Social Spillovers in Personal Bankruptcies*

Astrid Dick
INSEAD

Andreas Lehnert
Board of Governors

Giorgio Topa
FRBNY

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Abstract

We use high-frequency, geocoded data on personal bankruptcies in the U.S. to study the extent of social spillovers in individual filings. At least two possible mechanisms lead to the presence of local social spillovers: information sharing and the reduction of social stigma associated with filing. In the U.S. over our sample period, bankruptcy laws are mainly determined at the state level; we exploit law changes in several New England states to estimate the magnitude and geographic extent of such spillovers, if any. The results are mixed: in the early law change episodes we find some evidence of spillovers in zipcodes close to the border with the state in which the change occurred. There is no evidence of spillovers in the later episodes, perhaps because a nationwide spike in bankruptcy filings coincided with the later episodes. The magnitude of the estimated spillovers ranges between about 8% and 16% relative to the baseline filing rate.

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1 INTRODUCTION

Personal bankruptcies are an important aspect of consumer finance and credit markets. In the U.S., borrowers who file for bankruptcy protection may discharge most unsecured debts. Indeed, Fay et al. (2002) report that lenders lost about $39bn in 1998 because of personal bankruptcies. Further, the rate of consumer bankruptcy in the U.S. has more than quadrupled over the last quarter century. There is also a very active policy debate regarding the optimal regulation of bankruptcies. On the one hand, with complete markets and perfect risk sharing, bankruptcy—that is, the ability to legally renege on debts—serves no purpose and may act mainly to restrict credit availability. On the other hand, with incomplete markets and imperfect risk sharing, bankruptcy—more precisely, debt with default—provides crude insurance against idiosyncratic risk.

Much of the current policy debate still centers around the question of who files for bankruptcy, and why. Understanding the mechanisms that lead to filing decisions is important in order to better understand bankruptcy dynamics and to design better policy. Several theories of the primary motives for filing have been advanced, including insurance against adverse events, myopia, irrationally exuberant expectations, and external habit (i.e. overconsumption related to "keeping up with the Joneses" style preferences). One specific mechanism focuses on the role of local social spillovers. There is some evidence that even after controlling for individual characteristics and changing financial, income and consumption circumstances, an individual's propensity to file is affected by lagged filing decisions in a local neighborhood (see Fay et al. (2002)).

With regard to social spillovers, one can think of at least two alternative explanations for why two proximate individuals might decide to file at about the same time. The first is an informational channel: filing for bankruptcy may be perceived to be complicated and involving several bureaucratic steps; knowing someone who has recently filed may make it easier for one to also file, since it reduces the informational barriers to filing. The second channel is related to stigma. Filing for bankruptcy may carry a certain amount of social stigma and disapproval because it is a public admission of one's financial failures. Bankruptcy may be seen as violating cultural or ethical norms about "always paying one's debts". In such a setting, an increase in bankruptcy rates in one's social environment may lessen the stigma attached to the act of filing.

These channels have been emphasized as possible explanations for observed spatial correlation in outcomes both in the context of bankruptcies and in other settings. Fay et al. (2002) report that a 1997 survey of recent bankruptcy filers by VISA U.S.A. Inc. finds that about half of filers first heard about bankruptcy from family or friends; further, "respondents reported that they were very apprehensive about filing for bankruptcy beforehand, but found the actual process of filing much quicker and easier than they expected". Bertrand et al. (2000) study informational and stigma effects in the context of public assistance programs, in order to explain local variation in welfare uptake rates.

More generally, a rich and growing literature has studied the presence and effects of social spillovers in several applications: network effects in the labor market (see Calvo-Armengol and Jackson (2004), Topa (2001), Ioannides and Loury (2004), Bayer et al. (2008)); crime (Glaeser et al. (1996), Calvo-Armengol et al. (2007), Patacchini and Zenou (2008)); learning about new technologies (Conley and Udry (2005), Bandiera and Rasul (2006)); education (Hoxby (2000), Sacerdote (2001), Zax and Rees (2002)); knowledge spillovers and economies of agglomeration (Jaffe et al. (1993), Audretsch and Feldman (1996), Glaeser et al. (1992)).

In this paper, we use a novel and highly detailed dataset on bankruptcy filings to try to detect the presence of local spillovers in filings and measure their magnitude. The central component of our data consists of administrative measures of filing counts at the zipcode level for the entire United States, at a weekly frequency, over the period 1996-2005. Our strategy exploits the existence of state-level changes in bankruptcy laws that made it easier to file in the states where the legal changes took place, but not in the adjacent states.[1] In the absence of local social spillovers and all else equal, a change in bankruptcy law in state ***A*** should only affect filing rates in state ***A*** itself but not in the immediately adjacent state ***B*** where the legal environment stayed the same. In its simplest form, our empirical strategy uses a difference-in-difference approach to estimate social spillovers, comparing filing rates before and after the law change in ***A***, between zipcodes in ***B*** that are immediately adjacent to the border with state ***A*** (our treatment group) and other zipcodes in ***B*** (our control group) that are farther from the border with ***A***.

We use a variety of specifications aimed both at estimating a mean effect of the law changes in the adjacent areas and at characterizing the dynamics of these effects over time. We also experiment with a variety of economic distance measures to better capture the dimensions along which social network effects may arise (in the spirit of Conley and Topa (2002)).

Our empirical results are mixed. In the earlier law change episodes, Vermont 1997 and Rhode Island 1999, we find some evidence of local spillovers. In the neighboring states to these change states, Chapter 7 filings rose more (following the effective date of the legislative change) in zipcodes close to the border with the change state than in zipcodes far from the border. The magnitude of the effect ranges from about 8% to about 16% of the baseline filing rates in these areas. We find no evidence of spillovers in the remaining three episodes (Massachusetts 2000, Rhode Island 2001, New Hampshire 2002), possibly because these later episodes coincided at least partially with a major and nationwide increase in filing.

We find that the spillovers effect tends to be temporary: filing rates rise faster in zipcodes close to the border than in far-away ones at first, but tend to revert back to similar levels within a year from the effective date of the law change. These patterns are broadly confirmed in the specifications that look at subsets of zipcodes that are "closest" to ***A*** in terms of several demographic and economic attributes.

The remainder of the paper is organized as follows. Section 2 describes the administrative bankruptcy data and the institutional history of the law changes considered in this paper. Section 3 describes our empirical strategy. We report our empirical findings in Section 4 and conclude in Section 5.

2 DATA AND INSTITUTIONAL BACKGROUND

Data sources. We use administrative data from the Department of Justice collected by Lundquist Consulting and the National Bankruptcy Research Center covering the period 1996-2005. The data give us the total counts of filings for personal bankruptcy per week broken down by zipcode of residence of the filer. Counts are reported separately for all different types of personal bankruptcy, including filings under Chapters 7, 11, 12 and 13 of the bankruptcy code. In what follows we focus on Chapter 7 filings only, to avoid the possibility that state law changes may induce substitution across different types of filings.[2] Moreover, homestead exemptions, which are central to our analysis, are most likely to make a difference under Chapter 7, since in practice there is little if any asset liquidation under Chapter 13.[3]

We also use data from the 2000 Decennial Census of households, in order to be able to merge demographic information with the filing information at the zipcode level. The merging requires a certain amount of GIS matching, since Census data are only available for aggregates of Census tracts (ZCTAs) that do not always have an exact correspondence to zipcodes. For those zipcodes that do not have a direct match with a ZCTA, we match based on minimizing

[1] Although the U.S. Constitution explicitly reserves the right to set uniform national bankruptcy laws to the federal government, between the two most recent major national legislative changes, in 1979 and 2005, bankruptcy laws were largely determined by individual states.

[2] Our empirical results are robust to the inclusion of Chapter 13 filings; filings under other chapters are vanishingly small.

[3] The evidence suggests that most Chapter 13 filers are consumers trying to save their homes (White and Zhu 2008).

geographic distance between centroids. The Census demographic information enables us to construct measures of weekly filing rates per zipcode (number of filings divided by population between 19 and 64 years old), as well as measures of socio-economic distance to complement simple geographic distance measures.

Institutional background. In this paper we focus on five instances of state law changes that fall within the sample period covered by our data. Because collecting information on the legislative histories of state law changes is difficult, we concentrated on states in New England, which tend to have contiguous urban areas that cross state lines and are more likely to share cultural attributes than larger, less densely populated states. Table 1 summarizes the legal changes we exploit. The states under consideration are Vermont, Rhode Island (which experienced two separate law changes), Massachusetts and New Hampshire. The earliest state law change occurred on January 1, 1997 (Vermont), whereas the latest took place on January 1, 2002 (New Hampshire). For each law change, we know both the "passage date", i.e. the date on which the new law was passed by the state's legislative body, and the "effective date" i.e. the date on which the new law went into effect.

With respect to the nature of the legislative changes, they all entailed an increase in the so-called "homestead exemption" limits. Under Chapter 7 bankruptcy rules, debtors are not obliged to use their future earnings to repay their debts, but they must turn over their assets – above the exemption threshold – to the bankruptcy trustee, who liquidates them and uses the proceeds to repay creditors. The exemption threshold can be set by each state independently, and in this paper we exploit five instances of increases in the exemption level.

As Table 1 shows, the mandated increases were substantial: in Vermont for instance, the exemption was raised from \$30,000 to \$75,000; in Rhode Island in 1999, it increased more than six-fold, from \$15,000 to \$100,000. We expect the higher asset protection to induce an increase in the propensity to file, all else equal, in the state where the change occurred. The changes in exemptions in these earlier periods are not only large but go from relatively low levels (\$30,000 in Vermont and \$15,000 in Rhode Island), which is more likely to make them binding to consumers, given the generally low level of income and home equity of the average filer. We then exploit this state-level change to look at whether filing rates were also affected in zipcodes that are close to the border with a "change state", but in states where no legislative change took place.

Judging from the public debate surrounding the legislative changes, and the legislative histories themselves, it seems that the changes were fairly exogenous to the filing processes themselves. The most commonly cited reason for raising exemptions was to keep pace with inflation and not in response to changes in filing rates, or to a rising trend in filing rates (the latter, if anything, should have led to a decrease in the homestead exemption). Indeed, many of the changes took place in the midst of relatively flush economic times. This is important for our estimation design, since we want to rule out the possibility that the law changes were endogenous to the filing processes in the geographic areas under consideration (although, in our defense, our study does not focus on the state where the law took place, but on neighboring areas. Thus, as long as the change in the law was endogenous to state-specific factors, this should not represent a problem).[4]

3 EMPIRICAL STRATEGY

We adopt a simple difference-in-differences approach to test for the presence of local social spillovers in Chapter 7 filings. We assume that bankruptcy filings in a state increase when asset exemptions are increased in that state. Filing rates in a neighboring state whose law did not change will increase *ceteris paribus* only because of spillover effects.

