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**THE EFFECT OF STATE TAXES ON THE
GEOGRAPHICAL LOCATION OF TOP
EARNERS: EVIDENCE FROM STAR
SCIENTISTS**

Enrico Moretti and Daniel Wilson

***INTERNATIONAL TRADE AND
REGIONAL ECONOMICS, LABOUR
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Centre for Economic Policy Research

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THE EFFECT OF STATE TAXES ON THE GEOGRAPHICAL LOCATION OF TOP EARNERS: EVIDENCE FROM STAR SCIENTISTS[†]

Abstract

Using data on the universe of U.S. patents filed between 1976 and 2010, we quantify how sensitive is migration by star scientist to changes in personal and business tax differentials across states. We uncover large, stable, and precisely estimated effects of personal and corporate taxes on star scientists' migration patterns. The long run elasticity of mobility relative to taxes is 1.6 for personal income taxes, 2.3 for state corporate income tax and -2.6 for the investment tax credit. The effect on mobility is small in the short run, and tends to grow over time. We find no evidence of pre-trends: Changes in mobility follow changes in taxes and do not precede them. Consistent with their high income, star scientists migratory flows are sensitive to changes in the 99th percentile marginal tax rate, but are insensitive to changes in taxes for the median income. As expected, the effect of corporate income taxes is concentrated among private sector inventors: no effect is found on academic and government researchers. Moreover, corporate taxes only matter in states where the wage bill enters the state's formula for apportioning multi-state income. No effect is found in states that apportion income based only on sales (in which case labor's location has little or no effect on the tax bill). We also find no evidence that changes in state taxes are correlated with changes in the fortunes of local firms in the innovation sector in the years leading up to the tax change. Overall, we conclude that state taxes have significant effect of the geographical location of star scientists and possibly other highly skilled workers. While there are many other factors that drive when innovative individual and innovative companies decide to locate, there are enough firms and workers on the margin that relative taxes matter.

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1. Introduction

In the United States, personal taxes vary enormously from state to state. These geographical differences are particularly large for high income taxpayers. Figure 1 shows differences across states in the individual income marginal tax rate (MTR) for someone with income at the 99th percentile nationally in 2010. The MTRs in California, Oregon, and Maine were 9.5%, 10.48%, and 8.5%, respectively. By contrast, Washington, Texas, Florida, and six other states had a MTR of zero. Large differences are also observed in business taxes. As shown in Figure 2, Iowa, Pennsylvania, and Minnesota had corporate income taxes rates of 12%, 9.99%, and 9.8%, respectively, while Washington, Nevada, and three other states had no corporate tax at all. And not only do tax rates vary substantially across states, they also vary within states over time. Figure 3, for instance, shows how states' individual income MTRs changed during various intervals spanning 1977 to 2010. Corporate tax rates exhibit similar within-state variation (see Figure 4).

If workers and firms are mobile across state borders, these large differences over time and place have the potential to significantly affect the geographical localization of highly skilled workers and employers. The literature on the effect of taxes on the labor market has largely focused on how taxes affect labor supply, and by enlarge it has ignored how taxes might affect the geographical location of workers and firms. This is surprising because the effect of state taxes on the ability of states to attract firms and jobs figures prominently in the policy debate in the U.S.

Many states aggressively and openly compete for firms and high skill workers by offering low taxes. Indeed, low-tax states routinely advertise their favorable tax environments with the explicit goal to attract households and business to their jurisdiction. Between 2012 and 2014, Texas ran TV ads in California, Illinois and New York urging businesses and taxpayers to relocate. Governor Rick Perry visited dozens of California companies to pitch Texas' low taxes, famously declaring: "Texas rewards success with no state income tax." Similarly, Kansas has paid for billboards in Midwestern states to advertise its recent tax cuts. Wisconsin Governor Scott Walker has called upon Illinois and Minnesota employers to "escape to Wisconsin." Louisiana and Indiana have followed similar strategies. In the 2014 election cycle, state taxes and their effect on local jobs were a prominent issue in many gubernatorial races.

But despite all the attention from policy makers and voters, the effect of taxes on the geographical location of high earners and businesses remains poorly understood because little

systematic evidence on their effects is available. As noted in a recent paper by Kleven, Landais and Saez (2013) there is “very little empirical work on the effect of taxation on the spatial mobility of individuals, especially among high-skilled workers”.

In this paper, we seek to quantify how sensitive is internal migration by high-skilled workers to personal and business tax differentials across U.S. states. Personal taxes might shift the supply of workers to a state: states with high personal taxes presumably experience a lower supply of workers for given before-tax average wage, cost of living and local amenities. Business taxes might shift the demand for skilled workers by businesses: states with high business taxes presumably experience a lower demand for workers, all else equal.

We focus on the locational decisions of star scientists, defined as scientists – in the private sector as well as academia and government – with patent counts in the top 5% of the distribution. Using data on the universe of U.S. patents filed between 1976 and 2010, we identify their state of residence in each year. We compute bilateral migration flows for every pair of states (50x50) for every year. We then relate bilateral out-migration to the differential between the destination and origin state in personal and business taxes in each year. To this end, we compiled a comprehensive data set on state personal income tax rates for taxpayers at the 99.9th, 99th, 95th, 50th percentiles of the income distribution; the state-level corporate income tax rate; the R&D tax credit rate; and the investment tax credit rate.

Star scientists are important for at least two reasons. First, star scientists earn high incomes – most of them are likely to be in the top 1% of the income distribution. By definition, star scientists are exceptionally talented workers. By studying them, we hope to shed some light on the locational decisions of other well-educated, highly productive and high income workers. Second, and more fundamentally, star scientists are an important group of workers because their locational decisions have potentially large consequences for local job creation. Unlike professional athletes, movie stars and rich heirs – the focus of some previous research – the presence of star scientists in a state is typically associated with research and production facilities and in some cases, with entire industries, from biotech to software to nano-tech.¹

¹ The literature has highlighted the role that stars scientists have historically played in the birth and localization of new R&D-intensive industries. For example, the initial location of star scientists played a key role in determining the localization of the three main biotech clusters in the US (Zucker, Darby and Brewer, 1998). Similar patterns have been documented for semiconductors, computer science and nano-tech (Zucker and Darby, 2006).

Of course, taxes are not the only factor that determines the location of star scientists. Indeed, we find no cross-sectional relationship between state taxes and number of star scientists as the effect is swamped by all the other differences across states. California, for example, has relatively high taxes throughout our sample period, but it is also attractive to scientists because the historical presence of innovation clusters like Silicon Valley and the San Diego biotech cluster. Indeed, California does not lose stars to Texas, even though Texas has no personal income tax and a low business tax rate.

For this reason, our baseline model estimates the elasticity of migration to taxes using a specification where the number of scientists who move from one state to another is a function of the tax difference between the two states, conditional on state-pair ordered fixed effects. By using changes over time to identify the effect of taxes, our models absorb all time-invariant factors that can shift the demand of scientists and the supply of scientists across state pairs. Our models also control for time-varying regional shocks by including region-of-origin*region-of-destination*year dummies; and in some cases, state-of-origin*year dummies or state-of-destination*year dummies.

We uncover large, stable, and precisely estimated effects of personal and corporate taxes on star scientists' migration patterns. The probability of moving from state o (origin) to state d (destination) increases when the tax rate differential between o and d increases. For taxpayers with income at the 99th percentile of their state, we find a long-run elasticity of about 1.7: a 1% decline in after-tax income in state d relative to state o driven by a change in the MTR for the top 1% of income earners is associated with a 1.7 percent increase in the number of star scientists who leave state o and relocate in state d in the long-run. As an illustration, our estimates imply that the effect of New York cutting its marginal tax rate on the top 1% of earners from 7.5% to 6.85% in 2006 was 12 fewer star scientists moving away and 12 more stars moving into New York, for a net increase of 24 stars, a 2.1% increase.

While economically large, our estimates do not appear to be unrealistic when compared with existing studies. Kleven, Landais, Saez and Schultz (2014) estimate a similar elasticity for international migration of skilled workers in Europe. One might have expected our elasticity to be larger, because it is presumably easier to relocate within a country than across countries.

We find a similar elasticity for state corporate income tax as well as the investment tax credit (in the opposite direction), while the elasticity for the R&D credit rate is smaller and statistically

insignificant in some specifications. In all, our estimates suggest that both the supply of, and the demand for, star scientists are highly sensitive to state taxes.

We can't completely rule out the possibility that our estimates are biased by the presence of unobserved shocks to demand or supply of scientists, but the weight of the available evidence lends credibility to our estimates. First, when we focus on the timing of the effects, we find no evidence of pre-trends: Changes in mobility follow changes in taxes and do not precede them. The effect on mobility is small in the short-run, and tends to grow over time, presumably because it takes time for firms and workers to relocate.

Second, we find no evidence that changes in state taxes are correlated with changes in the fortunes of local firms in the innovation sector in the years leading up to the tax change, suggesting that states do not strategically change taxes to help local patenters at times when they are struggling (or thriving). In particular, when we regress changes in state taxes on lagged changes in the market value of the top five patenting companies in the state, we find no relationship.

It is still possible that there are changes in economic policies other than taxes that could be correlated with taxes. For example, a pro-business state legislature could both cut taxes and relax state level regulations on labor and the environment. It is also possible that states tend to raise personal income taxes during local recessions, and that mobility may be affected by the recession. Our estimated elasticities, however, don't change when we include origin state*year effects or destination state*year effects. These models fully absorb differences in local business cycle and differences in time-varying policies across origin or destination state.²

We present a number of specification tests to further probe the validity of our estimates. First, star scientists are likely to be among the top earners in a state. Thus, if our identification assumption is valid, we should see that star scientists' locational decisions are more sensitive to changes in tax rates for high-income brackets than to changes in tax rates for the median-income bracket. We estimate models that include both changes in the MTR for the top 99th percentile of income and changes in the MTR for median income. Effectively, we compare the flow of scientist between two pairs of states with the same tax difference for top incomes, but a different tax

² Identification comes from the fact that for each origin state there are 50 possible destinations in any given year. Obviously we cannot estimate models that include origin state * destination state * year effects, as they would be fully saturated. The fact that local business cycle is not an important issue is not too surprising, given that the overwhelming majority of star scientists work in the traded sector, so that changes to their labor demand reflects national and global shocks, rather than local shocks

difference for median incomes. Consistent with our assumption, we find that star scientists migratory flows are sensitive to changes in the 99th percentile MTR, but are insensitive to changes in the 50th percentile MTR.

Second, corporate taxes should affect the demand for private sector scientists but not demand for academic or government scientists. Indeed, we find that the effect of changes in corporate income taxes is concentrated among private sector inventors. We uncover no effect on academic and government researchers. In addition, while individual inventors are not subject to corporate taxes, they can take advantage of R&D credits. Empirically, we find that individual inventors are not sensitive to corporate taxes but they are sensitive to R&D tax credits.

Third, corporate taxes should only matter in states where the wage bill has a non-trivial weight in the state's statutory formula for apportioning multi-state income. Empirically, corporate taxes have no effect on stars' migration in states that apportion a corporation's income based only or primarily on sales in which case labor's location has little or no effect on the tax bill.

Fourth, we isolate the variation in tax changes that stems from a change in the party of the governor or the majority of the state legislature *and* where the change results from a narrow election or narrow shift in legislative seat share (a fuzzy regression discontinuity design). Results are not very precise, but qualitatively consistent with the baseline estimates.

Overall, we conclude that state taxes have a significant effect of the geographical location of star scientists. While there are many other factors that determine where innovative individuals and innovative companies decide to locate, there are enough firms and workers on the margin that relative taxes matter. This previously unrecognized cost of high taxes should be taken into consideration by local policymakers when deciding whom to tax and how much to tax.

Our paper is part of a small group of papers on the sensitivity of high income workers to local taxes. Existing studies for the U.S. uncover generally mixed results. Cohen, Lai, and Steindel (2011) find mixed evidence of tax-induced migration. Young and Varner (2011) and Varner and Young (2012) find no evidence of tax-induced migration in the case of "millionaires taxes" in CA and NJ. Serrato and Zidar (2014) focus on corporate taxes and find moderate effects on wages, employment and land prices. Bakija and Slemrod (2004) find a moderate effect of state personal income tax on the number of federal estate tax returns. For Europe, Kleven, Landais, and Saez (2013) uncover a large effect of taxes on the location decisions of soccer players across countries.

Kleven, et al. (2014) study the mobility of high income workers to Denmark in response to a specific tax change and find large elasticities.³

2 Data and Basic Facts

2.1 Scientists

We use a unique dataset obtained by combining data on top scientists to data on state taxes. Specifically, to identify the location of top scientists, we use the COMETS patent database (Zucker, Darby, & Fong, 2011). Each patent lists all inventors on that patent as well as their address of residence. Exact addresses are frequently not reported, but state of residence nearly always is. We define “star” inventors, in a given year, as those that are at or above the 95th percentile in number of patents over the past ten years. In other words, stars are exceptionally prolific patenters. The 95th cutoff is arbitrary, but our empirical results are not sensitive to it. In some models we use star measures where patents are weighted by number of citations and obtain similar results.⁴

One advantage of our data is that our information on geographical location is likely to reflect the state where a scientist actually lives and works (as opposed to tax avoidance). Patenters are legally required to report their address and have no economic incentive to misreport it. There is no legal link between where a patent’s inventor(s) live and the taxation of any income generated by the patent for the patent assignee/owner.⁵

The sample of inventors consists of roughly 260,000 star-scientist*year observations over the period from 1977 to 2010. Year is defined as year of the patent *application*, not the year when the patent is granted. For each scientist observed in 2 consecutive years, we define him or her as a mover if the state in year $t+1$ is different than the state in year t ; we define him or her as a stayer if the state in year $t+1$ is the same as the state in year t . For each pair of states and year in our sample, we then compute the number of movers and stayers.

³ See also Kirchgassner and Pommerehne (1996) and Liebig, Puhani and Sousa-Poza (2007).

⁴ If a patent has multiple inventors, we assign fractions of the patent to each of its inventors. For example, if a patent has four inventors, each inventor gets credited with one-quarter of a patent.

⁵ In our analysis we use patents simply to locate the taxpayers, irrespective of where the R&D activity takes place. Consider a computer scientist who patents software. It is conceivable that one year he may apply for a patent while living in CA, then the next year apply for a patent in Texas, the year of a tax increase in CA. The fact that he may have been working on the technology he patented in Texas while living in CA seems irrelevant. Separately, we note that a small but growing share of patents is made of defensive patents. To the extent such patenting yields some misidentification of “stars”, this causes measurement error in the dependent variable and should reduce the precision of our estimates, but not bias our estimates. Moreover, when we alternatively define stars based on citations, which should downweight defensive patents, we find very similar results.

Not every star scientist applies for a patent every year, so we don't observe the location of every star in every year. We are therefore likely to underestimate the overall amount of geographical mobility of star scientists, as our data miss the moves that occur between patent applications. This problem is unlikely to be quantitatively very large. Our population of star scientists is by construction quite prolific and the typical individual is observed patenting in most years. The mean number of patents per star scientist in the decade between 1997 and 2006 is 15.7. Thus, star scientists in our sample patent an average of 1.5 patents per year. The 25th percentile, median and 75th percentiles over the decade are 4, 10 and 19 respectively. As a check, we have tested whether our estimates are sensitive to including all years in between patents, assuming that the scientist location in a year reflects the state where the most recent patent was filed. Our estimates are not sensitive to this change.

While we might underestimate overall mobility, our estimates of the effect of taxes remain valid if the measurement error in the dependent variable is not systematically associated with changes in taxes. Intuitively, we underestimate the amount of mobility in states that change taxes and in states that do not change taxes by the same amount. One concern is the possibility that moving lowers the probability of patenting – either because it is distracting and time consuming or because it coincides with a job spell with a new employer, and the scientist may be restricted from using intellectual property created while working for previous employer. This could lead us to *underestimate* mobility following tax increases and therefore it could lead us to underestimate the effect of taxes. In practice, we expect this bias to be relatively minor. As we mention, most of our scientists patent very frequently. Moreover, in many fields the timing of patent application and of measured formal R&D is close, often contemporaneous (Griliches, 1998). One exception, of course, is pharmaceuticals. There is also anecdotal evidence on patents being applied for at a relatively early stage of the R&D process (see Wes Cohen 2010). A similar bias would occur if taxes are systematically associated with selection in and out of the labor force. For example, if high state taxes induce some star scientists to exit the labor force and stop patenting, we underestimate the true effect.

Figure 5 shows where top scientists are located in 2006. Appendix Table A1 shows these levels along with their changes over the sample period. Star scientists are a mobile group: Not unlike top academic economists, they face a national labor market and intense competition among employers. Despite the labs and fixed capital associated with some fields, star scientists on the

whole manage to have high rates of cross-state mobility. In particular, the gross state-to-state migration rate for star scientists was 6.5% in 2006. By comparison, the rates in the entire US adult population and the population of individuals with professional/graduate degree were 1.7% and 2.3%, respectively (Kaplan & Schulhofer-Wohl 2013). Overall, 6% of stars move at least once; the average moves per star is 0.33; and the average moves per star, conditional on moving at least once, is 2.6.

