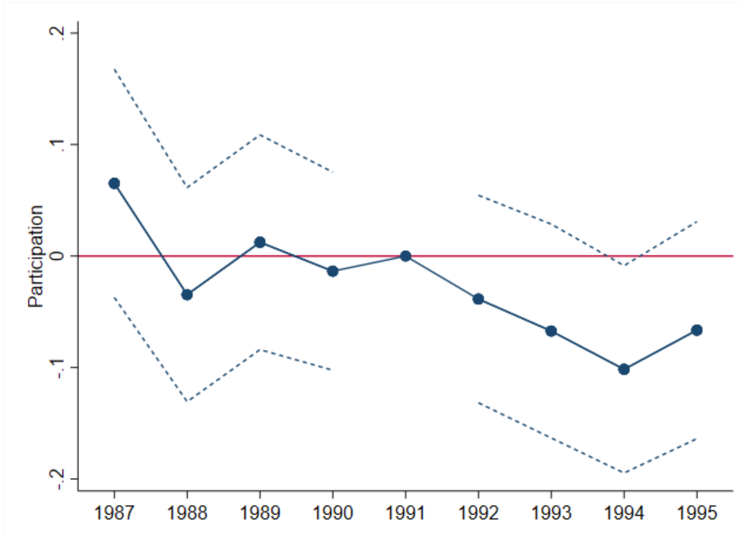


(d) Yearly Total Earnings by Golfer Rank

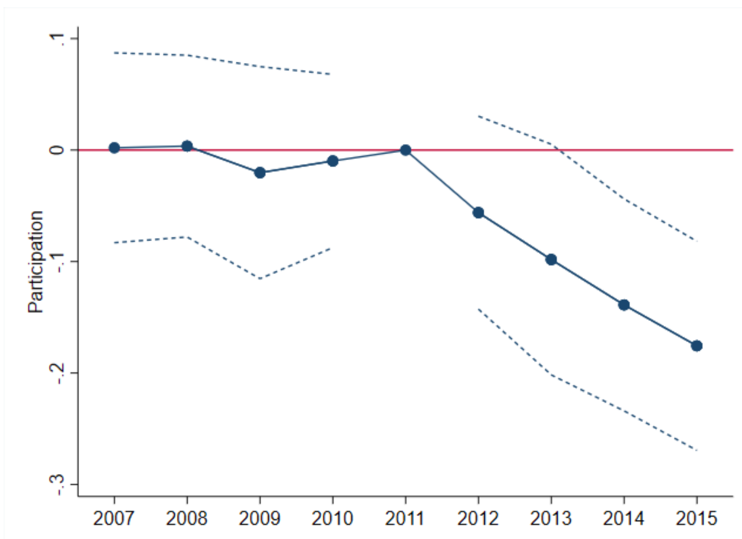
This figure shows total earnings by golfer deciles of our index measuring golfer ability/quality. The blue (circle) line shows golfer earnings in 2017, while the red (diamond) line shows golfer earnings in 1985. Each solid line represents a quadratic fit through the data. Panel (a) shows the mean earnings in each quality decile. Panel (b) shows the 90th percentile of earnings in each decile. Because the deciles partition based on our measure of golfer quality, if a golfer has a “hot hand” in one year, he may earn more than golfers in higher deciles, pulling up the average in that decile relative to adjacent deciles. Panel (c) shows the standard deviation of golfer earnings by decile. Finally, Panel (d) rank orders golfers based on annual earnings and plots total earnings with respect to golfer rank.

Figure 2.5: Case Studies Using Major Tax Reforms

(a) Connecticut

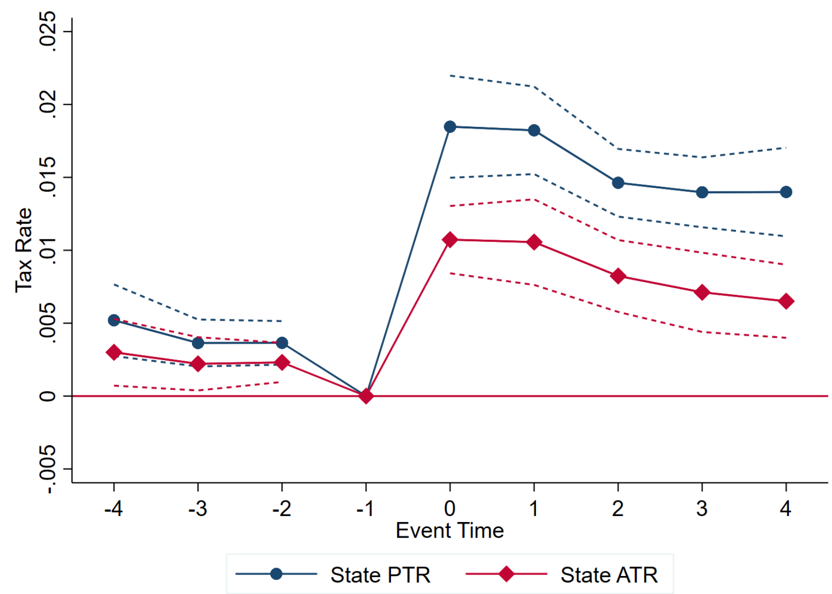


(b) California

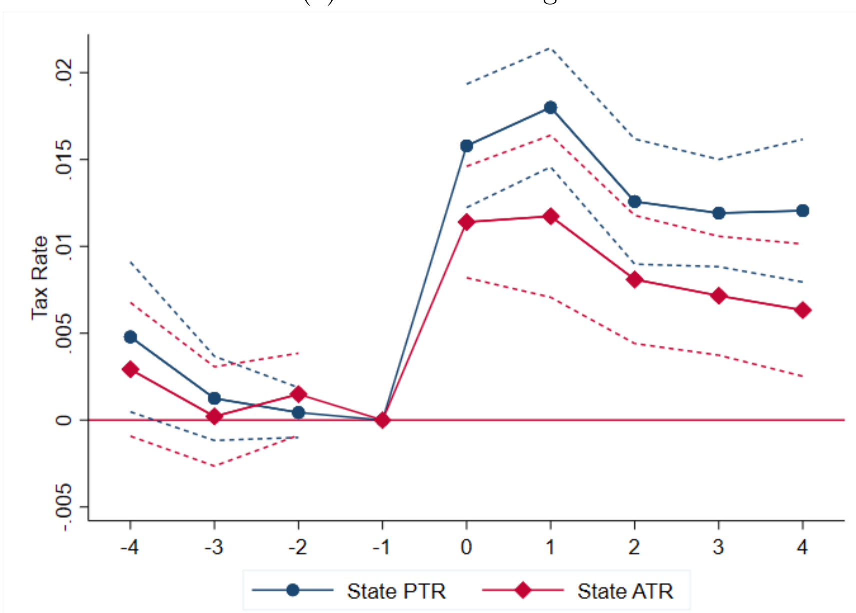


These figures represent event studies of the two largest tax reforms in our sample. We omit the year prior to each reform. The largest tax increase is in Panel (a), where Connecticut adopted its income tax for the first time in August of 1991. The income tax was adopted at a 4.5 percent rate starting in 1992. Although a reduced rate of 1.5 percent was applied retroactively to 1991 income, passage of the tax was after the golf tournament in the state. Moreover, passage of the income tax was unexpected until three state Senators to switched sides days before passage, justifying 1992 as the first year after the reform. Due to its comprehensive (flat) nature, the sample for the event study is all golfers with positive earnings. In Panel (b) we show the event study for the tax increases enacted by the passage of Proposition 30 in California, which raised the income tax rate from 10.3 to 13.3 percent for millionaires. Due to the tax only being implemented on high income earners, this sample is restricted to those who are in the top 25th percentile in previous year earnings. Although passed in 2012 and applied to 2012 income, as early as the end of 2011, the tax was expected to pass. CT contains only one tournament, while CA contains multiple. Standard errors are clustered at the golfer level and we present 95% confidence bands.

Figure 2.6: Tax Changes from Major Tax Reforms in Stacked Event Studies



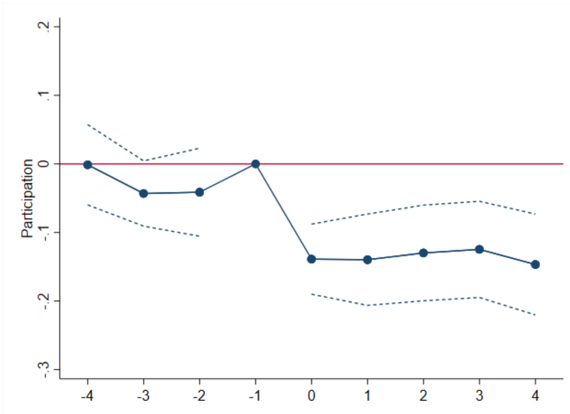
(a) Positive Earnings



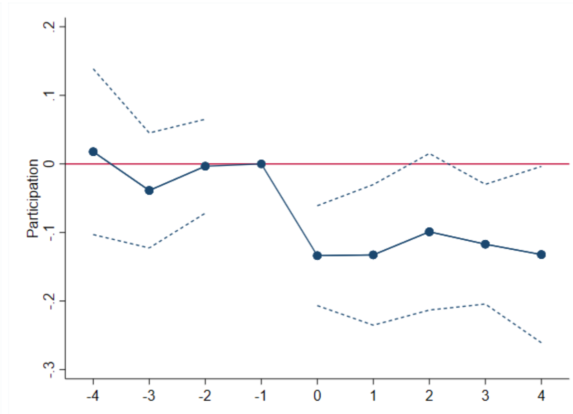
(b) Top 25 Percent of Earnings

These figures show how the timing of major tax reforms translate into changes in the PTR and the ATR (in percentage points). Panel (a) shows the effect of major tax reforms for golfers with positive earnings, while Panel (b) studies golfers in the top 25% of earnings. A major tax reform is an increase of more than one percentage point at the state level and excludes federal tax changes, and excludes federal tax reforms which have no natural comparison group. The average tax rate is the average tax rate on income earned in the source state. Event time -1 is the year before the reform. All figures are made using the stacked event study design with "clean" controls. Standard errors are clustered at the golfer and state of the tournament level and we present 95% confidence bands.

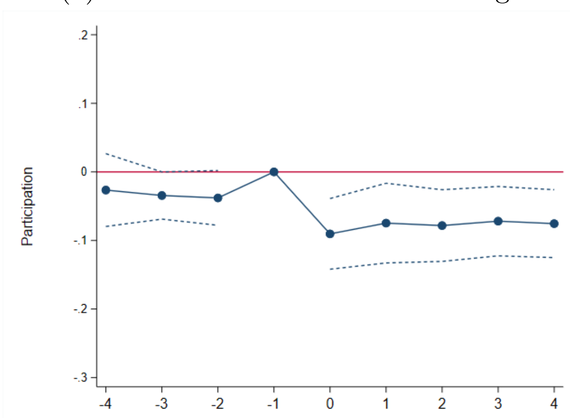
Figure 2.7: The Effect of Major Tax Reforms on Golfer Participation in Stacked Event Studies



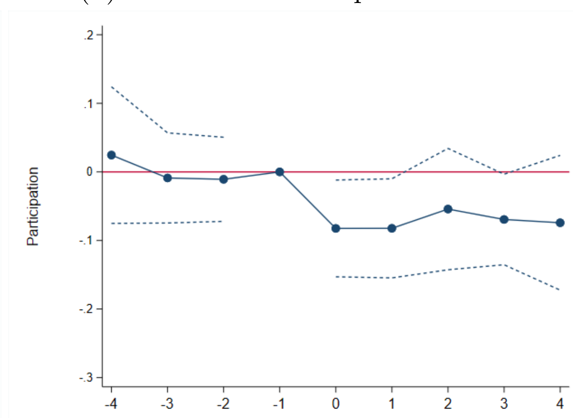
(a) Tax Increases: Positive Earnings



(b) Tax Increases: Top 25 Percent



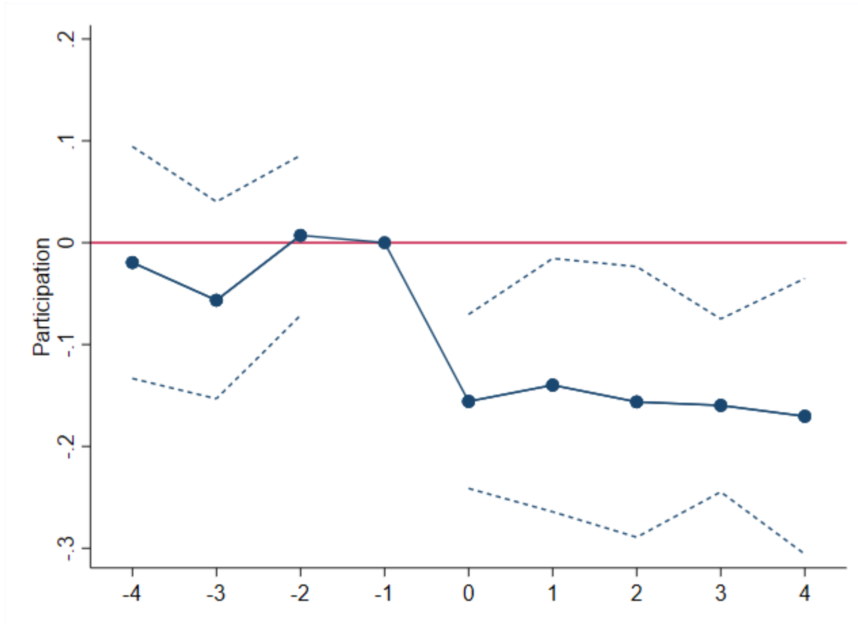
(c) All Tax Changes: Positive Earnings



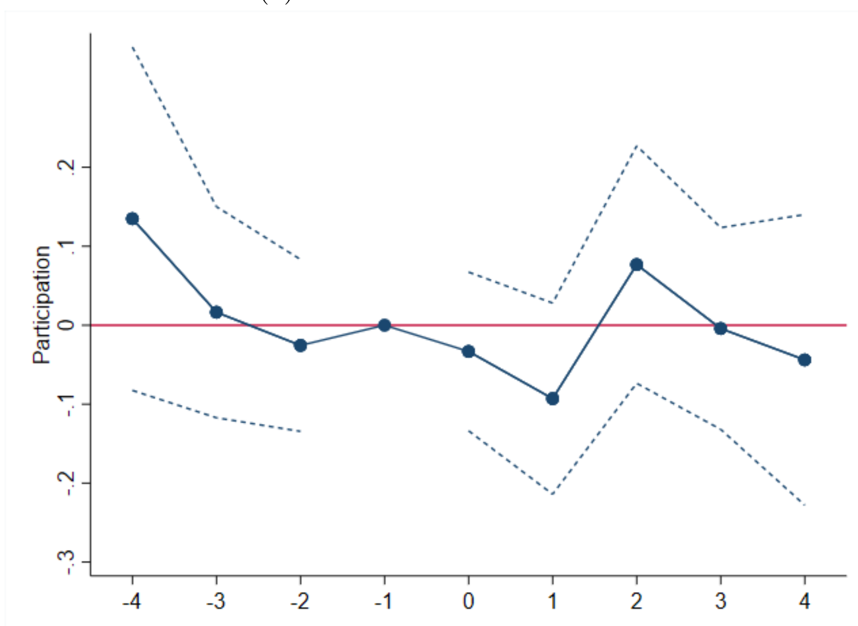
(d) All Tax Changes: Top 25 Percent

These figures show how the effect of major tax reforms on golfer participation using a stacked event study. Panel (a) focuses on tax increases for all golfers with positive earnings, while Panel (b) studies tax increases for golfers in the top 25% of earnings. Panel (c) and (d) are the analogous samples, but use both tax increases and decreases, following the approach of Moretti and Wilson (2017). A major tax reform is a tax change of more than one percentage point at state level, and excludes federal tax reforms which have no natural comparison group. All figures are made using the stacked event study design with "clean controls." Event time -1 is the year before the reform. Standard errors are clustered at the golfer and state of the tournament level and we present 95% confidence bands.

Figure 2.8: Participation from High and Low Tax States



(a) Low-tax State Residents

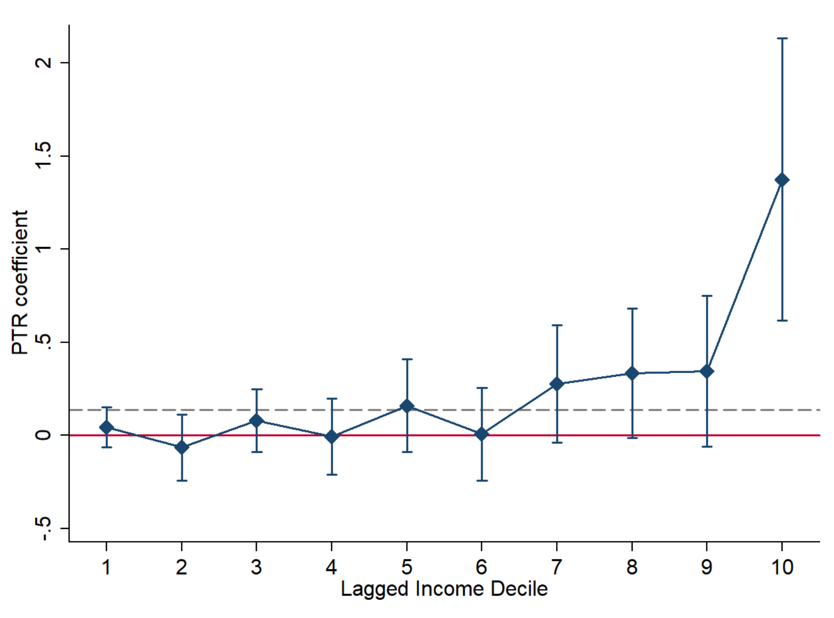


(b) High-tax State Residents

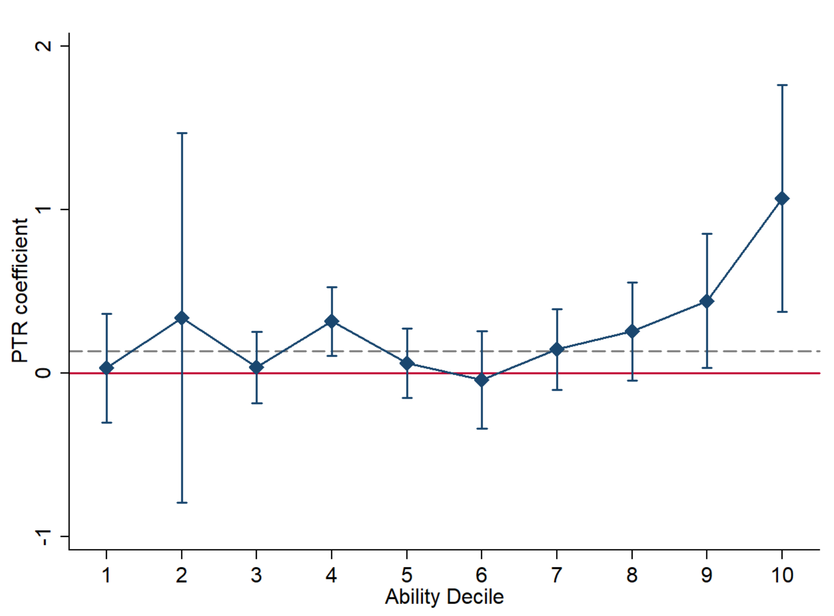
These figures show how the effect of major tax reforms on golfer participation using a stacked event study. We separately show the effect for golfers living in low-tax states (Panel a) and golfers living in high-tax states (Panel b). For golfers living in high-tax states, tax changes in the state of employment theoretically should not matter and tax changes for these golfers affect all tournaments, preventing a spatial reallocation of tournaments. For these figures, we focus on golfers in the top 25% of earnings. An event is defined as a major tax increase—more than one percentage point at state level. All figures are made using the stacked event study design with "clean controls." Event time -1 is the year before the reform. Standard errors are clustered at the golfer and state of the tournament level and we present 95% confidence bands.

Figure 2.9: Heterogeneity of the Effect of Taxes by Income and Quality Deciles

(a) Lagged Income Decile

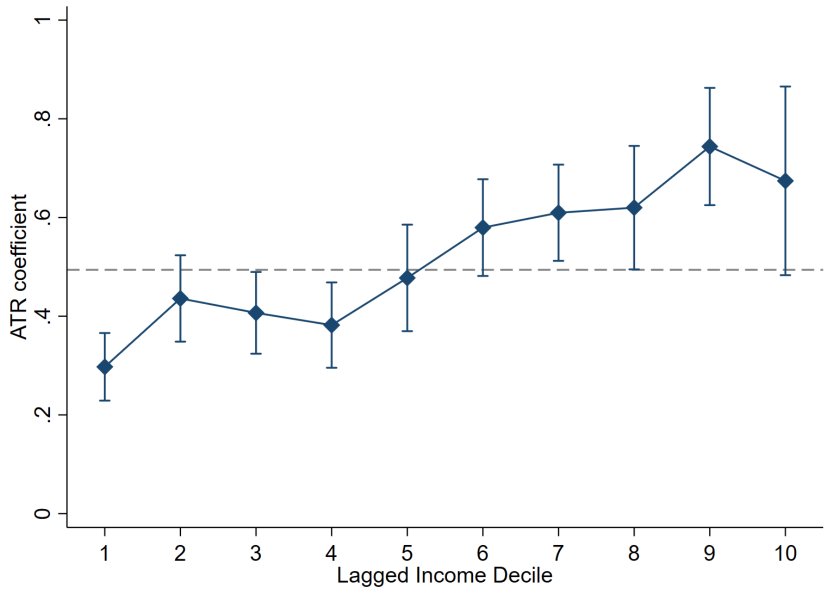


(b) Quality Decile

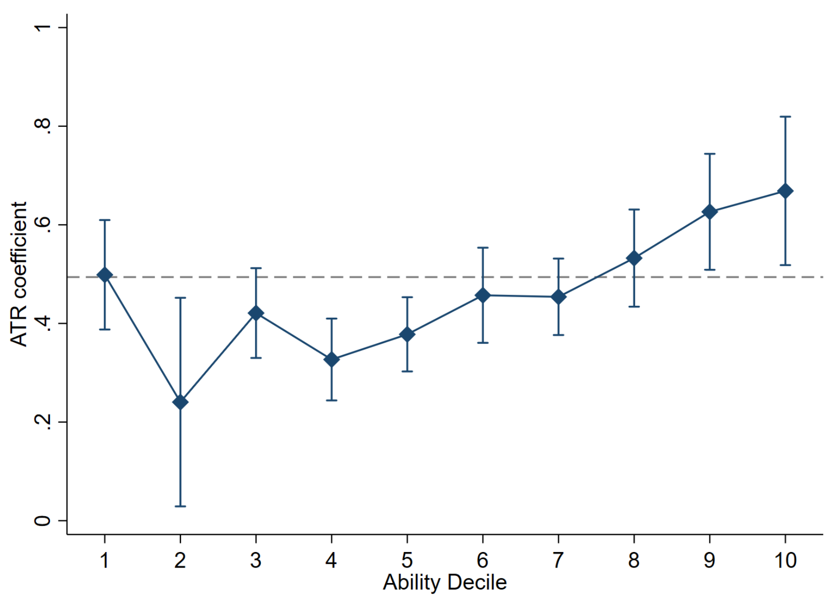


This figure estimates a specification similar to column 1 from Table 2.2 where the PTR, expected earnings, and all fixed effects are interacted with a indicators for the one year lag of yearly income deciles (Panel a) and indicators for the Lubotsky and Wittenberg (2006) index deciles (Panel b). We plot the marginal effect of an increase in the net-of-PTR for each decile. The grey dashed line represents the coefficient estimate from Table 2.2 column 1. Standard errors are clustered at the golfer and state of tournament level and bars indicate 95% confidence intervals.

Figure 2.10: Heterogeneous Effects of ATR by Income and Quality Deciles



(a) Lagged Income Decile



(b) Quality Decile

This figure regresses log(1-PTR) on log(1-ATR) in a specification similar to column 1 from Table 2.2. The log(1-ATR) is interacted with a indicators for the one year lag of yearly income deciles (Panel a) and indicators for the Lubotsky and Wittenberg (2006) index deciles (Panel b). We plot the marginal effect of an increase in the (log) net-of-ATR for each decile. The grey dashed line represents the average effect from pooling all deciles in a single regression coefficient. Standard errors are clustered at the golfer and state of tournament level and bars indicate 95% confidence intervals.

Table 2.1: State Nonresident Income Tax Collections

State	Nonresident Income Tax (in millions)	Resident Income Tax (in millions)	Nonresident Income Tax % of all Income Tax	Nonresident Income Tax Noncommuters % of all Income Tax	Year
AZ	$233.2	$3,718.9	5.90%	5.52%	2015
CA	$3,713.0	$80,338.1	4.42%	4.13%	2018
CT	$858.3	$7,420.7	10.37%	3.78%	2018
GA	$387.4	$12,176.9	3.08%	0.57%	2018
HI	$158.3	$2,110.1	6.98%	6.98%	2017
IL	$1,418.4	$15,912.9	8.18%	6.20%	2017
IA	$199.5	$3,284.3	5.73%	3.14%	2017
KS	$317.6	$2,453.2	11.46%	0.67%	2017
LA	$272.8	$3,250.5	7.74%	5.02%	2018
MD	$430.7	$8,081.3	5.06%	4.26%	2016
MI	$195.2	$8,430.5	2.26%	2.26%	2017
MN	$638.9	$10,385.5	5.80%	3.54%	2017
MS	$140.8	$1,514.6	8.51%	3.58%	2017
NJ	$1,321.0	$10,989.1	10.73%	6.08%	2016
NY	$7,087.1	$41,536.9	14.58%	1.98%	2017
OH	$282.9	$8,120.9	3.37%	3.37%	2018
OR	$492.6	$8,298.5	5.60%	0.77%	2018
PA	$836.7	$11,313.4	6.89%	6.07%	2017
RI	$231.7	$1,254.0	15.60%	1.04%	2018
SC	$474.0	$3,814.4	11.05%	6.49%	2018
VT	$65.2	$747.0	8.03%	2.85%	2018
Total	$19,755.3	$245,151.7	7.46%	3.74%	

This table presents the amount of nonresident income tax revenue collected for all states that release public statistics on nonresident income tax revenue. The portion from noncommuters is calculated by subtracting the fraction of total income earned in the state by nonresident cross border commuters as calculated in the 2018 American Community Survey, accounting for reciprocity agreements.