Define "change states" as those states increasing their bankruptcy exemptions. We compare filing rates in a subset of zipcodes in states that did not experience a law change but are close to the border with a change state, with filing rates in zipcodes located far from the border with a change state. In other words, zipcodes far away from the border of a change state act as a control group for the former set of zipcodes. We further compare filing rates pre- and post-change to control for persistent within-state differences in bankruptcy rates. Formally, let us define state ***A*** as a state in which a legislative change occurred (i.e. a change state); state ***B*** as a state that is adjacent to state ***A*** (i.e. shares a physical border with ***A***), but where no changes in exemption levels occurred (Table 1 lists the ***B*** states for each change state, the passage and effective change dates, and the amount of the exemption before and after the change). We only use zipcodes that belong to metropolitan areas within the states under consideration, in order to make these zipcodes more comparable.[5] Figure 1 shows the zipcodes used in each change episode.

Our baseline difference-in-difference comparison can be written as a linear regression:

$$(1) \qquad Y_{it} = \alpha_0 + \alpha_1 \delta_i + \alpha_2 \gamma_t + \alpha_3 \delta_i \gamma_t + \varepsilon_{it}$$

[4] Another potential concern is that a state may change the law in response to a spike in filings in neighboring states, in anticipation of possible contagion: given our design, this would likely lead to an under-estimate of any social spillovers.

[5] We start by using alternative definitions of metropolitan areas, including PMSAs, SMSAs, and CMSAs. We then further refine our definition of "urban" zipcodes by dropping zipcodes with fewer than 10 households per square mile, and adding zipcodes outside metropolitan areas with more than 25 households per square mile.

where Y_{it} is the Chapter 7 filing rate in zipcode *i* at time *t*, for zipcodes in states ***B***; δ_i is a dummy variable equal to one if zipcode *i* is close to the border with state ***A***, zero otherwise (in what follows, we refer to zipcodes that are close to the border with ***A*** as zipcodes in ***B***-close; zipcodes that are far from the border are in ***B***-far: see Figure 1 for maps of the various types of zipcodes); γ_t is a dummy variable equal to one if *t* is greater than the effective date week, zero otherwise. Alternatively, we also run specifications where δ_i is a continuous variable reflecting the physical distance (in km.) between the center of zipcode *i* and the border to state ***A***. We consider filings over a time window that goes from 52 weeks prior to the effective date to 52 weeks after.

We test for local spillovers in filings by testing the null hypothesis that the coefficient α_3 is greater than zero. When we use the continuous distance variable, we report the coefficient estimate with the opposite sign to make its interpretation comparable to the "closeness" dummy.

We also use an alternative specification to focus on the dynamics of filings after the enactment date:

$$(2) \qquad \Delta Y_{it} = \beta_0 + \beta_1 \delta_i + \beta_2 S_t + \beta_3 \delta_i S_t + u_{it}$$

Here, the dependent variable is the *change* in filing rate over time, using only observations after the effective law change date: $\Delta Y_{it} \equiv Y_{i,t} - Y_{i,t-1}$; δ_i is defined as in (1) and S_t is an integer variable that tracks the number of weeks (or months) since the enactment date. In this specification the interpretation of the coefficients is more complicated, but it enables us to better understand the dynamics of filings in state ***B*** following the legislative change in state ***A***. For instance, if filings initially increase at a faster rate in ***B***-close than in ***B***-far, then level off at a higher level than in ***B***-far and eventually revert to a similar level as in ***B***-far, then we would expect to find $\beta_1 > 0$, $\beta_2 < 0$, and $\beta_3 < 0$. Such behavior would be indicative of a temporary local spillover from state ***A*** to zipcodes in ***B***-close.

We also perform a series of robustness exercises using different notions of socio-economic distance. So far, we have been using physical distance to identify zipcodes that are close to a change state. More generally, because our goal is to estimate social spillover effects, one can use different definitions of distance, based on social or economic affinity between households populating zipcodes. A large literature in sociology has found that social networks exhibit strong assortative matching along racial, ethnic, gender, or educational lines (see for example Marsden (1987), (1988)). Thus, two zipcodes may not necessarily be "close" in social networks space simply because they are geographically close; rather, they may be considered "close" because they are similar with respect to relevant demographic and economic attributes.

In order to implement this idea, we use the following strategy. Consider for instance ethnic and racial distance. First, we compute the median racial/ethnic composition for the urban zipcodes in state ***A***. This is defined as the median across zipcodes in ***A*** of the vector *[%Black, %Asian, %Hispanic]*. Second, for each urban zipcode in ***B***, we compute the racial/ethnic distance d_{iA} from each zipcode *i* in ***B*** to ***A***.[6] Then, we restrict the set of zipcodes in ***B*** under consideration to the bottom 50% of the distribution of d_{iA} . In other words, we only consider those zipcodes that are most similar to ***A*** with respect to racial/ethnic composition, using the 50th percentile of d_{iA} as a cutoff. Finally, we apply our original geographic distance rules to this restricted set of zipcodes in ***B*** to define the two sets ***B***-close and ***B***-far.[7] We use the same approach for two additional definitions of socio-economic distance: median housing values and the fraction of college-educated residents in a zipcode.

4 RESULTS

Before turning to the estimation results, we present in Figure 2 descriptive plots of filing rates in each of the five change states, around the time of the law changes. Each plot in Figure 2 contains the four-week moving average of Chapter 7 filing rates in three sets of zipcodes: those in the change state ***A***, those in ***B***-close, and those in ***B***-far. Time is measured in weeks, and goes from 52 weeks before the passage date (AD) to 52 weeks after the effective date (ED) of the new law. The bottom horizontal axis marks weeks since the AD (in event time), whereas the ED is marked on the top horizontal axis.

The clearest patterns over time appear in Vermont 1997 and in Rhode Island 1999: here, soon after the law change date ED, filing rates seem to spike up both in ***A*** and in ***B***-close. Interestingly, filings seem to also increase, at least temporarily, in ***B***-far, but not to the same extent as in ***B***-close. Our identification strategy relies on the difference between filing rates in ***B***-

[6] We use a simple Euclidean distance to compute the socio-economic distance between A and each zipcode in B. See Conley and Topa (2002) for details of this construction.

[7] An alternative strategy would have been to divide our ***B*** zipcodes into ***B***-close and ***B***-far with respect to racial/ethnic distance, i.e. using racial/ethnic distance from ***A*** as a criterion for closeness (instead of physical distance). However, this approach violates the exchangeability assumption of the diff-in-diff

close vs ***B***-far: even if other factors influence filing rates over time in state ***B***, by looking at the increased propensity to file in zipcodes that are close to the border with ***A*** *relative to other zipcodes in* ***B*** we can isolate the local spillover effect from these other factors.

Figure 2 also suggests that the increased propensity to file after the legislative changes (in ***B***-close relative to ***B***-far) may be a temporary phenomenon. This pattern motivates our specification in first differences in (2) above, which is aimed at identifying these dynamic effects more directly. Finally, as we discuss in more detail below, nationwide filing rates spiked in early 2001.

The estimation results of our two main specifications (1) and (2) are reported in Table 2 and 3, respectively. Table 2 contains the results of our estimation in levels. The three sets of columns in the Table correspond to three different definitions of the distance variable; the first two use a binary dummy variable, with different distance cutoffs for ***B***-close and ***B***-far (20 and 30 Km). The last set of columns uses the continuous distance variable described above; to make the coefficients more readily interpretable, the signs of the coefficient estimates for the continuous distance variable are reversed.

For each definition of the distance variable, we report estimates using both weeks and months as our measure of time periods. As Figure 2 has shown, weekly filing rates tend to be very volatile over time, so we experimented with aggregating up to months to make sure that our estimation results are not simply due to the specific time scale employed.

The estimation results in Table 2 indicate that our difference-in-difference estimate of spillovers (the coefficient associated to the cross-term in (1)) is positive and statistically significant in only a few instances: namely, the two specifications using weekly data and the binary distance variables for Vermont (VT) 1997, and the specifications with weekly data and both a binary and the continuous definition of "close" for Rhode Island (RI) 1999. In all other instances, the estimated spillover is not significantly different than zero (and is significantly negative in one case for Rhode Island 2001).

One possible explanation for the failure to detect a positive spillover effect in the later episodes (Massachusetts (MA) 2000, Rhode Island (RI) 2001 and New Hampshire (NH) 2002) is the existence of a large spike in filing rates in the first half of 2001 in the entire region. This spike is apparent in Figure 2, as mentioned above; it occurred a few weeks after the ED in MA 2000, and right before the AD in both RI 2001 and NH 2002. As shown in Figure 3, national bankruptcy filings spiked in early 2001. The nationwide increase in filings was likely driven by

approach by construction, since it necessarily produces differences in demographic attributes between ***B***-close and ***B***-far.

media attention surrounding Congressional passage of a comprehensive Federal bankruptcy reform law. On March 15, 2001, the Senate passed S. 420; earlier the House had acted on H.R. 333; together these bills formed the first Federal change in bankruptcy law since the late 1970s. The bills were seen as significantly tightening eligibility for discharge of debts, which was understood to be a priority of the newly installed Administration. (In the end, the bills did not emerge from the Senate-House conference and final passage and enactment of a Federal bankruptcy law would not occur until 2005.)

Such a large and spatially correlated spike in filings may add noise to the time-patterns of filings and thus make any spillovers harder to detect using our simple before-after comparison. Indeed, if the spatial pattern of filings is related to the dispersion of information about bankruptcy, a nationwide rush to file might overwhelm the subtle effects of informational spillovers.

