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Divorce Spillover Effects:
The Effects of Marriage Participation on Future Divorce Rates

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Theoretical models have predicted that marriage markets are highly sensitive to exogenous shocks in the pool of available singles. Some models predict that an increase in the pool of singles positively impacts divorce rates by decreasing search costs and increasing the probability of remarriage. Despite the growing collection of theoretical literature on this issue, empirical work on the subject has been extremely limited. Historically, natural experiments that generate increases in the pool of available spouses (for example divorce law liberalization) tend to impact divorce rates directly, making the separate effects difficult to identify. This paper contributes to the literature by using an innovative approach that provides an explicit empirical test of how fluctuations in the availability of marriage market participants affect divorce rates.

I focus my analysis on border regions of each state and assess the impact of fluctuations in divorcee population in one state on the divorce rates of those in the neighboring states' border regions. Specifically, I test if state border counties are more affected than interior counties by the divorce rates of a neighboring state's border region. By utilizing cross-state border regions, I am able to identify the impact of increased singles in one state on the divorce rates of the border counties of a neighboring state, while controlling for resident state divorce laws and labor market conditions.

Despite the average effect being close to zero, I find a pattern of coefficients that strongly supports a divorce spillover effect. The relationship between population levels and spillover magnitudes is strongly predicted by the theoretical literature. The divorce rates of a state's border counties disproportionally increase in comparison to its interior regions, after a spike in the divorce rate of a neighboring state's border region, when the neighboring state's border population exceeds one's own state border population. The findings are robust to a number of specification alternatives and provide strong evidence that increased marriage market participants result in higher divorce rates.

Key Words: Marriage, Divorce, Matching, Divorce Law
JEL Codes: J10, J12

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Divorce can be viewed as a corrective mechanism in a market characterized by imperfect information. Individuals commonly match and marry with incomplete or incorrect assessments of their mates. The information gathered during the course of a marriage can change an individual's assessment of both their current match, as well their extramarital options. This process of continual marital re-assessment has led participants in the American marriage market to be described as having "permanent availability" (Farber, 1964).

Spousal alternatives, the number of options one has outside of marriage, can affect one's assessment of the quality of their current marital match. Survey data provides anecdotal evidence that spousal alternatives can affect marriage survival rates, as approximately 30% of divorces are preceded by extra-marital affairs (South and Lloyd, 1995 p29). However, even if one does not have a specific partner in mind, the perception of a high probability of remarriage may be sufficient to influence marital dissolution by affecting the net expected benefit of divorce and, consequently, remarriage. Survey data have shown that married persons who perceive a high likelihood of remarrying, should they divorce, are more likely than others to dissolve their marriages, holding marital satisfaction constant (White and Booth, 1991; Udry, 1981).

A significant insight from the theoretical literature on spousal alternatives is the plausible existence of a feedback mechanism that causes marriage markets to be highly sensitive to exogenous shocks. As Chiappori and Weiss (2007: p61) point out, "The increasing returns in matching, whereby it is easier to remarry if there are more divorcees around, creates a positive feedback from the expected remarriage rate to the realized divorce rates." Therefore, increases in the stock of available individuals in a given region should positively influence the rate of marital dissolution in the region. Despite the merit of these theoretical predictions and the growth in theoretical and descriptive literature on spousal alternatives, the empirical work on the subject has been quite limited. Natural experiments that create a treatment and control group to study are limited in the field of household development and structure. The few natural experiments that affect marriage participation, such as divorce law changes, also tend to impact divorce rates directly by changing the cost to divorce; this makes the impacts from the catalyst (the divorce law change) and the secondary feedback (the spillover effect) difficult to untangle.

This paper provides a comparative-static test of the positive feedback-mechanism, discussed by Chiappori and Weiss (2007), that exists between the current and the future divorce rate. In this paper I use a difference-in-differences approach to test how changes in the number of available singles in a marriage market affect the divorce rate of those whose incentives to divorce are otherwise unchanged. I exploit fluctuations in the number of marriage-market participants created in the previous years' divorces in neighboring states, and measure these effects on the divorce rate in one's own state.

I isolate neighboring state's divorcees as a proxy for the change in local marriage-market participants. Neighboring state divorce rates represent a reasonably exogenous source of change to the participation in the local marriage market. Changes in the never-married population could also be used, but exogenous variation in this group is not available. Additionally, divorcees represent unmatched entrants to the marriage market who are potentially more age appropriate for those in existing marriages and therefore are guaranteed to at least be potential extramarital options.

Specifically, I ask if a state's border counties are more affected than its interior ones by a neighboring state's divorce rates. This kind of border-interior comparison allows me to net out all the state-specific factors affecting divorce rates (such as divorce laws and welfare benefit-levels) and isolate the effects of divorcee population changes.

Despite the average effect being close to zero, I find a pattern of coefficients that strongly supports a divorce spillover effect. The relationship between population levels and spillover magnitudes is strongly predicted by the theoretical literature. The divorce rates of a state's border counties disproportionally increase in comparison to its interior regions, after a spike in the divorce rate of a neighboring state's border region, when the neighboring state's border population exceeds one's own state border population. Interaction coefficients between population ratios and the neighboring-state's border-county's divorce-rate show larger effects in areas with higher populations. The findings are robust to a number of specification alternatives and provide strong evidence that increased marriage-market participants result in higher divorce rates.

II. Conceptual Framework & Mechanisms

Although divorce spillover effects have never been directly tested, many papers have looked at matching mechanisms, such as meeting opportunities and participation rates, in the marriage market and how changes in these mechanisms can affect divorce rates. This literature informs my research, as it is through these matching mechanisms that a spillover effect would function. Therefore, understanding these mechanisms will help in specifying the correct regression equations and provides support for a causal finding.

In the following sections, I provide a cursory review of the existing research on marriage market matching and divorce rates. In sections 2A and 2B, I discuss how matching mechanisms affect divorce rates specifically. In 2C however, I address general matching of single participants and theories of returns to scale in matching markets.

2.A Remarriage Options and Divorce

Empirically, two key papers have looked at the effect one's own perceptions of remarriage prospects has on divorce rates. Udry (1981) used longitudinal-data on approximately 1,600 married couples spanning from 1974-1979; in the survey each person was asked to assess their ability to re-marry if the couple was to divorce. He finds that even when controlling for marital satisfaction, both the husband's and the wife's assessments of their alternatives help explain marital dissolution rates, independently and jointly. Significantly, the paper finds that marital alternatives are a better predictor of divorce than current marital satisfaction. White and Booth (1991) administered a similar survey to a panel of approximately 2,000 random, married individuals from 1980 to 1988; they also showed that when holding marital satisfaction constant, remarriage prospects are highly significant in predicting divorce.

The stock of participants in the marriage market also has potential to affect marital stability through people's ability to re-match in the marriage market. South and Lloyd (1995) used National Longitudinal Study of Youth and the 1980 Public Use Microdata to predict divorce using proportional hazard models. Their results indicate that many persons remain open to extramarital relationships and that divorce is more common in more urban areas where either the husband or the wife has numerous outside alternatives. These papers did not assess the impact of an exogenous change in the number of single men or women and it does not control for divorce costs across the states in their sample range. However their findings validate that perceptions of remarriage options are significant and that the stock of available people in a region can affect an individual's perceptions and consequently their marital stability.

2.B Meeting Opportunities and Divorce

McKinnish (2007) analyzes how changing search costs affect the opportunities to meet someone from the opposite sex and consequently how the change in these costs can affect the probability of divorce. The modern workplace is one of the primary avenues for extra-marital searches as it is a relatively low-cost venue for searches, because it provides ample meeting opportunities with the opposite sex without extra time spent searching or rising suspicions in a partner. McKinnish uses job-place sexual segregation to assess the impact of lower search costs on divorce rates of those who work in heavily integrated industries. Using both an instrumental variable and an industry fixed-effect strategy, McKinnish finds that those who work with a larger fraction of the opposite sex are more likely to be divorced. A robust methodological approach supports that the finding is identified by the decrease in search costs resulting from an increase in the meeting opportunities between those of the opposite sex in the workplace.

