State "Death" Taxes and Elderly Migration—The Chicken or the Egg?

Abstract - *Researchers and practitioners hypothesize that the elderly may move to avoid paying "death" (estate, inheritance and gift, or EIG) taxes. Past research on elderly migration, however, has been based on cross–sectional data. Cross–sectional analyses may be misleading as many states that are historical net–importers of the elderly were also among the first to reduce or eliminate their EIG taxes. Using time variability in migration patterns and EIG tax policies and using the young as a "control" group eliminates any evidence that EIG policies affect elderly migration. Rather, we provide evidence that the causality may instead run in the other direction.*

INTRODUCTION

The retired elderly population is one of the fastest growing population groups and also one of the most potentially mobile. Without strong ties to the labor market and facing a unique economic situation, the elderly may be drawn to jurisdictions that offer a certain combination of fiscal policies and other amenities. In particular, researchers and policymakers have hypothesized that the elderly may move to another state to avoid paying certain kinds of taxes or to avoid paying for certain kinds of services such as welfare or education. And the stakes are nontrivial; Longino and Crown (1989) estimate that Florida had a net gain of $5 billion in income from elderly migrants it received between 1985 and 1990, and Sastry (1992, p. 73) estimates that one new job is created for every 2.5 elderly migrants Florida receives.[1] Recognizing this possibility, some states, such as Mississippi, have repealed all income taxes on pension income or made other changes to their tax systems in an attempt to become retirement havens (Mackey and Carter, 1994).

Nowhere is this dynamic more evident than in the state "death" tax arena (i.e., estate, inheritance and gift taxes, henceforth called EIG taxes). As outlined in Conway and Rork

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[1] However, others have argued that the total effect of elderly migration is more complex. For instance, Crown (1988) notes that such migration only adds to the demand side of the economy, unlike younger migrants who also add to the labor force (the supply side). To our knowledge, no comprehensive analysis that carefully weighs all of the possible effects exists, a void that has been noted (e.g., Serow (1992)).

(2004), since 1976 more than 30 states have eliminated their *incremental* EIG taxes and instead rely solely on the "pick–up" tax since 1976.[2] And, the very nature of state EIG taxes has changed dramatically, even for those states that have retained them. In the 1960s, state EIG taxes were imposed on households with only modest levels of wealth. For instance, of the 43 states that had EIG taxes in 1964, 29 states imposed taxes on estates of $10,000 or less; 16 states imposed taxes on estates of $5,000 or less.[3] In contrast, at present only 11 states impose incremental EIG taxes at all, and the exemptions are higher, such that modest income households are rarely affected.[4] Furthermore, this phenomenon appears to be the result of strategic state EIG tax competition whereby states eliminate or reduce their incremental EIG taxes in an effort to attract the elderly. In our view, however, a central question remains—do the elderly actually move in response to state EIG (or "death") tax policies?

Past research, while mostly supportive, has important limitations casting doubt on their findings. And, as we point out shortly, these limitations have implications for elderly migration research more generally and addressing them helps to resolve some of the recurring, puzzling results that appear in this area. Our research seeks to address these shortcomings and, in so doing, helps to resolve other questionable findings regarding elderly migration behavior.

One very important shortcoming is that all past elderly migration studies use only cross–sectional data.[5] The validity of this approach is questioned, however, when one observes patterns in EIG taxes and elderly migration over time, and that the states that are historically big net–importers of the elderly are also the first to eliminate their incremental EIG taxes. For instance, Florida was among the first states to eliminate their EIG taxes, doing so in the 1930s—well before the modern phenomenon of widespread retiree migration. Did this early elimination lead Florida to become a retirement haven? On the other hand, Arizona is another state that has long been a popular retirement haven and yet it did not formally eliminate its incremental EIG taxes until 1980.[6] New Mexico, California and Oregon are

[2] Five states eliminated their *incremental* EIG taxes prior to 1960 or never had them. The "pick–up" tax, also known as the "soak–up" tax or "gap" tax, arises from federal estate tax law whereby state EIG taxes are credited dollar for dollar against the federal tax liability owed, up to a certain amount. This credit, therefore, allows the state to "pick up" or "soak up" some of the federal estate tax revenue without increasing the total tax liability of the estate. An important distinction exists, therefore, between states that impose only the "pick–up" tax (and, therefore, report EIG tax revenues, but impose no additional burden on the taxpayer) and those states that impose *incremental* EIG taxes, taxes that are in addition to the pick–up tax and add to the taxpayer's EIG tax liability. All states currently take advantage of the "pick–up" tax, although the federal tax package adopted in 2001 phases out this tax credit completely by 2005 and replaces it with a deduction.

[3] This is based on the exemption level for an estate left to an adult child as listed on the World Tax Database provided by the Office of Tax Policy Research at the University of Michigan.

[4] One of those 11 states, Connecticut eliminated its incremental estate tax as of January 1, 2005 and is in the process of eliminating its inheritance tax, which is slated to be completely eliminated by 2008. This number ignores the states that are effectively imposing incremental EIG taxes by "decoupling" from the 2001 federal tax law. We address this issue further in the second subsection of the sixth section.

[5] As we discuss in detail in the second section, there is one study that uses time variation—Bakija and Slemrod (2004). However, their data is not migration–based and migration is instead inferred from the distribution of estates subject to federal taxation across the 50 states. Furthermore, their measure is only potentially capturing the migration of the very high income elderly as most estates are exempt from federal taxation, especially in recent years.

[6] However, its EIG tax policy was constructed in such a way that there was no incremental EIG tax burden except between 1977 and 1979, so that it may not be the best example. Three other top importers of the elderly in 1970—New Mexico, California and Oregon—did have other incremental taxes that were not eliminated until 1976, 1982 and 1987, respectively.

similar cases. Examining cross–sectional data (after 1980) could lead to the erroneous conclusion that low EIG taxes “caused” these states to be attractive destinations.

This begs the question of causality: do lower EIG taxes truly lead to heavy net in–migration; or do low EIG taxes capture some unobserved, longstanding desirability of the state for the elderly; or are they perhaps the result of the political influence of these in–migrants? This question extends beyond EIG taxes to other “suspected” determinants of elderly migration such as cost–of–living, welfare expenditures and income taxes. Our analysis addresses this issue by using migration data from four different censuses (1970, 1980, 1990, and 2000) combined with information about changes in state policy and characteristics to track how changes in elderly movements are related to policy changes. And, as discussed briefly above and in more detail shortly, there has been a great deal of change to state EIG tax policy during this time period, providing ample opportunity to see whether the elderly have responded.

A second improvement lies in our measures of state EIG tax policy. Measuring a tax burden is a challenge when using aggregate data because in addition to the complexity of the tax law itself, one must also arrive at a summary measure that is representative for all individuals. For this reason, many studies have relied on measures such as tax shares and effort indices. However, these measures will be nonzero and, more importantly, may vary in magnitude for those states that only impose the “pick–up” tax (i.e., states that impose an EIG tax liability that is exactly equal to the credit allowed against the federal liability), even though the true net burden to the estate is zero. We account for the role of the “pick–up” tax and use several alternative measures that seek to capture the average burden of the state’s EIG tax. We also explore whether the progressivity of the state’s EIG tax has an impact, which seems likely as the vast majority of the elderly do not owe any federal estate taxes and, in a progressive EIG state system, may owe little or nothing in state EIG taxes.

Our third advance borrows from the difference–in–difference approach, using nonelderly age groups as a “pseudo–control” group. We use the term “pseudo–control” group, however, as a cautionary reminder that this exercise is not a typical “treatment vs. control” experiment because the young may also be affected by EIG tax policy. For instance, if EIG tax policy attracts the elderly and this stimulates the local economy, it may also end up attracting the young. Of course, the reverse also likely holds: because one motive for elderly migration is to move closer to one’s children, any factor that attracts workers may also attract the elderly. To explore these issues and move closer to capturing the *differential* effects on elderly migration of state EIG policy, we also estimate these models for the young and for the difference between the elderly and young.

Finally, we offer a supplemental analysis that explores whether the causality may perhaps run the other way—i.e., whether elderly migration may be influencing EIG tax policy. In particular, we turn our empirical analysis around by estimating the effects of various migration measures on our different measures of incremental EIG tax policy.

The end result of our research is a more accurate picture of how elderly migration responds to state fiscal policy, especially state “death” taxes. Moreover, our results reveal that the improvements we bring have a real impact on the estimated impact of EIG taxes and other state policies while resolving several persistent, puzzling results found in the broader elderly migration literature. They also reveal that it appears just as likely that the causality runs in the opposite direction, i.e., that migration patterns affect state EIG tax policy.

PAST RESEARCH

Past research on the effects of state EIG taxes on elderly migration is fairly sparse, although growing in recent years.[7] Early studies tend to include only one or two fiscal variables and exclude cost–of–living (e.g., Cebula (1990), and Voss, Gunderson, and Manchin (1988)). More recent research addresses these deficiencies by more fully specifying the public sector and including cost–of–living (e.g., Clark and Hunter (1992), Conway and Houtenville (1998, 2001, 2003), Gale and Heath (2000)). The majority of these studies employ aggregate data such as migration rates or migration flows, although some, such as Dresher (1994) and Farnham and Sevak (forthcoming), use individual–level data. While there are certainly advantages to using individual–level data, such studies are hampered by the small number of movers present in the sample; for example, Dresher (1994) studies only 91 movers using data from the Panel Study of Income Dynamics (PSID). Furthermore, using individual–level data also tends to limit the time period one can study. For instance, Farnham and Sevak (forthcoming) use the Health and Retirement Study (HRS), which, by focusing on the elderly, bolsters the number of movers studied, but is limited to the period between 1992 and 2000.[8]

The small number of elderly migration studies that consider state EIG taxes produce some support that the elderly move to avoid state EIG taxes, but have also produced some counter–intuitive results. Voss et al. (1988) and Conway and Houtenville (2001, 2003) use state–to–state migration flows and find that state EIG taxes make for a less desirable destination. Clark and Hunter (1992) use imputed county net–inmigration rates and find similar results for those aged 50–70 years. Interestingly, the one study that uses individual–level data—Dresher (1994)—finds no effect of EIG taxes, although her representation of the public sector is incomplete as she aggregates all expenditures into one variable.[9]

A difficulty in interpreting many past elderly migration studies, regardless of their focus, is the pervasive "same–sign" problem. Migration flow studies reveal the "same–sign" problem with destination and origin coefficients that are the *same* rather than opposite sign—e.g., EIG taxes have negative origin and destination coefficients. This problem has plagued all past migration flow studies and extends well beyond EIG taxes and other fiscal policies to other determinants as well (e.g., McLeod, Parker, Serow, and Rives (1984), Fournier et al. (1988)). A similar problem arises with migration *rate* research in which the factors that increase (decrease) out–migration rates are also found to increase (decrease) in–migration (see Conway and Houtenville (1998) for further discussion). In migration rate research, the

[7] Rather, most elderly migration research has been interdisciplinary, emphasizing amenities (such as climate and health services) and cost–of–living as primary factors, while typically excluding fiscal policy as a potential factor (e.g., Meyer and Speare (1985), Fournier, Rasmussen, and Serow (1988), Kallan (1993), and Newbold (1996)). Building on the "developmental context" put forth by Litwak and Longino (1987), it also has tended to focus more on the different motives for moving, such as amenity–movers, assistance–movers and return migration.

[8] Farnham and Sevak (forthcoming) study elderly migration by examining what happens to the fiscal bundle facing elderly households both before and after a move is made, compared to elderly households that did not move. They do not consider EIG taxes, however.