Another possible explanation for the lack of evidence of local spillovers in the later episodes is that the earlier law changes in the region (in Vermont and Rhode Island) may have provided information for the New England region as a whole. If these law changes and their effects on filings were publicized region-wide then perhaps enough information was disseminated to swamp the local spillover effects of the later law changes. This line of argument would also point to the possible role of local media markets in disseminating information and reducing stigma.[8]

The magnitude of the estimated spillovers in VT 1997 and RI 1999 can be computed as a fraction of the overall filing rate in these states and their neighbors over the period under consideration. In both cases, the overall filing rate oscillates around roughly 10 filings per 100,000 residents, over four-week periods (see Figure 2). The estimated spillover effect varies between 0.8 in RI 1999 and 1.6 in VT 1997; relative to the baseline filing rate of ten per 100,000 these coefficient estimates imply an increase in the propensity to file between 8% and 16%. These magnitudes suggest a substantial role for social spillovers. For comparison, Fay et al. (2002) report that an increase in homestead exemption from about \$25,000 to \$60,000 in their sample would yield a 16% increase in the number of bankruptcy filings. A one standard deviation increase in the district-wide filing rate yields a 31% increase in filings in the district.

Table 3 reports the results of the estimation of model (2), which looks at patterns over time. The structure of the Table is the same as for Table 2, with the main columns reporting results for different definitions of distance variables, and with both weekly and monthly data being used. The key differences here with respect to Table 2 are that (a) we are looking at the *changes* in filing rates over time after the law change EDs, and (b) we are using elapsed time since the law change instead of the simple before-after law change dummy variable.

Focusing again on VT 1997 and RI 1999, we observe a consistent pattern across most specifications: the coefficient associated with the distance dummy is significantly positive, the coefficient associated with the elapsed time variable tends to be negative (albeit not always significantly), and the coefficient associated with the cross-term is negative (and significantly so in most RI 1999 specifications). Since the observations in these specifications are the first-differences of filing rates, this pattern of coefficient estimates is consistent with the presence of a *temporary* local spillover from ***A*** to ***B***-close: filing rates initially rise faster in ***B***-close than in ***B***-far following the law change, level off at a higher level in ***B***-close than in ***B***-far for a while, and eventually return to the same baseline level in both sets of zipcodes.

An important caveat arises from an examination of the observable demographic and economic attributes in ***B***-close and ***B***-far, in the various episodes under consideration. A requirement of the difference-in-difference approach is that the exchangeability assumption be satisfied, i.e. that the two sets ***B***-close and ***B***-far be comparable in terms of their observed and unobserved attributes. Table A1 reports means, medians and standard deviations for a set of observable attributes in the two sets of zipcodes, for the two earlier law change episodes, VT 1997 and RI 1999. The last column reports the test statistic for the null hypothesis that the two population means be the same. As Table A1 shows, the demographic composition of the zipcodes in ***B***-close and ***B***-far tends to be significantly different with regard to their racial/ethnic composition, median incomes, housing values and rents, and household size. Therefore, the evidence presented so far in support of the presence of local social spillovers has to be considered only suggestive. We address this issue more fully in the robustness exercises that we discuss next.

Estimates using socio-economic distance.

As we discuss in Section 3, we perform several robustness exercises using alternative definitions of socio-economic distance, namely racial/ethnic composition, median housing values, and percentage of college-educated residents.[9] This analysis serves a dual purpose. On the one hand, incorporating demographic attributes into our notion of "closeness" helps us focus on dimensions

[8] Local media markets could also provide an alternative channel through which information may spill over from a change state to neighboring states. We plan to use detailed zipcode-level information on the boundaries of local media markets to explore this possibility.

[9] In a companion paper we find that these three attributes are strongly associated with bankruptcy filing rates, using a national cross-section of zipcodes in calendar year 2000.

(other than geographic distance) that have been found to be important determinants of distance in personal networks.

On the other hand, by restricting our analysis to the subset of zipcodes in B that are most similar to A along various observed attributes, we also force the two sets B-close and B-far to be more similar to each other at least in terms of these observed attributes, thus making it more likely that the exchangeability assumption be satisfied. In fact, Tables A2 through A4 report summary statistics for the same set of observed attributes as in Table A1, using the restricted set of zipcodes that are most similar to ***A*** according to the notion of socio-economic distance used in each case. These Tables indicate that the newly defined sets ***B***-close and ***B***-far tend to be much more similar to each other when using these alternative socio-economic distance measures (especially for housing value distance), thus lending more credibility to the results of our difference-in-difference analysis.

Tables 4, 5 and 6 report the results of our main specification (1) for each of the three alternative distance measures.[10] The results seem remarkably stable across specifications. Again, there is some evidence of local social spillover effects in the earlier law change episodes in VT 1997 and RI 1999. The magnitude of the estimated effects ranges from about 0.8 to about 2.1, slightly larger than in our baseline specification in Table 2. This suggests that our findings, while mixed, seem to be fairly robust to using stricter definitions of distance and more comparable treatment and control groups.

CONCLUSION

We used a high-frequency, geocoded dataset on personal bankruptcies to detect the presence of social spillovers in filings. Our approach exploits the existence of legislative changes in several states over time in the exemption levels for assets. We use a simple difference-in-differences design, comparing the change in filing rates before and after the law changes between zipcodes located close to the border with a change state (and thus presumably more exposed to local social spillovers) and other zipcodes located further away from the change state.

The results are mixed, but there is suggestive evidence that spillovers may have played a role in the two earlier episodes considered here. In our main specification, the magnitude of the estimated spillovers ranges from about 8% to about 16% of the baseline filing rate in these two areas. The evidence also suggests that these social spillovers may be temporary, since any

[10] The results of the specification in first differences are also similar to our baseline findings in Table 3 and are omitted for the sake of brevity. They are available from the authors upon request.

increase in filings in ***B***-close relative to ***B***-far seems to dissipate within a year from the legislative changes. These findings are robust to the use of several alternative definitions of socio-economic distance to define sets of zipcodes that are closest to change states with respect to dimensions along which social networks are more likely to form.

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Figure 1a – Vermont

(pink = rural A; red = urban A; purple = urban B_close; blue = urban B_far; light blue = rural B)

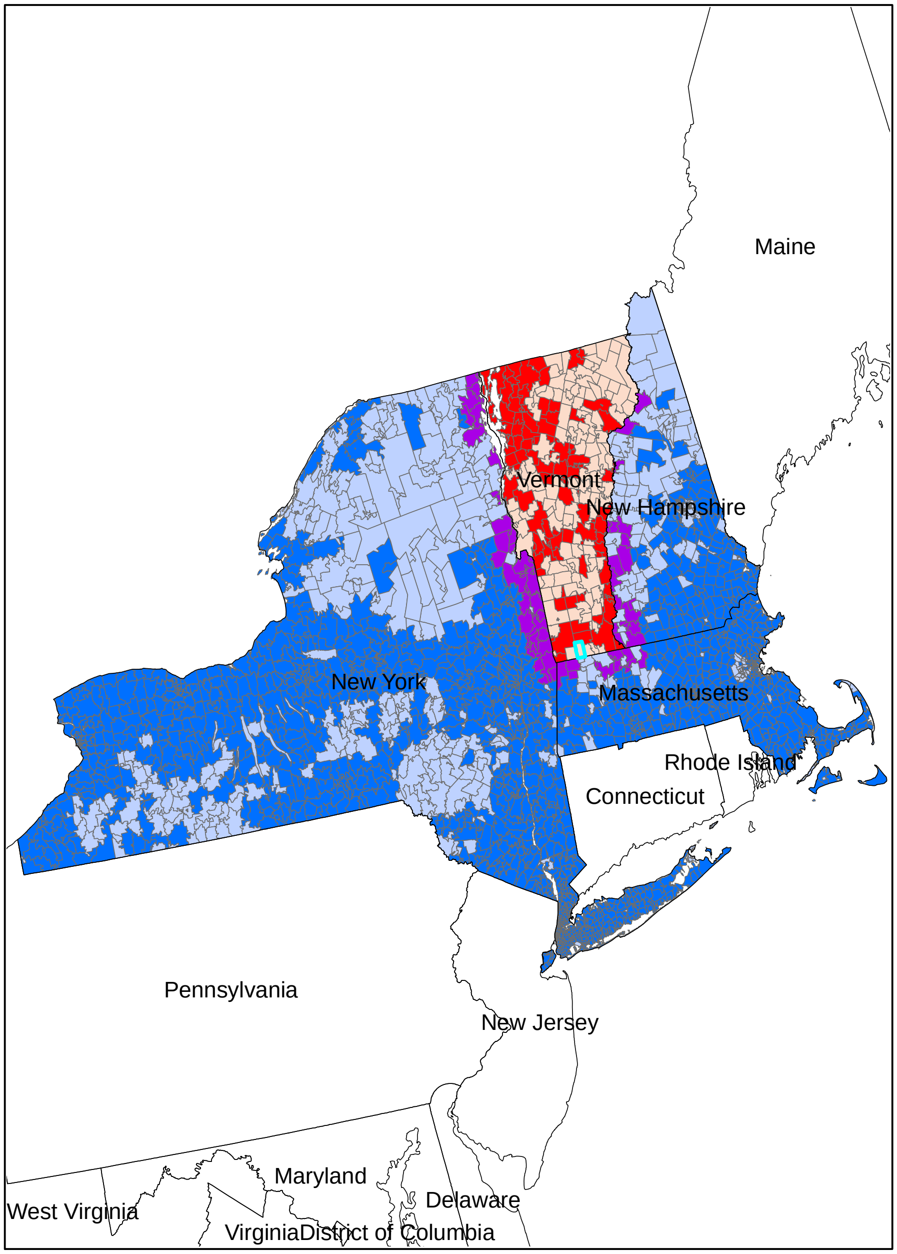


Figure 1b – Rhode Island

(pink = rural A; red = urban A; purple = urban B_close; blue = urban B_far; light blue = rural B)