In Table 1, we show the bilateral outflows among the ten largest states in terms of population of star scientists on average over the recent decade, 1997-2006. The state pair with the most bilateral flows over this decade was California-Texas. 26 star scientists per year moved from Texas to California and 25 per year moved from California to Texas. Close behind was California-Massachusetts, with 25 per year moving from Massachusetts to California and 24 moving in the opposite direction.

2.2 State Taxes

We combine this patenter data with a rich comprehensive panel data set that we compiled on personal and business tax and credit rates at the state-year level.⁶ For personal taxes, we focus our analysis on state-level marginal tax rates (MTR) for taxpayers at different points of the national income distribution: 99.9%, 99%, 95% and 50%. For business taxes, we focus our analysis primarily on the corporate tax rate, which is the primary business tax faced by corporations at the state level. Since states generally have only a single corporate income tax rate, marginal and average rates are the same. We also consider the effects of the investment tax credit rate and the R&D tax credit rate, as most states offer one or both of these credits. The investment tax credit typically is a credit against corporate income tax proportional to the value of capital expenditures the company puts in place in that state. Similarly, an R&D tax credit is proportional to the amount of research and development spending done in that state.

Figure 3 shows how top individual income tax rates have evolved over time. (See Figure 4 for an analogous set of maps for the CIT rate.) Two key points emerge. First, changes in taxes do not appear very systematic. We see examples of blue states with high taxes lowering their rates, and

⁶ From the NBER's TaxSim calculator, we obtained both marginal and average individual income tax rates by state and year for a hypothetical scientist whose salary and capital gains income are at a specific national income percentile for that year (50th, 95th, 99th, or 99.9th percentile). The national income percentiles are obtained from the World Top Incomes Database (Alvaredo, Atkinson, Piketty & Saez 2013). We obtain statutory rates by state and year for corporate income taxes, investment tax credits, and research and development tax credits from Chirinko & Wilson (2008), Wilson (2009), and Moretti & Wilson (2014).

examples of red states with low taxes raising their rates. For example, California kept the individual income tax rate for 99th percentile income steady between 1977 and 1985, cut it significantly between 1985 and 1990, raised it moderately between 1990 and 1995, cut it moderately between 1995 and 2000, kept it steady between 2000 and 2005, and raised moderately between 2005 and 2010.

Second, changes in personal taxes and business taxes are not systematically correlated. For example, in the cross section, we see a correlation between the 99th percentile MTR and CIT equal to 0.46. But the correlation net of state fixed effects (i.e., between mean-differences) is 0.01 and is not statistically different from zero. Similarly, the 99th percentile MTR's correlation, net of state fixed effects, with the ITC and R&D credit is -0.06 and -0.05, respectively. It is also the case that the correlations are low for changes in tax rates across these tax variables. Figure 6 shows a scatter plot of the five-year change in the CIT against the five-year change in the 99th percentile MTR for the five-year intervals from 1980 to 2010. The estimated slope of this relationship is far below one, at 0.09. This suggests that any source of endogeneity applying to one of these tax policies is unlikely to apply to the other.

In principle, workers deciding on where to locate should focus on average tax rates, not marginal tax rates. However, average rates are measured with considerable error, and the literature on geographical mobility has typically focused on MTR (Kleven, Landais, and Saez, 2013). Average rates and MTR are of course highly correlated, as the ATR is monotonically increasing in the MTR (given that in every state the MTR increases monotonically with taxable income). For star scientists, this correlation is even higher: because stars scientists are highly paid, their average rate is well approximated by the top marginal rate in many states.⁷ Empirically, our results based on MTR and ATR are similar.

It is important to clarify exactly why corporate taxes might be expected to affect employer location decisions. As we note above, state taxation of income generated by patents (e.g., royalties) is independent of where the patents' inventors reside. For instance, suppose a patent's inventors live in California and work for a multistate corporation, the patent owner. California has no more claim to taxing the corporate income generated by that patent than any other state in which the corporation operates. The patent income becomes part of the corporate taxpayer's

⁷ In addition, recent evidence suggests that the MTR might be a better proxy for a household's perceived ATR than the actual ATR. Gideon (2014) finds that the perceived ATR of survey respondents is very close to their actual MTR, but far above their actual ATR.

national business income, which is apportioned to states based on each state's apportionment formula (described below). Hence, to the extent that labor demand for star scientists in a state is affected by that state's corporate tax rate, it is because star scientists and the associated R&D operations account for part of the company's payroll in that state, *not* because the patenting income is disproportionately taxed by that state.

Note that what matters for an employer is not just the part of the company's payroll that accrues to the scientist directly. Even more relevant is the part of the payroll associated with the entire R&D operation (lab, research center, etc.). In most cases, payroll for the entire R&D activity is much larger than the scientist payroll. In other words, a scientist location affects corporate taxes not simply because her salary directly affects the amount of corporate income taxes in that location, but because the presence of all the other R&D personnel affects the amount of corporate income taxes in that location.

Each state has an apportionment formula to determine what share of a corporation's national taxable business income is to be apportioned to the state. Prior to the 1970s, virtually all states had the same formula: the share of a company's income in the state was determined by an equal-weighted average of the state's shares of the company's national payroll, property, and sales. Over the past few decades, many states have increased the weight on sales in this formula as a means of incentivizing firms to locate in their state. Today, many states have a sales-only apportionment formula. In such states, a corporate taxpayer's tax liability is independent of the level of employment or payroll in the state. Therefore, the effect of changes in corporate tax rates should be small in states with a sale-only formula.⁸

3 Framework and Econometric Specification

In this section, we present a simple model of scientist and firm location designed to guide our empirical analysis. We use the model to derive our econometric specification.

3.1 Location of Scientists and Firms

⁸ Even for states with zero apportionment weight on payroll we expect some sensitivity to the corporate tax rate because of single-state corporations and the potential effect of corporate taxes on new business start-ups. In most states, R&D tax credits can be claimed on either corporate or individual income tax forms (but not both), so they could potentially affect locational outcomes of all scientists, not just those working for corporations. Investment tax credits (ITCs), on the other hand, generally can only be taken against corporate income taxes. ITCs and the corporate income tax itself, of course, we would expect to affect only locational outcomes of scientists working for corporations. We test this hypothesis below.

We model personal income taxes as shifters of the supply of scientist labor supply to a state: higher taxes lower labor supply to a state, all else equal. We model business taxes and credits as shifters of the demand for scientists in a state: higher taxes lower the labor demand for scientists in a state, all else equal.

Scientist Location. In each period, scientists choose the state that maximizes their utility. The utility of a scientist in a given state depends on local after-tax earnings, cost of living, local amenities, and the worker's idiosyncratic preferences for the state. Mobility is costly, and for each pair of states o (origin) and s (destination), the utility of individual i who lived in state o in the previous year and moves to state d at time t is

$$U_{iodt} = \alpha \log(1-\tau_{dt}) + \alpha \log w_{dt} + Z_d + e_{iodt} - C_{od}$$

where w_{dt} is the wage in the current state of residence before taxes; τ_{dt} is personal income taxes in the state of residence; α is the marginal utility of income; Z_d captures amenities specific to the state of residence and cost of living; e_{iodt} represents time-varying idiosyncratic preferences for location, and measures how much worker i likes state d net of the after-tax wage, amenities and cost of living; and C_{od} is the utility cost of moving from o to d , which does not need to be symmetric and is assumed to be 0 for stayers ($C_{oo} = 0$).⁹

The utility gain (or loss) experienced by scientist i if she moves from state o to state d in year t is a function on the difference in taxes, wages, amenities and any other factors that affect the utility of living in the two states, as well as moving costs:

$$U_{iodt} - U_{ioot} = \alpha [\log(1-\tau_{dt}) - \log(1-\tau_{ot})] + \alpha [\log(w_{dt}/w_{ot})] + [Z_d - Z_o] - C_{od} + [e_{iodt} - e_{ioot}]$$

Individual i currently living in state o chooses to move to state d if and only if the utility in d net of the cost of moving is larger than the utility in o and larger than the utility in all other 48 states: $U_{iodt} > \max(U_{iod't})$ for each $d \neq d'$. Thus, in this model, scientists move for systematic reasons (after-tax wage and amenities) and idiosyncratic reasons. A realistic feature of this setting is that it generates migration in every period, even when taxes, wages and amenities don't change. This idiosyncratic migration is driven by the realization of the random variable e_{iodt} . Intuitively, workers experience shocks that affect individual locational decisions over and above after tax-wages and amenities. Examples of these shocks could include family shocks or taste shocks.

Consider the case of an unexpected and permanent change in taxes occurring between $t-1$ and t . We are interested in how the change in taxes affects the probability that a scientist chooses

⁹ We simplify the exposition by assuming that each scientist works and ignore labor supply reactions.

to relocate from state o to d . The magnitude of the effect of a tax increase depends on how many marginal scientists are in that state, and therefore on the distribution of the term e . If e_{iodt} follows an i.i.d. Extreme Value Type I distribution (McFadden, 1978), the log odds ratio is linear in the difference in utility levels in the origin and destination state:

$$\log (P_{odt} / P_{oot}) = \alpha [\log(1-\tau_{dt}) - \log(1-\tau_{ot})] + \alpha [\log (w_{dt} / w_{ot})] + [Z_d - Z_o] - C_{od} \quad (1)$$

where P_{odt} / P_{oot} is the scientist population share that moves from one state to another (P_{odt}) relative to the population share that does not move (P_{oot}).

This equation can be interpreted as the labor supply of scientists to state d . Intuitively, in each moment in time, residents of a state have a distribution of preferences: while some have strong idiosyncratic preferences for the state (large e), others have weak idiosyncratic preferences for the state (small e). A tax increase in the origin state induces residents with marginal attachment to move away. By contrast, inframarginal residents – those with stronger preferences for the origin state – don't move: they optimally decide to stay and to take a utility loss. Of course, both movers and stayers are made worse off by the tax. The marginal movers are made worse off because they need to pay the moving costs. The inframarginal stayers are made worse off because the tax reduces some of the rent that they derive from the state.

Firm Location. The location of star scientists is not only a reflection of where scientists would like to live, but also where firms who employ scientists decide to locate. Hence, migration flows may well be sensitive not only to the individual income tax rates faced by star scientists, but also to the business tax rates faced by employers.

In each period, firms choose the state that maximizes their profits. Assume that firm j located in state d uses one star scientist and the state productive amenities to produce a nationally traded output Y with price equal to 1:

$$Y_{jdt} = f(Z'_d, u_{jdt})$$

where Z'_d represents productive amenities available in state d . In each potential state and year, a firm enjoys a time-varying productivity draw u_{jdt} that captures the idiosyncratic productivity match between a firm and a state. Some firms are more productive in a state because of agglomeration economies, location of clients, infrastructure, local legislation, etc. For instance, in the presence of agglomeration economies, the existence of a biotech cluster in a state might increase the productivity of biotech firms in that state but have no effect on the productivity of software firms. The term u_{jdt} is important because it allows firm owners to make economic profits

in equilibrium, so that the elasticity of location choice with respect to taxes is not infinite (Serrato and Zidar, 2014). It is possible to generalize this technology to include other types of labor and capital.

If a firm relocates to state d , its profits are equal to revenues minus labor costs net of corporate taxes in d and net of moving costs. Assume in particular that the profits of a firm that relocates from state o to state d can be approximated as

$$\log \pi_{jodt} = \beta \log [(Y_{jdt} - w_{dt}) (1 - \tau'_{dt})] + Z'_d + u_{iodt} - C'_{od} .$$

where τ'_{dt} is the corporate income tax rate; and C'_{od} is the cost for firms of moving from o to d , assumed to be 0 for stayers and potentially different from the cost faced by individuals. Firm j moves to state d if and only if $\pi_{iodt} > \max(\pi_{iod't})$ for each $d \neq d'$. If u_{iodt} follows a iid Extreme Value Type I distribution, the log odds ratio takes the form

$$\log (P'_{odt} / P'_{oot}) = \beta [\log(1 - \tau'_{dt}) - \log(1 - \tau'_{ot})] - [\log(w_{dt} / w_{ot})] + [Z'_d - Z'_o] - C_{od} \quad (2)$$

where (P'_{odt} / P'_{oot}) is the population share of firms starting in o that move to d between t and $t+1$, divided by the population share of firms initially in state o that do not move. Since each firm employs one star scientist, this equation can be interpreted as the labor demand for scientists in state d .

In our setting, taxes and credits matter for individual and firm location choice, although the elasticity is not necessarily infinite as it would be in the case of homogenous individuals and firms (Moretti 2011, Serrato and Zidar 2014).

3.2 Econometric Model

Equation (1) expresses the probability that a star scientist relocates from state o to state d as a function of the wage difference between o and d and the personal tax difference. Equation (2) expresses the probability that a firm employing a star scientist relocates from state o to state d as a function of the wage difference between o and d and the corporate tax difference. In equilibrium, labor demand needs to equal labor supply in each state and year. Equating $\log(P_{odt} / P_{oot})$ in (1) to $\log(P'_{odt} / P'_{oot})$ in (2) yields an expression for the equilibrium number of scientists who move as a function of tax differentials:

$$\log(P_{odt} / P_{oot}) = \eta [\log(1 - \tau_{dt}) - \log(1 - \tau_{ot})] + \eta' [\log(1 - \tau'_{dt}) - \log(1 - \tau'_{ot})] + \gamma_d + \gamma_o + \gamma_{od} \quad (3)$$

where $\eta = \alpha / (1 + \alpha)$ is the effect of personal taxes; $\eta' = \beta \alpha / (1 + \alpha)$ is the effect of corporate taxes; $\gamma_d = [\alpha / (1 + \alpha)] [Z_d + Z'_d]$ is a vector of state fixed effects that captures both consumption and production amenities in the state of origin; $\gamma_o = [\alpha / (1 + \alpha)] [Z_o + Z'_o]$ is a vector of state fixed

effects that captures both consumption and production amenities in the state of destination; and $\gamma_{od} = -(C_{od} + C'_{od})$ is a vector of state-pair fixed effects that captures the cost of moving for individuals and firms for each state pair.

Equation (3) represents our baseline econometric specification, although in some models, we include additional controls. (To control for time-varying regional shocks, we augment equation (3) by including region-pair*year effects. To control for time-varying state specific shocks, in some models we augment it by including state-of-origin*year effects or state-of-destination*year effects.)

In interpreting our estimates of the parameters η and η' , three important points need to be considered. First, the parameters η and η' are the reduced form effect of taxes on employment. As such, their magnitude depends on labor demand and supply elasticities, which in turn are a function of β and α . Changes in personal income taxes shift the supply of scientists to a specific state and therefore involve a movement along the labor demand curve. The ultimate impact on the equilibrium number of scientists depends on the slope of labor demand curve. If labor demand is highly elastic, the change in scientists will be large. If demand is inelastic, the change in number of scientist will be small – not because taxes don't matter, but because with fixed demand the only effect of the policy change is higher wages.

Similarly, changes in business taxes affect the demand of scientists in a given state and therefore involve a movement along the supply curve. The ultimate impact on the equilibrium number of scientists depends on the slope of labor supply curve. For a given shift in demand, the change in number of scientists will be larger the more elastic is labor supply. Empirically, we expect labor demand faced by a state to be elastic in the long run. Since in the long run firms can locate in many states, local labor demand is likely to be significantly more elastic than labor demand at the national level.

Although our estimates of η and η' are reduced form estimates, they are the parameters of interest for policy makers interested in quantifying employment losses that higher taxes might cause.

A second point to consider is that our empirical models capture the long run effect of taxes. The long run effects are likely to be larger than the immediate effect, because it takes time for people and firms to move. In addition, some of the tax changes are temporary. Permanent tax changes are likely to have stronger effects than temporary changes. Moreover, our models

measure not only the direct effect of taxes, but also any indirect effect operating through agglomeration economies, if they exist. For example, consider the case where a tax cut results in an increase in the number of biotech scientists in a state; and such exogenous increase makes that state endogenously more attractive to other biotech scientists. Our estimates will reflect both the direct (exogenous) effect and the indirect (endogenous) agglomeration effect.

A third point to keep in mind in interpreting our estimates is that variation in taxes may be associated with variation in public services and local public goods. Our estimates need to be interpreted as measuring the effect of taxes on scientist mobility *after allowing for endogenous changes in the supply of public services*. If higher taxes mean better public services, our estimates are a lower bound of the effect of taxes holding constant the amount of public services. Consider for example the case where a state raises taxes in order to improve schools, parks and safety. If scientists and their families value these services, the disincentive effect of higher taxes will be in part offset by the improved amenities. In the extreme case where the value of the new public services is equal to the tax increase, the disincentive effect disappears.

