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Abstract

Prior studies show that taxes matter for the residential locations of high-income earners. But, states raise a significant share of revenue from nonresidents. Using variation in state tax rates, we provide causal evidence on the effect of the net-of-tax rate on the location of labor supply for professional golfers. State taxes induce high-income earners to shift employment to low-tax states without a residence change. The elasticity of working in a state is 0.34, and consistent with superstar phenomenon, increases with earnings. Our results suggest a novel margin of mobility responses for top-earners: the spatial relocation of labor supply by nonresidents.

JEL-Codes: J220, J610, H260, H730, R500.

Keywords: state taxes, superstars, taxing the rich, avoidance, mobility, high-frequency labor supply.

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Fiscal systems raise a large share of tax revenue from high-income individuals (Scheuer and Werning, 2016; Scheuer and Slemrod, 2020). But, effectively taxing these individuals raises important policy challenges. A critical feature of high-income earners—such as superstars, consultants, entertainers, athletes, rental property owners, and business owners—is that they often have employment contracts in many different states, earning *nonresident* income. Nonresident income may be taxable by the state of employment. Thus, they may live and work in (and pay taxes in) different locations.

More generally, 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 high-income taxpayers limits the effectiveness of information reporting, providing new and creative ways to avoid taxes.

As an example of the importance of nonresident income, U.S. states raise 7.5% of personal income tax revenue from nonresidents, with nonresident employees contributing as much as 15% of revenue in some states. Even states with little or no cross-border commuters, such as Hawaii, raise 7% of revenue from nonresident income taxes. After removing interstate commuters, states collect over 13 billion dollars or 4% of revenue from nonresidents,[1] likely from high-income earners able to work in non-adjacent states.

Following the COVID-19 pandemic, taxation of nonresident income will likely rise in importance. Telecommuting will allow companies to hire workers from all over the world. As companies increasingly allow their workers to go fully remote without any need to work in a (physical) office or abandon office space entirely, teleworking will decouple the state of residence and the state of work (Brueckner, Kahn and Lin, 2022).[2] As remote work increases, how to tax nonresident income will become an increasingly important policy issue (Agrawal and Stark, 2022), just like foreign-sourced income has increasingly posed new challenges to the international tax system.[3] Because telework allows individuals to make separate decisions over where to live and work, the elasticity of labor mobility will diverge from the residential mobility elasticity. Deciding to earn income in a nonresident state was always possible for consulting contracts, interstate commutes, or rental property income, but telework makes these choices commonplace.

Labor income taxes in the U.S. are generally first due to the state where the

[1]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.

[2]See Kathryn Vasel. "These Companies Decided to Go Fully Remote – Permanently." CNN (January 27, 2022); Felicity Hannah. "The Firm with 900 Staff and No Office." BBC (July 5, 2019).

[3]If states adopt a "convenience of the employer rule", then employees working remotely from another state can be taxed in the employer's state without the worker having a physical presence there.

income is *earned*, with additional taxes only due to the residence state if it is higher tax. For an individual with employment opportunities in other states, moving residences can reduce tax liabilities *only if* the initial tax rate in the state of residence is higher than in the employment state. If the residence rate is lower, *without* changing her state of residence, a higher tax in the nonresident state of employment may deter the individual from accepting a 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 the taxation of nonresidents is virtually nonexistent. Yet, both elasticities matter for measuring the efficiency costs of taxation. A simple conceptual model highlights when the elasticities of residence versus employment matter and why empirical estimates do not currently capture this margin.

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 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. Even then, 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, Landais and Saez (2013) inspired a literature on international residential mobility using the example of football players, and much of the following literature has focused on niche 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 location, their skill characteristics, the prizes they win, and most critically, the decision of which states they do/don't play in. Important for 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 and digitize data on the careers of professional golfers from 1977 to the present. We 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

[4]See Agrawal and Foremny (2019); Akcigit, Baslandze and Stantcheva (2016); Bakija and Slemrod (2004); Kleven, Landais and Saez (2013); Kleven et al. (2014); Milligan and Smart (2019); Moretti and Wilson (2017); Muñoz (2020); Schmidheiny and Slotwinski (2018); Young and Varner (2011); Young et al. (2016).

of our estimates.

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, *many* 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. 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.

Golfers are unique because they 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 in order to simulate participation tax rates (Immervoll et al., 2007) 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 life-cycle factors. 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 raises the baseline participation rate in a tournament by 0.174 percentage points. Given the average participation rate is around 52%, this implies an extensive margin elasticity of approximately 0.335. We show this elasticity increases with earnings. Those golfers in the top quartile of earnings having elasticities that are more than double the elasticity for all golfers. 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. This elasticity is smaller than the residential response in Kleven, Landais and Saez (2013), 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). At the margin, taxes appear to be an important factor for work location decisions.

Relating our elasticities to the literature on superstar effects, we document realized earnings are 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 superstar phenomenon because the tournaments that these high earners typically participate in have higher prizes. Thus, our participation elasticities increasing with earnings likely underestimate the effect of superstar phenomenon on increasing earning elasticities.

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 a *high-frequency* labor supply elasticity.[5] 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 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. Our focus is on participation, and not the intensive (effort) margin of labor supply.

Returning to the tax policy debate, high-income earners subject to nonresident income taxes, have potentially two margins of response: the conventional relocation of residence and an employment responses that effectively relocates nonresident income. Although often assumed to be the same, many high-income professionals make *separate* decisions on these margins. Thus, studies that find residential location responses may miss important spatial distortions 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 or the source principle, a topic that is increasingly relevant as telework allows individuals to optimize their place of employment and place of work separately.

1 Institutional Details

1.1 The PGA Tour

The PGA Tour runs U.S.-based golf tournaments. Most events are "open" tournaments that all PGA Tour members are eligible to participate in and a handful of "invitationals"

[5] See Oettinger (1999), Farber (2005), Thakral and Tô (2021) and Chen et al. (2019).

that can limit participation. 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 prior year and any tournament winners.[6]

Each tournament has a total "purse" for prize earnings. Half-way through each four-day tournament, golfers with the worst scores – and thus significantly far away from winning – are "cut".[7] These approximately fifty percent of players earn no income in the tournament. The remainder of the golfers that are not cut, earn income that is a share of the total "purse"; this share is increasing in their rank in the tournament. The prizes for winners and golfers at the very top of the leaderboard are much higher than for lower-placing golfers. Thus, golfers face income risk.

The ability to adjust prizes is limited due to strict rules surrounding PGA payout structures (PGA Tour, 2019). Under these rules, for many tournaments, prizes are given by a formula where each placement must receive a pre-determined share of the total purse. Absent approval from the PGA Tour, any attempt to adjust payout structures would require raising the total purse–not just those prizes most attractive for top earners. This limits the ability to attract marginal high quality players by increasing prizes for top placements, especially when tax increases affect only very elite golfers.[8]

The average number of PGA Tour tournaments per year is 34, with the average golfer participating in roughly 46 percent of tournaments. Golfers may choose not to participate in a tournament for a variety of reasons, including rest, overlap with a prominent European event, injury, personal course preferences, purse sizes, or state taxes. While there are reasons 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.

As we will exploit information on state taxes, our analysis will necessarily focus on U.S.-based tournaments. Of course, international tax differences could influence the decision of participating in foreign tournaments. However, in 2017, of the 47 recognized events on the PGA Tour, only 7 were played outside of the fifty U.S. states. This number was substantially lower in earlier years of the Tour, as there were only two internationally recognized tournaments in 1977. Of course, golfers can also participate in European Tour events not recognized by the PGA Tour, but participating in events without a PGA Tour tie is uncommon. For example, the 2017 Irish Open, one of the most prestigious European Tour events, only had 4 U.S. nationals out of 160 entrants.

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

[7] Golfers likely cannot discern that they will be cut until the second round of golf. But if they realize they will not advance to the next round, it is possible this will influence effort.

[8] For example, given the top prize is required to be 18% of the purse in many tournaments, raising the top prize by one dollar requires an investment of $5.56 in the total purse.

1.2 Tax Setting

Golfers are considered self-employed[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 tournament 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 observed, but golfers will often 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 a nonresident income tax in every state they play in.[10] For example, Patrick Rodgers, a typical golfer, competed in 29 events across 17 states in 2016 (Finch, 2017). Golfers are allowed to deduct expenses, which are unobservable to us, from their tax returns. Generally, expenses such as air travel or payments to caddies, are allocated to the tournament state as they are directly related to the "cost of business" there. This means that golfers must file taxes in the state of every tournament they participate in regardless of tournament earnings. Then, golfers file a resident income tax return in their home state. To avoid double-taxation, taxes paid in nonresident states are generally credited on resident state returns.[11] Additional taxes on earnings are only due to the resident state if it levies a higher tax rate than the tournament state. Thus, the *maximum* rate between the employment and resident states determines the effective tax rate.

States also differ in how they apportion nonresident income to calculate 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 method. To see this, let the

[9] A golfer could also incorporate and hire himself as an employee, but this is not common.

[10] Some states have reciprocity agreements (Agrawal and Hoyt, 2018) in which the state of employment agrees to not to tax nonresident income. However, reciprocity agreements only apply to income earned from an employer, not self employed income, and therefore are of little concern in this context.

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

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, 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 multi-state rental income, 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. Thus, the tax rules for other occupations are in line with golfers, which strengthens our decision to focus on golfers, where the full menu of employment choices is known to us.

These rules are not just a curiosity of the U.S. International taxing rules depend on bilateral treaties, treaties. But, as a general rule, athletes in the European Union are taxed based on where the athletic event takes place. More generally, in the EU, income related to real estate is often taxed in the source country and for consulting work, it depends on where the work is performed.

2 Conceptual Framework

Next, we highlight the conceptual issues of why the employment location elasticity differs from the residential mobility elasticity. To highlight the differences, consider how nonresidents and residents influence state income tax revenue. State revenue is:

$$revenue(\tau) = \tau N_R z_R + \tau N_{NR} z_{NR} \quad (1)$$

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 taxable income of the average resident and the nonresident, respectively. Consider a single bracket tax system where τ denotes the tax rate. 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.[12]

[12] Alternatively, define z_g as the revenue raised in the top tax bracket. Then, if the tax system is progressive, our analysis can be interpreted as the effect of the top marginal tax rate on revenue raised from the top bracket. For superstars, the top rate approximates the average rate (Kleven, Landais and Saez, 2013).

We totally differentiate $\ln(revenue)$ to obtain:

$$\widehat{revenue} = \widehat{\tau} + [\eta_R(1 - s_{NR}) + \eta_{NR}s_{NR}]\,\widehat{1-\tau} + [\epsilon_R(1 - s_{NR}) + \epsilon_{NR}s_{NR}]\,\widehat{1-\tau}. \quad (2)$$

where we let the percent change of any variable x be defined by $\hat{x} = \frac{dx}{x}$. Further, s_g is the share of state income earned by group g. For nonresidents, this is $s_{NR} = N_{NR}z_{NR}/(z_{NR}N_{NR} + z_R N_R)$, implying $s_R = 1 - s_{NR}$. Finally, the taxable income elasticities are given by $\eta_g = \frac{(1-\tau)\,dz_g}{d(1-\tau)\,z_g}$ and the mobility elasticities are given by $\epsilon_g = \frac{(1-\tau)\,dN_g}{d(1-\tau)\,N_g}$. The elasticity of taxable income (ETI) captures how individuals adjust their taxable income via labor supply responses and other avoidance strategies. For professional golfers, these labor supply responses will include any intensive margin responses relating to golfer effort induced by tax changes, in so much as changes in effort result in the golfer receiving a lower prize. The mobility elasticities capture the extensive margin response of where to live (for residents) or work (for nonresidents).

The percent 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 term lowers tax revenues. Finally, the third term is the decline in tax revenue resulting from a change in 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 nonresidents from a single jurisdiction (state), then—using a balanced sample of taxpayers—the researcher can estimate a single taxable income elasticity, η. Then, η can replace the first term in brackets in (2). However, using administrative data, the researcher generally does not 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 death, or just because of losing contact with the tax administration. Critically, someone who stops working in the nonresident state will *not* be captured in the taxable income response because studies using individual data generally drop multi-state nonresident income because of the complex changes in state taxes discussed in this paper.

Using administrative data, a researcher could estimate ϵ_R based on current mobility papers. But, we still do not know ϵ_{NR}, which requires a different methodology. This would not be a problem if one could assume $\epsilon_{NR} = \epsilon_R$, but given home biases or the costs of changing jobs not equaling the cost of moving, this is unlikely to be true. Thus, our conceptual model suggests a critical need to separately estimate ϵ_{NR} when nonresident income is an important source of state tax revenue.

To further justify the relevance of this elasticity, data indicates that the share of state tax revenue raised from nonresident income, s_{NR}, is important. In Table 1, we show the fraction of state income tax revenue from nonresidents. To show that this share is not driven by workers living in cross-border urban areas, we also report the fraction of income tax revenue from nonresidents adjusting for cross-border 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.

In an economy where everyone lives and works in the same state, the mobility elasticity, ϵ_R, is equal to the elasticity of employment locations, ϵ_R. However, as globalization makes it so taxpayers increasingly earn income from nonresident jurisdictions, such an assumption is no longer reasonable. The COVID-19 crisis has further increased the globalized nature of taxpayer income. Increases in telework means that individuals make separate decisions over where to live and where to work (Brueckner, Kahn and Lin, 2022). Because not all states tax teleworkers solely based on the residence principle, both ϵ_{NR} and ϵ_R are likely to become increasingly different in the future.

To conclude, a complete characterization of the revenue effects would require estimating η, ϵ_R, and ϵ_{NR}. Our focus, in part due to data limitations and because it is most novel, is the last elasticity. With respect to labor supply responses, as noted in Farber (2005), a complete analysis of the labor supply responses to the net-of-tax rate would also estimate the effects on the intensive margin (effort in a given tournament). Due to data limitations, we cannot investigate the effort response of professional golfers nor can we investigate other avoidance responses that would affect taxable income. Presumably, any effort responses would involve (1) playing less intensely during the tournament, (2) reducing tournament-specific practice, or (3) reducing general practice that improves all tournaments. The second channel might be the most likely margin, but if much of the practice effort is conducted by the golfer's caddie then the response could be limited. Reducing effort may also have implications for endorsement income. However, note that if effort responses to state taxes exist, then those responses will be captured in the standard elasticity of taxable income if lower effort results in a lower prize.

3 Data

3.1 Data Sources

We scrape data on the universe of U.S.-based PGA Tour sponsored golf tournaments between 1977-2018 from golfstats.com. The data include information about golfer participation, tournament prizes, placements, round-by-round scoring, golfer year of birth, golfer nationality, and the location and day of the final round. While this provides a detailed snapshot, in order to properly calculate the participation tax rates, additional golfer information is needed on residential location.[13] Thus, we supplement these data by digitizing yearly residency information from the annual PGA Tour "Media Guides". The combined data then allows us to observe golfers across time, including both the state of residence and the state where income is earned. Infrequently, a golfer may not appear in the "Media Guides" for every year he plays. Appendix A.2 discusses how we handle these cases, which generally involves exploiting residency information before and after the edition that the golfer is missing from. All data sources and citations are enumerated in the appendix.

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 in European Tour events. 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. The unit of observation is golfer×tournament×year.

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. However, the concentration of golfers' residences is in warm, sunny, and *low-tax* places such as Florida, Tennessee, and Texas, which implies the employment tax rate will be salient for 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 participation and not relocation of venues, 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, these changes are uncorrelated with taxes: the average tax difference between new and old state is –0.00008 percentage points.

[13] Unfortunately, golfstats.com biographical information only contains time-invariant residential information (at the time of scraping).

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 based on their *expected* income rather than observed wages. As in Kleven, Landais and Saez (2013), expected income for each tournament is created by constructing a quality index (ability measure). We then apply a grouping estimator (Blundell, Duncan and Meghir, 1998), which is a cell average of earnings in a tournament×year×quality group. 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 tournaments as proxies. Although using exogenous and lagged characteristics, the index is strongly correlated with current-year performance. Then, for each year, we group golfers by deciles of quality and assign those golfers the mean value of earnings in a tournament based on 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.