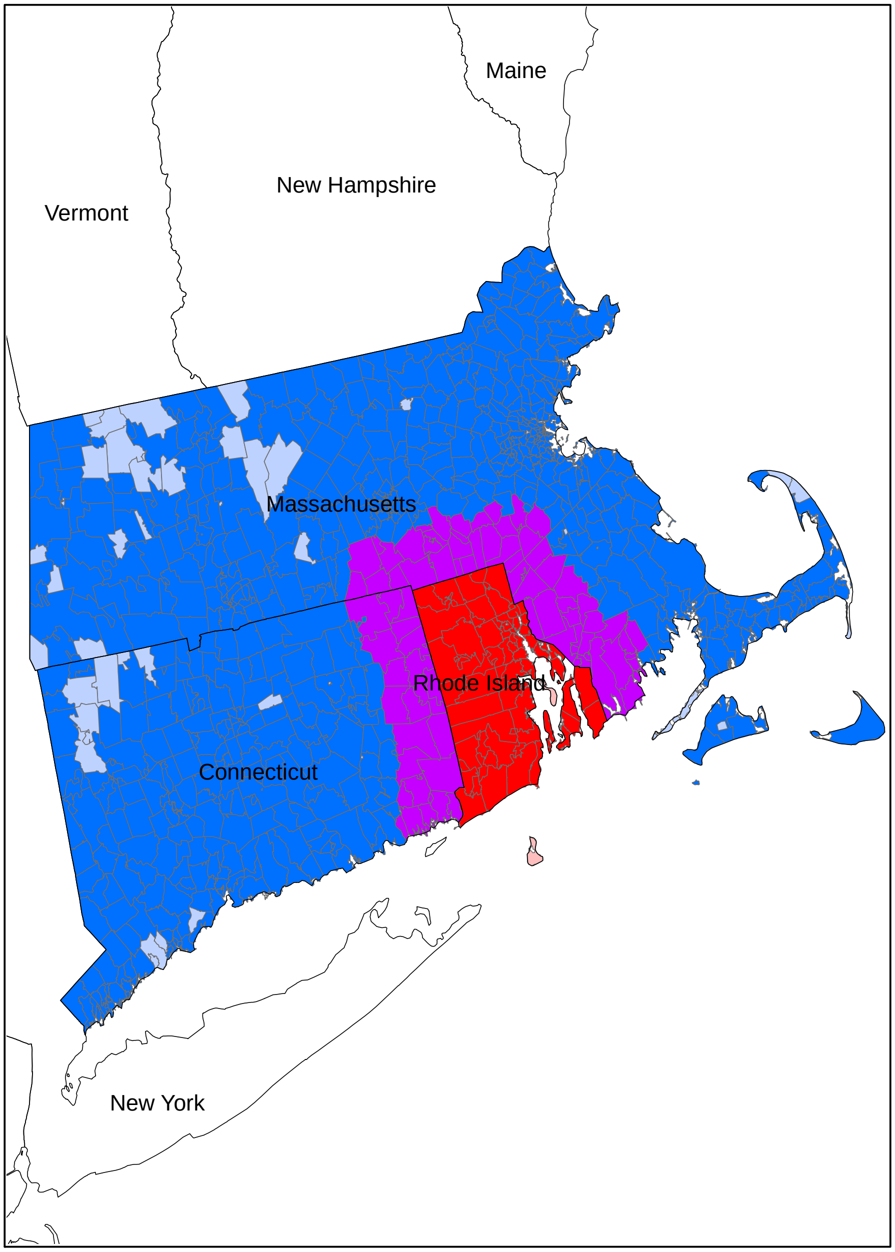


Figure 1c – Massachusetts

(pink = rural A; red = urban A; purple = urban B_close; blue = urban B_far; light blue = rural B)

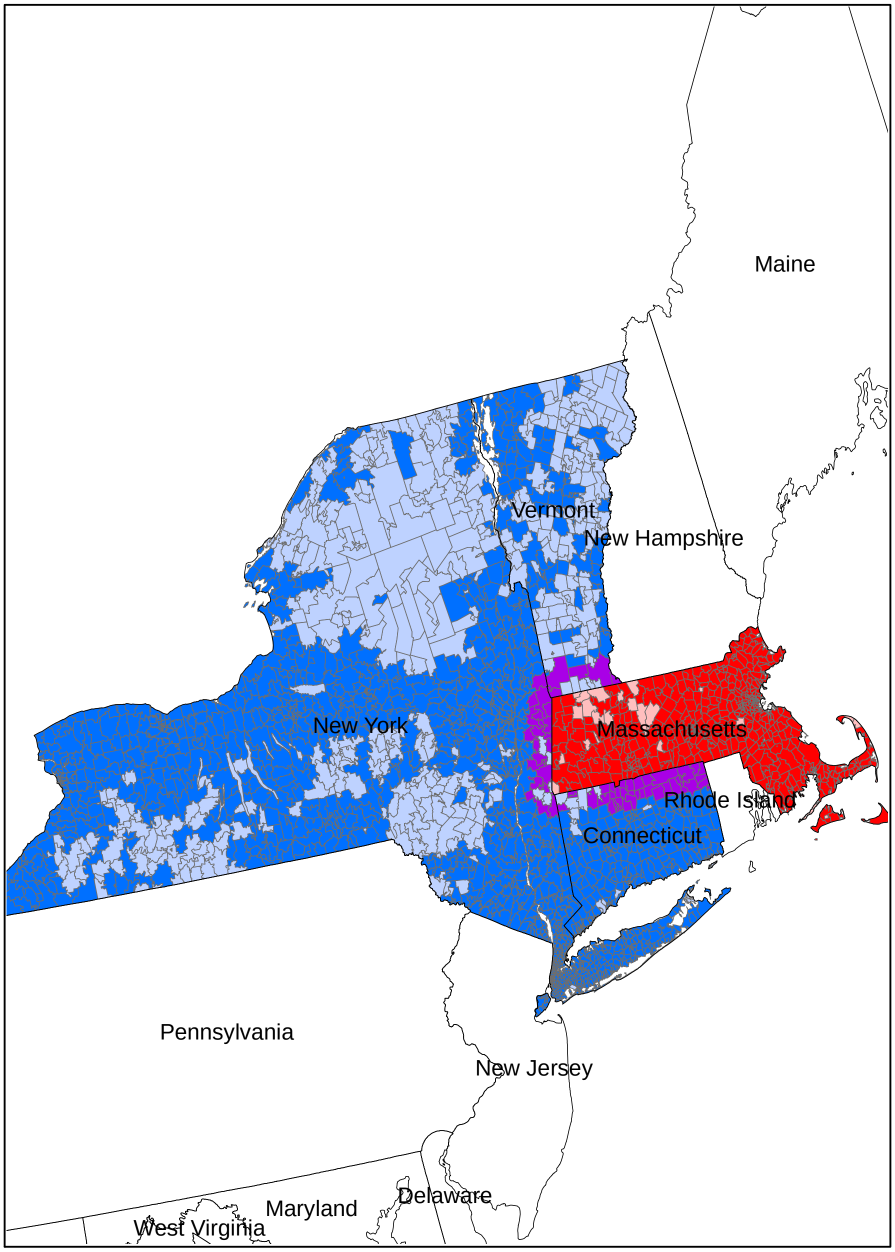
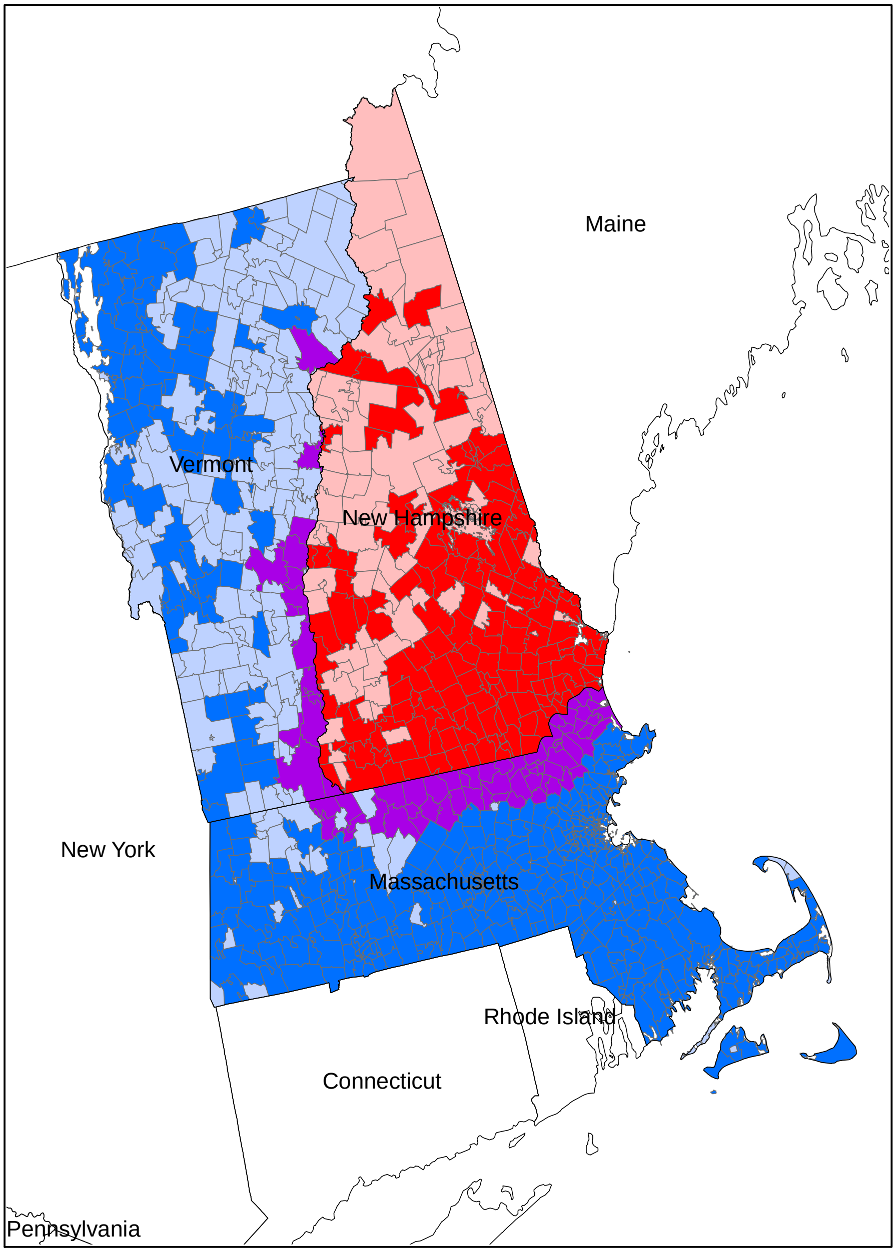
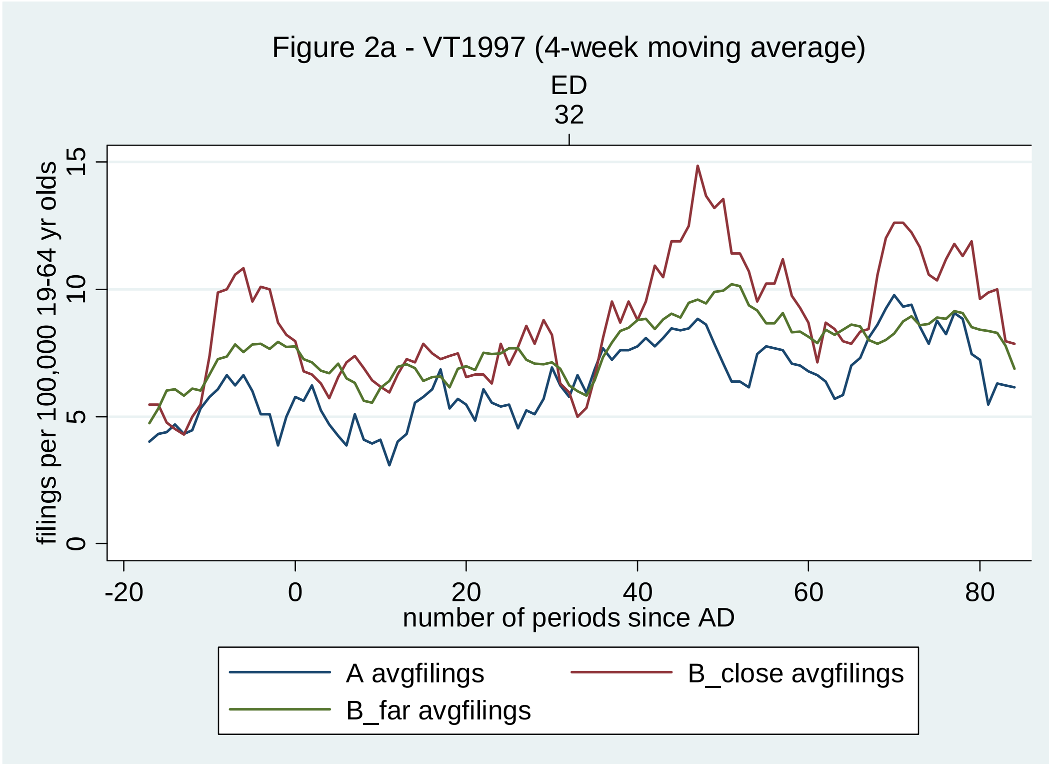
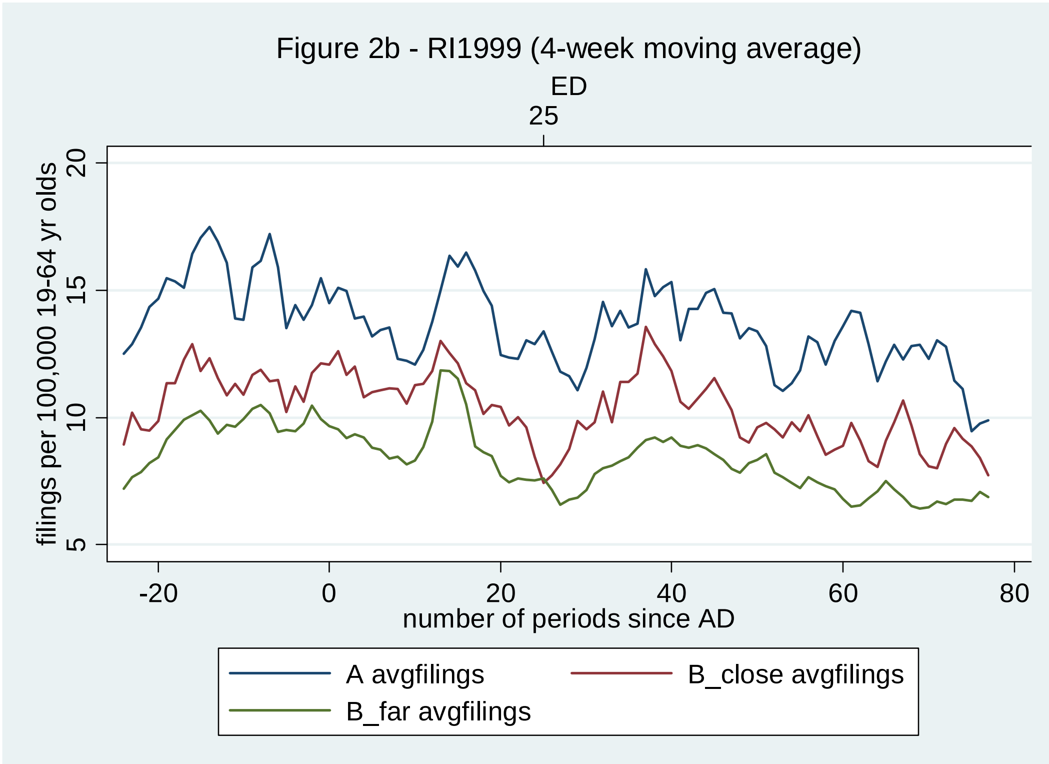