In general, however, the cost of taxation to high income taxpayers is unlikely to be identical to the benefits of public services enjoyed by high income individuals. First, a significant part of state taxation is progressive and redistributive in nature. High income individuals tend to pay higher tax rates and receive less redistribution than low income individuals. Second, it is realistic to assume that high income individuals are likely to consume less public services than low income individuals. For example, high income individuals are more likely to send their children to private schools than low income individuals. Finally, due to deadweight loss of taxation, and overall inefficiency of the public sector, the disincentive effect will still exist, but it will be lower than the pure monetary value of the tax increase.

Throughout the paper, our standard errors are robust and allow for two-way clustering by origin*year and destination*year. This is necessary because in our data each origin and each destination state appear 50 times in each year and states do not impose different tax rates on immigrants from different states. In addition, we present estimates that allow for serial correlation over time, as emphasized by Bertrand, Duflo and Mullainathan (2004) and Kezdi (2004). We deal with this possibility by clustering by origin-destination pair – thus allowing for unrestricted serial correlation for each pair. Alternatively, in some models we allow for unrestricted serial correlation by two-way clustering by origin and destination state.

With estimates of the parameters η and η' , it is straightforward to calculate the average elasticity of the probability of moving with respect to the net-of-tax rate. For example, for personal taxes the elasticity is

$$e = E\left[\frac{d \log P_{odt}}{d \log (1 - \tau_{ot})}\right] = \eta(1 - P) \quad (4)$$

where P is the weighted average of P_{odt} over all (d, o, t) observations, weighting each combination by the number of individuals in that observation cell. In our sample, $P = 0.06$, so the elasticities will be very close to η . The revenue maximizing tax rate is the inverse of the elasticity.

4 Identification and Threats to Validity

Taxes are set by state legislatures, certainly not in random ways. The key concern is that there are omitted determinants of scientist migration decisions that may be correlated with changes in personal or corporate taxes. Our models control for permanent heterogeneity in migratory flows, not just at the state level, but at the state *pair* level. In other words, we account not only for the fact that in our sample period some states tend to lose scientists and other states tend to gain scientists, but also for the typical migratory flow between every pair of states during our period. For example, if star scientists tend to move to California from mid-western states because they value local amenities like weather or because California has historically important clusters of innovation-driven industries (e.g., Silicon Valley), state pair effects will account for these factors, to the extent that they are permanent.

Of course, there may be omitted determinants of *changes* in migratory flows that are correlated with *changes* in taxes. Consider the business cycle, for example, which in many states is an important determinant of changes in taxes. If states tend to increase personal income taxes during recessions, and scientists tend to leave the state during recessions, then our estimates overestimate the true effect of taxes because they attribute to taxes some of the effect of recessions.¹⁰ On the other hand, if states are reluctant to impose higher taxes during recessions, when local employers are struggling, then we underestimate the effect of taxes.

The possibility of unobserved labor demand shocks driven by local business cycle is probably not a major issue because the overwhelming majority of star scientists work in the traded sector. Shocks to the demand of star scientists are likely to reflect shocks that are national or global in scope, and unlikely to reflect state specific shocks. For example, Google is based in California,

¹⁰ Diamond (2014) for example argues that local governments are more likely to extract rents during good times.

but its customers are all over the country and the world. When demand for search engines and online advertising increases, Google hires more scientists and engineers; and when it declines Google hires fewer scientists and engineers. These shocks are unlikely to reflect the state of the California economy *relative to other states*. Rather, these shocks more likely reflect the general state of the national economy, interest rates, etc.¹¹

In practice, our models control for region-of-origin*year and region-of-destination*year effects. Thus, the regional component of local business cycle is fully absorbed. In some of our models, we even control for state-of-origin*year effects or state-of-destination* year effects. Thus, the state component of local business cycle is partially absorbed. In addition, we show that the estimated effect of taxes on star migration is robust to conditioning on the contemporaneous unemployment rates in both the origin and destination states.

Another concern is the possibility that state legislatures change tax policies to help local patenters. Our models yield inconsistent estimates if state governments tend to change their tax policies based on criteria that are correlated with unobserved determinants of the economic fortunes of local employers – and in particular if they target local patenters. This may happen, for example, if state governments tend to use tax policy to help local patent-intensive industries that are struggling and are experiencing declines in employment over and above those experienced in other states. A similar concern arises in the opposite scenario, if state governments tend to use tax policy to help employers that are thriving, and experiencing increases employment over and above those experienced in other states. In either case, OLS estimates will be biased. The sign of the bias is *a priori* undetermined. If states help winners, the correlation between taxes and unobserved labor demand shocks is negative. If states help losers (compensatory policies), the correlation is positive. Note that for this to be a problem it is not enough that states change taxes to help local employers. They also need to specifically target local patenters.

Empirically, changes in taxes do not appear to be systematically associated with the fortunes of local patenters. We identify the top publicly traded patenting companies in each state and then test whether personal and corporate state tax rates in a given year are a function of local patent stock returns in the same year or in previous years. If states change their taxes to help struggling local patenters, we should observe that tax changes systematically occur when local patenters'

¹¹ If anything, the weakness of the local economy may help retain Google scientists because of lower cost of housing.

stock are under-performing. If states change their taxes to help thriving local patenters, we should observe that tax change systematically occur when local patenters' stock are under-performing.

We first identify the top five patent assignees (in terms of patent counts over the full 1977-2010 sample) in each state. The majority of these assignees are publicly traded companies; for those, we match by company name to the Compustat financial database. We then calculate the sum of market capitalization over these top patenting companies, by state and by year. Finally, in a state-by-year panel, we regress state tax rates (for each of our four tax variables) on contemporaneous and lagged values of the percentage change in market capitalization of these companies, along with state and year fixed effects.

The results are shown in Tables 2a to 2d. We find no evidence that any of these state tax policies is systematically associated with the recent fortunes of local patenting companies.

Another potential concern is the possibility that changes in economic policies other than taxes are correlated with taxes. For example, a pro-business state legislature could both cut taxes and relax state level regulations on labor and the environment. In this case, our models would likely overestimate the true effect of taxes. In this respect, models that control for state of origin*year effects or state of destination* year effects are informative of the amount of bias. For example, if an origin state both cut taxes and regulations, a model that includes origin*year effects will account for it.

Of course, we can't rule out with certainty that there are other omitted determinants of changes in migratory flows that are correlated with changes in taxes. We provide indirect evidence on the validity of our estimates with four specification tests.

First, we provide a specification test based on differences in the degree of tax progressivity across states. In particular, we estimate models that include both changes in the MTR for the top 99% of income and changes in the MTR for median income. Star scientists have high income and should be significantly more sensitive to the MTR 99%. By contrast, if taxes did not matter, and our estimates are entirely driven by omitted changes in other state policies, we might find a significant effect of MTR 50%.

Second, we provide a specification test based on the fact that corporate taxes should affect some scientists but not others.

(i) On the most obvious level, changes in corporate taxes, ITC and R&D credits should affect scientists in the private sector but scientists employed by universities and government agencies, which are mostly non-profit and therefore unaffected by corporate income taxes.

(ii) Individual inventors provide an even sharper test: they should not be sensitive to corporate taxes and ITC but they should be sensitive to R&D tax credits.

(iii) Moreover, we exploit variation in the apportionment formula. A testable hypothesis is that corporations' demand for labor, including star scientists, should be sensitive to corporate tax rates in states where payroll is one of the apportionment factors and should be less sensitive in other states.

Third, we exploit close elections. In particular, we isolate only the variation in tax changes that stems from a change in the party of the governor or the majority of the state legislature resulting from a close election. The idea is that small variation in vote share can generate large changes in policy when the governor's or majority's party change.

Finally, we exploit the timing of tax changes and we test for pre-trends.

5. Main Empirical Results

5.1 Graphical Evidence

Cross-sectional analysis reveals little relationship between tax *levels* and number of scientists. Compare Figures 1 and 2 – which display map of personal and corporate taxes by state in 2006 – with Figure 5 – which displays where top scientists were located in the same year. A cross-sectional analysis, however, can be misleading because it fails to account for other important differences across states, such as in amenities, agglomeration economies, industry composition, market size, the presence of top research institutions, etc.

The variation that we use in the data is based on changes over time in taxes. In Figure 7 we show a series of bin-scatterplots of the log odds-ratio against the log net-of-tax rate after conditioning on state-pair and year fixed effects. In other words, we demean the log odds-ratio and the log net-of-tax rate by their within-pair and within-year sample means. The two panels on the far left show outmigration between a given origin-destination pair versus the indicated net-of-tax rate in the origin state. The two panels in the center use the destination state's tax rate on the x-axis. If taxes affect the migration decisions of scientists, one would expect the relationship between origin net-of-tax rates and outmigration to be negative, as higher net-of-tax rates in the origin state (i.e. lower taxes in origin state) result in less outmigration. Similarly, one would

expect the effect of destination net-of-tax rates to be positive, as higher net-of-tax rates in the destination state (i.e. lower taxes in destination state) result in more migration to that state. Empirically, this is what we observe.

The two panels on the far right of the figure show the same type of bin-scatterplots except that the measure on the x-axis is now the *difference* between the destination state's log net-of-tax rate and the origin state's log net-of-tax rate. This specification is equivalent to those in the first four panels, but it is more parsimonious in that it forces the effect of tax changes in the origin and destination state to be the same but of opposite sign. The plots shows that higher destination-origin net-of-tax rate (after-tax income) differentials are associated with higher origin-to-destination migration.

5.2 Baseline Regressions

The baseline regression results are shown in Table 3. Each cell – consisting of a coefficient and standard error – represents a single regression of the log odds ratio on the pairwise differential between the log of the net-of-tax rates in the destination and origin states for a given tax variable (indicated in the row heading). In other words, we estimate variants of equation (3) in Section 3. All regressions contain year fixed effects.

The regressions underlying column (1) do not condition on state or state-pair fixed effects and therefore are based on cross-sectional variation. In this case, the estimated coefficient on the net-of-tax rate for personal and corporate taxes is negative and significant – contrary to expectations of a positive effect from after-tax income.

Column (2) adds state of origin and destination fixed effects, while column 3 adds state-pair fixed effects. Now the coefficients on all net-of-tax rate variables becomes positive and statistically different from zero at conventional levels. Column (3) is equivalent to the fit lines shown in the two far-right panels of Figure 7. Columns (4) to (6) show estimates conditional on a year-specific fixed effect for the region of the origin state (using Census Bureau's nine-region categorization)(column 4), a year-specific fixed effect for the region of the destination state (column 5), a year-specific fixed effect for the pair defined by the origin region and destination region (column 6).

Our preferred specification is that of column (6), which absorbs both state-pair permanent characteristics as well as time-varying year-specific shocks that are specific to each region-pair. The coefficient on the 99th MTR net-of-tax rate is 1.78 and is significant at the 1% level. Given

the average probability of moving in our sample is 6%, we obtain an implied elasticity of the moving probability with respect to the 99th MTR net-of-tax rate of 1.67 (equation (4)). The elasticity for the CIT net-of-tax rate is 1.61, and the elasticity for the investment credit is 1.47. The implied elasticity for the R&D credit is near zero and statistically insignificant.

Our estimated elasticities for personal income taxes are in line with those by Kleven, Landais Saez and Schultz (2014). They find that the elasticity of the number of top earners who decide to move to Denmark with respect to taxes is between 1.5 and 2. One might have expected our estimates to be larger than their estimates. Moving across international borders entails larger costs than moving across US states. For example, a foreigner moving to Denmark faces a new language and a different set of institutions. Moreover, our estimates reflect the long term effect of taxes, which are likely to be larger than the short run effects.

To get a sense of the magnitude of our estimated elasticity, we estimate the implied increase in star scientists in New York following that state's 2006 cut in the 99th MTR from 7.5% to 6.85%. An elasticity of 1.7 implies that the long-run effect of this tax cut will be a decline in New York's outmigration rate of (95th percentile) star scientists by 1.1 percentage points, or 12 stars out of a stock of 1,118 stars. Similarly, it implies an increase of 12 in the inflow of stars to New York. The net increase in the stock of star scientists is then 24, which is a 2.1% increase.¹²

For completeness, in the last two columns, we add a year-specific fixed effect for the origin state (column 7), and a year-specific fixed effect for the destination state (column 8). These models are almost fully saturated. They generally confirm the qualitative picture that emerges from column 6. The net-of-tax rate effect on out-migration remains consistently positive and significant across the columns for all tax variables except those involving the R&D tax credit and the median income individual MTR.

One possible concern with these regressions is that they contain only a single tax variable at a time. These tax variables may well be correlated in which case the estimated coefficients in Table 3 would suffer from omitted variable bias. Recall, however, that empirically there is very little correlation in changes in different taxes over time. In particular, we have found that the correlation between mean-differences is around 0.01 and is statistically not different from zero.

¹² The outmigration rate from New York in 2005 was 6.3% (70 stars out of a stock of 1,118), while the immigration rate was 6.8% (72 stars out of a stock of 1,118). The 1.7 elasticity implies that the 0.65 p.p. tax cut should reduce the outmigration rate by $1.7 \times 0.65 = 1.11$ percentage points (12 stars out of 1,118) and increase the in-migration rate by 1.11 percentage points. This represents a net gain of 24 star scientists.

Nonetheless, in Table 4, we include personal income tax variable along with the three business tax variables jointly in the same regression. As before, we find strong and robust evidence of a positive and significant effect of the net-of-tax rate on out-migration for the top-income individual MTR, the corporate tax, and the investment tax credit. The estimated effect of the R&D tax credit is less robust. For all of the tax variables, the point estimates and implied elasticities are similar to those found in Table 3.

Next, we relax the assumption that destination and origin taxes have symmetric effects on pairwise migration. We repeat the regressions underlying Table 3 but relax the restriction that the log net-of-tax rate in the destination state has an equal but of opposite sign effect as that in the origin state. The results are shown in Table 5.¹³ Column (1) shows the results of a specification including state-pair fixed effects while column (2) adds region-pair*year fixed effects. For both the individual and corporate net-of-tax rates, we find evidence of a negative and significant effect from the origin state tax. That is, out-migration tends to fall as net-of-tax rates, and hence after-tax incomes, rise in the origin state. Out-migration tends to rise as net-of-tax rates rise in potential destination states, though the magnitude of this effect is smaller. It is statistically insignificant for the CIT when region-pair*year fixed effects are included. The finding that the effect of tax change in the origin state is larger than the effect in the destination state might indicate that individuals have more information on taxes in their state of residence – after all, there are 49 potential destination states, and they frequently change their rates.

The two tax credit variables, the relative importance of destination and origin state taxes is reversed, with the origin state effect smaller and often insignificant. The fact that tax incentives have a stronger “pull” effect is probably not too surprising: tax credits are designed to attract and targeted to out-of-state businesses doing site selection searches, and often advertised to specific industries.

5.3 Specifications Tests

Top-Income MTR vs Median-Income MTR. In Table 6, we report the results from regressions containing both the top-income MTR and the median-income MTR simultaneously. We test whether star scientists’ locational choices are more sensitive to top-income tax rates than to middle-income tax rates. Although we don’t have direct information on star scientists’ actual

¹³ For these specifications we cannot include either origin-state*year fixed effects or destination-state*year fixed effects as that would leave no variation with which to identify the coefficients of interest.

income, we expect them to be at the very high end of the income distribution. Thus, we expect that changes in the 99th MTR should matter more than changes in the 50th percentile MTR. Finding that changes in the 50th percentile MTR matter more would cast doubt on the validity of our estimates.

Each column in Table 6 shows the results from three different regressions: 99.9th percentile MTR vs. 50th percentile MTR (top panel), 99th percentile MTR vs. 50th percentile MTR (middle panel), and 95th percentile MTR vs. 50th percentile MTR (bottom panel). The regressions underlying column (1) include state-pair and year fixed effects (as in column 3 of Tables 3-4) while those underlying column (2) add region-pair*year fixed effects (as in column 6 of Tables 3-4). In all cases, we find the top-income MTR differential to have a positive and significant effect on out-migration. The median-income MTR, on the other hand, is generally small and statistically insignificant.

Specification Tests on Business Taxes. Next we consider several specifications tests based on business taxes. The results in Tables 3 and 4 indicate a strong effect from the corporate income tax rate and the investment tax credit rate and a more modest effect from the R&D tax credit. To mitigate concerns that these results reflect some spurious correlation, we perform several additional hypotheses tests.

First, we exploit the fact that states vary in whether, and how much, employment location matters for corporate taxation of companies generating income in more than one state. As we described in the previous section, a corporation in a given state must determine what share of its national taxable income to apportion to that state based on the state's apportionment formula, which is a weighted average of the state's shares of the company's national payroll, property, and sales. The weight on employment (payroll) varies across states and over time, and ranges from zero to one-third. With a zero weight, a corporation's tax liability in a given state is independent of its level of employment in that state (conditional on having a tax liability in that state at all). Therefore, a testable hypothesis is that corporations' demand for labor, including star scientists, should be more sensitive to corporate tax rates in states with larger apportionment weights on payroll.