[9] Duncombe, Robbins, and Wolf (2001) take a different approach and use the 1990 Census county–to–county migration flows to estimate a discrete choice framework, essentially treating the aggregate data as individual observations. Their results suggest that individuals are repelled by high inheritance taxes and perhaps by estate taxes.

problem is not always evident because researchers sometime fail to report both in–migration and out–migration results (or only have net in–migration, which they have imputed from births and deaths, as in Clark and Hunter (1992)). For this reason, we explore in–migration, out–migration and net in–migration separately in our empirical analysis. This “same–sign” problem and the general positive correlation between in–migration and out–migration extends beyond elderly migration research to migration in general and has been discussed for some time now (e.g., Sjaastad (1962), Greenwood (1975)).

Another persistent, counter–intuitive result has been the apparent desirability of crime to the elderly (e.g., Duncombe et al. (2001), Fournier et al. (1988)). Most elderly migration flow or rate studies have found a positive effect of crime—that is, the elderly are lured to destinations with high crime rates. The typical explanation put forward is aggregation bias (e.g., Conway and Houtenville (2003)), the fact that a state crime rate is not a terribly meaningful measure. Nonetheless, these puzzling results lead one to question the credibility of the empirical results of elderly migration research more generally. One goal of our research is to see whether the same–sign problem and odd crime result persist when one considers changes in migration patterns over time and/or comparisons across age groups.

Past elderly migration studies have also used EIG tax measures that are incomplete. Typically, EIG tax shares, effort indices or tax rates have been used, and it is not always clear how these measures take account of the “pick–up” tax. Perhaps most importantly, none of these studies has considered changes in migration over time nor have they considered using “young” migrants as a “pseudo–control” group as a way of estimating the differential effects of EIG policy on elderly migration specifically.

A recent paper by Bakija and Slemrod (2004) is an important advance in addressing these shortcomings, but uses a very different kind of data and methodology from past studies. As such, their study provides an excellent complement to ours. Specifically, Bakija and Slemrod (2004) use federal estate tax return filings by state for 18 selected years during 1965–98.[10] The authors construct an EIG tax calculator that takes account of the interdependence between the federal and state EIG tax codes, which they then use to calculate combined federal–state EIG marginal and average tax rates to arrive at their measure of the state EIG tax. They construct similar measures for other state taxes such as income and sales but, like Dresher (1994), do not consider the expenditure side of state policy. Like us, they use the young as a comparison group and use the time variability to identify the effects of EIG taxes.

Bakija and Slemrod (2004) then estimate the total number of federal estate returns for several different wealth classes in each state and year as a function of state tax variables, amenities and demographic controls (e.g., number of deaths, wealth of young population) and they include state fixed effects. Their results suggest that state EIG taxes decrease the number of federal estates filed, especially for the largest estates. From these results, they infer that state EIG taxes may affect elderly migration decisions.

However, there are important limitations to their approach. First, as the authors acknowledge, one must infer migration behavior indirectly from the number of estates within a state; e.g., an increase in the number of estates within a state is evidence of net in–migration. Although the authors attempt to control for

[10] The specific years included are 1965, 1969, 1976, 1982, and 1985–98 (Bakija and Slemrod, 2004, p. 6). The data, therefore, most strongly represent more recent years, in which state EIG taxes tended to be much lower.

other factors that might affect estates, the link to actual elderly movements remains somewhat tenuous in our view.[11] If one accepts that estates are capturing migration behavior, there is still the issue that the vast majority of elderly households does not pay federal estate taxes and yet may pay state EIG taxes, as noted by Page (2003) and as we discuss in more detail shortly. This is especially true given that the majority of their data refers to recent years in which both federal and state EIG taxation have been imposed on even fewer estates. Thus, their measure likely excludes the high middle–income elderly for whom the state EIG tax may be the only estate tax they must pay. Presumably, this is precisely the group most courted by the states, given their greater number, as opposed to the very rich. Finally, Bakija and Slemrod (2004) fail to consider the expenditure side of state fiscal policy.

In our investigation, we examine the effects of several measures of state EIG taxes, along with measures of both other taxes and expenditures, on the migration rates of the elderly (defined as over age 65). In this way, we can examine the impact that these policies have on the aggregate number of elderly movers, not just a small subset of the very rich. We also do not have to infer migration from distributional patterns, and our data is balanced equally over a 40–year period during which federal and state EIG taxes were both substantially reduced. For these reasons, we view our study as an excellent companion to Bakija and Slemrod (2004) in an attempt to answer the question of whether the elderly respond to state EIG tax policy.

EMPIRICAL STRATEGY AND APPARENT TRENDS

Most "Tiebout" elderly migration studies base their analyses on utility–maximizing behavior, but only Dresher (1994), Conway and Houtenville (1998, 2001) and Gale and Heath (2000) provide a formal theoretical model. Because our contribution lies primarily in our empirical methodology, we do not repeat this theoretical framework here, and instead only summarize it. Essentially, the elderly individual chooses the location i that maximizes his or her lifetime utility subject to a budget constraint and the economic costs associated with moving. Utility is assumed to be a function of amenities (nontraded goods associated with location i), publicly provided goods, and bequests, in addition to a composite consumption good. The individual's income is assumed to be invariant to location because he or she is retired; this income is allocated between saving for bequests, expenditures on private goods (which are influenced by cost–of–living) and tax payments. It follows that amenities and certain types of government spending that increase utility are desirable features of a location and, therefore, should increase in–migration and decrease out–migration, ceteris paribus. Similarly, taxes that are burdensome to the elderly and cost–of–living make a location less desirable, which decreases in–migration and increases out–migration.[12]

This theoretical framework provides the foundation for the empirical specification most often used with migration rate data:

[11] For instance, it seems possible that the presence of a large number of very high wealth elderly (those who owe federal taxes) might lead to political pressure to reduce state EIG tax rates.

[12] These models tend to ignore general equilibrium effects, whereby in–migration of some individuals raises the cost–of–living, and, in so doing, causes the out–migration of others, especially those with different preferences and resources. Likewise, some people may enjoy a hot climate while others enjoy a cold one. This highlights the need to limit one's analysis to a relatively homogeneous group, which we hope we are doing by focusing on the retired elderly. It also suggests an explanation for the "same sign" results frequently found in migration research.

$$[1] \quad M_{it} = \beta_o + \beta_{COL} COL_{it} + \beta_A A_{it} + \beta_G G_{it} + \beta_t T_{it} + \varepsilon_{it},$$

where M denotes the migration rate measure, COL denotes the cost–of–living variable (and its associated coefficient), A denotes amenities such as climate, G denotes government expenditures, T denotes the tax structure, and i indexes the 48 contiguous states. [13] Our study improves on past elderly migration literature by adding the time dimension, t, that indexes the four time periods from 1970 to 2000. The disturbance term, ε, is assumed to satisfy the normal conditions, but is allowed to have state–specific and year–specific components that are estimated as fixed effects in the models estimated with panel data. We consider three migration measures—the in–migration rate, the out–migration rate and the net in–migration rate. The in–migration rate, for instance, is constructed by dividing the number of elderly persons who move into the state during a period of time by the total population in the state.[14] The out–migration rate is constructed similarly. The net in–migration rate is the simple difference between in–migration and out–migration and best reflects the net movement of elderly individuals.

We consider in–migration, out–migration and net in–migration separately so that we can investigate whether the "same–sign" problem discussed earlier is present in our analysis. We also believe that the decision to move out of a state could be asymmetric with the decision to move into a state, which is missed if one focuses only on net in–migration. The data we use are from the 1970, 1980, 1990 and 2000 censuses and are constructed by comparing the reported residence five years prior to the census to the current residence. It, therefore, refers to migration over a five–year period—1965–70, 1975–80, 1985–90 and 1995–2000—and may miss some moves (e.g., individuals who move more than once during the five year period). This variable is the typical one used to investigate migration behavior, and to our knowledge we are the first to take advantage of the newly available 2000 census data.

This data allows us to investigate changes in migration behavior over a 30–year period, a period during which state EIG taxes were changing a great deal. One drawback to the data, however, is that it has more limited information than if one uses migration *flows*, as in Conway and Houtenville (2001, 2003), for example. Migration *flows* provide information about the individual's origin and destination, so that researchers can make a direct comparison between the characteristics of the sending and receiving states. Obtaining and using migration *flows* for this period would likely be preferable to using migration *rates.* However, using such flows has several drawbacks as well. First, the census migration flows (as used by Conway and Houtenville (2001, 2003) for 1990) are not currently publicly available for the important early years (1970 and 1980). While many past migration flow studies have used the PUMS, it is based on an at–best ten percent sample of the full population. Given the very low migration rates of the elderly (approximately five percent) and the persistence (as we demonstrate shortly) in migration patterns over time, we are concerned that using the PUMS to examine changes in

[13] As is a frequent practice in migration studies, we limit our analysis to the 48 contiguous states. We exclude D.C. because it does not have "state" fiscal policies, *per se.*

[14] Some might argue that we should use the elderly population as the base because it comes closer to approximating the probability of net movement. However, it also reflects past migration behaviors. For instance, a state that has enjoyed enormous elderly in–migration in the past might see its current in–migration rates decline, even if the number of elderly migrants remains unchanged. Nonetheless, as noted later, both our descriptive and econometric results are robust to the definition used.

flows over time would yield more noise due to sampling error than meaningful information. Even the census data is typically only based on ten to 15 percent of the population, such that the problem still exists, especially when flows are used.[15] An alternative strategy would be to use individual–level data, such as the PSID or the HRS, but this would likewise hamper our attempts to study the wide variation in EIG taxes over time.

We believe that the benefits of a wide time period with which to study the effects of EIG taxes outweigh the costs of using migration *rates* instead of flows or individual data. Foremost is the large variability of EIG taxes that existed in the beginning of the period and the dramatic way in which they have changed over time. Table 1 provides descriptive calculations that reveal how the "typical" elderly household was treated in terms of EIG taxes in 1962 versus 2000. To do so, we combine wealth information from the 1962 Survey of Financial Characteristics and the 2000 Census with EIG policy information from the World Tax Database.

For instance, Table 1 reveals that approximately 23 percent of elderly households had a net worth greater than zero but less than $50,000 in 2000 (summing the first three non–zero categories of net worth); these households would have owed EIG taxes in no state for a surviving spouse and only three states for an adult child. In contrast, such a level of net worth was equivalent to $10,874 or less in 1962, and this estate level would have been subject to taxation in at least eight states for a surviving spouse and 20 for an adult child.[16] Another way to examine this is to consider the impact on the median household in both years. In 2000, the median household had a net worth of between $100,000 and $499,999 and would have paid EIG taxes in only two states (assuming the beneficiary is a surviving spouse; 7 if it is an adult child). In 1962, the median household had a net worth of between $10,000 and $24,999 and would have paid EIG taxes in 34 states (39 if an adult child).

Still another interesting comparison is with the Federal estate tax. In the 1960s, estates of less than $60,000 were not subject to federal taxation, although as revealed in Table 1 the vast majority of states levied EIG taxes on these smaller estates. In 2000, the federal tax exempted estates of up to $675,000, and less than half of the 15 states with incremental EIG taxes levied EIG taxes at this level. This descriptive analysis, therefore, shows that state EIG taxes were much more widely used in the 1960s and reached further down into the wealth distribution than in the recent past. Thus, we believe there are very important advantages to including this early period in any study of the effects of EIG taxes on elderly migration.

Another advantage of considering this wide time period is that states that have historically been "importers" of the elderly are also the most likely to have eliminated their EIG taxes. This pattern makes cross–sectional evidence suspect; one must question the direction of causality. It would, therefore, be informative to look at the chronology of EIG tax elimination in combination with historical state migration patterns, as in Table 2. (The stepped line denotes the period before and after EIG elimination for each state.) Here we see that the five states that eliminated their incremental EIG taxes prior to 1960 (or never had them) are net–importers of

[15] Specifically, the 1970 census migration information is based on a 15 percent sample, 1980 is approximately 10 percent, 1990 is 15.5 percent and 2000 is 15.4 percent. These percentages apply to both migration rate and flow data, but flow data is likely to suffer more from sampling error because of its greater detail and specificity. We thank Marie Pees and Celia Boertlein of the U.S. Census for providing us with this information.