Next, the decision to work in a state is not based on an average tax rate, but rather on the taxes paid on the additional earnings from participation. To 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 measures the average tax rate from participating in a particular tournament, holding all other income constant. To do this, we construct total expected income $\mathrm{I_{ity}}$ for individual i in tournament t in year y, conditional on participating ($P_{ity} = 1$) or not ($P_{ity} = 0$) in a particular tournament. Then, the PTR is:

$$\mathrm{PTR}_{ity} = \frac{\mathrm{T_{sry}}[\mathbb{E}(\mathrm{I_{ity}}|P_{ity} = 1)] - \mathrm{T_{sry}}[\mathbb{E}(\mathrm{I_{ity}}|\mathrm{P_{ity}} = 0)]}{\mathbb{E}(\mathrm{I_{ity}}|P_{ity} = 1) - \mathbb{E}(\mathrm{I_{ity}}|P_{ity} = 0)} \tag{3}$$

where $\mathrm{T_{sry}}$ represents the tax function facing a golfer playing in state s and living in state r.[15] The relevant tax function is either the state of residence r or the state of employment s, depending on which rate is higher. Then, $\mathbb{E}(\mathrm{I_{ity}}|P_{ity} = 1)$ 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 decided to participate in, allowing participation in only tournament t to vary.[16] Then,

[14] A more detailed explanation of the index is in Appendix A.3.

[15] Note the left-hand side of (3) is not indexed by s or r because (i, y) uniquely pins down a residence state r and (t, y) uniquely pins down an employment state s, making indexes by s and r redundant.

[16] To construct the PTR, we assume golfers decide all participation decisions at the start of the year. Although an assumption, we will provide empirical evidence consistent with this assumption.

$\mathbb{E}(\mathrm{I_{ity}}|P_{ity} = 0)$ represents expected income if the golfer i chooses not to participate in tournament t, but holds all other tournament participation fixed. The denominator is 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 with respect to tournament participation are held constant, including both past and future tournaments in a given year.

Taxes are estimated using NBER TAXSIM (Feenberg and Coutts, 1993) Given that TAXSIM does not currently provide a way estimate taxes for nonresident income, we construct the appropriate tax rates by hand. This requires us to account for the taxes owed on earnings in the resident state from a given tournament, the taxes on earnings in the state of the tournament, and the specific apportionment structure of the state; TAXSIM must be run 7 times for each observation. A thorough explanation and specific examples are in Appendix A.4.

All data sources are cited in the online appendix and all data and code used in this analysis are available in our replication archive (Agrawal and Tester, 2022).

4 Evidence on Superstar Phenomenon

Although not all golfers may be superstars, some golfers on the PGA Tour are superstars. 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. In our context, an interesting extension is how the participation elasticity interacts with superstar phenomenon.

As initial evidence on the convexity of earnings for golfers, we present some descriptive statistics of the earnings distribution. First, Figure 1 shows the dollar value of prizes for each placement in a tournament. To better compare across time, we scale prizes by the average prize in each year. The figure shows 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 tournaments. Top prizes are anywhere between 20 and 50 times that of the average prize. As can be seen, the prize distribution is convex, and with minor exceptions, the markup compared to the average prize is relatively similar across time.

Second, is there a relationship between prizes and tournament quality? The highest prize tournaments are often the most well-regarded, so we use the the total purse or the top prize to rank tournaments. Figure 2 indicates ranking of tournaments by purse size maps almost one-to-one to ranking by the top prize. This is due to the PGA Tour rules that apportion prizes based on a formula (PGA Tour, 2019). There are a few exceptions, but generally speaking, the relationship between the prizes and tournament quality is linear 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 participate in come with much higher prizes. Figure 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 much 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.

Finally, given the linearly increasing purse size across tournament rank, but the convexity of prizes within tournaments, are golfer earnings convex in ability? Figure 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. Panel 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. In panel 4d, we rank golfers based on earnings and again show a convex relationship consistent with superstar effects. Overall, the convexity of golfer earnings is due to: prizes being convex, high ability golfers being more likely to win, and excellent golfers selecting into better tournaments.

5 The Effect of Taxes on Participation

In this section we will use two different estimation techniques—an event study approach and panel data approach—to study how the labor supply decisions of professional golfers are affected by state income taxes. We then compare the estimates from both approaches and discuss various robustness checks.

5.1 Stacked Event Studies

In the context of state tax changes, tax reforms affect various income groups differently, can feature multiple tax changes in close succession, and vary in how permanent the reforms are. Furthermore, it has recently been shown that the two-way fixed effect design with variation in treatment timing may produced biased treatment effects. We modify standard event study designs to address these features. Our modifications draw inspiration from Moretti and Wilson (2017), Cengiz et al. (2019), and Baker, Larcker and Wang (2022).

Following Moretti and Wilson (2017) we focus on "major" tax changes. A golfer's tax rate can change frequently because a golfer's expectation of earnings can change each year and because states frequently change various elements of the tax code (tax rate, tax base, exemption or deduction). First, for this reason, we focus on tax increases of at least one percentage point in absolute value – which allows us to define clear "major" state-level tax reforms. Second, we drop golfers that change quality deciles by more than two deciles or who move residences in the event year. Focusing on golfers of relatively constant quality combined with the large tax increase restriction generally allows us to isolate statutory tax changes rather than changes due to variation in expected income. Then, a golfer is treated if his PTR in a given state-year increases by 1 percentage point (or greater).[17] Critically, treatment status is defined using an *person-specific* PTR, rather than a state-specific tax rate. As a result, a change in the employment state tax rate may not affect individuals living in high-tax states and may affect high and low-income individuals differently. Our treatment and comparison groups account for this.

Next, we follow Moretti and Wilson (2017) to construct "clean" treatment events. In order to create a clean pre-treatment period free of prior 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 a series of major reforms. Second, following Moretti and Wilson (2017), we focus on "permanent" tax changes. 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 by a major tax decrease of one percentage point or more in the following years. However, further tax increases are allowed to follow the first tax increase if states choose to slowly increases taxes over time.

The comparison group for each 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.[18] To generate these, we create separate comparison groups for *each* event by using golfer-state pairs that do not have a major reform in the event year and *any* of the 4 years before or after the event year. Further, a golfers treated in one state but who plays in another state are excluded from the

[17] Alternatively, we could allow the definition of a major reform to also depend on large changes in tournament prizes. Although in standard models, the effects of taxes and prizes should be similar, the literature on behavioral public finance (e.g., Chetty, Looney and Kroft 2009) suggests that prize and tax changes may have different effects. For this reason, and because our paper is about taxes, we exclude major prize changes from our definition of a treated event.

[18] Nontrivial is defined as being a tax change in either direction greater than 1 percentage point. The results are similar if using a 0.5 percentage point cutoff.

comparison group, even if they experience no tax changes in the other states. This avoids contaminating the treatment group due to the golfer shifting toward other tournaments.

Let each major treatment and comparison event grouping that satisfies the above conditions be indexed j. Following, Cengiz et al. (2019), each grouping j, containing a treatment event along with its clean comparisons are then stacked over each other. We then estimate:

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

where P_{ityj} is an indicator of participation for golfer i, in tournament t, in year y, and for event j. The treatment indicator, D_j, takes on the value of 1 if the observation is treated, 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 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}). Identification comes from comparing the treatment group with each event-specific clean comparison group. Thus, we 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, Larcker and Wang, 2022).

We have two sets of controls. First, age indicators (σ_a) control for any common career and life-cycle dynamics. Second, as in a standard labor supply equation, we control for expected earnings, $\ln(w_{dty}^{\mathbb{E}})$ in decile d. Because expected earnings are created using a grouping estimator, they can reasonably viewed as exogenous. We do not include tournament by year fixed effects to ensure the stacked difference-in-difference does not compare treated golfers in the tournament with others considering participation in the tournament, the latter of whom may be affected by taxes deterring competitors to participate if those decisions are made in a Nash game. Further, unlike the literature on tax-induced migration, to identify the pure tax elasticity, we don't need to control for endogenous public services, given nonresidents consume minimal public services. Standard errors are clustered two-ways, by golfer and state.

In addition to studying tax increases, we analyze whether golfers respond differently to tax decreases, following the same rules as before, except with the reverse convention for tax decreases.[19]. We then separately estimate the effect for increases and

[19]For example, for a tax decrease event, we only allow further decreases in the following periods and

decreases. Theoretically, tax increases and decreases may have asymmetric effects if enticing a golfer to attend a tournament not previously played is different from discouraging a golfer from playing in a tournament previously played. The former may have larger fixed costs, as the golfer would need to prepare for an unfamiliar tournament. Regardless of costs, taxes should only matter if the tax change affects a tournament that was marginal for the golfer's participation.

We focus on select tax changes not only because they are big, but also because they are "clean". As a result, it is useful to know which states, time periods, and tax changes we are studying in the event study. Recall that because treatment is based on the person-specific PTR, not all golfers in a given tournament-year are treated by tax reforms: whether a golfer is treated in a given tournament depends on if the tournament's state tax policy changes, but also on his expected earnings and resident state tax rate. Appendix A.5 show which states/years are playing a role in the event studies. The first column shows the number of treated"events" by tournament states. Because the PTR can change because of both employment state and residence state tax changes, this column contains treated events in zero or low tax states that do not change taxes. The second column focuses only on tax changes resulting from the employment state changing its tax rate. The difference between the two columns corresponds to the number of units treated due to residency state changes.

5.1.1 Event Study Results

We first show a "first stage" with respect to how major tax reforms affect the participation tax rate. Figure 5 shows the results of estimating (4) where the outcome variable is the participation tax rate. Each major tax change raises the PTR by about 2 percentage points.[20] The mean increase for all golfers is very similar to the increase for golfers in the top 25%. The reason is that in the event studies, we focus on large state tax changes and these are generally at the top of the distribution.

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 on, among other things, the residential location of the golfer, the figures also show how changes in the PTR map to changes in the average tax rate on

do not allow for offsetting increases. This is similar to Moretti and Wilson (2017)

[20] Although we focus on permanent tax increases, there is some mean reversion, similar to Agersnap and Zidar (2021). These trends arise because the PTR depends on expected earnings. Although we restrict the sample to individuals that do not change more than two deciles, there is some mild mean reversion and we cannot focus on golfers that never change deciles over a ten year period.

income in the employment state. Both measures move very closely, where the employment state ATR change is around 25 percent lower than the PTR change. This is because the ATR excludes changes in the resident state and because changes in statutory rates shift the average tax rate less than marginal rates due to progressivity. PTR changes are somewhere in between the two. Similar patterns hold for the sample of tax decreases.

Figure 6 estimates (4) when the dependent variable is participation. We show results separately for golfers with positive earnings and golfers in the top 25% of quality. Panels (a) and (b) focus on tax increases, while panels (c) and (d) studies tax decreases. Following a tax increase, participation declines. Interestingly, the effects of tax increases are larger, with almost no response to decreases. This suggests that eliminating a tournament is easier than adding a new tournament, perhaps because it requires the golfer to incur fixed costs of preparation. With respect to heterogeneity by golfer earnings, we see little heterogeneity in the participation response, although the effect on top golfers is more persistent. Given these figures focus on major reforms, the reforms are highly salient, representing a non-trivial tax change for both sets of golfers.

In terms of magnitudes, for golfers with positive earnings, a tax increase lowers the participation rate in the first year after the reforms by 0.086. Given the average participation rate in the pre-period for the treatment group is 0.56, this corresponds to an approximately 15% decline. We can then scale this by the percent change in the net-of-tax rate to obtain an elasticity. Although Figure 5 shows the changes at the state level, we rescale by percent changes in the state plus federal tax rate. This has the advantage of capturing any deductibility of federal taxes and any federal tax changes that are contemporaneous to state reforms.[21] Moreover, such a rescaling will be comparable with our subsequent analysis that exploits changes in the total rate and, in the next section of the paper, will allow us to compare this elasticity to an elasticity obtained using all tax changes. The implied elasticity from our event studies of golfers with positive earnings is 2.122. For top 25% golfers, the elasticity is 1.929.

Next, we show the results separately for tournaments where a golfer's resident state is low-tax or high-tax relative to the employment state. If the distortion to the location of participation is due to state taxes, then employment state tax rates should explain more of the result, as resident state taxes change the participation tax rates for all tournaments in a state that has a lower tax rate.[22] A golfer living in a high-tax

[21]This rescaling by the percent change in the state plus federal rate inherently makes the elasticity smaller through two mechanisms. The first is a smaller net-of-PTR due to federal taxes, so a 1 percentage point change in the state tax rate would shift the state plus federal net-of-PTR by 1.67 percent versus 1.03 percent. The second reason is that federal taxes are also allowed to change contemporaneously. Indeed, many states link elements of their tax law to the federal tax code. Thus, any federal changes, presumably in the same direction, will increase the total change in taxes for a given behavioral response.

[22]Unlike federal tax changes, resident state taxes can still cause a spatial distortion of the location

state is defined separately for *each* employment state as a golfer who has zero expected employment state tax liability from participating in an event there, and thus any change in his PTR would come from tax changes in his resident state.

Figure 7 shows the results separately for golfers living in low-tax states and high-tax states. Given the prior results were similar for top golfers and golfers with positive earnings, we focus on the larger sample of golfers for this analysis. Focusing on golfers living in low-tax states yields a larger decrease (-0.126) in participation rates than our prior results (-0.086). The event studies indicate that when the residence state is higher tax, golfers are unaffected, providing evidence that the mechanism underlying our responses is a spatial distortion due to nonresident state income taxes. Nonresident state tax matter more than resident state tax changes because nonresident state tax changes distort the relative price of playing in tournaments in *all* other states, while resident state tax changes alter the relative price with only a subset (and if the highest tax state, none) of the other tournaments.[23]

5.2 Panel Data Estimation

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. To exploit all tax changes and to obtain an elasticity of labor supply for small and large changes, we now turn to a standard panel data design.

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

$$P_{ity} = \beta_0 \ln(1 - \text{PTR}_{ity}) + \beta_1 \ln w^{\mathbb{E}}_{dty} + \mu_i + \delta_{ty} + \sigma_a + u_{ity} \tag{5}$$

where P_{ity} is an indicator variable for participation for golfer i, in tournament t, of year y. And $(1 - \text{PTR}_{ity})$ 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.[24]

of participation due to relative changes in tax rates, as increases in the residential rate raises the participation tax rate for participating in lower-tax states, but has no effect on the participation rate for tournaments in higher-tax states.

[23]The null effect in high-tax residence states also suggests any wage incidence effects are small. This result is confirmed in event studies that show how the purse, prizes and expected earnings do not change after a tax increase (see the discussion in Appendix A.6). As discussed previously, incidence may be limited due to strict rules surrounding PGA payout structures (PGA Tour, 2019).

[24]Tournament-year effects were excluded in the event study specification because they would have forced the comparison to be within tournament, meaning the comparison group will *exclusively* consist of golfers in tournaments that are treated, but who were not treated to the same extent. This is especially problematic in the event study because we used a threshold size to define large tax changes. Excluding these fixed effects in the event study circumvents this issue. However, when exploiting continuous tax changes, tournament-year effects are useful to eliminating confounding state-year level shocks.

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., $\mathbb{E}(u_{ity}|\text{PTR}_{i1y}, ...\text{PTR}_{iTy} = 0)$). In order to overcome this, we use year-over-year differences within a tournament (eg., Safeway 2018 - Safeway 2017). If tournament quality is generally constant over time, the primary source of variation would come from changes in the tax rate. We estimate:

$$\Delta P_{ity} = \beta_0 \Delta \ln(1 - \text{PTR}_{ity}) + \beta_1 \Delta \ln w^{\mathbb{E}}_{dty} + \delta_{ty} + \sigma_a + \Delta u_{ity} \tag{6}$$

where the Δ operator represents the tournament-year difference.

Standard errors are clustered at both the golfer and the state of the tournament levels. However, the participation tax rate is based on an estimated measure of golfer quality. Following the conventions in the prior literature, the expectation that enters our tax rate is a group mean rather than a golfer-specific predicted measure of quality. Nonetheless, some unmodelled uncertainty may remain because those group means are based on a predicted variable. For this reason, we also calculate bootstrapped standard errors following the procedure discussed in Appendix A.7. We present both simple clustered standard errors and bootstrap clustered standard errors, which given the prior literature has used the former allows the reader to assess the extent of the uncertainty from estimating quality.