Svarer (2007) expands on McKinnish's findings by utilizing individual-level data from Sweden that identifies the current marital status of those in his sample. His findings are consistent with McKinnish (2007) for those already married: the results show that as the ratio of women to men becomes more evenly balanced in a workplace the risk of marital dissolution for the employees of the workplace increase. However, workplace ratios have minimal effect on relationship formation for those who are currently single. His findings are consistent with a search model where the costs of market searches increase for those already married, making the workplace an increasingly important meeting location for this subsample. The findings of Svarer and McKinnish support that meeting opportunities between the opposite sexes are a significant factor in determining divorce. McKinnish and Svarer do not analyze how search costs change when the numbers of participants change; however, their findings suggest that divorcees may have a larger impact on the stability of existing marriages in areas where meeting opportunities are more frequent.

2.C. Increasing Returns to Scale in the Marital Matching Market

The structure of the remarriage market, and how matching occurs, also has the potential to affect the probability of remarriage. Chiappori and Weiss (2007) make arguments for increasing returns to scale (IRTS) in marriage markets, simply stated: "(re)marriage is easier, the larger the number of singles around" (Chiappori & Weiss, 2007 pg 43). There are several reasons why increasing returns to scale--where a one percent increase in market participants will increase matching by more than one percent --may be present in marriage markets.

Bisin et al. (2001) empirically supports increasing returns to scale in marriage markets using religious groups as a sorting mechanism in marriage matching. Given this sorting mechanism, they show that the probability of marrying within one's given religious group increases with the share of the population that subscribes to the religion. Importantly, Bisin et al. provides evidence that the increase in the probability of intra-group marriage is disproportionally larger than the positive change in the share of the population, which suggests increasing returns to scale. In other words, a one percent increase in the Jewish population raises the probability of a Jewish-to-Jewish marriage by more than one percent.

The first key reason IRTS may exist is how increases in participants affect the meeting opportunities of the participants already in the market. From the perspective of a potential partner, when two individuals meet at work, social functions, or sporting events, the meeting can either be "wasted" when one individual is committed and reluctant to leave their spouse or "fruitful" when both individuals are single and have potential to easily form a union. As the number of single individuals increases in a given marriage market the number of "wasted" meetings declines and the probability of meeting a potential mate increases in all social functions.

Since meetings are considered a good thing in any matching market, an additional searcher (or single person, for the purposes of this paper) creates a positive externality for other searchers when meeting opportunities are limited (Diamond and Maskin, pg 283). Part of this externality comes from how increased participants affect the construction of events or avenues where singles meet only singles. These more focused channels can be expensive to establish, and therefore may only be created when there exists a large-enough singles market to warrant the expense. Additionally, it has been found that the search intensity of single individuals tends to increase with the proportion of single people in the population (Mortesen, 1988). In other words, when there are limited matches available, single individuals may become discouraged and voluntarily decrease their participation in social events and meeting opportunities when they observe too many meetings are "wasted." Likewise, as more single individuals enter a market, existing singles are more willing to engage in activities which provide opportunities for "fruitful" meetings.

Given the theoretical model of a marriage market which exhibits IRTS in matching, it is imperative to consider when this assumption would be valid in the data. As Diamond and Maskin discuss in their 1979 paper, *Equilibrium Analysis of Search and Breach of Contracts 1: Steady States,* IRTS (or the quadratic matching case) is a reasonable model only when there is a low density of potential partners. When there is a high density of potential partners the model would be a poor fit, as an additional searcher would have minimal effect on the probability of matching for others already in the market. Therefore, the predictions of IRTS would likely be present in a marriage market in a more rural area but likely not in a highly urban area.

Extrapolating Diamon's and Maskins' work implies that increases in the stock of single participants would be subject to diminishing returns in their influence on an individual's perceived remarriage potential. For example, the perceived impact on the probability of remarriage would be larger when moving from a dozen fish in the sea to 13 fish than the impact on re-matching when moving from a thousand fish in the sea to 1,001. Additionally two of the key arguments for IRTS in the dating market hinge on a low threshold of singles: that avenues and venues specifically for singles requires a minimum number of participants to warrant operating and that the search energies of single individuals in the market are strongly affected by participation levels in the market, particularly when the level of pre-existing levels of singles is low.

The theoretical models inform my regression specifications and indicate that a non-parametric specification will likely fit the model most appropriately. Using variation in the population levels at the border, I will be able to examine how the effect of increased divorcees varies pending on the neighboring state's population. It is likely that larger effects would be found when the neighboring state's population is larger than one's own state, such that increased divorcees represent a significant increase in the stock of singles, but not so populous in comparison to one's own state that new divorcees would be hardly noticeable.

Although it has been shown that marital alternatives matter, that increasing returns to scale may be present in marriage markets, and that lower search costs positively impact divorce rates, none of the previous literature has examined how changes in the number of market participants affect the divorce rates. However, these papers support a causal finding in *this* paper by validating the mechanisms by which new divorcees would be destabilizing to current marital matches.

III Data

This paper utilizes a newly-digitized dataset of all divorces and annulments granted in each county in the United States from the years 1969 to 1988. The divorce data are obtained from the annual editions of the *National Vital Health Statistics*, Volume 3, Marriage and Divorce for the years of interest.[1] To these data, I add intercensal county population provided by the US Census to create the crude divorce rate, or the number of divorces per 1,000 people.[2] Additionally, I add in key controls for variables known to affect divorce rates, such as population density and employment rates (Wolfers, 2006; Hillerstein & Morrill, 2008; South & Lloyd, 1995). Total employment for each county from 1969 to 1988 was attained from the Bureau of Economic Analysis, and used to create a crude employment rate in each county.[3] Lastly, I control for state level divorce laws. Controls are modeled off of seminal divorce law literature, accounting for time since a unilateral divorce law transition. Lagged, divorce-law liberalization dummy variables are used in two-year

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[1] Data obtained from: http://www.cdc.gov/nchs/products/vsus.htm. Note, that no divorce data at the county level is available post 1988 as the federal government stopped collecting data in this year.

[2] Data obtained from: http://www.nber.org/data/census-intercensal-county-population.html. One could also use the true divorce rate which is the number of divorce per the married population; however, previous work has shown little difference between the two measurements and the annual married stock is not available at the county level (Wolfers, 06; Kneip &Bauer, 06; Hellerstein & Morrill, 08).

[3] Total employment from each county from 1969 to 1988 was attained from the Bureau of Economic Analysis, and used to create a crude employment rate in each county.

intervals, until year 10, when a 10+ year dummy is used, with the omitted category as any time prior to law passage (Wolfers, 2006).

3.A Observational Unit

Due to the geographical nature of the observations, I present both a specific example and general description to provide a better understanding of how the panel data set is constructed. To create each observation, I first determine the closest neighboring state to each county, and the distance to that neighboring state from the county centroid. Figure 1 shows a specific example, as well as the steps in creating observations using the state of South Carolina. As can be seen in Figure 1_A, South Carolina has two neighboring states, North Carolina and Georgia.

Upon determining each county's closest neighboring state I aggregate the sample into groups of counties within a state that share the same neighbor state and label these groupings "County Neighbor Groups" (CNGs). Figure 1_B shows this step, with all counties in South Carolina sectioned into either a Georgia CNG or a North Carolina CNG. Then, within a given CNG, I further identify two key groups, a CNG's border counties and the interior counties. I define a border county as a county that is within 30 miles of the state border or a county that is in a Census Bureau defined interstate Labor Market Areas with the neighboring state of interest.[4]

I define an interior county as one that is not in an interstate LMA and the centroid of which is between 50 and 300 miles from the border. Any county that does not qualify as a border or interior county (in other words, those that are not in an interstate LMA and whose county centroid is between 30 and 50 miles of the border, or over 300 miles from the border) are excluded from the final sample[5]. Counties that

[4] LMAs are constructed and defined by the Census Bureau using commuter-flow data to model areas that have high levels of daily social interactions and community integration (Tobert and Killian, 1987) Since LMAs are constructed using commuter-flow data, I choose to include counties that are more than 30 miles from the border, but whose commuter-flow data show heavy integration with the neighboring state. 96% of counties within 26 miles of the border are considered to be contiguous border counties. Nearest neighbor state is defined as the state, from all surrounding states, which has the minimum distance from the county center to the borderline in miles. This data was utilized in McKinnish (2005) and was generously provided by the author.