Table 2.2: The Effect of Taxes on the Location of Employment: Baseline Results

	(1) Baseline	(2) Earnings > 0	(3) Excludes Cut	(4) Lagged Percentile 25th-75th	(5) Lagged Percentile 75th-100th
$\Delta\ln(w^{\mathbb{E}}_{dty})$	0.009^{***}	0.011^{***}	0.013^{***}	0.017^{***}	0.019^{***}
	(0.003)	(0.004)	(0.003)	(0.003)	(0.006)
$\Delta\ln(1-\mathrm{PTR}_{isty})$	0.135^{**}	0.163^{**}	0.180^{***}	0.125^{*}	0.479^{**}
	(0.054)	(0.067)	(0.051)	(0.071)	(0.202)
$\frac{\Delta\ln(1-\mathrm{PTR})}{\Delta\ln(1-\mathrm{ATR})}$	0.494^{***}	0.554^{***}	0.489^{***}	0.508^{***}	0.727^{***}
	(0.030)	(0.034)	(0.030)	(0.038)	(0.056)
$\epsilon_{1-\mathrm{PTR}}$	0.319	0.316	0.589	0.235	0.812
Observations	285,110	233,913	232,798	131,211	70,762

This table shows the results estimating equation (2.9). Column 1 places no additional restrictions on the sample. Column 2 excludes those golfers with zero earnings in the current period. Column 3 excludes those golfers who fail to make the cut. Column 4 uses only golfers in the 25th-75th percentiles of earnings in the previous period, and column 5 uses only golfers in the 75th-100th percentiles of earnings in the previous period. Standard errors are clustered at the golfer and the state of the tournament level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.3: Verifying Spatial Distortions: The Effect of Only State Taxes vs Only Federal Taxes

	(1) Baseline	(2) Earnings > 0	(3) Exludes Cut	(4) Lagged Percentile 25th-75th	(5) Lagged Percentile 75th-100th
State Taxes					
$\Delta\ln(1-\text{PTR}_{isty})$	2.505***	3.273***	2.480***	3.129***	5.872***
	(0.552)	(0.742)	(0.523)	(0.852)	(1.107)
$\frac{\Delta\ln(1-PTR)}{\Delta\ln(1-ATR)}$	0.938***	1.015***	0.923***	1.07***	1.09***
	(0.096)	(0.074)	(0.096)	(0.080)	(0.030)
$\epsilon_{1-\text{PTR}}$	0.265	0.183	0.284	0.410	3.093
Federal Taxes					
$\Delta\ln(1-\text{PTR}_{ity})$	-0.011	-0.024	0.051	-0.075	-0.172
	(0.047)	(0.062)	(0.045)	(0.073)	(0.105)
$\frac{\Delta\ln(1-\text{PTR})}{\Delta\ln(1-\text{ATR})}$	0.532***	0.572***	0.539***	0.492***	0.667***
	(0.055)	(0.060)	(0.056)	(0.060)	(0.118)
$\epsilon_{1-\text{PTR}}$	-0.025	-0.046	0.161	-0.141	-0.293

In the top panel, we construct the PTR using only state taxes for all golfers. For the second panel, we use golfers residing in zero-tax states and apportion all income to the state of residence. We only use these golfers to eliminate any effects of deductibility of state taxes. The state tax elasticity $\varepsilon_{1-\text{PTR}}$ for the first panel is constructed by scaling elasticity estimates by $\text{dln}(1-\widehat{\text{PTR}}(\text{state})_{isty})/\text{dln}(1-(\widehat{\text{PTR}}(\text{state+federal})_{isty})$ so that both panels are comparable. Column 1 places no additional restrictions on the sample. Column 2 excludes golfers with zero earnings in the current period. Column 3 excludes golfers who fail to make the cut. Column 4 uses golfers in the 25th-75th percentile of earnings in the previous period, and column 5 uses golfers in the 75th-100th percentile of earnings in the previous period. Standard errors are clustered at the golfer and the state of the tournament level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.4: The Effect of Employment-Taxes in Paces Where They Should Not Matter: Golfers Residing in High Tax States

	(1) Baseline	(2) Earnings > 0	(3) Excludes Cut	(4) Lagged Percentile 25th-75th	(5) Lagged Percentile 75th-100th
$\Delta\ln(w^{\mathbb{E}}_{dty})$	-0.000	0.000	0.005**	0.008	0.001
	(0.003)	(0.005)	(0.002)	(0.003)	(0.007)
$\Delta\ln(1-\text{PTR}_{isty})$	-0.031	-0.044	0.030	-0.106**	-0.178***
	(0.035)	(0.043)	(0.031)	(0.047)	(0.055)
$\frac{\Delta ln(1-\text{PTR})}{\Delta ln(1-\text{ATR})}$	0.434***	0.498***	0.423***	0.466***	0.630***
	(0.032)	(0.042)	(0.030)	(0.054)	(0.066)
Observations	144,488	115,283	117,690	64,348	33,536

This table estimates equation (2.9) using only golfers residing in high-tax states relative to the state of employment. To do this, we limit the sample to golfers who would expect zero tax liability from working the state where the tournament is held. Column 1 places no additional restrictions on the sample. Column 2 excludes golfers with zero earnings in the current period. Column 3 excludes golfers who fail to make the cut. Column 4 uses golfers in the 25th-75th percentile of earnings in the previous period, and column 5 uses golfers in the 75th-100th percentile of earnings in the previous period. Standard errors are clustered at the golfer and the state of the tournament level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 2.5: Triple Difference Results

	(1) Baseline	(2) Earnings > 0	(3) Exludes Cut	(4) Lagged Percentile 25th-75th	(5) Lagged Percentile 75th-100th
$\Delta\ln(1-\text{PTR}_{isty})$	-0.005	-0.010	0.054	-0.053	0.024
	(0.002)	(0.003)	(0.033)	(0.051)	(0.108)
$\Delta\ln(1-\text{PTR}_{isty}) \times lowtax_{isty}$	0.279***	0.331***	0.250***	0.335***	0.723***
	(0.068)	(0.051)	(0.067)	(0.088)	(0.194)
Marginal effect: low-tax states	0.273***	0.321***	0.304***	0.282***	0.747***
	(0.053)	(0.079)	(0.065)	(0.089)	(0.224)
$\epsilon^{H}_{1-\text{PTR}}$ (total: high-tax)	-0.012	-0.019	0.183	-0.098	0.039
$\epsilon^{L}_{1-\text{PTR}}$ (total: low-tax)	0.620	0.628	0.961	0.539	1.308
$\epsilon^{DDD}_{1-\text{PTR}}$ (differenced: low-tax)	0.633	0.648	0.790	0.641	1.266
Observations	285,110	233,913	232,798	131,211	70,762

This table shows results estimating equation (2.9), where the participation tax rate and the expected earnings are interacted with an indicator for living in a low-tax state, where low-tax state is defined by having positive expected tax liability in the state of employment. Column 1 places no additional restrictions on the sample. Column 2 excludes those with zero earnings in the current period. Column 3 excludes those who fail to make the cut, column 4 represents those in the 25th-75th percentile of earnings in the previous period, and column 5 represents those in the 75th-100th percentile of earnings in the previous period. The elasticity $\epsilon^{H}_{1-\text{PTR}}$ represents the elasticity for high-tax states, which comes from the estimates on the non-interacted net-of-PTR coefficient, $\epsilon^{L}_{1-\text{PTR}}$ is from the marginal effect of living in a low-tax state, and $\epsilon^{DDD}_{1-\text{PTR}}$ is taken directly from the interaction between the net-of-PTR and low-tax indicator. Standard errors are clustered at the golfer and the state of the tournament level. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Table 2.6: Robustness to Individual Growth Heterogeneity

	(1) No Trend	(2) Linear Trend	(3) Quadratic Trend	(4) Double Difference
$\Delta\ln(w^{\mathbb{E}}_{dty})$	0.011***	0.012***	0.012***	0.008*
	(0.004)	(0.004)	(0.004)	(0.004)
$\Delta\ln(1-\widehat{\text{PTR}}_{isty})$	0.163**	0.156**	0.160**	0.149*
	(0.067)	(0.070)	(0.072)	(0.080)
$\frac{\Delta ln(1-PTR)}{\Delta ln(1-ATR)}$	0.554***	0.552***	0.545***	0.561***
	(0.034)	(0.035)	(0.035)	(0.035)
$\epsilon_{1-\text{PTR}}$	0.316	0.303	0.311	0.284
Observations	233,913	233,913	233,913	198,037

This table shows the robustness of the results when estimating equation (2.10). Column 1 presents the results from column 2 of Table 2.2. Column 2 includes a golfer specific linear time trend. Column 3 includes a golfer specific quadratic trend. Column 4 represents a "difference-in-differences" estimate for individual heterogeneity (Kniesner and Ziliak, 2015). Results using only top golfers are similarly robust. Standard errors are clustered at the golfer and the state of the tournament level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Chapter 3 Is Altering the Tax Base Different than Changing the Tax Rate?[1]

3.1 Introduction

Tax systems face the important question of what the composition of the tax base and what should be excluded. For the income tax, many countries exclude imputed rent from taxable income, in the United States, some states and municipalities differ on whether to include services in the general sales tax base. Perhaps the most studied phenomena in the United States, before *South Dakota v. Wayfair*, goods purchased online were excluded from the tax base if the online retailer did not have physical nexus in the destination state. The policy rationale of these differences in the tax base can range from encouraging home ownership to reducing administrative burdens for firms. When governments decide on the tax base Yitzhaki (1979) describes the optimal tax base as a trade off between the benefit of expanding the tax base with the administrative costs of adding another good to the tax base, which can justify a more narrow base if the good or service is costly to administer. However, while the theoretical tax literature suggests that the choice of tax base, and of particular importance the consumption tax base, is important, little empirical work examines specific changes in the consumption tax base and how consumers and firms respond to said changes.

Better understanding the efficiency consequences of changes in the tax base are especially important for state and local tax systems as the sales tax base has

[1]Calculated based on data from The Nielsen Company (US), LLC and marketing databases provided by the Kilts Center for Marketing Data Center at The University of Chicago Booth School of Business. The conclusions drawn from the Nielsen data are those of the researchers and do not reflect the views of Nielsen. Nielsen is not responsible for, had no role in, and was not involved in analyzing and preparing the results reported herein.

become increasingly narrow. Consumer preferences and administrative costs have change substantially in the last 40 years. This is largely driven by the shift from the consumption of goods to the consumption of services, which many states still exclude from their tax base, and also by the increasing use of digital services. In contrast, the consumption tax base has largely been static. While some states have expanded their tax base by choosing to tax limited services in response to the gradual erosion[2] most have instead chose to increase rates on an increasingly narrow base. This is despite the optimal tax base suggesting that tax bases should instead broaden as the substitution between the untaxed sector (services/digital services) and the tax base (goods)(Wilson, 1989) increases.

The choice of tax base can also be used a way to make the tax system less regressive even though there is some efficiency loss, which is done quite extensively for consumption taxes. In the United Kingdom, their VAT system has a zero rating for unprepared food, books, and children's clothing primarily as a way to make these goods more accessible to lower income households.[3] In the United States, because consumption taxes are almost entirely decentralized, states vary greatly in what they choose to exempt. The most common exemptions for equity concerns are drugs (both prescription and nonprescription), unprepared food, and clothing all of which are targeted at lower income or more needy households. However well intended these exemptions are, the tax base is likely not the best place to address these issues and other forms of redistribution are more effective (Mirrlees et al., 2011).

In order to better understand the effects of changing the tax base, I look at the removal of grocery taxes in South Carolina and West Virginia. The reasons for this is twofold, the first is that food expenditures represent large budget shares for

[2] To a certain extent, states abilities to broaden the base have been limited by Supreme Court rulings such as *Quil Corp v. North Dakota*.

[3] The United Kingdom is known for having very unusual zero rating rules. Shelled nuts are zero rated, while unshelled nuts are not, motorcycle helmets are also zero rated.

households and therefore would be a meaningful change for most individuals and the second is that data on food sales is readily available. I utilize the Nielsen Retail Scanner data that reports recorded sales for over 3 million UPCs in over 40,000 retailers in the United States. This dataset also contains detailed geographic information in regards to retailer location including 3 digit zip code and county. I then use the variation across time and across states from changes in the tax rate and the inclusion of food in the tax base. Using this variation, I follow a similar identification as Fetter and Lockwood (2018)[4] and use state border pair identification based on retailers locating in state border counties. This method can be viewed as a generalized case study analysis of each state border pair, which directly controls for any unobserved changes within that border area. I also use this method in a dynamic setting to estimate the effect of changes in the tax rate in a distributed lag model (Fuest et al., 2018; Suárez Serrato and Zidar, 2016).

The use of borders to examine policy differences goes back to Card and Krueger (1994), who use the border between New Jersey and Pennsylvania as a case study analysis of the effects of the minimum wage on employment. The idea of comparing geographically similar areas for labor market outcomes was generalized further in Dube et al. (2010), where they use county pairs to examine the effect of minimum wage on employment and earnings and find that failing to account for spatial heterogeneity leads to biased estimates of the effect of the minimum wage on employment. In other studies, border policy differences have been used to examine firm location (Holmes, 1998; Rohlin et al., 2014), tax competition (Agrawal, 2015), and old age insurance (Fetter and Lockwood, 2018). I extend these applications to the rate at which food is taxed and the inclusion of food in the tax base. This is particularly useful in this application as demand for unprepared food may differ across local areas and confining

[4]Also see Thompson and Rohlin (2012) and Rohlin (2011).

it to border areas at least crudely controls for localized differences in preferences, prices, and local economic conditions. It also seems unlikely that state policies would directly influence the sale of unprepared food outside of the food tax rate, outside of small exemptions generally targeted towards soda or candy.[5]

Using the variation in food tax rates, I find evidence that the elasticity of sales revenue with respect to the food gross tax price is less than one for retailers that primarily sell food using state border pairs. This is around half the size of traditional panel methods suggest. Even with alternative specifications the elasticity is still below the level found with simple panel models. The effects are also smaller than results found in other studies that examine sales differences at borders (Knight and Schiff, 2012; Fox, 1986; Baggio et al., 2020). While these results do suggest that consumers respond as expected to changes in the tax rate of food, the results suggest that increasing the tax rate on food would be an effective way to increase revenue. In addition, for the distributed lag model, I find results that are largely consistent with the panel estimates looking at when the tax changes becomes the statutory rate and when the tax change is announced. When examining the removal of food from the tax base I find mixed evidence on the effects of the removal. When examining each removal individually, it appears that West Virginia did see a significant increase in retailers that primarily sell food. Because there is no definitive effect of the choice of tax base and the rate elasticities are small this suggests that the efficiency costs of the removal of the tax base are potentially high, but because there are significant equity concerns, it cannot definitively be said that food should remain excluded.

Given that the analysis is of border areas, it is likely where we would expect the effects of any consumption tax to be the largest. This is because the effect likely consists of two distinct effects, a demand effect from the relative price change and

[5]In order to address this, I exclude soda and candy sales from the estimation.

any shift in cross border shopping. While the two effects are not uniquely identified, both effects represent real responses and are important for tax revenue purposes in these areas. While for the interior counties, these estimates represent the upper bound on any demand effect as cross border shopping is close to zero. In addition, the elasticity inclusive of the cross border and demand effect is the policy relevant elasticity for border areas (Devereux et al., 2007). Given the elasticities are small relative to previous literature, this upper bound would be suggestive of a relatively high efficiency cost of food taxation. This is even suggested when one examines changing the base to exclude food taxes, as there does not seem to be a definitive effect of removal. Because the base changes are only targeted at food, any general equilibrium effects of a simultaneous tax change on other goods are not present and thus may be different from changes in rates. While I do not find evidence that this is the case, the lack of any clear effect is consistent with Mirrlees et al. (2011) and food exemptions appear to have measurable efficiency costs.

While the food tax itself is controversial, other base broadening measures are decidedly less so. There is no clear reason why barber services are untaxed in many states while razors and shaving cream remain taxed when both lead to the same service being provided. Understanding the efficiency consequences of these exemptions is important, as traditional brick and mortar retailers have struggled from both e-commerce and the overall economy wide consumption shift from goods to services, which absent the preferential tax treatment, may have been less pronounced. This paper suggests that these efficiency costs are potentially large and governments while well intentioned, may be using a less than ideal policy measure by choosing to continue to shift the tax burden burden to the ever narrower existing tax base.

3.2 Institutional Details

In general, the taxation of food is a controversial policy for states to adopt. This is due to concerns about the progressivity of the tax, as it is thought to disproportionately hurt low to moderate income households where food constitutes a larger budget share. In contrast, traditional economic theory would suggest that optimal tax systems would consist of a broad base with lower rates (Kopczuk, 2005)[6]. Recently, several states have sought to include or expand the taxation of groceries including New Mexico, Connecticut, Utah, and South Carolina[7] tax while other states have tried to create constitutional protections against such taxes (Oregon and Washington). This is also a relevant issue for countries with a VAT, as many choose to zero rate or exempt food from the VAT base.

The primary concern around the adoption of food taxes is focused about concerns around equity. Retail sales taxes are thought to disproportionately impact lower income households as consumption makes up a larger portion of income than higher income households (Slemrod, 2006). This is even more so the case with taxation on food, as expenditure on food as a fraction of income in 2015 is almost 40 percent higher for households at the bottom 20 percent of the income distribution compared to those households at the top 20 percent (BEA, 2015). In addition, food taxes are often highest in states that already have relatively high shares of households in poverty, which amplifies concerns about fairness (Newman et al., 2011). Food taxes have also been linked to substitution between food at home and food away from home by lower income households (Dong et al., 2020). This is viewed negatively because food away from home is generally regarded as more calorically dense and less healthy

[6] This is even more important in an instance with high substitutability with taxed and untaxed goods much like prepared and unprepared food

[7] Notable examples among these states include Connecticut increasing the tax on prepared food in 2019 and Utah passing and repealing a food tax increase in 2019.

than food prepared at home (Mancino et al., 2009) and have been linked to childhood obesity (Taveras et al., 2005; Gillis and Bar-Or, 2003). Given the long term negative consequences of poor diet and obesity there may be health benefits to the elimination of food taxes.

Currently, 36 states and the District of Columbia tax food at some capacity, with a majority of these states choosing to tax soda or candy to discourage consumption (Cornelsen et al., 2014). Narrowing down the scope to those that tax groceries specifically, figure 3.1 shows the distribution of food specific taxes in the United States in the first quarter of 2006 and 2017. State food taxes are more common in the southern and central portions of the US as zero states on both the west coast and the north east include food in the tax base.[8] Rates vary from a low of one percent in Illinois to a high of seven percent in Mississippi. The most notable difference between 2017 and 2006 is the exclusion of two states that previously taxed food at a higher level in South Carolina and West Virginia. The other small, but noticeable difference is the general decline in the rate of the food tax. While exceptions exist like Kansas and South Dakota, most states reduced their tax rate on food in the sample period and some by substantial amounts.[9]

Specific studies regarding food taxes are fairly rare. Empirical work in the US is primarily focused around corrective taxes on unhealthy food such as soda or snack food (Cawley and Frisvold, 2017; Fletcher et al., 2010). There are some case studies on food taxes in West Virginia, which find that food tax rates decrease the sale of food in border counties by a large amounts (Tosun and Skidmore, 2007; Walsh and Jones, 1988). While these studies do give some compelling evidence of strong border

[8]The reasoning behind why this is the case is hypothesized Newman et al. (2011). Taxes in several southern states have large shares of tax revenue from consumption taxes and low property and income taxes as a way to shift the tax burden down the income distribution.

[9]Arkansas's rate fell from six percent to one and a half percent, Utah reduced their rate by three percent.

effects, any connection to causality is difficult due to the lack of a comparison group as they only examine time series variation in West Virginia. This paper improves on the previous work by both providing an analysis that aggregates case studies across all states with food tax differences and by examining the effect of removing food from the tax base.

3.3 Conceptual Framework

Here I address the behavioral issues of why a change in the tax rate may cause different responses then a change in the tax base. A natural starting point on why this distinction matters is to start with Ramsey (1927) where one good is allowed to be untaxed (leisure). In a world where all goods, including leisure, can be taxed a commodity tax is equivalent to a lump sum tax and it is non-distortionary thus any change in the tax rate will not create any excess burden. In the case where only one good is untaxed, then the optimal tax rate depends on whether the good is a substitute or complement with leisure and the optimal tax rate is uniform only if all goods are equally complementary with leisure and under the assumption of zero cross price elasticities with each other (Deaton, 1979). Thus highlighting that the removal of a good in the inclusion of the tax base can have substantial effects on both consumers and optimal tax policy.

Additionally, in the income tax literature the differences between tax rate and tax base changes is further amplified. In Kopczuk (2005), he finds evidence that changes in the tax base are a major factor in determining the ETI highlighting that access to deductions, or reductions in the tax base, allow income to be more responsive to tax rate changes finding that deductions and taxable income are strong substitutes. Both Kopczuk (2005) and Slemrod (1994) provide evidence that suggests base broadening through limiting deductions can be an efficient alternative to raising taxes in a progressive tax system. In a similar manner, goods or services exempt from the consumption tax base may be substitutes for goods subject to taxation and by increasing the scope of the tax base, increases tax revenue by both bringing that good into the tax base and through limiting shifting of consumption outside the tax base.

Another issues that is relevant in this case is policy makers must determine what is included in the tax base and have to draw lines that may make products that are very similar in characteristics have different tax rates (Gillitzer et al., 2017). The economic reasoning behind these lines can vary from administrative costs , sales from nonprofits versus firms, or due to statutory exclusions in the tax base like services. In this context, the tax treatment of unprepared food versus food ready to eat can lead to products in the same store, such as fried chicken being taxed differently if it is served hot (taxed) or cold (often untaxed or at a reduced rate) often occupying a similar physical location in store. This can even lead to producers responding by creating particular products that occupy characteristics of a tax good, but characteristically fall into the untaxed space, called tax driven product innovation (Gillitzer et al., 2017). Any changes in the tax base, either in including or excluding additional goods and services leads to line drawing, where consumers and producers will try to move consumption and productions to the tax preferred side of the line.

In an environment with many tax rates and goods can be taxed at different rates, a rate differential between the taxed and untaxed good can allow for different rates on each side of the characteristic line leading to smaller distinction between a rate change and an exclusion from the tax base. However, there is reason to believe that salience may play a role when it comes to the tax base. In Cabral and Hoxby (2015), property taxes being obscured by escrow payments lead to higher property tax rates, while Goldin (2015) shows how low salience can influence optimal tax policy. If consumers are more familiar with the tax base than the specific tax rate a good faces in a multiple rate setting, this could lead to “schmedulng” (Rees-Jones and

Taubinsky, 2020) where consumers assume the average tax rate from a trip would be the statutory rate at which all products are purchased. If this is the case, then the difference between a change in the tax rate and a change in the tax base has another dimension in which a difference could be encompassed.

3.4 Data

The data used in this paper comes from the Kilts Nielsen Retail Scanner data provided by the Kilts Center at the University of Chicago. This data set contains weekly price and quantity sold data on over 3,000,000 UPCs within 125 different product groups, across 35,000 major retailers located across all 50 states. This dataset is unique in that it provides detailed geographic information such as three digit zip code and county identifiers which are used to identify border areas. As the data is provided at the upc-product-week-store level, I aggregate the data to the quarter-store level to reduce noise that may be present in smaller time periods from weather or other unobservable changes and exclude soft drinks and candy from the estimation. I use all the available observations from 2006-2017 to conduct my analysis.

I then use the US Census Bureau County Adjacency File to match retailers with their own county and all the adjacent counties that the county shares a border with. Next I keep one observation for each unique state border that the retailer shares, so if the retailer is in a county that has a border with two (or more) different states then each retailer in that county will have an observation for each state pair.[10] With the sample constructed, I then use food tax rate data from the Book of the States supplemented by state legislative acts to determine precisely when a tax change is announced and when it is effective. Finally, in order to control for economic

[10]These instances are indeed rare, as few counties share a border with multiple states. Most that do have relatively low populations, which suggest few sample observations, but there are some notably exceptions. Shelby County, Tennessee (Memphis) comes to mind.

conditions I bring in county level controls for personal income from the Bureau of Economic Analysis, population from the Census, unemployment from the Bureau of Labor Statistics, and SNAP participation data from the Food and Nutrition Service of the United States Department of Agriculture.

Summary statistics are provided in table 3.1 and in table 3.2. It is important to note that the summary statistics are provided at the observation level, not the county or a more aggregated level. This is important to keep in mind as larger counties have more retailers represented in the dataset such as Cook County, Illinois or Fairfax County, Virginia. With this said, overall retailers in border counties do not seem substantially different from the entire population of retailers. There does seem to be some discrepancy between retailers that border states that tax food and retailers in states that tax food. Once again this is likely driven by a select high density, high population areas with several retailers than a general trend as a whole. In order to better address the emphasis on food, I limit the sample to retailers that would be standard places to purchase food which I define as retailers classified as either food stores or mass merchandisers. This is done to best limit the effect of food taxes on food and not on trips to smaller stores where food ends up being bought as a secondary purchase, like one might expect from a drug or convenience store. All retailers are included in additional specifications as a robustness check and the results are larger in magnitude, but still consistently smaller than previous studies.

3.5 Methodology

An ideal design for examining any tax effect, whether a rate reduction or a change in the tax base would involve having a treatment and control group where retailers are randomized to receive one tax rate or are required to collect taxes while the others receive a different tax rate or are not required to collect taxes. Unfortunately, this scenario is unrealistic as governments can't choose which stores are required to collect taxes. Instead we have to rely on geographic variation in the rate and measurable changes in the sales tax base. In the context of retailer level sales and state tax variation, this is inherently difficult because prices, preferences, and accessibility can vary significantly across states and across time. As a result, it would make sense to use states that are likely to have similar characteristics as a way to create similar comparison groups. Because states that share borders are likely to be similar, at least along the border, they provide a transparent and plausible comparison group. The primary threat to identification would be any unobserved time varying state level policies that directly effect the sale of food in that state. Because local rates would not be exogenous to state, local, and border conditions (Agrawal, 2015) they are excluded to limit the variation to only come from the most exogenous source, which is state tax changes. Given that some state borders are large, such as the Texas-Oklahoma border, there could also be concerns that the treatment and comparison groups are too geographically dissimilar. However excluding the western states with the longest borders does not drastically alter the results.

3.5.1 Border Pairs

State border pairs are defined by counties that share a border with a different state. State border pairs allow me to use a similar geographic area with outcomes that should only change by the variation in state level policies and not due to unobserved economic or cultural differences. This is controlled for by the inclusion of border-pair by quarter fixed effects. For example, a retailer in Yuma County, Arizona in January 2014 would have a Arizona-California by quarter 1 2014 fixed effect. In the rare instance of a county having two separate state borders, say Clark County, Nevada, the retailer is included in the sample twice, once with a California-Nevada by quarter fixed effect and a Arizona-Nevada by quarter fixed effect. This is in contrast to Dube et al. (2010), where they use county border pair identification, where each county is matched to each border county in the other state. While in the context of aggregate data this may be a reasonable approach to more closely match labor markets, in the context of individualized data, it is less clear that inflating the sample without adding any additional variation is an improvement.

The counties that have food tax differentials and are thus potentially included in the sample are in figure 3.2. We can see that the middle and southern parts of the United States are primarily where the variation comes from. It is also clear that in the Western United States, counties are larger than in the eastern portion of the United States. This could mean that in the west using a county with a state border is too loose to define as a border area and may include retailers that are far from a untaxed or taxed border and may bias the results downward. In order to show that these outliers are not a primary driver of results I perform a robustness check without the westernmost states and the results are generally unchanged.

