An Empirical Exploration of the Determinants of Divorce

By

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The final copy of this thesis has been examined by the signatories, and we find that both the content and the form meet acceptable presentation standards of scholarly work in the above mentioned discipline.

Murray, Sheena L. (Ph.D., Economics)

An Empirical Exploration of the Determinants of Divorce

Thesis directed by Assistant Professor Brian Cadena

This dissertation explores catalysts to divorce and the effects of different shocks to marital stability. In order to determine how marriage market participation and job opportunities affect marital stability, a panel data set was constructed of all marriages and divorces (or annulments) granted in each county in the United States from 1965 to 1988. The divorce records are merged with county- level employment and population levels to estimate the employment and divorce rates. Using county level data this dissertation is able to exploit a number of labor-market geographical observation levels, such as state border regions, Statistical Metropolitan Areas (SMA), and Labor Market Areas (LMA).

The first chapter analyzes how changes in the number of available marriage-market participants in a community affect the marital stability of existing couples in the area. The analysis focuses on border regions of neighboring states and assesses the impact of fluctuations in divorcee population in one state on the divorce rates in the neighboring states' border region. Large and statistically significant effects are identified in border regions where the neighboring state's border population is larger than one's own border population, which is consistent with the theoretical models on the subject.

In the second and third chapters, attention is turned to how employment opportunities affect marital stability. In chapter two, I use my unique data to more precisely determine the relationship between employment rates and divorce. Using a fixed-effect panel-data model at the LMA level, the results indicate a strong a pro-cyclical relationship between divorce and the business cycle. Finally, in chapter three, the focus of the research transitions, from temporary employment fluctuations, to how permanent changes in the labor market affect marriage. Exploiting structural changes to the labor markets of steel and coal mining, an instrumental variable approach is used that interacts county-level steel and coal industry-concentrations with a national-level demand measure. The model estimates a strong positive relationship between the real-earnings of low-skilled male workers in the county and the county-level divorce rate in steel regions but finds minimal effects on divorce rates in coal regions.

Dedication

To my parents, who have supported and encouraged me unconditionally throughout this process.

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Any remaining errors or omissions are my own

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Introduction

This dissertation explores different catalysts to divorce and the effects of different shocks to marital stability. In order to determine how marriage market participation and job opportunities affect marital stability, a panel data set was constructed of all marriages and divorces (or annulments) granted in each county in the United States from 1965 to 1988. The divorce and marriage data is attained from the annual editions of the National Vital Health Statistics, Volume 3, Marriage and Divorce. To this data, the intercensal county population data, provided by the US Census, is added to create a per-capita measurement, or the county-level crude divorce and marriage rates. Using county level data this dissertation is able to exploit a number of labor-market geographical observation levels, such as at Statistical Metropolitan Areas (SMA), or Labor Market Areas (LMA).

The first chapter analyzes how changes in the number of available marriage market participants in a community affects the marital stability of existing couples in the area. Theoretical models have predicted that an increase in the pool of singles will positively impact 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 available marriage market participants affect divorce rates.

The analysis, in chapter one, focuses on border regions of neighboring states and assesses the impact of fluctuations in divorcee population in one state on the divorce rates in the neighboring states' border region. Specifically, the research question asks if border counties of a state are affected more than the state's interior counties by the divorce rates of the neighboring state's border region. By utilizing integrated, cross-state border regions, I am able to identify the impact of increased marriage market participation on the divorce rates of those in border counties of the neighboring state, while controlling for their divorce laws and labor market conditions.

Border-county divorce rates are found to increase in comparison to interior regions of the state, after a spike in the divorce rate of the neighboring state's border region. The large and statistically significant effects are only identified in border regions where the neighboring state's border population is larger than one's own border population, which is consistent with the theoretical models on the subject. The findings are robust to a number of specification alternatives and provide strong evidence that an increase in the number of marriage market participants is destabilizing to existing marriages.

In the second and third chapters, attention is turned to how employment opportunities affect marital stability. How the local economy affects divorce is an important element in understanding divorce during turbulent economic times. All previous research on divorce and the business cycle has used either cross-sectional analysis with county- or metropolitan-area data, or panel analysis with state-level observations. Using the unique data set discussed above, this chapter better measures how employment opportunities affect marital stability. Panel-level data provides a more refined observation level than has previously been used to research the topic. The divorce records are merged with county- level employment levels from the Bureau of Economic Analysis (BEA) to estimate the employment rate in each county.

To more precisely measure local marriage and labor markets, the county-level observations are aggregated to census-defined Labor Market Areas (LMAs). LMAs are determined using commuter flow data and are designed to accurately capture the geographical unit of a labor market. Using panel data and a fixed-effect model, I am able to more accurately assess how fluctuations in the local employment rate affect local divorce rates. Lastly, the observation level provides enough variation over-time and across observations, to test for heterogeneous treatment effects depending on the urban density levels in the LMA. A strong, robust, and pro-cyclical relationship between employment rates and divorce rates is consistently found. Importantly the heterogeneous testing shows that the strong positive effect is driven by rural regions. When the effects of the employment rate are allowed to vary with urban density, results show a counter-cyclical effect in urban areas, but a positive effect in rural areas.

In the final chapter, the focus of the research transitions from temporary employment opportunity fluctuations to the effects of permanent changes in the labor market on marriage. How job destruction and non-transitory shocks to wages affects marital stability are analyzed using the industries of steel production and coal mining. These industries are utilized in the paper on as both industries are characterized by relatively well-paid, low-skilled, male workers. Additionally, during the 1970s and 1980s both of these industries underwent major structural changes. During this time the U.S. experienced an almost fifty percent decline in steel production, and fluctuating oil prices resulted in both the boom and bust of coal mining. The loss of these high-paying, low-skilled, and predominantly male-filled jobs represents a permanent wage shock to the county and state residents where these industries were concentrated.

In this chapter, I evaluate how the loss of high-paying low-skilled male jobs has affected the divorce rate in the regions where these jobs were concentrated. Using an instrumental variable approach, which interacts county-level industry concentration with a national-level demand measure, real earnings are estimated at the county level and the relationship between low-skilled male earning potential and divorce rates are analyzed, in regions dependent on steel and coal production. Results indicate that in regions with a high concentration of steel, divorce rates were disproportionally driven down during the decline of the industry; however, the bust of the coal industry did not appear to disproportionally affect the local divorce rates in counties with high coal concentrations.

Chapter 1

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

1.1 Introduction

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. A difference-in-differences approach is used 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.

The neighboring state's divorcees are used 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, a pattern of coefficients that strongly supports a divorce spillover effects is found. 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.

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

1.2.1 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.

1.2.2 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.

1.2.3 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. (2004) 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, 1979, 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 the regression specifications and indicate that a non-linear 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.

1.3 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 intercensal county population, provided by the US Census, is added, to create the crude divorce rate, or the number of divorces per 1,000 people.[2] Additionally, key controls for variables known to affect divorce rates, such as population density and employment rates are added to the data set (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, state level divorce laws are added for controls. The 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 intervals, until year 10, when a 10+ year dummy is used, with the omitted category as any time prior to law passage (Wolfers, 2006).

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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, 09; 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.

1.3.1 Observational Unit

Due to the geographical nature of the observations, both a specific example and general description are provided to give a better understanding of how the panel data-set and observation level is constructed. To create each observation, the determine the closest neighboring state to each county, and the distance to that neighboring state from the county centroid, is determined first. 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 the sample is aggregated 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, two key groups are further identified, a CNG's border counties and the interior counties. A border county is defined 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] Thirty miles was selected as a cut off for the "border" counties as in quantitative research 96% of contiguous border counties were within 26 miles of the border.

The 30 mile limit on border counties was defined in an attempt to capture the majority of all contiguous counties while also providing a large enough sample for accurate identification. As the data used in the analysis does not contain information on whether a county is contiguous,

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[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.

or directly shares a border with a neighboring state, this cut off allows the sample to capture the majority of all contiguous counties. Additionally, approximately 25% of the counties in the defined sample are within 30 miles of the border. Limiting the mile cutoff to less than 30 miles, say for example at 20 miles, there would be far fewer observations, with approximately 10% of the sample counties meeting the criteria; as many counties are more than 40 miles wide, and the measurement is taken from the county centroid. Therefore by using a 30 mile cut off the limit attempts to capture all contiguous counties, without knowing if they are contiguous, and create a representative sample of counties for identification.