To test this hypothesis, we construct an indicator variable that equals one if the origin state, in that year, has a payroll weight equal to the traditional formula's one-third weight and that equals zero otherwise. (About half of our sample have a one-third payroll weight; the other half generally

have either zero or 0.1.) We construct an analogous variable for destination states. We then estimate the pair fixed effects model as in the CIT rows of Table 3, Column (2) but adding these two indicator variables as well as their interactions with the origin and destination CIT net-of-tax rates. The results are shown in Table 7. The only difference between the two columns is whether non-CIT tax rates are included. We find that the origin state's net-of-tax corporate rate reduces outmigration significantly more in origin states with a relatively high apportionment weight on payroll. Similarly, the destination state's net-of-tax corporate rate increases migration to that state significantly more when the destination state has a high payroll weight. The results are robust to conditioning on non-CIT rates (see column 2).

Next, we investigate whether migration of star scientists who (initially) work in the private sector are more sensitive to business taxes than are scientists who work for not-for-profit organizations, like academia and government. The COMETS patent database classifies the organization type of the patent owner into the following categories: firm, research institution, government, or other. Furthermore, it identifies the actual name of the organization. This allows us to additionally screen out firms that appear to be pass-through entities (such as S corporations) and not standard C corporations, and therefore are not subject to the corporate income tax. Specifically, we screen out organizations (patent assignees) whose name in the patent record contains "limited liability corporation," "limited liability partnership", and related abbreviations.

The results are shown in Table 8. We find that all three business tax policies – the corporate income tax, the investment tax credit, and the R&D tax credit – significantly affect migration for star scientists working for corporations but have no significant effect on migration of stars working for academic or governmental institutions.

In Column (4), we look at migration of individual inventors – that is, patenters who own their own patent rather than assigning it to some organization. Consistent with our prior that these inventors would not be subject to the corporate income tax rate, their migration appears to be insensitive to the corporate income tax rate.¹⁴ In most states, both individual and corporate income taxpayers can claim R&D tax credits for qualified expenditures. Many individual inventors may have non-trivial R&D expenditures (especially given that R&D includes their own labor expenses) and hence could be sensitive to the R&D tax credit. The results in column (4)

¹⁴ The only case in which CIT would matter for an individual inventor is the case where the inventor plans to create a firm in the future and is forward looking enough to take future CIT into consideration.

suggest this is the case: the coefficient on the investment net-of-tax credit rate is insignificant while the coefficient on the R&D net-of-tax credit rate is relatively small but statistically significant.¹⁵

We attempted to identify pass-through entities based on the name of the firm reported on the patent application. Unfortunately, the pass-through sample is too small to allow meaningful estimates. The small sample size reflects the fact that we are only able to identify pass-throughs if they have an obvious identifier like “Limited Liability,” “LLP”, or “LLC” in their organization name listed on the patent (under patent assignee). Moreover, pass-through entities, while common for professional services (doctor, lawyers, etc.), are not very common in high tech fields. We also miss all pass-through subsidiaries of large firms, if patents are filed under the parent firm.

5.4 Isolating Variation in Taxes that Comes from Close Elections

We turn to a regression discontinuity design based on the idea that (1) state governments controlled by the Republican (Democratic) party are more likely to enact tax cuts (increases) and (2) political party control is determined by sharp cut-offs. The party in control of the governor’s office is determined by the vote share in the most recent election, while the party in control of each legislative chamber is determined by the share of seats in the chamber controlled by the majority party. In each case, party control switches discontinuously at 50%.

We use an instrumental variable setting where identifiers for Republican control of the governor office, the state House, and the state Senate are the excluded instruments, and we condition on the margin of victory in most recent gubernatorial election and the percentages of seats held by the majority party in the state House and Senate. These models isolate the variation in tax changes that stems only from a change in the party of the governor or the majority of the state legislature resulting from a close election or shift in legislative seats close to 50%.

The results are provided in Table 9. To economize on space, the table does not show the full first-stage results; we suffice to report here that the coefficients on the Republican dummies are consistent with the prior that tax decreases are more likely under Republican governments. We also find that the party of the governor and the state house tend to be more important than the party of the state senate. The first-stage F statistics are well above standard critical values for

¹⁵ While not shown in these tables, we also estimated the effect of the individual income net-of-tax rate, for the 99th percentile, on migration of individual inventors. This net-of-tax rate has a coefficient of 2.08 (with a standard error of 0.67), roughly similar to the coefficients on the individual MTR from the full sample.

weak-instrument bias except in the case of the CIT, for which the F statistic is below standard critical values.

The IV results are qualitatively similar, though larger and much less precise, than the corresponding OLS results, with the exception of the R&D tax credit which has an unexpected negative sign. The coefficients are statistically significant for the MTR (and R&D credit). The coefficient on the CIT net-of-tax differential is economically large but not statistically significant, which is not surprising given the low first-stage fit for the CIT. We take away from these results that, at least for the MTR and CIT, IV estimates are qualitative consistent with our baseline estimates, but not precise enough to draw definitive conclusions.

5.5 Robustness

In this section, we assess whether our findings are robust to a number of alternative specifications and alternative samples.

First, we consider whether our baseline results are sensitive to the precise definition of “stars.” So far, we have defined stars as scientists in the top 5% of patent counts over the prior ten years. Appendix Tables A2 and A3 repeat the same set of regressions as in Table 3 but using a sample where stars are defined as those in the top 1% or 10% of the distribution. The results are generally similar to the baseline results. The effects are somewhat larger for the top 1% stars, probably because they are likely to have higher incomes. We can also define stars based on *citation counts* instead of patent counts in order to include scientists that produce fewer but higher quality patents and exclude scientists that produce many but less influential patents. Appendix Table A4 shows that the estimated effects are similar but slightly larger than the baseline results, possibly because these stars have higher average incomes than the baseline set of stars.

In Columns (2) and (3) of Table 10 we probe the robustness of our baseline standard errors to alternative assumptions about potential correlation of the residuals. For ease of comparison, we redisplay the baseline results in Column (1). Recall that in our baseline results allow for residuals to be clustered, within year, by origin-state *and* by destination-state. This accounts for the fact that the origin tax used in the net-of-tax differential regressor is the same, within a year, across all pairs involving that origin state, and similarly for the destination tax. In Column (2), we allow this two-way clustering by origin and by destination not just within a year but over the full sample. This is an extremely broad level of clustering as it allows for any type of serial correlation of errors across time in addition to the cross-sectional dependence across pairs. This results in about

a tripling of the standard errors. The effects for the individual top MTR remain statistically significant but at the 10% level. The effects for the three business tax variables lose their statistical significance. In Column (3), we cluster by state-pair. This allows for any type of serial correlation of errors but rules out cross-sectional dependence. We obtain standard errors very similar to the baseline case. We infer from this that the statistical significance of our estimated migration effects is robust to either cross-sectional dependence or serial correlation, but it is less robust to allowing for both simultaneously.

In Column (4), we assess how the baseline results change if one gives more weight to observations from state-pairs with larger bilateral migration flows. Specifically, we weight each pair of states based on the average outflow of stars over the full sample. This weighting leads to somewhat larger estimated effects.

In column (5), we condition on current origin and destination state economic conditions via the unemployment rate and the population in each state. We opt not to include these variables in our baseline regressions because they may well reflect endogenous responses of the state economy to changes in state taxes.

Column (6) shows that results do not change when we drop post-2006 observations from the sample, indicating that our estimates are not driven by the Great Recession.¹⁶

In column (7), we use an alternative patent database, constructed by Li, et al. (2014), that applies a disambiguation algorithm to the reported scientist (patent inventor) names to attempt to (1) separately identify multiple assignees who share a common name (e.g., John Smith) and (2) identify unique assignees who report their name slightly differently on different patents (e.g., Alex Bell, Alexander Bell, Alexander G. Bell, Alexander Graham Bell, etc.). Using this alternative database, we find somewhat larger tax effects for the personal tax rates and somewhat smaller tax effects for the CIT and the tax credits.

Finally, Appendix Table A5 shows estimates based on average tax rates, instead of marginal tax rates.

6 Timing of the Effects

¹⁶ Some eventually-granted patents are missing in the COMETS patent database because at the time of its construction many patents applied for in 2007 – 2010 (and likely a small number from even earlier years) had not yet been granted and hence are not included in this database. This affects both the numerator and the denominator of the odds ratio that we use as our dependent variable, so it is expected to represent classical measurement error.

We now explore the timing of the migration responses to tax changes. We start by investigating how the contemporaneous effect of tax changes on migration depends on the length of the time interval considered. We then trace the impulse response of scientist migration to tax changes. This allows us to determine exactly how long it takes for a tax change to affect mobility as well as test for pre-trends. In this section, we shift from the mean-difference model considered in the previous section to a time-difference (e.g., first-difference) model. Both are standard empirical models for eliminating fixed effects and are asymptotically equivalent, but the time-difference estimator avoids the well-known finite sample bias associated with estimating dynamic panel models via mean-differencing (Nickell 1981).

Appendix Figures 1 and 2 show how the estimated tax effects differ as one varies the length of the time-differencing (i.e., short-differences versus long-differences). Each data point in the graph is from a separate regression based on difference length of differencing. For example, the estimate for length of differencing = 5 is from a regression of 5-year changes in mobility on 5-year changes in the relevant tax variable. For the individual MTR, the ITC, and the R&D credit, first-differencing yields near-zero and statistically insignificant tax effects, while longer differencing, especially time differences above 5 years, yields net-of-tax rate elasticities similar to our baseline mean-differencing results. This is to be expected, as our baseline estimates in levels are to be interpreted as long run estimates.

The larger effects uncovered by long difference models reflect the fact that migration responses to tax shocks are likely to be gradual, as it takes time for scientists and firms to relocate. They also reflect the fact that short-differences, such as a one-year first-difference, are more likely to reflect temporary tax changes or increments of a multi-year phased-in tax change, while long-differences are more likely to reflect permanent, or at least persistent, tax changes. We expect mobility to be more sensitive to permanent tax changes.

We conclude that first-differences are too high frequency to adequately capture the full effect of tax changes on outmigration. By contrast, long-differencing, especially using differences in the neighborhood of five years, yields results similar to mean-differencing and arguably is better suited for exploring the full dynamic responses to net-of-tax rate shocks.

We now turn to exact timing of the migration response. We use the direct projections estimator (Jorda 2005) to estimate the impulse response function of outmigration with respect to tax change treatments. Specifically, we estimate the model:

$$y_{o,d,t+h} - y_{o,d,t-k} = \beta^h(\tau_{o,d,t} - \tau_{o,d,t-k}) + F_{t,R(o),R(d)} + \epsilon_{o,d,t+h} \quad (4)$$

where the dependent variable is the change in outmigration (log odds-ratio) from before the treatment, $t - k$, to h periods after the treatment. $F_{t,R(o),R(j)}$ represents a year-specific fixed effect for each pair of regions defined by the origin state's region and the destination state's region. We estimate the regression separately for each horizon from $h = 0$ to 10. k is the duration of the treatment period. In typical event studies, k is assumed to equal one but we also consider durations of 3 years and 5 years. To be consistent with our previous estimates, we condition on region-pair*year effects. To test for pre-trends, we extend this estimator by looking at the association between the treatment (occurring from $t - k$ to t) and changes in outmigration over intervals prior to when the treatment began (i.e., prior to $t - k$).

Results for the top individual MTR and the CIT are in Figures 8 and 9. Results for the R&D and investment credits are in Figures 10 and 11. There are three panels in each figure, one where treatment duration is assumed to be 1 year, one where it is assumed to be 3 years, and one where it is assumed to be 5 years. As we have seen, one year tax changes are likely too noisy to be informative. Given the results above, our preferred specifications are the one for 3 or 5 years. Note that in these models, the years before treatment are the ones prior to $t-3$ and $t-5$, respectively.

When tax changes are measured by either three- or five-year changes, clear dynamic patterns emerge. First, for all four tax variables star outmigration shows no pre-trends. Second, star migration rises after the change and keeps growing over time. This likely reflects the fact that it takes time for people and firms to relocate in response to a tax change.

For the individual MTR, the CIT, and the R&D credit, outmigration begins to rise shortly after the treatment period and continues to grow for at least 10 years. For investment credits, outmigration's response begins almost immediately upon the start of a tax change, grows up to about five years after the end of the tax change, and then gradually declines through the end of the ten-year horizon. The more immediate response to investment credits could reflect the fact that changes in (typically enactments of) these credits, in contrast to changes in individual and corporate income tax rates, are nearly always permanent and are fully phased-in the first year.

In models where the treatments are defined by one-year changes, the estimated impulse response is generally positive, as expected, but imprecisely estimated and showing no clear dynamic pattern.

7. Conclusions

Using data on the universe of U.S. patents filed between 1976 and 2010, we quantify how sensitive is internal migration by star scientist to changes in personal and business tax differentials across states. We uncover large, stable, and precisely estimated effects of personal and corporate taxes on star scientists' migration patterns. The long run elasticity of mobility relative to taxes is 1.6 for personal income taxes, 2.3 for state corporate income tax and 2.6 for the investment tax credit. These elasticities are economically large and significantly larger than the conventional labor supply elasticity. The effect on mobility is small in the short run, and tends to grow over time.

While we can't rule out that our estimates are biased by unobserved demand or supply shocks, a number of additional pieces of evidence lend credibility to a causal interpretation of our estimates. First, we find no evidence of pre-trends: Changes in mobility follow changes in taxes and do not precede them. Second, consistent with their high income, star scientists migratory flows are sensitive to changes in the 99th percentile marginal tax rate, but are insensitive to changes in taxes for the median income. Third, the effect of corporate income taxes is concentrated among private sector inventors: no effect is found on academic and government researchers. Fourth, corporate taxes only matter in states where the wage bill enters the state's formula for apportioning multi-state income. No effect is found in states that apportion income based only on sales (in which case labor's location has little or no effect on the tax bill). We also find no evidence that changes in state taxes are correlated with changes in the fortunes of local firms in the innovation sector in the years leading up to the tax change.

Overall, we conclude that state taxes have significant effect of the geographical location of star scientists and possibly other highly skilled workers. While there are many other factors that drive when innovative individual and innovative companies decide to locate, there are enough firms and workers on the margin that relative taxes matter.

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Table 1: Average Annual Outflow of Star Scientists, 1997-2006

Origin State	Destination State									
	California	Illinois	Massachusetts	Michigan	Minnesota	New Jersey	New York	Ohio	Pennsylvania	Texas
California	4767	13	24	10	9	17	21	8	12	25
Illinois	14	649	4	2	2	2	4	2	2	5
Massachusetts	25	3	873	2	3	4	5	2	4	4
Michigan	12	2	3	654	2	2	3	2	2	4
Minnesota	13	3	2	3	768	2	2	2	1	4
New Jersey	23	2	7	2	1	879	8	2	6	4
New York	25	3	6	4	3	10	1629	4	3	7
Ohio	8	2	2	3	1	2	2	600	3	3
Pennsylvania	13	2	4	1	1	5	4	3	679	3
Texas	26	3	5	5	4	5	7	3	3	1166

Table 2a: CIT Regressed on Annual Stock Return of Top Patenters

	No Lags No Fixed Effects	No Lags Year, State FEs	1-year Lag No Fixed Effects	1-year Lag Year, State FEs	1-4 years Lag No Fixed Effects	1-4 years Lag Year, State FEs
Annual Stock Return Growth	-0.0001 (0.0008)	0.0002 (0.0002)	-0.0001 (0.0008)	0.0002 (0.0002)	-0.0001 (0.0009)	0.0001 (0.0002)
Annual Stock Return Growth- 1 year lag			-0.0001 (0.0009)	0.0000 (0.0002)	-0.0001 (0.0009)	0.0000 (0.0002)
Annual Stock Return Growth- 2 year lag					-0.0002 (0.0009)	0.0001 (0.0002)
Annual Stock Return Growth- 3 year lag					-0.0003 (0.0009)	0.0000 (0.0002)
Annual Stock Return Growth- 4 year lag					-0.0003 (0.0009)	0.0000 (0.0002)
Joint Sig. p-value			.8787	.9202	.9899	.9964

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Each column is a separate regression. Sample consists of 1527 observations over 50 states and 33 years

Table 2b: MTR (99th Percentile) Regressed on Annual Stock Return of Top Patenters

	No Lags No Fixed Effects	No Lags Year, State FEs	1-year Lag No Fixed Effects	1-year Lag Year, State FEs	1-4 years Lag No Fixed Effects	1-4 years Lag Year, State FEs
Annual Stock Return Growth	0.0010 (0.0010)	0.0001 (0.0003)	0.0011 (0.0010)	0.0001 (0.0003)	0.0008 (0.0009)	-0.0000 (0.0003)
Annual Stock Return Growth- 1 year lag			0.0010 (0.0010)	0.0001 (0.0003)	0.0009 (0.0009)	-0.0000 (0.0003)
Annual Stock Return Growth- 2 year lag					0.0010 (0.0009)	0.0001 (0.0003)
Annual Stock Return Growth- 3 year lag					0.0011 (0.0009)	0.0002 (0.0003)
Annual Stock Return Growth- 4 year lag					0.0009 (0.0009)	0.0001 (0.0003)
Joint Sig. p-value			.3096	.7801	.4205	.9629