Although use of the PTR is correct for labor supply elasticities, the policy parameter is the elasticity with respect to statutory components of the tax code, summarized in the average tax rate. Thus, we estimate a variant of (6), which yields the effect of tax rate changes from the simulated average tax rate in the employment state, ATR, on the PTR:

$$\Delta \ln(1 - \text{PTR}_{ity}) = \alpha_0 \Delta \ln(1 - \text{ATR}_{ity}) + \alpha_1 \Delta \ln w^{\mathbb{E}}_{dty} + \delta_{ty} + \sigma_a + \Delta u_{ity}. \tag{7}$$

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 only the state of employment. Such an elasticity is useful for revenue implications.

To study the effects of employment-state policy changes, we can transform our elasticities with respect to the PTR by multiplying by our estimates in (7) to obtain:

$$\epsilon_{1-ATR} = \epsilon_{1-PTR} \cdot \frac{d\Delta\ln(1 - \text{PTR})}{d\Delta\ln(1 - \text{ATR})}. \tag{8}$$

Ultimately, the panel results are robust to including or excluding tournament by year fixed effects.

The transformation yields the policy-relevant elasticity. When presenting these elasticities, we use the parametric bootstrap to obtain confidence intervals.

5.2.1 Panel Data Results

The results estimating (6) are presented in Table 2. Overall, a one percent increase in the net-of-PTR increases the participation of golfers by 0.155 percentage points.[25] We convert each coefficient to an elasticity using the average participation rate of each estimating sample. Given, on average, 46 percent of golfers participate in a tournament, the implied participation elasticity is 0.34. Such an elasticity is larger than standard intensive margin elasticities for high income earners in labor economics (Moffitt and Wilhelm, 2000), but smaller than many residential mobility elasticities (Kleven et al., 2020). This elasticity is very similar when restricting to golfers with positive earnings for the year.

As noted previously, the policy relevant elasticity is not with respect the PTR, but with respect to 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 (8). As indicated in Table 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 0.178. Intuitively, the average tax rate is less than or equal to the participation tax rate, so that $1 - ATR \geq 1 - PTR$. 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 PTR larger.

The remainder of the table explores heterogeneity by golfer characteristics. Column (2) shows the results are almost identical (an elasticity of 0.335) when focusing on golfers with positive earnings. This is our preferred sample because it eliminates golfers that may be participating for non-monetary reasons. Column (3) excludes golfers who fail to make the cut, because these golfers may have a different incentives for playing golf. 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 $y - 1$.[26] Current year earnings may be a poor approximation for what a golfer would *expect* to earn due to hot hand effects (Livingston, 2012) or due to a golfer's inability to forecast future performance. Columns (4) and (5) focus on middle-income and top golfers, respectfully. Top 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

[25]Given the mean net-of-tax rate, a 1% increase implies a 0.61 percentage point change in the PTR.

[26]We have verified that the relationship still holds cutting on longer lagged, average past, or contemporaneous earnings, although with smaller elasticities the more current are earnings for top golfers.

distribution (median earnings \$1.07 million in 2018) have an elasticity of 0.79, which is more than twice as large as lower income golfers. The results in column (5) also show that higher 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 (8), the elasticity with respect to the net-of-ATR is $0.79 \times 0.83 = 0.66$. We have verified the results are robust to excluding all golfer-year observations where the golfer changes residency states.

In Table 2, the effect of expected earnings is smaller than the effect of taxes. There are several explanations. 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, top earners may just not like paying taxes. Third, 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 $w_{dty}^{\mathbb{E}}$ than for $1 - \mathrm{PTR}_{ity}$. Intuitively, any measurement error in the tax term is mitigated by the division in (3). To see this, note that if tax systems are flat, then $1 - \mathrm{PTR}_{ity}$ contains no measurement error as the division perfectly cancels income from the expression and returns the flat tax rate. For progressive taxes, such cancellation will not eliminate measurement error, but the division will mitigate any noise in the participation tax rate. Thus, we expect the attenuation bias on taxes to be minimal compared to the earnings term. Appendix A.8 provides further econometric evidence.

Next, to examine heterogeneous effects more flexibly than in Table 2, we estimate (6) by interacting the model with annual decile indicators of ability. The first set of decile indicators are constructed using the annual actual earnings in $y - 1$. The latter set uses our quality index to construct indicators of ability. The very fine interaction with deciles allows us to test for the existence of heterogeneous effects on superstars at the very top of the distribution, which allows us to relate our results to the literature on superstar effects. The results of these estimates are presented in Figure 8. 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 lower income golfers facing less salient tax differentials. Both figures show a clear, positive relationship between earnings and taxes after the sixth decile. Focusing on the most elite golfers, the mean income in the 10th decile is \$1,100,000.[27] At the 10th decile, the

[27] These bins are constructed by year, so the 10th decile in 1980 has a mean income of \$380,000 while 10th decile in 2018 has a mean income of \$1,800,000.

implied elasticity with respect to the net-of-PTR is roughly 2.09 and is more than twice as large as the top quartile of golfers studied previously, suggesting a strong response to taxes at superstar income levels. The increasing elasticity over golfer ability, consistent with the literature on superstar effects, raises the earning elasticity as ability increases. Note that this although this flexible specification shows that higher-earners have a higher *participation* elasticity, it likely understates the rise in the *earnings* elasticity because, as shown previously, these higher earners typically participate in tournaments with higher prizes. Although these elasticities for superstars are large, as shown in Appendix A.9, states are well to the left of the peak of the Laffer curve for taxing nonresident superstars.

Although Figure 8 can be translated to an elasticity with respect to the net-of-PTR, rescaling to an elasticity with respect to the net-of-ATR requires knowing how the ATR influences the PTR over the deciles of ability. Figure 9 shows how the net-of-PTR changes with respect to the net-of-ATR over the ability distribution. We estimate (7) by interacting the model with decile indicators of earnings or quality from our index. Figure 9 presents estimates of the interacted version of (7). 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.

To verify we are identifying a spatial distortion and not simply a decline in the number of total tournaments played, we compare estimates where the PTR depends only on state tax changes and only federal tax changes. Table 3 presents these results. To obtain elasticities with respect to state tax rates, we scale the coefficients such that the elasticities are comparable to the tax changes in Table 2. The elasticities are slightly larger when exploiting only state tax changes. We then 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.[28] This specification does not have any spatial variation in taxes and utilizes only temporal variation in tax rates. The estimated elasticities are approximately zero. Given that federal taxes do not reduce the number of times a golfer participates in tournaments, our strong state tax results suggests that state taxes spatially distort employment rather than simply reduce the number of tournaments played. Thus, we conclude that the mobility response in our theoretical model drives the results, rather than a taxable income response.

5.2.2 Tax Changes in the Residence versus Nonresident State

We also exploit a unique feature of U.S. tax law that essentially makes the effective tax rate the maximum of the employment and residence rates. As a result, when individuals

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

live in a high-tax state relative to the tournament state, additional taxes will always be due to the state of residence, implying that the lower-tax employment state tax rates are irrelevant for participation decisions. Thus, residence state tax changes will only change the after-tax price of playing in all states that are lower tax. Spatial distortions from residence state tax changes are mitigated relative to employment state changes, which distort the relative after-tax price of playing in that one state with respect to all other states. At the extreme, for a golfer living in the highest tax state in the country, residence state changes are like federal tax rate changes.

To study this heterogeneity, we define a golfer as living in a low-tax state relative to a tournament state if the golfer does not have positive tax liability in the residence state after paying taxes to the employment state. Otherwise, a golfer is defined as living in a high-tax state relative to the tournament state. Table 4 shows results of interacting the net-of-PTR and expected earnings with this indicator. When residency rates are higher than employment state rates, the elasticity is generally zero and insignificant. Even for golfers in the top quartile, this elasticity is 0.21. But, focusing on cases where the residency rate is lower tax, indicates a much stronger response, with the elasticity at the top of the distribution rising to 1.17. Thus, for the reasons above, residence state taxes appear to be a less important factor for where to work. Instead nonresident state taxes drive the result.

As in the event studies, 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.

To further address this issue, we add another layer of differencing that uses golfers residing in relatively high tax states as a comparison to golfers residing in relatively low-tax states. 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: higher pre-tax prizes make a tournament more lucrative. This additional difference then allows us to purge out any incidence effects assuming that residency rate changes do not directly distort the participation decisions of golfers living in high-tax states (i.e., they act like federal rate changes) and that the incidence effects in response to home state tax changes are similar to those of employment state tax changes. While the first assumption is strong, it allows us to think of what is necessary to place a lower bound on the elasticity.

The differenced elasticity derived from the coefficient on the interacted term in Table 4 shows these triple difference estimates. As can be seen, this additional layer of differencing does not matter much for the elasticity of golfers living in relatively low-tax states. For golfers with positive earnings, the elasticity prior to differencing is 0.668, but after using high-tax state golfers to difference out any wage incidence, the elasticity is 0.700. Under the assumptions above, 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.

5.3 Comparing Event Studies and Panel Models

With these results in hand, we can compare the elasticities from the event studies with the panel data model. Given the event studies focus on only state tax changes, we compare the implied elasticities from the event studies to state tax elasticities in Table 3. In the event studies, recall we estimated a short-run elasticity of 2.12 for golfers with positive earnings and an elasticity of 1.93 for golfers in the top 25%. The comparable elasticities for state tax changes in Table 3 are 0.54 and 1.00, respectively. The event study elasticities are clearly larger.

We reconcile these elasticities by reestimating our panel data model using the same set of restrictions imposed in the event studies (permanent state tax changes of 1 percentage point or greater that are not pre-dated by other reforms; excluding golfers that substantially change quality deciles; and increases and decreases estimated separately). After doing this, the elasticities from the panel data model are 2.06 for all golfers and 2.43 for top golfers. These elasticities are slightly larger, but statistically indistinguishable from the elasticities in the event studies. Table 5 summarizes the elasticity comparisons between the event studies and the panel model. Given imposing the restrictions from the event studies appears to reconcile the elasticity estimates, any differences with the elasticities in Table 3 are mainly a result of pooling both large and small tax changes in addition to pooling tax increases and decreases.

5.4 Robustness

In this section we verify robustness of the empirical model and the assumptions we made to calculate participation tax rates.

First, our 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. We estimate our model including golfer-specific linear time trend, a quadratic time trend and a double-differences model (Kniesner and Ziliak, 2015). Controlling for heterogeneity in the growth term leaves the elasticity results generally unchanged with our baseline elasticity for golfers with positive earnings (0.335) falling to between 0.318 and 0.323 in these specifications. This suggests that the results are not being driven by golfer-specific differences in earnings profiles nor by growth in prizes over time.

Second, 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 explain observed earnings based on golfer characteristics. Earnings are scaled to be between zero and one, where zero corresponds to missing the cut and one corresponds to the top prize. We then calculate fitted earnings for participants and non-participants using this model. We divide predicted earnings into deciles and assign the cell means to construct our grouping estimator. As as discussed in Appendix A.10, the results are robust to this alternative grouping estimator.

Third, golfers are also exposed to income risk and expected earnings may not capture this risk. To address this, we calculate the standard deviation of expected income in each decile and control for it in the regressions. 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. After controling for risk, the elasticites increase slightly to 0.48 for golfers with positive earnings and to 1.16 for elite golfers.

Fourth, in constructing the participation tax, we assume that golfers make a one-time decision to participate in tournaments at the start of the year. 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. Appendix A.11 shows this does not appear to be a concern. For states with only one tournament, the sequence of tournaments in that state is irrelevant. The coefficients in these states appear to drive our results. States with more than one tournament have smaller effects – but given only a few states are in this category, confidence intervals are large.[29] Instead, what we want to

[29]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

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.

Additionally, we study whether tournaments early in the season are different from those later in the season. We find no systematic differences in the effect of taxes on golfer participation by month. Furthermore, inspired by Farber (2005) who shows that the probability of stopping work is related to hours worked up to that point, we calculate the median number of tournaments played in by a golfer and analyze whether state taxes have a bigger or a smaller effect in the tournaments before/after the golfer hits that median. Again, we find no systematic differences. Finally, we modify our baseline regression to control for the cumulative number of tournaments participated in before each tournament and the cumulative earnings before the tournament. Appendix A.11 shows that controlling for cumulative participation, similar to Farber (2005), yields no meaningful differences. We conclude that our assumption that golfers make a one-time participation decision is valid, or at least, that any changes a golfer makes during the year are idiosyncratic (e.g., resulting from injury).

Fifth, it may be tempting to include golfer-by-year fixed effects in our specification to more precisely control for any PGA Tour season-specific golfer variation. Notably, these fixed effects will eliminate almost all variation from the federal tax system, which may make their inclusion appealing. However, these fixed effects would also eliminate variation in the resident state tax code, but only for some golfers that live in the highest tax states. Residence tax variation is important and, for this reason, we exclude these fixed effects. Moreover, including these fixed effects would treat a golfer living in the highest tax state differently than a golfer living in a lower tax state.

Nonetheless, we have explored specifications including golfer-by-year fixed effects. Adding them to the state PTR specifications makes the point estimates larger, particularly for high income earners, but does not change the results in any systematic way. Comparing the results to Table 3 that exploits only state tax changes, the elasticity for golfers with positive earnings rises to 0.74, while the elasticity of elite golfers rises to 1.56. As expected, the magnitudes increase because these fixed effects remove some variation in residence tax rates. Consistent with the results in Section 5.3, changes in employment state tax rates distort the price relative to all other tournaments, while changes in resident rates change only the relative price compared to some tournament states.

Finally, golfers should be less responsive to taxes for prestigious tournaments. Although we have eliminated Majors from the analysis, using the total purse each year as a proxy for quality, we partition the approximately 35 tournaments each year into

in states with one tournament versus states in multiple tournaments.

quartiles. Appendix A.11 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. State taxes affect golfer participation decisions when a tournament is the marginal tournament.

6 Conclusion

We document a novel behavioral response for top earners: adjustments to the location of employment contracts, independent of residential relocation. We estimate a high-frequency labor supply participation response for self-employed workers that is increasingly relevant in the digital economy. Using variation in the location of professional athlete events and changes in state tax systems, we show that high-income superstars are more likely to play in places with lower state tax rates. The place of employment is responsive to taxes because the U.S. tax system, 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 elasticity 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. States are also likely to enforce taxes on nonresident athletes more than on other nonresident workers. 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 in order 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, Landais and Saez (2013)'s study of football players triggered a literature on the effect of taxes on *residential* mobility. Researchers could study the work location decisions of consultants, pass-through owners, 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 using administrative data will grapple with the lack of information on *declined* offers, but our methodological approach to deriving tax rates on nonresident income completely generalizes to administrative data.