The main advantage of the border pair analysis over a standard border panel design is the ability to control for local economic conditions. Income, preferences, economic conditions, and consumption patters can vary across geographic areas. An important assumption in the panel border design is that any unobservable changes in border areas across time are homogeneous. This assumption would be plausible if food tax rates were distributed in a less geographically concentrated manner. However food taxes are largely concentrated in southern states and a few states in the west, suggesting that a specification that controls for local economic activity may be more important. With the border pair method, it controls for heterogeneous time effects across different regions but it also allows for potentially two spillovers. The first of which is if taxes increase in region A, that drives cross border shopping from region A to region B, generally this is called the amplification effect (Dube et al., 2010). There is also the attenuation effect, where in response to a tax increase in A, stores in B may raise prices to match the effective prices in region A. The importance of both effects will be discussed further when the empirical model is discussed. However, controlling for local conditions should be more beneficial than introducing both the amplification and attenuation effect.

3.5.2 Rate Identification

In order to identify the effects of changing the rate at which food is taxed on retailer sales revenue I use the following equation:

$$ln(Y_{ipt}) = \beta ln(1 + \tau_{st}^{f}) + \boldsymbol{X_{ct}'}\boldsymbol{\gamma} + \delta_i + \delta_{pt} + \varepsilon_{ipt} \tag{3.1}$$

Where $ln(Y_{ipt})$ is the natural log of food sales in retailer i, in state border pair p and in quarter t. This is calculated by taking the sum of all food products in a retailer excluding candy and soft drinks[11]. The expression $ln(1 + \tau_{st}^{f})$ is the log gross of tax price for food in state s at time t, $\boldsymbol{X_{ct}}$ is a vector of county controls for economic conditions such as unemployment rate, personal income, snap participation rate, and population, δ_i are retailer fixed effects, and δ_{pt} are border-pair by quarter fixed effects. The parameter of interest, β is the elasticity of retail sales with respect to the gross tax price. The key identifying assumption to interpret the estimates as causal is that the difference in the tax rate are unrelated to differences in residual sales at the retailer level. Given these are state level decisions and for most states border areas do not represent a disproportionate share of the population, this is a plausible assumption. A secondary but important assumption is that prices in the state with the tax change do not *disproportionately* respond to the tax change.

In most similar specifications in the tax literature, some measure of prices are included due to the incidence of the tax changing the pretax price or explicitly

[11]This is because these foods tax treatment is much different from other groceries. For a detailed discussion on candy see McCullough (2018) and for soft drinks see Chriqui et al. (2008).

stated to be equivalent without any meaningful justification. However recent research has suggested that local price shocks, much like the incidence response to state tax changes, are largely dampened by uniform pricing of large retailers (DellaVigna and Gentzkow, 2017). Because these results are found using identical data as the data used in this paper, it seems unlikely that I would see a significant adjustment in price that would alter the results in a meaningful way, thus making the attenuation effect discussed earlier a small problem. The state pairs provide a further restriction that the prices would have to change in a geographically restricted area, which seems even less likely in the context of the DellaVigna and Gentzkow (2017) paper.

Because prices are not likely to adjust significantly to grocery tax differentials, it is likely that any distortion that exists in a border setting is likely to amplify results relative to the interior of the state. This is because the only additional demand effect will be the relative price change and any demand that changes to the number of crossborder shoppers. This is in contrast to Dube et al. (2010), where the minimum wage legislation equalizes the price of labor across the different jurisdictions, thus dampening the effect of minimum wages on employment in border areas. However because large retailers are unlikely to adjust prices across jurisdictions to equalize the net of tax price, these estimates should be thought of as an upper bound on the elasticity of food with respect to the grocery tax rate. In addition, taking the results from Devereux et al. (2007) for the revenue maximizing tax rate, the policy relevant elasticity is the one that contains both the cross border effect and the demand effect, which my estimates provide.

While the ability to flexibly control for local economic conditions and trends is an important advantage in the border pair strategy, it does cause some issues for the identification of estimates. While cross border shopping is important for the policy relevant elasticity, because border pairs will pick up the change in the state that changes it tax rate as well as any decrease in the cross border state, then the effect of a change in the rate will be biased upwards from the contamination from the treatment state from cross border shopping. Whether this identification is preferred to more traditional methods depends upon the importance of controlling for local economic conditions versus contamination from cross border shopping. Ideally both should be controlled for to ensure precise estimates of the effect of the change in the food tax rate.

In order to examine the dynamics of statutory changes in the food tax rate, I also use a distributed lag model to estimate the dynamic impact using the same border pair identification. This allows for variation to not only come from timing, but also from different magnitudes. Following Fuest et al. (2018) and Suárez Serrato and Zidar (2016):

$$ln(Y_{ipt}) - ln(Y_{ipt-1}) = \sum_{j=-8}^{8} \beta_j [ln(1+\tau_{s,t-j}) - ln(1+\tau_{s,t-j-1})] + \Delta \boldsymbol{X'_{ct}} \boldsymbol{\gamma} + \delta_i + \delta pt + \varepsilon_{ipt} \tag{3.2}$$

The estimated coefficients $\hat{\beta}_j$ are the effect of leads and lags of a change in the food tax rate on quarterly retailer food sales. Starting from $j = -8$, the coefficient βj is added to all previous coefficients $\sum_{j=-8}^{\hat{j}} \beta_j$, where $\hat{j}$ represents the current value of j, to construct the cumulative effect at each period j. The interpretation of the $\sum \hat{\beta}_j$ up to that time period j are identical to event study estimates (Schmidheiny and Siegloch, 2020) where the leads are analogous to the pre-trends test in an event study, while the lags can directly be interpreted as the causal effect of the policy.

3.5.3 Base Change Identification

Besides measuring the response of changes in the rate of taxation, there is a unique opportunity to see the effects of the removal of a part of the tax base. This empirical strategy follows a Generalized Difference-in-Difference (GDiD) (Goodman-Bacon, 2021) framework using the repeal of the food tax in South Carolina in November of 2007, and West Virginia in July of 2013. I use South Carolina and West Virginia retailers in border counties as the treatment group and all other states with food tax border differentials as the comparison group. It is important to note that in the standard two way fixed effects difference-in-difference designs create bias estimates of the dynamic treatment effect due to contamination between early adoption and late adoption groups into the control group. In my design, the identifying variation comes from border pair variation, where stores are compared to stores on the other side of the state border. Because West Virginia and South Carolina do not share a border, this should not cause any contamination of the control group and the results can be directly interpreted as causal. I estimate the following equation on a balanced panel of retailers:

$$ln(Y_{ipt}) = \sum_{y=-6}^{-2} \pi_y D_s 1(t - T_s^* = y) + \sum_{y=0}^{10} \rho_y D_s 1(t - T_s^* = y) + \boldsymbol{X'_{ct}} \boldsymbol{\gamma} + \delta_i + \delta_{pt} + \varepsilon_{ipt} \quad (3.3)$$

Where D_s is a dummy variable for whether the retailer is in a state s that repeals its food tax and $1(t - T_s^* = y)$ is an indicator which is equal to one for the retailer when the quarter t is y quarters away from the event quarter T^*. For example, South Carolina repealed its food tax in the fourth quarter of 2007. If a retailer is observed in the second quarter of 2008, then the corresponding y value is 2, if it is observed in the 1st quarter of 2007, then the y value is -3. The vector $\boldsymbol{X_{ct}}$ is similar to the vector in equation 3.1, but also includes state quarter fixed effects[12] to correct for seasonality in the treatment effects. Because the period of $y = -1$ is omitted, the π coefficients represent the evolution in sales before the taxation of food is repealed in states that taxed food while the ρ coefficients will represent the differences in quarterly food sales once food taxes are repealed. This identification is useful for a few reasons, the first of which is that it provides suggestive evidence of the standard parallel trends

[12]As in Alabama first quarter, not Alabama first quarter 2008.

assumption as the π set provides the pre-repeal difference in outcomes between the treatment and the control group. The second benefit is it allows us to see a dynamic view of the treatment effect with the ρ coefficients versus an average effect over time with a standard difference in difference approach. This can give us insights about how quickly individuals and firms adopt to changes in the tax base.

3.6 Results

3.6.1 Rate Elasticities

The results of the regression specified in equation 3.1 are presented in table 3.3. The first column shows the effects of food taxes when traditional estimation methods using border counties which tax food and include individual and time fixed effects which I will refer to as traditional panel methods. The elasticity here is close to -2.2, which can be interpreted as a one percent increase in the gross tax price of food decreases food sales for the retailer by 2.2 percent. Columns 2-5 show the results using the state border pair identification method in which each specification gradually includes more controls. Overall, I consistently find a sales elasticity between -.9 and -1 for a point estimate, although the results are only marginally significant. It is clear that without fully controlling for local areas, estimates of the effect of food taxes can be misleading, as the estimates are roughly half of those without the border specific time fixed effects. This is important to note because this should be thought of as the upper bound on the effect of food taxes due to the amplification effect and suggests that controlling for local conditions is important for properly identifying this elasticity. Even under the assumption of zero cross border shopping, where the interior effect equals the border effect, increases in the food tax rate would lead to large increases in revenue for the state government. With even a small cross-border shopping effect, the revenue power of unprepared food taxes is large.

Comparing these results to recent literature on "cross-border shopping" with the internet (Einav et al., 2014) and with the lottery (Knight and Schiff, 2012) the results are significantly smaller. Einav et al. (2014) finds that a one percentage point increase in the sales tax rate increases online purchases by around two percent, while Knight and Schiff (2012) finds price elasticities between 2.78 and 3.53. This is not unsurprising, as food is much less elastic than internet sales or lottery tickets. There is also the issue of the cost of avoidance which is, or perhaps more accurately was, very low with internet sales and lottery tickets can be purchased in large quantities with relative ease, as gas stations and small retailers carry large selections of lottery tickets and there are no significant capacity constraints for an individual. This is in sharp contrast with food, where traveling long distances runs the risk of spoilage and food is more voluminous and less valuable than lottery tickets or cigarettes.

In order to examine the dynamic effects of changes in the tax rate, I present the cumulative effects of equation 3.2 in figure3.3.[13] Looking a the 8 leads of panel (a), we see some signs of a worrying, negative pretrend when looking only at the effect of the statutory rate on retail sales but when looking at the 8 lags, changes in the sales tax rate seem to have a consistent negative effect. In order to better investigate the potential causes of the negative pretrend before the new rate becomes the statutory rate, I modify the definition of the tax rate to match the quarter when the legislation that changes the tax rate is signed by the governor instead of the quarter the rate becomes the legal rate. Because the timing of the statutory tax change is predetermined, it should be largely uncorrelated with other drivers of retail food sales, especially when seasonal effects are already controlled for. Looking at panel (b), there is a noisy, but generally flat pretrend in the 8 leads before the announcement of the tax change followed by a gradual decline starting in period 0.

[13]coefficient estimates can be found in table 3.6.

This result is largely consistent with related papers in the cigarette tax literature, that find sizable decreases in cigarette consumption between when a tax is enacted and before it goes into effect (Gruber and Köszegi, 2001; Rees-Jones and Rozema, 2019). Under the assumption that is no systematic relationship between the length between the announcement and retail food sales in border areas, the cummulative effect from the announced rates can be interpreted as the causal effect of changing the food tax rate. This highlights the importance of both the timing of the enactment of tax changes, as well as the timing of the implementation of the tax change. Ultimately the distributed lag model finds results in both specifications that are similar, but not identical to estimates found it table 3.3 with an implied elasticity slightly larger in magnitude than -1.0.

3.6.2 Robustness and Falsification Test

In order to test the validity of my empirical specification, I do a falsification test using state border pairs where both states do not tax food and use the sales tax rate, denoted $ln(1 + \tau^{g}_{st})$, to estimate the effect on sales taxes on quarterly food sales, with the idea being that food should be generally unaffected by an increase in an unrelated tax. This assumption depends upon the cross price elasticity between unprepared food and all other goods in the consumption tax base (Agrawal and Hoyt, 2018). This falsification test is implicitly assumes an aggregate zero cross price elasticity between other goods in the sales tax base and untaxed food. It does not explicitly rule out "shopping" trips where individuals cross border shop for preferential tax rates on goods and also purchase food at the same time. The results are presented in table A.6 and are noisy, but do not suggest that the sales tax rate is a determinant of food sales for retailers. The columns in this table have identical specifications as their corresponding column in table 3.3 to make direct comparisons with the estimates using food tax rates. The purpose of this exercise shows that the results are not driven by underlying differences in the tax structure, such as states with more emphasis on consumption taxes consuming less than other states or that the results are primarily driven by bundling shopping trips in lower tax states, because the food tax rate is often the same as the retail tax rate. Because these differences do not exist in states that do no tax food, there is no clear reason why they would exist when states choose to include food in the tax base.

I also provide a series of robustness checks to ensure that the results are not driven by any notable geographic features or model selection on my part. Table 3.5 presents the results of some important checks. The first concern is that states which have geographically large counties might attenuate the results by including border areas that are distant from the border. Column 1 addresses this by removing states that are traditionally thought of as “western" states (Utah, Idaho, South Dakota, and their neighboring states) because states in the west seem to have larger counties and less connected infrastructure, and longer driving times between borders, as a whole. Column 2 is also has a similar rationale but I use a more agnostic exclusion by removing any states west of the Mississippi River. This also reduces several borders that have river crossings, which would limit the ability for consumers to cross border shop by only having a few crossing points.

Overall, the results are nearly identical between columns 1 and 2 and the specification in table 3.3 column 5, suggesting there is nothing systematically different about western states geographic differences attenuating the results. Columns 3 and 4 include all retailers available in the sample such as gas stations or drug stores while the difference between the two is based whether it is estimated using equation 3.1 or if it is done using traditional panel methods with border counties. While the magnitudes of the estimates are larger with the non traditional food retailers included, the difference between the methodologies is fairly consistent. The state border-pair by quarter fixed effects reduce the estimated elasticity by 1, once again suggesting heterogeneous time effects or changing economic conditions are overstating results.

For column 5, I present estimates of equation 3.1 as if it were a balanced panel. This inherently means that stores that opened more recently, store closures, and new entrants to the panel will be excluded from the estimation. The results are again larger and significant much like the inclusion of all retailer types, but the same pattern holds; elasticity estimates one unit larger when the traditional panel methods are used instead of the specification in equation 3.1. While overall the point estimates do seem to fluctuate some between specifications, the overall relationship is consistently smaller than what previous estimates would suggest and continue to suggest a fairly limited response to grocery sales when the food tax rate changes.

3.6.3 Eliminating Food Taxes

Using the generalized difference-in-difference specification in equation 3.3, I estimate the effects of the removal of food taxes from the tax base. The plots of the coefficients are provided in Figure 3.4, where panel a represents the results if the tax rates are excluded while panel b represents the inclusion of the tax rates in estimation. In panel (a), the pretrends have some upward shift in levels, they otherwise have no distinct pattern three quarters before the repeal. Once the repeal occurs, there seems to be a fairly limited effect initially. This could potentially be a learning process for consumers, as it may take time for them to notice a difference in their grocery bills. However roughly one year post reform, there seems to be a significant and persistent increase in the sale of food at retailers in our main sample. When one looks at panel (b) where the tax rate is included in the regression, the pretrend is improved over the previous specification but post reform there is no discernible effect of the removal of food from the tax base beyond a rate reduction. Perhaps because the food taxes were gradually phased out in order to smooth state shocks to sales tax revenue, as taxes on food constitutes a substantial portion of sales tax revenue, consumers could respond differently than to a sudden removal of food from the tax base. Table 3.7 we show the difference-in-difference point estimates for the removal of the food tax for Figure 3.4 and the following figures along with the decomposition of the weights in the treatment and control group (Goodman-Bacon, 2021). While the coefficients are somewhat larger than the estimates in the dynamic model, this could potentially be from time units far outside the event window. Importantly, the weights show little influence from timing groups, suggesting that inference from both the static and dynamic difference-in-difference should be very close to the true causal effect.

Because the removal of the food tax base can be heterogeneous between the two states and to help reduce the noise in the coefficient estimates, I estimate the generalized difference-in-difference separately for each state in figure 3.5. While it does seem to increase the precision of the estimates, the point estimate of π and ρ for South Carolina vary wildy. The pre period primarily consists of a sharp increase while the ρ estimates are almost zero for the first 6 quarters, and then increase but without any real consistency in level or trend. In contrast, in West Virginia has a relatively flat pretrend, followed by a modest increase in the first year and a subsequently larger increase in the following year that seems to be fairly persistent. While the coefficient estimates are almost implausibly large, they do seem to suggest that the retail sale of food did increase following the removal of the tax base. This is especially convincing because the rate at which food was taxed in West Virginia was 1% for the pre-period in this sample, which the rate change alone would not suggest such a large response, suggesting that distortions in the tax base, and not only the rate at which items are taxed, may create some distortions.

In order to further examine how the removal of food from the tax base might effect the sale of food, we examine the effect on all retailers in Figure 3.6. The effect seems to be about the same size for all retailers than for for typical places where food is purchased, which given the elasticities are almost double in equation 3.1 this is somewhat surprising. When one looks at panel (b) it is then no surprise to see that once the rate is controlled for, food sales are actually less than what would be expected from an equivalent rate reduction, although the results are not significantly different from zero in most periods. This figure gives further evidence that the removal of the food tax did not systematically change food sales beyond their equivalent rate reduction in this sample.

Because I do not find any evidence that removing unprepared food from the tax base alters sales, I examine the heterogeneous effect for different firm sizes of the food tax removal without controlling for the underlying tax rate. This estimate is presented in Figure 3.7. Frame (a) presents estimates for firms with average quarterly sales below $1,000,000 dollars, which is roughly the bottom 75 percent of the sales distribution while frame (b) presents estimates for firms with average quarterly sales above $1,000,000 dollars which represents the top 25 percent of the sales distribution. There is evidence that the retailers with larger sales volume do see increased sales once the reform occurs, but it does not seem to be persistent beyond the first few years. In contrast, there seems to be a slow increase in the sales volume for smaller firms, that peaks beyond the second year and seems to be persistent[14]. While in all cases the effect seems to be roughly the same size, around a 4 percent increase, it seems that the removal could have boosted the sales to smaller retailers or retailers that target smaller populations, but the evidence is weak.

[14]There is a noticeable dip in the sales right at the time of the reform. This is likely due to the fact that South Carolina repealed their food tax in the middle of the 4th quarter in 2007. It is possible that there was a small preemptive decline in the sale of food that quarter that could explain that dip.

3.7 Conclusion

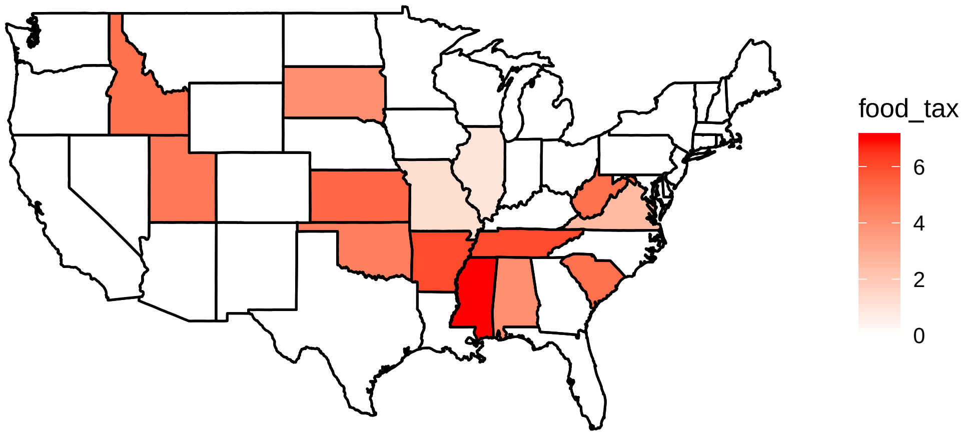
Using retailer level sales data from Nielsen I find two primary results. First, estimates of changes within the tax system by changing the rate at which food is taxed has relatively small impacts on the sale of food at retailers. Using border-pair by quarter fixed effects and examining traditional food retailers I estimate a gross tax elasticity between -.8 and -1 and are robust to a variety of exclusions in sign but there seems to be some level differences. Comparing these estimates to more traditional specifications that use standard panel methods and border counties and with the broader literature on food taxes the effects are substantially smaller. This suggests that a singular time effect does not fully control for time variant characteristics at the regional level even with controls for economic conditions. In addition, I estimate the dynamic effects of a change in the rate and find results that are consistent with the border pair panel regression, but also find evidence of a response when the rate is first announced, instead of when the rate becomes first announced.

Second, I find an inconclusive result on the effect of changes to the sales tax system by exempting food from taxation. Using a GDiD identification, where retailers in border counties of states that repeal a food tax are treated and the retailers in counties that border those states are the controls, I find that the removal of the food tax has a fairly limited effect, although there is evidence that the effect was more pronounced in West Virginia in comparison to South Carolina. This contrasts somewhat with the general salience literature (Chetty et al., 2009) but the context here might lead to food taxes being more salient due to preemptive reductions in the food tax rate and two separate sales tax systems at the border where residents of border counties may be more aware of rate differences.

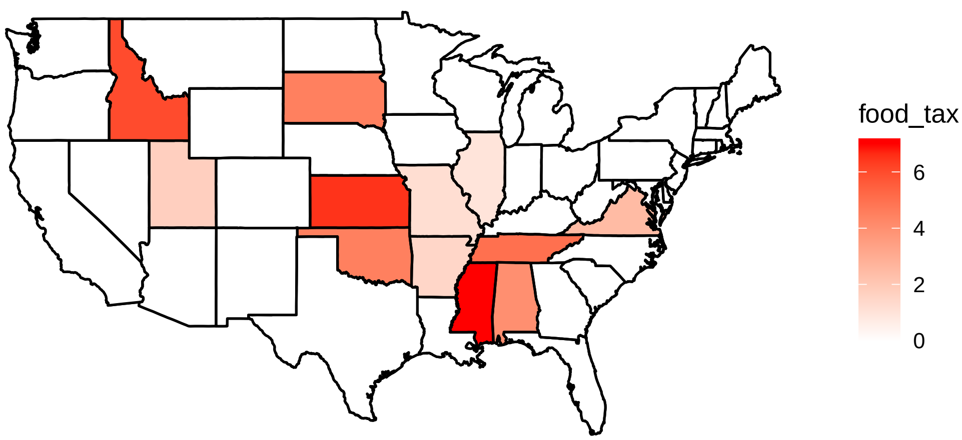
Overall, it is important for policymakers to know the implications of the decisions to make exemptions or special rates in the tax base. While almost all states are now collecting sales tax revenue from internet sales from retailers without physical nexus, states should look to include other forms of consumption in their retail sales tax that are currently excluded. Increasing tax rates on a gradually narrowing tax base will be of limited value for state and local governments to raise revenue. The future of the retail sales tax in in base broadening measures, such as expanding the sales tax to the service sector and online services which can provide an important new source of tax revenue, even with existing rates and structures (Agrawal and Fox, 2021). While the food tax specifically has substantial equity concerns associated with it, other base broadening measures are decidedly less controversial but will likely still have minimal efficiency consequences. While this paper does not find inconclusive evidence that the removal of a part of the tax base has a direct effect independent of the tax rate, it does suggest that these tax differentials do create a real responses and choosing to make these exemptions, no matter how beneficial the policy reasoning have real revenue consequences. It is important to consider how best to optimize the tax base and redistribution methods to address important policy concerns, such as efficiency and fairness, and to do so in a way that minimizes distortions in the tax base.

Figure 3.1: State Food Tax Rates

(a) 2006

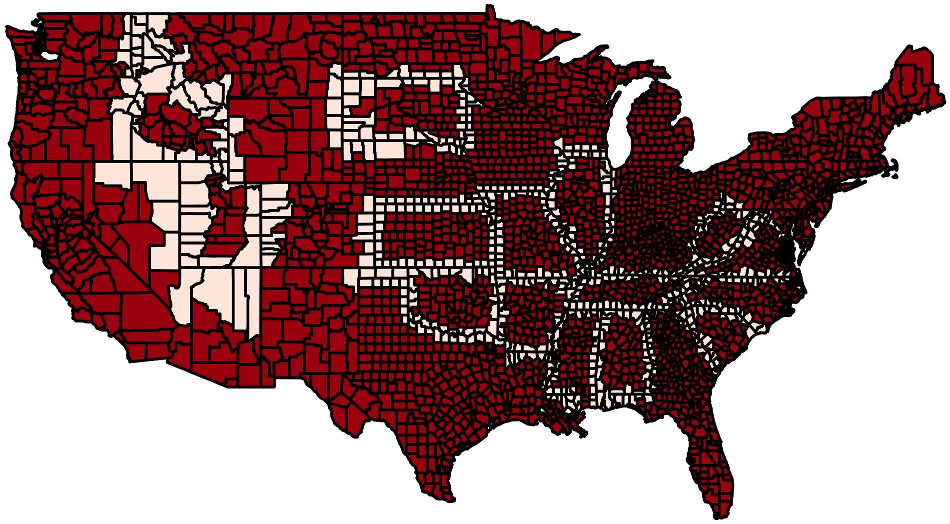


(b) 2017



This figure shows the evolution of food tax rates overtime from 2006 to 2017. White areas represent states that do not have a food tax while the different shades of red represent the rate at which food is taxed in those states.

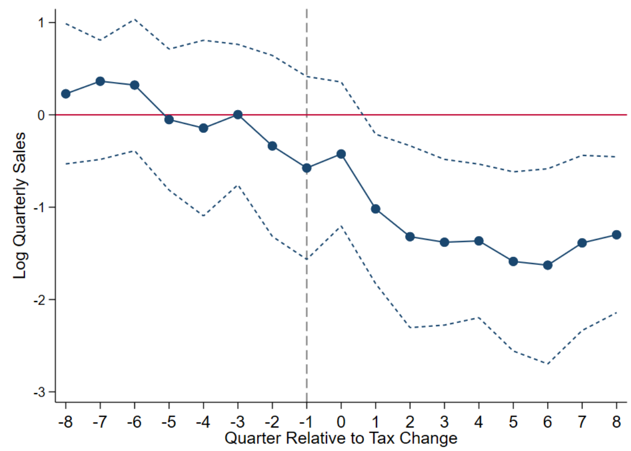
Figure 3.2: Potential Coverage Area



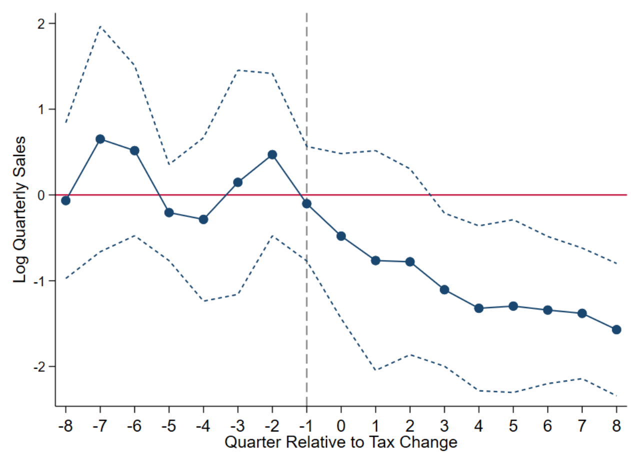
Map is of all counties that have a food tax rate differential. Potential because Kilts Nielsen Retail Scanner Dataset does not have complete coverage in all counties. I am prohibited from releasing the covered counties to protect the identity of retailers.