Figure 1d – New Hampshire
(pink = rural A; red = urban A; purple = urban B_close; blue = urban B_far; light blue = rural B)

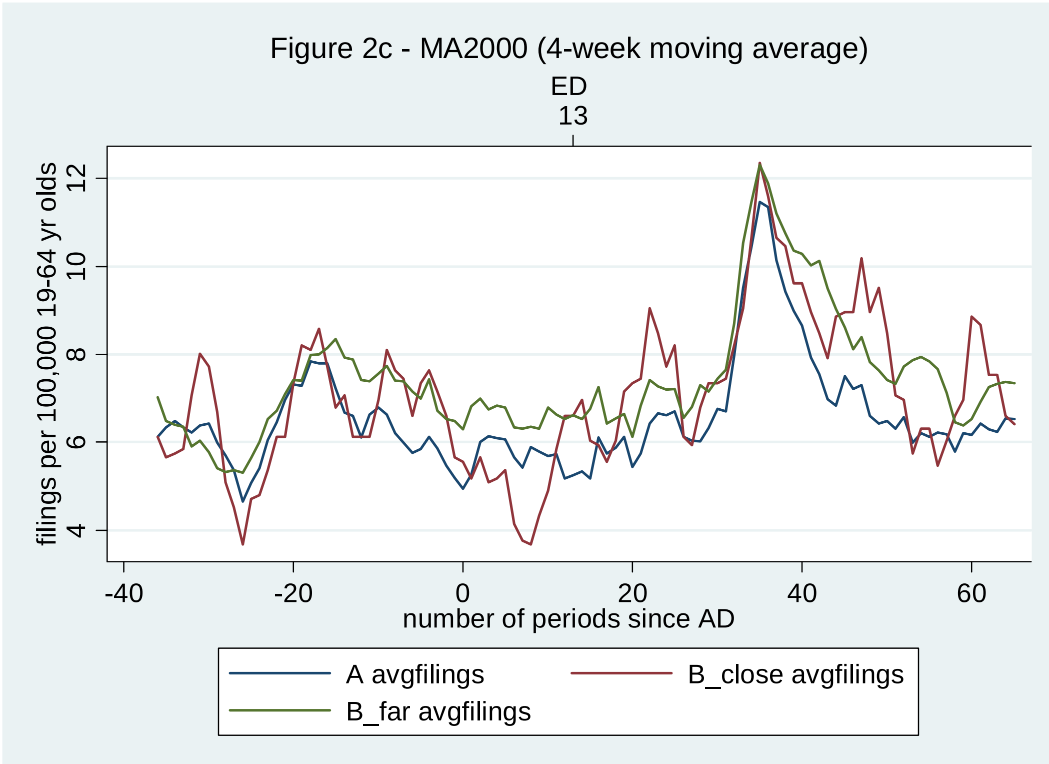




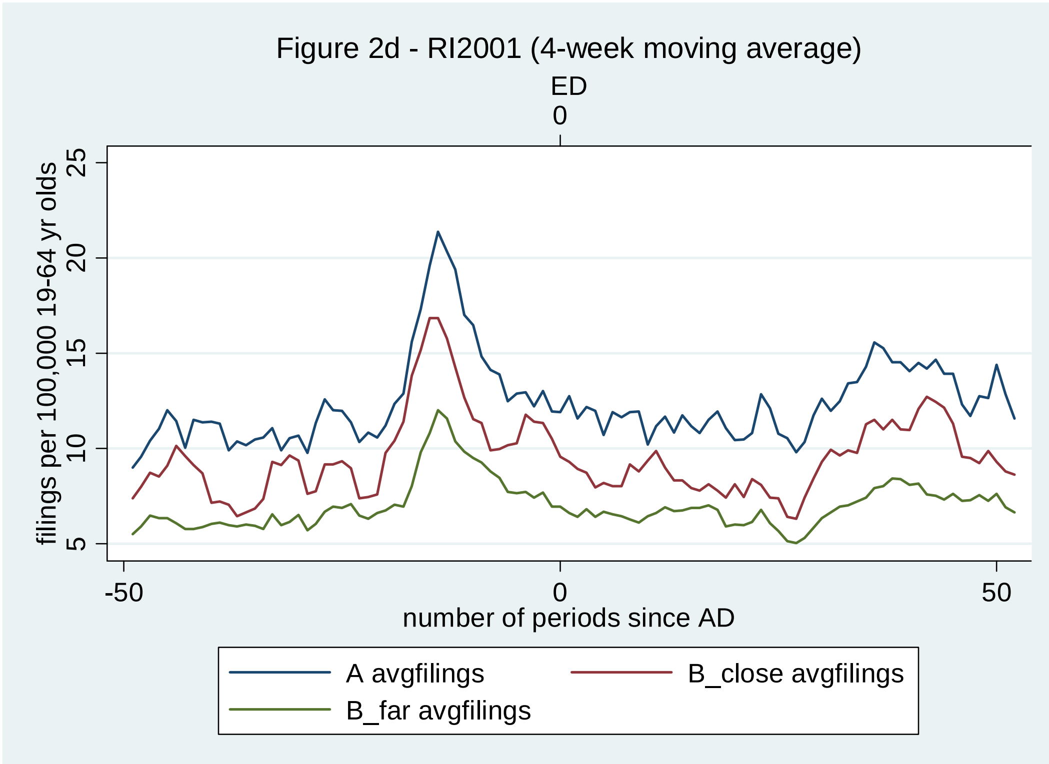
Type	Number of ZIP Codes
A	103
B_close	70
B_far	1845