[16] The implicit price deflator (equal to 21.659 in 1962) is used to deflate the 2000 figures into their 1962 counterparts. The $100,000–499,999 range in 2000 translates into $21,569–107,845 in 1962.

TABLE 1
IMPACT OF STATE EIG TAXES BY WEALTH DISTRIBUTION OF THE ELDERLY

Net Worth in 2000	Percentage of 2000 population	Number of states EIG taxes paid in	As a percentage of states with EIG taxes	Net Worth in 1962	Percentage of 1962 Population	Number of states EIG taxes paid in	As a percentage of states with EIG taxes
EIG Taxes Using Exemption for Spouse when Applicable							
$0 or less	6.7	N/A	N/A	$0 or less	9.5	N/A	N/A
$1 to $9,999	9.3	0	0	$1 to $9,999	39.8	8	19
$10,000 to $24,999	5.2	0	0	$10,000 to $24,999	24.1	34	81
$25,000 to $49,999	8.4	0	0	$25,000 to $49,999	16.7	38	88
$50,000 to $99,999	17.3	0	0	$50,000 to $99,999	5.4	42	98
$100,000 to $499,999	41.1	2	13	$100,000 to $499,999	3.8	43	98
$500,000 and up	11.6	2	13	$500,000 and up	0.7	43	100
EIG Taxes Using Exemption for Adult Children when Applicable							
$0 or less	6.7	N/A	N/A	$0 or less	9.5	N/A	N/A
$1 to $9,999	9.3	0	0	$1 to $9,999	39.8	20	47
$10,000 to $24,999	5.2	1	7	$10,000 to $24,999	24.1	39	91
$25,000 to $49,999	8.4	3	20	$25,000 to $49,999	16.7	40	93
$50,000 to $99,999	17.3	4	27	$50,000 to $99,999	5.4	42	98
$100,000 to $499,999	41.1	7	47	$100,000 to $499,999	3.8	43	100
$500,000 and up	11.6	8	53	$500,000 and up	0.7	43	100

Notes:
Fifteen states have EIG taxes in 2000; 43 states have EIG taxes in 1964.
Households face federal taxation at $675,000 in 2000 and $60,000 in 1962.
For the top net worth bracket, the percentage of states is based on the lowest end of the bracket.
The EIG tax data are from the World Tax Database.
The 1962 wealth data are from the Survey of Financial Characteristics.
The 2000 wealth data are from the US Census Wealth and Asset Ownership Data.

TABLE 2
CHRONOLOGY OF EIG TAX ELIMINATION AND STATE NET INMIGRATION RATES

State	Year of Elimination	NET INMIGRATION RATES			
		1970	1980	1990	2000
Alabama	prior to 1960	0.1489	1.3252	0.7504	0.5491
Arkansas	prior to 1960	1.4779	3.4061	1.5595	0.6876
Florida	prior to 1960	22.1611	18.2154	12.0076	5.8125
Georgia	prior to 1960	2.0533	1.7435	1.7435	1.9614
Nevada	prior to 1960	6.3478	18.4415	20.4874	13.5299
New Mexico	1976	1.9700	4.7070	2.4274	1.3812
Utah	1977	*–0.2779*	2.8750	1.7703	1.2402
North Dakota	1979	*–3.1500*	*–2.0319*	*–0.8943*	*–1.6447*
Arizona (a)	1980	16.9695	19.6322	11.1685	9.7511
Colorado	1980	1.2406	1.5730	1.5521	0.5418
Vermont	1980	0.3644	0.7275	1.0381	0.0271
Virginia	1980	0.4472	1.2363	1.1042	0.9568
Missouri	1981	*–0.2922*	*–0.3487*	*–0.1493*	0.0689
Washington	1982	0.5705	3.1003	3.0986	0.1893
California	1982	1.8818	2.2890	0.5287	*–1.0209*
Illinois	1983	*–3.9065*	*–4.3055*	*–2.7617*	*–2.9115*
Wyoming	1983	*–2.2207*	*–4.1656*	*–2.2952*	*–0.0558*
Texas	1983	1.0245	2.5802	1.2530	0.9613
West Virginia	1985	*–1.8258*	*–1.5359*	*–0.4263*	*–0.3325*
Minnesota	1986	*–1.7633*	*–1.1618*	*–0.0706*	*–1.0710*
Maine	1986	*–0.0937*	0.2631	0.0053	0.9593
Oregon	1987	2.6335	3.2199	4.7253	0.3175
Idaho	1988	0.3000	1.7671	0.5898	2.1174
Rhode Island	1991	*–1.1253*	*–1.2559*	*–1.0043*	*–0.4826*
South Carolina	1992	0.8325	3.1922	3.5088	3.6230
Wisconsin	1992	*–0.4191*	*–1.0604*	*–0.6098*	*–0.5792*
Michigan	1993	*–3.3083*	*–3.4912*	*–2.5418*	*–1.8617*
Massachusetts	1997	*–1.8688*	*–1.8595*	*–2.1391*	*–1.7001*
Kansas (b)	1998	*–0.8008*	*–0.5815*	*–1.2746*	*–0.1229*
Delaware	1999	1.1231	3.0104	2.0597	3.0101
North Carolina	1999	0.6407	2.8205	3.5965	2.3667
Mississippi	2000	*–0.5910*	1.0923	0.5327	0.7328
New York	2000	*–4.7342*	*–6.1649*	*–4.8293*	*–4.7710*
Montana	2001	*–1.8045*	*–1.3712*	*–0.5490*	0.7816
South Dakota	2001	*–1.6974*	*–1.6524*	*–0.6271*	*–0.2343*
New Hampshire	2003	1.8904	1.9488	1.2746	0.5294
Louisiana	2004	0.1409	*–0.1053*	*–0.7163*	*–0.5004*
Connecticut	2005	*–1.0347*	*–1.6828*	*–3.2430*	*–2.0415*
Indiana	still	*–1.4910*	*–1.5454*	*–0.8931*	*–0.8604*
Iowa	still	*–0.9434*	*–1.9447*	*–0.9522*	*–1.1275*
Kentucky	still	*–0.6132*	*–0.1643*	0.0580	*–0.2857*
Maryland	still	2.0459	*–1.1790*	*–0.4978*	*–0.7836*
Nebraska	still	*–1.0391*	*–0.8031*	*–0.2366*	*–0.8213*
New Jersey	still	*–0.5141*	*–2.4368*	*–3.1867*	*–2.1795*
Ohio	still	*–1.6693*	*–2.7179*	*–1.2773*	*–1.2467*
Oklahoma	still	0.9366	0.9003	0.4160	0.2424
Pennsylvania	still	*–1.4292*	*–1.7746*	*–0.8221*	*–0.8282*
Tennessee	still	0.5586	1.4196	1.1618	1.5932

(a) Arizona's tax policy was constructed in such a way so that although its incremental EIG taxes were formally eliminated in 1980, there was no incremental EIG tax burden except between 1977–1979.
(b) Kansas re–enacted a succession tax in 2002, which is an inheritance tax exempting lineal ancestors and descendants, spouses, and siblings.

the elderly in every census we consider. The first state to eliminate its incremental EIG tax after 1975—New Mexico—at first looks to have "benefited" as its net in–migration rate increased; however, by 2000, its rate has fallen below its 1970 level. Of the other early eliminators, Arizona, Colorado, Vermont, and Washington behave similarly, although almost all of them experienced their biggest increases *just prior* to eliminating their incremental EIG taxes. California and Texas actually saw decreases after eliminating their EIG taxes that are continuing in the most recent census. On the other hand, Utah, North Dakota, Missouri, Illinois and Wyoming experienced "improvements" immediately after eliminating their incremental EIG taxes that have continued to the present.[17]

Of course, this analysis is incomplete because one must consider other factors that might change a state's desirability and must also compare states to one another. What is apparent, however, from this table is that 1) states that eliminated their incremental EIG taxes early also tend to be historic net–importers of the elderly, and 2) elderly migration patterns are fairly stable over time. These two tendencies may lead to a spurious correlation between state EIG taxes and state net in–migration in cross–sectional data. Correlation between these two variables is certainly evident in the data. First, the average net in–migration rate during our sample period for states with no incremental EIG tax is +2.96, compared to a net in–migration rate of –0.04 for those with one. This difference in means remains if one looks at each census year individually, although there is evidence that these differences are closing over time.[18] In addition, the correlation coefficients we calculate between our continuous measures of EIG tax burdens (discussed shortly) and our migration variables are likewise negative and statistically significant. However, if one considers the changes in these variables over time, all significant correlation disappears.[19] It is also noteworthy that the migration of the young is likewise negatively correlated with these EIG measures, although less significantly so.

EIG tax policy has changed dramatically over the last 30 to 40 years, so it is important to use data that allows us to study this period. In this way, we can discover whether elderly migration seems to have responded to these changes in policy. A final advantage of using migration *rate* data is that, because it is measured in the same observational units as the EIG policies (one measure per state/year), it facilitates our final exercise of exploring the possibility of reverse causality—whether elderly migration patterns might be influencing state EIG policy. Having the two key variables measured in the same unit of observation allows us to treat them symmetrically.

DESCRIPTION OF THE DATA

As discussed above, we consider three migration variables—the in–migration rate, out–migration rate and net in–migration rate—for two age groups—the elderly (aged 65 and over) and, our "pseudo–control" group, the young (aged 25 to 44). Each migration rate is constructed by dividing the number of movers by the total population (in thousands) in the state the year before. It, therefore, measures the number of migrants over a five–year period per 1,000 state residents. We use

[17] Because this is a descriptive analysis and, thus, we have less concern about endogeneity, we report the results defining elderly migration as a percent of the elderly population; however, using total population did not have an appreciable effect. See footnote 15 for more discussion.

[18] Specifically, in 1970 the average net in–migration rate for no incremental EIG states was 6.08 compared to –0.071 for EIG states. In 1980, the corresponding numbers are 8.69 vs. 0.302; in 1990, 3.02 vs. –0.265; and in 2000, they are barely different at 1.28 and –0.393.

[19] Conway and Houtenville (2001) consider a related issue—how state policy variables in general are correlated with elderly migration at a point in time. They find that almost all of the state policy variables they consider have negative, pairwise correlations with net in–migration, which they argue underscores the importance of fully specifying the public sector in migration models.

the total population both to be consistent across our young and elderly migration measures and, more importantly, because we want to avoid having our migration measures reflect past migration decisions (see footnote 15). We observe these measures at four points during a 30–year period—1965–70, 1975–80, 1985–90 and 1995–2000.[20] Because our dependent variables refer to a five–year period, we use explanatory variables that refer to the prior year (e.g., 1964 values for migration during 1965–70) whenever possible. This helps eliminate possible endogeneity if public sector variables are determined by migration patterns (an issue raised by Cebula (1979) and one we discuss further in the sixth section). In addition, all migrants, even those who migrated in the first year of the period, have access to the information in the prior year—and they cannot have directly affected policies that are in place before they arrive.[21] Another complexity that arises is the likely heteroskedasticity that comes from low population states having more variable migration rates than high population ones; for this reason, we weight all of the regressions by the state's population. We also calculate robust estimated standard errors that allow for general heteroskedasiticity and for possible clustering of the errors by state.