Figure 3.3: Results from Distributed Lag Model

(a) Statutory Rates



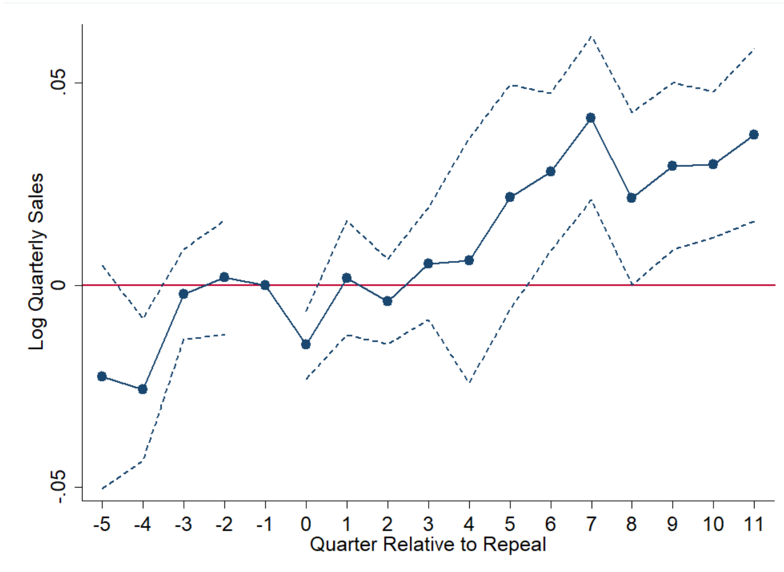
(b) Announced Rates



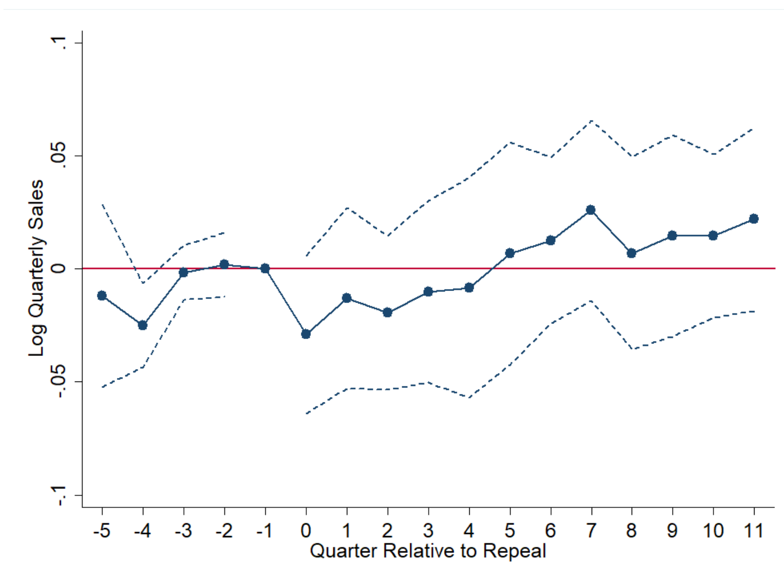
Both plots represent distributed lag model estimates and 95 percent confidence intervals from 3.2 the controls found in 3.3 column 5. The effects for each quarter are a sum of the previous quarter to present a cumulative effect of a tax change. panel 3.3a is the model estimated using the actual food tax rate in a given quarter, while 3.3b uses the date in which the tax change was signed into law by the governor of each state. Both figures use a full set of individual, border-pair by quarter-year, and state by quarter fixed effects plus controls. Standard errors are clustered two ways by the state and neighboring state following Dube et al. (2010).

Figure 3.4: Generalized Difference-in-Difference Results

(a) Tax Rate Excluded



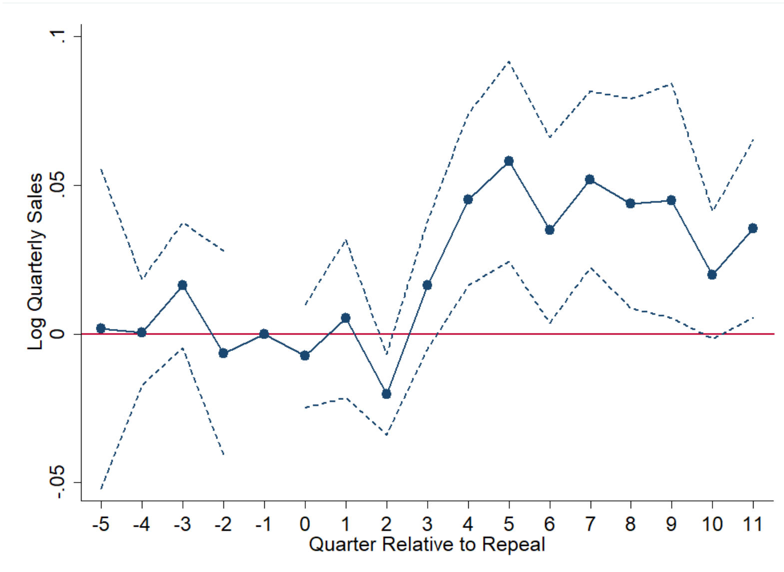
(b) Tax Rate Included



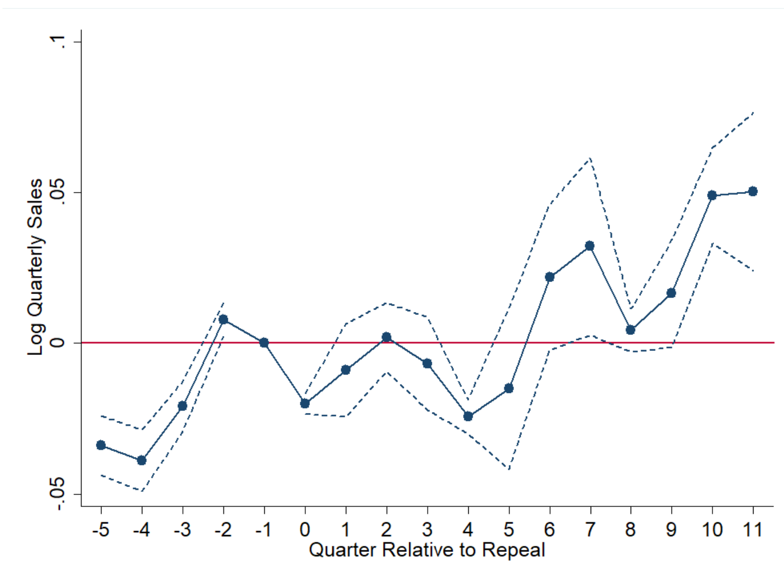
Figures show the results of equation 3.3 on a balanced panel for retailers that primarily sell food. The treatment group is border retailers in South Carolina and West Virginia where the food tax was repealed. The comparison group is all other states with a food tax differential at the border. Both figures include a full set of individual, border-pair by quarter-year, and state by quarter fixed effects plus controls. Standard errors are clustered at the state specific border-pair level. The blue dashed lines represent 95 percent confidence intervals while the blue connected line represents the difference-in-difference estimator for the repeal of food taxes.

Figure 3.5: Generalized Difference-in-Difference Results: Separated by State

(a) West Virginia



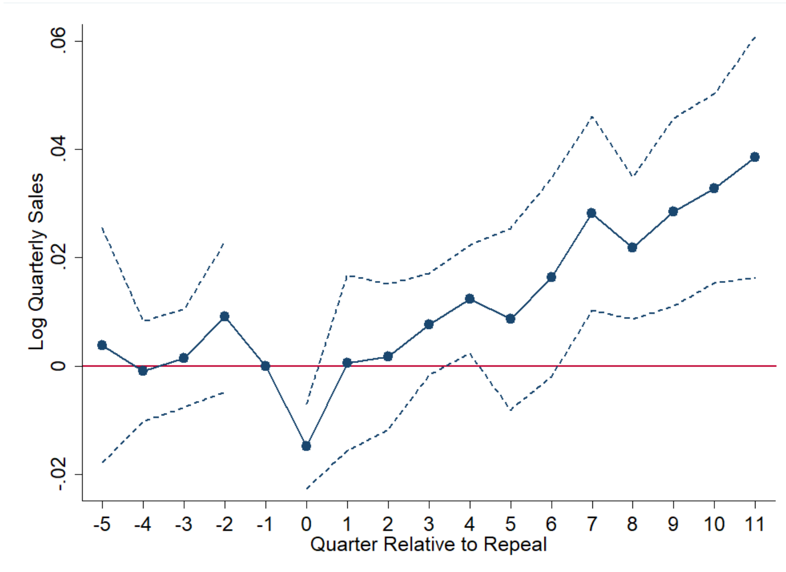
(b) South Carolina



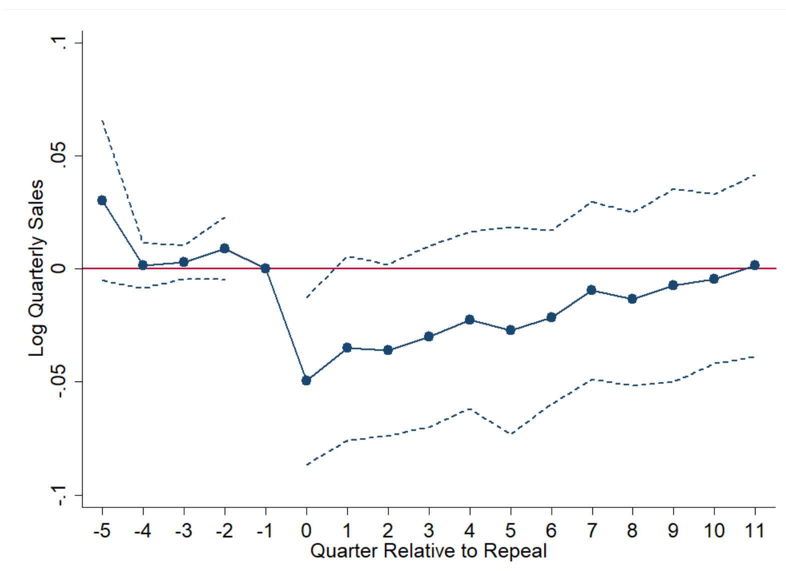
Figures show the results of equation 3.3 on a balanced panel for retailers that primarily sell food. This figure separately estimates figure 3.4 by state. The treatment group is border retailers in South Carolina and West Virginia where the food tax was repealed. The comparison group is all other states with a food tax differential at the border. Both figures include a full set of individual, border-pair by quarter, and state by quarter fixed effects plus controls. Standard errors are clustered at the state specific border-pair level. The blue dashed lines represent 95 percent confidence intervals while the blue connected line represents the difference-in-difference estimator for the repeal of food taxes.

Figure 3.6: Generalized Difference-in-Difference Results: All Retailers Included

(a) Tax Rate Excluded



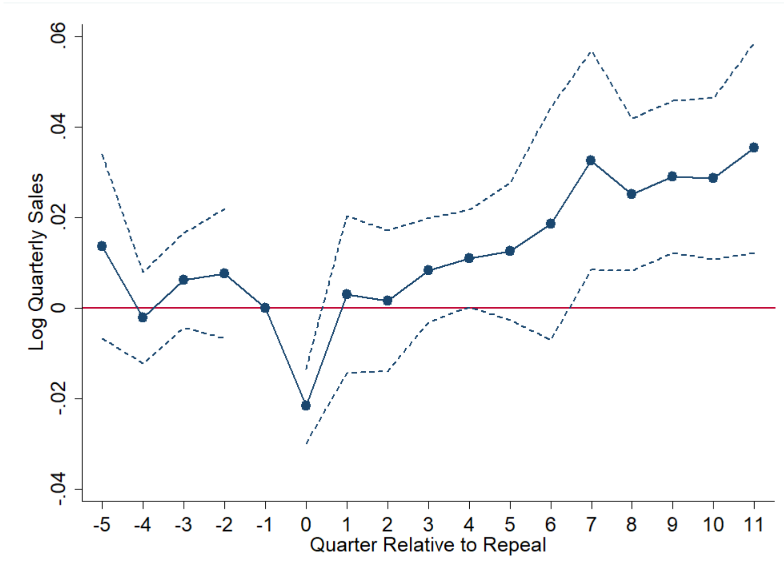
(b) Tax Rate Included



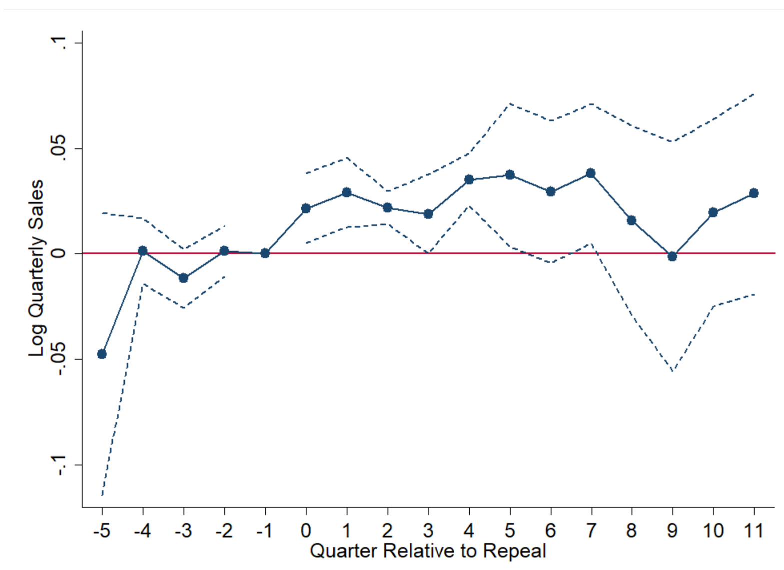
Figures show the results of equation 3.3 on a balanced panel for all retailers. The treatment group is border retailers in South Carolina and West Virginia where the food tax was repealed. The comparison group is all other states with a food tax differential at the border. Both figures include a full set of individual, border-pair by quarter, and state by quarter fixed effects plus controls. Standard errors are clustered at the state specific border-pair level. The blue dashed lines represent 95 percent confidence intervals while the blue connected line represents the difference-in-difference estimator for the repeal of food taxes.

Figure 3.7: Generalized Difference-in-Difference Results: Effect by Sales Volume

(a) Retailers Under $1,000,000 in Quarterly Sales



(b) Retailers Over $1,000,000 in Quarterly Sales



Figures show the results of equation 3.3 on a balanced panel for all retailers divided by quarterly sales. The treatment group is border retailers in South Carolina and West Virginia where the food tax was repealed. The comparison group is all other states with a food tax differential at the border. Both figures include a full set of individual, border-pair by quarter, and state by quarter fixed effects plus controls. Standard errors are clustered at the state specific border-pair level. The blue dashed lines represent 95 percent confidence intervals while the blue connected line represents the difference-in-difference estimator for the repeal of food taxes.

Table 3.1: Summary Statistics by Retailer-Pair-Year-Quarter: All Retailers

	All Retailers			***Retailers in Border Counties***		
	Mean	Std.Dev.	Median	Mean	Std.Dev.	Median
State Tax Characteristics						
Food Tax Rate	0.01	0.02	0.00	0.01	0.02	0.00
Sales Tax Rate	0.05	0.02	0.06	0.05	0.02	0.06
Retailer Sales						
Quarterly Sales	896970.61	1517252.64	86157.10	920337.30	1580896.17	89237.60
County Economic Characterstics						
Personal Income	43655.32	14672.85	40852.00	44933.25	16237.22	41981.00
Population	914179.54	1516057.67	399206.00	879879.51	1293046.68	415957.00
SNAP Recipients	120077.71	209976.52	38685.00	127526.09	226910.08	38324.00
Unemployment Rate	0.07	0.03	0.06	0.07	0.03	0.06
Observations	2283974			777415		

Unweighted summary statistics are at the observation level

Table 3.2: Summary Statistics by Retailer-Pair-Year-Quarter: Sample Retailers

	Retailers That Share Border with Food Tax States			***Retailers in Food Tax States***		
	Mean	Std.Dev.	Median	Mean	Std.Dev.	Median
State Tax Characteristics						
Food Tax Rate	0.01	0.02	0.00	0.03	0.02	0.02
Sales Tax Rate	0.05	0.01	0.06	0.06	0.01	0.06
Retailer Sales						
Quarterly Sales	758218.93	1251533.34	79994.72	856989.10	1383275.76	100350.33
County Economic Characterstics						
Personal Income	39203.87	11299.07	36991.00	43280.91	12285.08	42222.00
Population	322194.61	346096.86	159597.00	1464679.74	2066051.56	345739.00
SNAP Recipients	52541.03	68695.92	19534.00	237583.89	360836.10	32253.00
Unemployment Rate	0.07	0.03	0.07	0.07	0.03	0.06
Observations	176015			225595		

Unweighted summary statistics are at the observation level

Table 3.3: Marginal Effect of a Change in the Food Tax

	(1)	(2)	(3)	(4)	(5)
$ln(1+\tau^f_{st})$	-2.62**	-1.00**	-0.81*	-0.80*	-0.88*
	(1.21)	(0.42)	(0.462)	(0.46)	(0.44)
Border Pair by Time Fixed Effects	N	Y	Y	Y	Y
Population & Income Controls	Y	N	Y	Y	Y
SNAP & Unemployment Controls	Y	N	N	Y	Y
State by Quarter Fixed Effects	N	N	N	N	Y
Observations	129,113	230,816	230,760	215,063	215,063

Standard errors are clustered at the state specific pair level. A full set of retailer and quarter-year fixed effects are included. Column 1 is the standard panel fixed effects analysis done on border regions while columns 2-5 include border pair by year-quarter fixed effects. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Table 3.4: Falsification Test: Effect of Sales Tax on Food Sales

	(1)	(2)	(3)	(4)	(5)
$ln(1+\tau^g_{st})$	-1.76	0.75	0.98	1.31	1.31
	(1.45)	(1.62)	(1.60)	(1.47)	(1.47)
Border Pair by Time Fixed Effects	N	Y	Y	Y	Y
Population & Income Controls	Y	N	Y	Y	Y
SNAP & Unemployment Controls	Y	N	N	Y	Y
State by Quarter Fixed Effects	N	N	N	N	Y
Observations	309,342	252,443	252,443	234,672	234,672

Standard errors are clustered at the state specific pair level. A full set of retailer and quarter-year fixed effects are included. This analysis looks at the effect of the general sales tax on the sale of food for states that do not tax food. Column 1 is the standard panel fixed effects analysis done on border regions while columns 2-5 include border pair by year-quarter fixed effects. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Table 3.5: Robustness of Results

	(1)	(2)	(3)	(4)	(5)
$ln(1+\tau_{st}^{f})$	-0.73	-0.75	-3.17***	-1.62***	-1.95***
	(0.48)	(0.49)	(0.91)	(0.24)	(0.52)
Border Pair by Quarter Fixed Effects	Y	Y	N	Y	Y
No "Western" States	Y	Y	N	N	N
No States West of Mississippi	N	Y	N	N	N
All Retailers Included	N	N	Y	Y	N
Balanced Panel	N	N	N	N	Y
Observations	195,310	185,841	208,146	329,413	113,140

Standard errors are clustered at the state specific pair level. A full set of retailer and quarter-year fixed effects are included along with controls for personal income, population, snap participation rate, and unemployment rate. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$

Table 3.6: Distributed lag Model Coefficient Estimates

	(1) Statutory Rate	(2) Announced Rate
F8	0.227	-0.066
	(0.375)	(0.448)
F7	0.136	0.717***
	(0.284)	(0.239)
F6	-0.379	-0.135
	(0.250)	(0.263)
F5	-0.037	-0.722**
	(0.313)	(0.318)
F4	-0.092	-0.080
	(0.414)	(0.293)
F3	0.146	0.432
	(0.316)	(0.437)
F2	-0.340	0.323
	(0.366)	(0.269)
F1	-0.239	-0.572**
	(0.240)	(0.260)
L0	0.152	-0.378
	(0.386)	(0.232)
L1	-0.595***	-0.285
	(0.186)	(0.242)
L2	-0.301	-0.015
	(0.221)	(0.120)
L3	-0.060	-0.326*
	(0.110)	(0.167)
L4	0.014	-0.217**
	(0.166)	(0.107)
L5	-0.222	0.026
	(0.147)	(0.056)
L6	-0.041	-0.046
	(0.107)	(0.105)
L7	0.241**	-0.039
	(0.112)	(0.156)
L8	0.088	-0.190***
	(0.142)	(0.057)
Observations	129,666	129,666

This table shows the coefficient estimates from the distributed lag model in equation 3.2. Column 1 presents the results from the regression on the statutory rates while Column 2 present the results using the announced rates. Standard errors are clustered two ways at the state and neighboring state level following Dube et al. (2010). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 3.7: The Effect of Food Tax Repeal: Difference and Difference Coefficients

	(1) Baseline No Taxes	(2) Baseline With Taxes	(3) All Retailers No Taxes	(4) All Retailers With Taxes	(5) Retailer Sales <$1,000,000	(6) Retailer Sales >$1,000,000
$treat \times post$	0.083***	0.030	0.087***	-0.002	0.075**	0.029**
	(0.015)	(0.023)	(0.016)	(.021)	(0.023)	(0.013)
$ln(1 + \tau_{st}^{f})$	–	-1.629 **	–	-2.713***	–	–
		(0.713)		(.642)		
Bacon Decomposition						
Never Treated	.971	.704	.970	.711	.965	.978
Timing Groups	.025	.028	.026	.030	.030	.013
Within	.004	.268	.004	.260	.004	.009

This table shows the results estimating equation (3.3). Column 1 is the results of the baseline sample figure 3.4 panel (a). Column 2 is also the baseline figure, but includes the sales tax rate, which corresponds to figure 3.4 panel (b). Column 3 shows the effect on all retailers which is equivalent to figure 3.6 panel a and column 4 corresponds to panel (b). Column 5 and 6 are for retailers with different levels of food sales with 5 corresponding to figure 3.7 column (a) and 6 to column (b). The weights that drive the treatment effect for a standard panel design are presented in the Bacon Decomposition section, with Never Treated referring to the weight on never treated units, Timing Groups referring to the weight in differences in timing, and Within coming from differences in the controls, including the tax rate (Goodman-Bacon, 2021). Standard errors are clustered at the state and neighboring state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Chapter 4 Do Place-Based Policies Lead to Lasting Renewal? Evidence from the Renewal Community Program[1]

4.1 Introduction

Recent empirical work suggests that low quality neighborhoods can have strong negative effects for residents on several general measures of wellbeing (Deryugina and Molitor, 2020; Chetty et al., 2016; Chyn, 2018). In order to mitigate these negative effects, policies were implemented in the 1990s and 2000s to improve the economic outcomes of particularly poor neighborhoods. Busso et al. (2013) find that one program, Empowerment Zones, improves the status of these neighborhoods relative to otherwise similarly poor neighborhoods that applied for, but were not selected, for these programs. However, it is unclear whether these improvements will persist without continual government support. This paper examines the persistence of these policies by exploiting the expiration of funding for Renewal Communities in 2009 relative to Empowerment Zones, which continued through calendar year 2020.

While most work on regional development programs try to understand the direct and immediate (contemporaneous) impact of place-based policies, where place-based policies are defined as government efforts to improve a targeted area within its jurisdiction (Neumark and Simpson, 2015). While, in theory, place-based policies are thought to be inefficient (Glaeser and Gottlieb, 2008) as they are thought to reallocate resources away from more productive areas to less productive areas, empirical evidence is less clear. Some studies examining Empowerment Zones and similar policies have

[1] Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau's Disclosure Review Board and Disclosure Avoidance Officers have reviewed this information product for unauthorized disclosure of confidential information and have approved the disclosure avoidance practices applied to this release. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2376. (CBDRB-FY21-P2376-R9059)

found positive impacts on wages and employment with minimal loss from other areas (Busso et al., 2013; Ham et al., 2011; Givord et al., 2018; Siegloch et al., 2021), while others have found a mixed picture of success (Reynolds and Rohlin, 2015; Elvery, 2009; Neumark and Young, 2019). In addition, recent work on the newest place-based policy in the United States, Opportunity Zones, suggests that the sales price of undeveloped commercial property increases (Sage et al., 2019) with limited effects on residential property values (Chen et al., 2019a) and measures of well being on Opportunity Zone residents (Freedman et al., 2021). While these studies themselves have found differing results, they also only span across short time periods during the existence of the programs and do not examine the persistence of place-based policies once these programs expire. Thus, the current literature is unable to determine whether place-based policies for low-income neighborhoods simply have transitory benefits to those communities, or whether these improvements are more permanent.

While place-based policies are often viewed as inefficient reallocation of resources across geographical boundaries, this need not be the case. With aggolmeration economies, firms benefit from being near other firms creating a positive externality, that without a subsidy that brings firms to one location may cause firms to be sub-optimally located. [2]Place-based policies can effectively act as a way to make an area more attractive for business to locate, and while this does cause relocation, it need not be efficient if agglomeration effects are strong. Agglomeration effects can also act as a potential mechanism for persistence, as the subsidy may have initially attracted firms to the area, once enough firms relocate the externality from other firms in the area may be larger than the net benefit of relocating elsewhere. This is largely the mechanism that has found persistence in other studies, but other mechanisms could lead to persistent effects. Due to the low education and high poverty that is present

[2]For a detailed overview of the agglomeration effects literature see Behrens and Robert-Nicoud (2015) for theory and Combes and Gobillon (2015) for empirics.

throughout these communities, employment incentives that lead to less churn in the labor force could lead to more attachment to the labor force from historically low attachment populations and cause long run improvements in labor force participation in targeted areas.

Long run agglomeration effects from past policies or qualities that are no longer of relevance is well documented as path dependence (Jedwab et al., 2017; Bleakley and Lin, 2012; Brooks and Lutz, 2019). The idea of path dependence is that these large advantages, whether natural or man-made create initial conditions favorable for economic development. Then long after these features were useful, still find large effects on economic activity and development. While this paper is likely too short to determine whether Renewal Communities exhibit path dependence, the short to medium run effects should make it clear whether or not this is possible, and the mechanisms behind path dependence are likely to be closely related with persistence in the short to medium run.

Importantly, there is an existing body of literature that does find long run benefits from certain regional development programs. Perhaps the most notable example is Kline and Moretti (2013) which finds large, long run gains in manufacturing employment attributed to the creation of the Tennessee Valley Authority. Similarly, Ziliak (2012) finds long run persistent effects, but not convergence, from the Appalachian Regional Development Act, and Ehrlich and Seidel (2018) finds strong evidence of agglomeration effects from the West German Zonenrandgebiet. While largely structured and targeted differently, all studies find evidence of long run benefits after the end of funding for the policies[3] , suggesting that policies can persist past their intention. In particular these public investment intensive programs seems to have created agglomeration effects, where firms are more productive from being close to each

[3] Although the benefits might slow over time as funding decreases.

other. While the literature suggests that policies and time sensitive benefits can have significant permanent effects, most of these papers look at larger geographical areas such as groups of counties, regions, large international borders, etc. This project will examine a smaller geographic level with areas defined at the census tract level and span across cities (for urban RCs/EZs) or small groups of counties (rural RCs/EZs). Smaller geographic areas present a unique set of issues, as both businesses and labor are more mobile, allowing resources to move more freely in response to government intervention.