An interior county is defined as one that is not in an interstate LMA and that has a centroid 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 have a centroid between 30 and 50 miles are omitted because they could be considered partially treated[6]. Counties that 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

[5] Additional mileage cut offs are intended for supplementary robustness checks..

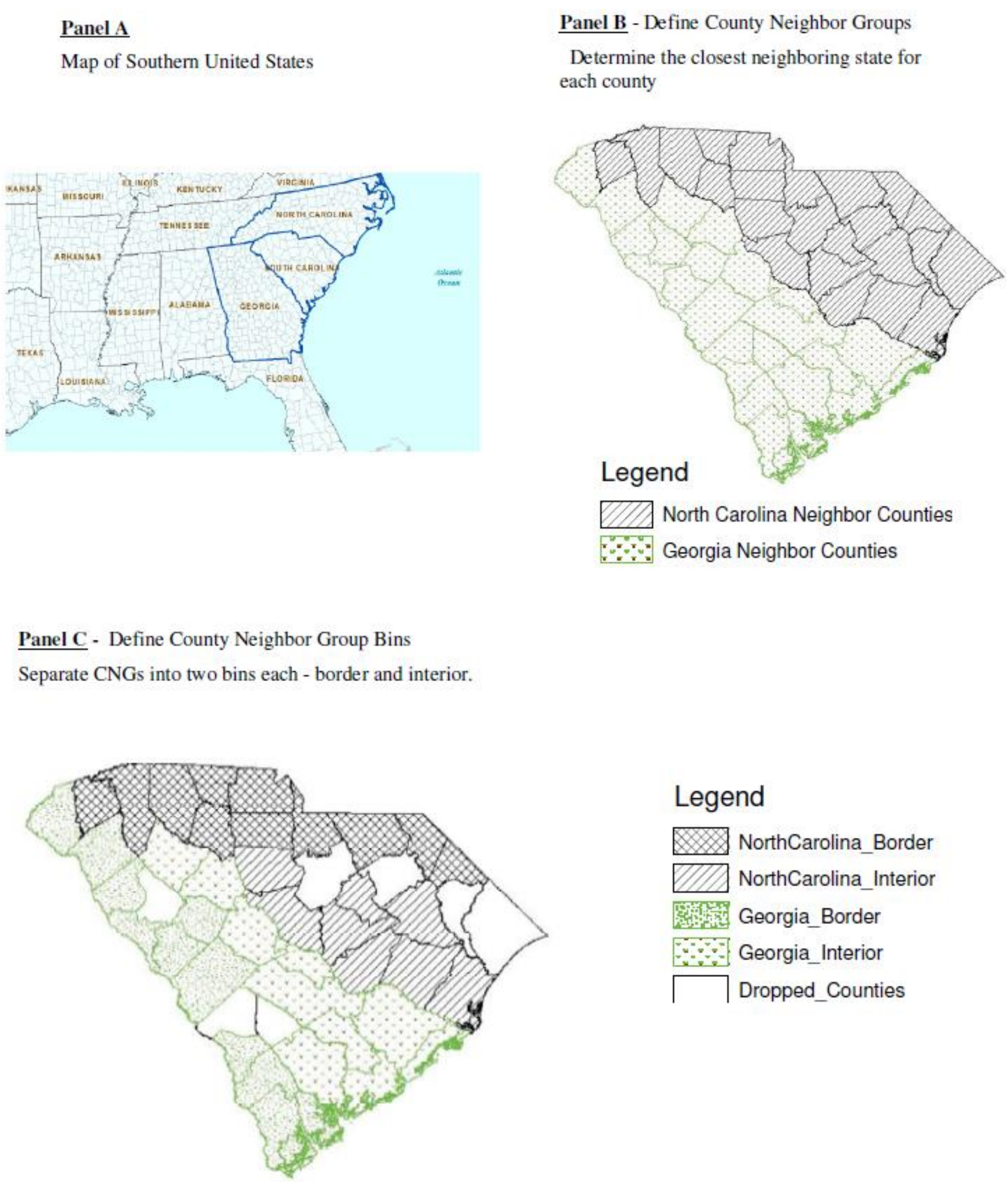
[6] Ideally, I would allow contiguous counties, whose centroid is between 30 and 50 miles to be included in the border observations (at least as a robustness check to the current specification). However, due to data limitations, I am unable to identify these counties without examining each dropped observation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

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 A.1.1, for a CNG sorted map of Alabama with four neighboring states.

Figure 1.1

Creation of County Neighbor Group Bins- South Carolina Example



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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).

1.3.2 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 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 3,033 counties in the continental United States, 1,893 counties remain in the sample defined above. Of those 1,893 counties, there are 1042 counties residing on the border of states and 851 counties classified as interior.[9]

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[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 1.1.

[9] See Appendix Table A.1.1 for a more thorough breakdown of observations lost in each sample restriction.

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 1.1, and all averages are given in terms of the treated state.

Table 1.1 displays a comparison of descriptive statistics for the interior and border CNG bins taken in 1980. 1980 was selected 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 population-weighted average of the variables across the counties in the given CNG bin.

Table 1.1 - Descriptive Statistics of County Neighbor Group Bins

Descriptive Statistics of County Neighbor Group Bins	**Border**	**Interior**	**Pr(|T |>| t|)** * Indictes a Significant difference at a 5% level	**Border**	**Interior**
Total Number of Counties	1042	851			
Total Number of CNGs	96	96			
	Mean			**Median**	
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 1,000	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)			

All Statistics Observed in 1980. 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. The mean distance exceeds the 30 mile limit due to the inclusion of counties who are in interstate LMAs. 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 a Standard Metropolitan Statistical Area (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.

1.4 Empirical Strategy & Results

In what follows, difference-in-differences and interaction strategies are used 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 can be made, 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 that 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 that could disproportionately affect the border region's divorce rate in each state.

To provide further support to the divorce spillover interpretation, heterogeneous effects of neighboring state divorce rates depending on population levels are examined. 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 that can capture this relationship must be constructed from the data. Although the data set does not have an explicit measure of the stock of singles, general population measures can be used 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.

1.4.1 Construction of Population Level Treatment Variable

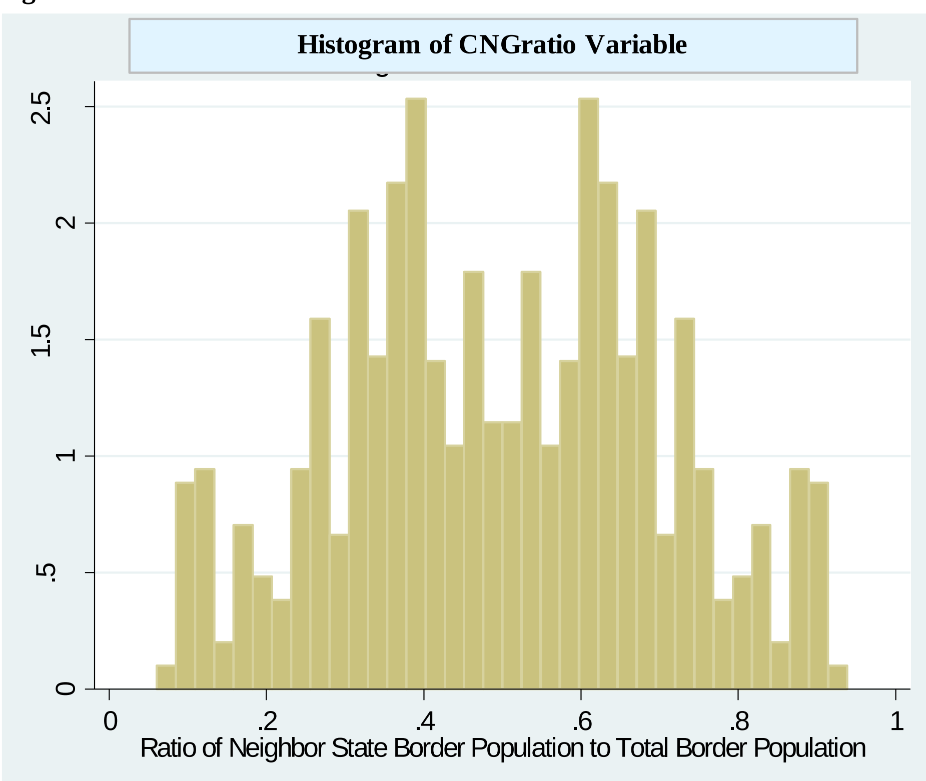
A ratio, *CNGratio*, is created and shown in equation 1.1, using the size of the population of each state's border region to act as a treatment, or dosage, variable. *CNGratio* is equal to the population in the neighboring state's border region over the total population in the conjoined border regions. 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), linked CNG border areas were created to enable the model to assess how the ratio of the populations in the linked border region affect spillover effect. For example, in 1980, South Carolina's border region with