Our key explanatory variable is the state's EIG tax policy. We, therefore, try several alternative specifications in our attempts to capture its influence. Our first measure is a dummy variable for whether the state has an *incremental* EIG tax or not (i.e., does not rely *only* on the "pick–up" tax). This captures the discrete effect of the mere presence of a state EIG tax. However, as noted by Conway and Rork (2004), many states reduced their incremental EIG taxes during this period even though they did not eliminate them. There is also a great deal of variability in state EIG taxes. For example, in 1964 California had a very low exemption ($5,000) after which it began taxing the inheritance at a rate of 0.02. Its top bracket began at an inheritance of $500,000 and its top tax rate was 0.10. In contrast, Kansas did not begin taxing inheritances until they exceeded $75,000, after which it taxed at a rate of 0.01. Its top bracket was the same as California's ($500,000), but its top tax rate was only half as large (0.05). The highest marginal tax rate belonged to North Dakota at 0.23 on estates greater than $1.5 million in 1964. In contrast, New Mexico taxed all inheritances above $10,000 at a constant rate of 0.01. Less than 20 years later, three of these four states (New Mexico, North Dakota and California) had completely eliminated their incremental taxes. However, even with widespread elimination, real differences among EIG tax rates and brackets persist. In 1994, the last year of our analysis, the top marginal tax rate ranged from 0.01 in Maryland and Nebraska (both of which have flat rates) to 0.21 in New York (on estates in excess of $10.1 million).

Clearly, then, there is real variability in those states that have EIG taxes that is missed by the dummy variable. We, therefore, consider three alternative measures. The first is the simple share of total state tax revenue that is accounted for by

[20] We are prevented from using migration data from even earlier censuses by the lack of comparable state fiscal policy information.

[21] This is a common practice in elderly migration studies (e.g., Conway and Houtenville (1998, 2001 and 2003)). Some studies, such as Serow, Charity, Fournier, and Rasmussen (1986), use the data in the first year of the period—i.e., 1965 data for migration that took place between 1965–70. Gale and Heath (2000) also use the prior year, but augment their analysis with the *change* in the variables during the migration period; they then treat one of these change variables as endogenous (using political variables as instruments), but do not instrument the others.

incremental state EIG tax revenues.[22] The second is an aggregate average tax rate, constructed by dividing these incremental revenues by the aggregate value of gross estates reported on federal estate tax returns.[23] The value of gross estates reported (as well as the variable used to calculate the *incremental* tax revenue) is only available for a limited number of years. However, we have been able to locate it for 1963, 1973, 1983 and 1993, so the measures match reasonably well with our migration data. The third alternative measure is the effective average state EIG tax rate on a $1 million (in constant 1996 dollars) bequest divided equally between two adult children, reported by Bakija and Slemrod (2004, Table 2).[24] These rates are reported every five years from 1965 to 2000; we, therefore, use 1965, 1975, 1985 and 1995.

These four measures should behave similarly, but they do capture slightly different aspects of a state's EIG tax. The tax share captures its relative importance within the state's budget. The aggregate average rate will be larger for states that either levy large incremental EIG taxes on large estates or levy EIG taxes on estates that are too small to be subject to federal estate taxation. Recall that this occurs because the "tax base" is the value of gross estates that paid *federal* estate taxes, and many smaller estates are not subject to federal taxation. Still, it should help adjust for the fact that incremental EIG revenues are higher in states with a greater number and size of estates. Bakija and Slemrod's tax rate is a more precise measure of the average tax rate, but is only strictly valid for estates of a specific (large) size.

None of these measures captures the progressivity of the EIG tax. We explore whether this might be important by including the state's EIG exemption, that is, the smallest estate (received by an adult child) that is subject to state EIG taxation. All things equal, elderly residents should find a low exemption undesirable—it is more likely their estate is subject to taxation. However, this variable is problematic to use because many states do not tax *any* estate. One solution is to include the *incremental* EIG tax dummy and interact it with this variable. A second solution is to assign an arbitrarily large value to the exemption for states without EIG taxes.[25] Due to the many different ways we try to use this variable, we do not report it explicitly, but rather summarize these exercises.

Table 3 reports the means and definitions of these variables, as well as the other explanatory variables we include. For the other variables, we emphasize variables that we could gather in a comparable way over the entire time period. Cost–of–living indices by state are sparse during this period and nonexistent dur-

[22] We calculate the *incremental* EIG tax revenues by subtracting the amount of the state tax credit (which should equal the state's "pickup" revenues) reported by the IRS' Statistics of Income. Because the timing of the data does not match exactly (one refers to fiscal year and the other to calendar year), we subtract this credit from the two year average of the state EIG tax revenue data. Nebraska is an important exception because its "pickup" revenues are allocated to local governments first and are not typically included in its state EIG revenue figures; no subtraction is, therefore, required. For states without an incremental EIG tax, this incremental revenue is set equal to zero since it is not always *exactly* equal to zero via this calculation. We thank Joel Michael for suggesting the state tax credit data to us. We also re–estimate the models without subtracting out these revenues and the results are similar.

[23] This information can be found in the *Compendium of Federal Estate Tax and Personal Wealth Studies*, and in various issues of the *Statistics of Income Bulletin,* published by the IRS Statistics of Income.

[24] We thank Jon Bakija for giving us permission to use this data.

[25] We try, alternately, using three, five and ten times the maximum exemption given in each year, and also three, five and ten times the maximum given in the last year (1994) of the sample and then using the inflation–adjusted amount in the previous years.

TABLE 3
MEANS AND SOURCES OF VARIABLES

Variable Name	Source	Mean	Standard Deviation
1. Amenities and Cost of living			
heating days	Statistical Abstract of the United States	5,118.760	2,065.210
median house value	Calculated from the PUMS	58,206.930	25,968.110
average mfg wage	Statistical Abstract of the United States	11.389	1.740
state unemployment rate	Statistical Abstract of the United States	5.832	1.756
% population 65 or over	Statistical Abstract of the United States	0.111	0.022
crime rate (per 100,000)	Statistical Abstract of the United States	3,771.161	1,901.079
2. Government Expenditures			
health & hospitals	State Government Finances (b)	0.217	0.129
education expenditures	State Government Finances	0.994	0.330
welfare expenditures	State Government Finances	0.343	0.265
all other expenditures	State Government Finances	1.414	0.716
3. Other Tax Variables			
property tax share	State Government Finances	0.205	0.094
sales tax rate	World Tax Data Base	3.653	1.805
average income tax rate	State Government Finances (a)	1.397	1.047
other per capita tax share	State Government Finances	0.159	0.083
4. EIG Tax Measures			
incremental EIG tax (YES=1)	Authors' Calculations	0.738	0.441
average EIG tax rate	IRS Statistics of Income (c)	0.026	0.028
EIG tax share	State Tax Collections (b)	0.006	0.007
Bakija & Slemrod tax rate	Bakija and Slemrod (2004)	1.288	1.535
5. Migration Variables			
elderly inmigration rate	Mobility of States and Nation (US DOC)	5.869	5.439
elderly outmigration rate	Mobility of States and Nation (US DOC)	5.052	2.127
elderly netinmigration rate	Mobility of States and Nation (US DOC)	0.816	4.218
elderly total migration rate	Mobility of States and Nation (US DOC)	10.922	7.100
young inmigration rate	Mobility of States and Nation (US DOC)	48.196	2.434
young outmigration rate	Mobility of States and Nation (US DOC)	43.076	14.135
young netinmigration rate	Mobility of States and Nation (US DOC)	5.121	18.077
young total migration rate	Mobility of States and Nation (US DOC)	91.272	35.458
6. EIG Tax Equation Variables			
per capita state debt	State Government Finances	964.564	1,006.112
per capita federal transfers	State Government Finances	420.216	288.801
per capita income	Statistical Abstract of the United States	15,898.190	4,777.270
% population over 85	Statistical Abstract of the United States	0.011	0.004
same party—democrat	Statistical Abstract of the United States	0.319	0.470
same party—republican	Statistical Abstract of the United States	0.188	0.392
election year dummy	Statistical Abstract of the United States	0.556	0.499

Notes:
All expenditures are for state & local governments combined and are measured in per–capita (thousands of dollars) amounts.
All taxes are measured as shares of total state tax revenue unless otherwise indicated.
All dollar values are in real 2000 dollars, converted using GDP deflator.
(a) Includes additional calculations by authors.
(b) For 1963, is known as Governmental Finances.
(c) From issues of Statistics of Income Bulletin and the *Compendium of Federal Estate Tax and Personal Wealth Studies.*

ing the early part of it. We, therefore, use the median house value, calculated using the PUMS from each census year, as our proxy for cost of living in each state. Because we estimate migration equations for the young as a control group, we also include the average manufacturing wage. For young migrants, this is a key variable reflecting labor market potential; for the elderly, it is likely capturing the cost of living. A similar logic (but with reverse effects) applies to the state’s unemployment rate. Our other amenity variables (*A*) are heating degree days (to capture climate), the crime rate and the percentage of the population who are elderly. Our government expenditure variables are measured as state and local expenditures per capita and are broken into several categories—1) health and hospitals, 2) education, 3) welfare programs, and 4) all other. The general wisdom is that the elderly value spending on health and hospitals, and do not attach much value to education and welfare expenditures. Many of the early migration studies focus on welfare expenditures as the key variable, and most studies that include it find some evidence that it discourages elderly migration.

In addition to EIG taxes, we must also control for other forms of state taxation. Conway and Houtenville (1998) use tax shares, arguing that they approximate the relative “price” of one dollar of government expenditures and that some taxes are less burdensome than others. However, in their later paper (Conway and Houtenville, 2001) they discover that personal income taxes are not well captured with this measure and suggest using a combination of marginal tax rate and tax bill variables, both interacted with the amount of pension income that is exempt from taxation. Obtaining this information for all of the years of our analysis is quite difficult and, again, our need for parsimony requires that we limit the number of variables. In addition, our focal point here is on EIG taxes, not income taxes. With sales taxes and personal income taxes, we, therefore, experiment with using revenue shares (as in our first average EIG burden measure) or average pseudo tax rates (as in our second) and find that it makes little difference.[26] The property tax rate obviously varies within a state, so we use a property tax share measure to capture its approximate burden within the tax system.

EMPIRICAL RESULTS

We estimate several specifications of equation [1] in order to investigate these issues. Our main purpose here is to see how bringing the passage of time and a “pseudo–control” group into our empirical analysis affects our results. We also want to explore the sensitivity of our results to alternative EIG tax measures. Finally, we want to verify that our results are not sensitive to (or diluted by) the other variables we include. For instance, one might argue that if lowering EIG taxes attracts the elderly and stimulates the economy, then this would show up in a lower unemployment rate (and perhaps higher wages). We, therefore, estimate the model five ways—starting with only the EIG measure (*simple*), then adding alternately amenities, the expenditure variables, the other tax variables and then all of the variables.

Cross–Sectional Comparisons

We begin by estimating our models using only cross–sectional data, so that we can establish a baseline for comparison and see whether we find the “same–sign” problem in our data that so many others have found in this setting. Estimating the model for each census year also allows us to see if the relationship is apparently stable. These results are summarized for the different EIG coefficients in Appendix Table A1 and the full model’s results are summarized for one EIG measure, *incremental EIG tax*, in Appendix Table A2. For the sake of brevity, we only report

[26] The “average tax rate” for the personal income tax is calculated by dividing total personal income tax revenues by total personal income for the state.

the results from two years—1980 and 1990. We choose these two years because the majority of previous studies have used data from these years. (All results discussed but not reported are available upon request.)

Several patterns are immediately apparent from this exercise. The 1980 results strongly suggest that high EIG taxes discourage elderly migration, whereas the 1990 results suggest little impact. This trend holds more generally; results for the early years (1970 and 1980) are much more suggestive of an impact than those for the later years. This is not too surprising when one recognizes that the variability and presence of state EIG tax policy has declined steeply during this period. Nonetheless, in all the models we estimate, the EIG coefficients are overwhelmingly negative, especially from the model specifications that contain fewer variables. For instance, out of the 80 in–migration models we estimate (4 years × 4 EIG measures × 5 model specifications), 72 of the 80 EIG coefficients are negative and 32 are statistically significant. This exercise, therefore, confirms that relying on cross–sectional data, especially in an early census year or with a limited number of covariates, is likely to produce evidence that high EIG taxes discourage elderly migration.