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Each column is a separate regression. Sample consists of 1527 observations over 50 states and 33 years

Table 2c: ITC Regressed on Annual Stock Return of Top Patenters

	No Lags No Fixed Effects	No Lags Year, State FEs	1-year Lag No Fixed Effects	1-year Lag Year, State FEs	1-4 years Lag No Fixed Effects	1-4 years Lag Year, State FEs
Annual Stock Return Growth	0.0002 (0.0006)	0.0003 (0.0004)	0.0002 (0.0006)	0.0003 (0.0004)	0.0002 (0.0007)	0.0005 (0.0004)
Annual Stock Return Growth- 1 year lag			-0.0001 (0.0006)	0.0002 (0.0004)	-0.0001 (0.0007)	0.0003 (0.0004)
Annual Stock Return Growth- 2 year lag					-0.0001 (0.0007)	0.0003 (0.0004)
Annual Stock Return Growth- 3 year lag					-0.0000 (0.0007)	0.0002 (0.0004)
Annual Stock Return Growth- 4 year lag					-0.0003 (0.0007)	-0.0001 (0.0004)
Joint Sig. p-value			.9356	.6887	.9957	.8655

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Each column is a separate regression. Sample consists of 1347 observations over 50 states and 33 years

Table 2d: RD Regressed on Annual Stock Return of Top Patenters

	No Lags No Fixed Effects	No Lags Year, State FEs	1-year Lag No Fixed Effects	1-year Lag Year, State FEs	1-4 years Lag No Fixed Effects	1-4 years Lag Year, State FEs
Annual Stock Return Growth	0.0000 (0.0014)	0.0011 (0.0011)	0.0001 (0.0014)	0.0012 (0.0011)	0.0003 (0.0015)	0.0012 (0.0011)
Annual Stock Return Growth- 1 year lag			-0.0003 (0.0015)	0.0007 (0.0011)	-0.0000 (0.0015)	0.0007 (0.0011)
Annual Stock Return Growth- 2 year lag					0.0002 (0.0015)	0.0005 (0.0011)
Annual Stock Return Growth- 3 year lag					0.0006 (0.0015)	0.0004 (0.0011)
Annual Stock Return Growth- 4 year lag					0.0007 (0.0015)	0.0003 (0.0011)
Joint Sig. p-value			.8636	.5395	.9869	.9469

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Each column is a separate regression. Sample consists of 1527 observations over 50 states and 33 years

Table 3: The Effect of Net-of-Tax Rate on Outmigration of Star Scientists: Baseline Models

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MTR, 99.9th Perc.	-2.7406*** (0.6759)	2.9158*** (0.5017)	2.8674*** (0.5095)	2.0301*** (0.4455)	2.8332*** (0.5396)	1.8735*** (0.4186)	1.5335** (0.6170)	4.1247*** (0.9737)
MTR, 99th Perc.	-2.5427*** (0.6793)	2.7726*** (0.4891)	2.6890*** (0.5026)	1.9527*** (0.4476)	2.6173*** (0.5309)	1.7791*** (0.4194)	1.6195*** (0.6150)	3.6554*** (0.9722)
MTR, 95th Perc.	-2.6035*** (0.6842)	2.7718*** (0.4887)	2.6639*** (0.5005)	1.9302*** (0.4507)	2.6421*** (0.5344)	1.8191*** (0.4261)	1.4981** (0.6218)	3.8623*** (0.9750)
State CIT Rate	-5.3489*** (0.6585)	2.4331*** (0.7112)	2.4884*** (0.6850)	2.5014*** (0.6152)	1.7422** (0.7671)	1.7157*** (0.6385)	2.3718*** (0.7220)	2.8912** (1.4407)
State ITC	6.7598*** (1.3809)	1.8805*** (0.4355)	1.8494*** (0.4945)	1.5892*** (0.5490)	1.8143*** (0.4460)	1.5615*** (0.4561)	2.6377*** (0.9410)	1.3301* (0.6799)
R&D Credit	4.3575*** (0.6108)	0.5428*** (0.1883)	0.5426*** (0.1995)	0.0793 (0.2179)	0.2423 (0.2016)	-0.2956 (0.1959)	1.2629*** (0.3243)	-0.4899 (0.3749)
Origin, Destination State FE	No	Yes	No	No	No	No	No	No
Origin*Destination Pair FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Origin Region*Year FE	No	No	No	Yes	No	No	No	No
Destination Region*Year FE	No	No	No	No	Yes	No	No	No
Origin & Destination Pair Region*Year FE	No	No	No	No	No	Yes	No	No
Origin State*Year FE	No	No	No	No	No	No	Yes	No
Destination State*Year FE	No	No	No	No	No	No	No	Yes

Each entry is from a separate regression. Coefficients are estimates of η or η' from equation (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Standard errors in parentheses, with two-way clustering by origin-state*year and destination-state*year.
All regressions include year fixed effects, and have 11941 observations

Table 4: The Effect of Net-of-Tax Rate on Outmigration of Star Scientists: Tax Variables Included Simultaneously

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MTR, 99th Perc.	0.8606 (0.6305)	2.6650*** (0.5046)	2.5701*** (0.5117)	1.8417*** (0.4563)	2.5850*** (0.5476)	1.7700*** (0.4382)	1.5921*** (0.5850)	3.5096*** (1.0202)
State CIT Rate	-2.9903*** (0.6716)	1.9406*** (0.6943)	1.9307*** (0.6657)	2.0875*** (0.6018)	1.2050 (0.7620)	1.2418* (0.6392)	1.9402*** (0.7062)	2.2120 (1.4287)
State ITC	5.3735*** (1.3042)	1.7741*** (0.4167)	1.7342*** (0.4727)	1.5769*** (0.5383)	1.8118*** (0.4509)	1.6573*** (0.4633)	2.3771*** (0.8518)	1.4385** (0.7191)
R&D Credit	3.4561*** (0.5951)	0.4581** (0.1824)	0.4676** (0.1927)	0.0326 (0.2057)	0.1705 (0.2045)	-0.3404* (0.1951)	1.1708*** (0.2975)	-0.4861 (0.3798)
Origin, Destination State FE	No	Yes	No	No	No	No	No	No
Origin*Destination Pair FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Origin Region*Year FE	No	No	No	Yes	No	No	No	No
Destination Region*Year FE	No	No	No	No	Yes	No	No	No
Origin & Destination Pair Region*Year FE	No	No	No	No	No	Yes	No	No
Origin State*Year FE	No	No	No	No	No	No	Yes	No
Destination State*Year FE	No	No	No	No	No	No	No	Yes

Each column is from a separate regression. Coefficients are estimates of η or η' from equation (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Standard errors in parentheses, with two-way clustering by origin-state*year and destination-state*year.
All regressions include year fixed effects, and have 11941 observations.

Table 5: Asymmetric Effects for Origin and Destination Taxes

	(1)	(2)
MTR, 99th Perc., Origin	-4.0570*** (0.8762)	-2.4337*** (0.6803)
MTR, 99th Perc., Dest.	1.2392** (0.5103)	1.0845** (0.4856)
State CIT Rate, Origin	-3.0659** (1.2709)	-3.1130*** (1.1809)
State CIT Rate, Dest.	1.8983*** (0.6220)	0.2824 (0.6172)
State ITC, Origin	-1.5918** (0.6233)	-0.9141 (0.7602)
State ITC, Dest.	2.1004*** (0.7332)	2.1872*** (0.5172)
R&D Credit, Origin	0.2277 (0.3168)	1.2868*** (0.3326)
R&D Credit, Dest.	1.2936*** (0.2669)	0.6640*** (0.2287)
Origin*Destination Pair FE	Yes	Yes
Origin & Destination Pair Region*Year FE	No	Yes

Each pair of entries is from a separate regression. Coefficients are estimates of η or η' from equation (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, with two-way clustering by origin-state*year and destination-state*year. All regressions include year fixed effects, and have 11941 observations.

Table 6: Top-Income MTR vs Median MTR, Star 95th Percentile

	(1)		(2)
		Panel A	
MTR, 99.9th Perc.	3.7790*** (0.7992)		1.9637*** (0.7293)
MTR, 50th Perc.	-0.3243 (0.7222)		0.5108 (0.5909)
		Panel B	
MTR, 99th Perc.	3.5410*** (0.8312)		1.7664** (0.7815)
MTR, 50th Perc.	-0.2341 (0.7369)		0.5823 (0.6144)
		Panel C	
MTR, 95th Perc.	3.2852*** (0.8351)		1.7075** (0.8041)
MTR, 50th Perc.	-0.1324 (0.7432)		0.5902 (0.6286)
Origin*Destination Pair FE	Yes		Yes
Origin & Destination Pair Region*Year FE	No		Yes

Each pair of entries is from a separate regression. Coefficients are estimates of η or η' from equation (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, with two-way clustering by origin-state*year and destination-state*year. All regressions include year fixed effects and have 11519 observations.

Table 7: Effect of Payroll Apportionment Formula Weight on Sensitivity to Corporate Tax

	(1)	(2)
State CIT Rate - Origin	-1.9017 (1.2804)	-0.8742 (1.2481)
State CIT Rate - Destination	-0.0114 (0.6783)	-0.3505 (0.7078)
Payroll Dummy*State CIT Rate- Origin	-2.1239** (0.8292)	-2.7752*** (0.8456)
Payroll Dummy*State CIT Rate- Destination	4.3111*** (0.6049)	3.8483*** (0.5333)
Payroll Dummy- Origin	-0.2865*** (0.0670)	-2.7752*** (0.8456)
Payroll Dummy- Destination	0.3006*** (0.0483)	3.8483*** (0.5333)
MTR, 99th Perc. Origin		-0.3280*** (0.0684)
MTR, 99th Perc. Destination		0.2875*** (0.0456)
State ITC - Origin		-3.6510*** (0.9537)
State ITC - Destination		1.5930*** (0.4786)
R&D Credit - Origin		-1.9381*** (0.6655)
R&D Credit - Destination		0.8255*** (0.2282)

Each column is from a separate regression. Coefficients are estimates of η or η' from equation (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Standard errors in parentheses, with two-way clustering by origin-state*year and destination-state*year.
All regressions include year and origin-destination state pair fixed effects, and have 11941 observations.

Table 8: Effects of Business Taxes in Selected Subsamples

	Log Odds Ratio (1)	Log Odds Ratio (2)	Log Odds Ratio (3)	Log Odds Ratio (4)
	Original Sample	Firm Only	Academic & Gov't Scientists	Individual Inventors
State CIT Rate	2.2433*** (0.5525)	2.9735*** (0.6849)	-1.4993 (1.9934)	2.7599** (1.1078)
State ITC	1.7393*** (0.4659)	1.9136*** (0.6066)	0.2082 (0.8473)	0.7344 (0.5531)
R&D Credit	0.4749*** (0.1807)	0.5174** (0.2293)	0.4485 (0.4228)	0.5442* (0.2957)
No. Observations	15247	9934	1233	2249

Each entry is from a separate regression. Coefficients are estimates of η or η' from equation (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses, with two-way clustering by origin-state*year and destination-state*year.

All regressions include year and origin-destination state pair fixed effects.

Sample Definitions: Column 1: All Scientists. Column 2: Scientists working for a for-profit firm only

Column 3: Scientists working for Universities, Governments, and non-profit entities only. Column 4: Unaffiliated Scientists only

Table 9: Instrumental Variables Based on Close Elections and Close Legislatures

	(1)	(2)	(3)
	OLS Results	First-stage F-Stat	Second-stage Coefficient
MTR, 99.9th Perc.	2.3754*** (0.4695)	50.1203	5.2802*** (1.7947)
MTR, 99th Perc.	2.0087*** (0.4949)	49.0960	5.5527*** (1.9140)
MTR, 95th Perc.	1.5488*** (0.4884)	41.3184	6.1494*** (2.0817)
State CIT Rate	1.1662** (0.5190)	7.6040	3.9960 (4.9680)
State ITC	1.5757*** (0.2888)	23.1387	-0.4522 (1.6151)
R&D Credit	0.2660** (0.1311)	24.3181	-2.8981*** (0.7327)

Column 1: Each entry represents a separate regression.

Columns 2 and 3: Each pair of entries represents a separate regression.

Column 2: First Stage F-Statistic. Column 3: Coefficient estimate on the net of tax differential

All regressions include origin-dest. state pair and region pair*year fixed effects

All regressions have as excluded instruments:

Victory-margin for Republican Governor, Senate-control, and House-control, and interactions.

IV regressions have as included instruments dummies for:

Republican Governor, Senate-control, and House-control, and interactions.

Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Robustness Checks

	(1) Baseline	(2) Cluster: 2-way Origin-Dest. State	(3) Cluster: State Pair	(4) Weighted by Average Outflow	(5) Including Pop. and UE controls	(6) Through 2006 Only	(7) Disambiguation Data Set
MTR, 99.9th Perc.	2.1117*** (0.4730)	2.1117* (1.1709)	2.1117*** (0.4495)	3.1980*** (0.7673)	1.6094*** (0.3730)	1.9666*** (0.4187)	3.8458*** (0.5717)
MTR, 99th Perc.	2.0300*** (0.4826)	2.0300* (1.1851)	2.0300*** (0.4641)	3.1081*** (0.7981)	1.4538*** (0.3781)	1.8787*** (0.4289)	3.7725*** (0.6173)
MTR, 95th Perc.	2.0760*** (0.4988)	2.0760* (1.2329)	2.0760*** (0.4814)	3.1111*** (0.7935)	1.4525*** (0.3921)	1.9579*** (0.4473)	3.6121*** (0.6376)
State CIT Rate	2.2433*** (0.5910)	2.2433 (1.4696)	2.2433*** (0.6565)	2.4780** (1.0882)	1.9471*** (0.6152)	2.4383*** (0.6711)	1.9299** (0.8529)
State ITC	1.7393*** (0.4984)	1.7393 (1.3556)	1.7393*** (0.3699)	3.1091*** (0.5658)	1.3919*** (0.3077)	1.9495*** (0.3571)	1.6200*** (0.5007)
R&D Credit	0.4749** (0.1933)	0.4749 (0.5936)	0.4749** (0.2048)	1.2655*** (0.3548)	0.0245 (0.1709)	0.4460** (0.1907)	0.3528 (0.2646)
No. Observations	15247	15247	15247	15247	14809	14809	7916

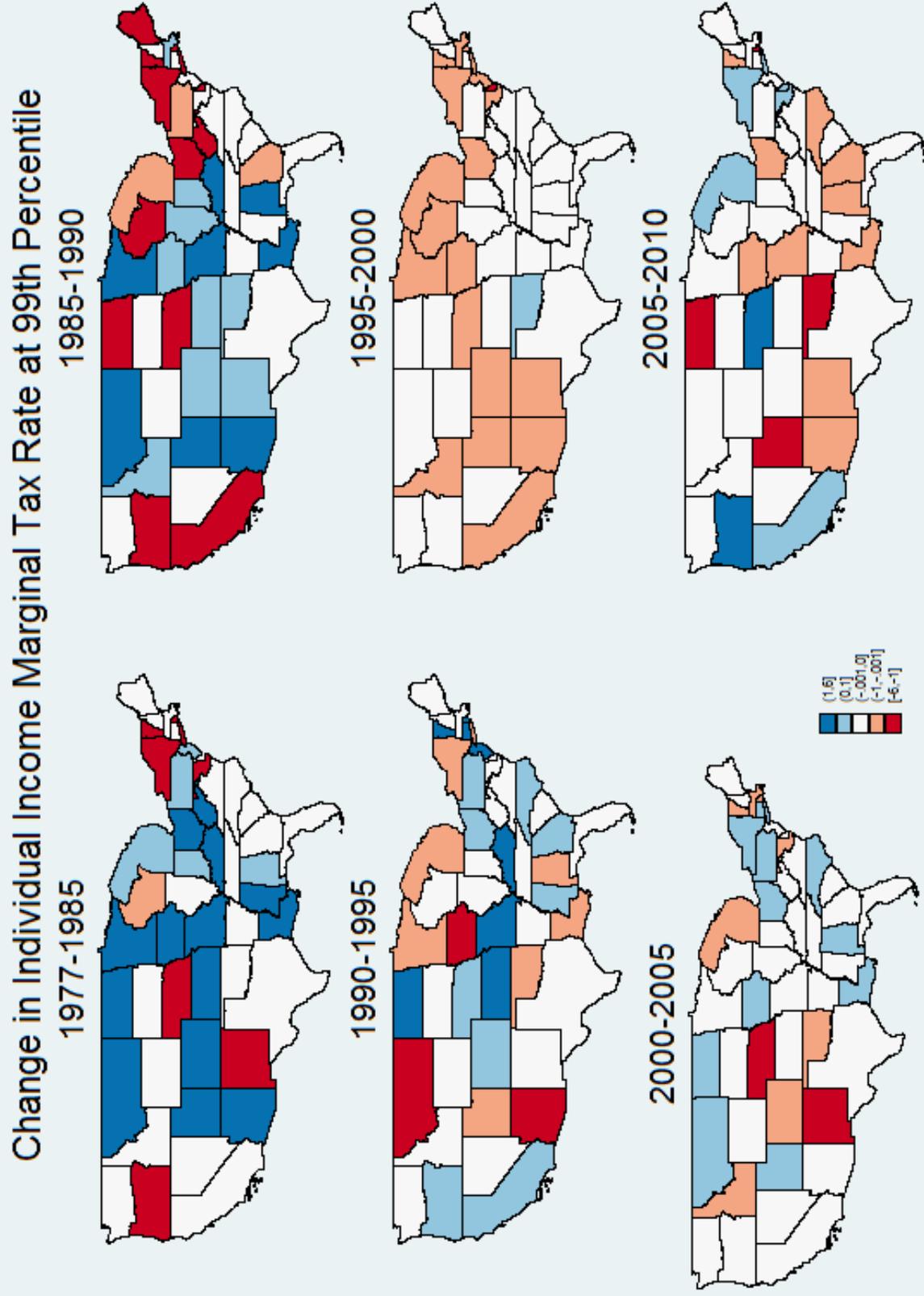
Each entry is from a separate regression. Coefficients are estimates of η or η' from equation (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors in parentheses; with two-way clustering by origin-state*year and destination-state*year, except for those in column (2) and (3).