Georgia consisted of approximately 480,000 residents on the South Carolina side of the border and 783,000 on Georgia's side. Therefore, in 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 1.2 shows the distribution of *CNGratio* where 25% have 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.

$$CNGratio = \qquad (1.1)$$

$$[(\text{Neighbor state Border Population}) / (\text{Own State Border Pop} + \text{Neighbor State Border Pop})]$$

Figure 1.2



1.4.2 Difference-in-Difference Regression Results

Equation 1.2 shows the preliminary regression equation,

$$
\begin{aligned}
\ln\left(CNG\ Div\ rate_{CNGj,yr}\right) \\
&= \alpha + \beta_1(\ln(NghbrDivRate2))_{CNGj,yr-2} + \beta_2(Border) \\
&+ \beta_3(\ln(NghbrDivRate2) * Border)_{CNGj,yr-2} + \left(X_{CNGj,yr-1}\right)\beta_4 \\
&+ \left(X_{CNGj,yr-1}\right) * Border)\ \beta_{4'} + (CNG)\varphi + \left((CNG) * Border\right)\varphi' + (Year)\phi \\
&+ \left((Year) * Border\right)\phi' + (State * time)\gamma + ((State * time) * Border)\gamma' \\
&+ \varepsilon_{CNG.yr}
\end{aligned}
\tag{1.2}
$$

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 interacted 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 and Mosher, 2002). 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. A falsification test of this lag is presented in Table A.1.5 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 region's 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 1.2, 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. As the regression equation does not consider the population ratio, the initial results assume that the effect of divorce rates on current marriages is homogeneous across population levels.

Table 1.2

Homogeneous-Treatment Specification Results		
	1	**2**
VARIABLES	**ln(div rate)**	**ln(div rate)**
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.

1.4.3 Triple Interaction Regression Results

A dosage variable is introduced in Equation 1.3 using CNGratio that 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.

$$\begin{aligned} \ln\left(CNG\ Div\ rate_{CNGj,yr}\right) &= \alpha + \beta_1(\ln(NghbrDivRate2))_{CNGj,yr-2} + \beta_2(Border) \\ &+ \beta_3(\ln(NghbrDivRate2) * Border)_{CNGj,yr-2} + \beta_4 Dose \\ &+ \beta_5(Dose * Border) + \beta_6(Dose * \ln(\ln(NghbrDivRate2))_{CNGj,yr-2} \\ &+ \beta_7(\ln(NghbrDivRate2) * Border * Dose)_{CNGj,yr-2} + \left(X_{CNG,yr-1}\right)\beta_8 \\ &+ \left(X_{CNG,yr-1}\right) * Border)\ \beta_{8'} + (CNG)\varphi + \left((CNG) * Border\right)\varphi' + (Year)\phi \\ &+ \left((Year) * Border\right)\phi' + (State * time)\gamma + ((State * time) * Border)\gamma' \\ &+ \varepsilon_{CNG.yr} \end{aligned} \tag{1.3}$$

In order to test this theory, the empirical specification is adjusted 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 1.3, show

CNGratio interacted linearly with the difference-in–difference variable of interest. In equation 1.3, 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 a more flexible regression is specified 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.

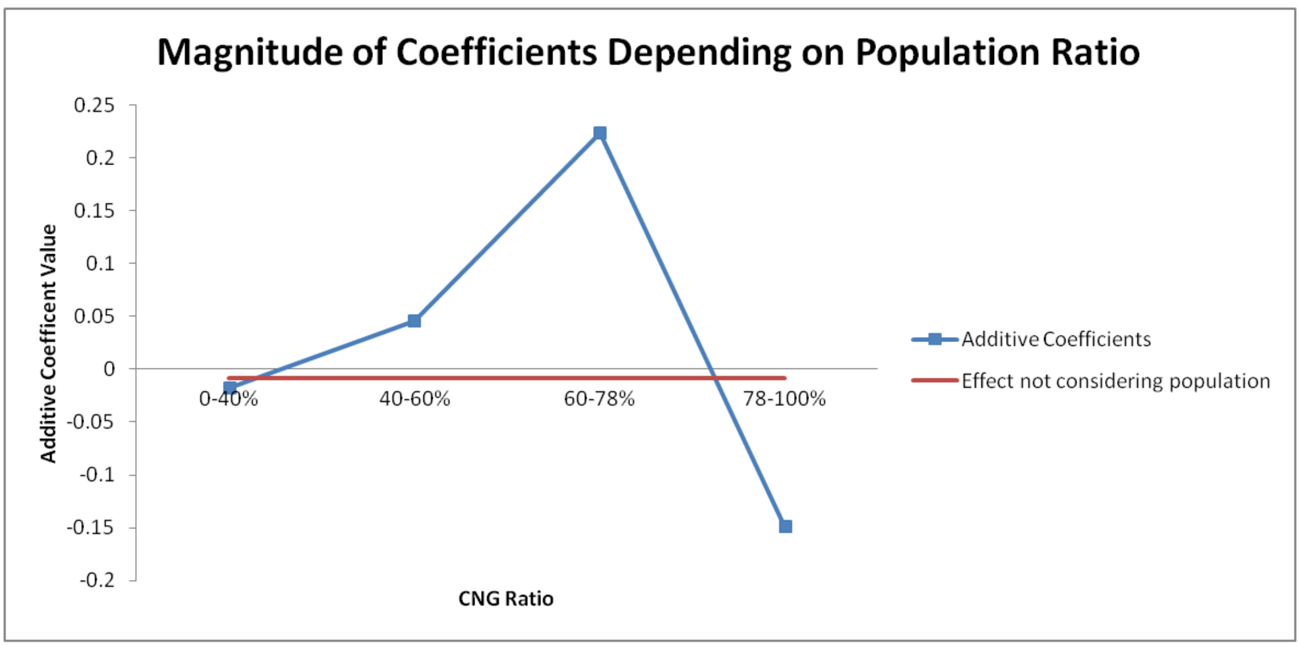
Table 1.3

Heterogeneous Treatment & Non-Linear Specification Results			
	1	**2**	**3**
Variables	**lndivrate**	**lndivrate**	**lndivrate**
Ln(Neighbor divrate)yr-2	0.0383	0.0188	0.0199
	(0.0545)	(0.0359)	(0.0363)
Ln(Neighbor divrate)$_{yr-2}$* border	-0.0669	-0.0356	-0.0375
	(0.0519)	(0.0288)	(0.0291)
Ln(Neighbor divrate)$_{yr-2}$*(CNGratio)*border	0.1688		
	(0.1495)		
Ln(Neighbor divrate)$_{yr-2}$*(0.40<CNGratio<0.60)*border		0.0542	0.0635
		(0.0439)	(0.0440)
Ln(Neighbor divrate)$_{yr-2}$*(0.60<=CNGratio<1.00)*border		0.1601*	
		(0.0730)	
Ln(Neighbor divrate)$_{yr-2}$* (0.60<= CNGratio<.78) * Border			0.2411***
			(0.0703)
Ln(Neighbor divrate)$_{yr-2}$*(0.78 <= CNGratio<=1.0)*border			-0.1314
			(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
Year Dummies	X	X	X
Linear State Time Trends	X	X	X
Observations	3,560	3,560	3,560

Standard errors presented in parentheses. SE are clustered at the CNG level.
The results are robust to clustering the SE at the state level, these results can be seen in Appendix Table A.1.3
*** 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 1.3 graphs the net effect, or the magnitude of the additive coefficients shown in column 6 for each given range of CNGratio.

Figure 1.3



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.

1.5 Conclusion

In this paper a comparative statics test is performed 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 rat. 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. Results consistently show 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.

Chapter 2

Matrimonial Bonds and Bank Accounts: The Effects of local Macroeconomics Shocks on Divorce Rates

2.1 Introduction

As the United States works through one of its deepest recessions, anecdotal evidence continues to build in the popular media regarding individuals delaying divorce due to financial constraints (Leland 2008, Yglesisas 2012, Editors 2009). Although not heavily researched, the existing literature on the subject provides support for the media's anecdotal inference that divorce rates are driven down during hard times. Whether divorce is pro-cyclical to the business cycle (as postulated in the media) or counter-cyclical is theoretically ambiguous and remains a source of controversy in the family economics literature (Amato & Beattie, 2011; Hellerstein & Morrill, 2010; Roy, 2011; Weiss & Willis, 1997). Individual household-level data appears to suggest that unemployment is destabilizing to marriage; however, analysis of state-level data indicates that divorce is pro-cyclical, as divorce or marital-dissolution rates decline during periods of poor economic growth.