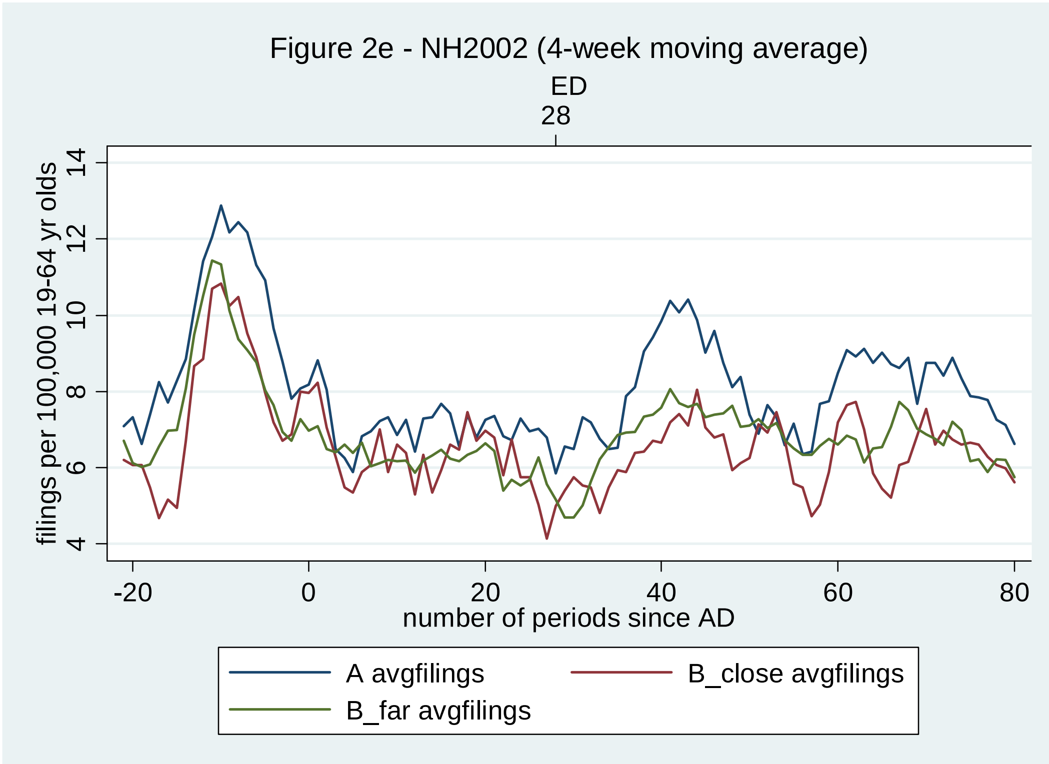
Type	Number of ZIP Codes
A	71
B_close	82
B_far	651



Type	Number of ZIP Codes
A	471
B_close	72
B_far	1616



Type	Number of ZIP Codes
A	71
B_close	82
B_far	649



Type	Number of ZIP Codes
A	157
B_close	81
B_far	488

Figure 3 – Weekly Nonbusiness Bankruptcy Filings

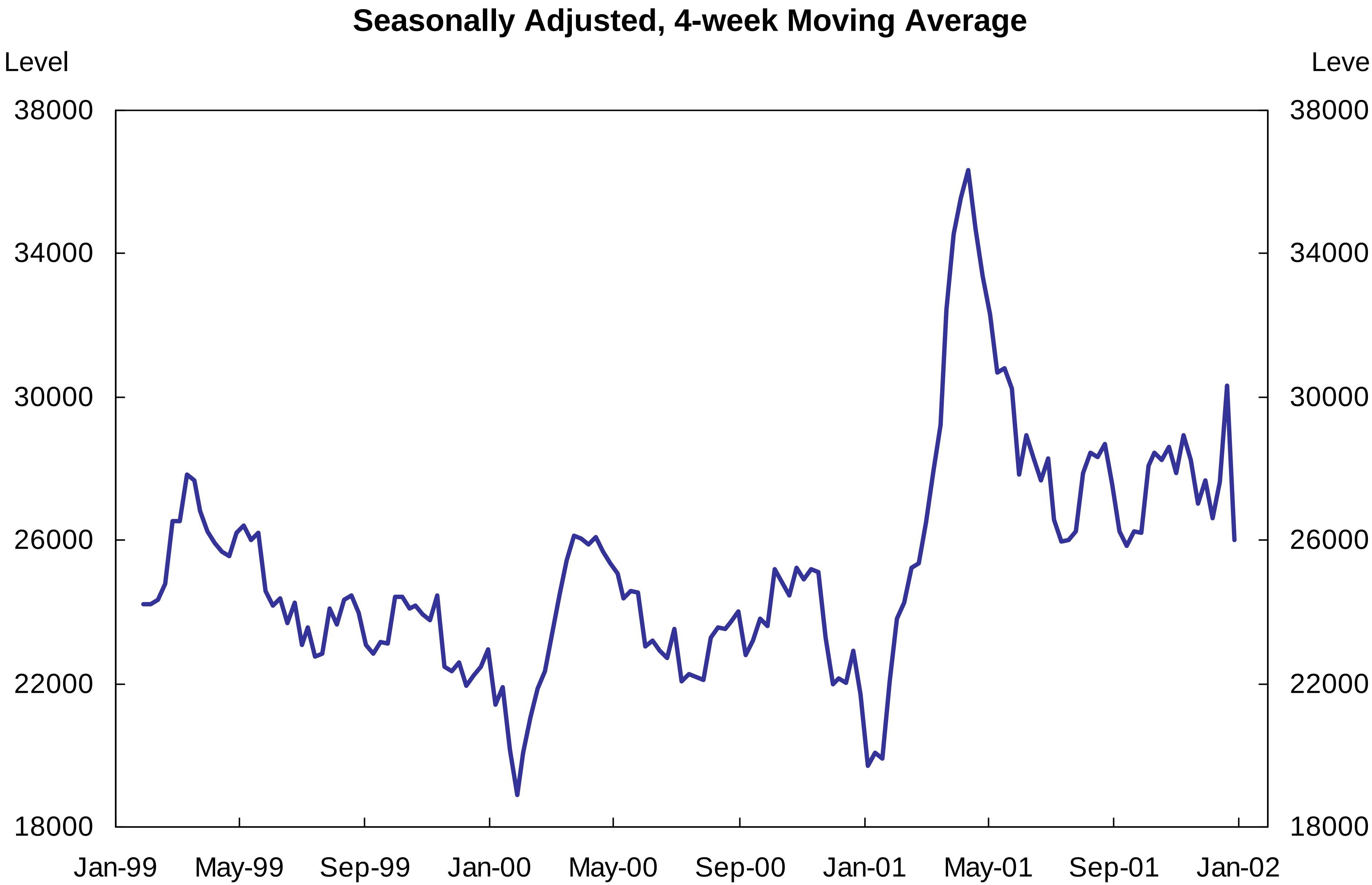


Table 1
Law change information

State	Passage Date	Effective Date	Time window	Adjacent states	Old exemption	New exemption
VT	5/22/1996	1/1/1997	5/95-1/98	NY NH MA	$30,000	$75,000
RI	7/9/1998	1/1/1999	7/97-1/00	CT MA	$15,000	$100,000
MA	8/4/2000	11/2/2000	8/99-11/01	VT NY CT	$100,000	$300,000
RI	7/13/2001	7/13/2001	7/00-7/02	CT MA	$100,000	$150,000
NH	6/19/2001	1/1/2002	6/00-1/03	VT MA	$30,000	$50,000

Table 2
Regressions in levels

VT1997	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	1.6998	1.9435	1.6886	1.8122	0.0002	0.0005
	(0.002)	(0.001)	(0.000)	(0.000)	(0.826)	(0.609)
Law change dummy	2.1450	2.0740	2.1421	2.0705	2.3612	2.1089
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
cross term	1.6244	1.3199	1.1040	0.9167	0.0009	-0.0001
	(0.040)	(0.127)	(0.087)	(0.193)	(0.523)	(0.960)

RI1999	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	1.6053	1.6682	1.4053	1.6451	0.0080	0.0099
	(0.000)	(0.000)	(0.000)	(0.000)	(0.013)	(0.003)
Law change dummy	-1.3820	-1.2522	-1.4797	-1.2744	-0.8150	-0.9649
	(0.000)	(0.000)	(0.000)	(0.000)	(0.011)	(0.005)
cross term	0.5232	0.3294	0.8169	0.3085	0.0083	0.0041
	(0.282)	(0.524)	(0.036)	(0.457)	(0.070)	(0.402)

MA2000	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	-1.3781	-1.4560	-1.1860	-1.2699	-0.0040	-0.0036
	(0.053)	(0.047)	(0.033)	(0.026)	(0.002)	(0.007)
Law change dummy	1.4716	1.4301	1.4818	1.4404	1.3004	1.1327
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)
cross term	0.3307	0.2904	0.0546	0.0285	-0.0012	-0.0019
	(0.744)	(0.783)	(0.945)	(0.972)	(0.527)	(0.311)

RI2001	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	2.3368	2.3064	2.8062	2.7243	0.0200	0.0198
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Law change dummy	-0.5759	-0.4706	-0.3973	-0.3214	-0.8311	-0.7199
	(0.000)	(0.011)	(0.022)	(0.098)	(0.010)	(0.048)
cross term	-0.1765	-0.2311	-1.0333	-0.9121	-0.0038	-0.0036
	(0.720)	(0.676)	(0.009)	(0.040)	(0.408)	(0.485)

NH2002	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	-0.0677	-0.0794	-1.0433	-1.0197	-0.0203	-0.0198
	(0.867)	(0.856)	(0.003)	(0.007)	(0.000)	(0.000)
Law change dummy	-0.0452	-0.0212	-0.1242	-0.0848	0.2330	0.2032
	(0.835)	(0.929)	(0.583)	(0.734)	(0.537)	(0.625)
cross term	-0.1988	-0.1431	0.2389	0.2037	0.0052	0.0041
	(0.729)	(0.821)	(0.627)	(0.707)	(0.338)	(0.487)

The signs of the coefficient estimates for the "continuous distance" variables are reversed for comparability with the other distance dummy variable coefficients. P-values are reported in parentheses.