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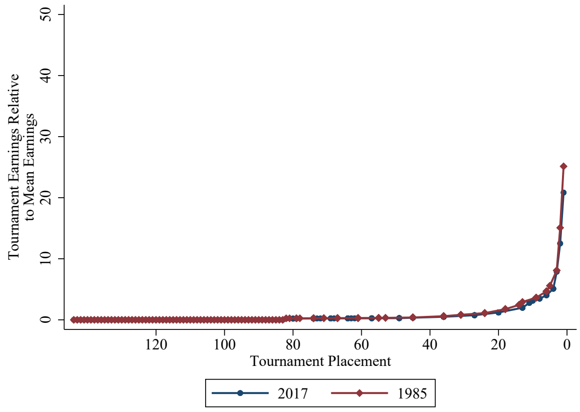
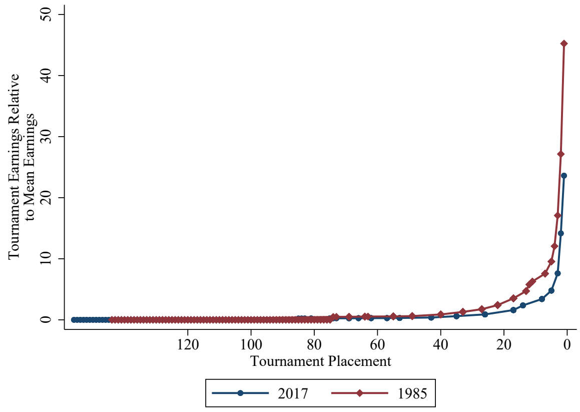
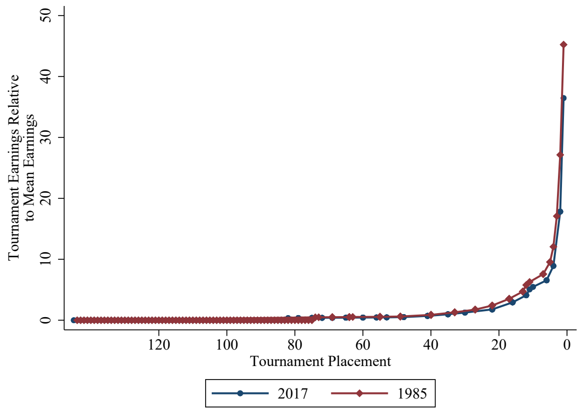
Figure 1: Relative Earnings by Placement for Select Golf Tournaments



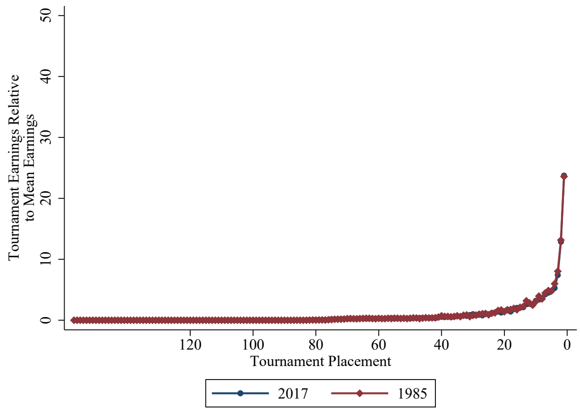
(a) Sony Open



(b) Travelers Championship



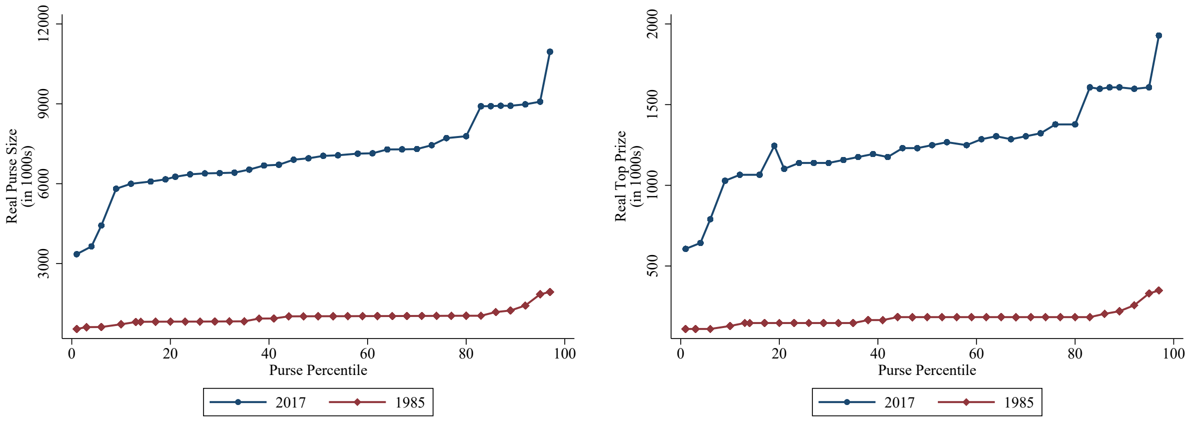
(c) Players Championship



(d) Average of All Tournaments

This figure shows tournament prizes based on golfer placement. To ease interpretation and to avoid capturing differences in the purse sizes over time, we scale each year's series by the mean prize, inclusive of golfers cut, in the year. In this way, the vertical axis is interpreted as earnings relative to the mean prize. 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) lines show golfer earnings in 2017, while the red (diamond) lines show 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.

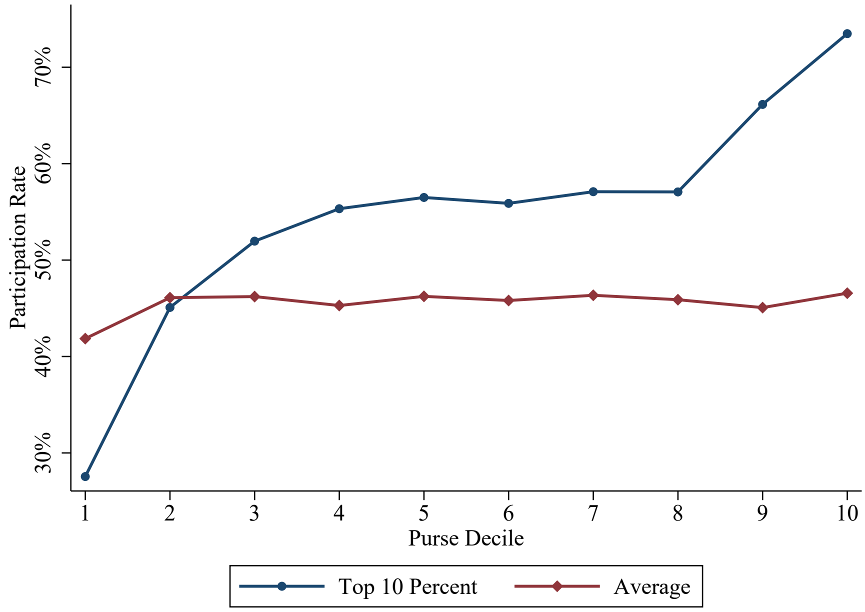
Figure 2: Tournament Payouts by the Size of the Purse



(a) Total Purse (b) Top Prize

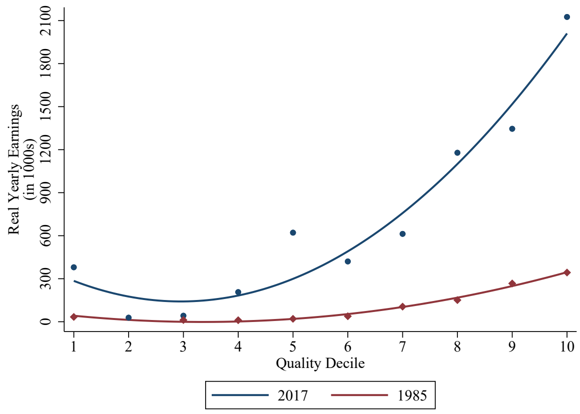
This figure shows tournament payouts based on a tournament's ranking. Tournaments are ranked using the total purse size; survey evidence among golfers indicates that purse sizes are highly correlated with tournament rank. The blue (circle) lines show the purse/top prize in 2017, while the red (diamond)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

Figure 3: Participation Rate by Purse Decile: Top Golfers

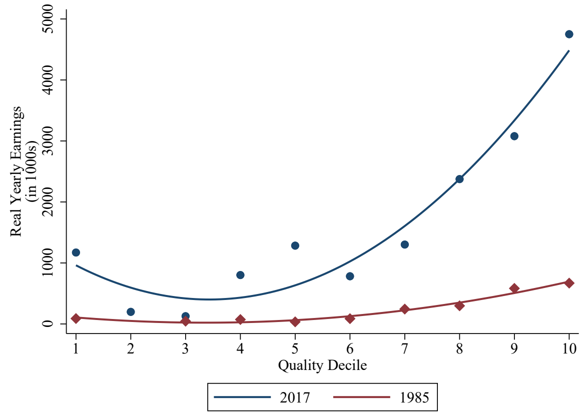


This figure shows the participation rate of golfers. For each year, we rank tournaments by the size of the purse, with the tenth decile having the largest purse. We also use our quality index to partition out the top 10% of golfers from the average golfer. Golfers and tournaments are ranked each year 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 our quality index.

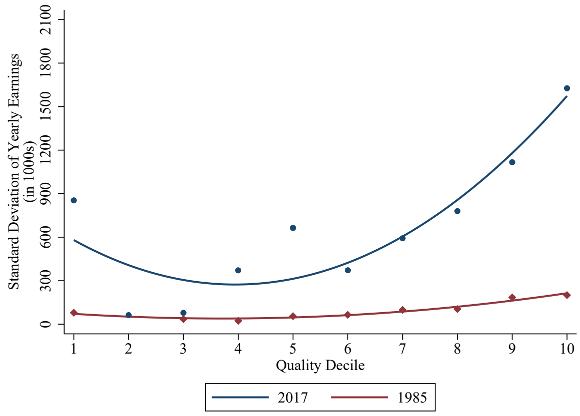
Figure 4: Yearly Earnings by Quality Decile



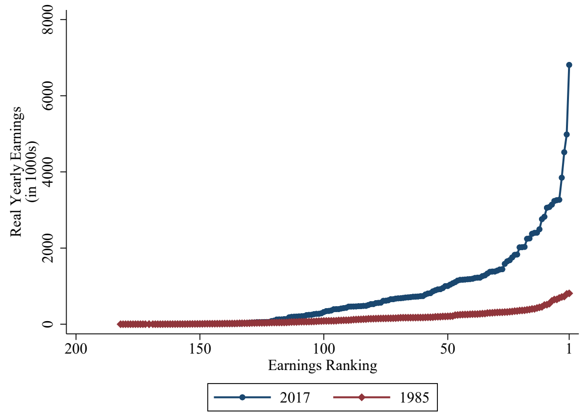
(a) Average Earnings in Decile



(b) 90th Percentile in Each Decile



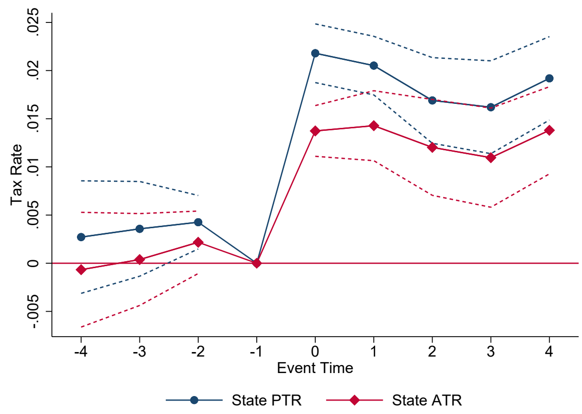
(c) Standard Deviation by Decile



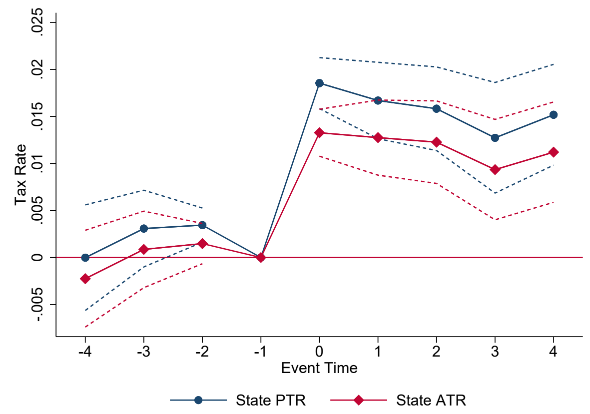
(d) Yearly Total Earnings by Golfer Rank

This figure shows total earnings by golfer deciles of our index measuring golfer quality. The blue (circle) lines show golfer earnings in 2017, while the red (diamond) lines 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.

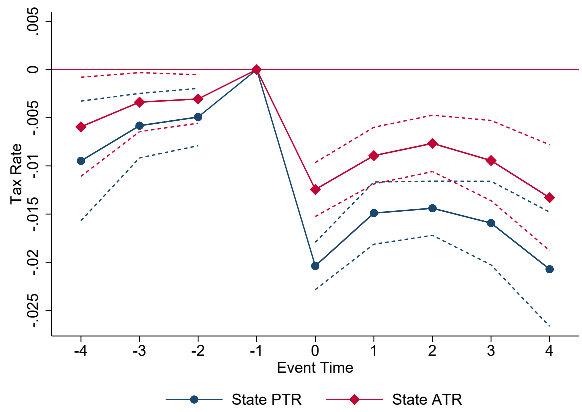
Figure 5: Tax Changes from Major Tax Reforms in Stacked Event Studies



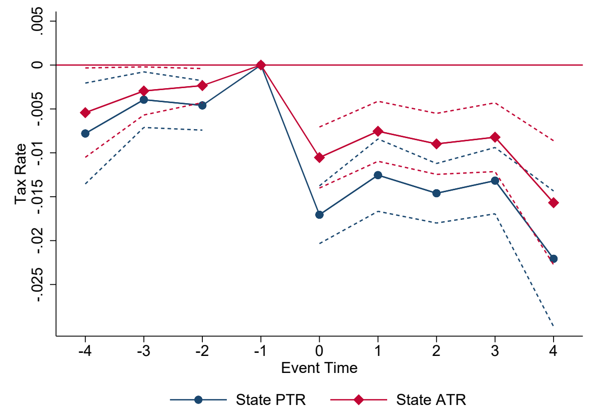
(a) Tax Increases: Positive Earnings



(b) Tax Increases: Top 25 Percent of Earnings



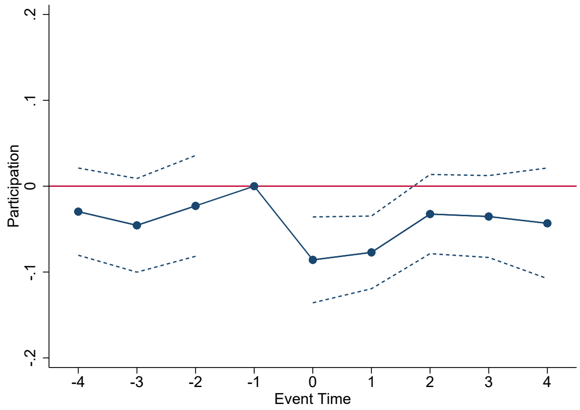
(c) Tax Cuts: Positive Earnings



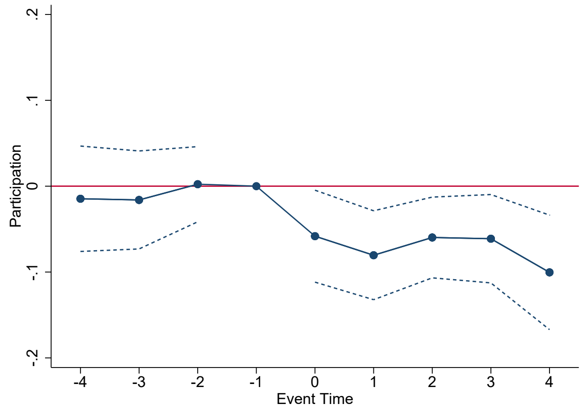
(d) Tax Cuts: Top 25 Percent of Earnings

These figures show how the timing of major tax reforms translate into changes in the PTR and the ATR. Panel (a) shows the effect of major tax increases for 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 only tax decreases. A major tax reform is an increase (decrease) 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 as dashed lines.

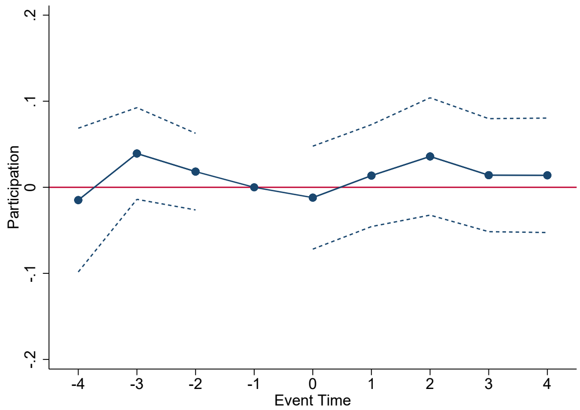
Figure 6: The Effect of Major Tax Reforms on Golfer Participation in Stacked Event Studies



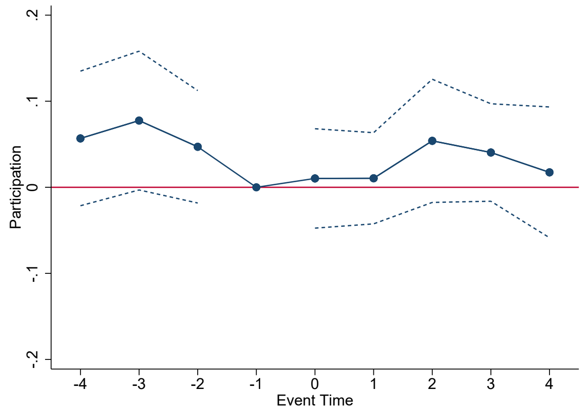
(a) Tax Increases: Positive Earnings



(b) Tax Increases: Top 25 Percent



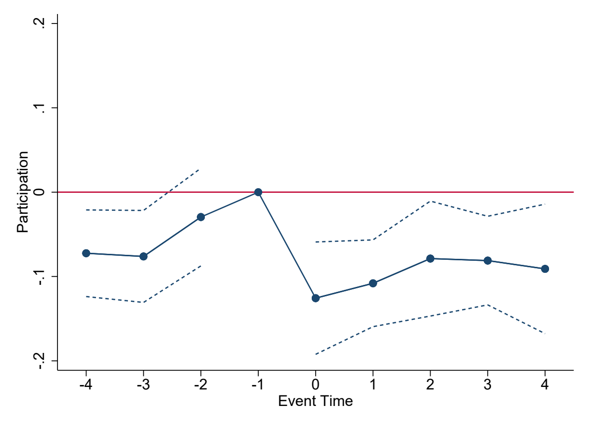
(c) Tax Cuts: Positive Earnings



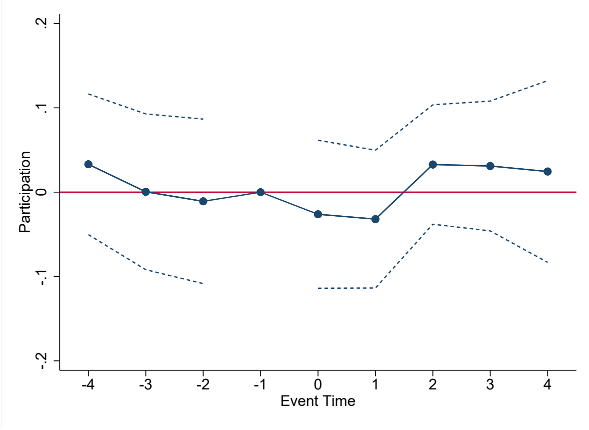
(d) Tax Cuts: Top 25 Percent

These figures show 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 only tax decreases. 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 as dashed lines.