The "same–sign" problem is also evident. The vast majority of the EIG coefficients are the same sign for in–migration as for out–migration. The same holds true for several other variables, most notably crime. The crime rate displays the same puzzling result that has plagued past elderly migration studies in that it *positively* affects in–migration and net in–migration, as well as out–migration (see, for example, Serow et al. (1986), Fournier et al. (1988), and Conway and Houtenville (2001, 2003)). The manufacturing wage also has a consistent effect (negative) across all three migration measures.

Panel Specification Results

The last three columns of Appendix Tables A1 and A2 report the coefficient estimates from simply pooling the data. We see the predictable dilution of the negative EIG coefficients as the less significant later years mix with the more significant early years. The EIG coefficients continue, however, to be overwhelmingly negative and frequently statistically significant.[27] In terms of the other variables, the "same–sign" problem is just as evident as before. Thus, simply pooling the data does not change the salient results.

More importantly, estimating equation [1] using panel data allows us to include state and time fixed effects that may capture underlying factors associated with the desirability of the state and overall time trends. Note that we eliminate our climate variable from this specification as it has no meaningful variability over time. In fact, climate, shoreline, lakes, mountains and any natural amenities should be captured in our state fixed effects. Table 4 summarizes the EIG coefficients from the 20 different specifications (4 EIG measures × 5 specifications) for the elderly, the young and the difference between the two. Again, we estimate in–migration, out–migration and net in–migration separately to look for the "same–sign" problem, and we estimate the model for our "pseudo–control" group, the young.

It is immediately apparent that EIG taxes lose their importance to migration behavior. The vast majority of EIG coefficients are nowhere close to statistically significant, and the few that are tend to be of the wrong sign (e.g., a high EIG tax share appears to discourage out–migra-

[27] For instance, all 20 of the in–migration coefficients estimated (4 EIG measures × 5 specifications) are negative and seven are statistically significant. The out–migration and net–migration coefficients behave similarly, although they are slightly less likely to be negative or statistically significant.

TABLE 4
PANEL ESTIMATION RESULTS FOR ELDERLY (65+), YOUNG (25–44) AND DIFFERENTIAL MIGRATION

	Elderly			Young			Elderly–Young		
	In	Out	Net	In	Out	Net	In	Out	Net
Incremental EIG Tax (Yes=1)									
simple	–0.390	–0.180	–0.210	0.454	0.076	0.379	–0.844	–0.256	–0.589
	[–0.40]	[–0.57]	[–0.19]	[1.49]	[0.37]	[1.06]	[–0.98]	[–0.65]	[–0.54]
with amenities	–0.085	0.049	–0.134	0.127	0.204	–0.076	–0.212	–0.155	–0.058
	[–0.17]	[0.17]	[–0.22]	[0.40]	[1.03]	[–0.20]	[–0.55]	[–0.35]	[–0.08]
with expenditures	–0.894	–0.174	–0.720	0.376	–0.059	0.435	–1.271	–0.115	–1.156
	[–0.76]	[–0.58]	[–0.57]	[1.08]	[–0.41]	[1.04]	[–1.21]	[–0.33]	[–0.95]
with taxes	–0.764	–0.216	–0.549	0.425	0.05	0.375	–1.189	–0.265	–0.924
	[–0.69]	[–0.61]	[–0.45]	[1.42]	[0.27]	[1.03]	[–1.14]	[–0.63]	[–0.73]
full	–0.588	0.034	–0.622	–0.005	0.087	–0.092	–0.583	–0.053	–0.529
	[–0.77]	[0.12]	[–0.74]	[–0.02]	[0.55]	[–0.24]	[–0.91]	[–0.14]	[–0.62]
EIG Tax Share									
simple	–5.300	–14.924**	9.624	7.030	–4.142	11.172**	–12.330	–10.782**	–1.548
	[–0.22]	[–3.52]	[0.38]	[1.36]	[–1.32]	[2.77]	[–0.53]	[–1.88]	[–0.06]
with amenitites	–10.208	–15.235**	5.027	1.325	–2.527	3.852	–11.534	–12.708**	1.175
	[–0.75]	[–3.14]	[0.33]	[0.21]	[–1.12]	[0.53]	[–1.04]	[–2.02]	[0.09]
with expenditures	–13.348	–21.607**	8.259	3.433	–2.513	5.946	–16.781	–19.094**	2.313
	[–0.51]	[–5.73]	[0.32]	[0.65]	[–1.00]	[1.18]	[–0.67]	[–4.37]	[0.09]
with taxes	–8.427	–14.462**	6.036	5.209	–5.343	10.552	–13.636	–9.120	–4.516
	[–0.42]	[–2.69]	[0.30]	[0.97]	[–1.56]	[1.51]	[–0.71]	[–1.41]	[–0.21]
full	–17.895	–17.693**	–0.202	5.649	–2.567	8.216	–23.544	–15.126**	–8.417
	[–1.02]	[–3.74]	[–0.01]	[0.75]	[–0.98]	[0.99]	[–1.60]	[–2.67]	[–0.52]
Average EIG Tax Rate									
simple	7.463	–3.748	11.211	5.372	1.837	3.535	2.090	–5.585	7.676
	[1.51]	[–1.26]	[1.59]	[1.62]	[0.82]	[0.97]	[0.45]	[–1.33]	[0.98]
with amenities	10.719*	1.398	9.321	1.513	5.393**	–3.880	9.206	–3.995	13.201
	[1.70]	[0.41]	[1.23]	[0.46]	[2.51]	[–1.04]	[1.51]	[–0.81]	[1.54]
with expenditures	1.487	–2.947	4.434	2.831	0.828	2.004	–1.345	–3.775	2.430
	[0.22]	[–0.84]	[0.64]	[0.65]	[0.46]	[0.38]	[–0.22]	[–0.91]	[0.30]
with taxes	4.977	–3.918	8.895	4.527	1.587	2.94	0.450	–5.505	5.956
	[1.00]	[–1.14]	[1.23]	[1.27]	[0.83]	[0.70]	[0.08]	[–1.24]	[0.67]
full	7.087	1.969	5.118	–0.051	4.128**	–4.179	7.138	–2.159	9.297
	[1.13]	[0.53]	[0.81]	[–0.01]	[2.35]	[–1.01]	[1.16]	[–0.51]	[1.24]
Bakija & Slemrod Tax Rate									
simple	–0.040	–0.052	0.012	0.099	0.025	0.074	–0.139	–0.077	–0.062
	[–0.17]	[–0.82]	[0.05]	[1.17]	[0.58]	[1.29]	[–0.74]	[–0.91]	[–0.25]
with amenities	–0.080	–0.018	–0.062	0.008	0.044	–0.035	–0.088	–0.062	–0.026
	[–0.48]	[–0.30]	[–0.32]	[0.11]	[0.97]	[–0.46]	[–0.66]	[–0.66]	[–0.13]
with expenditures	–0.138	–0.035	–0.103	0.053	0.016	0.036	–0.190	–0.051	–0.139
	[–0.53]	[–0.48]	[–0.39]	[0.59]	[0.48]	[0.45]	[–0.86]	[–0.64]	[–0.55]
with taxes	–0.079	–0.070	–0.009	0.088	0.016	0.072	–0.167	–0.087	–0.081
	[–0.34]	[–1.04]	[–0.04]	[1.08]	[0.48]	[1.08]	[–0.87]	[–1.06]	[–0.33]
full	–0.107	–0.024	–0.083	–0.025	0.035	–0.060	–0.082	–0.059	–0.023
	[–0.58]	[–0.42]	[–0.41]	[–0.30]	[1.16]	[–0.75]	[–0.55]	[–0.87]	[–0.12]

Notes:
Standard errors adjusted for clustering and general heteroskedasticity; t–statistics reported in brackets. **significant at 5% level. *significant at 10% level.
State and year fixed effects included; all regressions weighted by state population.
All explanatory variables listed in Table 5 are included in groups as noted.

tion). Not only has the statistical significance diminished, but the size of the predicted effects has fallen dramatically as well. For instance, using 1980 cross–sectional data and the full model suggests that having an incremental tax reduces a state's net in–migration by more than five elderly individuals per 1,000 state residents (where the average rate is approximately one individual), with a 95 percent confidence interval ranging from 1.36 to 9.2 individuals. That same model estimated with panel data and fixed effects predicts a (statistically insignificant) reduction of 0.622 elderly individuals with a 95 percent confidence interval ranging from a decrease of 2.26 individuals to an *increase* of 1.03 individuals. Looking at the differential effects on elderly migration (the last three columns of Table 4, under "Elderly–Young") tends to weaken the results even further. In the next section we check the robustness of these results, but first we consider the other variables.

So what *does* affect elderly migration? Turning to Table 5, which reports the coefficients from the full model and the incremental EIG tax dummy, we see that crime is important—and that the persistent, counterintuitive result is eliminated within this framework.[28] Crime encourages out–migration among both age groups, but it especially discourages the *differential* in–migration and net in–migration of the elderly. In other words, crime is a disamenity for both groups and acts to drive them out of a location, but it has a stronger effect on the elderly. The elderly are also discouraged by a high cost of living (as approximated by median house value); this result occurs both in the elderly migration and differential migration models. High manufacturing wages and high unemployment rates also tend to attract the elderly relatively more than the young.[29] Interestingly, the elderly are relatively less likely to move to states with a larger elderly population, as the young appear to be drawn to these states. This could be due to the phenomena of "return migration," whereby the elderly eventually return to their "home" states for assistance and to be closer to family or to the economic stimulus caused by the "demand–side" effect of having a large elderly population. When we examine the results from separate regressions for each of the two groups, we find that young in–migration is more significantly encouraged by an older population than elderly in–migration is discouraged and that an older population discourages the out–migration of both groups. These results, therefore, give more weight to the latter (demand–side) explanation.

Very few fiscal policy variables appear to matter. Expenditures on health and hospitals is a rare occurrence of the "same sign" problem, so we hesitate to conclude much from it; in addition, it does not have a significant effect on net in–migration. Perhaps surprisingly, the elderly appear relatively more drawn to states with high education spending, mostly due to the fact that the young appear repelled by it. We suspect that this counterintuitive result is due mostly to difficulties in measuring education spending at

[28] The other EIG measures produce similar results. Dropping entire sets of variables sometimes has an impact, typically in the predictable direction of making the remaining coefficients more statistically significant. The tax variables' coefficients, however, are rarely, if ever, statistically significant.

[29] One would expect these two variables, along with cost–of–living, to be highly correlated as low unemployment rates and high cost–of–living should put upward pressure on wages. Unemployment and cost–of–living have the expected effects on both elderly and differential elderly migration. Conversely, the effect for manufacturing wages is counterintuitive; we suspect that this result is due to the high collinearity between these variables (it is not clear what the wage is capturing once you control for unemployment and cost–of–living) and note that it has only a marginal effect on only one aspect of elderly migration (in–migration).