[5] For Robustness checks using other mileage cut offs see forthcoming appendix section.

Figure 1

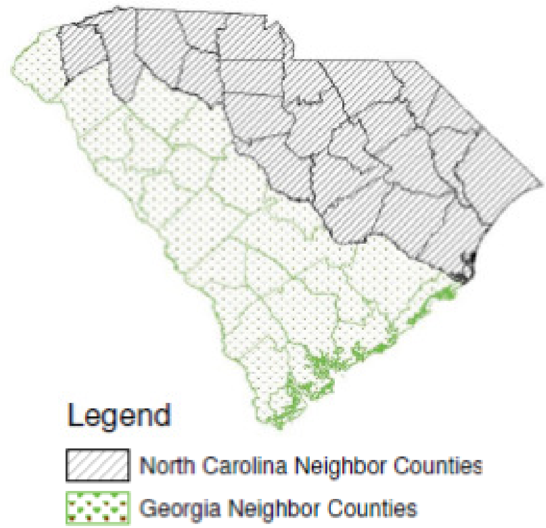
Steps to Dividing South Carolina into County Neighbor Group bins

Panel A

Map of Southern United States

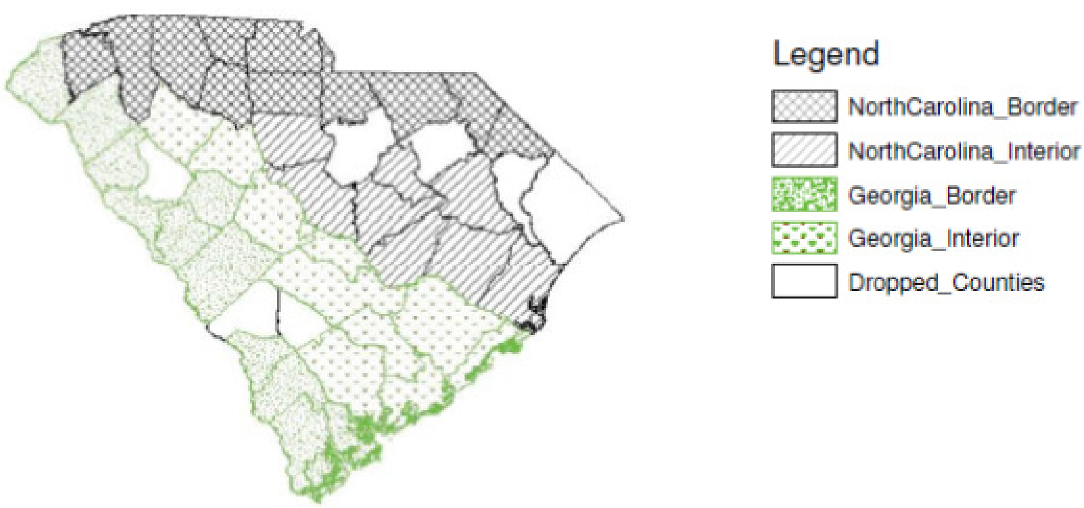
Panel B - Define County Neighbor Groups

Determine the closest neighboring state for each county



Panel C - Define County Neighbor Group Bins

Separate CNGs into two bins each - border and interior.



have a centroid between 30 and 50 miles are omitted because they could be considered partially treated[6]. Counties which are more than 300 miles from their neighboring state are omitted as they are considered too far from the border to be a valid counterfactual for the border region.

Lastly, I aggregate the county-level data to the border / interior CNG level. Doing so provides me with two observations for each CNG in each year. In my analysis, observations are border and interior CNG groups, with variables constructed as population-weighted means taken over the constituent counties in each area.[7] My unit of observation is the CNG border or CNG interior, otherwise referred to as CNG bins. In the specific example of South Carolina, four bins exist in the utilized sample, and can be seen in Figure 1_C, North Carolina's interior and border CNG, and Georgia's interior and border CNG. For a more complex example, see the appendix Figure 1, for a CNG sorted map of Alabama with four neighboring states.

3B. Sample Limits & Descriptive Statistics

Certain states had to be removed from the country-wide sample because they do not have both the necessary interior and exterior counties to construct both bins of a CNG, as I have described above. An example of this would be New Jersey, where all the counties in the state are considered to be "border counties" and there is no interior average that can be taken. Additional states dropped from the analysis due to a lack of any "interior" counties are: Connecticut, Delaware, Maryland, New Hampshire, Rhode Island, and Washington DC.

To measure the social integration across the border, I use the average population density of each CNG border region. In the sample of all border CNG counties, the median population density in border regions is 73 and the mean population density is 230 people per square mile.[8] In regions with extremely low

[6] Ideally, I would allow contiguous counties, whose centroid is between 30 and 50 miles to be included in the border observations (at least as a robustness check to the current specification). However, due to data limitations, I am unable to identify these counties without examining each dropped observation on a map. Due to time constraints I have been unable to re-classify these observations at this time.

[7] The neighboring state divorce rates affects the groups of counties in the border bin of the CNG and therefore could cause correlation in the standard errors of counties within the same border group. By collapsing the data to the county border group level, which is the true level of treatment for the neighboring states divorcee stock, I resolve the Moulton problem that could be created by utilizing the individual county observations (Moulton, 90).

[8] Due to the skewed nature of the population distribution, median values are used for sample considerations instead of mean values. Median and mean values of all key variables are presented in Table 1.

populations, observations where either state's border-CNG had a population density in the lowest fifth-percentile of the population-density distribution were dropped. This is based on the rationale that interactions across state lines are likely negligible and the opportunities to perceive an increase in divorcees would be minimal to the neighboring region. Coincidently, the fifth percentile of the population-density distribution is approximately five people per square-mile. The resulting sample, then, is all CNGs with both an accompanying border and interior region and a population density above five people per square-mile in the border region. The final sample contains a total of 192 CNG bins, with 96 border and 96 interior bins. Of the 3,113 counties in the continental United States, 1,906 counties remain in the sample defined above. Of those 1,906 counties, there are 1061 counties residing on the border of states and 845 counties classified as interior.[9]

The following discussion uses the terms "own state," or "resident state," to indicate the treated state and "neighbor state" when discussing the treatment state in the difference-in-differences empirical specification. Using the example of South Carolina, in order to discuss how Georgia's divorce rate affects South Carolina's divorce rate, South Carolina is the treated state, or "own state" and Georgia is the treatment state, or "neighbor state." Key variables used in the paper are presented in Table 1, and all averages are given in terms of the treated state, except for the measure of the lagged, neighboring state divorce rate.

Table 1 displays a comparison of descriptive statistics for the interior and border CNG bins taken in 1980. I selected 1980 arbitrarily as a date to provide summary statistics because it is approximately in the middle of my sample, and as a decennial-census year, its data potentially contain less measurement error than in other years. Due to the skewed nature of the mileage and population distributions, both the means and medians are presented in the table. Recall these variables are constructed by taking the populated-weighted average of the variables across the counties in the given CNG bin.

[9] See Forthcoming Appendix additions for a more thorough breakdown of observations lost in each sample restriction.