Table 3
Regressions in first differences

VT1997	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	4.5519	5.9560	3.2849	4.3766	0.0036	0.0061
	(0.001)	(0.000)	(0.005)	(0.002)	(0.160)	(0.046)
elapsed time	-0.0013	0.0311	-0.0014	0.0307	-0.0082	-0.0253
	(0.891)	(0.493)	(0.880)	(0.504)	(0.640)	(0.763)
cross term	-0.0374	-0.0519	-0.0217	-0.0196	-0.0001	-0.0003
	(0.430)	(0.820)	(0.574)	(0.915)	(0.716)	(0.440)

RI1999	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	2.6000	2.2880	4.2513	3.4933	0.0312	0.0220
	(0.002)	(0.013)	(0.000)	(0.000)	(0.000)	(0.009)
elapsed time	-0.0355	0.0068	-0.0256	0.0395	-0.0740	-0.1240
	(0.000)	(0.864)	(0.009)	(0.345)	(0.000)	(0.115)
cross term	-0.0318	-0.1278	-0.0712	-0.2517	-0.0006	-0.0019
	(0.259)	(0.297)	(0.001)	(0.009)	(0.029)	(0.091)

MA2000	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	-0.3416	0.5138	-1.0567	-0.3862	-0.0007	-0.0005
	(0.812)	(0.747)	(0.350)	(0.758)	(0.792)	(0.861)
elapsed time	0.0025	0.0598	0.0014	0.0574	-0.0216	0.0079
	(0.807)	(0.189)	(0.892)	(0.214)	(0.213)	(0.918)
cross term	-0.0072	-0.1060	0.0097	-0.0323	-0.0002	-0.0003
	(0.880)	(0.612)	(0.795)	(0.845)	(0.092)	(0.456)

RI2001	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	1.5016	0.5408	1.3375	0.7943	0.0119	0.0047
	(0.021)	(0.454)	(0.009)	(0.162)	(0.044)	(0.464)
elapsed time	0.0113	0.0958	0.0107	0.0896	0.0219	0.1481
	(0.111)	(0.002)	(0.150)	(0.005)	(0.117)	(0.016)
cross term	0.0145	0.0478	0.0113	0.0593	0.0001	0.0007
	(0.505)	(0.618)	(0.509)	(0.430)	(0.461)	(0.381)

NH2002	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	0.2800	-0.4714	-0.6361	-0.5209	-0.0143	-0.0120
	(0.769)	(0.673)	(0.433)	(0.588)	(0.108)	(0.260)
elapsed time	0.0065	0.0081	0.0029	0.0041	0.0160	0.0701
	(0.589)	(0.889)	(0.817)	(0.946)	(0.441)	(0.479)
cross term	-0.0055	0.1415	0.0128	0.1109	0.0002	0.0007
	(0.861)	(0.346)	(0.634)	(0.388)	(0.555)	(0.614)

The signs of the coefficient estimates for the "continuous distance" variables are reversed for comparability with the other distance dummy variable coefficients. P-values are reported in parentheses.

Table 4
Regressions in levels - housing value distance

VT1997	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	-0.6799	-0.6523	-0.2107	-0.2306	0.0020	0.0022
	(0.231)	(0.283)	(0.645)	(0.637)	(0.080)	(0.065)
Law change dummy	2.4108	2.2827	2.4001	2.2698	2.5879	2.3696
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
cross term	1.0209	1.3891	0.7412	0.9925	0.0007	0.0000
	(0.206)	(0.113)	(0.254)	(0.159)	(0.654)	(0.996)

RI1999	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	0.1239	0.1818	0.2070	0.5648	0.0090	0.0124
	(0.793)	(0.708)	(0.605)	(0.169)	(0.090)	(0.023)
Law change dummy	-1.7487	-1.7273	-2.0053	-1.7917	-0.7777	-1.2625
	(0.000)	(0.000)	(0.000)	(0.000)	(0.119)	(0.015)
cross term	0.9759	0.9006	1.5959	0.8173	0.0141	0.0055
	(0.146)	(0.197)	(0.005)	(0.167)	(0.062)	(0.485)

MA2000	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	-0.2108	-0.3351	-0.2840	-0.3839	0.0034	0.0038
	(0.686)	(0.550)	(0.493)	(0.388)	(0.017)	(0.013)
Law change dummy	1.5543	1.4512	1.5610	1.4580	1.7103	1.5103
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
cross term	0.0326	0.0895	-0.0359	-0.0023	0.0013	0.0004
	(0.965)	(0.912)	(0.951)	(0.997)	(0.531)	(0.844)

RI2001	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	1.3798	1.3431	2.7357	2.6536	0.0247	0.0250
	(0.007)	(0.017)	(0.000)	(0.000)	(0.000)	(0.000)
Law change dummy	-0.9913	-0.8839	-0.5365	-0.4484	-1.2699	-1.1816
	(0.001)	(0.008)	(0.089)	(0.205)	(0.019)	(0.051)
cross term	0.2205	0.1898	-1.5845	-1.5308	-0.0055	-0.0058
	(0.761)	(0.815)	(0.010)	(0.026)	(0.500)	(0.530)

NH2002	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	-0.3648	-0.3322	-1.1832	-1.1500	-0.0284	-0.0275
	(0.506)	(0.563)	(0.017)	(0.026)	(0.000)	(0.000)
Law change dummy	-0.0272	0.0509	-0.1553	-0.0692	-0.4412	-0.4277
	(0.934)	(0.884)	(0.647)	(0.848)	(0.445)	(0.485)
cross term	-0.6923	-0.7112	0.0247	-0.0241	-0.0049	-0.0060
	(0.375)	(0.390)	(0.972)	(0.974)	(0.556)	(0.501)

The signs of the coefficient estimates for the "continuous distance" variables are reversed for comparability with the other distance dummy variable coefficients. P-values are reported in parentheses.

Table 5
Regressions in levels - college education distance

VT1997	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	-0.7705	-0.6993	-0.1388	-0.1761	0.0002	0.0006
	(0.235)	(0.302)	(0.785)	(0.741)	(0.891)	(0.631)
Law change dummy	2.2370	2.0834	2.2652	2.1039	2.4831	2.2073
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
cross term	1.4875	2.1304	0.4449	0.9518	0.0011	0.0003
	(0.107)	(0.029)	(0.539)	(0.215)	(0.489)	(0.881)

RI1999	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	0.2779	0.3940	0.7274	1.1546	0.0073	0.0112
	(0.588)	(0.454)	(0.079)	(0.006)	(0.149)	(0.030)
Law change dummy	-1.8763	-1.8250	-2.1098	-1.8964	-0.9327	-1.4657
	(0.000)	(0.000)	(0.000)	(0.000)	(0.062)	(0.005)
cross term	0.5962	0.4473	1.4551	0.6000	0.0143	0.0050
	(0.414)	(0.555)	(0.013)	(0.325)	(0.047)	(0.504)

MA2000	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	0.1609	0.0104	-0.0610	-0.1808	-0.0028	-0.0025
	(0.812)	(0.988)	(0.908)	(0.746)	(0.044)	(0.088)
Law change dummy	1.6634	1.6379	1.6575	1.6318	1.6895	1.5327
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
cross term	-0.6106	-0.4758	-0.2969	-0.2158	0.0004	-0.0005
	(0.526)	(0.643)	(0.694)	(0.788)	(0.838)	(0.803)

RI2001	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	0.6494	0.6735	3.1032	3.0118	0.0239	0.0241
	(0.227)	(0.258)	(0.000)	(0.000)	(0.000)	(0.000)
Law change dummy	-0.9139	-0.8282	-0.4271	-0.3627	-1.1482	-1.1834
	(0.001)	(0.006)	(0.131)	(0.254)	(0.028)	(0.044)
cross term	0.8473	0.7712	-1.8058	-1.7497	-0.0056	-0.0074
	(0.267)	(0.369)	(0.003)	(0.011)	(0.459)	(0.381)

NH2002	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	-0.1033	-0.1328	-0.9560	-0.9657	-0.0102	-0.0101
	(0.817)	(0.781)	(0.015)	(0.022)	(0.018)	(0.029)
Law change dummy	-0.1614	-0.0646	-0.2308	-0.1241	-0.2546	-0.1560
	(0.537)	(0.820)	(0.398)	(0.676)	(0.563)	(0.745)
cross term	-0.5143	-0.4436	-0.0742	-0.0655	-0.0001	-0.0003
	(0.418)	(0.520)	(0.894)	(0.914)	(0.987)	(0.968)

The signs of the coefficient estimates for the "continuous distance" variables are reversed for comparability with the other distance dummy variable coefficients. P-values are reported in parentheses.

Table 6
Regressions in levels - racial/ethnic distance

VT1997	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	1.5377	1.7940	1.2474	1.3854	-0.0023	-0.0019
	(0.037)	(0.025)	(0.040)	(0.035)	(0.130)	(0.253)
Law change dummy	2.5492	2.5346	2.5435	2.5317	2.8612	2.5984
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
cross term	1.5461	1.1373	1.0598	0.7673	0.0013	-0.0001
	(0.141)	(0.324)	(0.220)	(0.418)	(0.548)	(0.971)

RI1999	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	1.0011	1.0382	0.9645	1.3586	0.0190	0.0214
	(0.048)	(0.044)	(0.024)	(0.002)	(0.000)	(0.000)
Law change dummy	-1.3222	-1.2174	-1.5484	-1.2713	-0.5521	-0.8159
	(0.000)	(0.000)	(0.000)	(0.000)	(0.281)	(0.124)
cross term	0.8496	0.7928	1.5035	0.7357	0.0106	0.0046
	(0.237)	(0.287)	(0.013)	(0.240)	(0.147)	(0.539)

MA2000	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	-2.9607	-3.0672	-2.7845	-2.8593	-0.0010	-0.0007
	(0.004)	(0.004)	(0.001)	(0.001)	(0.615)	(0.746)
Law change dummy	1.1975	1.2491	1.2008	1.2600	0.8748	0.8257
	(0.002)	(0.002)	(0.002)	(0.002)	(0.160)	(0.200)
cross term	0.7837	0.7986	0.4823	0.4182	-0.0022	-0.0027
	(0.594)	(0.600)	(0.691)	(0.739)	(0.453)	(0.357)

RI2001	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	1.4285	1.4052	3.1338	3.0047	0.0320	0.0313
	(0.008)	(0.018)	(0.000)	(0.000)	(0.000)	(0.000)
Law change dummy	-0.9261	-0.8448	-0.4694	-0.4348	-1.4364	-1.3433
	(0.002)	(0.011)	(0.137)	(0.213)	(0.009)	(0.027)
cross term	0.0712	-0.0031	-1.8874	-1.7373	-0.0086	-0.0082
	(0.926)	(0.997)	(0.004)	(0.016)	(0.272)	(0.343)

NH2002	20Km		30Km		continuous distance	
	weeks	months	weeks	months	weeks	months
Distance variable	0.0253	-0.0339	-1.1848	-1.1879	-0.0063	-0.0061
	(0.961)	(0.951)	(0.008)	(0.012)	(0.192)	(0.238)
Law change dummy	-0.2551	-0.2263	-0.3645	-0.3129	0.1570	0.1748
	(0.399)	(0.489)	(0.253)	(0.364)	(0.755)	(0.748)
cross term	0.1674	0.2998	0.5436	0.5445	0.0062	0.0057
	(0.818)	(0.703)	(0.389)	(0.425)	(0.362)	(0.443)

The signs of the coefficient estimates for the "continuous distance" variables are reversed for comparability with the other distance dummy variable coefficients. P-values are reported in parentheses.