TABLE 5
FULL PANEL ESTIMATION RESULTS FOR ELDERLY (65+), YOUNG (25–44) AND DIFFERENTIAL MIGRATION, USING THE INCREMENTAL EIG DUMMY

	Elderly			Young			Elderly–Young		
	In	Out	Net	In	Out	Net	In	Out	Net
Amenities									
median house value	–0.00002**	0.00000346	–0.00002**	–3.36E–06	2.21E–06	–5.58E–06	–0.00001**	1.25E–06	–0.00001
	[–2.33]	[0.80]	[–2.65]	[–0.69]	[0.90]	[–0.96]	[–2.02]	[0.21]	[–1.58]
average mfg wage	0.505*	0.064	0.440	0.006	0.079	–0.073	0.499**	–0.014	0.513**
	[1.69]	[0.70]	[1.47]	[0.04]	[1.50]	[–0.39]	[2.26]	[–0.17]	[2.17]
state unemployment rate	–0.047	–0.166**	0.119	–0.276**	–0.078*	–0.198**	0.228**	–0.089	0.317**
	[–0.40]	[–3.02]	[0.92]	[–3.68]	[–1.91]	[–2.12]	[2.34]	[–1.09]	[2.15]
% population 65 or over	–59.409	–23.774**	–.35.636	37.174**	–16.355**	53.528**	–96.583**	–7.419	–89.164**
	[–1.54]	[–2.18]	[–0.95]	[2.42]	[–2.80]	[3.10]	[–3.04]	[–0.66]	[–2.76]
crime rate	–0.0007	0.0003**	–0.001*	0.0002	0.0001*	7.28E–06	–0.0008**	0.0002	–0.001**
	[–1.27]	[2.91]	[–1.84]	[0.68]	[1.82]	[0.03]	[–2.10]	[1.05]	[–2.20]
Expenditures									
expenditures on health & hospitals	3.927	2.450**	1.477	–0.288	0.233	–0.522	4.216*	2.217*	1.999
	[1.30]	[2.06]	[0.45]	[–0.22]	[0.38]	[–0.37]	[1.70]	[1.69]	[0.67]
expenditures on education	2.793	–0.061	2.853	–0.119	1.488**	–1.607	2.912	–1.549**	4.460*
	[1.06]	[–0.13]	[1.02]	[–0.11]	[4.12]	[–1.40]	[1.43]	[–2.36]	[1.86]
expenditures on welfare	1.816	–0.178	1.994	1.188	0.254	0.934	0.628	–0.432	1.060
	[0.92]	[–0.35]	[1.15]	[0.95]	[0.90]	[0.66]	[0.56]	[–0.66]	[0.90]
all other expenditures	–0.916	–0.417	–0.500	–0.593	0.311*	–0.904	–0.323	–0.728*	0.404
	[–0.91]	[–1.54]	[–0.49]	[–1.04]	[1.75]	[–1.43]	[–0.57]	[–1.84]	[0.60]
Taxes									
property tax share	1.709	0.061	1.648	0.142	0.966	–0.824	1.567	–0.905	2.472
	[0.29]	[0.03]	[0.26]	[0.05]	[0.85]	[–0.26]	[0.31]	[–0.36]	[0.37]
sales tax rate	–0.077	0.170	–0.246	0.072	–0.015	0.087	–0.148	0.185	–0.333
	[–0.42]	[1.29]	[–1.15]	[0.66]	[–0.26]	[0.61]	[–0.86]	[1.41]	[–1.57]
average income tax rate	63.556	2.083	61.473	6.952	–10.499	17.451	56.604	12.581	44.023
	[0.94]	[0.13]	[0.90]	[0.25]	[–1.29]	[0.61]	[1.11]	[0.78]	[0.81]
other per capita tax share	5.970	–1.051	7.022	2.068	–0.473	2.541	3.902	–0.579	4.481
	[0.74]	[–0.52]	[0.80]	[0.86]	[–0.38]	[0.90]	[0.55]	[–0.26]	[0.53]
incremental EIG tax	–0.588	0.034	–0.622	–0.005	0.087	–0.092	–0.583	–0.053	–0.529
(Yes=1)	[–0.77]	[0.12]	[–0.74]	[–0.02]	[0.55]	[–0.24]	[–0.91]	[–0.14]	[–0.62]
constant	3.818	5.999**	–2.180	0.823	2.528**	–1.705	2.995	3.470**	–0.475
	[0.92]	[4.31]	[–0.49]	[0.36]	[3.65]	[–0.72]	[1.04]	[2.47]	[–0.15]

Notes: Standard errors adjusted for clustering and general heteroskedasticity; t–statistics reported in brackets. **significant at 5% level. *significant at 10% level.
State and year fixed effects included; all regressions weighted by state population.

the state level. First, as noted by others (e.g., Conway and Houtenville (2001)), it (more than any other expenditure) likely suffers from aggregation bias as it can vary a great deal within a state. It also may be capturing demographics. States with higher per capita education spending are likely states with disproportionately young populations (both children and their parents, the latter of whom are likely in our "Young" aged 25–44 group, plus college students).[30] Moreover, such states likely experience a greater number of young out–migrants simply because there is a larger pool "at risk" of moving. This confounding influence could, therefore, cause education expenditures to appear to increase the out–migration of the young. Again, examining separate regressions for each of the two groups reveals that this is precisely the force underlying the apparent relative attractiveness of education to the elderly. For both this reason and the likely aggregation bias, we caution against drawing a conclusion from this result.

Finally, it is notable that none of the tax coefficients is statistically significant. In general, then, elderly migration appears mostly affected by amenities. Tax policy, including EIG taxes, appears unimportant. The reasonableness of our other results and the appearance that many of the problems that have plagued elderly (and other) migration research, such as the "same–sign" problem and the "desirable" effect of crime, are solved with this methodology leads us to conclude that it is a worthwhile approach. That the resulting estimates from this approach suggest that EIG taxes have no differential or absolute effect on elderly migration is a serious blow, in our view, to the belief that widespread elderly migration is affected by state EIG taxes.

Sensitivity Checks

First of all, we want to make sure that our results are not sensitive to the census years included, especially because Appendix Tables A1 and A2 reveal a fair amount of instability. We, therefore, re–estimate the panel models alternately excluding the 1970 census data and the 2000 census data. The empirical results are substantively the same, especially for the EIG tax variables. Another issue is that the percentage of the population that is elderly could be reflecting past EIG policy and migration decisions. Excluding this variable has no appreciable impact. Similarly, the EIG measures could be endogenous, especially those that involve EIG tax revenues, as states that experience elderly migration in the past have a larger number of estates or experience political pressure to reduce their EIG taxes. We deal with this in part by using lagged values for our EIG measures. However, to verify the validity of our approach, we also use 2sls and the parameters of the state EIG tax code (e.g., exemptions, top and bottom tax brackets and rates) for identification. The resulting estimated EIG coefficients are even less statistically significant, if anything.

Past research, such as Clark and Hunter (1992) and Conway and Houtenville (2003), has found that the younger elderly are more responsive to state EIG taxes. To allow for this possibility, we re–estimate both the panel and cross–sectional models restricting our elderly migration measure to those between the ages of 65 and 74. In no case are the EIG tax results substantively different.

We also explore redefining all of our explanatory variables to be relative to the state's "neighbors." In our analyses thus far, the characteristics of one state

[30] Recall that only the percent elderly is included as an explanatory variable and that our migration rates are constructed using total population.

are implicitly being compared with those of all others. As outlined in Conway and Houtenville (2001, 2003), however, the elderly are most likely to move to a bordering state or a “haven” state. In this spirit, we redefine all explanatory variables to be the simple difference between that state’s variable and the weighted mean of those of its neighbors, or $X_i' = X_i - \Sigma w_{ij} X_j$, for all i not equal to j and where w_{ij} is the weight between state j and the state of interest, i, and is equal to 1.0 if the states share a border. Redefining the variables in this way has no appreciable effect on the EIG results.[31]

Finally, we explore whether the progressivity of the EIG tax has an impact by including as a variable, either exclusively or in addition to the incremental EIG measures, the lowest estate (assumed inherited by an adult child) subject to the state’s tax. As mentioned above, the many states that do not levy an incremental EIG tax make the practical implementation of this exercise difficult, so we try several alternative methods of dealing with it. In the vast majority of cases, the variable is statistically insignificant and when it is significant, it tends to be the “wrong sign” (e.g., a high exemption discourages in–migration). The one scenario in which the results are suggestive (but far from consistently important) is the case in which the incremental tax dummy is included linearly and interacted with the exemption. In this scenario the exemption tends to increase in–migration and net in–migration and sometimes decrease out–migration, but is often not statistically significant. In addition, its inclusion has little impact on the estimated linear effects of having an incremental EIG tax; they are never statistically significant. We, therefore, conclude that the evidence is not persuasive that the progressivity of the EIG tax is important to migration behavior.

THE CHICKEN OR THE EGG?

Given that we find little evidence that the elderly are responding to changes in EIG tax policies, we now explore whether the causality perhaps runs in the other direction—that elderly migration is somehow affecting/shaping EIG tax policy. Perhaps state legislators are reacting to elderly migration patterns in their state when drafting their state EIG policies. Indeed, reverse causality might help explain why the “same–sign” problem remains for EIG taxes if states that have a mobile elderly population feel more pressure to reduce their EIG taxes.

We explore this possibility with two exercises in which we estimate the changes in the states’ EIG policies over time as a function of their past migration experience. Of course, we must also control for other state characteristics that may affect EIG policies, and we look to Conway and Rork (2004) for guidance. Specifically, our main model specification includes the same explanatory variables as in their model of state EIG tax competition—the state’s reliance on sales taxation and on personal income taxation, the percentage of the population over age 65 and over age 85, respectively, per capita state income, federal transfers and debt, the state unemployment rate and several political variables. The sources and descriptive statistics for these variables are listed at the bottom of Table 3. We also include the EIG tax policy of each state’s “neighbors.” To avoid possible endogeneity,

[31] Some of the other coefficients are affected by these two exercises. Cost–of–living (median house value), the state unemployment rate and the expenditure variables are less likely to be statistically significant. The last exercise makes the property tax share more likely to be important—but always with the opposite sign from what is expected (e.g., it discourages out–migration).

we use lagged values of the explanatory variable values. Finally, our key variable is the state's past experience with elderly migration, which is only available every ten years, and, thus, limits our analyses, as we describe below.

Cox Proportional Hazard Model

The first exercise explores the states' decisions to eliminate their incremental EIG taxes. We estimate this decision with a Cox proportional hazards model, using the date that the law became effective and allowing for time–varying covariates (Greene, 1997, pp. 997–9). Specifically, the hazard model specifies the hazard rate:

$$[2] \quad \lambda(t_i) = e^{-\beta X_i},$$

where X contains the explanatory variables. Cox's partial likelihood allows estimation of β without specifying λ by conditioning on the risk set at time T_i. Specifically, the probability that state i exits at time T_i given that exactly one state exits at that time is:

$$[3] \quad Pr(t_j = T_i \mid riskset_i) = \frac{e^{\beta X_i}}{\sum_{j \in R_i} e^{\beta X_j}},$$

where the denominator includes the entire risk set, i.e., all those states that have not yet eliminated their EIG taxes at time T_i. Allowing the *X's* to vary over time complicates the estimation only inasmuch that the appropriate values of X must be chosen for each event time.

To illustrate, suppose we are considering the probability that New Mexico eliminates its incremental EIG tax given the 43 states that still had one in 1975. We use 1975 values for all of the *X's* in this case. Then we consider who will eliminate next. The next event occurs in 1977 (for Utah), so the 1976 values will be used and the risk set includes 42 states (and excludes New Mexico). As our data is only complete through 1999, we use the 1999 (or 2000, when available) values for the two events that occur in 2001 (Montana and South Dakota). Because the next event is not until 2003, four years after our data ends, we treat all events after 2001 (NH in 2003, LA in 2004, and CT in 2005) as censored observations and instead use them as an out–of–sample prediction check of our model.