Table 1

Descriptive Statistics of County Neighbor Group by Bin	**1980 Means** Border	Inerior	**Pr(|T |>| t|)** * Indictes a Significant Difference at 5% level	**1980 Medians** Border	Interior
Total Number of Counties	1042	851			
Total Number of CNGs	96	96			
Average Number of Counties	11.09	9.05	0.072	10	7
	(7.45)	(9.35)			
Average Total Population	486.21	615.97	0.185	361.632	282.24
	(458.16)	(1016.55)			
Average Milage to Border	37.18	82.21	0.00*	29.9	74.33
	(18.38)	(28.19)			
Average Percentage of CNG that is Urban	38.56	47.02	0.587	44.65	43.11
	(32.39)	(36.95)			
Average Population Density Per Square Mile	234.22	214.43	0.679	96.65	112.95
	(381.34)	(271.05)			
Average Employment per 1000	460.29	466.01	0.559	462.23	465.39
	(65.15)	(70.12)			
Average Divorce Rate per 1,000	5.30	5.49	0.162	5.29	5.44
	(1.79)	(1.77)			
Average Neighbor Divorce Rate per 1,000 (y-2)	5.11	5.11	1.000	5.09	5.09
	(1.91)	(1.91)			

Standard Deviation in parenthesis. Mean distance to nearest state is calculated in miles and the crude divorce rate is number of divorces per 1,000 population. Note that there exists 3033 total continental USA counties

On average, there are approximately 11 counties in a CNG border bin and nine counties in a CNG interior bin. The mean mileage from the county centroid to the border is 37 miles for border regions, while the median distance is 30 miles. For interior regions, the mean distance to the border is 82 miles and the median distance is approximately 75 miles. Although the total population is higher in the interior regions, the mean population per square-mile is driven up by highly-populous regions on the border. As a result, the mean population per square mile is approximately 10% higher on the border, despite the fact that the median population density is higher in the interior. The urban density, as defined by the percentage of the counties that reside in an Standard Metropolitan Statistical Areas (SMSA) in 1980, are similar in terms of medians, but the mean percentage of urban counties is approximately 5% higher for the interior regions. The divorce rate in 1980 in the CNG bins is relatively similar, with the mean rate at approximately 5.30 divorces per thousand on the border and slightly higher at 5.49 in interior regions. The neighboring state's divorce rate is presented in the final row of the Table 1. The neighbors divorce rate is lagged two-years, is discussed in depth in the empirical section, and is the same for both border and interior observations, as it is the neighboring state's border regions' divorce rate that is the treatment for both interior and border observations.

IV Empirical Strategy & Results

In what follows, I use difference-in-differences and triple-difference strategies to identify the effects of a neighboring state's divorce rates on resident state's divorce rates. I exploit variation in the neighbor state's divorce rates and test for their impact on the divorce rates of the resident state's border counties. All else equal, these border counties should be disproportionally affected by the neighbor state's divorcee population in comparison to the state's interior counties (that are not exposed to the additional divorcees).

The empirical work begins with a difference-in-differences specification that compares the border of the state to the interior. This comparison, which I am able to use because of divorce residence requirements, implicitly controls for any determinants of divorce that fluctuate at the state-level, such as divorce laws or welfare benefits. However, this identification strategy could be threatened by an omitted variable that is affecting the two state's border regions more than their interior regions. For observable variables which can cross over state-lines, such as employment conditions or population density, the regression directly control for these variables. Despite these controls, the difference-in-differences specification is still vulnerable to an unobservable variable which disproportionately affects the border region's divorce rate in each state.

To provide further support to the divorce spillover interpretation, I examine heterogeneous effects of neighboring state divorce rates depending on population levels. A heterogeneous relationship between population rates and divorce rates is strongly predicted by the theory, and therefore, if one is found, it provides support to a causal interpretation. For variables that cannot be explicitly observed or controlled for, a triple difference specification provides robustness against spurious findings. If the spillover effect varies by neighboring state's border-region population this supports the identification of a causal relationship between pre-existing divorce rates and current divorce rates. If, instead of a true spillover effect, things such as sticky wages or laws were driving a lagged spike in divorce rates then the size of a neighbor-state border population relative to own-state border population should be irrelevant.

One of the arguments for assuming increasing returns to scale in marriage markets is the diminishing chance of "wasted" opportunities when more singles are in the market. As other papers have noted, divorce rates tend to increase when search costs diminish (McKinnish, 2005; Svarer, 2007). Population density affects search costs of finding a mate through transportation and the level of frictions (or difficulty in finding matches) in a less dense marriage-market. Therefore, in more densely populated areas the impact of an increase in the neighboring state's divorcee-stock should be more visible to participants due to increased daily interactions between the participants. If the lagged spike in resident divorce rates are driven by sticky laws and not increased marriage- market participation then the population in border regions should not affect the estimates found.

The stock of available singles, or the population level, can affect the rate of matching and consequently the magnitude of the spillover effect. An interaction variable which can capture this relationship must be constructed from the data. Although I do not have an explicit measure of the stock of singles, I have population measures to use as a proxy. In order to model a market with IRTS when there are minimal participants and decreasing returns to scale when there exist many participants, the regression equation will require a triple-difference, in order to allow for these heterogeneous effects to depend on relative population levels. The theory suggests IRTS when there exist few people in the market and DRTS when there are many. Therefore, it is likely that the largest effects will be found when the neighboring state's border-region is more populous than one’s own state, but also not so populous that the addition of the divorcees would not be noticeable or considered a significant change in the stock of available marital-market participants.

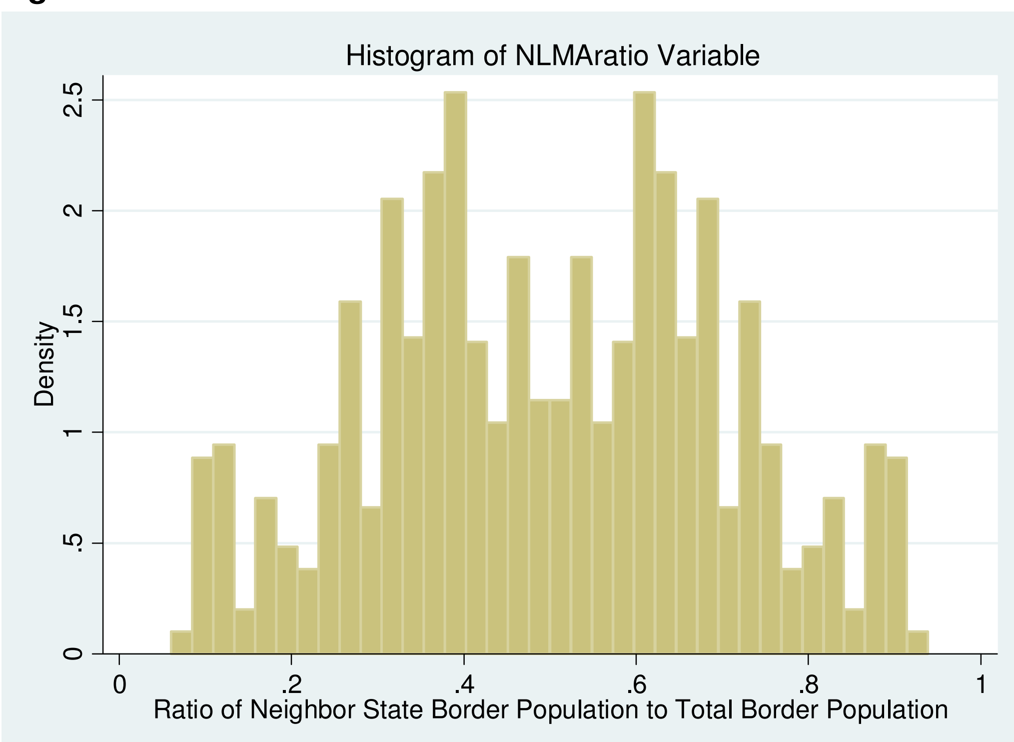
4.A. Construction of Population Level Treatment Variable

I create a ratio, *CNGratio,* using the size of the population of each state's border region to act as a treatment, or dosage, variable. *CNGratio* is equal to the population in the neighboring state's border region over the total population in the conjoined border regions. A *CNGratio* of 0.50 would be the value of the variable if each border area's population was the same. By matching each CNG with its respective neighbor CNG (e.g., matching South Carolina's Georgia-CNG to Georgia's South Carolina-CNG), I create linked CNG border areas and can assess the ratio of the populations in the linked border region. For example, in 1980, South Carolina's border with Georgia consisted of approximately 480,000 residents in the state of South Carolina and 783,000 in Georgia's. Therefore, of the linked border region, Georgia would be considered the more populous of the linked CNGs. By interacting *CNGratio* with divorce rates I am able to map the percent divorced in the neighbor-state to the stock of divorcees per population in the linked CNG border areas; thereby creating a dosage level. Due to the perfect matching required by the sample specification, such that every state has an interior, border, and a neighbor, the distribution is symmetrical. Figure two shows the distribution of *CNGratio* where 25% has a *CNGratio* of .34 or below and the symmetry in the sample results in the top 25% of the sample has a CNG ratio of .66 or above.