The relationship between job opportunities and marital stability is defined by two conflicting influences. The first force is the stress that economic hardship can put on relationships due to psychosocial and financial distress, which can increase the likelihood of divorce. However, the second influence on households comes from the costs of divorce, loss of shared goods, and risk aversion, which can decrease the likelihood of divorce. Furthermore, options one has for outside his/her marriage also become less attractive during a recession, as employment and income levels decline for all members of the marriage and labor market.

Even if a couple is not experiencing a firsthand job loss, they experience fear and anxiety in a down economy. If this destabilizing relationship between poor macroeconomic conditions and divorce dominates, then divorce would be counter-cyclical to the business cycle. Consequently, we would expect an increase in divorce during a recession. However, when seeking to divorce and to dissolve a household, one must consider the legal fees associated with divorce, as well as the need to support two separate households instead of one. If risk aversion and high costs force households to re-evaluate or postpone divorce, it would imply that divorce is pro-cyclical to the business cycle and we would expect lower divorce rates during economic downturns.

To these already conflicting forces we must add the additional component of how local employment opportunities in the community can affect an individual's evaluation of their marriage, relative to the alternative options such as re-marriage or becoming single. A partner's failure (or success) during different points of the business cycle may have heterogeneous effects on marital stability, particularly if the job loss (or gain) is considered a signal of future employment issues. For example, a spouse losing his/her job during a time of economic expansion may provide a stronger signal of future employment struggles than when that same spouse loses a job during a recession. Therefore, it is important to control for the general economic conditions in the labor force in both aggregate and household-level research on divorce.

Since the results of the existing studies vary with different levels of observational analysis, selecting the appropriate observation level is crucial to determine how employment opportunities in a labor market affect marital stability. A labor market is composed of companies and employees; therefore, in order to capture how social variables are affected by employment markets, the geographical unit of analysis must encompass both the place of work and the place of residence of the labor-market population. Although obviously a large and inclusive observational unit, states are arguably not unified labor or marriage markets. One would not expect the job opportunities in Los Angeles to affect the relationships of the individuals in San Francisco. Therefore it is unfortunate that all existing panel research on the relationship between divorce and the business cycle using macro-level fluctuations has only been conducted at the state level.

I seek to provide a more accurate level of observational analysis by combining county-level data to create census-defined Labor Market Areas (LMAs). By using census-defined LMAs (as determined by journey-to-work data in the decennial Census), I am able to more accurately test how local job creation and destruction affects the local divorce rate. Specifically, this paper examines how changes in the lagged and current employment rate in an LMA affect the current divorce rate. Additionally, I exploit variation in urban densities across LMAs to test if there exists heterogeneous treatment effects of employment opportunities by urban density levels.

2.2 Previous Work

Research at the household level has found that job loss or earning fluctuations are destabilizing to households (Weiss & Willis, 1997). However, firsthand job loss can be related to family stability and is therefore likely endogenous. Charles and Stephens (2004) examine more plausibly exogenous shocks to a household, such as disability or plant closing. Their findings indicate that the risk of divorce increases in response to a primary earner's layoff, but not if the job loss is the result of disability or plant closure. Therefore their research supports that it is not just labor market struggles, but the sources of those struggles, which can affect a partner's considerations. Additionally, the study highlights the effect that general macro-economic conditions can have on divorce decisions.

2.2.1 Macro-Level Studies

Unemployment rate fluctuations are plausibly exogenous to individual households and affect individuals through both actual and potential loss of a job. Recent research mapping state-level unemployment to state-level divorce rates finds that divorce is pro-cyclical. Amato and Bettie (2011) use state level data from 1960 to 2000 in five-year increments to estimate the state level relationship between divorce and unemployment rates. They find that since 1980 there is a negative and statistically significant relationship between unemployment and divorce. However, their use of state-level data in only five-year increments makes it difficult to truly extrapolate from their findings.

Hellerstein and Morrill (2011) increase Amato and Bettie's sample size and map annual unemployment rates to state-level, annual divorce rates for all years from 1976 to 1998. Additionally, they add linear state-time trends to control for large secular changes over time. Their findings suggest a robust negative relationship between the concurrent unemployment rate and the divorce rate, indicating a pro-cyclical relationship. However, they use only state-level data and their estimation technique assumes an equal percentage point change across all states as a result of macroeconomic conditions.

Schaller (2013) expanded the state-level panel to 2008 and estimates her regressions in terms of percentage points instead of percentages, as other authors had done prior to her. In this sample, she also finds a pro-cyclical relationship as her results indicate that a one percentage point increase in unemployment is associated with a decrease in 0.34 divorces per 1,000. She also tests for heterogeneous effects by race and age groups. Age and race data are obtained from the CPS and limit her sample to 1978-1995. In her samples, stratified by age and race, she finds that employment affects the marriages of those between the ages of 16 and 45 years old most strongly. Additionally she found that there does not appear to be a disproportionate effect on the marriage outcomes of African Americans versus the marriage outcome of white individuals, when subjected to the same group-specific unemployment rates.

Amato and Bettie (2011) postulate that the pro-cyclical relationship between the business cycle and the divorce rate may be driven by increases in the standard of living in recent years, which have caused divorce to be more costly. Additionally, the loss of shared goods and high levels of risk-aversion may be strengthening the pro-cyclical relationship found in recent decades. These macro-level studies that regress state-level divorce rates on state-level unemployment rates have shown a pro-cyclical relationship in more recent years, but the magnitude of the relationship and the robustness of the findings to different functional forms is questionable.

2.2.3 Micro-Level Studies

The studies that check the robustness of these state-level studies to the inclusion of family-level demographic variables have found weaker, although mostly pro-cyclical, results between divorce rates and the business cycle. These studies continue to use the general unemployment rate as the primary independent variable of interest, but test its effect on individual and household-level outcomes instead of aggregate level divorce rates. Roy (2011), using data from the Household Income and Labor Dynamics in Australia, finds that unemployment shifts in either the female or male work sector are destabilizing for cohabitating couples, but have negligible effects on married couples.

Using an American sample from participants in the Survey of Income and Program Participation (SIPP), Hellerstein and Morrill (2011) analyze the effects of state-level unemployment on couples who have been together for 10 years or less and find a pro-cyclical relationship. Their findings indicate that an increase in the state unemployment rate decreases the probability of separation for cohabitating and married couples, although most drastically for married couples. Arkes and Shen (2014), also use an American sample, to analyze the effects of unemployment on couple's divorce decisions. Using data on married couples, in their first marriage, from households surveyed in the National Longitudinal Study of Youth -level data they find a weakly counter-cyclical relationship between divorce and the business cycle. Recessions are found to increase the risk of divorce for couples in their first marriage, but only for couples in years 6 through 10 of their marital tenure.

These above mentioned household-level findings vary between pro-cyclical and counter-cyclical results depending on the subsample of couples being examined. The micro-level studies do not always support the aggregate level findings, but the above household-level studies limit their sample by the duration of marriage or to only first marriages and are therefore not representative of the married population. Consequently, the results from these studies do not necessarily undermine the aggregate-level studies, mentioned in section 2.A, which find stronger pro-cyclical patterns at the state level.

2.3 Contribution

The existing conflict in the previously reviewed literature presents a conundrum. If state level data is simply an aggregation of micro–level household data, the household research suggests divorce should be either minimally or positively affected by recessions. However, all of those using state-level data have consistently found that divorce is suppressed by recessionary periods. A first key step to understanding the difference in researchers' findings is to ensure that in the macro-level data, the level of observation is correct. There is as much variance in employment within a state as there is across states. By aggregating data up to the state level, much of the variation is washed out and the unemployment rate assigned to the entire state does not typically reflect the employment conditions of individuals in the state. This creates a key explanatory variable constructed with a great deal of measurement error. Therefore the bulk of these macro-level findings are questionable, as they use observations that are arguably not labor markets to answer questions on the effects of labor market conditions on households.