Due to the timing of the Renewal Community expiration lining up closely with the height the great recession, the results help shed light on the impacts of removing an important source of government aid during an economic downturn. Evidence suggests that wage growth during periods of tight labor market conditions can be limited at job loss (Schmieder and Von Wachter, 2010). For labor market entrants, these negative impacts are particularly pronounced, and there is extensive evidence of long run negative effects on income, even compared to newer cohorts (Rothstein, 2020; Schwandt and Von Wachter, 2019). If the ending of place-based policies leads to substantial job loss in targeted communities, it could further harm disadvantaged communities relative to higher socioeconomic status communities. Thus highlighting the importance of both the persistence of removal, but also how timing can create long run negative impacts, through scarring and extensive periods of unemployment as well.

I use the difference in funding renewal status between RCs and EZs to identify the persistence of these policies. This difference allows for causal identification using a generalized difference-in-difference specification. Because both EZs and RCs use census tracts to determine their boundaries, individual level data would require geographic identification at the census tract level. I use restricted-access data from the American Community Survey from 2005-2018 to estimate the causal effect of the

Renewal Community expiration relative to the continuing Empowerment Zones. For zone residents, I find limited evidence of a persistent effect of Renewal Communities, with zone resident and commuter employment following post expiration. I also find negative effects on resident income and monthly rents with Renewal Communities. However, there does seem to be significant heterogeneity, with no decline in resident workers employment, which would be targeted by the employment tax credit. While these results currently do not examine the precise mechanism behind persistence, future work needs to be done in determining which aspects of Renewal Communities caused lasting improvements. Better understanding the precise mechanisms that create lasting effectiveness of these programs will help policymakers design optimal place-based policies moving forward.

4.2 Empowerment Zones and Renewal Communities

The Federal Empowerment Zone (EZ) program was authorized under the Empowerment Zones and Enterprise Communities Act of 1993 and the first round of EZs were announced in 1994. Round I EZs consisted of six urban zones (within Atlanta, Chicago, New York, Baltimore, Detroit, and Philadelphia-Camden) and three rural areas (within Kentucky Highlands, Mid-Delta Mississippi, and the Rio Grande Valley),[4] which were selected out of 500 applicants. The EZs received benefits such as federal grants and employment and business tax credits. The EZ programs were further expanded in 1997 and 2002, with Round II and III designations. The Renewal Community (RC) program, authorized by the Consolidated Appropriations Act for 2001, created 40 RCs (28 urban and 12 rural) that received designation in 2002 at almost the exact time Round III EZs received their designation. A complete overview of all zones is provided in table 4.1. We exploit the difference in the length of funding

[4]Supplemental Empowerment Zones for Cleveland and Los Angeles were added by executive order in 1996, did not receive the same benefits as initially designated zones.

between RCs and EZs. The RCs and EZs were set to expire at the end of 2009, but EZs tax credit funding was initially extended through 2011 and was extended on separate occasions through the end of 2020.

In table 4.2, the economic conditions of both RCs and EZs are presented from the 1990 Census. It is clear that both programs are in similar areas of high poverty and high unemployment. While rural tracts may have somewhat better initial conditions, they represent a relatively small fraction of both zones and consist of a relatively small population so results are unlikely to be exclusively driven by these areas. Because this is from the 1990 Census and before any designation of areas, it is evidence that groups who were ultimately selected for Renewal Communities were not systematically different in any way from the groups that were initially selected for Empowerment Zones, except through some initial selection process by HUD. This is consistent with using Renewal Communities as controls in (Busso et al., 2013) and absent any systematic change in Empowerment Zones besides their continuation relative to Renewal Communities, should provide be adequate controls in this work.

While the RC and EZ programs are broadly similar, they have some minor differences. Table 4.4 highlights the similarities and differences between the two programs. The two programs both have hiring credits that are designed to be used by individuals who reside and their location of employment is within the zone, which creates a strong incentive for within zone hiring. Both programs also create automatic eligibility for the work opportunity tax credit or WOTC, which enables any employer who hires a zone resident to receive a tax benefit in that first year.[5]The main point of difference between RCs and EZs is the treatment of capital gains on property and tangible assets. In Renewal Communities assets held for five years receives an exemption for capital gain taxes; however, in EZs, the sale of an asset is not exempt,

[5]This is in contrast to the zone tax credits, which can be used continuously as long as the residents are employed.

but if purchasing another asset in the zone or from outside an Empowerment Zone entirely, the capital gains can be rolled over into the new project. For example, if an investor owns stock in a publicly traded company, they can sell that stock and invest in a commercial development project in an EZ. Instead of paying taxes directly at the sale, they can defer the taxes by investing in the commercial development project. For Renewal Communities, if an individual purchases a vacant lot in a Renewal Community, builds apartments on the property over the course of the next 5 years, any gain in value is then tax exempt, much like the design of the Opportunity Zones program.[6] Given these latter policy differences are somewhat minor and are targeting similar objectives, EZs will provide a natural comparison for RCs. Comparing expected outlays from Federal projections, funding for both Renewal Communities and Empowerment Zones were expected to cost roughly 10.2 billion dollars between 2002-2010 (JCT, 2001), or around 500 dollars per zone resident, per year.

Tying these benefits directly to program benefits in table 4.3, we can see that most work has exclusively focused on Empowerment Zones, this is likely because decennial census data no longer exists for 2010 and the American Community Survey has large geographic gaps in coverage before 2005. In addition, finding an adequate control group is difficult, given that you can no longer use future zones as a control group. While the results are largely consistent with increases in employment, the most compelling evidence comes from the Busso et al. (2013) paper, which looks at the effect of Empowerment Zones on several key demographics, such as in-zone workers which are eligible for the empowerment zone tax credit but also on other potentially impacted populations such as commuters into the zone and those who reside in the zone and commute outside the zone. Moving forward, my results will be compared directly to the Busso et al. (2013) paper and most results will have parallels

[6] Opportunity Zones actually combine both rollover provisions and exemption provisions, although both are slightly more generous in OZs than in RCs or EZs.

to the effects found within this paper as both the identification strategy and data are similar.

4.3 Methodology

Given the start of RC funding in 2002, and the subsequent ending in 2009, this naturally yields a difference-in-difference model to estimate the effect of RC removal on economic outcomes. The treatment group will be all census tracts that receive RC status, while I use all EZs that retained status post 2009 as a comparison group. The comparison group allows us to compare the effects against a broad set of rollouts, including those that were found to directly improve neighborhoods in the Round I EZs (Busso et al., 2013). While recent work has suggested that some papers suffer from improper control groups in the place-based policies literature (Neumark and Young, 2019), this paper largely overcomes this issue by narrowing the comparison group to already treated units and the selection process was likely uncorrelated with local conditions. I begin my examination of the causal effect of RC expiration with the following empirical specification.

$$Y_{ict} = \beta_0 RC_{ic} \times Post_{ct} + X'_{ict}\gamma + \delta_c + \sigma_t + \varepsilon_{ict} \tag{4.1}$$

Where Y_{ict} represents the individual level outcome of interest for individual i, in census tract c, in year t. The vector X_{ict} represents a set of individual controls, which includes a quadratic in age, the number of children in the household, and dummies for race, gender, and marital status. While δ_c and σ_t represent census tract and year fixed effects. The parameter β_1 represents the causal effect of the expiration of Renewal Communities. Because treatment time here is invariant, the β_0 parameter is well identified as a direct comparison between the treatment group and the control group (**?**).

An important aspect of Empowerment Zones and Renewal Communities is that their benefits should have differential impact on populations that either live or work in the zone. For example the employment credits require individuals to both reside and work within the zone, so a particular population of interest may be those residents who live and work in the zone. In addition, capital gains benefits may attract marginal employers within the zone and lead to increases in employment. As a result, I examine 4 separate populations to better understand the removal of Renewal Community benefits: zone residents, zone workers, residents who live and work in the zone, and zone residents who work outside the zone. While Busso et al. (2013) finds evidence of economic improvement across most of these populations, some populations have more direct links to benefits than others.

While the static difference-in-difference can be informative, better understanding the dynamic effect of funding is important, especially if positive benefits of the programs are short lived or if the negative impacts of removal are temporal. In order to better evaluate these dynamics, I use a generalized difference-in-difference specification:

$$Y_{ict} = \sum_{y=2005}^{2007} \pi_y 1(t = y) \times RC_{ic} + \sum_{y=2009}^{2018} \rho_y 1(t = y) \times RC_{ic} + X'_{ict}\gamma + \delta_c + \delta_t + \varepsilon_{ict} \quad (4.2)$$

The π_y and ρ_y represent the coefficients of interest in this specification. Because the year 2008 is omitted, the π_y coefficients represent the differential evolution of outcomes between EZs and RCs when they had similar policies in place, either with Renewal Community or Empowerment Zone status or no other place-based policy. The set of ρ_y coefficients similarly represents the differential evolution of outcomes when the RC status is terminated or when the treatment group receives no place-based benefits. I will produce figures and tables containing the coefficient estimates for π_y and ρ_y along with their corresponding standard errors, which creates a visual representation of the effect of program removal over time.

In addition to estimating equation 4.1 and 4.2 using individual level data, there are also census tract level outcomes that are of interest. In particular we estimate changes in total and log employment, female employment, population, in-migration, in-county-migration, average wage earnings, and average rent. These estimates well help better understand compositional changes in employment and neighborhood characteristics that occur in response to the change in Renewal Community benefits.

The ρ_y and β_1 coefficients can be interpreted as the causal effect of RC funding termination under the assumptions that, in the absence of repeal, the RCs and EZs would have evolved in a similar matter and that RCs expired for reasons unrelated to the economic conditions within RCs. While we can provide evidence that the first is likely to hold by examining the π_y coefficients, the second condition requires analysis of the institutional reasons for funding expiration. A survey done by the Government Accountability Office (GAO) suggests that around 78 percent of RCs had potential or pending projects if the RC programs were extended beyond December of 2009 (GAO, 2010). This is similar to the 80 percent of Round III EZs that identified potential or pending projects. Regarding legislation, it appears that the Obama administration advocated extending both RCs and EZs but other issues, such as extending unemployment, seemed to crowd out the original funding bill (CRS, 2011). This seems to suggest that the extension difference between the two programs is idiosyncratic, which supports using the EZs as a counterfactual for the RCs. In addition, because Renewal Community removal is closely aligned with the onset of the Great Recession, I examine how local economic conditions respond to the removal of renewal community status by estimating equation 4.2 on the county employment rate inclusive and exclusive of the zone census tracts.

4.4 Data

My data on Renewal Community and Empowerment Zone designated census tracts comes from Housing and Urban Development Economic Development Programs webpage. This data contains information about the specific 1990 census tracts that were included in designated Empowerment Zones and Renewal Communities. For our outcomes data, I use restricted access American Community Survey (ACS) microdata from 2005-2018.[7] I examine both individual level outcomes from ACS microdata and also aggregate microdata to 1990 census tract levels. The use of restricted access data is necessary for two reasons, the first of which is that the smallest available geography in the publicly available ACS is the PUMA (roughly 100,000 in population) which would not begin to accurately assign treatment and control groups given the census tracts are roughly 3,000 persons each. The second issue the publicly available census tract estimates are made by averaging outcomes over 5 years, which would make casual inference with entirely treated tracts difficult.

Because both programs were determined based upon 1990 census tracts, I use the Longitudinal Tract Database (Logan et al., 2014). The LTDB allows for researchers to match census tracts from as early as 1970 through the 2010 Decennial Census. It also provides the fraction of residents in the 1990 census tract that are in both 2000 and 2010 census tracts, which can be used to determine the fraction of residents in the census tract that would be treated by Renewal Community or Empowerment Zone designation which we use along with survey weights to estimate equations 4.1 and 4.2. I also use the fraction of observed census tract residents and ACS survey weights to construct the aggregate measures for the tract level estimates.

[7] While the American Community Survey is available before 2005, the first year in which it has comprehensive nationwide coverage is in 2005 and when looking at a subsample of census tracts, missing coverage is problematic.

4.5 Results

Because the nature of the tax benefits of these programs may have differential effects depending upon both the location of an individuals residence and the location of their employment, I separately look at distinct populations to see if there is heterogeneous effects of policy expiration.

4.5.1 Residents

The broad class of zone residents is likely the most important population from a policy perspective, as the programs were designated to improve the status of residents, and not workers, within the designated areas. The clearest benefit from Busso et al. (2013) is in employment outcomes of zone residents. Therefore, I estimates 4.1 and 4.2 on the extensive margin, intensive margin, and income to examine how Renewal Community expiration impacted these important measures of residential wellbeing. In Table 4.5, I present sign and significance results from the event study and difference-in-difference specifications on the extensive margin. Overall, across all specifications, there is no strong evidence of a concerning pre-trend which is followed followed by declines, often significant, post expiration. The results are particularly notable for both women and for those with a high school diploma or less, suggesting that groups who historically have less labor force attachment are more negatively affected. Given that one of the main benefits is an employment credit targeted towards residence, it is suggestive that the expiration of the credit caused firms to reduce their employment levels of RC residents. However, the primary credit required individuals to both live and primarily work within a Renewal Community, thus these results alone are not sufficient to make this determination.

While individual level extensive margin responses may be informative, they may also mask compositional changes within the census tract level. I thus aggregate reported employment to the census tract level for residents of a RC or an EZ which are weighted by the ACS survey weights and by the fraction of census tract residents in the current census tract that are in a 1990 designated tract for Renewal Communities and Empowerment Zones. The results of the tract level analysis for the event study and difference-in-difference are in Table 4.9. Columns 1 and 2 show the effect of expiration on the total employment while columns 3 and 4 show the effect on the log of total employment. [8] While the event study estimates seem to show a clear decline across all specifications whether in logs or levels, the difference-in-difference estimates do not indicate a statistically significant effect for levels suggesting that perhaps pre-trends or noisy parameter estimates may lead to more noise than otherwise would be anticipated. However for logs, the effect of expiration is much more clear, suggesting a clear negative effect on both the log of total employment and the log of total female employment. In Busso et al. (2013) they find significant effects of receiving Empowerment Zone status on the log of total jobs,and this is consistent with benefits found being relatively short lived.

Moving towards the intensive margin in table 4.6. This specification is limited to employed individuals and it should be interpreted as the effect of expiration on hours worked for those who are already employed and not inclusive of a full labor supply response. I find no discernible trends either before or after expiration, which widely suggest that the intensive margin response is fairly limited. [9] While previous studies have not examined the intensive margin effects of these programs, it is likely because the effects wouldn't be expected to be large as the structure of the benefits would generally only encourage further work from individuals working part-time for

[8] For reference, Busso et al. (2013) use logs for their employment effects.

[9] Some evidence of a positive effect, but it is not significant and could potentially be small. The hourly wage shows a similar effect in Table A.7.

economic reasons which cannot be observed directly in the ACS unlike the Current Population Survey.

Moving back to results that can directly be compared to results that have been found previously in the literature which find a rise in income of zone residents (Busso et al., 2013), I estimate the effect of Renewal Community expiration on wage income and household income. In this case individuals are allowed to report zero income and because income is in logs, the zeros are transformed using the inverse hyperbolic sine transformation (Burbidge et al., 1988). For both wage income and household income, the pre-reform period does not appear to be trending one way disproportionately, the post expiration clearly suggests a decline in wage income. Because these estimates do contain zeros, this is likely driven by the fall in employment, as Tables 4.6 and A.7 are not indicative of a decline in income from the working population. The effects on income, consistent with the effects on employment, appears to be strongest from individuals with a high school education or less. Keeping in mind that the composition of the census tracts will be predominately from lower income households with low levels of education, this represents a substantial fraction of RC households.

Beyond outcomes centered around employment status, Renewal Community and Empowerment Zone programs were designed to improve the quality of the community and make it more attractive for both residents and potential businesses. While the specific literature on place-based policies and residential mobility is fairly scarce, I investigate whether ending the RC program had any impacts on measures of desirability like in migration and rent. In table 4.8, I find no strong evidence of a change in in-migration patterns either from all migrants or from those within the same county, suggesting that ending the program does not appear to negatively impact inflow, at least in this crude measure. This also is true for families with children as well. However, it does appear that there is potentially a decline in the monthly rent of residents once the RC programs ends which could suggest a general decline in the desirability of the neighborhood or a decline in the quality of rental properties from the expiration.[10] In the appendix table A.8, I look at the effects on another set of important quality of life indicators in fertility and marriage, in a similar fashion I find no robust evidence that ending Renewal Communities, and the subsequent loss of income associated with the expiration changes family formation behavior in a measurable way.

The final issue for residents is there could be potential unobserved confounders that could lead to biased estimates of the treatment effect. The most one that is most relevant due to the timing of Renewal Community expiration is if for some reason unrelated to the cause of expiration, RC's were disproportionately harmed by the great recession relative to Empowerment Zones. In order to examine whether this could be the case, I create county level employment rates using the ACS to create a measure of regional economic conditions. Because some counties contain both Empowerment Zones and Renewal Communities, I aggregate to the census tract level to ensure that the mix of treatment status is preserved within that county. The results of this exercise are in Table 4.10 Column 2 where it appears there is potentially a stronger decline in employment rates in RC counties than in EZ counties. In order to ensure this is not driven by employment changes in RCs, I construct the county employment rates excluding zone residents of both RCs and EZs which we see in Table 4.10 Column 3. While the difference-in-difference coefficient is still negative, the even study is less indicative of an overall economic difference, with the peak of unemployment, 2009 and 2010, having higher employment rates, although not significantly different, in the RC counties than in the EZ counties. In addition, because the 1990 census tract status is not provided in the ACS and the fraction of residents from the current tract in the 1990 census tract is imputed, I also run

[10] These measures are not adjusted by any observable measures of quality, simply a reduced form event-study. Because of the designs of these programs, there are reasons to think that there are multiple margins in which housing could adjust.

this estimate on tract population to ensure nothing systematically changes when the tracts change from the 2000 to the 2010 census. The estimate of the effect on tract population is in Column 1 of the same table and it is clear that there is nothing measurably different between RC tracts and EZ tracts from the change in census tracts, which suggests that the imputation provides a consistent approximation of the population in 1990 census tracts in changing from 2000 census tracts to 2010 census tracts which should not systematically bias the results in any one direction.[11]

4.5.2 Workers

While residents are the target of place-based policies, they are not the only populations that are affected. Notably, anyone who works within a targeted area will work for an employer than has access to these benefits like employment tax credits, special deductions, and preferential treatment on capital gains. The previous literature does tend to find positive effects of program designation on the level of employment in a zone and here I investigate whether this too is persistent.

Because employment location is unobserved for nonworkers and the ACS is not a panel where employment status can be observed for past jobs, instead of estimating an individual level reduced form extensive margin estimation, I aggregate to create the total number of jobs, from individuals who report the location of their employer, to the census tract level and then estimate equations 4.1 and 4.2. The results of this estimation are in Table 4.12 and column 1 and 2 provide the estimates in levels for the total population and for females while 3 and 4 are in logs. For levels, it looks like there is potentially a negative effect of total employment given the lack of evidence

[11] Presumably it could be the case that renewal communities tracts have higher weights post 2010 and thus a stronger decline would have taken place absent these weights and vice versa, but this would have likely occurred sharply at 2010 and the event study does not show any significant change from the 2008 difference, meaning that population loss happened very quickly or the fraction is consistent as I suggest.

of a problematic pre-trend and the consistent negative, and occasionally significant, coefficient estimates post reform. However the log estimates have some problematic pre-trends that make a reasonable attempt at inference difficult. Being generous, it appears that ending the Renewal Community program led to a decline of in zone employment, which while consistent with results found in (Busso et al., 2013) it is not clear that this can be directly interpreted as causal.

As with residents, I also examine the intensive margin response of zone workers to the repeal. The mechanisms on why they could be effected also differ. If RC residents are laid off due to losing their subsidy, this may cause other workers to increase their hours to compensate for fewer workers at the place of employment. The results of the intensive margin response are in Table 4.11, similar to Table 4.6 this sample consists only of workers and should be thought of in the same context. Here, there appears to be a positive effect across all specifications overall, but the dynamics of the effect are very heterogeneous through event time, with earlier years appearing to initially decline, but increase over time. Overall there is no strong evidence of any intensive margin adjustments by workers in RCs once the program ends, which is largely consistent with the effect for residents.[12]

4.5.3 Effect on Residents by Work Location

The individuals who are eligible for the Empowerment Zone and Renewal Community tax credit are those who both live and primarily work within the RC or EZ. As a result, given that we have seen evidence that suggests an overall lack of persistence of these policies would see the greatest negative effect from their expiration. While those who work outside the zone, the only real benefit for employers is the work opportunity tax credit, which while large, can only be used for the first year an

[12]While income is not examined for workers, the effect of expiration on hourly wages is provided in A.9 where there is no meaningful effect.

individual works, while the zone credits can be used as long as they are employed for 90 days that year. In order to examine how employment levels change between the two separate groups, I aggregate employed zone residents into two separate groups by their tract of residence, those who work in an RC or EZ and those who work outside an RC or EZ.

I first present the effects where given the previous literature and the results so far would be the largest, those who work in the zone. These can be seen in the first section of Table 4.15. Overall, there is no evidence of any decline in levels of employment of zone residents and perhaps a positive, but insignificant effect on the total number of men, women, and individuals with less than a high school education employed within the zone. In contrast, the results in logs are decidedly more mixed and suggest a decline in the number of residents who work in the zone. However, it is not of the significance level of the who residence employment results, suggesting that they alone are not the drivers of the reduction in employment.

In the second section of Table 4.15, I look at the employment effects of residents who live work outside the zone. Here I find consistent negative effects of RC expiry on both the level and the log of total employment across all specifications, although the difference-in-difference coefficient is not significant for levels, it still broadly suggests that the decline in resident employment is being primarily driven by workers outside the zone. This is not where the benefits are primarily found in Busso et al. (2013), but an important distinction is that the Work Opportunity Tax Credit (WOTC) was not initially part of Empowerment Zone legislation and was added in 2001. If the WOTC increased hiring of zone residents regardless of the location of employment, and zone residents face high job turnover, its expiration could plausibly cause a reduction of Renewal Community employment relative to Empowerment Zones, but evidence on the effect of the WOTC on employment is limited.

Looking exclusively at the intensive margin of those who live and work in the zone, I some evidence that hours actually increased for this population at the individual level in table 4.13. The evidence is strongest for those with a high school diploma or less, but is positive across all specifications, although some have significant pre-trends that are of concern. This further suggests that this particular group does not appear to be harmed in terms of employment outcomes by this expiration, but broad comparisons with all workers in a zone should be done carefully, it is likely that there are differences in industry and other compositional changes that would matter given the precise benefit mechanism in the zone that should be explored further.

In the sample of all residents, it appeared that there were significant declines in both wage income and in household income. Because workers in the zone appear not to see any decline in employment or hours worked, one would expect their income to generally be unaffected as well. Looking at Table 4.14, we can see that that for wage income, while not entirely consistent within the difference-in-difference and event study, has no consistent increase or decrease for those who work in the zone.[13] When looking at household income, there does seem to be consistent evidence for a decline in the difference-in-difference, suggesting that income loss from other members of the household, or other sources of income, are falling. Given the results in Table 4.15, this is potentially driven by employment losses of individuals outside the zone.

4.5.4 Nonresidents

The final subpopulation of interest is the workers who reside outside of a Renewal Community or Empowerment Zone and commute into a zone. These workers would not be eligible for the employment credits, but their workplace, absent reform,

[13]Because this by design excludes nonzeroes, the wage results cannot directly be compared to Table 4.7 at least for wage income results as the resident worker table will not contain employment losses.

would still be recipients of a subset of tax benefits. Therefore if firms relocated in response to the expiration of Renewal Communities, we would see an negative effect on employment levels. Table 4.18 shows tract level estimates of equations 4.1 and 4.2 on total employment within designated 1990 census tracts. In a familiar pattern, there appears to be a negative effect on total employment levels for both the entire population and for females in the difference-in-difference and the event study, but only year 2 and 3 post repeal are significant. However for those with a high school diploma or less, there is negative effect in the event study but in this case it is negative in the difference-in-difference as well. A similar story is apparently in logs as well. Connecting these results back to Tables 4.12 and 4.15, these results suggest that the employment decline within Renewal Communities is driven by commuters. However without specifically looking at changes within different industries and commuter and resident demographics its difficult to determine whether this lead to agglomeration economies within Renewal Communities for firms that are more likely to employ zone residents or if more mobile firms which primarily consist of commuters leave once the benefits end.

I also examine the intensive margin for zone commuters in Table 4.16. In most specifications there appears to be a positive effect on hours worked, but it is not statistically significant in most cases. Because of the compostional changes in the employment level, it is difficult to infer whether this represents the decline in employment consisting of workers who tended to work fewer hours or if it is a relative increase in hours. If one looks at the effects on nonresident income in Table 4.17, there does not seem to be strong evidence of an increase in wage income from nonresident workers although there are some serious issues with pre-trends in columns 3. Household income seems to be similarly unchanged.[14]Household income seems to

[14]This picture is similarly inconclusive when one looks at the effects of hourly wages in Table A.10.

follow a similar picture, with a lack of any discernible effect, which given the lack of results from income from workers in the zone should be expected.

4.6 Conclusion

Looking at the results in aggregate, they generally suggest that the gains that were found in Busso et al. (2013) did not persist once the Renewal Community program ended. While they find increases in employment, wages, and housing values, I find evidence of decreases in all three outcomes for zone residents but the effect appears to be heterogeneous. The decreases in employment seem to be concentrated in individuals who reside in Renewal Communities and commute elsewhere to work while RC workers who were residents saw no subsequent decreases.While further analysis is needed, this would suggest that employment credits created some level of agglomeration economies for certain industries that predominantly hire zone residents or that they lead to a stronger attachment between workers and employers. The number of individuals who commute into the zone seems to decline as well, which could be driven by the outmigration of firms due to the ending of generous tax benefits. Declines in income are likely driven by the disemployment of zone residents and not by adjustments to wages or hours, as no significant or consistent effect are found on wages or hours across all subpopulations and while income did not seem to decline for residents who worked within RCs, their household income fell, suggesting that other sources, such as spousal employment, declined.

While trying to figure out which specific benefits lead to which outcomes is difficult, it appears that the employment credits are likely to be the most effective in creating lasting benefits. Unfortunately, in the most recent iteration of place-based policies in Opportunity Zones, contains no employment credit and exclusively relies on preferential treatment of capital gains. The initial evidence on Opportunity Zones suggests little benefit for Opportunity Zone residents to begin with (Freedman et al., 2021), there should not be great expectations for leading to lasting improvements in broad economic conditions. Comparing these programs with those that were found to have more persistent benefits (Ehrlich and Seidel, 2018; Kline and Moretti, 2013; Ziliak, 2012) the common theme in these programs having persistent effects is substantial public investment projects such as infrastructure, schools, and other local amenities. Even within private subsidies, it is likely the case that those focused on supporting labor of disadvantage communities would be more beneficial than capital investment (Neumark, 2020) and future place-based policies should be designed with these two elements in mind.