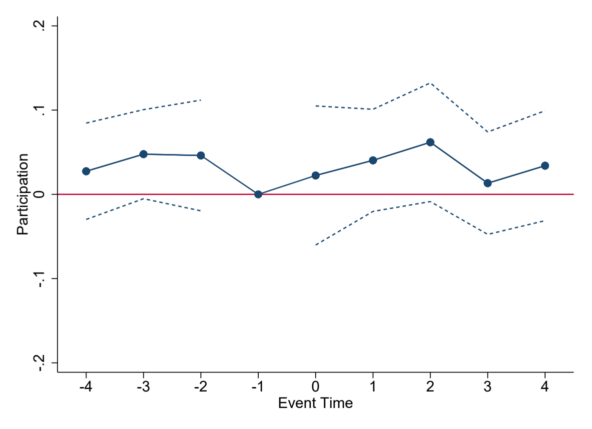
Figure 7: Participation Depending on if Residence State is Relatively High or Low Tax



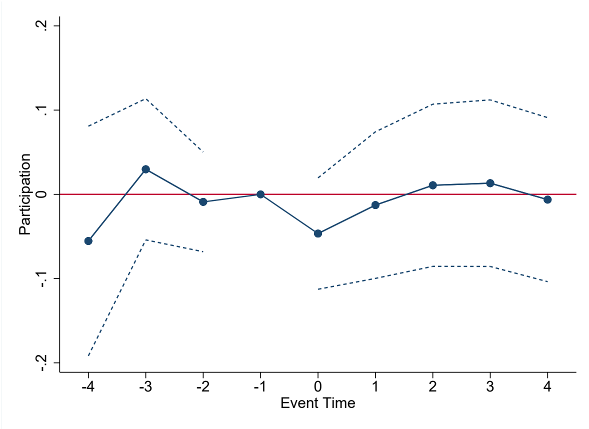
(a) Low-tax State Increases: Positive Earnings



(b) High-tax State Increases: Positive Earnings



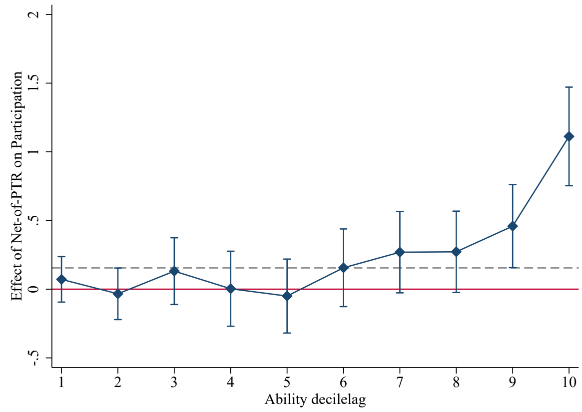
(c) Low-tax State Cuts: Positive Earnings



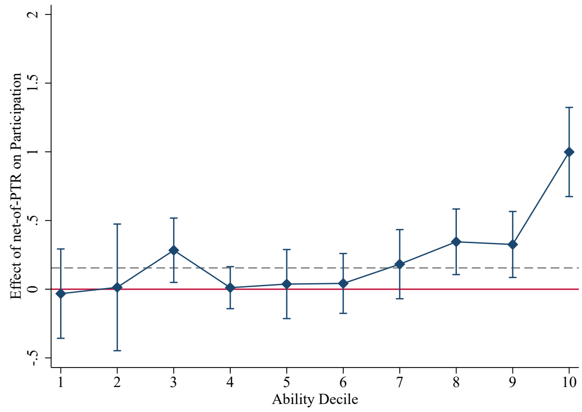
(d) High-tax State Cuts: Positive Earnings

In this figure, we focus on the sample of golfers with positive earnings. These figures show how the effect of major tax reforms on golfer participation using a stacked event study. We separately show the effect when golfers live in low-tax states relative to the tournament state (Panels a and c) and golfers living in high-tax states relative to the tournament state (Panels b and d). Low-tax is defined as the golfer not having positive tax liability in the residence state after paying taxes to the employment state. For golfers living in high-tax states, tax changes in the state of employment theoretically should not matter, minimizing the possibility of a spatial reallocation of tournaments. For these figures, we look at tax increases (Panels a and b) and tax decreases (Panels b and d) separately. 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 8: Heterogeneity of the Effect of Taxes on Participation by Income and Quality Deciles



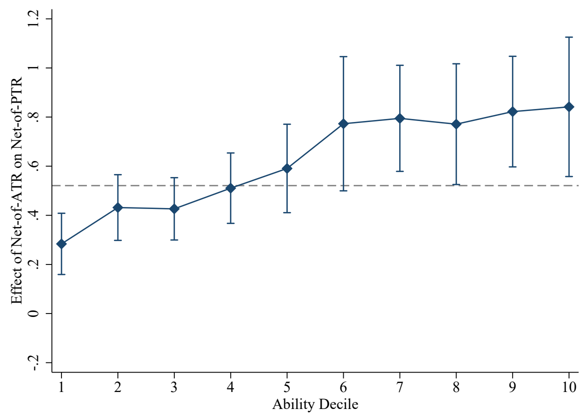
(a) Lagged Income Decile



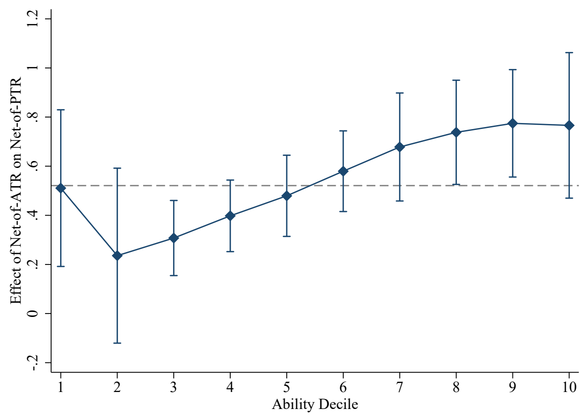
(b) Quality Decile

This figure estimates a specification similar to column 1 from Table 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) or indicators for our quality index deciles (Panel b). We plot the marginal effect of an increase in the net-of-PTR on participation for each decile. The grey dashed line represents the coefficient estimate from Table 2 column 1. Standard errors are clustered at the golfer and state of tournament level and bars indicate 95% confidence intervals constructed from bootstrapped standard errors.

Figure 9: Heterogeneous Effects of ATR on PTR by Income and Quality Deciles



(a) Lagged Income Decile



(b) Quality Decile

This figure flexibly regresses log(1-PTR) on log(1-ATR) in a specification similar to column 1 from Table 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 quality 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 constructed from bootstrapped standard errors.

Table 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	7.68%	3.73%	

This table presents the amount of nonresident income tax revenue collected for all states that release public statistics on nonresident income tax revenue. The column "nonresident income tax noncommuters" removes an estimate of revenue from cross-border workers that commute across state lines daily. 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 cross-border commuter share of total income from the share of nonresident income taxes. This isolates the fraction of tax revenue from nonresidents who are not interstate commuters. All revenue data is sourced from individual state tax websites.

Table 2: The Effect of Taxes on the Location of Participation: Baseline Results

	(1) Baseline	(2) Earnings > 0	(3) Excludes Cut	(4) Lagged Percentile 25th-75th	(5) Lagged Percentile 75th-100th
$\Delta \ln(w^{\mathbb{E}}_{dty})$	0.012	0.014	0.017	0.020	0.020
	(0.003)	(0.004)	(0.003)	(0.004)	(0.007)
	{0.004}	{0.004}	{0.004}	{0.010}	{0.005}
$\Delta \ln(1 - \text{PTR}_{ity})$	0.155	0.174	0.199	0.124	0.462
	(0.056)	(0.066)	(0.055)	(0.073)	(0.192)
	{0.068}	{0.078}	{0.066}	{0.095}	{0.134}
$\frac{d\Delta \ln(1-\text{PTR})}{d\Delta \ln(1-\text{ATR})}$	0.521	0.599	0.506	0.638	0.833
	(0.027)	(0.033)	(0.027)	(0.042)	(0.055)
	{0.062}	{0.065}	{0.062}	{0.084}	{0.111}
$\epsilon_{1-\text{PTR}}$	0.341	0.335	0.609	0.226	0.793
	[0.048,0.634]	[0.042,0.628]	[0.209,1.009]	[-0.114, 0.567]	[0.342,1.243]
$\epsilon_{1-\text{ATR}}$	0.178	0.200	0.308	0.144	0.660
	[0.024,0.344]	[0.024,0.389]	[0.103,0.537]	[-0.072,0.374]	[0.272,1.103]
Observations	269,807	232,138	218,087	127,195	66,893

This table shows the results of estimating equation (6). 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 that are simply clustered at the golfer and the state of the tournament level are in parentheses, ( ), while bootstrapped standard errors are in braces, { }. Ninety-five percent confidence intervals on the elasticities are in brackets, [ ], and are obtained using a parametric bootstrap utilizing the latter standard errors.

Table 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					
$\Delta \ln(1 - \text{PTR}_{ity})$	1.829	2.041	1.839	1.990	3.324
	(0.451)	(0.508)	(0.443)	(0.533)	(0.923)
	{0.609}	{0.697}	{0.587}	{0.768}	{1.222}
$\frac{d\Delta \ln(1-PTR)}{d\Delta \ln(1-ATR)}$	0.939	0.968	0.931	0.979	1.043
	(0.036)	(0.031)	(0.036)	(0.038)	(0.022)
	{0.136}	{0.146}	{0.157}	{0.186}	{0.125}
$\epsilon_{1-\text{PTR}}$	0.520	0.535	0.730	0.501	1.003
	[0.199, 0.841]	[0.188,0.882]	[0.273, 1.187]	[0.121,0.879]	[0.279, 1.727]
$\epsilon_{1-\text{ATR}}$	0.488	0.518	0.680	0.490	1.047
	[0.177,0.846]	[0.173,0.918]	[0.236,1.208]	[0.111,0.948]	[0.284, 1.886]
Federal Taxes					
$\Delta \ln(1 - \text{PTR}_{ity})$	0.010	0.007	0.021	-0.047	-0.269
	(0.047)	(0.051)	(0.032)	(0.069)	(0.100)
	{0.098}	{0.111}	{0.094}	{0.135}	{0.216}
$\frac{d\Delta \ln(1-\text{PTR})}{d\Delta \ln(1-\text{ATR})}$	0.373	0.603	0.364	0.373	0.530
	(0.040)	(0.034)	(0.039)	(0.061)	(0.098)
	{0.072}	{0.077}	{0.071}	{0.083}	{0.142}
$\epsilon_{1-\text{PTR}}$	0.023	0.013	0.064	-0.086	-0.463
	[-0.403, 0.450]	[-0.408,0.435]	[-0.511. 0.639]	[-0.570,0.400]	[-1.193, 0.267]
$\epsilon_{1-\text{ATR}}$	0.008	0.008	0.023	-0.032	-0.245
	[-0.156,0.174]	[-0.250,0.267]	[-0.192,0.243]	[-0.226,0.153]	[-0.712, 0.139]

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 $d\ln(1 - \text{PTR(state)}_{isty})/d\ln(1 - (\text{PTR(state+federal)}_{isty})$ 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 that are simply clustered at the golfer and the state of the tournament level are in parentheses, ( ), while bootstrapped standard errors are in braces, { }. Ninety-five percent confidence intervals on the elasticities are in brackets, [ ], and are obtained using a parametric bootstrap utilizing the latter standard errors.

Table 4: Triple Difference Results

	(1) Baseline	(2) Earnings > 0	(3) Exludes Cut	(4) Lagged Percentile 25th-75th	(5) Lagged Percentile 75th-100th
$\Delta \ln(1 - \text{PTR}_{ity})$	0.005	-0.016	0.057	-0.069	0.124
	(0.039)	(0.042)	(0.036)	(0.056)	(0.139)
	{0.057}	{0.066}	{0.056}	{0.087}	{0.117}
$\Delta \ln(1 - \text{PTR}_{ity}) \times lowtax_{ity}$	0.294	0.363	0.278	0.370	0.560
	(0.076)	(0.086)	(0.074)	(0.098)	(0.199)
	{0.064}	{0.071}	{0.060}	{0.090}	{0.137}
Marginal effect: low-tax states	0.299	0.347	0.335	0.301	0.684
	(0.072)	(0.083)	(0.071)	(0.094)	(0.214)
	{0.086}	{0.097}	{0.082}	{0.125}	{0.180}
$\epsilon^{H}_{1-\text{PTR}}$ (total: high-tax)	0.011	-0.032	0.175	-0.126	0.212
	[-0.235,0.258]	[-0.282,0.218]	[-0.160,0.511]	[-0.436,0.184]	[-0.180,0.604]
$\epsilon^{L}_{1-\text{PTR}}$ (total: low-tax)	0.656	0.668	1.028	0.548	1.173
	[0.288,1.024]	[0.302,1.034]	[0.535,1.522]	[0.102,0.995]	[0.568,1.778]
$\epsilon^{DDD}_{1-\text{PTR}}$ (differenced: low-tax)	0.645	0.700	0.853	0.674	0.961
	[0.371,0.918]	[0.431,0.967]	[0.489,1.215]	[0.352,0.995]	[0.498,1.421]
Observations	269,807	232,138	218,087	127,195	66,893

This table shows results estimating equation (6), where the participation tax rate and the expected earnings are interacted with an indicator for living in a relatively low-tax state. Low-tax is defined as the golfer not having positive tax liability in the residence state after paying taxes to the employment state. 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 that are simply clustered at the golfer and the state of the tournament level are in parentheses, ( ), while bootstrapped standard errors are in braces, { }. Ninety-five percent confidence intervals on the elasticities are in brackets, [ ], and are obtained using a parametric bootstrap utilizing the latter standard errors. For simplicity, we omit estimated of $\epsilon_{1-\text{ATR}}$ in this table.