This method of estimation uses all of the censored observations (the states that have not yet eliminated EIG taxes) because they always appear in the risk set. It also does not require us to choose a specific date for "time zero," but instead we only need to assume that "time zero" is the same for all states. Another interesting characteristic is that it is only the *ordering* of the events that matters—we are estimating which state will eliminate death taxes *next*, not *when*. The Cox proportional hazards model is close in spirit to a conditional probit, which is how we view this phenomenon, and to the empirical approach taken by Alm, McKee, and Skidmore (1993) in their research on the introduction of state lotteries.[32]

Our key variable of interest, however, is the net in–migration rate of the elderly in 1965–70. We use 1965–70 values for the migration variables because the move to begin eliminating taxes did not begin until 1976, so the policies could not have

[32] Other issues involved in estimating a Cox proportional hazard model include the treatment of ties, obtaining appropriate estimated standard errors, testing for model misspecification, and predicting out of sample. We tried several methods of dealing with ties and found it made little difference. The estimated standard errors we use to construct the Z–statistics are calculated using the variance–covariance matrix of Lin and Wei (1989), allowing for clustering for each state. We test every model for the validity of the proportional hazard assumption using Schoenfeld residuals and the global and local tests of Grambsch and Therneau (1994). All reported models pass both the global and local tests. For a clear and succinct discussion of these issues, see the Stata Reference P–St manual, st stcox chapter, release 6. See also Hosmer and Lemeshow (1999).

"caused" the migration patterns. Because migration data is only available every ten years, updating the variable would cause an arbitrary "bump;" furthermore, concerns about its endogeneity caution against interpolating the years in between. For these reasons, we use the 1965–70 values for all of the years of the analysis.

The results of this exercise are reported in Table 6. We also estimate, but do not report, several simpler specifications that drop sales and income tax reliance out of concern for their possible endogeneity or the political variables and other variables that are statistically insignificant. In every specification we estimate, all three migration measures are associated with a statistically significant increased likelihood of eliminating EIG taxes. In–migration and net in–migration appear particularly

TABLE 6
COX PROPORTIONAL HAZARD RATIO ESTIMATES OF DECISION TO ELIMINATE STATE INCREMENTAL EIG TAXES

Independent Variable (from previous year)	[1] In–Migration	[2] Out–Migration	[3] Net–Migration
migration measure (1965–1970)	1.188*** [3.59]	1.406** [2.15]	1.267*** [3.30]
prior year's reliance on sales taxation	1.046** [2.47]	1.045** [2.30]	1.045** [2.42]
prior year's reliance on personal income taxation	1.056** [2.16]	1.049* [1.94]	1.055** [2.23]
percent of population 85 years or older	3.509 [0.84]	1.004 [0.00]	4.088 [0.91]
percent of population 65 years or older	0.599* [–1.76]	0.757 [–1.08]	0.574* [–1.74]
state per capita income	0.999 [–1.36]	0.999 [–1.37]	0.999 [–0.88]
state per capita federal transfers	1.004*** [3.43]	1.003** [2.32]	1.005*** [3.88]
state per capita debt	0.999 [–0.68]	0.999 [–0.40]	0.999 [–0.68]
state unemployment rate	1.031 [0.24]	0.998 [–0.02]	1.044 [0.34]
same party—democrat (Yes = 1)	0.806 [–0.43]	0.837 [–0.35]	0.728 [–0.65]
same party—republican (Yes = 1)	1.533 [0.86]	1.288 [0.49]	1.778 [1.14]
election year dummy (Yes = 1)	1.641 [1.00]	1.544 [0.89]	1.648 [1.00]
contiguous neighbors' policies	0.986 [–1.15]	0.995 [–0.36]	0.985 [–1.35]
next 5 states predicted to fall (in order)	LA, KY, IN OH, NE	IN, LA, OH KY, NE	LA, KY, OH IN, TN

Notes:
t–statistics in parentheses.
***significant at the 99th percent level. **significant at the 95th percent level. *significant at the 90th percent level.

important. This is after controlling for the percentage of the population that is elderly. This exercise, therefore, yields evidence that suggests it is at least equally plausible that elderly migration is causing EIG tax policy changes, not the reverse. However, it could still suffer from spurious correlation because it is essentially a cross–sectional analysis, and it is incomplete because wholesale elimination of the incremental EIG tax is only part of the story. Our second exercise attempts to address these issues.

Panel Data Analysis in Which EIG Policy is a Function of Migration Patterns

Another approach to this problem is to simply reverse the panel analysis that we performed for migration in the fifth section. Specifically, we now estimate our four incremental *EIG* measures as a function of past migration patterns, the control variables described above, and state and year fixed effects. We have located the necessary information to construct the incremental EIG tax dummy, share and aggregate average tax rate in 2003, and Bakija and Slemrod (2004) provide 2000 values for their average effective tax rate. We, therefore, estimate, for example, 2003 EIG measures (2000 for the Bakija and Slemrod EIG tax measure) as a function of migration that took place in the prior census (1995–2000) and explanatory variables from 1994.[33] In this way we can construct a panel dataset consisting of four waves (EIG measures for 1973, 1983, 1993 and 2003) as a function of the previous census' migration measures. Specifically, we estimate separate regressions for each EIG measure and include either in–, out– or net in–migration rates for the elderly, the young (which we expect to have little impact) or the difference. In addition, because we believe that a state might be affected by the overall *mobility* of their elderly (i.e., it could feel pressure if many of the elderly are leaving the state), we include that total migration rate, which equals in–migration rate *plus* out–migration rate.

The results for the migration coefficients from these regressions with and without state fixed effects are reported in Table 7 and Appendix Table A3, respectively. For both the incremental EIG dummy variable and the Bakija and Slemrod tax rates, the evidence is suggestive that recent elderly migration, especially in–migration, may reduce EIG taxes. Although clearly far from definitive, they are much more suggestive than the ones from the migration models, in which EIG taxes are found to have little impact.

It is also notable that these reported results tend to be the most conservative we find. Omitting state fixed effects clearly strengthens the relationship to where the migration measures are almost always negative and statistically significant. In addition, omitting state effects has a much bigger impact and leads to much stronger results in this analysis (Appendix Table A3) than in the corresponding migration models (Appendix Table A1). In other words, whether or not state fixed effects are included, the evidence is more compelling that migration affects EIG taxes rather than the typically assumed

[33] We choose 1994 (and 1984, 1974 and 1964) in order to be consistent with the migration analyses. As a practical matter, many of the variables are not available more recently than 2000. Also, in this analysis we instrument the neighbors' tax policy variable, using the weighted values of the explanatory variables for the neighbors, so that we may be more consistent with Conway and Rork (2004) in particular and the spatial econometric/tax competition literature more generally. As we discuss shortly, dropping this variable entirely has no substantive impact and makes the results stronger if anything. Finally, because EIG revenues are likely to be more volatile in states with small populations, we weight the regressions that are a function of this variable (EIG tax share and EIG average tax rate) by the state's population.

TABLE 7
SUMMARY OF PANEL ESTIMATION RESULTS USING EIG MEASURES AS DEPENDENT VARIABLE

	Incremental EIG	Bakija & Slemrod	Incremental EIG or Decoupled	EIG Tax Share	Average EIG Tax Rate
Elderly					
in–migration	–0.026 [–1.58]	–0.065 [–1.57]	–0.044** [–4.20]	0.001 [0.66]	–0.001 [–0.61]
out–migration	0.018 [0.31]	–0.031 [–0.16]	0.0002 [0.00]	–0.003 [–1.10]	0.007 [1.13]
net–migration	–0.030* [–1.65]	–0.065 [–1.52]	–0.047** [–3.50]	0.001 [0.82]	–0.001 [–1.07]
total migration	–0.019 [–1.26]	–0.053 [–1.21]	–0.408 [–0.55]	0.0001 [0.26]	0.0002 [0.17]
Young					
in–migration	–0.005 [–0.17]	0.122 [1.15]	–0.051 [–1.57]	0.002 [0.93]	–0.001 [–0.21]
out–migration	0.015 [0.26]	–0.341 [–1.50]	0.145** [2.03]	–0.002 [–0.67]	0.003 [0.54]
net–migration	–0.006 [–0.24]	0.135 [1.55]	–0.057** [–2.17]	0.002 [0.95]	–0.001 [–0.46]
total migration	–0.003 [–0.08]	0.055 [0.52]	–0.024 [–0.67]	0.001 [0.89]	0.0002 [0.08]
Elderly–Young					
in–migration	–0.029* [–1.80]	–0.122** [–2.56]	–0.033* [–1.74]	0.002 [0.34]	–0.001 [–0.59]
out–migration	0.008 [0.17]	0.118 [0.62]	–0.060 [–1.43]	–0.002 [–1.15]	0.004 [0.95]
net–migration	–0.025 [–1.62]	–0.119** [–2.32]	–0.019 [–0.95]	0.0004 [0.70]	–0.001 [–0.96]
total migration	–0.023 [–1.51]	–0.086* [–1.66]	–0.037** [–2.38]	–0.0002 [–0.63]	0.0001 [0.13]

Notes:
Standard errors adjusted for clustering and general heteroskedasticity; t–statistics reported in brackets.
**significant at 5% level. *significant at 10% level.
State and year fixed effects are included; all regressions involving EIG Tax Share or Average EIG Tax Rate weighted by state populations.
All explanatory variables are included in the estimation; we report only the migration variables for simplicity.

reverse direction. Dropping the other tax variables (again, out of concern for endogeneity) or omitting the neighbors' tax policy also strengthens the results, if anything.[34]

Another consideration is the change in the federal estate tax law (EGTRRA of 2001), which phases out the state tax credit (and source of “pickup” revenues) beginning in 2002 and eliminates it entirely by 2005. As documented by Michael (2004) and Conway and Rork (2004), this change provided states with new and more subtle ways of imposing an incre-

[34] The importance of the other variables is fairly weak and varies across the measures, with the exception of per capita federal transfers, which consistently reduce EIG taxes.

mental EIG tax, frequently referred to as "decoupling."[35] It also eliminated a source of tax revenues and, thus, may provide states with a strong motive to decouple. Our reported results classify states as having an incremental EIG tax solely on whether their law is written to depend on the "pickup" tax. In other words, a state that relied only on a pickup tax but decided to "decouple" (by failing to update its law, for example) is still treated as not having an incremental EIG tax. We adopt this approach because this issue is unique to the last period of our data and it allows the states such a new, subtle way to impose an incremental tax; we are, therefore, concerned about comparability. When we treat these "decoupled" states as having an incremental EIG tax in 2003, many of the migration coefficients, especially those related to elderly in–migration, are negative and highly statistically significant. These results are reported in the third column of Table 7. Compared with the results from column 1, they strongly suggest that migration patterns are affecting the states' decisions to decouple. They also suggest that the mobility (especially out–migration) of the young may be playing a role. However, given the very different and recent nature of the decision to "decouple," we caution against drawing too strong of a conclusion from these results.

It also makes sense that the EIG tax share and EIG aggregate tax rate, reported in the last two columns, are less affected by migration because elderly in–migration is more likely to have a confounding influence on them. The incremental tax dummies and the Bakija–Slemrod tax rates are policy parameters. These other two measures, however, are created out of incremental *EIG* tax revenues and are, therefore, a function of the characteristics of the deceased. If in–migrants are richer, for example, than the typical elderly individual, then recent in–migration could increase EIG tax revenues without a change in policy and even after controlling for the percent of the population that is elderly or the average income level. (Indeed, this is why we re–estimated the migration models with 2sls as a sensitivity check.) And, as discussed earlier, many elderly who are subject to state EIG taxes are exempt from federal taxes, so dividing by the total value of estates subject to federal taxation does not entirely correct the problem. It, therefore, seems likely that in–migration would have a positive effect on these two measures that acts to offset the negative impact suggested by the pure policy variables.

Taken together, our two exercises provide modest evidence that the causality may indeed go the other way—that elderly migration influences EIG policy. However, the cross–sectional nature of the first exercise and the less than definitive results from the second caution against drawing a firm conclusion that migration affects EIG policy.[36] Nonetheless, these results clearly show that it is at least as likely, if not more so, that migration behaviors are influencing state EIG tax policy than the typically assumed reverse relationship.