While the empirical results are some difficult to interpret with simple sign and significance, this work does indicate that the Renewal Communities program did not lead to a lasting improvement in economic conditions within the targeted areas. While the effect does seem to be heterogeneous and those who were eligible for the employment credit do seem to be least negatively impacted, more work on deciphering what specific industries see the greatest decline/persistence and whether the persistence seen in resident workers is due to the type of employment or due to job losses from commuters. Given the goal of these policies is to reduce deep pockets of poverty and improve the quality of neighborhoods for children, it is important to know how effective these measures are in making permanent improvements as many place-based policies in the United States only exist for relatively short time frames like that of Renewal Communities. This evidence, while limited, suggests that we should not expect lasting improvement from the Renewal Communities and other similarly designed programs and any expectation of lasting benefits is unlikely to occur for the latest iteration of place-based policies, Opportunity Zones.

4.7 Tables

Table 4.1: List of Empwerment Zones and Renewal Communities with Census Tract Counts

Zone	RC	EZ	# of Census Tracts
Aroostook County, Maine		X	9
Atlanta, Georgia	X		63
Baltimore, Maryland		X	25
Boston, Massachusetts		X	30
Buffalo-Lackawanna, New York	X		62
Burlington, Vermont	X		4
Camden, New Jersey	X		14
Central Louisiana	X		53
Charleston, South Carolina	X		18
Chattanooga, Tennessee	X		12
Chicago, Illinois		X	96
Chicago, Illinois	X		71
Cincinnati, Ohio		X	24
Cleveland, Ohio		X	32
Columbia, South Carolina		X	20
Columbus, Ohio		X	24
Corpus Christi, Texas	X		14
Cumberland County, New Jersey		X	9
Desert Communties, California		X	4
Detroit, Michigan		X	49
Detroit, Michigan	X		59
Eastern Kentucky	X		16
El Paso County, Texas	X		5
El Paso, Texas		X	14
Flint, Michigan	X		18
Fresno, California		X	18
FUTRO, Texas		X	5
Gary-Hammond, Indiana		X	28
Greene-Sumter, Alabama	X		10
Griggs-Steele, North Dakota		X	2
Hamilton, Ohio	X		4
Huntington, West Virginia		X	15
Jacksonville, Florida	X		22
Jamestown, New York		X	4
Kentucky Highlands		X	7
Knoxville, Tennessee		X	22
Lawrence, Massachusetts	X		12
Los Angeles, California		X	41
Los Angeles, California	X		39
Lowell, Massachusetts	X		11
Memphis, Tennessee	X		49
Miami-Dade, Florida		X	17
Mid-Delta, Mississippi		X	8
Milwaukee, Wisconsin	X		88
Minneapolis, Minnesota		X	24

Mobile County, Alabama	X		41
New Haven, Connecticut		X	14
New Orleans, Louisiana	X		20
New York City, New York		X	65
Newark, New Jersey	X		70
Niagra Falls, New York	X		6
Norfolk-Porstmouth, Virginia		X	24
Northern Louisiana	X		56
Oglala Sioux Tribe, South Dakota		X	4
Oklahoma City, Oklahoma		X	34
Ouachita Parish, Louisiana	X		19
Philadelphia, Pennsylvania	X		66
Philadelphia-Camden, PA-NJ		X	18
Pulaski County, Arkansas		X	20
Rio Grande Valley, Texas		X	6
Rochester, New York	X		56
San Antonio, Texas		X	27
San Diego, California	X		18
San Fransisco, California	X		9
Santa Ana, California		X	10
Schenectady, New York	X		15
Southern Alabama	X		43
Southern Illinois Delta		X	6
Southwest Georgia		X	6
St. Louis, Missouri		X	30
Syracuse, New York		X	25
Tacoma, Washington	X		8
Tuscon, Arizona			16
Turtle Mountain, North Dakota	X		2
West-Central Mississippi	X		52
Yakima, Washington	X		6
Yonkers, New York		X	6
Youngstown, Ohio	X		5

Table 4.2: Empowerment Zone and Renewal Community Initial Characteristics

Type	Pov Rate	Unemp Rate	Pop	Area
Urban Empowerment Zone	43.66 %	17.17 %	71,926	11.33 sq. mi
Rural Empowerment Zone	37.12%	12.89 %	29,948	1,192 sq. mi
Urban Renewal Community	41.33 %	18.62%	69,485	12.7 sq. mi
Rural Renewal Community	38.23 %	13.57 %	88,650	2704.6 sq. mi

Estimates are from the 1990 Census and are taken from the following GAO report (GAO, 2004).

Table 4.3: Previous Effects from Place-Based Policies

Study	Policy Type	Method	Outcome	Effect
Hanson (2009)	EZ	OLS & IV	Employment Rate	OLS: +* IV: 0
Hanson (2009)	EZ	OLS & IV	Poverty Rate	OLS: -* IV: +
Ham et al. (2011)	EZ	DDD & IV	Unemployment Rate	DDD: -* IV: -*
Ham et al. (2011)	EZ	DDD & IV	Poverty Rate	DDD: -* IV: -
Busso et al. (2013)	EZ %	DD & Adjusted DD	Job Growth	DD: +* ADD +*
Busso et al. (2013)	EZ %	DD & Adjusted DD	Employment	DD: +* ADD +
Busso et al. (2013)	EZ %	DD & Adjusted DD	In-Zone Res Emp	DD: + ADD +*
Busso et al. (2013)	EZ %	DD & Adjusted DD	Out-Zone Res Emp	DD: + ADD +
Busso et al. (2013)	EZ %	DD & Adjusted DD	Commuter Emp	DD: + ADD +
Busso et al. (2013)	EZ %	DD & Adjusted DD	Wages	DD: +* ADD +
Busso et al. (2013)	EZ %	DD & Adjusted DD	In-Zone Res Wages	DD: +* ADD +*
Busso et al. (2013)	EZ %	DD & Adjusted DD	Out-Zone Res Wages	DD: + ADD +
Busso et al. (2013)	EZ %	DD & Adjusted DD	Commuter Wages	DD: - ADD -
Busso et al. (2013)	EZ %	DD & Adjusted DD	Housing Prices	DD: +* ADD +*
Neumark and Young (2019)	EZ %	PSM on 1980 & 1990 levels	Unemployment Rate	PSM: -*
Neumark and Young (2019)	EZ %	PSM on 1980 & 1990 levels	Poverty Rate	PSM: -
Neumark and Young (2019)	EZ %	PSM on 1980 & 1990 levels	Frac with Wage Inc	PSM: -
Neumark and Young (2019)	EZ %	PSM on 1980 & 1990 levels	Wage Income	PSM: +
Neumark and Young (2019)	EZ %	PSM on 1980 & 1990 levels	Employment	PSM: -

All observations come from tables directly in the cited paper. For the method column I refer to what the authors refer to their results as and make no attempt to say whether their methods satisfy the appropriate identification conditions or are well identified.

Table 4.4: Place-Based Tax Benefits

Tax Benefits	EZ	RC
Employment Credit	X	X
Work Opportunity Credit	X	X
Commercial Revitalization Deduction		X
Increased Section 179 Deduction	X	X
Facility Bonds	X	
Qualified Zone Academy Bonds	X	X
Rollover of Capital Gains	X	
Increased Exclusion of Capital gains	X	
Exclusion of Capital Gains		X

Table 4.5: The Effect of Renewal Community Expiration on Resident Employment

	(1) Employed All	(2) Employed Female	(3) Employed Male	(4) Employed Education ≤ 12	(5) Employed Education > 12
$RC \times 2005$	+	-	+	+	-
$RC \times 2006$	-	-	-	-	-
$RC \times 2007$	-	-	+	-	-
$RC \times 2009$	-*	-***	-	-**	-
$RC \times 2010$	-**	-	- **	-***	-
$RC \times 2011$	-**	-***	-	-***	-
$RC \times 2012$	-*	-	-	-	-
$RC \times 2013$	-	- **	-	-	-
$RC \times 2014$	-**	-**	-	-	-
$RC \times 2015$	-	-*	-	-**	-
$RC \times 2016$	-**	-***	-	-***	-
$RC \times 2017$	-**	-***	-	-*	- *
$RC \times 2018$	-	-*	-	-	-
$RC \times Post$	-**	-***	-	-**	-

This table shows the results estimating equation (4.2) on employment status. Column 1 includes all individuals with reported employment, column 2 is a sample of all females with reported employment, column 3 consists of an all male sample with reported employment, column 4 is all individuals with reported employment with a high school education or less, and column 5 is all individuals who report employment with more than a high school diploma. Regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.6: The Effect of Renewal Community Expiration on Resident Hours Worked

	(1) Log Hours All	(2) Log Hours Female	(3) Log Hours Male	(4) Log Hours Education ≤ 12	(5) Log Hours Education > 12
$RC \times 2005$	-	-	+	-	-
$RC \times 2006$	-	+	-	-	+
$RC \times 2007$	+	-	+	-	+
$RC \times 2009$	+	-	+	+	+
$RC \times 2010$	-	+	-	-	+
$RC \times 2011$	-	-	-	-	+
$RC \times 2012$	+	-	+	+	+
$RC \times 2013$	+	+	+	+	+
$RC \times 2014$	+	-	+	+	-
$RC \times 2015$	+	+	-	-	+
$RC \times 2016$	+	+	+	-	+
$RC \times 2017$	+	+	-	-	+
$RC \times 2018$	-	-	+	-	+
$RC \times Post$	+	+	+	+	+

This table shows the results estimating equation (4.2) on the intensive margin, or log of hours worked. Column 1 includes all employed individuals with self reported hours worked, column 2 is a sample of all employed females with self reported hours worked, column 3 consists of an all employed male sample with self reported hours worked, column 4 is all employed persons who self report hours worked with a high school education or less, and column 5 is all employed persons who self report hours worked with more than a high school diploma. The regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.7: The Effect of Renewal Community Expiration on Resident Income

	(1) Log Wage Inc All	(2) Log HH Inc All	(3) Log Wage Inc Female	(4) Log Wage Inc Education ≤ 12	(5) Log HH Inc Education ≤ 12
$RC \times 2005$	+	+	-	+	+
$RC \times 2006$	-	+	-	-	+
$RC \times 2007$	-	+	-	-	-
$RC \times 2009$	-	-	-*	-**	-*
$RC \times 2010$	-	-	-	-*	-***
$RC \times 2011$	-	-	-	-	-
$RC \times 2012$	-	-	-	-	-
$RC \times 2013$	-	- *	-**	-	-**
$RC \times 2014$	-	-	-**	-	+
$RC \times 2015$	-*	-**	-***	-**	-
$RC \times 2016$	-*	-	-**	-***	-***
$RC \times 2017$	-*	-**	-**	-**	-**
$RC \times 2018$	-	-	-	-	-
$RC \times Post$	-**	-**	-**	-***	-***

This table shows the results estimating equation (4.2) on various measures of income. Column 1 includes all individuals with reported employment status, column 2 is a sample of all females with reported employment status, column 3 consists of an all male sample with reported employment, column 4 is all individuals with reported employment status with a high school education or less, and column 5 is all individuals who report their employment status with more than a high school diploma. Regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.8: The Effect of Renewal Community Expiration on Resident Mobility

	(1) Moved In All	(2) Moved In Same County	(3) Log Rent All	(4) Moved In Children > 0	(5) Log HH Inc Same County: Children > 0
$RC \times 2005$	+	+	-	+	+
$RC \times 2006$	-	-	+	+	-
$RC \times 2007$	+	+	-	-	+
$RC \times 2009$	-	+	$-^{*}$	-	-
$RC \times 2010$	+	-	-	+	+
$RC \times 2011$	-	-	-	-	-
$RC \times 2012$	-	-	$-^{*}$	-	+
$RC \times 2013$	+	-	-	+	+
$RC \times 2014$	$-^{**}$	$-^{**}$	-	-	-
$RC \times 2015$	-	-	-	-	-
$RC \times 2016$	$-^{*}$	-	-	-	-
$RC \times 2017$	+	+	-	+	+
$RC \times 2018$	+	+	-	-	-
$RC \times Post$	-	-	-	-	-

This table shows the results estimating equation (4.2) on different measures of mobility. Column 1 includes all individuals with reported employment status and the outcome of interest takes a value of 1 for anyone who moves into a new residence in the last year. Column 2 includes all individuals with reported employment status and the outcome of interest takes a value of 1 for anyone who moves into a new residence this last year who already lived in the county. column 3 includes all individuals with reported employment status and the outcomes is the log of the monthly rent on their property. column 4 is identical to column 1, except it only contains households with children present and column 5 is the analog to column 2 for households with children. Regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.9: The Effect of Renewal Community Expiration on Residential Tract Employment Levels

	(1) Total Employment All	(2) Total Employment Female	(3) Log Total Employment All	(4) Log Total Employment Female
$RC \times 2005$	+	-	+	+
$RC \times 2006$	-	-	-	-
$RC \times 2007$	-	-	-	-
$RC \times 2009$	$-^{*}$	$-^{**}$	-	$-^{*}$
$RC \times 2010$	-	-	$-^{**}$	-
$RC \times 2011$	-	$-^{**}$	$-^{**}$	$-^{**}$
$RC \times 2012$	-	-	-	$-^{**}$
$RC \times 2013$	-	$-^{*}$	$-^{***}$	$-^{***}$
$RC \times 2014$	-	$-^{*}$	$-^{***}$	$-^{***}$
$RC \times 2015$	-	$-^{*}$	$-^{**}$	$-^{***}$
$RC \times 2016$	$-^{**}$	$-^{**}$	$-^{***}$	$-^{***}$
$RC \times 2017$	$-^{**}$	$-^{***}$	$-^{***}$	$-^{***}$
$RC \times 2018$	$-^{*}$	$-^{**}$	$-^{***}$	$-^{***}$
$RC \times Post$	-	-	$-^{***}$	$-^{***}$

This table shows the results estimating equation (4.2) on employment levels at the census tract level. Column 1 includes all individuals who reported being employed aggregated to the 1990 census tract. column 2 is a sample of all females who reported being employed aggregated to the 1990 census tract. column 3 and 4 are identical to columns 1 and 2 except in logs. Aggregates are constructed using weights from the ACS survey weights and the fraction of the current census tract in the 1990 census tract Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.10: Potential Confounders with Causal Effects

	(1) Tract Population All	(2) County Employment Rate All	(3) County Employment Rate Excluding RCs & EZs
$RC \times 2005$	+	+	+
$RC \times 2006$	+	-	-
$RC \times 2007$	+	+	+
$RC \times 2009$	-	-	+
$RC \times 2010$	+	-	+
$RC \times 2011$	-	-	-
$RC \times 2012$	-	-	-
$RC \times 2013$	-	- *	-
$RC \times 2014$	-	-	-
$RC \times 2015$	-	-	-
$RC \times 2016$	-	-**	-
$RC \times 2017$	-	-	-
$RC \times 2018$	-	-	-
$RC \times Post$	-	-	-

This table shows the results estimating equation (4.2) on potential confounding variables at the census tract level. Column 1 includes all individuals. column 2 is the county employment rate calculated from the American Community Survey. column 3 is the county employment rate excluding Renewal Community and Empowerment Zone designated census tracts. Aggregates are constructed using weights from the ACS survey weights and the fraction of the current census tract in the 1990 census tract Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.11: The Effect of Renewal Community Expiration on Hours Worked of Workers

	(1) Log Hours All	(2) Log Hours Female	(3) Log Hours Male	(4) Log Hours Education $\leq$ 12	(5) Log Hours Education > 12
$RC \times 2005$	+	+	+	+	+
$RC \times 2006$	+	+	+	-	$+^{**}$
$RC \times 2007$	+	+	+	+	-
$RC \times 2009$	-	-	-	-	+
$RC \times 2010$	-	+	-	+	-
$RC \times 2011$	-	-	+	-	+
$RC \times 2012$	+	+	+	+	+
$RC \times 2013$	+	+	+	-	+
$RC \times 2014$	+	+	+	+	+
$RC \times 2015$	+	+	-	-	+
$RC \times 2016$	+	+	+	-	+
$RC \times 2017$	+	+	+	+	+
$RC \times 2018$	$+^{*}$	+	+	$+^{*}$	+
$RC \times Post$	+	+	+	+	+

This table shows the results estimating equation (4.2) on the intensive margin, or log of hours worked. Column 1 includes all employed individuals with self reported hours worked work work in a zone, column 2 is a sample of all employed females with self reported hours worked who work in a zone, column 3 consists of an all employed male sample with self reported hours worked who work in a zone, column 4 is all employed persons who self report hours worked with a high school education or less who work in a zone, and column 5 is all employed persons who self report hours worked with more than a high school diploma who work in a zone. The regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract of the location of employment for which Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.12: The Effect of Renewal Community Expiration on Within Tract Employment Levels

	(1) Total Employment All	(2) Total Employment Female	(3) Log Total Employment All	(4) Log Total Employment Female
$RC \times 2005$	+	+	$-^{**}$	-
$RC \times 2006$	-	-	$-^{**}$	-
$RC \times 2007$	+	-	-	-
$RC \times 2009$	-	-	-	-
$RC \times 2010$	$-^{**}$	$-^{**}$	$-^{**}$	-
$RC \times 2011$	$-^{**}$	$-^{**}$	$-^{**}$	$-^{**}$
$RC \times 2012$	-	-	-	-
$RC \times 2013$	-	-	$-^{*}$	-
$RC \times 2014$	+	+	-	-
$RC \times 2015$	$-^{*}$	$-^{*}$	-	-
$RC \times 2016$	-	-	+	-
$RC \times 2017$	-	-	-	-
$RC \times 2018$	-	-	-	$-^{*}$
$RC \times Post$	-	-	+	-

This table shows the results estimating equation (4.2) on employment levels at the census tract level. Column 1 includes all individuals who reported being employed in a designated census tract aggregated to the 1990 census tract. column 2 is a sample of all females who reported being employed in a designated census tract aggregated to the 1990 census tract. column 3 and 4 are identical to columns 1 and 2 except in logs. Aggregates are constructed using weights from the ACS survey weights and the fraction of the current census tract as the location of their employer in the 1990 census tract for which Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.13: The Effect of Renewal Community Expiration on Resident Workers Hours Worked

	(1) Log Hours All	(2) Log Hours Female	(3) Log Hours Male	(4) Log Hours Education $\leq$ 12	(5) Log Hours Education > 12
$RC \times 2005$	$+^{**}$	$+^{**}$	+	-	$+^{***}$
$RC \times 2006$	+	+	+	-	$+^{**}$
$RC \times 2007$	+	-	+	-	+
$RC \times 2009$	-	+	+	-	+
$RC \times 2010$	+	+	+	+	+
$RC \times 2011$	$+^{*}$	+	$+^{**}$	+	$+^{***}$
$RC \times 2012$	$+^{**}$	+	$+^{*}$	+	$+^{**}$
$RC \times 2013$	$+^{***}$	$+^{**}$	+	+	$+^{**}$
$RC \times 2014$	$+^{*}$	+	$+^{*}$	$+^{*}$	+
$RC \times 2015$	$+^{**}$	$+^{**}$	+	+	$+^{***}$
$RC \times 2016$	$+^{**}$	+	$+^{**}$	+	$+^{**}$
$RC \times 2017$	+	+	+	+	$+^{***}$
$RC \times 2018$	$+^{***}$	$+^{***}$	$+^{***}$	+	$+^{***}$
$RC \times Post$	+	+	+	$+^{**}$	+

This table shows the results estimating equation (4.2) on the intensive margin, or log of hours worked of individuals who live and work in a zone. Column 1 includes all employed individuals with self reported hours worked, column 2 is a sample of all employed females with self reported hours worked, column 3 consists of an all employed male sample with self reported hours worked, column 4 is all employed persons who self report hours worked with a high school education or less, and column 5 is all employed persons who self report hours worked with more than a high school diploma. The regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract for which Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.14: The Effect of Renewal Community Expiration on Resident Worker Income

	(1) Log Wage Inc All	(2) Log HH Inc All	(3) Log Wage Inc Female	(4) Log Wage Inc Education $\leq$ 12	(5) Log HH Inc Education $\leq$ 12
$RC \times 2005$	+	+	-	-	+
$RC \times 2006$	-	+	-	-*	-
$RC \times 2007$	+	+	-***	-	+
$RC \times 2009$	-	-	-**	-	-
$RC \times 2010$	+	+	-	-	-
$RC \times 2011$	+	-	-*	-	-
$RC \times 2012$	-	-	-*	-	-
$RC \times 2013$	+	+	-	-	-
$RC \times 2014$	+	+	-	+	+
$RC \times 2015$	+	-	-	+	-
$RC \times 2016$	+	+	-	-	-
$RC \times 2017$	-	-	-*	-*	-
$RC \times 2018$	+	-	-	-	-
$RC \times Post$	-	-*	-	+	-*

This table shows the results estimating equation (4.2) on various measures of income. Column 1 includes all individuals with reported employment status, column 2 is a sample of all females with reported employment status, column 3 consists of an all male sample with reported employment, column 4 is all individuals with reported employment status with a high school education or less, and column 5 is all individuals who report their employment status with more than a high school diploma. Regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.15: The Effect of Renewal Community Expiration on Employment Levels by Workplace Location

	(1) Total Emp All	(2) Total Emp Female	(3) Total Emp Educ $\leq$ 12	(4) Log Emp. All	(5) Log Emp. Female	(6) Log Emp. Educ $\leq$ 12
Within Zone						
$RC \times 2005$	-	+	-	-	+	-
$RC \times 2006$	-	-	-	-	-	+
$RC \times 2007$	-	-	-	+	-	-
$RC \times 2009$	$-^{**}$	-	$-^{*}$	-	-	-
$RC \times 2010$	+	+	+	-	-	-
$RC \times 2011$	+	-	-	-	$-^{*}$	-
$RC \times 2012$	+	+	+	-	-	+
$RC \times 2013$	+	+	-	-	-	-
$RC \times 2014$	+	+	+	-	-	+
$RC \times 2015$	+	+	-	-	$-^{**}$	+
$RC \times 2016$	+	+	+	-	-	+
$RC \times 2017$	+	+	+	-	-	+
$RC \times 2018$	+	+	+	-	-	+
$RC \times Post$	+	+	+	-	-	-
Outside Zone						
$RC \times 2005$	+	-	+	+	+	-
$RC \times 2006$	-	-	-	-	-	$-^{*}$
$RC \times 2007$	+	-	-	-	-	-
$RC \times 2009$	-	-	$-^{*}$	-	-	-
$RC \times 2010$	-	-	-	$-^{**}$	-	$-^{*}$
$RC \times 2011$	$-^{*}$	$-^{**}$	$-^{**}$	$-^{***}$	$-^{*}$	$-^{**}$
$RC \times 2012$	-	-	+	$-^{**}$	$-^{*}$	-
$RC \times 2013$	$-^{*}$	-	$-^{***}$	$-^{***}$	$-^{**}$	$-^{***}$
$RC \times 2014$	$-^{**}$	$-^{*}$	$-^{*}$	$-^{***}$	$-^{***}$	$-^{***}$
$RC \times 2015$	$-^{*}$	$-^{*}$	$-^{*}$	$-^{***}$	$-^{**}$	$-^{***}$
$RC \times 2016$	$-^{**}$	$-^{***}$	$-^{**}$	$-^{***}$	$-^{***}$	$-^{**}$
$RC \times 2017$	$-^{***}$	$-^{***}$	$-^{**}$	$-^{***}$	$-^{***}$	***
$RC \times 2018$	$-^{**}$	$-^{***}$	$-^{**}$	$-^{***}$	$-^{***}$	$-^{***}$
$RC \times Post$	-	-	-	$-^{**}$	$-^{**}$	$-^{**}$

This table shows the results estimating equation (4.2) on employment levels at the census tract level, split by whether the workers work in or outside a zone. Column 1 includes all individuals who reported being employed in a designated census tract aggregated to the 1990 census tract. column 2 is a sample of all females who reported being employed in a designated census tract aggregated to the 1990 census tract. Column 3 is a sample of all workers with a high school diploma or less aggregated to the 1990 census tract level. Columns 4,5, and 6 are analogous to 1,2, and 3 except in logs instead of levels. Aggregates are constructed using weights from the ACS survey weights and the fraction of the current census tract as the location of their residence in the 1990 census tract for which Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.16: The Effect of Renewal Community Expiration on Hours Worked of Nonresident Workers

	(1) Log Hours All	(2) Log Hours Female	(3) Log Hours Male	(4) Log Hours Education $\leq$ 12	(5) Log Hours Education > 12
$RC \times 2005$	-	-	-	+	-
$RC \times 2006$	+	+	+	-	+
$RC \times 2007$	+	+	+	+	-
$RC \times 2009$	-	+	-	-	+
$RC \times 2010$	-	+	-	+	-
$RC \times 2011$	-	-	-	-	-
$RC \times 2012$	-	+	-	+	+
$RC \times 2013$	+	+	+	-	+
$RC \times 2014$	+	$+^{**}$	-	+	+
$RC \times 2015$	+	+	-	+	+
$RC \times 2016$	+	+	-	+	+
$RC \times 2017$	+	+	+	+	+
$RC \times 2018$	+	+	+	+	+
$RC \times Post$	+	+	-	+	+

This table shows the results estimating equation (4.2) on the intensive margin, or log of hours worked. Column 1 includes all employed individuals with self reported hours worked work work in a zone, column 2 is a sample of all employed females with self reported hours worked who work in a zone, column 3 consists of an all employed male sample with self reported hours worked who work in a zone, column 4 is all employed persons who self report hours worked with a high school education or less who work in a zone, and column 5 is all employed persons who self report hours worked with more than a high school diploma who work in a zone. The regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract of the location of employment for which Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.17: The Effect of Renewal Community Expiration on Nonresident Worker Income

	(1) Log Wage Inc All	(2) Log HH Inc All	(3) Log Wage Inc Female	(4) Log Wage Inc Education $\leq$ 12	(5) Log HH Inc Education $\leq$ 12
$RC \times 2005$	$+^{**}$	$+^{**}$	$+^{**}$	+	$+^{*}$
$RC \times 2006$	+	+	+	-	-
$RC \times 2007$	+	+	$+^{***}$	-	$-^{*}$
$RC \times 2009$	+	+	+	-	-
$RC \times 2010$	+	+	$+^{**}$	+	+
$RC \times 2011$	+	+	+	-	+
$RC \times 2012$	+	+	+	+	+
$RC \times 2013$	+	+	+	-	-
$RC \times 2014$	+	+	$+^{**}$	-	-
$RC \times 2015$	+	+	$+^{**}$	+	+
$RC \times 2016$	+	+	+	+	-
$RC \times 2017$	+	+	+	+	+
$RC \times 2018$	+	+	+	+	-
$RC \times Post$	-	+	+	-	+

This table shows the results estimating equation (4.2) on various measures of income. Column 1 includes all individuals with reported employment status, column 2 is a sample of all females with reported employment status, column 3 consists of an all male sample with reported employment, column 4 is all individuals with reported employment status with a high school education or less, and column 5 is all individuals who report their employment status with more than a high school diploma. Regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 4.18: The Effect of Renewal Community Expiration on Within Tract Nonresident Employment Levels

	(1) Total Emp All	(2) Total Emp Female	(3) Total Emp Educ $\leq$ 12	(4) Log Emp. All	(5) Log Emp. Female	(6) Log Emp. Educ $\leq$ 12
$RC \times 2005$	+	+	+	-*	+	+
$RC \times 2006$	-	-	+	-	-	-
$RC \times 2007$	+	-	+	-	-	+
$RC \times 2009$	-	-	+	-	+	+
$RC \times 2010$	-**	-**	-	-**	-**	-
$RC \times 2011$	-**	-**	-**	-**	-*	-***
$RC \times 2012$	-	-	-	-	-	-
$RC \times 2013$	-	-	-	-*	-	-
$RC \times 2014$	+	+	+	-	-	-
$RC \times 2015$	-*	-*	-	-	-	-*
$RC \times 2016$	-	-	-	+	-	+
$RC \times 2017$	-	-	-	-**	-*	-**
$RC \times 2018$	-	-	-	-	-	-
$RC \times Post$	-	-	-*	-	-	-**

This table shows the results estimating equation (4.2) on employment levels at the census tract level for nonresidents. Column 1 includes all individuals who reported being employed in a designated census tract aggregated to the 1990 census tract. column 2 is a sample of all females who reported being employed in a designated census tract aggregated to the 1990 census tract. column 3 and 4 are identical to columns 1 and 2 except in logs. Aggregates are constructed using weights from the ACS survey weights and the fraction of the current census tract as the location of their employer in the 1990 census tract for which Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Appendix

A.1 PGA Qualification

Golfers that are not card holders for the PGA Tour must participate in the Korn Ferry Tour and either win three events or place in the top 75 and play the golfers ranked 126-200 in the PGA Tour at the Web.com Tour Championship. The top 50 finishers in the Web.com Tour Championship tournament receive PGA cards for the following season. Additionally, in tournaments marked as open, people can participate in Monday sessions to qualify for the larger event later in the week. If they consistently qualify and perform well enough to be in the top 125 of points, they would receive a tour card. Previously this was based upon earnings alone, whereas points are now a function of placements and tournaments.[15] There are also special rules for winners of the Majors.[16] those that win any of the major events receive a five year automatic tour card renewal. The PGA Tour tournaments generally have two different tournament eligibility rules: most events are "open" tournaments that all PGA Tour members are eligible to participate in and a handful of "invitationals" that can limit participation to only certain golfers.[17]

A.2 Quality Index Construction

We use Lubotsky and Wittenberg (2006) to construct a data-driven measure of player quality. We use N proxy variables of player quality to construct the index, denoting the n^{th} proxy variable as $Z^n_{i,y}$. The N proxy variables include age, lagged values of earnings, the golfer's lagged top placement, the golfer's lagged aver-

[15]This is not a common method to earn a tour card, with Jordan Speith being a notable exception.