Table A1
Summary statistics for attributes in B-close and B-far: all urban zipcodes

Vermont 1997

	B-close			B-far			
	mean	median	std.dev.	mean	median	std.dev.	t-statistic
percentage black	0.014	0.003	0.060	0.061	0.010	0.143	-3.098
percentage hispanic	0.013	0.006	0.031	0.060	0.018	0.109	-3.128
percentage asian	0.005	0.005	0.006	0.025	0.009	0.051	-2.131
percentage female	0.500	0.509	0.057	0.509	0.512	0.033	-0.142
percentage with at least college degree	0.295	0.283	0.114	0.359	0.332	0.152	-1.114
median household income	41533	40500	7057	50547	46885	19993	-9.034
percentage unemployed	0.052	0.048	0.032	0.055	0.043	0.045	-0.089
median housing value	98214	89000	35926	158018	133400	105202	-11.681
median rent	533	519	100	682	629	241	-10.846
vacancy rate	0.156	0.111	0.149	0.112	0.059	0.135	0.958
median household size	2.077	2.000	0.269	2.188	2.000	0.455	-3.180
percentage renters	0.244	0.231	0.105	0.304	0.234	0.211	-1.093

Rhode Island 1999

	B-close			B-far			
	mean	median	std.dev.	mean	median	std.dev.	t-statistic
percentage black	0.015	0.007	0.024	0.044	0.011	0.100	-1.848
percentage hispanic	0.026	0.013	0.038	0.054	0.018	0.101	-1.446
percentage asian	0.011	0.008	0.010	0.026	0.013	0.041	-1.110
percentage female	0.510	0.511	0.017	0.514	0.516	0.022	-0.068
percentage with at least college degree	0.322	0.322	0.121	0.430	0.410	0.170	-1.947
median household income	53838	55519	14731	59474	56089	21749	-3.039
percentage unemployed	0.043	0.036	0.026	0.045	0.035	0.036	-0.092
median housing value	156122	147200	42796	207842	172550	117150	-7.738
median rent	623	610	123	759	713	230	-8.279
vacancy rate	0.050	0.040	0.034	0.082	0.040	0.112	-1.209
median household size	2.259	2.000	0.441	2.138	2.000	0.405	2.344
percentage renters	0.273	0.207	0.171	0.291	0.226	0.203	-0.348

The t-statistic reported in the last column is for the test of the null hypothesis that the two population means be the same across B-close and B-far.

Table A2

Summary statistics for attributes in B-close and B-far: housing value distance

Vermont 1997

	B-close			B-far			
	mean	median	std.dev.	mean	median	std.dev.	t-statistic
percentage black	0.013	0.003	0.064	0.038	0.008	0.089	-1.495
percentage hispanic	0.012	0.006	0.032	0.044	0.013	0.096	-1.946
percentage asian	0.005	0.005	0.005	0.012	0.005	0.027	-0.729
percentage female	0.501	0.508	0.060	0.507	0.509	0.033	-0.074
percentage with at least college degree	0.289	0.286	0.093	0.317	0.298	0.108	-0.448
median household income	41292	40500	6211	44553	43516	10298	-3.644
percentage unemployed	0.052	0.047	0.030	0.053	0.045	0.037	-0.060
median housing value	95468	89700	18836	105618	101800	25742	-3.835
median rent	525	519	74	590	572	134	-5.976
vacancy rate	0.144	0.088	0.129	0.130	0.073	0.144	0.296
median household size	2.070	2.000	0.258	2.101	2.000	0.351	-0.853
percentage renters	0.249	0.224	0.107	0.277	0.230	0.171	-0.474

Rhode Island 1999

	B-close			B-far			
	mean	median	std.dev.	mean	median	std.dev.	t-statistic
percentage black	0.013	0.007	0.016	0.041	0.011	0.093	-1.452
percentage hispanic	0.027	0.015	0.037	0.046	0.019	0.078	-0.786
percentage asian	0.011	0.009	0.009	0.017	0.009	0.026	-0.374
percentage female	0.512	0.511	0.014	0.513	0.514	0.021	-0.020
percentage with at least college degree	0.284	0.283	0.090	0.351	0.343	0.107	-1.037
median household income	50068	52103	11980	51728	51677	10688	-0.994
percentage unemployed	0.047	0.038	0.028	0.043	0.039	0.025	0.143
median housing value	141645	141950	20041	145124	145800	20612	-1.217
median rent	590	587	103	666	660	102	-5.175
vacancy rate	0.053	0.044	0.031	0.078	0.043	0.091	-0.747
median household size	2.167	2.000	0.376	2.083	2.000	0.301	1.619
percentage renters	0.295	0.220	0.177	0.274	0.236	0.160	0.330

The t-statistic reported in the last column is for the test of the null hypothesis that the two population means be the same across B-close and B-far.

Table A3
Summary statistics for attributes in B-close and B-far: college education distance

Vermont 1997

	B-close			B-far			
	mean	median	std.dev.	mean	median	std.dev.	t-statistic
percentage black	0.005	0.003	0.005	0.049	0.010	0.122	-3.148
percentage hispanic	0.007	0.005	0.006	0.048	0.016	0.081	-2.602
percentage asian	0.005	0.005	0.005	0.023	0.008	0.055	-1.343
percentage female	0.509	0.511	0.023	0.511	0.512	0.023	-0.023
percentage with at least college degree	0.330	0.322	0.048	0.327	0.324	0.056	0.033
median household income	43430	42768	5772	47880	46800	11429	-4.239
percentage unemployed	0.052	0.047	0.035	0.050	0.043	0.033	0.061
median housing value	99143	93200	19623	135986	122700	62165	-9.377
median rent	543	534	86	648	616	161	-6.776
vacancy rate	0.148	0.086	0.135	0.118	0.059	0.144	0.494
median household size	2.086	2.000	0.284	2.179	2.000	0.395	-1.862
percentage renters	0.233	0.204	0.107	0.284	0.234	0.172	-0.697

Rhode Island 1999

	B-close			B-far			
	mean	median	std.dev.	mean	median	std.dev.	t-statistic
percentage black	0.014	0.006	0.030	0.040	0.011	0.090	-1.215
percentage hispanic	0.021	0.012	0.030	0.040	0.019	0.060	-0.820
percentage asian	0.010	0.008	0.008	0.019	0.009	0.046	-0.559
percentage female	0.509	0.508	0.014	0.512	0.514	0.022	-0.041
percentage with at least college degree	0.317	0.322	0.054	0.352	0.358	0.064	-0.470
median household income	54367	55565	8003	52867	52612	10466	1.124
percentage unemployed	0.041	0.035	0.026	0.041	0.038	0.023	-0.018
median housing value	147418	151800	21578	161293	156800	40950	-3.488
median rent	616	610	82	692	689	122	-5.372
vacancy rate	0.050	0.037	0.032	0.087	0.041	0.113	-1.023
median household size	2.222	2.000	0.420	2.114	2.000	0.339	1.644
percentage renters	0.250	0.189	0.140	0.274	0.229	0.167	-0.350

The t-statistic reported in the last column is for the test of the null hypothesis that the two population means be the same across B-close and B-far.

Table A4
Summary statistics for attributes in B-close and B-far: racial/ethnic distance

Vermont 1997

	B-close			B-far			
	mean	median	std.dev.	mean	median	std.dev.	t-statistic
percentage black	0.005	0.003	0.005	0.005	0.004	0.006	-0.041
percentage hispanic	0.008	0.005	0.007	0.009	0.008	0.007	-0.135
percentage asian	0.005	0.005	0.005	0.005	0.004	0.006	-0.068
percentage female	0.507	0.509	0.020	0.508	0.508	0.023	-0.005
percentage with at least college degree	0.293	0.283	0.098	0.346	0.326	0.128	-0.866
median household income	41460	40483	7099	48856	45972	14389	-7.154
percentage unemployed	0.052	0.048	0.030	0.045	0.040	0.027	0.262
median housing value	97731	89000	35726	125012	104100	71459	-5.259
median rent	534	519	101	601	573	169	-4.705
vacancy rate	0.159	0.106	0.153	0.140	0.080	0.152	0.403
median household size	2.066	2.000	0.250	2.141	2.000	0.358	-2.194
percentage renters	0.241	0.224	0.103	0.205	0.189	0.094	0.639

Rhode Island 1999

	B-close			B-far			
	mean	median	std.dev.	mean	median	std.dev.	t-statistic
percentage black	0.006	0.005	0.005	0.006	0.005	0.005	-0.034
percentage hispanic	0.011	0.010	0.007	0.011	0.011	0.007	0.008
percentage asian	0.008	0.007	0.006	0.007	0.006	0.007	0.023
percentage female	0.509	0.508	0.012	0.512	0.511	0.017	-0.042
percentage with at least college degree	0.343	0.342	0.103	0.437	0.421	0.130	-1.329
median household income	57658	57363	10540	62308	58692	17356	-2.656
percentage unemployed	0.037	0.033	0.025	0.034	0.031	0.022	0.129
median housing value	159306	155800	39990	199401	173900	89920	-5.327
median rent	641	615	118	721	692	200	-4.039
vacancy rate	0.047	0.033	0.035	0.102	0.047	0.129	-1.638
median household size	2.315	2.000	0.469	2.211	2.000	0.417	1.527
percentage renters	0.203	0.182	0.085	0.183	0.164	0.095	0.344

The t-statistic reported in the last column is for the test of the null hypothesis that the two population means be the same across B-close and B-far.