[35] For instance, in some cases, the state's tax law references the federal tax law at a certain date. These states can essentially impose an incremental EIG tax simply by failing to update their tax laws. Other states have to take legislative action, and still others require a change to the state constitution. Illinois, Maine, Massachusetts, Minnesota, New York, North Carolina, Oregon, Rhode Island, Washington, Wisconsin, Vermont and Virginia are "pickup–only" states that had chosen to decouple by the end of 2003.

[36] A logical next step would be to perform panel Granger causality tests on the two variables (e.g, Holtz–Eakin, Newey, and Rosen (1988), and Arrelano and Bond (1991)). We attempted to perform these tests under a variety of specifications, but our data (in which there is only four time periods) does not appear rich enough to satisfy the associated model specification tests. These findings cast serious doubt on any conclusion we might draw from the analyses. The results of these efforts are available upon request.

CONCLUDING REMARKS

Our research casts doubt on the view that the elderly react to state EIG tax policies in making their migration decisions. In fact, using two different analyses, we find some evidence that the causality may instead run in the other direction—states that experience high elderly in–migration may be more likely to subsequently eliminate or reduce their incremental EIG taxes.

Past evidence on elderly migration, with the important exception of Bakija and Slemrod (2004) whose focus is the very rich elderly, has been based on cross–sectional data. We reveal that any cross–sectional analysis may be misleading as many states that are historical net–importers of the elderly were also among the first to eliminate their incremental EIG taxes or otherwise reduce them. This, combined with the relative stability of elderly migration patterns, makes any cross–sectional correlation highly suspect. Using time variability in both migration patterns and EIG tax policies and using the migration patterns of the young as a “pseudo–control” group almost completely eliminates what weak evidence there is that EIG policies affect elderly migration.

Contrasting our results with those of Bakija and Slemrod (2004), who find evidence that the very rich elderly are discouraged by high EIG taxes, leads to an interesting conclusion. It is entirely possible that the residence choice of the very rich elderly is affected by EIG taxes, whereas the overall level of elderly migration is not. The proportion of the elderly considered in the study by Bakija and Slemrod is small, typically far less than ten percent, as they are only the elderly subject to federal estate taxation. Changes in their movements may, therefore, not have much impact on the aggregate level of migration. These individuals are also the ones with the most to gain from moving and perhaps the easiest method of doing so (especially those with multiple homes)—simply switching their legal domicile. Likewise, the implications for the states—whether this elderly migration is “good” for the state—differ between these two types of elderly migrants. Any estate that “moves” to a state will benefit the state in terms of the estate revenue it generates. However, if the state is after the type of economic gains documented by Longino and Crown (1989) and Sastry (1992), which arise from the economic stimulus of new migrants, then the legal switch of domicile by a small number of very high–income elderly is going to fall short.

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APPENDIX

APPENDIX A1
SUMMARY OF CROSS–SECTION RESULTS WITH VARIOUS ELDERLY MIGRATION MEASURES

EIG Tax Measure	1980			1990			1970–2000 Pooled		
	In	Out	Net	In	Out	Net	In	Out	Net
Incremental EIG Tax (Yes=1)	–7.230** [–2.51]	–1.939 [–1.45]	–5.291** [–2.63]	1.112 [0.93]	0.582 [0.93]	0.530 [0.48]	–1.843 [–1.49]	–0.056 [–0.19]	–1.787 [–1.53]
EIG Tax Share	–103.159* [–1.81]	–63.272* [–1.84]	–39.886 [–0.92]	7.915 [0.11]	18.554 [0.45]	–10.639 [–0.16]	–18.590 [–0.63]	–3.332 [–0.45]	–15.258 [–0.61]
Average EIG Tax Rate	–62.146* [1.97]	–51.678** [–2.91]	–10.469 [–0.43]	6.818 [0.44]	2.562 [0.30]	4.256 [0.30]	–5.647 [0.48]	0.094 [0.02]	–5.741 [–0.51]
Bakija & Slemrod Tax Rate	–0.336 [–1.17]	–0.421** [–3.02]	0.085 [0.37]	0.161 [0.46]	–0.076 [–0.43]	0.237 [0.80]	–0.191 [–0.73]	–0.186** [–2.49]	–0.005 [–0.02]

Notes:
Standard errors adjusted for clustering and general heteroskedasticity; t–statistics reported in brackets. **significant at 5% level. *significant at 10% level.
State and year fixed effects included; all regressions weighted by state population.
All explanatory variables listed in Table 5 are included.

APPENDIX A2
FULL CROSS–SECTION RESULTS WITH VARIOUS ELDERLY MIGRATION MEASURES

	1980			1990			1970–2000 Pooled		
	In	Out	Net	In	Out	Net	In	Out	Net
Amenities									
heating degree days	0.0002 [0.41]	0.0005** [2.36]	–0.0003 [–0.99]	0.0007 [1.16]	0.0003 [1.23]	0.0004 [0.86]	–0.0003 [–0.71]	0.0004** [2.66]	–0.0007** [–2.24]
median house value	–0.00003 [–0.29]	0.00004 [0.86]	–0.0001 [–1.08]	0.0001 [1.11]	0.00002 [0.81]	0.00004 [1.06]	–5.28E–07 [–0.03]	4.32E–06 [0.66]	–4.84E–06 [–0.33]
average mfg wage	–1.123** [–3.01]	–0.078 [–0.44]	–1.045** [–3.52]	–1.013** [–2.26]	–0.386* [–1.68]	–0.628* [–1.75]	–0.600** [–2.98]	–0.213** [–2.05]	–0.387** [–2.16]
state unemployment rate	–0.165 [–0.32]	–0.235 [–0.76]	0.070 [0.14]	0.143 [0.45]	0.024 [0.16]	0.118 [0.42]	–0.181 [–1.13]	–0.064 [–0.77]	–0.117 [–0.78]
% population 65 or over	56.760 [1.50]	–5.748 [–0.29]	62.508** [2.37]	91.602** [5.21]	8.685 [0.76]	82.917** [5.41]	74.766** [2.70]	6.153 [0.80]	68.614** [2.81]
crime rate	0.002** [3.38]	0.0003 [0.81]	0.002** [3.66]	0.002** [3.97]	0.0005* [1.80]	0.002** [3.77]	0.002** [4.23]	0.0006** [4.49]	0.001** [3.14]
Expenditures									
expenditures on health & hospitals	–22.270** [–2.38]	2.225 [0.44]	–24.496** [–3.83]	1.537 [0.16]	–0.885 [–0.21]	2.422 [0.34]	–9.634** [–2.50]	–0.339 [–0.16]	–9.295** [–2.53]
expenditures on education	8.518 [1.02]	2.615 [0.59]	5.903 [0.94]	0.869 [0.26]	0.252 [0.14]	0.617 [0.23]	6.954 [2.09]	1.197 [1.23]	5.757* [1.92]
expenditures on welfare	–2.278 [–0.40]	1.048 [0.41]	–3.326 [–0.80]	–22.818** [–3.92]	0.375 [0.10]	–23.193** [–5.26]	–3.809 [–1.60]	–0.452 [–0.47]	–3.357 [–1.61]
all other expenditures	2.956 [0.63]	2.418 [1.27]	0.538 [0.16]	0.986 [0.64]	0.906 [1.04]	0.080 [0.07]	0.521 [0.36]	1.031* [1.81]	–0.510 [1.199]
Taxes									
property tax share	–10.949 [–1.44]	–0.953 [–0.19]	–9.996** [–2.02]	–14.894 [–1.62]	2.707 [0.38]	–17.601 [–1.50]	–10.756* [–1.80]	–0.006 [0.00]	–10.750** [–2.23]
sales tax rate	–0.068 [–0.19]	–0.253 [–1.29]	0.185 [0.75]	0.169 [0.40]	–0.104 [–0.44]	0.273 [0.72]	–0.217 [–1.01]	–0.128 [–1.24]	–0.089 [–0.47]
average income tax rate	99.180* [–1.87]	–78.095** [–2.04]	–21.085 [–0.49]	30.730 [0.41]	–27.931 [–0.72]	58.662 [1.00]	–46.253 [–0.81]	–24.027 [–0.90]	–22.225 [–0.47]
other per capita tax share	–2.901 [–0.53]	0.184 [0.04]	–3.093 [–0.91]	4.311 [0.52]	0.296 [0.07]	4.012 [0.64]	–2.124 [–0.38]	–1.637 [–0.79]	–0.487 [–0.09]
incremental EIG tax (Yes=1)	–7.230** [–2.51]	–1.939 [–1.45]	–5.291** [–2.63]	1.112 [0.93]	0.582 [0.93]	0.530 [0.48]	–1.843 [–1.49]	–0.056 [–0.19]	–1.787 [–1.53]
constant	7.200 [0.96]	0.511 [0.11]	6.689 [1.27]	–8.529* [–1.71]	1.008 [0.28]	–9.537** [–2.13]	6.907** [2.16]	2.795* [1.69]	4.113 [1.31]

Note: footnotes to Table 5 apply, with the exception that state fixed effects are not included.

APPENDIX A3
SUMMARY OF PANEL ESTIMATION RESULTS USING EIG MEASURES AS DEPENDENT VARIABLE AND NO STATE FIXED EFFECTS

	Incremental EIG	Bakija & Slemrod	Incremental EIG or Decoupled	EIG Tax Share	Average EIG Tax Rate
Elderly					
in–migration	–0.028**	–0.063**	–0.090**	–0.0006**	–0.002**
	[–4.84]	[–3.62]	[–2.07]	[–2.43]	[–2.47]
out–migration	–0.039*	–0.189**	–0.090*	–0.0004	–0.003
	[–1.82]	[–2.88]	[–1.89]	[–0.53]	[–0.90]
net–migration	–0.041**	–0.064**	–0.120**	–0.0007**	–0.003**
	[–4.88]	[–2.82]	[–2.43]	[–2.82]	[–2.93]
total migration	–0.020**	–0.052**	–0.063*	–0.0004*	–0.002**
	[–4.27]	[–3.36]	[–1.88]	[–1.98]	[–2.07]
Young					
in–migration	–0.064**	–0.146**	–0.177**	–0.001**	–0.005**
	[–3.69]	[–2.99]	[–2.14]	[–2.11]	[–2.24]
out–migration	–0.030	–0.233**	–0.139	–0.0009	–0.005
	[–0.76]	[–2.69]	[–1.48]	[–0.86]	[–1.34]
net–migration	–0.092**	–0.122**	–0.197**	–0.001**	–0.006**
	[–4.22]	[–2.28]	[–2.46]	[–3.01]	[–2.63]
total migration	–0.036**	–0.107**	–0.110*	–0.0006*	–0.003**
	[–2.61]	[–3.01]	[–1.96]	[–1.67]	[–1.99]
Elderly–Young					
in–migration	–0.036**	–0.079**	–0.106**	–0.0008**	–0.003**
	[–4.30]	[–3.69]	[–2.05]	[–2.50]	[–2.43]
out–migration	–0.052*	–0.199**	–0.065*	–0.0002	–0.001
	[–1.82]	[–2.35]	[–1.81]	[–0.16]	[–0.36]
net–migration	–0.039**	–0.067**	–0.117**	–0.0008**	–0.003**
	[–3.45]	[–2.26]	[–2.14]	[–2.51]	[–2.62]
total migration	–0.665**	–0.070**	–0.079*	–0.0006*	–0.002*
	[4.23]	[–3.34]	[–1.96]	[–1.96]	[–1.89]

Notes:
Standard errors adjusted for clustering and general heteroskedasticity; t–statistics reported in brackets.
**significant at 5% level. *significant at 10% level.
Year fixed effects are included; all regressions involving EIG Tax Share or Average EIG Tax Rate weighted by state populations.
All explanatory variables are included in the estimation; we report only the migration variables for simplicity.