eq 1. *CNGratio =*

[(Neighbor state Border Population)/ (Own State Border Pop + Neighbor State Border Pop)]

Figure 2



4.B Difference-in-Difference. Regression Equation

Equation 2 shows the preliminary regression equation,

eq 2. $$\ln(CNG\ Div\ rate_{CNGj,yr}) = \alpha + \beta_1(\ln(NghbrDivRate2))_{CNGj,yr-2} + \beta_2(Border) + \beta_3(\ln(NghbrDivRate2) * Border)_{CNGj,yr-2} + (X_{CNGj,yr-1})\ \beta_8 + (X_{CNGj,yr-1} * Border)\ \beta_{8'} + (Year)\phi + ((Year) * Border)\phi' + (State * time)\gamma + ((State * time) * Border)\gamma' + \varepsilon_{CNG.yr}$$

The dependent variable is the logged divorce rate in a given CNGbin where the subscript j, has two possible values, j =i = interior or j=b=border. The model is a fully saturated fixed-effects model, with every variable interacted with a dummy variable *border,* which equals 1 for CNG border observations. The independent variable of interest is the logged average divorce rate in the border region of the neighboring state, lagged two-years. A two-year lag was selected for the independent variable of interest as this length of time provides enough time for those who divorced to be active and visible in the remarriage market, but not so long as to risk the majority being remarried. Research shows that during the 1970s and 1980s the median duration of time from divorce to remarriage was approximately three to four years (Kreider, 2006). Within five years after divorce, almost 60% of men and women had remarried in the 1970s and 1980s (Bramlett MD and Mosher WD). Therefore a two-year lag was selected as this is prior to the median remarriage time, but long enough to assume divorcees would be dating. Sensitivity analysis of this lag is presented in Table 6 of the appendix.

Vector *X* explicitly controls for own state divorce laws, the employment rate and population density in each given CNG bin, lagged one year. The own-state controls are lagged one-year to measure changes in the state during the time prior to the divorce, and to account for possible separation time prior to divorce. To control for the state's own divorce laws, I use a dummy variable to indicate if the state has equitable distribution laws and dummy variables for time, since unilateral divorce laws were passed in two year bins. Consistent with the existing seminal literature on the effects of unilateral divorce law, the dummy bins are in two year increments beginning with 1 to 2 years since enactment, 2 to 3, ...8 to 9 and 10plus years, such that the omitted category is no unilateral divorce laws (Wolfers, 2006). Other controls include CNG-bin fixed-effects, year dummies and linear state-time trends. State-time trends are included to help control for the large social and secular changes to divorce that occurred during my sample period of 1969-1988. Additionally, in robustness and specification checks I include the own CNG logged divorce-rate, lagged two-years.

The log-log specification was selected as it avoids the assumption that an increase in the stock of divorcees will affect each regions divorce rate by the same level (in terms of percentage points). This is an appropriate specification as it is likely that the distribution of marital happiness is not uniform, or the exact same in all CNGs; therefore, an increase in re-marital possibilities will not affect the divorce rate in each CNG by the same level. The provided empirical specification estimates the effect of an increase in divorcees in terms of percentages instead of percentage points, therefore allowing for the quantity of marginal marriages across CNGs to vary. Additionally, the coefficient of interest can be interpreted as an elasticity, or the given percentage change in the dependent variable from a one percent change in the independent variable.

The results of the initial regression that does not take population into consideration are shown in Table 2, column 1 and 2. In column 2, controls in vector X are added to the regression. Without considering relative population rates, the border-region divorce rates have no statistical effect on the divorce rates of residents in the accompanying state's border-region, when compared to the divorce rates of those on the interior of the resident state. However, it is likely the estimate of the effects of neighbor-state divorce rate is weakened by not accounting for the size of the neighboring-state's border population relative to own-state border population.

Table 2

VARIABLES	1 lndivrate	2 lndivrate
Ln(Neighbor Divorce Rate)$_{yr-2}$	0.0231	0.0216
	(0.0292)	(0.0303)
Ln(Neighbor Divorce Rate)$_{yr-2}$ * Border	-0.0082	-0.0083
	(0.0258)	(0.0259)
State Divorce Law Controls	.	X
Population Density Controls	.	X
Employment Rate controls	.	X
CNG Bin FE	X	X
Year Dummies	X	X
Linear State Time Trends	X	X
Observations	3,560	3,560

Robust standard errors in parentheses
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10

As a single individual would be more prone to search for a new partner in the region's most populous area, one would expect that a more populous neighbor-state would have a larger effect on the resident-state divorce rates than a neighbor-state that is relatively less populous then the resident-state. However, as discussed in the theory, if the neighboring state is excessively populous in comparison to the resident state, then an increase in divorcees in the neighbor state will likely have minimal effect in resident state divorce rates. Consequently, the relationship would not be linear between population and divorce effects, but most likely vary pending on the relative populations of the region.

4.C Triple Difference. Regression Equation

A dosage variable is introduced in Equation 3 using CNGratio which exploits the natural variation in population levels among the border regions. Population size is essentially acting as a proxy for a dosage level of new divorcees. In a more populous area, the same percentage-point change in divorce rates is a larger number of new divorcees, and therefore a larger increase in the probability of remarrying. Furthermore, when a neighbor-state border population exceeds own-state, the amount of new divorcees recognizable to own-state residents would be higher than if the neighboring-state border-population was small relative to own state. The interaction of the *CNGratio* with the divorce rate is a proxy for the number of new singles in the region. In a more populous region the same percentage point change results in a larger increase the stock of singles.

eq 3.
$$\ln\left(CNG\ Div\ rate_{CNGj,yr}\right) = \alpha + \beta_1(\ln(NghbrDivRate2))_{CNGj,yr-2} + \beta_2(Border) + \beta_3(\ln(NghbrDivRate2) * Border)_{CNGj,yr-2} + \beta_4 Dose + \beta_5(Dose * Border) + \beta_6(Dose * \ln(\ln(NghbrDivRate2))_{CNGj,yr-2} + \beta_7(\ln(NghbrDivRate2) * Border * Dose)_{CNGj,yr-2} + \left(X_{CNG,yr-1}\right)\beta_8 + \left(X_{CNG,yr-1}\right) * Border)\ \beta_{8'} + (Year)\phi + \left((Year) * Border\right)\phi' + (State * time)\gamma + ((State * time) * Border)\gamma' + \varepsilon_{CNG.yr}$$

In order to test this theory, I allow for a linear and a non-linear relationship between the affects of the neighboring state divorce rate and the neighboring state's population, using different measures of CNGratio. Columns 1 and 2, in Table 3, show CNGratio interacted linearly with the difference-in–difference variable of interest. In equation 3, the coefficient of interest on the triple-difference is measured by B_7. Results in columns 1 and 2 indicate that when the neighboring state's border region makes up an increasingly larger percentage of the linked-border-region, the effect of the neighboring state's divorce rate on resident state divorces rates is positive, but not statistically significantly. In columns 3-6 I specify a more flexible regression by creating three categories to measure relative population size, or dosage.

The three relationship categories specified are, state population is smaller or represents less than 40% of the linked CNG area (the omitted category), *CNGratio* between 40% to 60% or approximately the same size populations and *CNGratio* greater than 60% indicating a neighbor who is definitively larger. A triple interaction is created by interacting the dummy variables for between 40% and 60%, and above 60%, with the interaction of lagged, logged neighbor-divorce-rate and *border*. Results shown in column 3 and 4 indicate that when the neighboring state is more populous, relative to own-state the effect of the neighboring-state divorce rate on own-state, border divorce rates is greater and statistically significant. When the neighbor-state population makes up more than 60% of the linked-CNG border region, an increase in the neighbor-state divorce rate (of one percent above its mean-rate) will increase the own-state border-region divorce rate by 0.16% more than if neighbor-state border population constitutes less than 40% of the linked border region.