Column 2: Std. errors clustered two-way by origin and destination state. Column 3: Std. errors clustered by state pair. Column 4: Weighted by average outflow for origin-destination pair over sample.

Column 5: Includes unemployment rate and population of origin and destination states as controls. Column 6: Sample excludes years after 2006. Column 7: Regression based on Li, et al. (2014) Disambiguation patent dataset

Figure 3



Notes: Categories are identical across maps. White indicates no change.

Figure 4

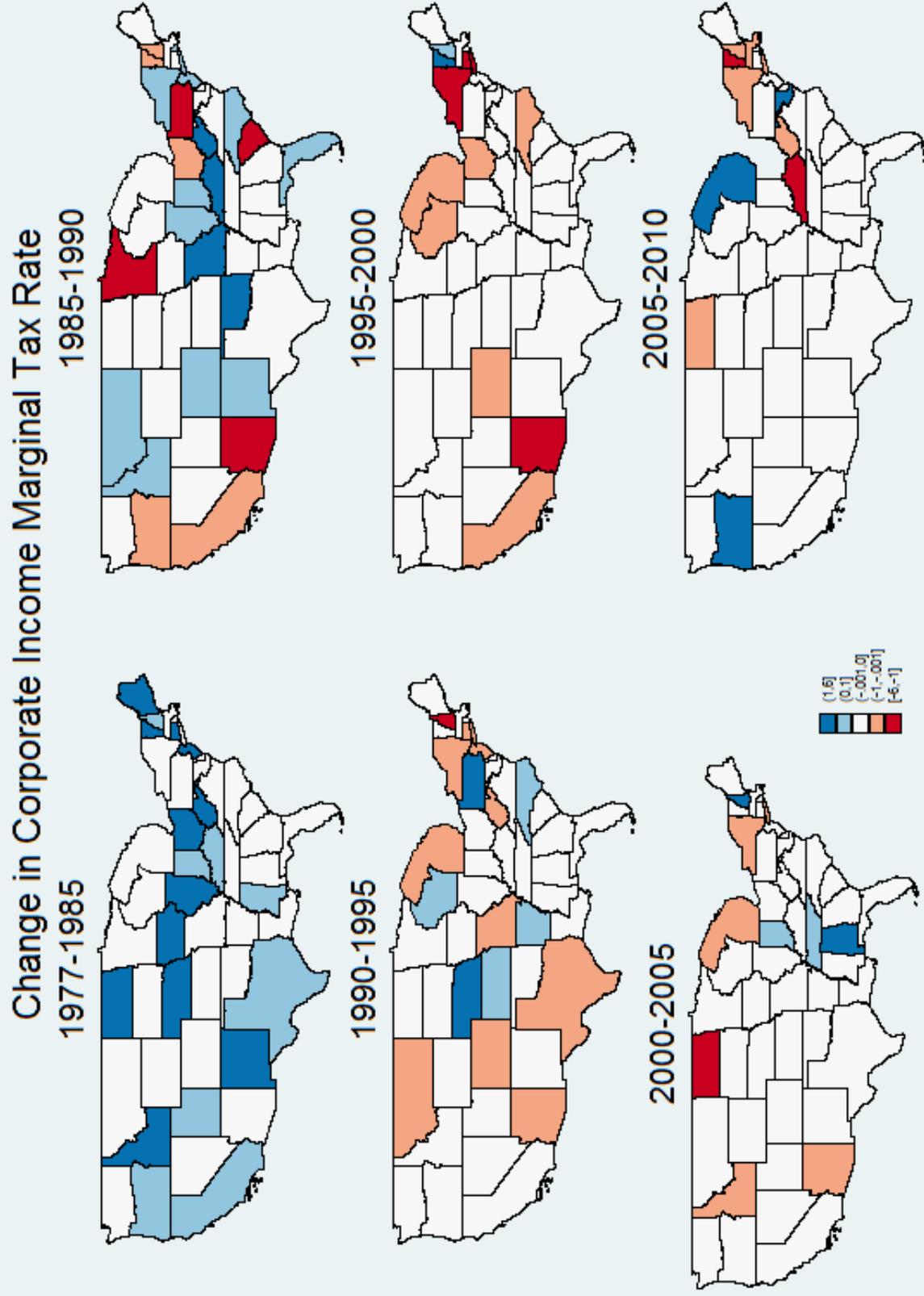


Figure 5

Number of top 5% stars in 2006

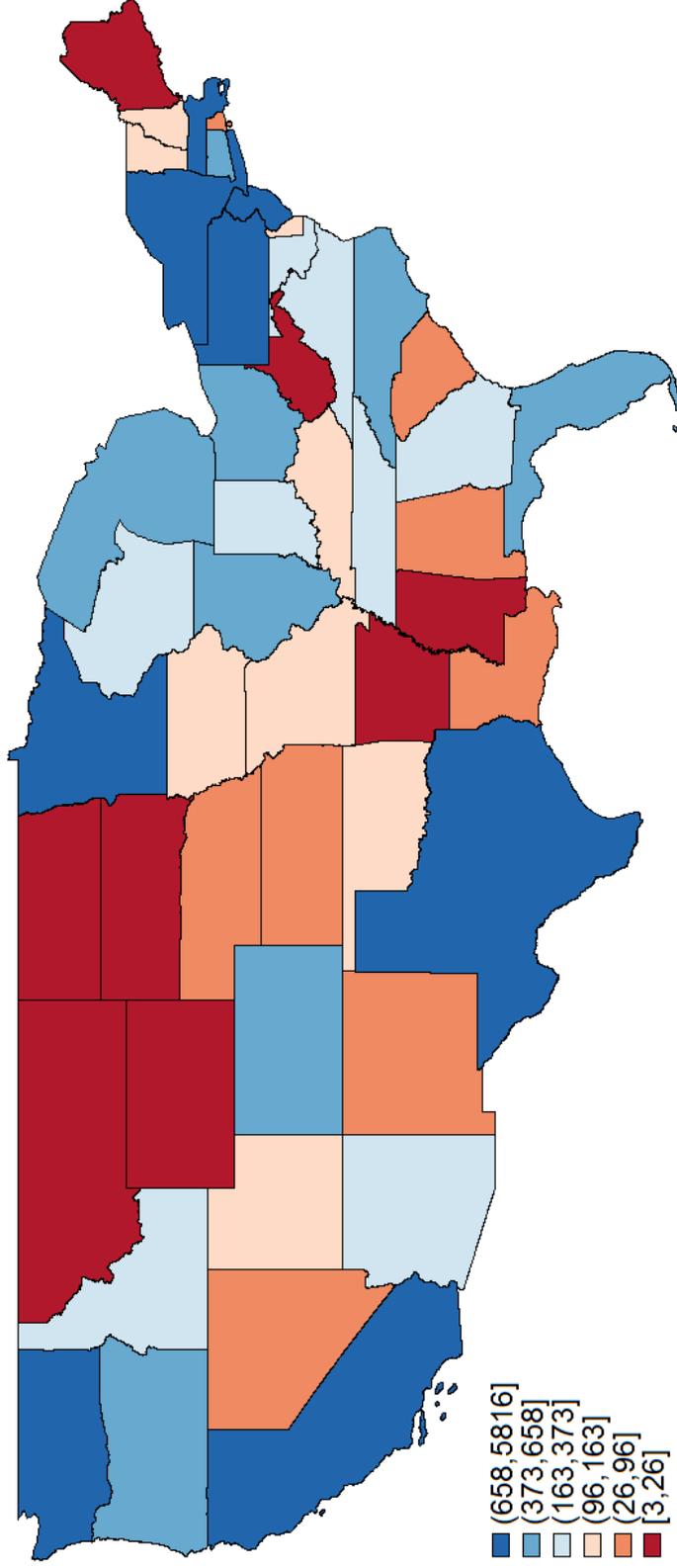


Figure 6

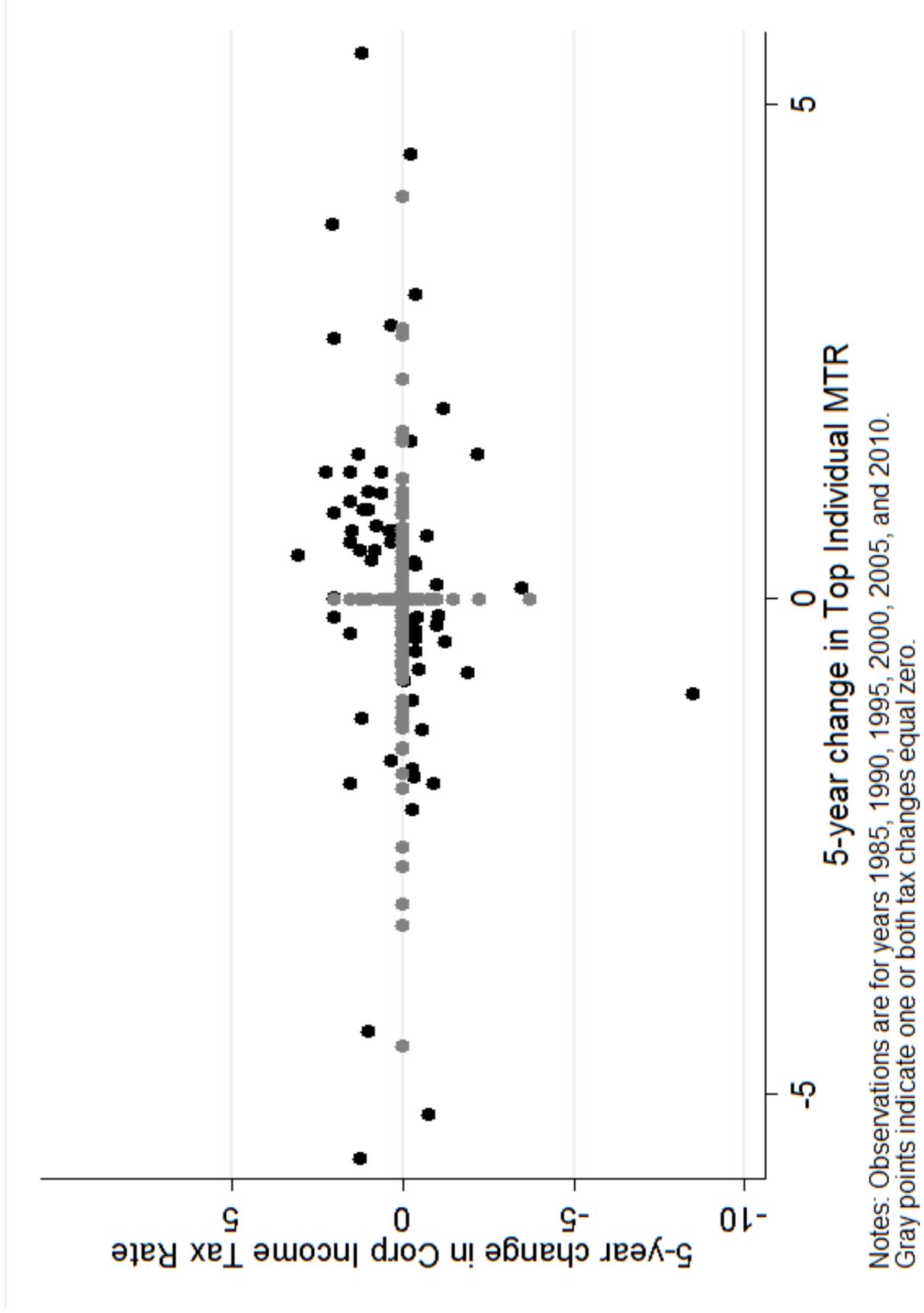
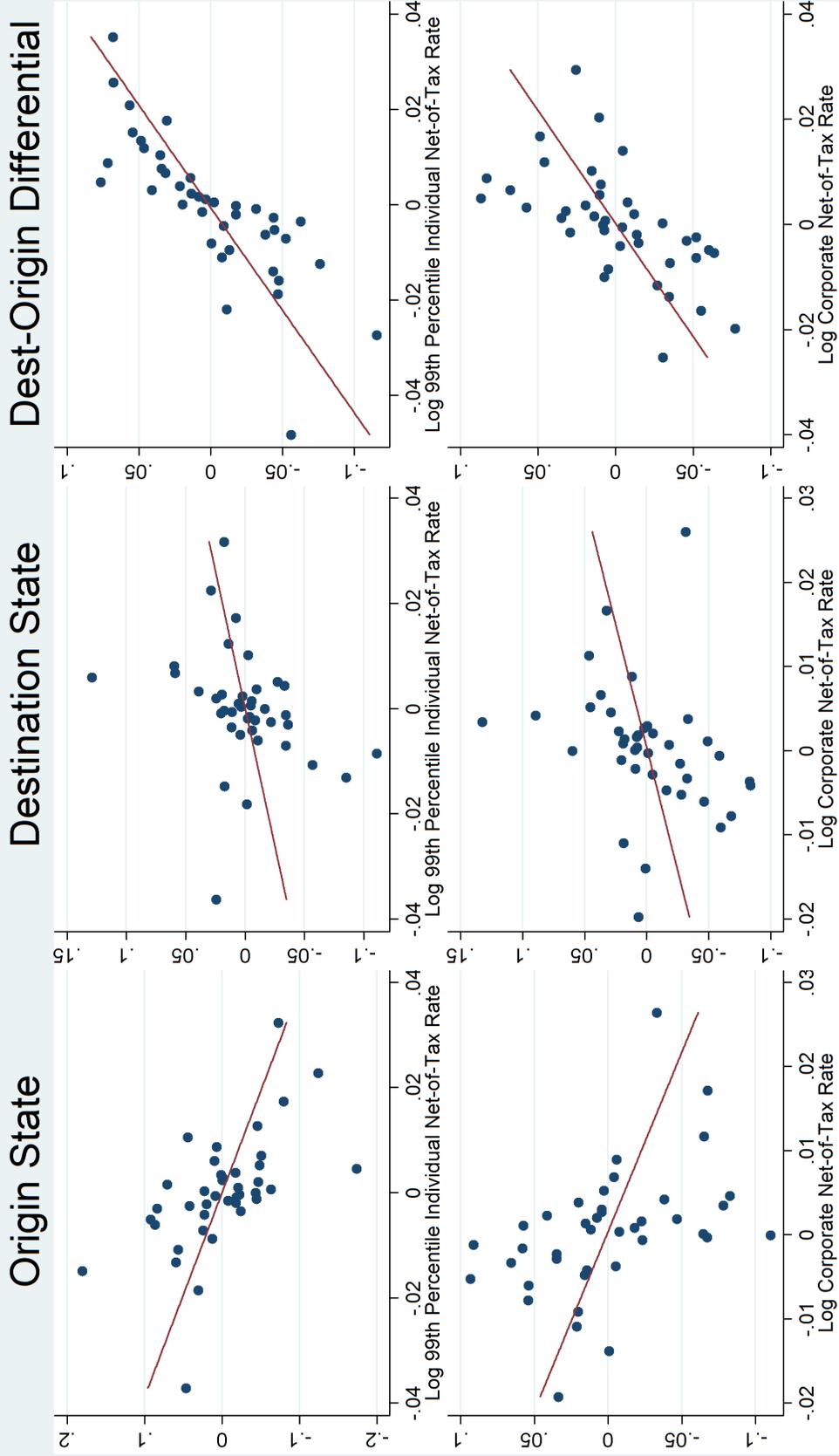