In this paper, local labor market variables, which vary within a state, are used to study the relationship between local labor market conditions and divorce rates. Using county-level court and Bureau of Labor Statistics records, this study is able to analyze the relationship between employment and divorce at a more appropriate geographical scale than that of the state. County-level records are aggregated up to census defined "Labor Market Areas" (LMAs) to better capture the risk of job loss present in a given community. Therefore this analysis aims to more accurately assess the effects of employment rates on local households. Findings indicate that employment rates strongly predict divorce rates at the LMA level. Employment rates are shown to be positively related to divorce rates indicating that divorce is pro-cyclical. Additionally, results of tests for heterogeneous effects, indicate that the marriages of those who live in more urban LMAs are less affected by employment rate fluctuations than those living in more rural areas.

2.3.1 Observation Level

This paper is unique because it uses a new geographical measurement, Labor Market Areas (LMAs) to analyze the relationship between divorce and employment rates. LMAs were constructed in 1980 by a joint research effort between the United States Department of Agriculture (USDA) and the Economic Research Service Group (ERS). The primary goal in identifying LMAs was to enable researchers to "assess the influence of the local economy on the socioeconomic well-being of workers residing in markets" (Tolbert & Killian 1987, pg 2).

In order to address the many limitations of the existing geospatial measurements in 1980, such as counties, Census-County-Groups and Metropolitan statistic Areas, the USDA and ERS decided that a geographical specification would be created that included, "all U.S. counties and county equivalents, used uniform criteria for designating labor market areas, employed the most recent journey-to-work data, did not require each area to have an urban center and could meet prevailing U.S. census confidentiality standards" (Tolbert & Sizer, 1996, pg 3).[10] The USDA and the ERS used the journey-to-work data from the decennial census to analyze commuting flows between counties, and consequently, the integration of labor markets. For counties to be linked together, in order to create an LMA, the first criterion is that they must be contiguous. Second order requirements are that: at least 25% of the employed residents of one county must have commuted to work in other counties within the LMA -- or -- at least 25% of the employees in a given county are accounted for by workers commuting from other counties in the LMA.[11] Lastly, to adhere to privacy standards all LMAs are required to have at least 100,000 residents.

2.3.2 Modeling

In addition to changing the unit of measurement from states to LMAs, I adjust the regression analysis to be estimated in percents instead of percentage-points by logging the dependent variable. Most existing research has estimated the effect of unemployment changes

[10] Counties cannot cross state lines and are too limited in scope to capture a larger portion of the links between living and working areas. Census-County-Groups are likewise limited in their scope, as they are defined by each state and therefore lack a consistent definition across all states. Furthermore, they are arbitrarily limited to state borders and therefore miss relationships that occur in interstate labor markets. Lastly, MSAs do not include rural regions, are not mutually exclusive at the county level, and do not encompass the entire US population. (Tolbert & Killian, 1987; Lichter et all, 1992; South and Lloyd, 1995).

[11] For more detail on how LMAs are constructed reference Tolbert and Sizer (1996) pages 14 - 18.

on the divorce rate in terms of percentage points. This estimation strategy requires the stringent assumption that the share of people in marginal marriages (those in which an economic shock can be destabilizing enough to instigate divorce) is the same in all states. In statistical terms, this estimation technique would require that the probability-density function of marital satisfaction is the same in all states.

However, states' existing divorce statistics provide evidence that different social norms make the residents of some states more or less prone to divorce than those in other states. Consequently, the same economic shock may affect divorce differently in a state with a historically low divorce rate than a state with a higher existing rate. By logging the dependent variable, divorce rates, the model allows the number of marginal marriages in each state to be modeled proportionally to the established propensity to divorce in each given state. Furthermore, as divorce rates are always positive, logging the variable is a natural and arguably more appropriate specification for the variable. (Lee & Solon, 2011)

2.3.3 Heterogeneity by Urban Density

The observational level of LMAs provides an ideal opportunity for additional testing for heterogeneous affects depending on urban density. A primary motivation in creating LMAs was to construct a more accurate measure of rural labor markets. Therefore, the use of this observational level provides the perfect opportunity to test for differential affects by urban density characteristics. There is ample variation across LMAs in the number of urban counties and rural counties within LMAs. It has been established in the literature that divorce rates tend to be higher in more urban areas, where many extramarital partners are available (South and Lloyd, 1995; South and Spitze, 1986). However, few tests have been run to see if urban areas respond more strongly to fluctuations in employment conditions. LMAs are an all-encompassing geospatial measure, and were intentionally designed to include all counties in the US, urban and rural. Because the level of urban density (or urbanicity), as measured by the percent of the population living in standard metropolitan statistical areas, varies across LMAs, I am able to test for heterogeneous effects by levels of urban density.

2.4 Data

This paper analyzes a newly constructed and hand-digitized panel data-set of all divorces and annulments granted in all counties from 1970 to 1988 in the continental U.S. The divorce data is obtained from the annual editions of the National Vital Health Statistics, Volume 3, Marriage and Divorce. The data is censored at 1988, as past this date the federal government stopped collecting county-level divorce data. Therefore, there are not any centralized records of the number of divorces granted at the county level after 1988. The intercensal county population data provided by the US Census are used to create a divorce rate-per-capita measurement, or the crude divorce rate.[12] County-level employment rates are obtained from the U.S. Department of Commerce, Bureau of Economic Analysis.[13] Total employment at the county level is used to construct an employed-to-population ratio, or the crude employment rate.

Lastly, the 1980 classification of LMAs is used to define unified geographical regions where people live and work in unified labor and marriage markets.[14] Counties are aggregated to the 1980 definitions of Labor Market Areas. The variables of interest are population-weighted averages taken over all counties in the LMA to create a total of 379 LMA-year observations.

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[12] data found at : http://www.nber.org/data/census-intercensal-county-population.html. Although true divorce rate (the number of divorce per the married population) may be superior to the crude rate, previous work has shown little difference between the two measurements in estimation and the annual married stock is not available at the county level (Wolfers, 06; Kneip &Bauer, 09; Hellerstein & Morrill, 08).

[13] Total Employment (Full time and Part Time) by industry as categorized by the SIC http://www.bea.gov/iTable/iTable.cfm?reqid=70&step=1&isuri=1&acrdn=5

[14] LMAs were first constructed in 1980, are constructed using the decennial census, and given the range of my data from 1970 to 1888 the 1980 LMA definitions are most appropriate.

Table 2.1 provides descriptive statistics of the full sample by region of the U.S., to give a sense of the geographical distribution and concentration of LMAs in the US. Although most analysis is conducted at the LMA level, basic descriptive statistics on state-level data are also provided in Table 2.1, as all previous research to date has been done only on state-level variables. Table 2.1 reports variable means in 1980, a year that is approximately midway through the analysis period. The average unemployment rate was 6.9 while the average employment rate was approximately 50%, with 502 people per 1000 employed on average in states. The average divorce rate was 5.5 per state, while it was 5.54 per LMA. The employment rate is the highest in the West in the state-level observations and also one of the highest in the LMA averages. At the LMA level, the West has the highest divorce rate with approximately 6.88 out of 1,000 individuals divorcing and the North has one of the lowest divorce rates at 3.7 divorces per thousand.