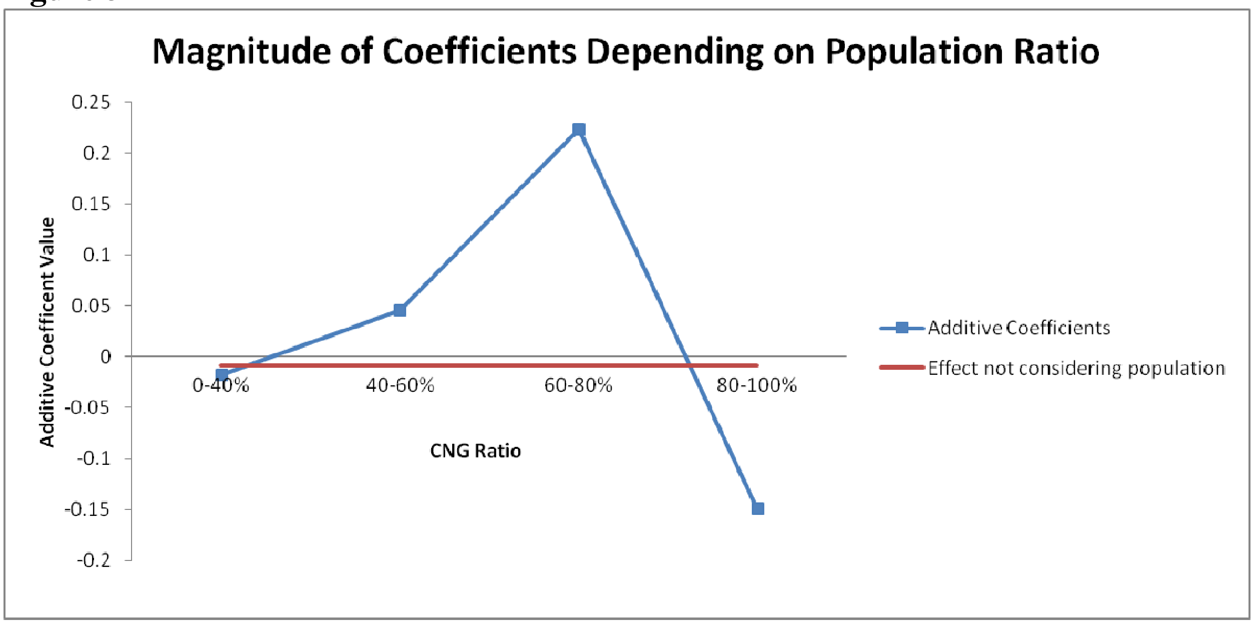
Table 3

Non-Parametric Specification						
	1	2	3	4	5	6
Variables	Indivrate	Indivrate	Indivrate	Indivrate	Indivrate	Indivrate
Ln(Neighbor divrate)yr-2	0.0412	0.0383	0.0199	0.0188	0.0203	0.0199
	(0.0562)	(0.0545)	(0.0348)	(0.0359)	(0.0350)	(0.0363)
Ln(Neighbor divrate)$_{yr-2}$* border	-0.0731	-0.0669	-0.0372	-0.0356	-0.0390	-0.0375
	(0.0520)	(0.0519)	(0.0290)	(0.0288)	(0.0295)	(0.0291)
Ln(Neighbor divrate)$_{yr-2}$*(CNGratio)*border	0.1862	0.1688				
	(0.1482)	(0.1495)				
Ln(Neighbor divrate)$_{yr-2}$*(0.40<CNGratio<0.60)*border			0.0604	0.0542	0.0689	0.0635
			(0.0446)	(0.0439)	(0.0450)	(0.0440)
Ln(Neighbor divrate)$_{yr-2}$*(0.60<=CNGratio<1.00)*border			0.1664*	0.1601*		
			(0.0704)	(0.0730)		
Ln(Neighbor divrate)$_{yr-2}$* (0.60<= CNGratio<.78) * Border					0.2415***	0.2411***
					(0.0688)	(0.0703)
Ln(Neighbor divrate)$_{yr-2}$*(0.78 <= CNGratio<=1.0)*border					-0.1026	-0.1314
					(0.1349)	(0.1304)
State Divorce Law Controls	.	X	.	X	.	X
Population Density Controls	.	X	.	X	.	X
Employment Rate controls	.	X	.	X	.	X
CNG Bin FE	X	X	X	X	X	X
Year Dummies	X	X	X	X	X	X
Linear State Time Trends	X	X	X	X	X	X
	3,560	3,560	3,560	3,560	3,560	3,560

Standard errors presented in parentheses. SE are clustered at the CNG level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10

In columns 5 and 6, the third category or 60% and above dummy is separated into two categories, 60%-78%, and the top-tenth percentile of the CNGratio distribution, 78% to 100%. As can be seen in the resulting columns, the effects of a neighbor-state divorce rate vary greatly with population ratios. The findings indicate that when a neighbor-state is less populous then the resident state that there is a minimal effect of neighbor-state divorce rates on resident-state border-region divorce rates. However, the effects become positive (although still close to zero) if the two border regions have the same approximate population. The effect size increases substantially to a highly significant and positive +0.24 when the neighbor-state is larger than resident-state but not huge in comparison to resident state. As can be seen on the final coefficient, estimated, for the effects when a neighbor-state represents more than 78% of the linked border region, there is a minimal to potentially negative effects when the neighbor-state is that populous. Figure 3 graphs the net effect, or the magnitude of the additive coefficients shown in column 6 for each given range of CNGratio.

Figure 3



New participants appear to be most destabilizing to the survival of current marriages in a given marriage market when the neighboring state is more populous than own-state, but not extremely more populous. When the neighbor state makes up between 60% and 78% of the linked border region a 1% increase in the neighbor-state divorce rate will increase own-state border regions' divorce rate by 0.24%. Given the average border divorce rate of 5.3, a 0.24% increase will increase the divorce rate to 5.43 per 1,000. This increase in the average divorce rate, given an average border-population of 486,210 will result in an approximate addition of 64 divorces. The non-monotonicity of the coefficients, as seen in column 6, strongly rejects a liner interaction specification.

Conclusion

In this paper, I provide a comparative statics test of the theoretical prediction that an increase in divorces will further lead to more divorces due to a feedback mechanism existing between the expected remarriage rate and the realized divorce rate. Using an original approach and county level data I am able to examine how border counties of states respond to an increase in the stock of divorcees in the border region of their neighboring state. I consistently find that in areas where the neighbor-state represents a larger portion of the marriage-market that the divorce rate in a neighbor state positively affects resident-state border-region divorce rates. The findings indicate that in regions where the neighboring state is less populous then one's own state, there are minimal to no effects on border residents. However, when a neighbor state is more populous than the resident state (but not over 78% of the marriage market) that there are large, positive and highly statistically significant effects on resident-state border-region divorce rates. The findings are robust to a number of specifications and provide strong evidence that the theoretical spillover effect in marriage-markets does exists.