[16]The Masters Tournament, PGA Championship, U.S. Open, and the Open Championship are the four most selective tournaments.

[17]For concerns about identification, these eligibility rules are time invariant.

age placement, the lagged number of tournaments entered, and both the lagged share and lagged count of top 5, top 10, and top 25 placements. Critical to identification is that these proxies are exogenous to the current participation decision. We then regress realized earnings for a tournament w_{ity} on the set of exogenous proxy variables, obtaining a coefficient β^n on each proxy variable. This regression with multiple proxies can then be used to construct an index Z_{iy}^{ρ}:

$$Z_{iy}^{\rho} = \frac{1}{\beta^{\rho}} \sum_{n=1}^{N} \beta^n Z_{iy}^n, \tag{A.1}$$

where $\beta^{\rho} = \sum_{n=1}^{N} \beta^n \frac{cov(w_{ity}, Z_{iy}^n)}{cov(w_{ity}, Z_{iy}^1)}$. We select the normalization, $cov(w_{ity}, Z_{iy}^1)$, such that the quality results are benchmarked to the number of tournaments participated in in the previous year, however, this normalization is irrelevant in our setting. The use of this index creation procedure dominates any ad hoc index creation method.[18] Using the index values for each golfer-year, we create year-specific deciles of quality such that a given player (eg., Tiger Woods) in a given year (eg., 2005) is assigned to a specific decile in that year (eg., Tiger Woods in 2005 is in the 10th decile). Using these deciles, grouped earnings are constructed in the following manner:

$$w_{dty}^{\mathbb{E}} = \frac{1}{G_d} \sum_{i_d=1}^{G_d} w_{ity}, \tag{A.2}$$

where w_{ity} is the realized earnings for individual i, in decile d, for tournament t, in year y and G_d is the number of golfers in the decile who participate in a tournament. The expected earnings $w_{dty}^{\mathbb{E}}$ is simply the cell average of earnings in a tournament by quality decile$\times$year, which gives an estimate of earnings that is uncorrelated with any golfer specific residuals and is not affected by an individual's specific decisions.

[18]In their paper, the procedure is constructed to minimize measurement error.

Construction of this grouped average includes those who participate and do not make the cut, so expected earnings can be zero in some instances for lower quality golfers.

As discussed above, when constructing the index, we use exogenous golfer characteristics and lagged measures of performance. Figure A.3 verifies that our index is strongly correlated with contemporaneous performance.

Given the earnings are constructed using decile$\times$year$\times$tournament cell averages of earnings for participants, the expected earnings are not representative of the sample of participants and nonparticipants. Furthermore, one could imagine that because we use lagged quality measures, an over- or under-performing golfer could cause unwanted variation in the expected earnings. In order to adjust the decline means, we predict earnings of each tournament by estimating, separately for each decile and year,

$$w_{it} = \beta_0 + \delta_i + \delta_t + \varepsilon_{it}, \tag{A.3}$$

where w_it are observed earnings for golfer i in tournament t. In this specification, δ_i represents an individual fixed effect and δ_t represents a tournament specific fixed effect. After obtaining predicted vales $\widehat{w}_{it}$, we then subtract the individual specific fixed effect δ_i from the predicted value which produces means that are adjusted for individual performance differentials within a decile-year. The results of this exercise are presented in Appendix Table A.2.

In addition, we construct an alternative measure of earnings that is more straightforward that leads to broadly similar results. This is done by using a fractional probit to predict expected earnings. The fractional probit model allows us to scale tournament earnings to be between zero and one, where zero corresponds to missing the cut and one corresponds to the top prize of of the tournament. Using only tournament participants, we regress the scaled earnings on the same variables used in the Lubotsky and Wittenberg (2006) index. We then construct fitted values for both participants and nonparticipants. In a similar fashion to the prior approach, we divide the predicted values into year-specific deciles and assign earnings using the decile×year cell averages for all golfers. All the results are robust to this alternative grouping estimator. These can be seen in Tables A.3 along with Figure A.5.

A.3 Constructing the PTR using TAXSIM

Because of the decentralized nature of sourced-based taxation in the United States, creating an accurate measure of tax liability is especially daunting. While TAXSIM (Feenberg and Coutts, 1993) does help greatly in this regard, in its current form, TAXSIM does not adjust for income earned in different states. As a result, this requires a careful and extensive use of TAXSIM to construct our PTRs. Recalling the formula for the participation tax rate:

$$\mathrm{PTR}_{isty} = \frac{\mathrm{T_{sy}}(\mathbb{E}(\mathrm{I_{ity}}|P_{ity} = 1)) - \mathrm{T_{sy}}(\mathbb{E}(\mathrm{I_{ity}}|\mathrm{P_{ity}} = 0))}{\mathbb{E}(\mathrm{I_{ity}}|P_{ity} = 1) - \mathbb{E}(\mathrm{I_{ity}}|P_{ity} = 0)} \tag{A.4}$$

Note that the PTR, although based upon the summation of several decile-specific expected earnings, $w^{\mathbb{E}}_{dty}$, is subscripted with an i because it also depends upon an individual golfer's participation decisions at the start of the year and the individual golfer's residential location.

It is necessary to estimate taxes for when the golfer participates in a tournament and when they choose to forgo the tournament, while holding all other participation, and expected income, in tournaments constant in a given year. To do this, we assume golfers make a one-time decision on which tournaments to participate in at the start of the year. Expected income $\mathrm{I_{ity}}$ is constructed by first taking the sum of expected earnings (see equation A.2) *for all tournaments that the golfer actually participates in.* Then, $\mathbb{E}(\mathrm{I_{ity}}|P_{ity} = 1)$ is then constructed by adding the expected earnings from tournament t if the golfer did not participate in that tournament and requires no modification if the golfer did participate in that tournament. Second,

$\mathbb{E}(\mathrm{I_{ity}}|P_{ity} = 0)$ is constructed in a similar fashion except for subtracting expected earnings for the tournament if the golfer actually participated in it. Note that although expected earnings in a single tournament are indexed by decile d. Thus, all of the tournament-specific components of income I_{ity} are decile-specific, but income is subscript by i because it depends on the choice of other tournaments that each individual plays in. Finally, for each golfer×tournament×year, we have to estimate their expected PTR from their expected income. Again, although the inputs are decile specific, the PTR varies by individual even for golfers that participate in the same set of tournaments because the home state of residence may differ across golfers.

An example would be a golfer in 2010 who resides in Florida and potentially competes in 3 tournaments, one in Arizona, and two in Georgia. At the start of the year, this golfer decides to participate in all tournaments. Given his decile, assume that expects to earn 100,000 in each tournament he participates in. Given Florida has no income tax, we can focus on the employment states. Then for his tournament in Arizona, his PTR would be:

$$\mathrm{PTR} = \frac{[\mathrm{T_{FED}}(300,000) + \mathrm{T_{AZ}}(100,000) + \mathrm{T_{GA}}(200,000)] - [\mathrm{T_{FED}}(200,000) + \mathrm{T_{GA}}(200,000)]}{300,000 - 200,000}, \tag{A.5}$$

Where $\mathrm{T_{FED}}$ is the federal taxes due and $\mathrm{T_{ST.}}$ are the taxes in a given state. The above expression ultimately simplifies to the additional taxes owed federally and the taxes on the 100,000 owed to Arizona divided by the 100,000 dollars he expects to earn:

$$\mathrm{PTR} = \frac{\mathrm{T_{FED}}(300,000) + \mathrm{T_{AZ}}(100,000) - \mathrm{T_{FED}}(200,000)}{100,000}. \tag{A.6}$$

His PTR is 35.4, ATR is 27.3, and an MTR of 41.2. While for each of his tournaments in Georgia, the participation tax rate would be:

$$\mathrm{PTR} = \frac{[\mathrm{T_{FED}}(300,000) + \mathrm{T_{GA}}(200,000) + \mathrm{T_{AZ}}(100,000)] - [\mathrm{T_{FED}}(200,000) + \mathrm{T_{GA}}(100,000) + \mathrm{T_{AZ}}(100,000)]}{300,000 - 200,000}. \tag{A.7}$$

Then, for each tournament, we have the additional taxes on 100,000 dollars of income owed to the federal plus the additional taxes on the 100,000 dollars owed to Georgia, where he already owes taxes, divided by the 100,000 dollars in income. This simplifies to:

$$\mathrm{PTR} = \frac{[\mathrm{T_{FED}}(300,000) + \mathrm{T_{GA}}(200,000)] - [\mathrm{T_{FED}}(200,000) + \mathrm{T_{GA}}(100,000)]}{300,000 - 200,000}. \tag{A.8}$$

The PTR is equivalent for both tournaments because expected earnings are the same for both tournaments. If the expected earners were not the same because of differences in the prizes, then the PTR would differ for both tournaments. If we put this example through TAXSIM, we calculate a PTR of 39.0, an ATR of 28.1, and an MTR of 43.9.

Now, suppose that the golfer could also participate in a tournament in California, where he also expects to earn 100,000 dollars. However, at the start of the year, he chooses not to participate for undisclosed reasons and we observe his non-participation. In this instance, each of the above PTRs remain unchanged because the PTR is based on the set of tournaments that the golfer decides to participate in at the beginning of the year (and the tournament being considered for participation). However, for the CA tournament he elects not to participate in, immediately showing the simplified expression, his PTR in CA is:

$$\mathrm{PTR} = \frac{[\mathrm{T_{FED}}(400,000) + \mathrm{T_{CA}}(100,000)] - [\mathrm{T_{FED}}(300,000)]}{400,000 - 300,000}, \tag{A.9}$$

where the PTR is only based off of his federal taxes on the additional 100,000 dollars he expects to earn and taxes on 100,000 dollars of income in California. Using TAXSIM, we estimate a PTR of 40.2, an ATR of 31.1, and an MTR of 46.15.

We can then estimate the PTR for all golfers by following NBER TAXSIM (Feenberg and Coutts, 1993) guidance for calculating the tax rate in a state[19]. If state taxes were simply based on residence, this would be all that would be necessary to compute the relevant PTR.[20]

However due to source based taxation, more work is required as TAXSIM does not currently account for state nonresident income taxation and all income must be allocated to one state. For each golfer×tournament×year observation, we need to estimate the relevant tax rate the golfers face, taking into account that taxes are due to both the state of residence and the state where income is sourced and the relevant apportionment rules. As mentioned in the tax setting section, states have a few distinct ways of handling nonresident income taxes. For example, under the apportionment of all income earned in a state, if an individual living in Florida earns \$100,000 in Alabama, his income tax liability would be based upon the \$100,000 dollars earned in Alabama. By contrast, if taxes are apportioned based on the fraction of income in the state, if an individual lives in Florida and earns \$300,000 in Florida and \$100,000 in New Jersey, his total tax liability in New Jersey would be 25 percent of the taxes that would be owed if all \$400,000 were taxed in New Jersey.

First, consider the scenario where states choose to only allocate income earned in that state. This leads to a straightforward calculation in TAXSIM where income is summed by state×year and is run through the nonresident state tax system. The

[19]All taxpayers assumed to be married, long term capital gains of 0.66 percent of earnings, 10 percent of income as mortgage interest/property tax/ other itemized deductions, and 2 percent of income as charitable contributions

[20]Granted, this would be a rather trivial and uninteresting exercise given we would lose all interesting variation in taxes due to tournament location and there would be no interstate shifting of earnings.

taxes are then compared to the taxes owed to the resident state from earnings in the tournament state. If the resident taxes are greater, then the taxes are unchanged and if the taxes owed to the state of employment are larger then the additional nonresident state taxes are added to the residential taxes. The alternative apportionment method apportions all income to the nonresident state and then taxes are apportioned by the fraction of income earned in the nonresident state compared to Federal AGI. In order to best simulate this apportionment, we run TAXSIM with all income sourced in the nonresident state and multiply by the ratio between state specific nonresident income and income from all sources. This amount is similarly compared to the taxes due to the resident state and if the nonresident tax is larger, then it is added to the residential taxes in a similar fashion to the previous apportionment method.

While the PTR is simulated using TAXSIM, the properties should be similar to the average tax rates and marginal tax rates simulated in previous papers like Kleven et al. (2013) and Moretti and Wilson (2017). In particular, compare the construction of the PTR to the ATR in Kleven et al. (2013): they use a grouping estimator by country×year×foreign status×quality to directly estimate average tax rates, we instead use a tournament ×year× quality grouping to construct earnings from the cell average of *realized* earnings from participants in that group. Then based upon the golfer's *realized* participation decisions, the PTR is constructed from the grouped earnings measure. Similarly, Moretti and Wilson (2017) use the top 1 percent of earnings to construct the average tax rate that each of their superstar scientists face in each state. While both methods use a simulated measure of tax rates, both treat the tax rates as an exogenous variable in their estimates. In addition, it is commonplace in the peer effects literature to include group averages in the estimating equation without modifications (Lavy and Schlosser, 2011; Carrell et al., 2009), suggesting that both the PTR and expected earnings should be accurate measures of the tax rate and what they would expect to win. In particular, although we estimate quality deciles, the expectation that enters our tax rate is simply a group mean, which as a result, does not create issues with coefficient estimates or standard errors.

A.4 Additional Figures

Figure A.1: Snapshot of Data Sources Scraped

(a) Tournament Results

Florida Citrus Open Invitational < 1970 > - Last played as Arnold Palmer Invitational

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Final round: Mar 8th, 1970 at Rio Pinar C.C., Orlando, FL - 7,012 Yards, Par 72

Quick Link: Performance Stats			Score per Round				Position after Round								
Player	**#**	**Place**	**R1**	**R2**	**R3**	**R4**	**R1**	**R2**	**R3**	**ToPar**	**Total Score**	**Money**	**DraftKings Salary**	**Fedex (Rank)**	**More Info for this Player**
Bob Lunn		Win	66	68	67	70	3	1	2	-17	271	$30,000	-	-	bio - career - this year - this tournament
Bob Stanton	ANZ	T2	71	68	65	68	47	22	4	-16	272	$13,875	-	-	bio - career - this year - this tournament
Arnold Palmer		T2	64	72	64	72	1	4	1	-16	272	$13,875	-	-	bio - career - this year - this tournament
Dick Lotz		4	69	69	69	68	19	10	9	-13	275	$7,050	-	-	bio - career - this year - this tournament
Tom Shaw		T5	69	68	69	70	19	5	6	-12	276	$5,450	-	-	bio - career - this year - this tournament
Tom Weiskopf		T5	68	71	69	68	11	22	11	-12	276	$5,450	-	-	bio - career - this year - this tournament
Richard Crawford		T5	65	72	70	69	2	5	9	-12	276	$5,450	-	-	bio - career - this year - this tournament
Bruce Crampton		T8	67	71	68	71	6	10	6	-11	277	$4,075	-	-	bio - career - this year - this tournament
Howie Johnson		T8	66	69	70	72	3	3	5	-11	277	$4,075	-	-	bio - career - this year - this tournament
Bruce Devlin		T8	67	67	69	74	6	1	3	-11	277	$4,075	-	-	bio - career - this year - this tournament
Jack Montgomery		T11	68	71	70	69	11	22	19	-10	278	$3,038	-	-	bio - career - this year - this tournament
Bert Yancey		T11	69	68	71	70	19	5	11	-10	278	$3,038	-	-	bio - career - this year - this tournament
Dan Sikes, Jr.		T11	69	69	68	72	19	10	6	-10	278	$3,038	-	-	bio - career - this year - this tournament
Lionel Hebert		T11	69	72	67	70	19	43	11	-10	278	$3,038	-	-	bio - career - this year - this tournament
Bob E. Smith		T15	68	70	70	71	11	10	11	-9	279	$2,325	-	-	bio - career - this year - this tournament
Charles Coody		T15	68	71	69	71	11	22	11	-9	279	$2,325	-	-	bio - career - this year - this tournament
Lee Trevino		T15	69	69	72	69	19	10	24	-9	279	$2,325	-	-	bio - career - this year - this tournament
Sam Snead		T15	70	72	71	66	39	52	49	-9	279	$2,325	-	-	bio - career - this year - this tournament
Jack Nicklaus		T19	69	70	71	70	19	22	24	-8	280	$1,662	-	-	bio - career - this year - this tournament

This figure is an snapshot of the data scraped for each tournament. We scraped each row and used all columns plus the information in the yellow final round bar for tournament location. DraftKings Salary and Fedex(Rank) are only available in the most recent years.

(b) Player Information

Career Stats for Tiger Woods

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Born: 1975-12-30, Cypress, Calif.
Nationality: USA
Height: 6' 1, **Weight:** 185lbs
Home: Jupiter, Fla.
College: Stanford
Turned Pro: 1996
Joined PGA Tour: 1996
Official World Golf Ranking: 7

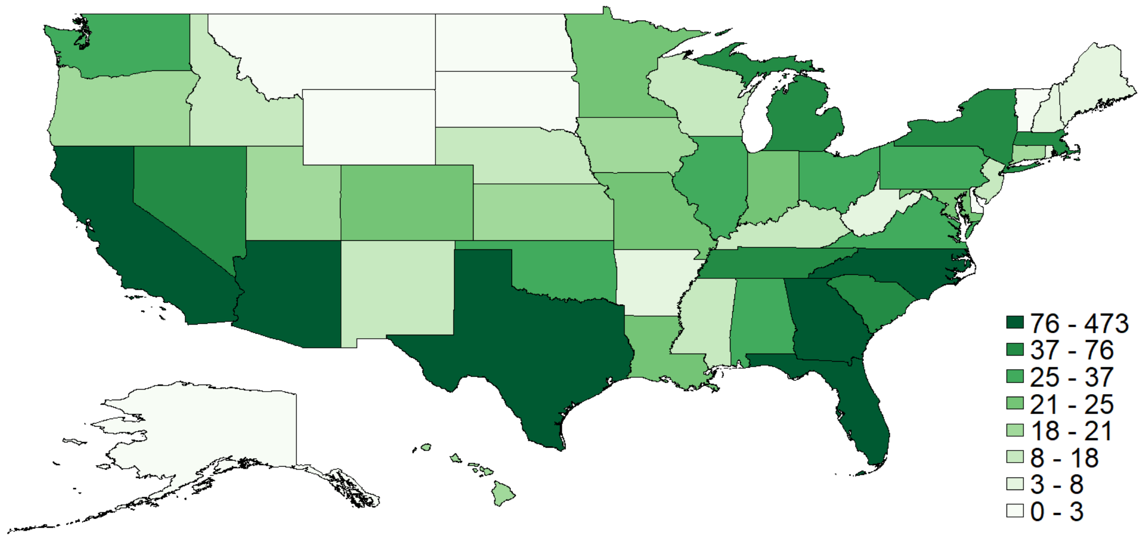
Notes: Woods has won 15 golf majors, second only to Jack Nicklaus' 18. He also has won 81 times on the PGA Tour, second to Sam Snead's 82. Was victorious at 1994, '95 and '96 U.S. Amateur, as well as 1991, '92 and '93 U.S. Junior Amateur and has won nine USGA Championships to share the all-time record with Bobby Jones. Woods 15th major came at the 2019 Masters a tournament he knows so well. But two years before this victory, Tiger returned to Augusta for the 2017 Champions Din...

CONTINUE READING

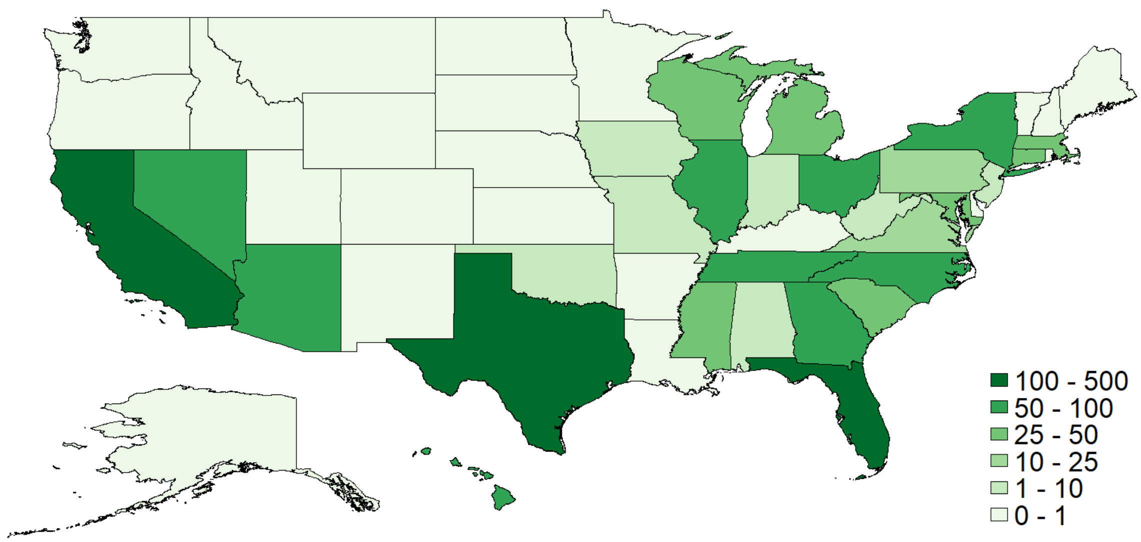
This figure is an snapshot of the data scraped for each player. We scraped the white box on the left third of the page. Unfortunately the turned pro year and joined PGA Tour year boxes have incomplete coverage, so using that data would lose a substantial number of golfers.

Figure A.2: The Location of Golfers and Tournaments

(a) Residence Location

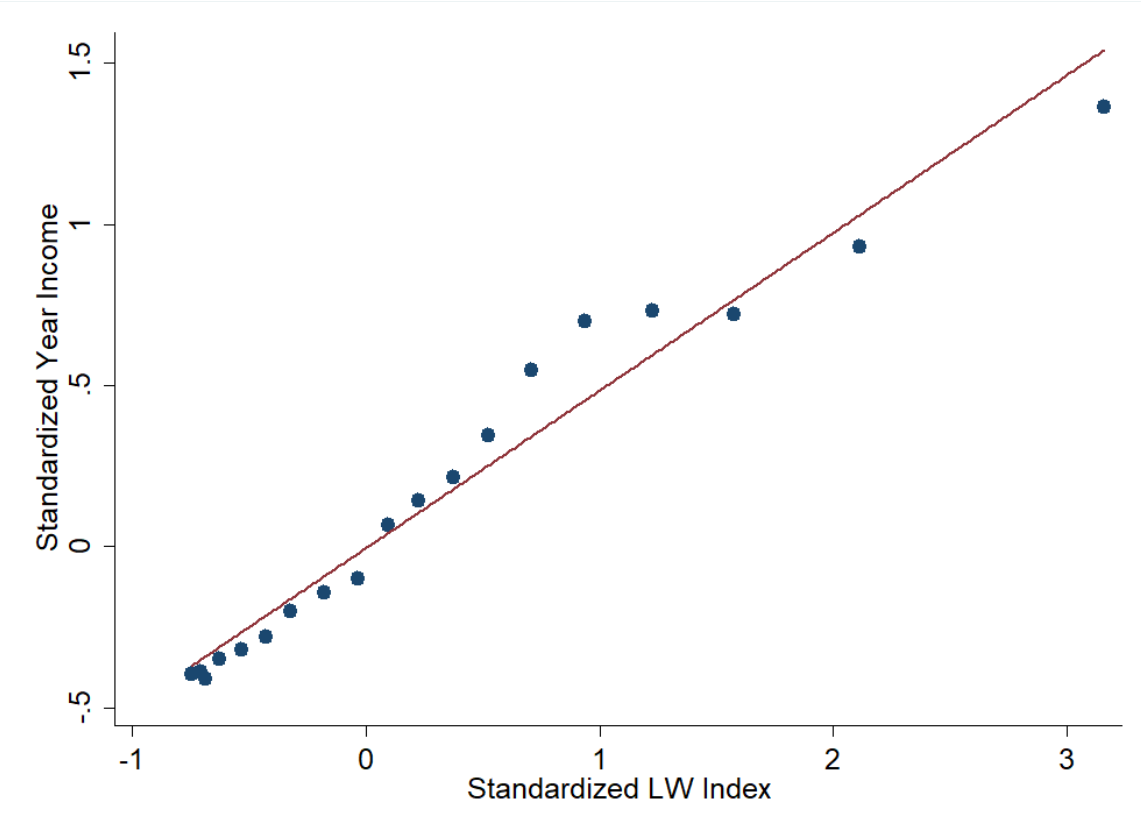


(b) Tournament Location



Part (a) shows the residence of golfers as observed in the data. Each golfer represents one observation. Part (b) represents the location of tournaments across time. To account for the fact that new tournaments are sporadically created over time, each tournament-year counts as one observation in the second panel.