Table 5: Comparison of Elasticity Estimates with Event Studies

	Tax Increases		Tax Decreases	
	(1)	(2)	(3)	(4)
	Earnings > 0	Lagged Percentile 75th-100th	Earnings > 0	Lagged Percentile 75th-100th
Panel I: Event Study Estimates				
Contemporaneous Impact				
period after reform (π_0)	-0.086	-0.058	-0.012	0.010
	(0.024)	(0.026)	(0.029)	(0.028)
$\epsilon_{1-\mathrm{PTR}_{t=0}}$	2.122	1.929	-0.245	0.228
	[0.939, 3.305]	[0.232, 3.627]	[-1.412, 0.923]	[-0.992, 1.447]
Average Effect				
treat × post	-0.038	-0.064	0.001	-0.018
	(0.015)	(0.020)	(0.022)	(0.029)
$\epsilon_{1-\mathrm{PTR}_{\mathrm{avg}}}$	0.950	2.121	0.021	-0.389
	[0.222, 1.678]	[0.812, 3.413]	[-0.852, 0.895]	[-1.649, 0.872]
Panel II: Panel Data Estimates				
$\Delta \ln(1 - \mathrm{PTR}_{ity})$	3.011	3.056	0.223	-0.848
	(1.080)	(1.560)	(0.929)	(2.178)
	{1.420}	{2.130}	{0.856}	{2.137}
$\epsilon_{1-\mathrm{PTR}}$	2.064	2.429	0.082	-0.235
	[0.153, 4.001]	[-0.889, 5.826]	[-0.367, 0.481]	[-1.405, 0.927]

The first panel of this table shows the elasticity estimates from the event studies, with one column corresponding to each of the panels in Figure 6. Column 1 corresponds to Figure 6 (Panel a), Column 2 to 6 (b), Column 3 to 6 (c), and Column 4 to 6 (d). The "contemporaneous impact" converts the coefficient on the first year after a major state tax reform, π_0, into an elasticity following the procedure described in the text. The "average effect" replaces the full set of event dummies by a simple treat by post indicator and converts the coefficient on that interaction into an elasticity. The second panel of the table restricts the panel data regressions to have the same tax reforms considered in the event study. Standard errors that are simply clustered at the golfer and the state of the tournament level are in parentheses, ( ), while for the panel data mode, bootstrapped standard errors are in braces, { }. Ninety-five percent confidence intervals on the elasticities are in brackets, [ ], and are obtained using a parametric bootstrap.

Online Appendix for "State Taxation of Nonresident Income and the Location of Work"

David R. Agrawal and Kenneth Tester

A 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.[30] There are also special rules for winners of the Majors.[31] 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.[32]

A.2 Data and Residency Status from Media Guides

We combine data from golfstats.com and the PGA Tour Media Guides. An overview of the data available is in Figure A.1, showing the online data scraped from golfstats and the residency information digitized from the Media Guides.

In order to properly measure residency for golfers we use the PGA Tour Media Guides between 1977 and 2018, excluding 2003, which is missing from the PGA Tour archives. Overall, 85 percent of golfers have some residency information in the media guides and the tour guides cover 65 percent of golfer-year observations before any extrapolation of residency information. In order to make sure golfers have a residence for every year and are as close to their true residency as possible, we fill in observations where residency information is missing but where we have information on prior and latter residency. If a golfer does not have a residency change between two observed years from which they appeared in the media guide, we simply fill in the residency information in the missing years. If a golfer changes residency somewhere in the range of missing years, the midpoint between the missing years is used to mark the change in residence. Similarly, the first observed residence is used to fill in any previous residences and the last observed residence is used to fill in any following residences. Golfers who never appear in the media guide, which are a small share of golfer-year observations (mainly low-quality golfers who only play a few years on the PGA Tour), are dropped from the data set.

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

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

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

We could use the residency information we scraped from online sources, but because this residency information is only observed at the time of scraping, we do not use it because the error would become larger for golfers in the data earlier in the sample. We have verified the results are robust to dropping extrapolated data and to including golfers we drop, but using their 2018 address. Table A.1 shows summary statistics on the number of moves. Less than 3% of golfer year observations involve a move, of which about 25% do not involve a tax change (e.g., moving from one zero tax state to another).

The distribution of the location of golfers/tournaments is provided in Figure A.2.

A.3 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 $Z_{i,y}^{n}$. The N proxy variables include age, lagged values of earnings, the golfer's lagged top placement, the golfer's lagged average 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.[33] Using the index values for each golfer-year, we create year-specific deciles of quality such that a given player (e.g., Tiger Woods) in a given year (e.g., 2005) is assigned to a specific decile in that year (e.g., 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×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. 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

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

strongly correlated with contemporaneous performance. But, we subsequently discuss robustness checks.

In constructing our grouping estimator we use lagged and time invariant characteristics. However, mean reversion may bias the assignment of individuals to groups when using lagged variables as predictors. As a solution, we calculate the predicted group cells based not just on prior year predictors, but on an average over several past years. After calculating the group cell means using up to five year lags of prior performance, the results are robust.

A.4 Constructing the PTR using TAXSIM

Because of the decentralized nature of 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:

$$\mathrm{PTR}_{ity} = \frac{\mathrm{T}_{sry}(\mathbb{E}(\mathrm{I}_{ity}|P_{ity}=1)) - \mathrm{T}_{sry}(\mathbb{E}(\mathrm{I}_{ity}|P_{ity}=0))}{\mathbb{E}(\mathrm{I}_{ity}|P_{ity}=1) - \mathbb{E}(\mathrm{I}_{ity}|P_{ity}=0)} \tag{A.3}$$

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.[34] Expected income 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,

[34] We will subsequently explore the robustness of this assumption.

his PTR would be:

$$\text{PTR} = \frac{[\text{T}_{\text{FED}}(300,000) + \text{T}_{\text{AZ}}(100,000) + \text{T}_{\text{GA}}(200,000)] - [\text{T}_{\text{FED}}(200,000) + \text{T}_{\text{GA}}(200,000)]}{300,000 - 200,000}, \tag{A.4}$$

Where T_{FED} is the federal taxes due and 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:

$$\text{PTR} = \frac{\text{T}_{\text{FED}}(300,000) + \text{T}_{\text{AZ}}(100,000) - \text{T}_{\text{FED}}(200,000)}{100,000}. \tag{A.5}$$

His participation tax rate is 35.4, average tax rate is 27.3, and marginal tax rate is 41.2. While for each of his tournaments in Georgia, the participation tax rate would be:

$$\text{PTR} = \frac{[\text{T}_{\text{FED}}(300,000) + \text{T}_{\text{GA}}(200,000) + \text{T}_{\text{AZ}}(100,000)] - [\text{T}_{\text{FED}}(200,000) + \text{T}_{\text{GA}}(100,000) + \text{T}_{\text{AZ}}(100,000)]}{300,000 - 200,000}. \tag{A.6}$$

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:

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

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:

$$\text{PTR} = \frac{[\text{T}_{\text{FED}}(400,000) + \text{T}_{\text{CA}}(100,000)] - [\text{T}_{\text{FED}}(300,000)]}{400,000 - 300,000}, \tag{A.8}$$

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[35]. If state taxes were simply based on residence, this would be all that would be necessary to compute the relevant PTR.[36]

However due to source based taxation, more work is required as TAXSIM does not

[35] 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

[36] 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.

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 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.

The two different apportionment methods require us to run our data in TAXSIM 7 distinct times: two times for the total residence taxes for both playing and not playing in a tournament, two times for apportionment method 1 (only income earned in the employment state), two times for apportionment method 2 (fraction of income earned in the employment state), and one time to calculate the resident income taxes due from income earned in all other employment tax states so that the amount of residence income taxes due from work in the employment state can be calculated.

A.5 Event Study Treatments

Tables A.2 and A.3 show which states and years are treated in the event study. The tables separately show treatments by increases and decreases as well as for all changes and those driven by changes in the employment state.

A.6 Stacked Event Studies: Incidence

In order to further explore the incidence effects of tax changes on golfer earnings, we use our stacked event study framework and apply it to tournament prizes. In this instance, we aggregate our data to the tournament-year level and define treatment to be a 1 percentage point increase in the mean PTR across all golfers (not conditional on participation) for a given tournament. In addition, our control group is further limited to tournaments that experienced only small changes in the entire event window. The effect of these events on the tax rate is presented in Figure A.4a, showing that our treatment is a strong predictor of higher taxes after the reform. Figures A.4b, A.4c, and A.4d all show no clear relationship between tax increases on the total purse, the top prize, and our measure of expected earnings. In particular, the incidence on prizes bounces around zero. As we discuss in the paper, this is likely due to PGA tour prize rules.

A.7 Bootstrap Procedure

While the PTR is simulated using TAXSIM, the statistical properties should be similar to the average tax rates and marginal tax rates simulated in previous papers like Kleven, Landais and Saez (2013) and Moretti and Wilson (2017). In particular, compare the construction of the PTR to the ATR in Kleven, Landais and Saez (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.footnoteIn addition, it is commonplace in the peer effects literature to include group averages in the estimating equation without modifications (Lavy and Schlosser, 2011; Carrell, Fullerton and West, 2009), suggesting that both the PTR and expected earnings should be accurate measures of the tax rate and what they would expect to win. Although we estimate quality deciles using a regression equation, the expectation that enters our tax rate is simply a group mean. This is done to mitigate concerns of using a generated regressor. Nonetheless, because group means are based on a regression equation predicting golfer quality, there may be some unmodeled uncertainty. For this reason we will always present bootstrapped standard errors, which improves upon the treatment of standard errors by the prior literature.

In our setting, bootstrapping the standard errors is complicated by the fact that for each bootstrap draw, we need to calculate the PTR in TAXSIM, which requires sending each sample multiple times through TAXSIM. Because we also have two-way clustering, the bootrap must sample over both clusters (golfers and tournament state). Then, we draw over golfer by tournament state to obtain the covariance necessary to obtain our standard errors (Cameron, Gelbach and Miller, 2011). This implies that for each bootstrap draw, our data needs to be sent through TAXSIM a total of 21 times.

Formally, we do this by first resample with replacement 500 times over the golfers clusters—note following Cameron, Gelbach and Miller (2011) the resampling is done over the entire clusters rather than over the individual observations. We then estimate the expected earnings measure on each sample and calculate the group means. We then separately calculate taxes in TAXSIM for each sample. Using this PTR, for each bootstrap sample, we estimate the regression for each sample and calculate the variance of these estimates. We repeat this exercise clustering by tournament state and golfer by tournament state in accordance with (Cameron, Gelbach and Miller, 2011) to obtain the bootstrapped standard errors.

A.8 Panel Regressions: Removing Individual Effects to Construct Ability

To shed additional light on the differences of the coefficients, we remove individual fixed effects from the data when constructing the group (decile) means.

Given the earnings are constructed using decile×year×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,

$$w_{it} = \beta_0 + \delta_i + \delta_t + \varepsilon_{it}, \tag{A.9}$$

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.

Under this approach, 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.4 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 salient and so golfers do not pay as much attention to earnings as they do to taxes.

A.9 Interpretation

It is often assumed that governments maximize revenue from 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, Foremny and Martínez-Toledano (2020), the change in tax revenue is positive if the elasticity is sufficiently small. Let ATR_s denote the average tax rate in state s and let $B_s(ATR_s)$ denote the income tax base. Then, differentiating revenue with respect to the ATR:

$$\frac{d\left(ATR_s \cdot B_s\right)}{dATR_s} \propto 1 - \frac{ATR_s}{1 - ATR_s}\epsilon_{1-ATR_s}. \tag{A.10}$$

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—2.09—and adjust it using equation (8), which yields $2.09 \times 0.84 = 1.759$. Using the Using the highest *average* tax rate (12.2%) over any state-year, implies that (A.10) evaluates to $0.878 > 0.122 \times 1.759$. 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.

A.10 Alternative Index Creation

As a robustness exercise, 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. Similar 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. The results of this alternative index can be seen in Tables A.5 along with Figure A.6. Results illustrate a similar pattern to those in the main text.

A.11 Robustness Checks

Table A.6 shows that the results are robust to excluding tournament by year fixed effects, as we discussed when presenting the estimating equation.

The remaining tables address robustness checks highlighted in Section 5.4. Table A.7 and A.8 show that the one-time decision of where to play is not critical. Table A.9 shows the results are driven by lower quality tournaments.

Figure A.5 visually shows our baseline regression using a binned scatter plot.

A.12 Data Sources

U.S. Bureau of Economic Analysis, Personal Consumption Expenditures: Chain-type Price Index [PCEPI], retrieved from FRED, Federal Reserve Bank of St. Louis; `https://fred.stlouisfed.org/series/PCEPI`.

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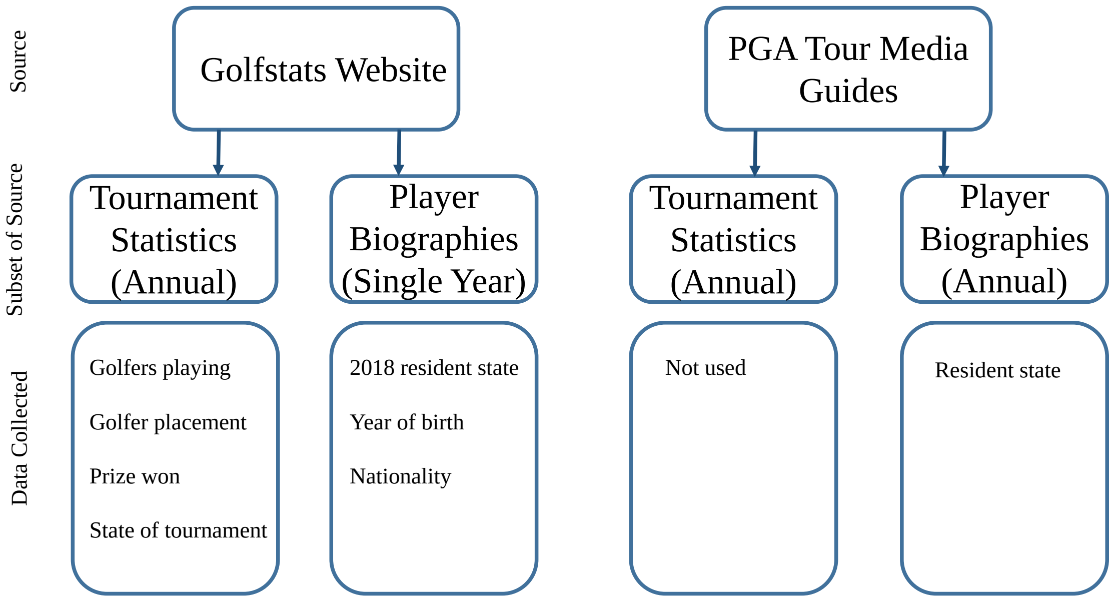
Golfstats. 2018. "Tournament Results". Available at "`https://www.golfstats.com/`." Accessed in October 2018.