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Appendix

Forthcoming Additions to Appendix:

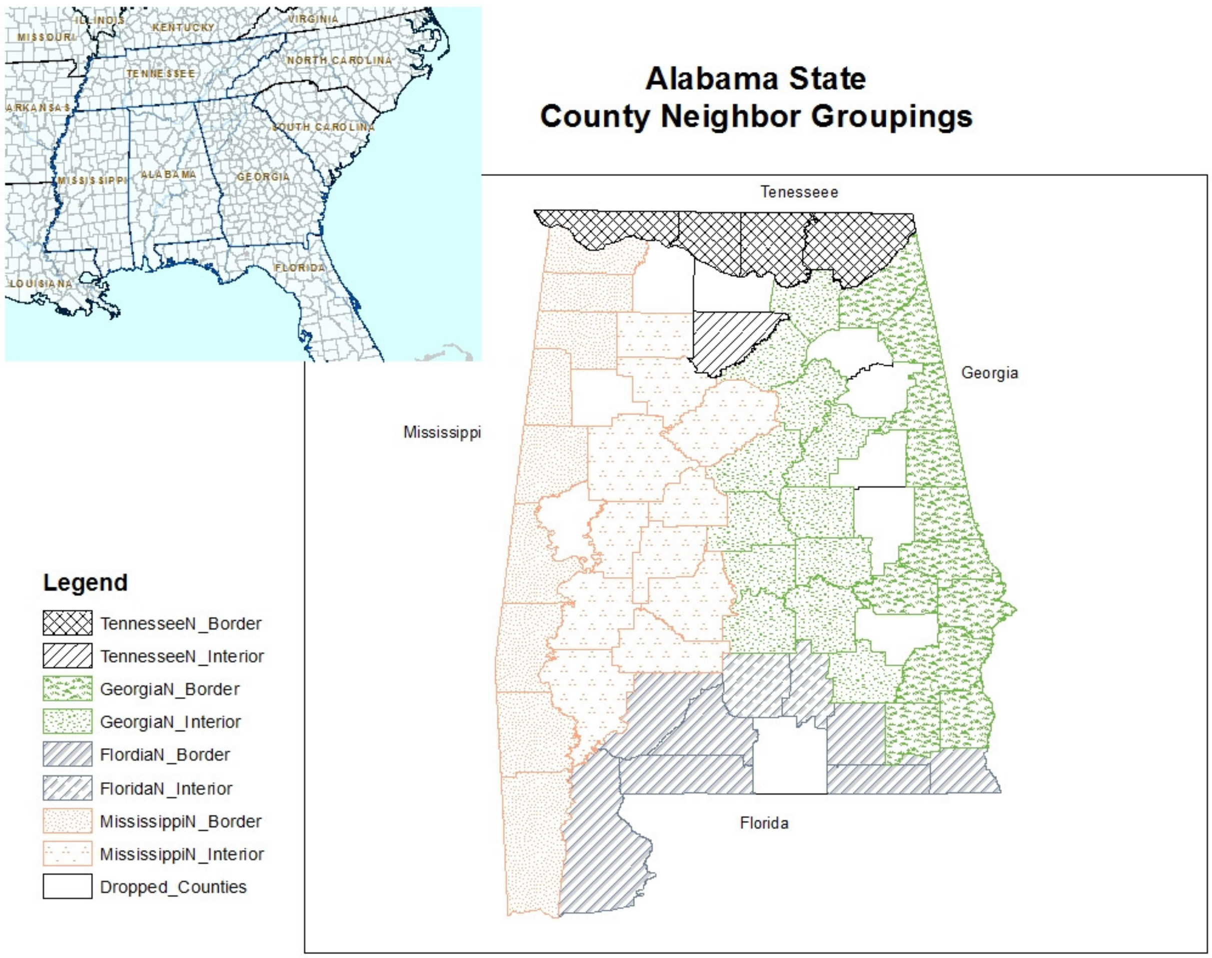
* Regression results for ln(divorce rate) regressed directly on and CNGratio & CNGratio ^2

* Table of observations lost to varying phases of sample specification: states dropped for low pop density, states dropped for no interior.

* Sensitivity analysis to the 30 mile cut-off for “Border” classification.

* Sensitivity analysis to the two-year lag of the neighboring state’s divorce rate.

Appendix – Figure 1



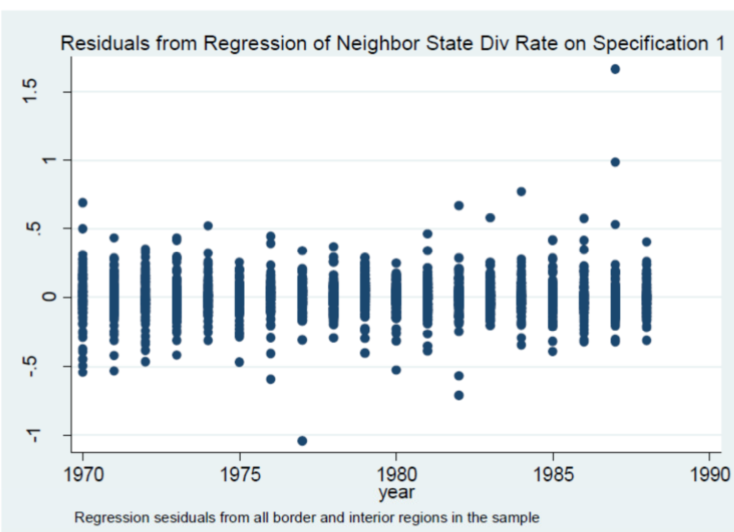
4. C. Test for Variation in Data

A key concern for the above methodology is the plausible exogenous variation that exists in the key variable of interest, the neighboring state's divorce rate. To ensure there remains variation in the neighboring state divorce rate that cannot be explained by the variables already controlled for in the primary specification I regress the neighboring state divorce rate on the remainder of the primary specification presented in equation 2:

eq 3. $(\ln(NghbrDivRate2))_{CNGb,yr-2} =$
$\alpha + \beta_1(ln(OwnDivRate2))_{CNGj,yr-2} + \beta_2(ln(OwnDivRate2) * Border)_{CNGj,yr-2} +$
$(X_{CNG,yr-1})\ \beta_3 + ((X_{CNG,yr-1}) * Border)\ )\ \beta_4 + (Year)\phi + ((Year) * Border)\phi' + (State *$
$time\gamma + (State * time * Border)\gamma' + \varepsilon CNG.yr$

The graphed residuals from the entire sample are provided in Figure 2. These residuals show that there exists ample of remaining variation in the key explanatory variable of interest, neighboring state's border region divorce rate, that is unexplained by the own state control variables.

Figure 2



Appendix Table 1

Robustness of Primary Findings to inclusion of own lagged logged-divorce-rate				
	1	2	3	4
Variables	lndivrate	lndivrate	lndivrate	lndivrate
Ln(Neighbor divrate)yr-2	0.0216	0.0385	0.0188	0.0199
	(0.0304)	(0.0553)	(0.0361)	(0.0365)
Ln(Neighbor divrate)$_{yr-2}$* border	0.0014	-0.0629	-0.0273	-0.0297
	(0.0264)	(0.0516)	(0.0299)	(0.0304)
CNGratio =Neighbors Current Percent of Total Border Pop		0.2297		
		(0.2936)		
CNGratio * border		-0.7586*		
		(0.3589)		
CNGratio *Ln(Neighbor divrate)yr-2		-0.0488		
		(0.1174)		
CNGratio*(Ln(Neighbor divrate)yr-2)* border		0.1860		
		(0.1436)		
0.40<= CNGratio<.6			-0.0394	-0.0339
			(0.0751)	(0.0752)
(0.40<CNGratio<0.60)* border			-0.0750	-0.0873
			(0.0800)	(0.0799)
Ln(Neighbor divrate)yr-2*(0.40<CNGratio<0.60)			0.0437	0.0401
			(0.0447)	(0.0447)
Ln(Neighbor divrate)yr-2*(0.40<CNGratio<0.60)*border			0.0583	0.0670
			(0.0432)	(0.0431)
0.60<=CNGratio<1.00			0.0458	
			(0.1015)	
(0.60<=CNGratio<1.00)* border			-0.2397+	
			(0.1253)	
Ln(Neighbor divrate)yr-2*(0.60<=CNGratio<1.00)			-0.0515	
			(0.0551)	
lnN2div*(0.60<=CNGratio<1.00)*border			0.1661*	
			(0.0704)	
0.60<= CNGratio<.78				0.1005
				(0.1038)
(0.60<= CNGratio<.78) * border				-0.3607**
				(0.1213)
lnN2div* (0.60<= CNGratio<.78)				-0.0868
				(0.0569)
lnN2div*(0.60<= CNGratio<.78)*border				0.2437***
				(0.0675)
0.78 <= CNGratio<=1.0				-0.0722
				(0.1902)
(0.78 <= CNGratio<=1.0)* border				0.0360
				(0.2603)
(0.78 <= CNGratio<=1.0) *lnN2div				0.0670
				(0.0993)
lnN2div*(0.78 <= CNGratio<=1.0)*border				-0.1089
				(0.1264)
Ln(Own Divorce Rate)$_{yr-2}$	-0.0082	-0.0076	-0.0113	-0.0126
	(0.0466)	(0.0467)	(0.0467)	(0.0465)
(Ln(Own Divorce Rate)$_{yr-2}$) * Border	0.1476+	0.1488+	0.1527*	0.1474+
	(0.0767)	(0.0772)	(0.0767)	(0.0753)
employment rate - lagged	-0.0005	-0.0005	-0.0006	-0.0006
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
(employment rate - lagged) * border	0.0006	0.0006	0.0007	0.0007
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Population density - Lagged	0.0000	0.0000	0.0000	0.0000
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
(Population density-lagged) * border	0.0001	0.0001	0.0001	0.0001
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
CNG bin fixed effect	X	X	X	X
Year Dummies	X	X	X	X
Linear State-Time Trends	X	X	X	X
	3,560	3,560	3,560	3,560