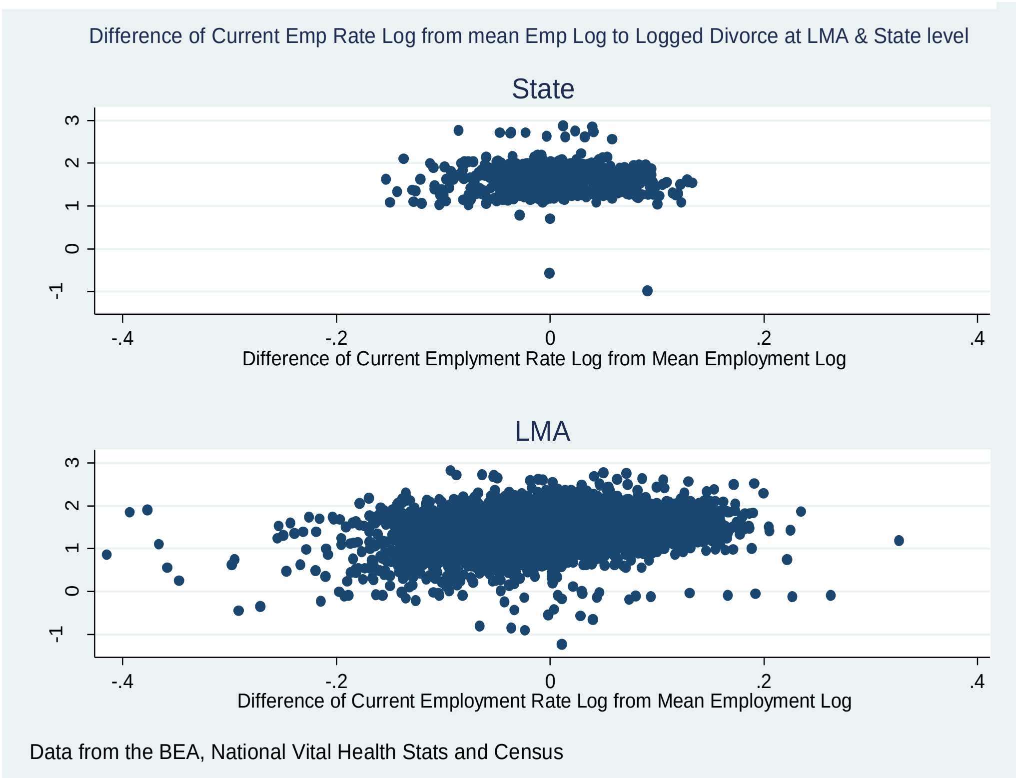
As can be seen in the Table 2.1, the standard deviation of employment is much larger in the LMA panel than it is in the state panel. Given the averaging of all counties to create state-level statistics, much of the across-state variation washes out. Figure 2.1 is a scatter plot of the difference in logs of the annual employment rate from the mean employment rate mapped to the logged crude divorce rate.[15] Clearly evident in the scatter plot is the increase in variation of both variables at the LMA level from the state level.

[15] Differences from the mean are used to show the variation in the data the regressions will be identified from. Given the FE model all identifying variation is from the mean.

TABLE 2.1 Descriptive Statistics at State and LMA Level					
STATE - Observations					
	Total	**North**	**Midwest**	**South**	**West**
Sample	48	9	12	16	11
Total Average Population	4,677,860 (4,745,664)	5,459,646 (511,456)	4,905,585 (3,750,197)	4,670,542 (3,277,161)	3,800,434 (6,687,537)
Average number of Counties	96.64 (56.07)	44.00 (24.78)	90.85 (15.45)	124.24 (67.10)	43.04 (14.18)
Divorces per 1000	5.51 (2.36)	4.21 (1.02)	4.24 (1.60)	5.63 (1.33)	7.77 (3.29)
Employment per 1000	501.57 (40.36)	508.78 (25.93)	512.93 (36.28)	479.26 (36.00)	515.74 (50.08)
Unemployment Rate	6.79 (1.56)	6.62 (1.08)	6.90 (2.43)	6.86 (1.31)	6.72 (1.16)
LMA - Observations					
	Total	**North**	**Midwest**	**South**	**West**
Sample	379	37	113	173	56
Total Average Population	570,900 (1,055.80)	1,266,080 (1,921.74)	513,570 (894.69)	409,350 (437.76)	726,350 (1,604.98)
Average number of counties	8.20 (4.81)	5.81 (2.76)	9.15 (5.47)	8.40 (4.67)	7.27 (4.25)
Divorces per 1000	5.54 (2.10)	3.70 (0.95)	4.82 (1.41)	5.97 (2.26)	6.88 (1.97)
Employment per 1000	468.05 (54.54)	462.35 (48.04)	479.98 (56.22)	457.72 (53.66)	479.67 (51.98)
Percentage of LMA that is Urban	0.45 (0.38)	0.65 (0.34)	0.40 (0.37)	0.44 (0.37)	0.43 (0.41)
Population Density	330.99 (940.40)	970.33 (2,578.75)	299.12 (591.22)	271.16 (440.14)	157.74 (298.12)

Standard deviations are presented in parenthesis. All data averages are taken in 1980.
Unemployment data is not available at the county or LMA level.

Figure 2.1
Difference in KeyVariation by Observation level



2.5 Methodology

$$(\text{Divorce})_{i,yr} = \propto + \beta_1(Labor\ Market\ Conditions_{i,yr-1}) + (X_{i,yr})\beta_2 + \delta_i + \phi_{yr} + (State * time)\gamma + \epsilon_{i,yr} \tag{2.1}$$

Equation 2.1 is similar to the existing work using state level data. The equation presents, a generic regression model that has a measure of divorce, as the dependent variable, and a measure of labor market condition, as the primary independent variable of interest. The observational subscript is "*i,yr*" where *i* will be either a state or LMA observation and *yr* is the year. In the regression, observation-level fixed-effects, dummy variables for the year, and a state-time trend are included to account for changing trends in divorce over time and across the regions of the sample. Lastly, vector *X* controls for time-varying state-level divorce laws.

To control for the state's own divorce laws a dummy variable indicates if the state has equitable distribution laws and the time since the passage of unilateral divorce law legislation. Consistent with the existing seminal- literature on the effects of unilateral divorce law, time varying dummy variables are used and created 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). For interstate LMAs the value of the dummy variables is between 0 and 1 and represents the population weighted portion of the LMA with unilateral divorce laws in place at the time.

2.6 Results

2.6.1 State-Level Findings

In Table 2.2 results for state-level regressions are presented. The top portion of Panel A attempts to mimic the work already produced on this topic by using the unemployment rate to measure labor market conditions. The regression in column 1 shows the coefficients estimated for the effect of last year's unemployment rate, when run in levels against the divorce rate[16]. The finding indicates that divorce is pro-cyclical, as the coefficient of interest is negatively related to divorce rates. In column 3, both the independent and the dependent variables are logged and while the effect of last year's unemployment remains negative, the estimate is not statistically significant.

[16] The current employment rate could also be used. However, due to a very high correlation between the current employment conditions and the lagged employment conditions only one measure should be used in the regression specification. The lagged employment condition was chosen to allow for the time of decision to separate to the actual divorce to be accounted for and to try to capture the majority of divorces that were triggered by the given economic conditions. The results are remarkably similar if the current employment rate is used, these robustness checks can be found in the appendix.

The bottom portion of panel A uses the same sample period as other researchers, with a lower limit at 1976 (when unemployment measures became available) but instead uses the employment rate, as the primary measure of labor market conditions. As can be seen, both the regression estimated in levels (in column 2) and the regression estimated in logs (column 4) is both statistically significant at a 1% level, and continue to support a pro-cyclical relationship. The finding in column 4 shows that for a one percent increase from the mean employment rate, the divorce rate is estimated to increase by 0.088%. In context of observation averages, this indicates that a 10% increase from the mean employment rate in a state will increase the average divorce rate of 5.5 per state by 10.78%, or 0.593 percentage points, to an average of 6.093. With a given population of approximately five- million people, that would be an additional 29,650 divorces.

Table 2.2

State-Level Regression Results	Panel A				Panel B	
	Sample Years 1976-1988				Sample Years 1971-1988	
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	y= divorce rate		y= ln(divorce rate)		y= divorce rate	y =ln(divorce rate)
Unemployment Rate yr-1	-0.080***					
	(0.022)					
ln(Unemployment Rate) yr-1			-0.042			
			(0.049)			
Employment Rate yr-1		0.011***			0.006**	
		(0.003)			(0.002)	
ln(Employment Rate) yr-1				1.078***		0.281
				(0.294)		(0.232)
	576	576	576	576	862	862

Robust standard errors in parentheses
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10
All regressions contain state FE, year dummies and linear state- time trends

In Panel B, the sample period is extended to include the additional years for which the employment rate is available, beginning in 1970. Due to the use of the *lagged* employment rate, the sample is estimated for the years of 1971-1988. Including the additional years causes a decrease in the magnitude of the coefficient and the statistical significance but the relationship between the lagged employment rate and divorce remains positive. The lowered magnitude of the coefficient in the sample starting in 1971, in contrast to the larger effects found in the limited sample, is consistent with those that showed the pro-cyclical pattern exhibited in aggregate divorce rates was strengthened in the data post 1980. (Amato & Beattie, 2011)

A log-log specification is the appropriate model to run, as it allows for the changes in the divorce rate and in the employment rate to be considered with respect to the LMA's long-run levels. Similar to the argument already made for logging the divorce rate, the value of the employment rate differs across LMAs. An increase in 50 jobs (out of 1000) is always a 5 percentage point increase, but it does not typically represent the same percent increase across LMAs. For example, if a given LMA has a current employment rate of 400 employees per 1,000, then an increase to 450 jobs is a 12.5% increase in their employment rate. However, if the LMA currently has an employment rate of 600 per 1,000 then an increase to 650 jobs only represents an 8.3% rise in their employment. Using the logged employment rate, as the primary independent variable, allows the model to account for the long-run employment participation level in each given LMA and estimates the effects in terms of in percentages instead of percentage points. Additionally, as both the independent and dependent variables are logged the coefficients of interest can be interpreted as the elasticity.