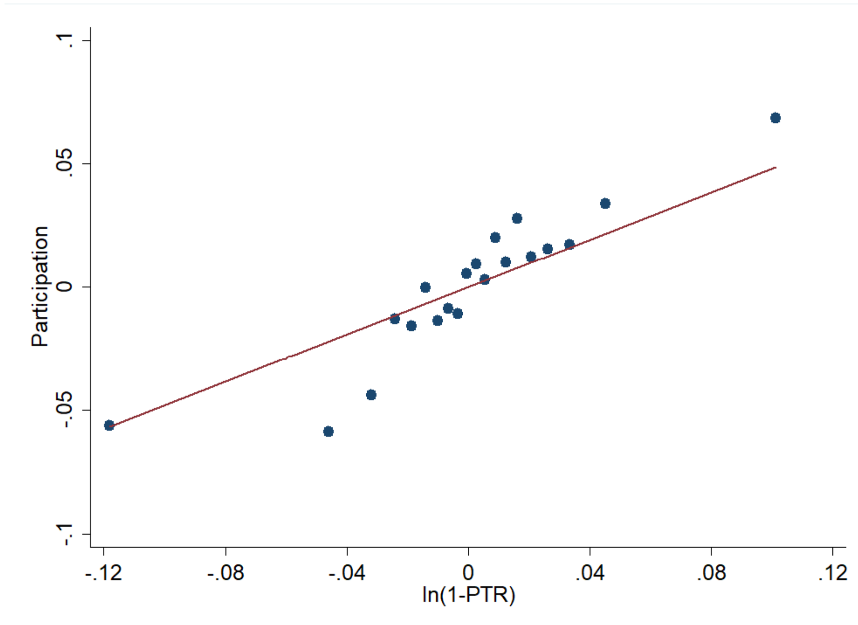
Figure A.3: Correlation of Quality Index with Observed Performance



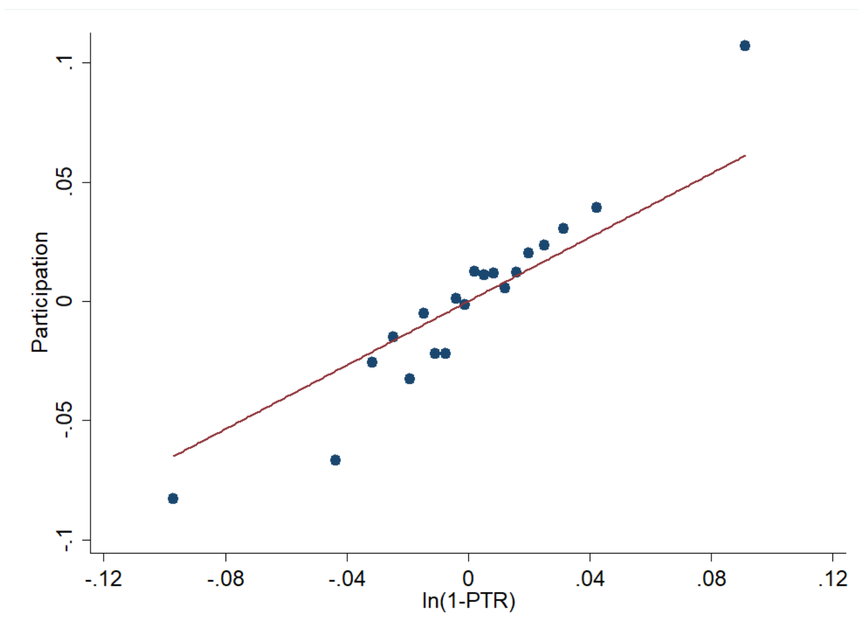
This figure is a visualization of the relationship between yearly earned income and our Lubotsky and Wittenberg (2006) data driven index of quality. To construct this figure, we standardize yearly income and the quality index such that they have a mean of zero and standard deviation of one. We then obtain grouped bins for yearly income and the index and plot a line of best-fit through the data. The slope of this line is the regression coefficient between yearly earnings and the index.

Figure A.4: The Effect of PTR on Participation

(a) Lubotsky and Wittenberg (2006) Index



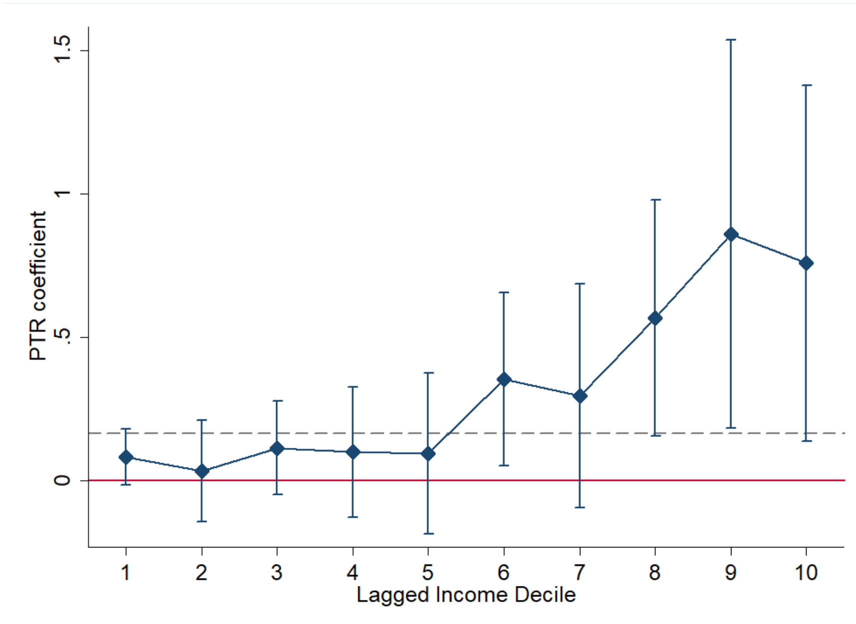
(b) Probit Index



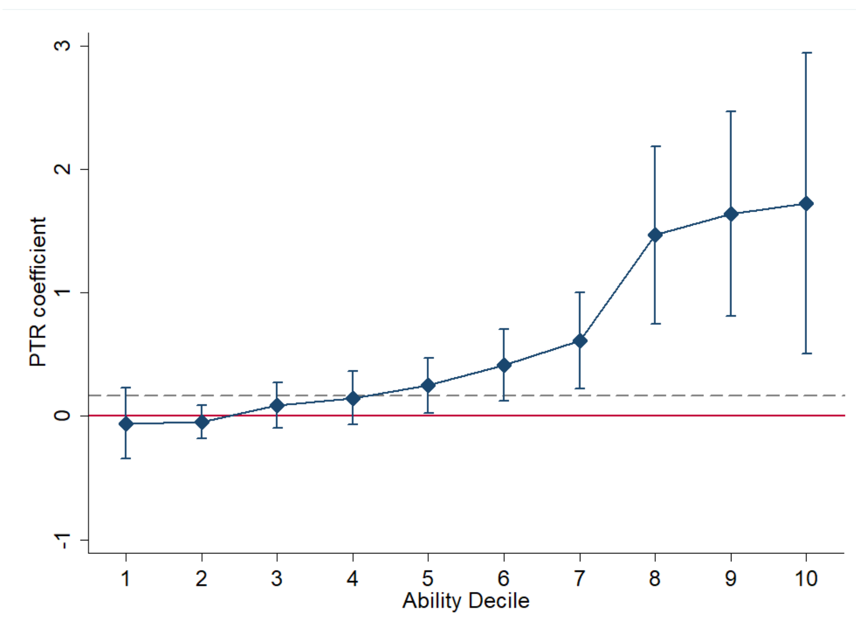
Panel (a) is a visualization of the regression of column 5 from Table 2.2 for and Panel (b) is a visualization of the regression of column 5 from Table A.3. To construct this figure, we regress the year difference in participation on the fixed effects and controls and obtain the residuals. We do the same for the log of the net-of-PTR. We then bin the residuals and plot a line of best-fit through the data. The slope of this line is the coefficient from the difference specification.

Figure A.5: Heterogeneity of the Effect of Taxes by Predicted Income and Quality Deciles: Fractional Probit

(a) Lagged Income Decile: Probit



(b) Quality Decile: Probit



This figure is similar to Figure 2.9. Instead of using actual earnings and the Lubotsky and Wittenberg (2006) index, we use a fractional probit model to predict earnings based on lagged characteristics of the golfer and construct a quality index. The PTR, expected earnings, and all fixed effects are interacted with indicators for the one year lag of predicted yearly income deciles from our probit model (panel a) and with indicators for the probit index deciles (panel b). We plot the marginal effect of an increase in the net-of-PTR for each decile. The grey dashed line represents the coefficient estimate from Table 2.2 column 1. Standard errors are clustered at the golfer and state of tournament level and bars indicate 95% confidence intervals.

A.5 Additional Tables

Table A.1: The Effect of Taxes on the Location of Employment: Using Quartiles from Period $y-2$ and y Earnings

	(1) $y-2$ Percentile 25th-75th	(2) $y-2$ Percentile 75th-100th	(3) y Percentile 25th-75th	(4) y Percentile 75th-100th
$\Delta\ln(w^{\mathbb{E}}_{dty})$	0.016***	0.023**	0.009***	0.017***
	(0.004)	(0.009)	(0.003)	(0.005)
$\Delta\ln(1-\text{PTR}_{isty})$	0.179*	0.835**	0.136**	0.334**
	(0.088)	(0.305)	(0.062)	(0.124)
$\frac{\Delta\ln(1-\text{PTR})}{\Delta\ln(1-\text{ATR})}$	0.466***	0.706***	0.515***	0.639***
	(0.032)	(0.047)	(0.046)	(0.060)
$\epsilon_{1-\text{PTR}}$	0.328	1.45	0.300	0.541
Observations	108,417	56,799	140,510	92,073

This table shows results estimating equation (2.9) except with cuts based upon different periods of income. Column 1 uses those golfers in the 25th-75th percentile of earnings two years ago, column 2 use those golfers in the 75th-100th percentile of earnings two years ago, column 3 uses those golfers in the 25th-75th percentile of earnings in the current period, and column 4 uses those golfers in the 75th-100th percentile of earnings in the current period. Standard errors are clustered at the golfer and the state of the tournament level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.2: The Effect of Taxes on the Location of Employment: Removing Individual-specific Effects to Construct Grouped Expected Income

	(1) Baseline	(2) Earnings > 0	(3) Excludes Cut	(4) Lagged Percentile 25th-75th	(5) Lagged Percentile 75th-100th
$\Delta\ln(w^{\mathbb{E}}_{dty})$	0.009***	0.010***	0.011***	0.014***	0.002
	(0.002)	(0.002)	(0.002)	(0.003)	(0.006)
$\Delta\ln(1-\mathrm{PTR}_{isty})$	0.122***	0.145***	0.115***	0.121**	0.297**
	(0.040)	(0.051)	(0.038)	(0.058)	(0.202)
$\frac{\Delta\ln(1-\mathrm{PTR})}{\Delta\ln(1-\mathrm{ATR})}$	0.479***	0.495***	0.475***	0.440***	0.558***
	(0.027)	(0.029)	(0.027)	(0.029)	(0.051)
$\epsilon_{1-\mathrm{PTR}}$	0.268	0.273	0.354	0.217	0.503
Observations	266,780	233,341	215,430	124,665	66,093

This table shows results estimating equation (2.9). Expected earnings are constructed by regressing realized earnings for participants on individual and tournament fixed effects separately for each decile×year. We then subtract individual fixed effects from the predicted values to obtain expected earnings. Column 1 places no additional restrictions on the sample. Column 2 excludes golfers with zero earnings in the current period. Column 3 excludes golfers who fail to make the cut. Column 4 uses golfers in the 25th-75th percentile of earnings in the previous period, and column 5 uses golfers in the 75th-100th percentile of earnings in the previous period. Standard errors are clustered at the golfer and the state of the tournament level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.3: The Effect of Taxes on the Location of Employment: Fractional Probit to Predict Earnings

	(1) Baseline	(2) Earnings > 0	(3) Excludes Cut	(4) Lagged Percentile 25th-75th	(5) Lagged Percentile 75th-100th
$\Delta\ln(w^{\mathbb{E}}_{dty})$	0.012***	0.014***	0.017***	0.017***	0.019***
	(0.003)	(0.004)	(0.003)	(0.004)	(0.006)
$\Delta\ln(1-\mathrm{PTR}_{isty})$	0.166**	0.203**	0.223***	0.185*	0.670**
	(0.062)	(0.083)	(0.059)	(0.090)	(0.260)
$\frac{\Delta\ln(1-\mathrm{PTR})}{\Delta\ln(1-\mathrm{ATR})}$	0.569***	0.855***	0.537***	0.863***	0.912***
	(0.040)	(0.046)	(0.039)	(0.056)	(0.090)
$\epsilon_{1-\mathrm{PTR}}$	0.377	0.394	0.715	0.345	1.138
Observations	280,825	234,224	228,492	130,181	69,504

This table shows results estimating equation (2.9), but using a measure of predicted earnings rather than quality to construct golfer expectations. We use a fraction probit model to predict earnings based on lagged characteristics of the golfer and construct a quality index as described in the appendix. Column 1 places no additional restrictions on the sample. Column 2 excludes golfers with zero earnings in the current period. Column 3 excludes golfers who fail to make the cut. Column 4 uses golfers in the 25th-75th percentile of earnings in the previous period and column 5 uses golfers in the 75th-100th percentile of earnings in the previous period. Standard errors are clustered at the golfer and the state of the tournament level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.4: The Effect of Taxes on the Location of Employment: Controlling for Risk

	(1) Baseline	(2) Earnings > 0	(3) Excludes Cut	(4) Lagged Percentile 25th-75th	(5) Lagged Percentile 75th-100th
$\Delta \ln(w^{\mathbb{E}}_{dty})$	0.010***	0.013***	0.016***	0.020***	0.027***
	(0.00333)	(0.00401)	(0.00317)	(0.00378)	(0.00807)
$\Delta \ln(1 - \text{PTR}_{isty})$	0.175***	0.223***	0.212***	0.176**	0.648***
	(0.0608)	(0.0739)	(0.0577)	(0.0773)	(0.220)
$\frac{\Delta \ln(1-\text{PTR})}{\Delta \ln(1-\text{ATR})}$	0.504***	0.559***	0.502***	0.506***	0.711***
	(0.032)	(0.035)	(0.032)	(0.039)	(0.061)
$\epsilon_{1-\text{PTR}}$	0.387	0.421	0.658	0.323	1.089
Observations	270120	224799	218440	126602	69745

This table shows results estimating equation (2.9) controlling for the year over year change in the standard deviation of expected income. Column 1 uses those golfers in the 25th-75th percentile of earnings two years ago, column 2 use those golfers in the 75th-100th percentile of earnings two years ago, column 3 uses those golfers in the 25th-75th percentile of earnings in the current period, and column 4 uses those golfers in the 75th-100th percentile of earnings in the current period. Standard errors are clustered at the golfer and the state of the tournament level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.5: The Effect of Taxes on the Location of Employment: By Tournament Order in State

$\Delta \ln(1 - \mathrm{PTR}_{isty})$	(1) Earnings > 0	(2) Lagged Percentile 75th-100th
Base specification	0.163^{**}	0.479^{**}
	(0.067)	(0.202)
	[0.031]	[.051]
States with only one tournament	0.278^{**}	0.960^{**}
	(0.105)	(0.381)
	[0.017]	[0.033]
States with more than one tournament	0.112	0.273
	(0.077)	(0.267)
	[0.207]	[0.307]
First in state with more than one tournament	0.161	0.261
	(0.093)	(0.334)
	[0.115]	[0.355]
Not first in state with more than one tournament	0.089	0.279
	(0.087)	(0.276)
	[0.419]	[0.381]
Second in state with more than one tournament	0.072	0.200
	(0.073)	(0.137)
	[0.285]	[0.077]
States with more than two tournaments	0.091	0.317
	(0.127)	(0.353)
	[0.556]	[0.504]

This table shows results estimating equation (2.9) except looking at differences ate the order of tournaments in the state and based on the number of tournaments in each state. Column 1 uses those golfers with positive earnings and column 2 use those golfers in the 75th-100th percentile of earnings one period ago. Standard errors are clustered at the golfer and the state of the tournament level. A wild cluster bootstrap is performed at 999 repetitions and the associated p-values are presented in brackets. Unfortunately, there are relatively few states with multiple tournaments, so sample sizes and the number of clusters in these latter rows are small, resulting in larger standard errors. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.6: The Heterogeneous Effect of Taxes by Tournament Quality

	(1) Baseline	(2) Earnings > 0	(3) Excludes Cut	(4) Lagged Percentile 25th-75th	(5) Lagged Percentile 75th-100th
$\Delta \ln(1 - \text{PTR}_{isty}) * Q_1$	0.178***	0.208***	0.224***	0.145	0.627**
	(0.056)	(0.068)	(0.061)	(0.100)	(0.239)
$\Delta \ln(1 - \text{PTR}_{isty}) * Q_2$	0.183**	0.236**	0.224***	0.248**	0.713**
	(0.072)	(0.090)	(0.070)	(0.092)	(0.263)
$\Delta \ln(1 - \text{PTR}_{isty}) * Q_3$	0.103*	0.128*	0.148***	0.080	0.511**
	(0.055)	(0.069)	(0.051)	(0.082)	(0.218)
$\Delta \ln(1 - \text{PTR}_{isty}) * Q_4$	0.064	0.079	0.111*	0.038	0.017
	(0.058)	(0.070)	(0.063)	(0.091)	(0.149)
ϵ_{Q_1}	0.442	0.443	0.782	0.287	1.423
ϵ_{Q_2}	0.390	0.417	0.708	0.422	1.200
ϵ_{Q_3}	0.257	0.268	0.519	0.144	0.758
ϵ_{Q_4}	0.141	0.143	0.338	0.078	0.025
Observations	285,110	233,913	232,798	131,211	70,762

This table shows estimates of tournament quality quartiles fully interacted with equation (2.9). The quality quartiles are constructed using the percentiles of the purse for each tournament each year. The best tournaments are in the fourth quartile. Column 1 places no additional restrictions on the sample. Column 2 excludes golfers with zero earnings in the current period. Column 3 excludes golfers who fail to make the cut. Column 4 uses golfers in the 25th-75th percentile of earnings in the previous period, and column 5 uses golfers in the 75th-100th percentile of earnings in the previous period. Standard errors are clustered at the golfer and the state of the tournament level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.7: The Effect of Renewal Community Expiration on Resident Hourly Wages

	(1) Log Hourly Wage All	(2) Log Hourly Wage Female	(3) Log Hourly Wage Male	(4) Log Hourly Wage Education $\leq$ 12	(5) Log Hourly Wage Education > 12
$RC \times 2005$	-	-	+	-	+
$RC \times 2006$	+	+	+	-	+
$RC \times 2007$	+	-	+	+	-
$RC \times 2009$	+	-	+	$-^{*}$	$+^{*}$
$RC \times 2010$	+	-	+	-	$+^{*}$
$RC \times 2011$	-	-	+	$-^{**}$	+
$RC \times 2012$	-	-	-	$-^{*}$	+
$RC \times 2013$	+	-	+	-	+
$RC \times 2014$	+	-	+	-	+
$RC \times 2015$	+	-	-	+	-
$RC \times 2016$	-	-	-	-	-
$RC \times 2017$	-	-	-	$-^{*}$	+
$RC \times 2018$	-	-	-	-	+
$RC \times Post$	-	-	-	$-^{*}$	+

This table shows the results estimating equation (4.2) on the log of hourly wages. Column 1 includes all employed individuals with self reported hours worked, column 2 is a sample of all employed females with self reported hours worked, column 3 consists of an all employed male sample with self reported hours worked, column 4 is all employed persons who self report hours worked with a high school education or less, and column 5 is all employed persons who self report hours worked with more than a high school diploma. Regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.8: The Effect of Renewal Community Expiration on Resident Family Outcomes

	(1) Married All	(2) Married Female	(3) Married Education $\leq$ 12	(4) Birth Last Year Female	(5) Birth Last Year Female: Education $\leq$ 12
$RC \times 2005$	-	+	-	+	$+^{**}$
$RC \times 2006$	+	$+^{*}$	+	-	-
$RC \times 2007$	$+^{*}$	+	+	-	+
$RC \times 2009$	$+^{***}$	$+^{**}$	-	+	+
$RC \times 2010$	+	+	-	$+^{*}$	$+^{*}$
$RC \times 2011$	+	+	+	+	+
$RC \times 2012$	+	+	-	+	+
$RC \times 2013$	+	+	-	-	+
$RC \times 2014$	+	+	-	-	+
$RC \times 2015$	+	+	-	-	+
$RC \times 2016$	+	+	-	+	+
$RC \times 2017$	+	+	-	+	+
$RC \times 2018$	+	+	-	+	+
$RC \times Post$	+	+	-	+	+

This table shows the results estimating equation (4.2) a variety of family demographic outcomes. Column 1 includes all individuals with reported employment where marriage is equal to 1 if individuals report they are currently married, column 2 is a sample of all females with reported employment on marital status, column 3 consists of a female sample of those with a high school diploma or less, column 4 looks at whether a female has given birth in the past year, and column 5 is identical to column 4 except it consists of those with a high school education or less. The regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.9: The Effect of Renewal Community Expiration on Workers Hourly Wages

	(1) Log Hourly Wage All	(2) Log Hourly Wage Female	(3) Log Hourly Wage Male	(4) Log Hourly Wage Education ≤ 12	(5) Log Hourly Wage Education > 12
$RC \times 2005$	$+^{*}$	$+$	$+^{*}$	$+$	$+^{***}$
$RC \times 2006$	$+$	-	$+$	-	$+^{*}$
$RC \times 2007$	$+$	$+^{**}$	-	$+$	$+^{***}$
$RC \times 2009$	$+$	$+$	$+$	-	$+^{**}$
$RC \times 2010$	$+$	$+$	$+$	$+$	$+^{**}$
$RC \times 2011$	$+$	-	$+$	$+$	$+$
$RC \times 2012$	-	-	-	-	$+$
$RC \times 2013$	$+$	$+$	$+$	$+$	$+$
$RC \times 2014$	$+$	$+$	$+$	-	$+$
$RC \times 2015$	$+$	$+^{*}$	$+$	$+$	$+^{*}$
$RC \times 2016$	$+$	-	$+$	$+$	$+$
$RC \times 2017$	$+$	$+$	$+$	-	$+$
$RC \times 2018$	$+$	$+$	$+$	$+$	$+$
$RC \times Post$	-	$+$	-	$+$	-

This table shows the results estimating equation (4.2) on the log of hourly wages for workers in the zones. Column 1 includes all employed individuals who work in a zone, column 2 is a sample of all employed females who work in a zone, column 3 consists of an all males who work in a zone, column 4 is all individuals who work in a zone with a high school education or less, and column 5 is all employed persons who work in a zone with more than a high school diploma. Regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract of employment by which Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table A.10: The Effect of Renewal Community Expiration on Nonresident Workers Hourly Wages

	(1) Log Hourly Wage All	(2) Log Hourly Wage Female	(3) Log Hourly Wage Male	(4) Log Hourly Wage Education ≤ 12	(5) Log Hourly Wage Education > 12
$RC \times 2005$	$+^{**}$	$+^{**}$	$+$	$+$	$+^{***}$
$RC \times 2006$	$+$	-	$+$	-	$+$
$RC \times 2007$	$+$	$+^{***}$	-	-	$+^{***}$
$RC \times 2009$	$+$	$+$	$+$	-	$+^{*}$
$RC \times 2010$	$+$	$+^{**}$	$+$	$+$	$+^{**}$
$RC \times 2011$	$+$	$+$	$+$	$+$	$+$
$RC \times 2012$	$+$	$+$	-	-	$+$
$RC \times 2013$	$+$	$+$	$+$	$+$	$+$
$RC \times 2014$	$+$	$+^{*}$	$+$	-	$+$
$RC \times 2015$	$+$	$+^{***}$	$+$	$+$	$+^{**}$
$RC \times 2016$	$+$	$+$	$+$	$+$	$+$
$RC \times 2017$	$+$	$+^{**}$	$+$	$+$	$+$
$RC \times 2018$	$+$	$+$	$+$	$+$	$+$
$RC \times Post$	$+$	$+$	$+$	$+$	$+$

This table shows the results estimating equation (4.2) on the log of hourly wages for workers who work in the zone but reside outside the zone. Column 1 includes all employed individuals who work in a zone, column 2 is a sample of all employed females who work in a zone, column 3 consists of an all males who work in a zone, column 4 is all individuals who work in a zone with a high school education or less, and column 5 is all employed persons who work in a zone with more than a high school diploma. Regression is weighted by the ACS survey weights and the fraction of the current census tract in the 1990 census tract of employment by which Empowerment Zones and Renewal Communities are defined. Standard errors are clustered at the state level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$

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Vita

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Education

M.S. Economics, University of Kentucky	2017
B.A. Economics *magna cum laude*, Indiana University Purdue University Indianapolis	2016

Professional Experience

Graduate Research Fellow

Kentucky Research Data Center	2020 - 2021
University of Kentucky Center for Poverty Research	2019 - 2020

Research Assistant

Center for Business and Economic Research	2018 - 2019

Teaching Experience

University of Kentucky	2016 - 2020
Instructor	
Principles of Microeconomics	(1 Section)
Intermediate Microeconomic Theory	(1 Section)
Math Camp for Incoming PhD Students	(2 Sections)
Teaching Assistant	
Principles of Microeconomics	
Labor Economics	
Intermediate Microeconomic Theory	
Intermediate Macroeconomic Theory	
Neoclassical Microeconomic Theory	(*Graduate*)
Advanced Microeconomic Theory	(*Graduate*)

Awards and Certificates

SEA Graduate Student Award Session	(2020)
Gatton Fellowship, University of Kentucky	(2018)
Luckett Fellowship, University of Kentucky	(2018)
Gatton Academic Achievement Award, University of Kentucky	(2017)
Max Steckler Fellowship, University of Kentucky	(2016)
Department of Economics Outstanding Junior Award, Indiana University Purdue University Indianapolis	(2015)

Conferences and Presentations

International Institute of Public Finance Annual Congress	(2021)
Symposium on Public Economics	(2021)
Southern Economic Association Annual Meeting	(2020)
Annual Meeting of the National Tax Association	(2020)
Kentucky Economic Association	(2020)
Kentucky Economic Association	(2019)