Despite the theoretical predictions of varying returns to scale pending on population levels, which indicate that a non-linear specification is most appropriate, *CNGratio* is initially interacted directly with the neighboring state divorce rate in a continuous and linear form.

TABLE HERE WITH X, X^2

Appendix Table 2

Non-Parametric Specification				
	1	2	3	4
Variables	lndivrate	lndivrate	lndivrate	lndivrate
Ln(Neighbor divrate)yr-2	0.0199	0.0188	0.0203	0.0199
	(0.0348)	(0.0359)	(0.0350)	(0.0363)
Ln(Neighbor divrate)$_{yr-2}$* border	-0.0372	-0.0356	-0.0390	-0.0375
	(0.0290)	(0.0288)	(0.0295)	(0.0291)
0.40<= CNGratio<.6	-0.0351	-0.0383	-0.0314	-0.0328
	(0.0727)	(0.0751)	(0.0727)	(0.0751)
(0.40<CNGratio<0.60)* border	-0.0779	-0.0651	-0.0900	-0.0785
	(0.0810)	(0.0820)	(0.0816)	(0.0820)
Ln(Neighbor divrate)$_{yr-2}$*(0.40<CNGratio<0.60)	0.0446	0.0433	0.0420	0.0397
	(0.0437)	(0.0446)	(0.0436)	(0.0445)
Ln(Neighbor divrate)$_{yr-2}$*(0.40<CNGratio<0.60)*border	0.0604	0.0542	0.0689	0.0635
	(0.0446)	(0.0439)	(0.0450)	(0.0440)
0.60<=CNGratio<1.00	0.0562	0.0463		
	(0.1017)	(0.1013)		
(0.60<=CNGratio<1.00)* border	-0.2446+	-0.2237+		
	(0.1255)	(0.1292)		
Ln(Neighbor divrate)$_{yr-2}$*(0.60<=CNGratio<1.00)	-0.0477	-0.0510		
	(0.0554)	(0.0540)		
Ln(Neighbor divrate)$_{yr-2}$*(0.60<=CNGratio<1.00)*border	0.1664*	0.1601*		
	(0.0704)	(0.0730)		
0.60<= CNGratio<.78			0.0924	0.1008
			(0.1019)	(0.1032)
(0.60<= CNGratio<.78) * border			-0.3621**	-0.3506**
			(0.1241)	(0.1248)
lnN2div* (0.60<= CNGratio<.78)			-0.0708	-0.0860
			(0.0555)	(0.0555)
lnN2div* (0.60<= CNGratio<.78) * Border			0.2415***	0.2411***
			(0.0688)	(0.0703)
0.78 <= CNGratio<=1.0			0.0223	-0.0713
			(0.2068)	(0.1896)
(0.78 <= CNGratio<=1.0)* border			-0.0523	0.0333
			(0.2741)	(0.2672)
(0.78 <= CNGratio<=1.0) *lnN2div			0.0323	0.0672
			(0.1096)	(0.0985)
Ln(Neighbor divrate)$_{yr-2}$*(0.78 <= CNGratio<=1.0)*border			-0.1026	-0.1314
			(0.1349)	(0.1304)
CNG bin fixed effect	X	X	X	X
Year Dummies	X	X	X	X
Linear State-Time Trends	X	X	X	X
	3,560	3,560	3,560	3,560
Robust standard errors in parentheses				
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10				

Table 3 - SE clustered at State Level

Non-Parametric Specification				
	1	**2**	**3**	**4**
Variables	**Indivrate**	**Indivrate**	**Indivrate**	**Indivrate**
Ln(Neighbor divrate)yr-2	**0.0199**	**0.0188**	**0.0203**	**0.0199**
	(0.0365)	**(0.0378)**	**(0.0368)**	**(0.0383)**
Ln(Neighbor divrate)$_{yr-2}$* border	**-0.0372**	**-0.0356**	**-0.0390**	**-0.0375**
	(0.0294)	**(0.0286)**	**(0.0301)**	**(0.0291)**
Ln(Neighbor divrate)$_{yr-2}$*(0.40<CNGratio<0.60)*border	**0.0604**	**0.0542**	**0.0689**	**0.0635**
	(0.0500)	**(0.0484)**	**(0.0513)**	**(0.0494)**
Ln(Neighbor divrate)$_{yr-2}$*(0.60<=CNGratio<1.00)*border	**0.1664***	**0.1601***		
	(0.0752)	**(0.0764)**		
Ln(Neighbor divrate)$_{yr-2}$* (0.60<= CNGratio<.78) * Border			**0.2415****	**0.2411****
			(0.0708)	**(0.0684)**
Ln(Neighbor divrate)$_{yr-2}$*(0.78 <= CNGratio<=1.0)*border			**-0.1026**	**-0.1314**
			(0.1561)	**(0.1409)**
State Divorce Law Controls	.	X	.	X
Population Density Controls	.	X	.	X
Employment Rate controls	.	X	.	X
CNG Bin FE	X	X	X	X
Year Dummies	X	X	X	X
Linear State Time Trends	X	X	X	X
	3,560	**3,560**	**3,560**	**3,560**

Standard errors presented in parentheses. SE are clustered at the State level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10

Table 3

Non-Parametric Specification				
	1	2	3	4
Variables	lndivrate	lndivrate	lndivrate	lndivrate
Ln(Neighbor divrate)yr-2	0.0199	0.0188	0.0203	0.0199
	(0.0348)	(0.0359)	(0.0350)	(0.0363)
Ln(Neighbor divrate)$_{yr-2}$* border	-0.0372	-0.0356	-0.0390	-0.0375
	(0.0290)	(0.0288)	(0.0295)	(0.0291)
Ln(Neighbor divrate)$_{yr-2}$*(0.40<CNGratio<0.60)*border	0.0604	0.0542	0.0689	0.0635
	(0.0446)	(0.0439)	(0.0450)	(0.0440)
Ln(Neighbor divrate)$_{yr-2}$*(0.60<=CNGratio<1.00)*border	0.1664*	0.1601*		
	(0.0704)	(0.0730)		
Ln(Neighbor divrate)$_{yr-2}$* (0.60<= CNGratio<.78) * Border			0.2415***	0.2411***
			(0.0688)	(0.0703)
Ln(Neighbor divrate)$_{yr-2}$*(0.78 <= CNGratio<=1.0)*border			-0.1026	-0.1314
			(0.1349)	(0.1304)
State Divorce Law Controls	.	X	.	X
Population Density Controls	.	X	.	X
Employment Rate controls	.	X	.	X
CNG Bin FE	X	X	X	X
Year Dummies	X	X	X	X
Linear State Time Trends	X	X	X	X
	3,560	3,560	3,560	3,560

Standard errors presented in parentheses. SE are clustered at the CNG level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10