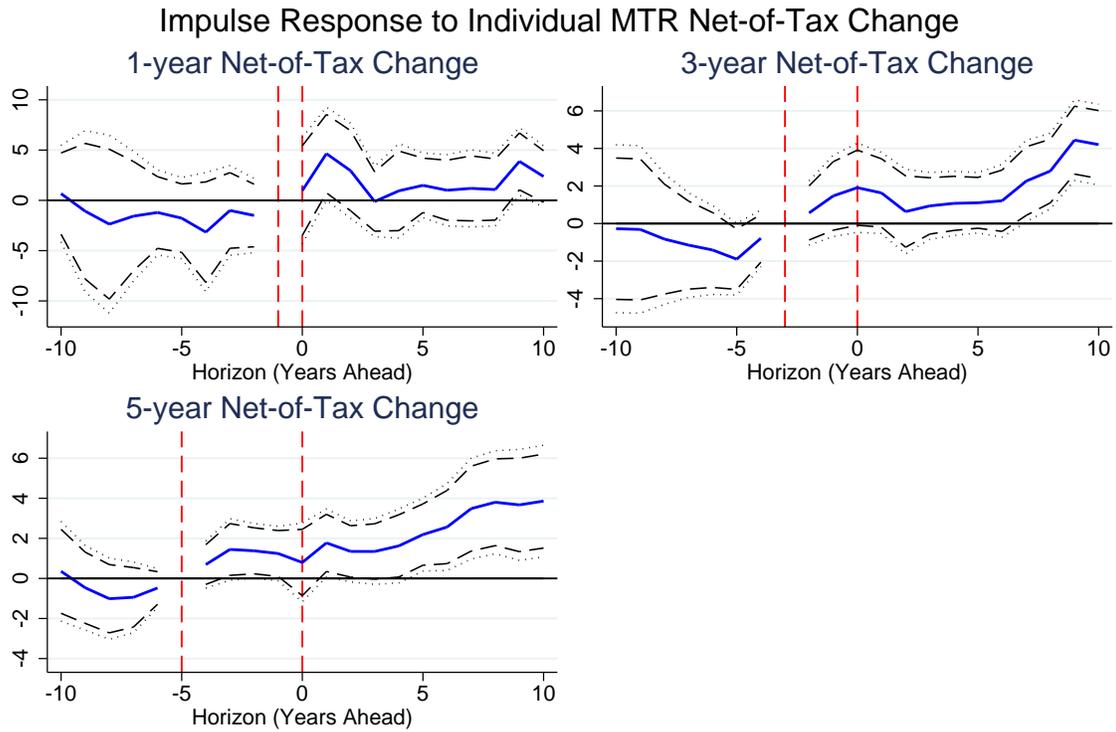
Figure 7

Outmigration Vs. Net-of-Tax Rates



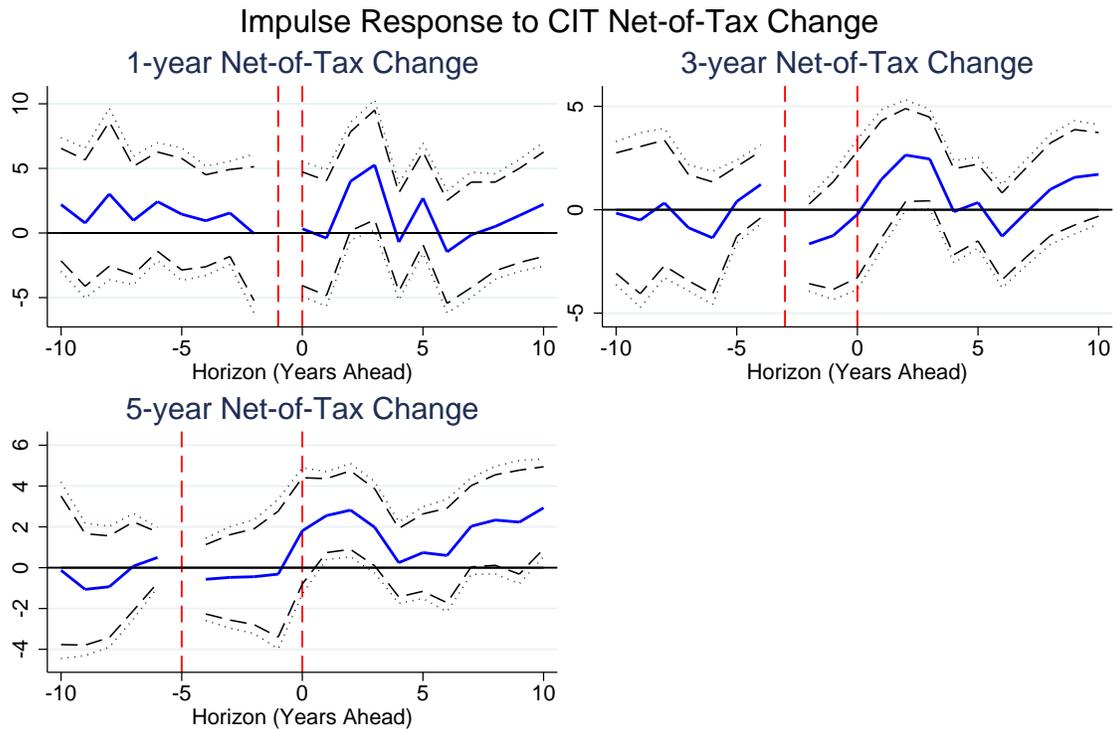
Notes: Points represent averages of x and y within quantile bins. All variables demeaned of their state-pair and year means. Star scientists defined as top 5% of patenters. Outmigration measured by log odds-ratio. Net-of-tax rate equals one minus tax rate. See text for details.

Figure 8



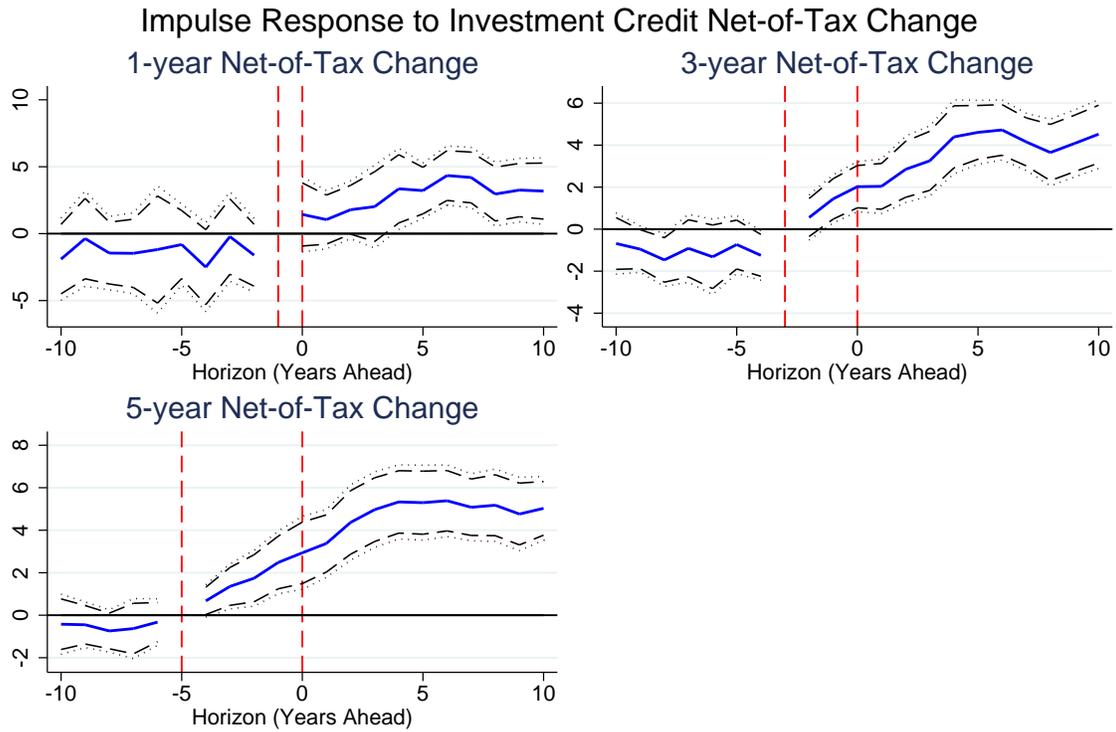
Brackets indicate 95% and 90% confidence intervals. Dashed red lines indicate start and end of treatment period. Observations weighted by average annual outflow for each dyad. All regressions use a common sample (observations available for 10 year horizon). Regression include region-pair*year fixed effects.

Figure 9



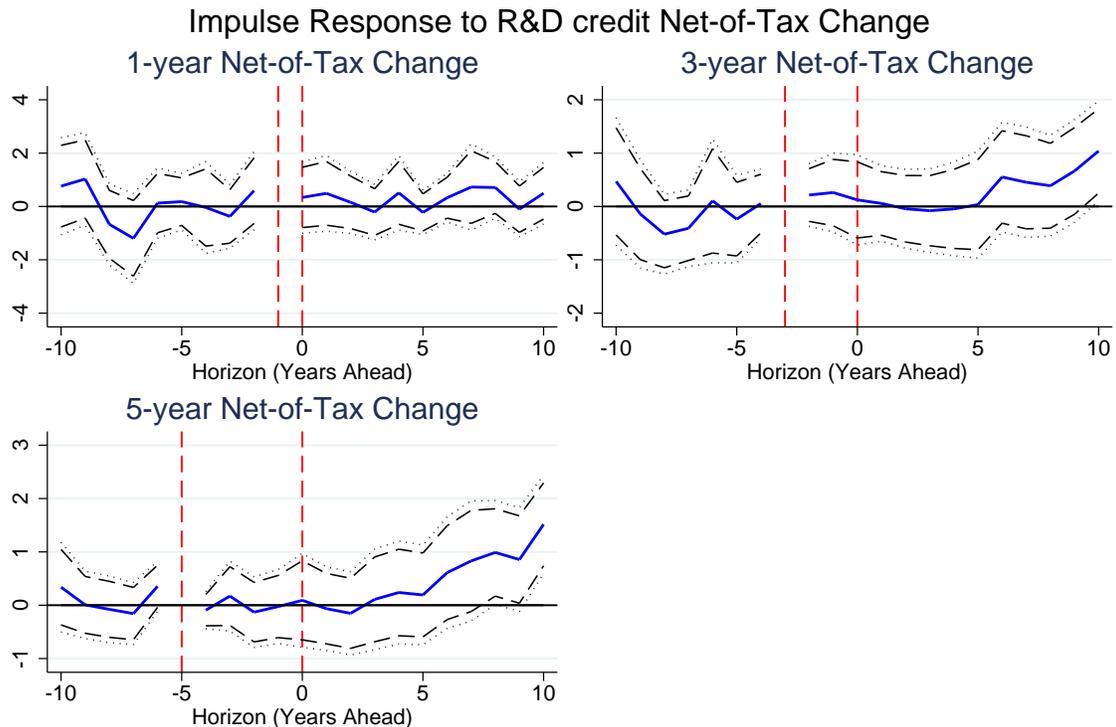
Brackets indicate 95% and 90% confidence intervals. Dashed red lines indicate start and end of treatment period. Observations weighted by average annual outflow for each dyad. All regressions use a common sample (observations available for 10 year horizon). Regression include region-pair*year fixed effects.

Figure 10



Brackets indicate 95% and 90% confidence intervals. Dashed red lines indicate start and end of treatment period. Observations weighted by average annual outflow for each dyad. All regressions use a common sample (observations available for 10 year horizon). Regression include region-pair*year fixed effects.

Figure 11



Brackets indicate 95% and 90% confidence intervals. Dashed red lines indicate start and end of treatment period. Observations weighted by average annual outflow for each dyad. All regressions use a common sample (observations available for 10 year horizon). Regression include region-pair*year fixed effects.

Table A1: Summary Statistics over 51 states-
Star Population, 99th Perc. MTR, and Corp. Income Tax

	Pop Star 95 (1) 2006 value	Pop Star 95 (2) 1977-2006 %-change	99th Perc. MTR (3) 2006 value	99th Perc. MTR (4) 1977-2006 %-change	CIT (5) 2006 value	CIT (6) 1977-2006 %-change
Alabama	26	-0.5667	0.0360	1.2360	0.0650	0.3000
Alaska	0	-1	0	-1	0.0940	0.0000
Arizona	267	1.3421	0.0466	0.7719	0.0697	-0.3362
Arkansas	10	0	0.0700	0	0.0650	0.0833
California	3869	1.8852	0.0930	-0.1545	0.0884	-0.0178
Colorado	192	0.3617	0.0463	0.8821	0.0463	-0.0740
Connecticut	290	-0.1667	0.0500	-1	0.0750	-0.2500
Delaware	77	-0.4539	0.0595	-0.6995	0.0870	0.0000
DC	7	-0.3000	0.0870	-0.1230	0.0998	0.0081
Florida	238	0.1174	0	-1	0.0550	0.1000
Georgia	174	1.2308	0.0577	-0.0383	0.0600	0.0000
Hawaii	10	0.4286	0.0778	-0.2157	0.0640	-0.0054
Idaho	219	20.9000	0.0780	0.0400	0.0760	0.1692
Illinois	419	-0.5117	0.0300	0.2000	0.0730	0.8250
Indiana	129	-0.4921	0.0340	0.7000	0.0850	0.4167
Iowa	82	0.1549	0.0647	0.4410	0.1200	0.2000
Kansas	41	-0.2115	0.0645	1.1644	0.0735	0.0889
Kentucky	60	-0.2208	0.0600	2.0928	0.0700	0.2069
Louisiana	24	-0.6800	0.0432	1.2979	0.0800	0.0000
Maine	14	0.0769	0.0850	-0.1500	0.0893	0.2757
Maryland	154	-0.2261	0.0475	-0.3667	0.0700	0.0000
Massachusetts	645	0.2697	0.0530	-0.0130	0.0950	0.0000
Michigan	470	-0.1826	0.0390	-0.1522	0.0190	-0.1915
Minnesota	593	2.1711	0.0785	0.5067	0.0980	-0.1833
Mississippi	12	-0.0769	0.0500	0.6234	0.0500	0.2500
Missouri	99	-0.3694	0.0600	2.0769	0.0625	0.2500
Montana	5	-0.2857	0.0690	0.6788	0.0675	0.0000
Nebraska	31	-0.1389	0.0509	-0.5402	0.0781	0.5778
Nevada	44	2.1429	0	-1	0	-1.0000
New Hampshire	66	0.9412	0	-1	0.0925	0.1562
New Jersey	605	-0.4271	0.0637	1.5480	0.0900	0.2000
New Mexico	40	1.2222	0.0530	-0.4111	0.0760	0.5200
New York	1265	0.2199	0.0685	-0.5433	0.0750	-0.2500
North Carolina	327	1.5349	0.0825	0.1786	0.0690	0.1500
North Dakota	5	0.2500	0.0535	0.8385	0.0700	0.1667
Ohio	398	-0.4033	0.0687	0.9629	0.0850	0.0625
Oklahoma	86	-0.5635	0.0636	0.1237	0.0600	0.5000
Oregon	291	4.1964	0.0900	-0.3077	0.0660	-0.0571
Pennsylvania	502	-0.3597	0.0307	0.5350	0.0999	0.0516
Rhode Island	33	0.1000	0.0920	-0.2042	0.0900	0.1250
South Carolina	59	0.0727	0.0700	0	0.0500	-0.1667
South Dakota	2	0	0	-1	0	-1.0000
Tennessee	91	0.0706	0	-1	0.0650	0.0833
Texas	956	1.0297	0	-1	0.0450	0.0588
Utah	84	0.7500	0.0600	1.3622	0.0500	0.2500
Vermont	163	7.5789	0.0843	-0.4304	0.0975	0.3000
Virginia	95	-0.2857	0.0575	0	0.0600	0.0000
Washington	535	4.4592	0	-1	0	-1.0000
West Virginia	4	-0.8750	0.0650	-0.2216	0.0900	0.5000
Wisconsin	216	0.0588	0.0675	-0.3415	0.0790	0.0000
Wyoming	3	-0.4000	0	-1	0	-1.0000