2.6.2 LMA level Findings

Primary analysis is conducted at the LMA level in the following tables. The change in observation level drastically increases the observation count, as well as the variation found in the data. Table 2.3 - Panel A, uses the sample years of 1976-1988. The labor market conditions measure is now only the lagged employment rate, as unemployment rates are not available at the county level. The most notable change from Table 2.2 to Table 2.3 is the increase in the magnitude and the precision with which the coefficients are estimated in both panels, or sample periods.

Table 2.3

LMA Level Regression Results	**Panel A** Sample Years 1977-1988		**Panel B** Sample Years 1971-1988	
	1 divorce rate	2 ln(divorce rate)	3 divorce rate	4 ln (divorce rate)
Employment Rate yr-1	0.001		0.001	
	(0.001)		(0.000)	
ln(Employment Rate) yr-1		0.214**		0.144**
		(0.075)		(0.050)
Observations	4,548	4,548	6,820	6,820
Rsquared	0.849	0.913	0.880	0.918

Robust standard errors in parentheses. Regressions are robust to Clustering SE at the state level, and can be found in the appendix
All regressions contain controls for state divorce laws, LMA FE, year dummies and linear state- time trends. *** p<0.001, ** p<0.01, * p<0.05, + p<0.10

Focusing on column 4, in Panel B - which uses the full sample years of 1971-1988, the magnitude of the coefficient on the logged lagged-employment is statistically significant at a 1% level and it indicates that a 1% increase in an LMA employment rate (above the mean) will increase the divorce rate by approximately 0.144%. In context, this indicates that a 10% increase from the mean employment rate in an LMA will increase the average divorce rate of 5.5

per LMA by 1.44% or to an average of 5.58 divorces per thousand. In an LMA with the average population of half a million people, that would be an increase of approximately 400 divorces.

The level regressions for both time periods are presented in the lower sections of panel A and B, for comparability to previous literature. The findings continue to indicate a pro-cyclical relationship between employment rates and divorce. However the results are lower in magnitude but are more precisely estimated using LMA level observations than the state-level observations.

2.6.3 Heterogeneous Treatment Effects by Urban Density

In the next section a new regression specification is used to examine if the pro-cyclical findings in Table 2.3 vary by the level of urban density in an LMA. The urbanicity variable is continuous, ranges from 0 to 1, and represents the percent of the population in an LMA that reside in a Standard Metropolitan Statistical Area, as measured in 1980. As discussed previously, urban areas have been found to have to higher base levels of divorce rates. LMAs are all-encompassing geographical measures and therefore provide the ability to test if these higher rates are also more volatile to economic shocks. The urban density variable is interacted with the lagged logged-employment-rate to allow for differential impacts of employment shocks on divorce rates depending on the urban density of the LMA. The variable *urban*, will not be identified in the regression, as it is a time-constant measure taken in 1980, therefore, it will be absorbed into the time in-varying LMA fixed-effects.

$$\text{Ln(Divorce rate)}_{lma,yr} = \propto + \beta_1(\ln(employment_{rate_{lma,yr}})) + \beta_2(Ln(employment\_rate_{lma,yr})) * (Urban\ Density_{lma,yr}) + (X_{i,yr})\beta_2 + \delta_i + \phi_{yr} + (State * time)\gamma + \epsilon_{i,yr} \tag{2.2}$$

In column 1, of Table 2.4, the findings from Table 2.3 (column 2) are replicated for reference of the original, non-interaction specification. Results for equation 2.2 are presented in column 2. The primary variable, the lagged logged-employment rate, is now identified strictly off of areas with no urban counties. The coefficient remains positive and highly statistically significant. However, the coefficient on the urban interaction variable is negative and indicates that in more urban areas there is a lesser impact of the business cycle on the marital stability of local households. The finding indicates that the divorce rates of urban residents are less volatile to changes in labor market conditions than those of rural citizens.

Table 2.4
Heterogeneous treatment by Urbancity

	Sample years 1976-1988			**Sample Years 1971-1988**		
	1	**2**	**3**	**4**	**5**	**6**
Variables	ln(div rate)	ln(div rate)	ln(div rate)	ln(div rate)	ln(div rate)	ln(div rate)
ln(lagged employment rate)	0.214**	0.689***	22.232	0.144**	0.698***	20.784*
	(0.075)	(0.101)	(13.779)	(0.050)	(0.067)	(8.334)
(% Urban) * Ln(lagged employment rate)		-0.794***	-0.530**		-0.954***	-0.551***
		(0.120)	(0.200)		(0.083)	(0.12)
Interactions of Age Demographics with Ln(lagged employment rate)	.	.	X	.	.	X
R squared	0.913	0.914	0.914	0.918	0.920	0.921
Observations	4,548	4,548	4,548	6,820	6,820	6,820

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

A key concern to the validity of this finding is that urban areas have a disproportionate amount of younger individuals, and therefore the two populations likely have different demographic characteristics. As people migrate to urban centers for work opportunities, urban areas tend to have younger populations than rural areas. If the population is generally younger in more urban areas, then the average marital tenure will be lower. As the length of a marriage is known to affect the marriage's resilience to labor-market fluctuations, the age-dynamics and consequently the marital life-cycle of the population should be considered (Arkes and Shen, 2014). Therefore, data to control for the age demographics at the county level are incorporated in column 3. The data for the demographic interactions was obtained from the 1980 decennial census and is the percent of the total population in 5 year age brackets, (0-5 years old, 5-9, 10-14,…… 80-84, 85 plus years old) with the omitted category for the regressions being those aged 0 to 10 years old.

By interacting measures of the LMA's age distribution with the lagged, logged employment rate the effects of changes in the employment rate are allowed to vary heterogeneously amongst age ranges. Therefore, if an effect is still found in urban areas, it is identified off of the *urban* variable alone, and not driven by the different age characteristics observed n urban areas. As shown in columns 3 and 6, the estimate on the urban interaction variable is remarkably stable to the inclusion of the age-group interaction variables.

The robustness of the heterogeneous effects observed in urban areas indicate that couples who reside in rural areas have marriages that are more prone to be affected by the business cycle than those in urban areas. Further tests are warranted to determine exactly what is driving this effect. One possible explanation could be that households in rural areas are more constrained by the real estate market. The rental market for housing tends to be smaller in more rural areas, and this inability to locate a new, affordable home could make rural residents more prone to delay divorce during financially hard times and therefore more influenced by employment and earnings than those in urban areas.

2.7 Conclusion

Large secular and legal changes over the last few decades have had an impact on shaping divorce decisions. However, how the local economy effects divorce is still an important element in understanding the changing dynamics of marital stability. By using a finer unit of observational analysis I am able to determine how local labor-market changes affect local divorce rates more accurately than previous researchers. Additionally, by using a logged specification, the strict assumption that macro-economic shocks will affect the married populations of all states in equal magnitude is relaxed. Using this specification and employment rates at the LMA level, a strong, robust, and positive relationship between employment rates and divorce rates is found. Lastly, the positive effects of employment rates on divorce are found to be larger in rural areas than in urban areas. This finding indicates that much of the pro-cyclical pattern exhibited by divorce rates reported at the state level may be primarily drive by the rural residents of states. These findings support that the destabilizing role employment shocks have at the individual household level appear to be dominated by risk aversion and credit constraints at the aggregate level.

Chapter 3

The Effects of Permanent Declines in Low-Skilled Male Earning Potential on Divorce Rates

3.1 Introduction

In his seminal work, *Treatise on the Family* (1981), Becker asserted that individuals can achieve higher incomes and utility by specializing and trading within a marriage than they can as single individuals. The key requirement for this assertion to be true is that individuals have different skill sets and/or a different return to a given skill set and can therefore take advantage of comparative advantage within a marriage. Assuming men have no advantage in home production, the more men earn relative to women, the more extensive the economic gains for both spouses would be.

However, in the last few decades, many have speculated that these proposed economic benefits of marriage may not extend to low-income less-educated women. In a 2004 editorial, in the *New York Times* it was argued:

> "It is undeniably true that women tend to become poorer after divorce and that children from single family homes are more likely to grow up in poverty, but the fiscal lift that occurs when middle-class couples marry and combine resources does not come about in neighborhoods where jobs have long since disappeared and men in particular tend to be unskilled and poorly educated."