PGA Tour. 1977-1981. "Official PGA Tour Media Guide (Various Years)." URLs provided in replication package.
PGA Tour. 1982-2002. "The Tour Book 1982: Official Media Guide of the TPA Tour." URLs provided in replication package.
PGA Tour. 2004-2018. "The Tour Book 1982: Official Media Guide of the TPA Tour." URLs provided in replication package.
Steven Ruggles, Sarah Flood, Ronald Goeken, Megan Schouweiler and Matthew Sobek. IPUMS USA: Version 12.0 [ACS 2018]. Minneapolis, MN: IPUMS, 2022. https://doi.org/10.18128/D010.V12.0

A.13 Additional Figures

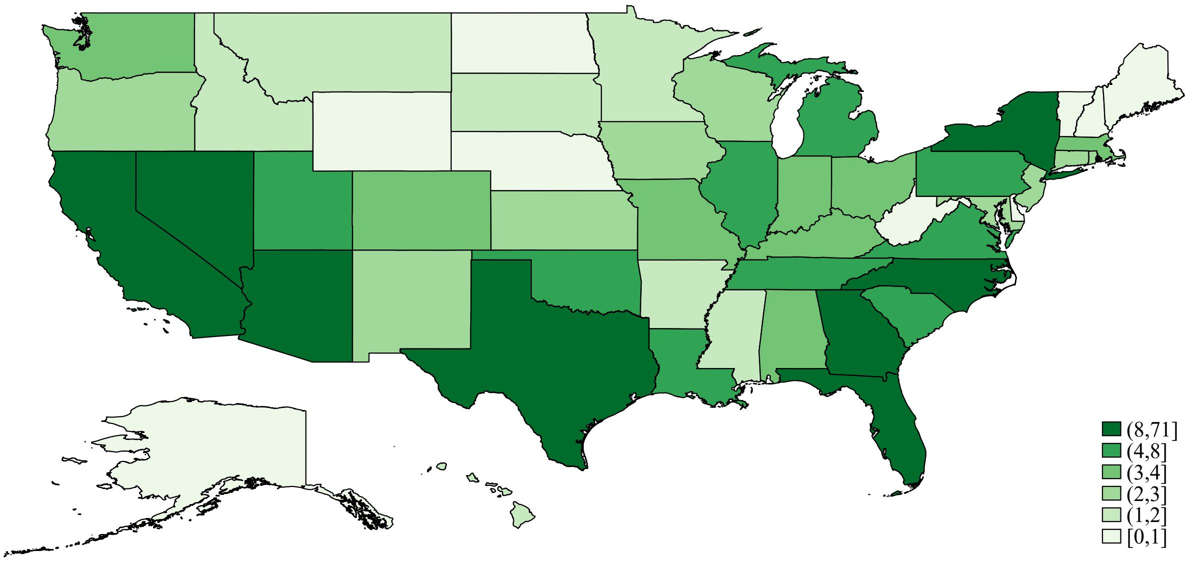
Figure A.1: Snapshot of Data Sources Scraped / Digitized



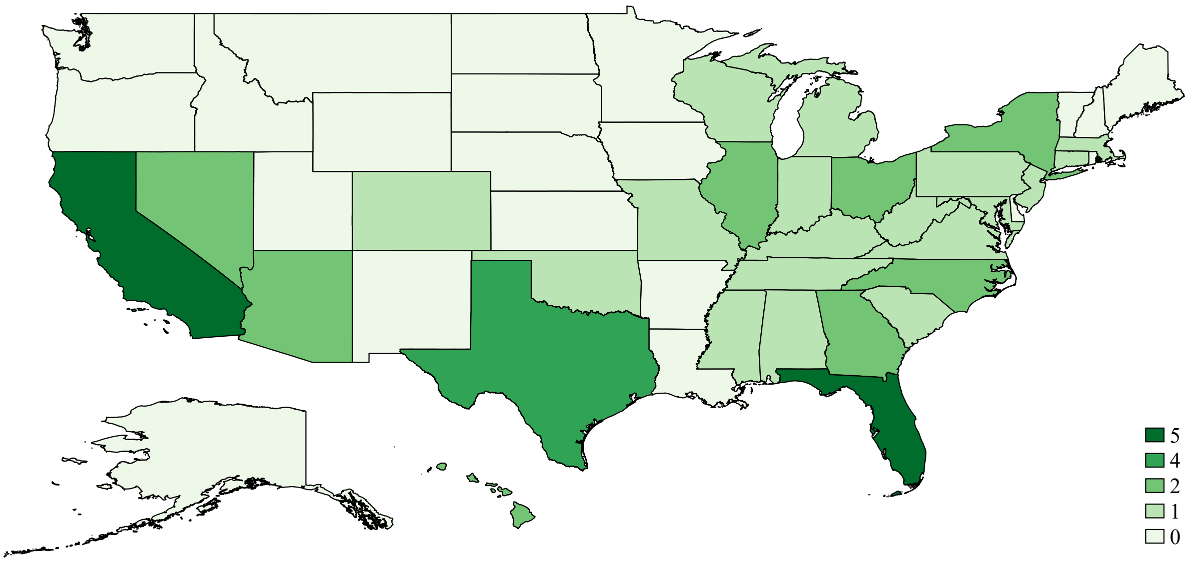
This figure is a snapshot of the data scraped from Golfstats and the data digitized from the PGA Tour Media Guides.

Figure A.2: The Location of Golfers and Tournaments

(a) Residence Location

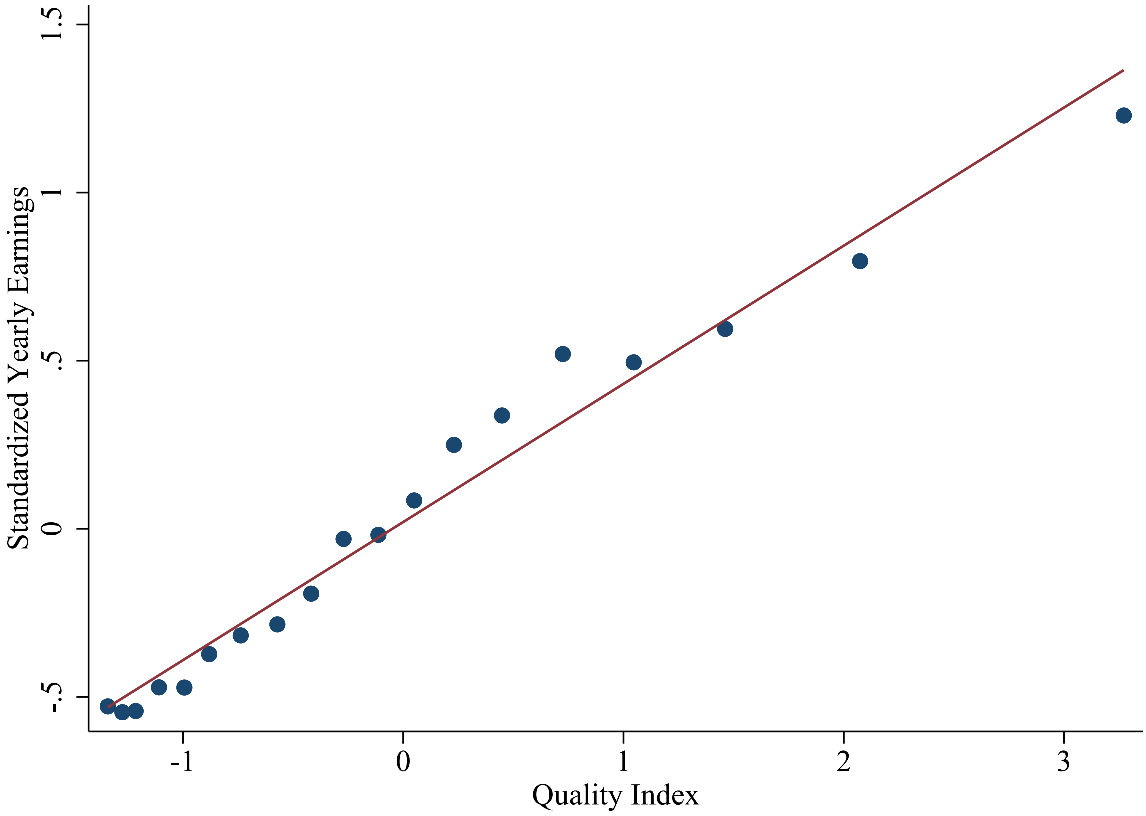


(b) Tournament Location



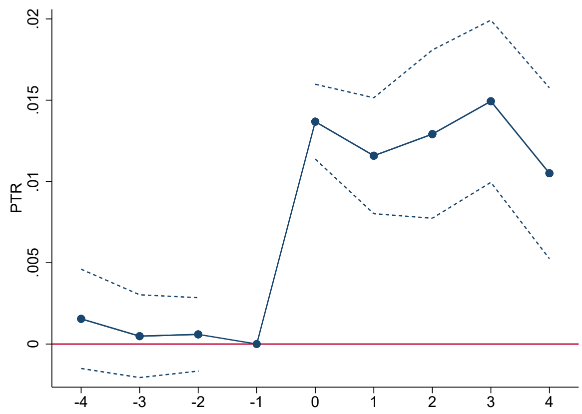
Part (a) shows the average number of residents in a year for a given state. Part (b) shows the average number of tournaments in a state for a given year, conditional on holding a tournament in that year.

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

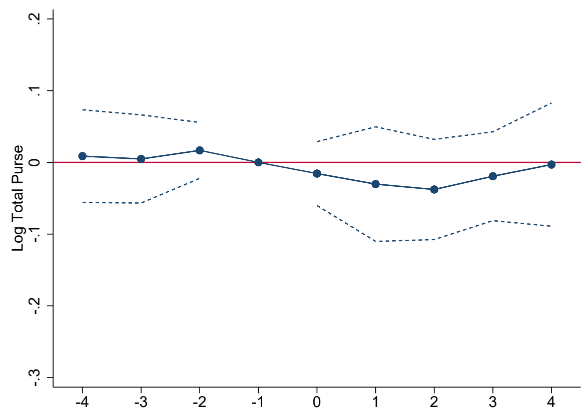


This figure is a visualization of the relationship between yearly earned income and our Lubotsky and Wittenberg (2006) data driven index of quality. To construct this figure, we standardize yearly income and the quality index such that they have a mean of zero and standard deviation of one. We then obtain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

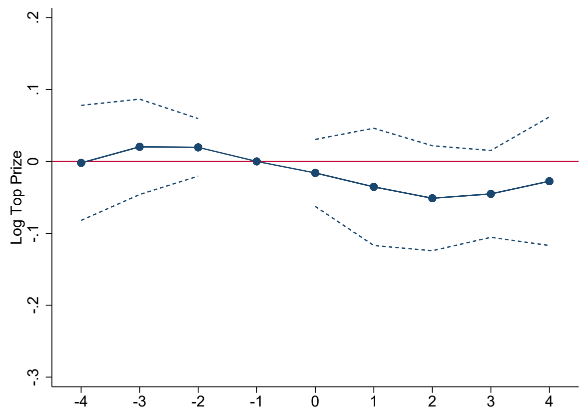
Figure A.4: Effect of Tax Changes on Earnings



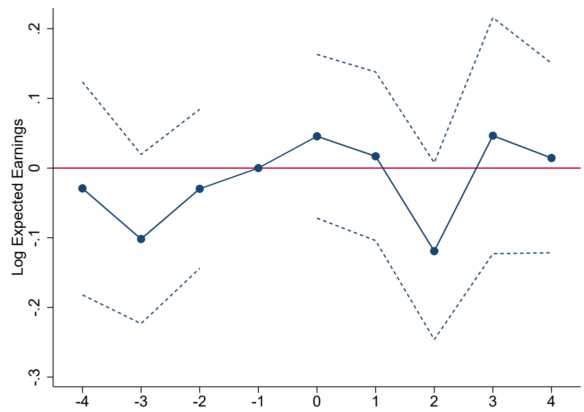
(a) Effect on Tax Rate



(b) Effect on Purse



(c) Effect on Top Prize

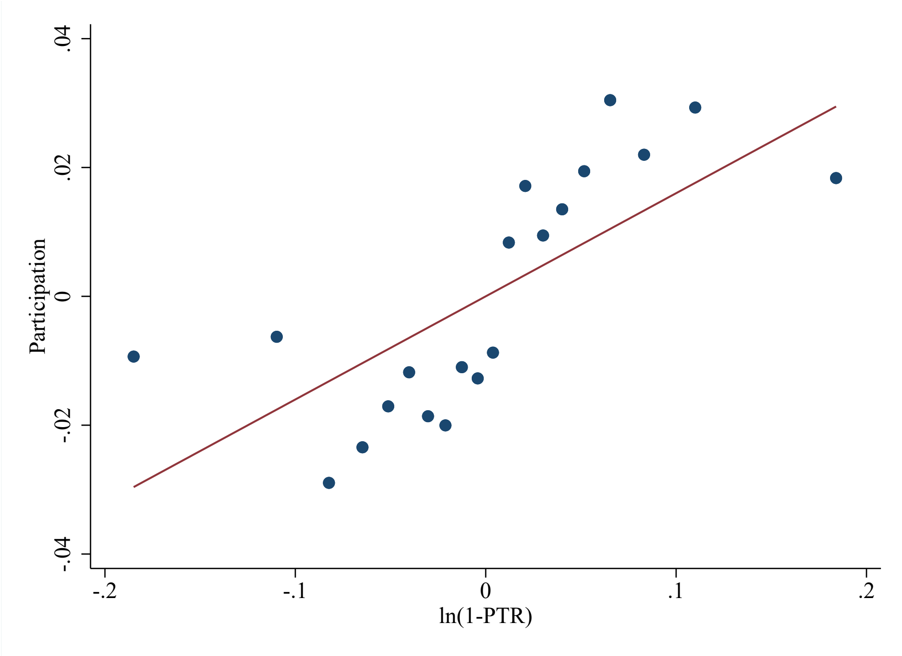


(d) Effect on Expected Earnings

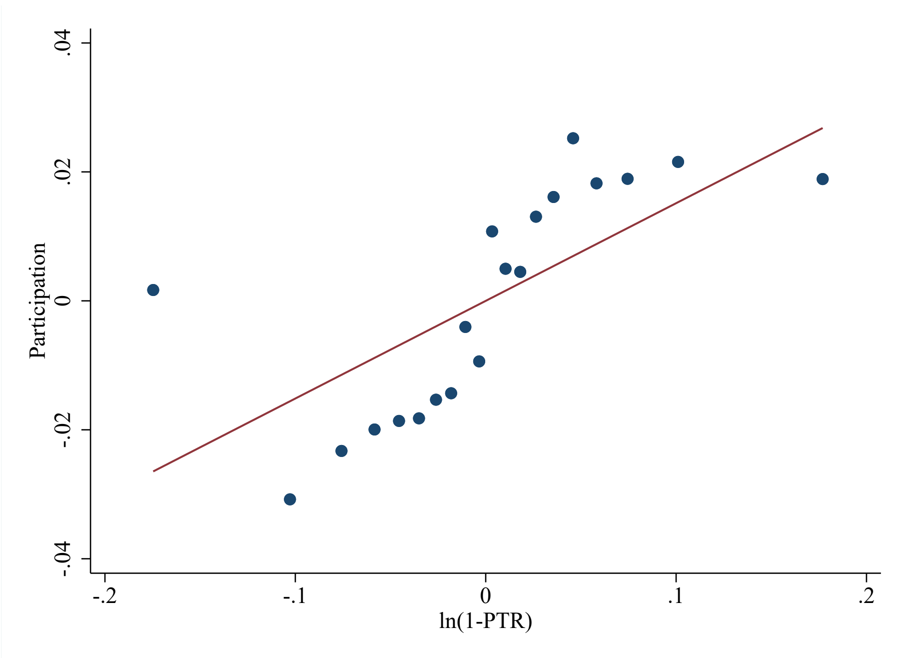
Using aggregate data by tournament-year, these figures show how the effect of major tax increase on various measures of golfer earnings. Panel (a) shows the first stage effect of a state tax increase on the tax rate. Panel (b) shows the effect on the purse size, panel (c) on the top prize, and panel (d) on our measure of expected earnings. In this instance, an event is defined as a major tax increase—a more than one percentage point increase in the mean (across golfers) participation tax rate at the 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 tournament state level and we present 95% confidence bands

Figure A.5: The Effect of Net-of-PTR on Participation

(a) Lubotsky and Wittenberg (2006) Index



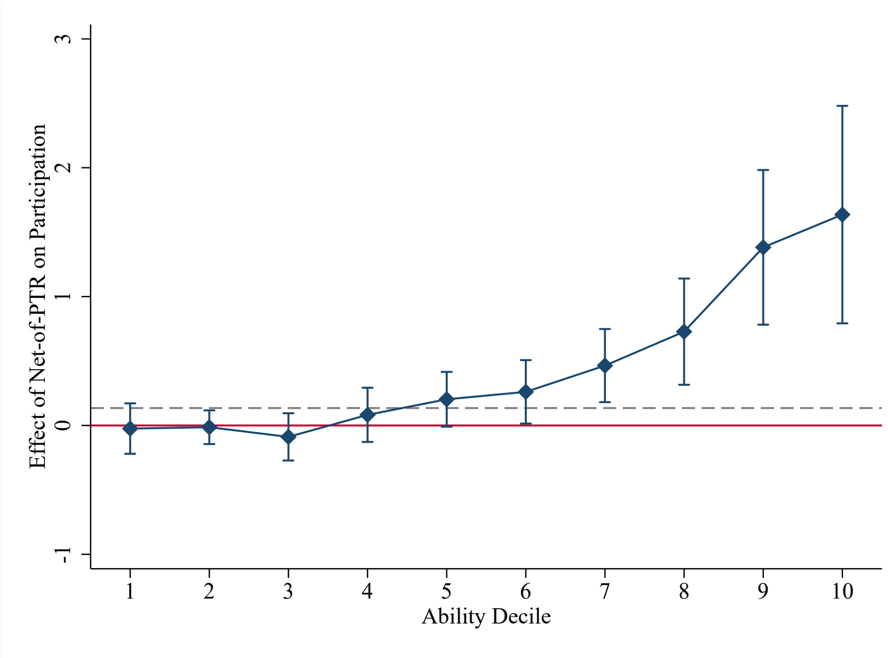
(b) Probit Index



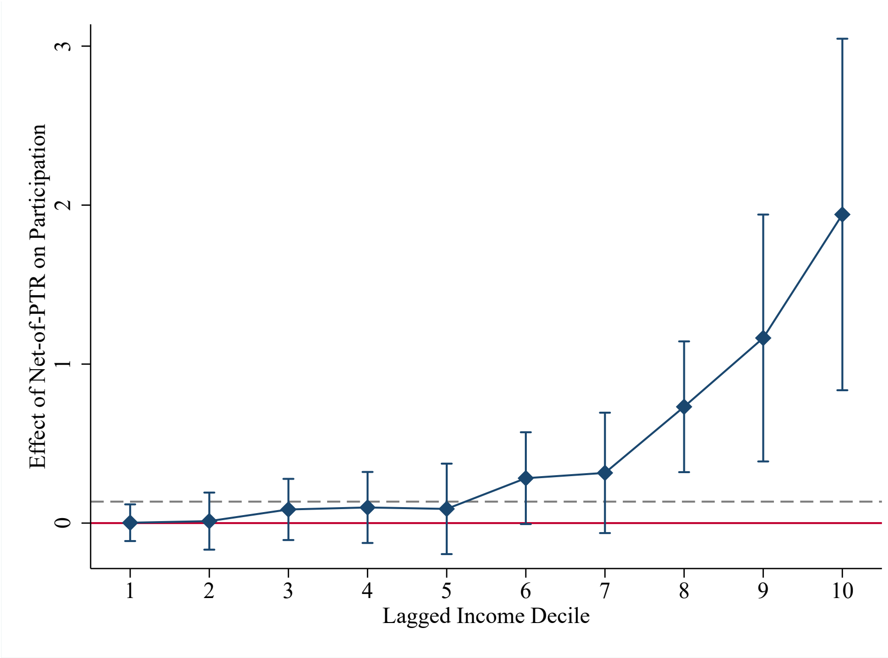
Panel (a) is a visualization of the regression of column 5 from Table 2 and Panel (b) is a visualization of the regression of column 5 from Table A.5. To construct this figure, we regress the year difference in participation on the fixed effects and controls and obtain the residuals. We do the same for the log of the net-of-PTR. We then bin the residuals and plot a line of best-fit through the data.