Table A2: Baseline Specification, Top 1% Stars

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MTR, 99.9th Perc.	-3.4097*** (0.7841)	4.4365*** (0.6364)	4.4942*** (0.6477)	3.2041*** (0.5808)	4.4461*** (0.7304)	3.2277*** (0.6012)	1.6859** (0.7862)	7.8390*** (1.5922)
MTR, 99th Perc.	-3.2499*** (0.7947)	4.3348*** (0.6239)	4.3645*** (0.6319)	3.1874*** (0.5781)	4.2400*** (0.7157)	3.1538*** (0.6036)	2.0446** (0.7948)	7.3936*** (1.6529)
MTR, 95th Perc.	-3.2926*** (0.7991)	4.4616*** (0.6257)	4.3895*** (0.6314)	3.2730*** (0.5883)	4.3060*** (0.7250)	3.3071*** (0.6240)	2.0229** (0.8085)	7.8764*** (1.6343)
State CIT Rate	-6.3729*** (0.7385)	3.4329*** (0.9647)	3.4581*** (0.9082)	3.5716*** (0.9330)	2.9124*** (1.0680)	2.8817*** (1.0400)	1.4807 (0.9942)	8.0372*** (2.1419)
State ITC	8.6758*** (1.5858)	1.7791*** (0.6259)	1.6382** (0.6526)	0.7456 (0.6742)	1.5666** (0.6759)	0.6440 (0.6078)	2.3159** (1.1272)	1.6069 (1.1791)
R&D Credit	4.8045*** (0.7416)	0.7599*** (0.2590)	0.6661** (0.2683)	-0.3163 (0.2869)	0.4245 (0.3131)	-0.7682*** (0.2916)	0.9533** (0.4169)	-0.2909 (0.6390)
Origin, Destination State FE	No	Yes	No	No	No	No	No	No
Origin*Destination Pair FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Origin Region*Year FE	No	No	No	Yes	No	No	No	No
Destination Region*Year FE	No	No	No	No	Yes	No	No	No
Origin & Destination Pair Region*Year FE	No	No	No	No	No	Yes	No	No
Origin State*Year FE	No	No	No	No	No	No	Yes	No
Destination State*Year FE	No	No	No	No	No	No	No	Yes

Each entry is from a separate regression. Coefficients are estimates of η or η' from equation (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Standard errors in parentheses, with two-way clustering by origin-state*year and destination-state*year.
All regressions include year fixed effects, and have 6258 observations

Table A3: Baseline Specification, Top 10% Stars

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MTR, 99.9th Perc.	-2.3282*** (0.6409)	2.8865*** (0.4873)	2.6450*** (0.4875)	1.8764*** (0.4369)	2.6389*** (0.5238)	1.8190*** (0.4113)	1.3685** (0.6239)	4.0279*** (0.9179)
MTR, 99th Perc.	-2.1263*** (0.6417)	2.7985*** (0.4850)	2.5204*** (0.4892)	1.8051*** (0.4446)	2.4699*** (0.5206)	1.7194*** (0.4133)	1.4130** (0.6466)	3.7147*** (0.9221)
MTR, 95th Perc.	-2.1916*** (0.6464)	2.7768*** (0.4915)	2.4730*** (0.4956)	1.7309*** (0.4562)	2.4411*** (0.5294)	1.6615*** (0.4264)	1.3979** (0.6656)	3.7618*** (0.9368)
State CIT Rate	-5.3547*** (0.6411)	1.9887*** (0.6858)	1.8812*** (0.6649)	1.7847*** (0.5951)	1.0311 (0.7540)	0.8871 (0.6142)	2.5883*** (0.7112)	1.4481 (1.2771)
State ITC	6.0969*** (1.2981)	1.7086*** (0.4011)	1.7025*** (0.4563)	1.4818*** (0.5312)	1.6679*** (0.4152)	1.4892*** (0.4532)	2.3795*** (0.8894)	1.1091 (0.6746)
R&D Credit	4.2379*** (0.5769)	0.3990** (0.1732)	0.3861** (0.1826)	0.0102 (0.2043)	0.0553 (0.1843)	-0.3934** (0.1816)	1.2568*** (0.3018)	-0.6651* (0.3397)
Origin, Destination State FE	No	Yes	No	No	No	No	No	No
Origin*Destination Pair FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Origin Region*Year FE	No	No	No	Yes	No	No	No	No
Destination Region*Year FE	No	No	No	No	Yes	No	No	No
Origin & Destination Pair Region*Year FE	No	No	No	No	No	Yes	No	No
Origin State*Year FE	No	No	No	No	No	No	Yes	No
Destination State*Year FE	No	No	No	No	No	No	No	Yes

Each entry is from a separate regression. Coefficients are estimates of η or η' from equation (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Standard errors in parentheses, with two-way clustering by origin-state*year and destination-state*year.
All regressions include year fixed effects, and have 14193 observations

Table A4: Baseline Specification, Stars Based on Citations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MTR, 99.9th Perc.	-2.8062*** (0.7421)	3.5170*** (0.6151)	3.5874*** (0.6419)	2.2380*** (0.6052)	3.6387*** (0.6715)	2.0963*** (0.5654)	2.1424** (0.9127)	5.3159*** (1.0221)
MTR, 99th Perc.	-2.5900*** (0.7470)	3.3103*** (0.6148)	3.3489*** (0.6588)	2.1207*** (0.6320)	3.3402*** (0.6854)	1.9560*** (0.5842)	2.2577** (0.9417)	4.7843*** (1.0911)
MTR, 95th Perc.	-2.6676*** (0.7523)	3.1548*** (0.6012)	3.1070*** (0.6396)	1.9681*** (0.6204)	3.1665*** (0.6740)	1.9143*** (0.5806)	1.9387** (0.9220)	4.6831*** (1.0741)
State CIT Rate	-5.2704*** (0.6827)	1.8121** (0.7431)	1.9413*** (0.6969)	1.8564*** (0.6454)	1.1658 (0.7810)	0.9763 (0.6674)	2.4371*** (0.8342)	1.2923 (1.4300)
State ITC	6.8684*** (1.4039)	1.8639*** (0.4442)	1.9298*** (0.4947)	1.6794*** (0.5446)	1.8376*** (0.4493)	1.5761*** (0.4499)	2.6185*** (0.9349)	1.4780** (0.6718)
R&D Credit	4.3838*** (0.6124)	0.4728** (0.1952)	0.4619** (0.2034)	0.0210 (0.2234)	0.1648 (0.2054)	-0.3147 (0.1988)	1.1870*** (0.3386)	-0.4950 (0.3866)
Origin, Destination State FE	No	Yes	No	No	No	No	No	No
Origin*Destination Pair FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Origin Region*Year FE	No	No	No	Yes	No	No	No	No
Destination Region*Year FE	No	No	No	No	Yes	No	No	No
Origin & Destination Pair Region*Year FE	No	No	No	No	No	Yes	No	No
Origin State*Year FE	No	No	No	No	No	No	Yes	No
Destination State*Year FE	No	No	No	No	No	No	No	Yes

Each entry is from a separate regression. Coefficients are estimates of η or η' from equation (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Standard errors in parentheses, with two-way clustering by origin-state*year and destination-state*year.
All regressions include year fixed effects, and have 10991 observations

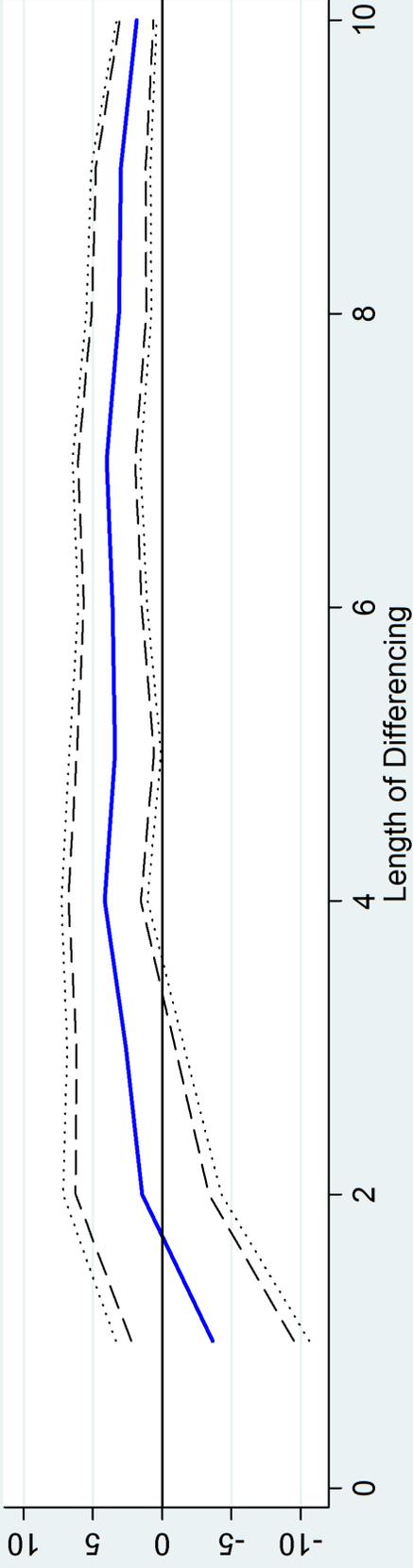
Table A5: Baseline Specification, Avg. State Tax Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MTR, 99.9th Perc.	-3.4203*** (0.7450)	3.0427*** (0.5623)	3.0566*** (0.5714)	2.6189*** (0.4987)	2.9757*** (0.6107)	2.4588*** (0.4812)	2.0374*** (0.6552)	3.6879*** (1.1148)
MTR, 99th Perc.	-2.4697*** (0.7497)	3.6649*** (0.6060)	3.5778*** (0.6233)	2.8159*** (0.5373)	3.4382*** (0.6619)	2.5591*** (0.5065)	2.4172*** (0.7428)	4.6317*** (1.2248)
MTR, 95th Perc.	-1.0825 (0.7429)	5.0735*** (0.6960)	4.9366*** (0.7161)	3.6108*** (0.6133)	4.6578*** (0.7536)	3.1408*** (0.5662)	3.5748*** (0.8918)	6.4391*** (1.3968)
Origin, Destination State FE	No	Yes	No	No	No	No	No	No
Origin*Destination Pair FE	No	No	Yes	Yes	Yes	Yes	Yes	Yes
Origin Region*Year FE	No	No	No	Yes	No	No	No	No
Destination Region*Year FE	No	No	No	No	Yes	No	No	No
Origin & Destination Pair Region*Year FE	No	No	No	No	No	Yes	No	No
Origin State*Year FE	No	No	No	No	No	No	Yes	No
Destination State*Year FE	No	No	No	No	No	No	No	Yes

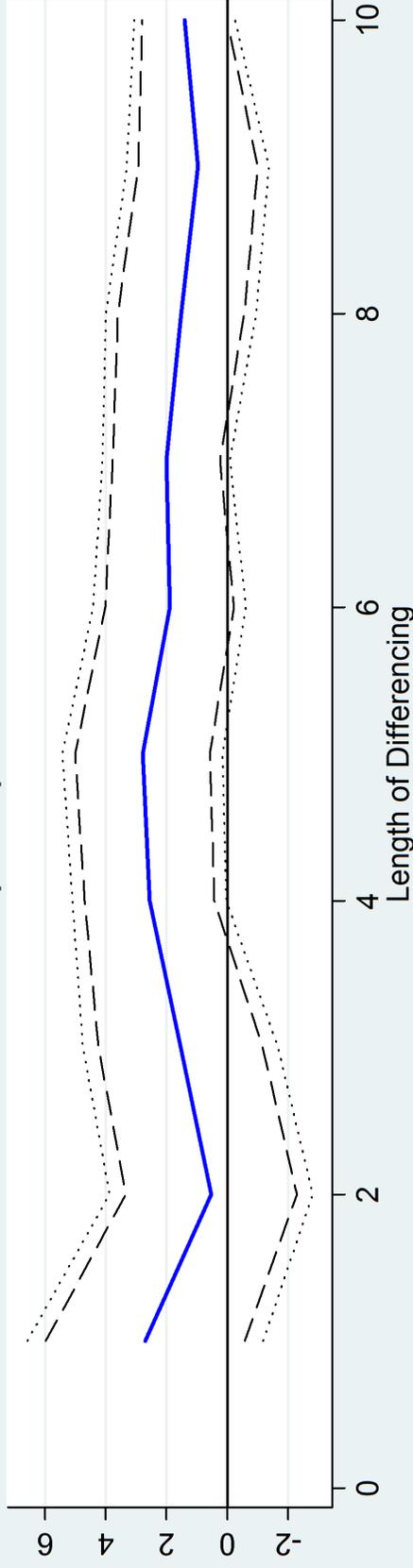
Each entry is from a separate regression. Coefficients are estimates of η or η' from equation (3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Standard errors in parentheses, with two-way clustering by origin-state*year and destination-state*year.
All regressions include year fixed effects, and have 11941 observations

Appendix Figure 1

Net-of-Tax Rate Effect on Outmigration Using Long-Difference Estimator Varying Lengths of Differencing Top Individual MTR



Top Corporate MTR

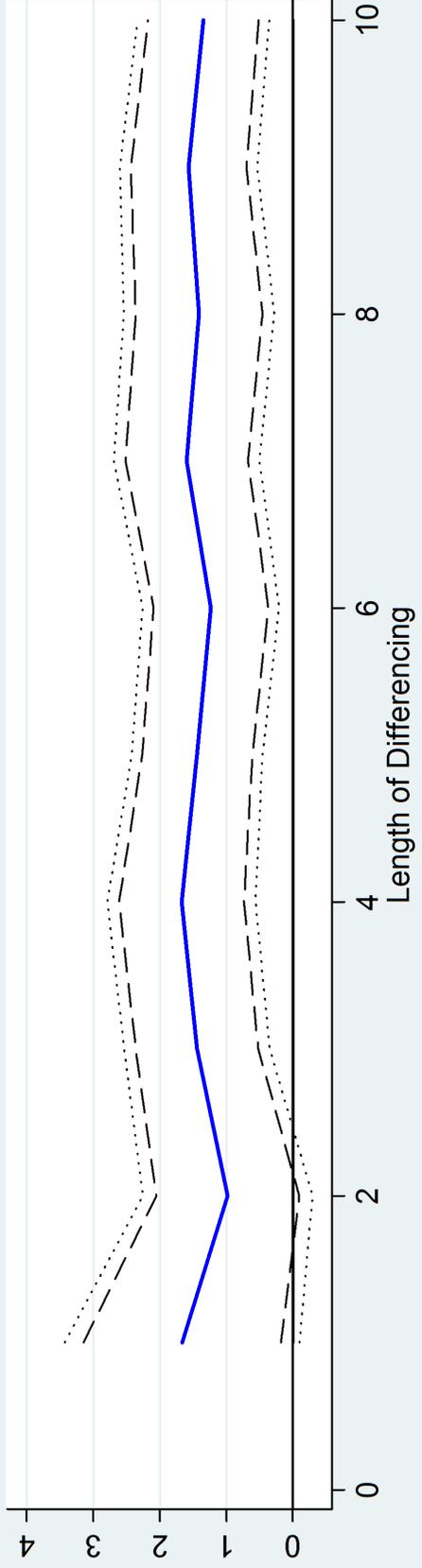


Brackets indicate 95% and 90% confidence intervals.
All regressions use common sample (sample available for $s = 10$).

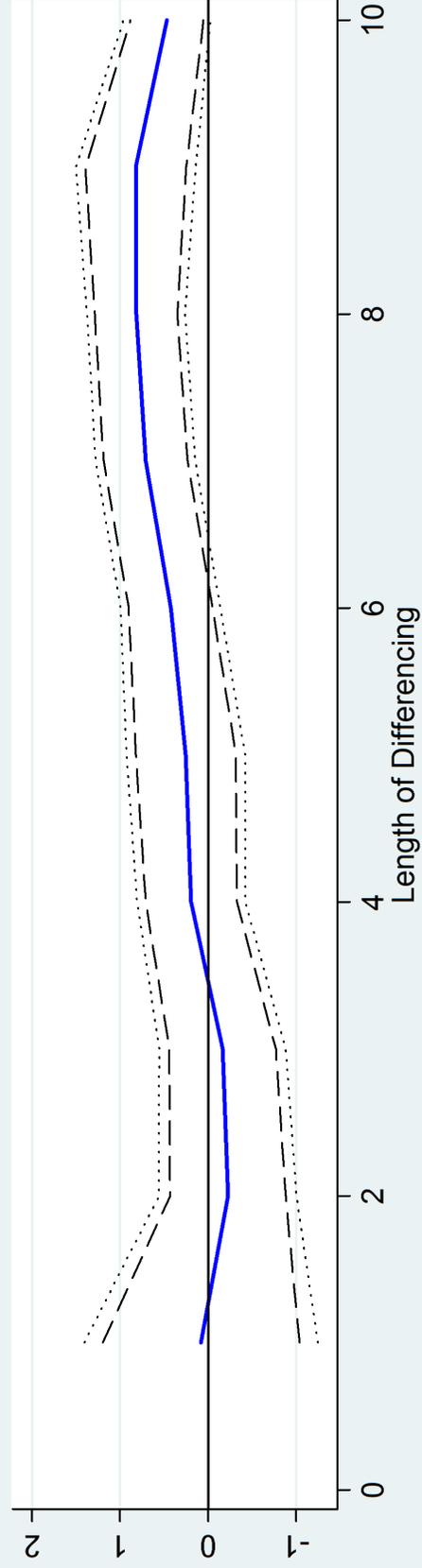
Appendix Figure 2

Net-of-Tax Rate Effect on Outmigration Using Long-Difference Estimator Varying Lengths of Differencing

Investment Credit Rate



R&D Credit Rate



Brackets indicate 95% and 90% confidence intervals.
All regressions use common sample (sample available for $s = 10$).