Figure A.6: Heterogeneity of the Effect of Taxes on Participation by Predicted Income and Quality Deciles: Fractional Probit

(a) Lagged Income Decile: Probit



(b) Quality Decile: Probit



This figure is similar to Figure 8. 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 A.5 column 1. Standard errors are clustered at the golfer and state of tournament level and bars indicate 95% confidence intervals.

A.14 Additional Tables

Table A.1: Information on Golfer Moves

	Number	Share
Panel A: Golfer-Year Observations		
Does Not Move	9,896	97.09%
Moves from High to Low Tax State	127	1.25%
Moves from Low to High Tax State	102	1.00%
Moves to Similar Tax States	68	0.67%
Total Moves	297	2.91%
Panel B: Golfers		
Golfers Who Move	189	23.95%
Age at Move	34.38	–

This table presents summary statistics on the residency changes of golfers observed in our sample. The tax determinations are done by comparing the average tax rate in the residence state in the year before the move to the average tax rate in the residence state in the year after the move.

Table A.2: Treatment Status by Tournament State

State	(1) Tax Increase	(2) Tax Increase Low-Tax Residency	(3) Tax Cut	(4) Tax Cut Low-Tax Residency
AL	0	0	0	0
AZ	139	111	132	70
CA	425	421	316	226
CO	0	0	0	0
CT	109	90	32	11
FL	241	0	223	0
GA	98	78	54	30
HI	136	128	72	48
IL	102	57	78	20
IN	0	0	0	0
KY	0	0	0	0
MD	36	21	33	17
MA	66	55	67	38
MI	57	45	48	22
MS	56	42	50	25
MO	0	0	0	0
NV	47	0	41	0
NJ	20	20	24	22
NY	154	148	101	91
NC	84	70	162	149
OH	148	139	68	44
OK	0	0	5	0
PA	21	12	19	16
SC	88	78	73	43
TN	39	0	31	0
TX	174	0	174	0
VA	23	22	24	10
WV	5	2	5	2
WI	77	73	33	29
Total	2345	1612	1865	913

This table presents the number of treated golfer-tournament events in each state (based on the location of the tournament) for various event studies. We present the number of treated events separately by tax increases and decreases as well as by separately if a golfer lives in a low-tax state (column 2 and 4). Low-tax is defined as the golfer not having positive tax liability in the residence state after paying taxes to the employment state. Therefore, the difference between column 1 and 2 and column 3 and 4 yields the number of golfers in a tournament that are treated due to the residency state rate changing.

Table A.3: Treatment Status by Tournament Year

State	(1) Tax Increase	(2) Tax Increase Low-Tax Residency	(3) Tax Cut	(4) Tax Cut Low-Tax Residency
1982	105	91	43	18
1983	105	74	15	10
1984	36	21	47	24
1985	15	14	28	21
1986	25	19	19	18
1987	122	65	245	83
1988	63	49	117	45
1989	54	45	35	19
1990	183	104	12	12
1991	104	71	38	19
1992	71	51	26	5
1993	107	88	22	8
1994	49	26	75	24
1995	9	9	163	54
1996	44	33	202	58
1997	39	39	12	12
1998	10	10	6	6
1999	40	40	13	11
2000	34	27	10	8
2001	42	38	41	9
2002	34	28	54	27
2003	113	67	9	9
2004	43	29	26	24
2005	197	62	38	13
2006	26	26	39	30
2007	33	21	29	17
2008	90	63	42	28
2009	247	188	22	15
2010	24	24	34	34
2011	84	84	17	17
2012	127	43	63	35
2013	24	19	103	38
2014	46	44	220	162
Total	2345	1612	1865	913

This table presents the number of treated golfer-tournament events in each year for various event studies. We present the number of treated events separately by tax increases and decreases as well as by separately if a golfer lives in a low-tax state (column 2 and 4). Low-tax is defined as the golfer not having positive tax liability in the residence state after paying taxes to the employment state. Therefore, difference column 1 and 2 and column 3 and 4 yields the number of golfers in a tournament that are treated due to the residency state rate changing.

Table A.4: 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
$\Delta\ln(w^{\mathbb{E}}_{dty})$	0.010	0.011	0.014	0.016	0.011
	(0.002)	(0.003)	(0.002)	(0.003)	(0.007)
$\Delta\ln(1-\text{PTR}_{ity})$	0.130	0.146	0.160	0.139	0.357
	(0.044)	(0.053)	(0.043)	(0.057)	(0.139)
$\frac{d\Delta\ln(1-\text{PTR})}{d\Delta\ln(1-\text{ATR})}$	0.326	0.367	0.314	0.364	0.558
	(0.021)	(0.028)	(0.021)	(0.030)	(0.053)
$\epsilon_{1-\text{PTR}}$	0.302	0.293	0.528	0.268	0.613
	[0.102, 0.503]	[0.085, 0.503]	[0.249, 0.804]	[0.053, 0.486]	[0.145, 1.082]
$\epsilon_{1-\text{ATR}}$	0.098	0.108	0.165	0.098	0.342
	[0.033, 0.167]	[0.031, 0.188]	[0.077, 0.257]	[0.019, 0.180]	[0.080, 0.621]
Observations	287,064	243,672	235,178	133,958	71,299

This table shows the results estimating equation (6). 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. Here, we do not perform the bootstrap procedure described in appendix A.7 due to the computational intensity required to compute standard errors on a different earnings expectation. Standard errors are clustered at the golfer and the state of the tournament level. Ninety-five percent confidence intervals on the elasticities are obtained using a parametric bootstrap.

Table A.5: 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	0.015	0.018	0.020	0.016
	(0.003)	(0.004)	(0.003)	(0.004)	(0.006)
$\Delta\ln(1-\text{PTR}_{ity})$	0.146	0.179	0.216	0.194	1.056
	(0.064)	(0.078)	(0.058)	(0.090)	(0.329)
$\frac{d\Delta\ln(1-\text{PTR})}{d\Delta\ln(1-\text{ATR})}$	0.468	0.575	0.442	0.645	0.916
	(0.030)	(0.034)	(0.031)	(0.046)	(0.058)
$\epsilon_{1-\text{PTR}}$	0.317	0.344	0.650	0.352	1.821
	[0.044,0.590]	[0.049,0.639]	[0.305,0.995]	[0.032,0.672]	[0.709,2.933]
$\epsilon_{1-\text{ATR}}$	0.148	0.198	0.287	0.227	1.669
	[0.020,0.279]	[0.028,0.371]	[0.133,0.448]	[0.020,0.438]	[0.645,2.730]
Observations	266,338	232,092	214,826	125,998	65,852

This table shows the results estimating equation (6), but using a measure of predicted earnings rather than quality to construct golfer expectations. We use a fraction probit model to predict earnings 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. Here, we do not perform the bootstrap procedure described in appendix A.7 due to the computational intensity required to compute standard errors on a different earnings expectation. Standard errors are clustered at the golfer and the state of the tournament level. Ninety-five percent confidence intervals on the elasticities are obtained using a parametric bootstrap.

Table A.6: The Effect of Taxes on the Location of Employment: No Tournament-by-Year Fixed Effects

	(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	0.012	0.015	0.018	0.014
	(0.003)	(0.003)	(0.003)	(0.003)	(0.006)
	{0.003}	{0.004}	{0.003}	{0.005}	{0.005}
$\Delta\ln(1-\text{PTR}_{ity})$	0.134	0.149	0.173	0.107	0.388
	(0.052)	(0.062)	(0.050)	(0.067)	(0.183)
	{0.065}	{0.074}	{0.063}	{0.092}	{0.123}
$\frac{d\Delta\ln(1-\text{PTR})}{d\Delta\ln(1-\text{ATR})}$	0.516	0.598	0.498	0.642	0.862
	(0.026)	(0.033)	(0.026)	(0.043)	(0.051)
	{0.062}	{0.065}	{0.062}	{0.084}	{0.109}
$\epsilon_{1-\text{PTR}}$	0.294	0.286	0.531	0.195	0.666
	[0.014,0.574]	[0.005,0.568]	[0.150,0.912]	[-0.133, 0.523]	[0.252,1.079]
$\epsilon_{1-\text{ATR}}$	0.152	0.171	0.265	0.125	0.574
	[0.007,0.308]	[0.003,0.350]	[0.072,0.477]	[-0.085,0.346]	[0.209,0.983]
Observations	269,807	232,138	218,087	127,195	66,893

This table shows the results estimating equation (6) excluding tournament by year fixed effects from the regression specification. 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 that are simply clustered at the golfer and the state of the tournament level are in parentheses, ( ), while bootstrapped standard errors are in braces, { }. Ninety-five percent confidence intervals on the elasticities are in brackets, [ ], and are obtained using a parametric bootstrap utilizing the latter standard errors.

Table A.7: The Effect of Taxes on the Location of Employment: By Tournament Order in a State

$\Delta \ln(1 - \text{PTR}_{ity})$	(1) Earnings > 0	(2) Lagged Percentile 75th-100th
Base specification	0.174	0.462
	(0.077)	(0.209)
	{0.078}	{0.139}
States with only one tournament	0.295	0.895
	(0.102)	(0.355)
	{0.093}	{0.241}
States with more than one tournament	0.118	0.272
	(0.092)	(0.246)
	{0.079}	{0.131}
First in state with more than one tournament	0.159	0.252
	(0.078)	(0.267)
	{0.088}	{0.168}
Not first in state with more than one tournament	0.099	0.281
	(0.116)	(0.269)
	{0.082}	{0.142}
Second in state with more than one tournament	0.092	0.212
	(0.065)	(0.134)
	{0.091}	{0.136}
States with more than two tournaments	0.114	0.299
	(0.167)	(0.343)
	{0.108}	{0.195}

This table shows the results estimating equation (6) except looking at differences based on the order of tournaments in the state and based on the number of tournaments in each state. Column 1 uses golfers with positive earnings and column 2 use those golfers in the 75th-100th percentile of earnings one period ago. Standard errors that are simply clustered at the golfer and the state of the tournament level are in parentheses, ( ), while bootstrapped standard errors are in braces, { }. 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.

Table A.8: The Effect of Taxes on the Location of Employment: Controlling for Cumulative Participation

$\Delta \ln(1 - \text{PTR}_{ity})$	(1) Earnings > 0	(2) Lagged Percentile 75th-100th
Base specification	0.174	0.462
	(0.066)	(0.192)
	{0.078}	{0.139}
$\epsilon_{1-\text{PTR}}$	0.335	0.793
	[0.042,0.628]	[0.371,1.214]
Controlling for cumulative earnings and participation	0.170	0.549
	(0.059)	(0.176)
	{0.071}	{0.126}
$\epsilon_{1-\text{PTR}}$	0.327	0.941
	[0.058,0.596]	[0.519,1.363]
More than median number of tournaments	0.291	0.390
	(0.115)	(0.171)
	{0.123}	{0.155}
$\epsilon_{1-\text{PTR}}$	0.426	0.621
	[0.061,0.684]	[0.126,1.043]
Less than median number of tournaments	0.195	0.410
	(0.098)	(0.235)
	{0.110}	{0.163}
$\epsilon_{1-\text{PTR}}$	0.291	0.636
	[-0.030,0.613]	[0.139, 1.133]

This table studies whether the assumption of golfers making a one-time participation decision is reasonable. The first panel presents our baseline results for golfers with positive earnings (column 1) and top 25% golfers (column 2). The second panel shows results estimating equation (6) controlling for cumulative earnings and participation (e.g., total earnings and number of tournaments participated in up to tournament t). The third and fourth panels study effects before and after the golfer participates in their median (in time) tournament. Standard errors that are simply clustered at the golfer and the state of the tournament level are in parentheses, ( ), while bootstrapped standard errors are in braces, { }. Ninety-five percent confidence intervals on the elasticities are in brackets, [ ], and are obtained using a parametric bootstrap utilizing the latter standard errors.

Table A.9: 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
$\Delta \ln(1 - \text{PTR}_{ity}) * Q_1$	0.218	0.237	0.252	0.195	0.555
	(0.058)	(0.070)	(0.061)	(0.091)	(0.205)
	{0.076}	{0.087}	{0.076}	{0.116}	{0.161}
$\Delta \ln(1 - \text{PTR}_{ity}) * Q_2$	0.214	0.246	0.261	0.224	0.766
	(0.074)	(0.087)	(0.075)	(0.094)	(0.241)
	{0.086}	{0.101}	{0.086}	{0.120}	{0.217}
$\Delta \ln(1 - \text{PTR}_{ity}) * Q_3$	0.130	0.156	0.171	0.099	0.464
	(0.061)	(0.068)	(0.059)	(0.081)	(0.238)
	{0.075}	{0.086}	{0.076}	{0.100}	{0.183}
$\Delta \ln(1 - \text{PTR}_{ity}) * Q_4$	0.047	0.052	0.095	0.001	0.017
	(0.059)	(0.068)	(0.068)	(0.088)	(0.133)
	{0.072}	{0.081}	{0.073}	{0.108}	{0.124}
ϵ_{Q_1}	0.493	0.472	0.803	0.348	1.230
	[0.156,0.831]	[0.133,0.813]	[0.330,1.276]	[-0.059,0.755]	[0.531,1.930]
ϵ_{Q_2}	0.464	0.466	0.800	0.397	1.289
	[0.095,0.833]	[0.091,0.841]	[0.286,1.315]	[-0.021,0.814]	[0.574,2.006]
ϵ_{Q_3}	0.283	0.296	0.521	0.177	0.768
	[-0.038,0.604]	[-0.023,0.615]	[0.067,0.976]	[-0.175,0.530]	[.172,1.364]
ϵ_{Q_4}	0.102	0.100	0.283	0.002	0.025
	[-0.205,0.409]	[-0.207,0.408]	[-0.140,0.707]	[-0.419,0.423]	[-0.329,0.378]
Observations	269,454	231,827	217,812	127,013	66,802

This table shows estimates of tournament quality quartiles fully interacted with equation (6). The quality quartiles are constructed using the percentiles of the purse for each tournament in 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 that are simply clustered at the golfer and the state of the tournament level are in parentheses, ( ), while bootstrapped standard errors are in braces, { }. Ninety-five percent confidence intervals on the elasticities are in brackets, [ ], and are obtained using a parametric bootstrap utilizing the latter standard errors. For simplicity, we omit estimated of $\epsilon_{1-\text{ATR}}$ in this table.