This quote conveys the perspective that low-income women and men are themselves each too limited in their earning capacities to contribute significantly to the family's resources. An idea at the core of this argument is that the decline in the earning capacity of low-skilled men, and consequently reduced gains from comparative advantage within marriage, is ultimately to blame for the rise in out-of-wedlock children, welfare participation, and declining marriage rates (Edin 2000, Wilson 1996).

Despite the significance that structural labor market changes may have had on the household dynamics of low income families the majority of researchers in family economics have focused on the effects of general macroeconomic conditions on household stability.

Consequently, the majority of research has used variation derived from business-cycle fluctuations, which represent mainly transitory changes to earnings levels for all. Far fewer researchers have analyzed the effects of non-transitory or structural changes in employment opportunities and earning capabilities on household structure. Additionally, little research has looked specifically at the effect of job-loss and declining earnings on the stability of low-skilled couples' marriages.

Of the studies that have focused on low-skilled couples, more specifically, the bulk of the research has focused on changes in welfare, or AFDC, uptake, child outcomes, and changes to marriage rates as a result of changing earnings. This research focus is in-line with the social and political concerns for low-income families regarding poverty management, outcomes of children in single-parent homes, and welfare uptake. In this paper I also focus on low income households, but examine how structural changes to the labor market and the permanent loss of high-earning jobs for low-skilled men affects divorce decisions of low-skilled couples. Specifically, I ask what effect the decline in earnings derived from steel and coal jobs had on the local divorce rates where these industries were concentrated.

3.2 Previous Work

3.2.1 Divorce and the Economy

The bulk of studies dealing with marriage markets and macroeconomic conditions use wide-ranging economic indicators, such as unemployment, that measure job opportunities for all workers. The literature on business-cycles and family structure supports that divorce has primarily been pro-cyclical, at the aggregate level, over the last 30 years (Hellerstein and Morrill (2011), Roy (2011), Amato and Bettie (2011)). This empirical work is identified off of both permanent and transitory changes in the labor-market conditions, given the general indicators used as key explanatory variables. However, temporal and structural changes in labor market demand may potentially have very different effects on family structures, and the existing literature makes few distinctions between the types of labor-market demand changes.

The only study that looks at the effects of unexpected and sometimes permanent shifts to earnings-potential on divorce is Charles and Stephens (2004). Using the Panel Study of Income Dynamics (PSID), Charles and Stephens investigate the effects of layoffs, plant closings and disability on divorce. Their findings indicate that layoffs affect divorce rates, but that plant closings and disability do not affect them[17]. This paper speculates that a plant closing is less of a negative signal to a spouse regarding future earning-potential than a layoff because a layoff can indicate a level of fault and therefore signal possible future labor-market struggles. Although the authors explanation is plausible in most situations their research did not identify how many plants had closed in the local economy and possible general effects that mass terminations could have on long-term earning potential.

This paper attempts to fill some of this void in the literature, by examining industries that underwent large structural changes and by focusing on communities where plant closings were abundant. I assert that plant closings can also provide signals of limited future earning-potential if those plant closings are abundant in a region and represent some of the only high paying jobs available for the low skilled. Charles and Stephens (2004) provide insight into how signals of labor market struggles within a household, for a given partner, can affect a marriage. This paper extends that work to analyze how labor market struggles and declines in future expected income for an entire community affect marriages within that community. To identify labor market

[17]This study acknowledges that older marriages are more stable and the onset of disability tends to be correlated with own age and length of marriage which could convolute some of these findings.

struggles this study uses exogenous variation identified by long-term and wide-spread structural changes in the labor market to estimate the percentage change in local earnings.

3.2.2 Low-Skilled Workers and Divorce

Studies that look at how general macroeconomic conditions affect divorce have found different effects at the household level, depending on the characteristics of the household. The effects of business-cycle fluctuations on divorce rates vary by couples' education levels and by the duration of marriage (Hellerstein, 2011; Arkes and Shen, 2014). Marriages where the wife is college educated are far less sensitive to business-cycle fluctuations than marriages where the wife has no college education (Hellerstein (2011)). These findings indicate and provide additional support that low-skilled couples may derive different benefits and experience different costs from marriage than high-skilled couples.

Focusing specifically on individuals with low education levels, Wilson (1996) asserts that the job opportunities for low-skilled men have declined on a structural level. He argues that it is the literal "disappearance of work", not short-term movements in employment or wages, that has caused much of the family dissolution and welfare uptake seen in the end of the 20th century (Wilson, 1996 ,p567). Black, McKinnish and Sanders (2003) test Wilson's hypothesis regarding the relationship between the disappearance of work and the increase in welfare uptake by using variation from the industries of coal and steel to analyze welfare (AFDC) uptake at the county level.

The coal and steel industries experienced large economic shocks that generated long-term changes in demand for low-skilled workers. Black, McKinnish and Sanders (2003) find that the coal and steel booms had large negative impacts on AFDC uptake. They attribute the majority of the decline in welfare participants to the increased fraction of women who were married, or no longer members of female-headed households. However, Black, McKinnish and Sanders did not focus on marriage, as their primary subject, and the authors did not analyze the divorce rates following the demise of these industries.

3.2.3 Structural Labor Market Changes and Divorce

Attempting to predict if a permanent loss in male earning-potential would lead to an increase or a decrease in the divorce rates of uneducated couples is challenging. As already discussed, the literature that focuses on macroeconomic conditions and marriage has found that divorce is pro-cyclical to the business-cycle, indicating that recessions or declining employment puts downward pressure on divorce(Amato and Bettie, 2011; Hellerstein and Morrill, 2008; Hellerstein and Morrill, 2011; Arkes and Shen, 2014). Additionally, lower-skilled couples tend to be more sensitive and have divorce-rates that are disproportionally pro-cyclical to the business-cycle in comparison to the rates of those with higher levels of human capital (Hellerstein and Morill, 2011; Arkes, and Shen, 2014). This established predisposition should apply downward pressure on divorce, as the local employment rate falls and industries shut down.

However, the hope that the economy would rebound and credit constraints would decline are also lowered in the communities studied in this paper, as the factories and mines closed down for good. If recessions do not induce divorce, but instead simply affect the timing of divorce by inspiring partners to delay a divorce until they are less credit constrained, it is possible this effect would be diminished in the situation where there is minimal hope of future credit increases. Therefore, it is not clear that couples would respond to this permanent change in job and earnings opportunities the same as transitory business-cycle fluctuations, which were used to identify the aforementioned trend in the literature.

In the marriage market and business-cycle literature, a key explanation for the effects of the business-cycle on divorce rates is that individual's re-asses the net-present value of their post-shock marriage in comparison to their outside options (Hellerstein and Morrill, 2011). However, in the scenario of industry destruction there is a general-equilibrium effect on a majority of low-skilled marriage-market participants. In these situations, not only has the job destruction affected the assessment of the value of a given marriage, but also that of possible alternatives available outside of marriage. Assuming a low-skilled woman will typically match with a low-skilled man, and the market for high-paid low-skilled male jobs has disappeared, this disappearance of work potentially affects the marital net-benefit of any other male that a woman would consider a spousal alternative. This decline in viable spousal alternatives, as the industries decline, could ultimately put additional downward pressure on divorce.

An additional issue, which is unique to low-skilled couples, is the US welfare structure and its impact on marriage in the low-skilled community. Although not considered a viable "spousal alternative" to a husband by many, when male unemployment is high, the possibility of receiving government aid could put upward pressure on divorce rates for the low skilled. The US welfare program, known as AFDC benefits during the time of my study of 1970-1988, strongly limited benefits to non-single headed households.[18] Therefore, these programs increase the opportunity cost of marriage, and consequently decrease the returns to marriage, among low-income families. Under a combined income many couples may be ineligible for government AFDC benefits but if unmarried (or unmarried and cohabitating) the woman could receive benefits (Edin, 2000).[19] Therefore, a reasonable spousal alternative could be divorcing,

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[18] AFDC, or Aid to Families with Dependent Children, became TANF, Temporary Aid for Needy Families, in July 1997.

[19] In early 1970s the Supreme court struck down the man-in-the-house rule (which prohibited female welfare recipients from cohabitating with a man)

cohabitating, or staying single and using government aid to mitigate the loss of the second income. Consequently, these policies may increase the divorce rate when opportunities for male employment are low.

3.2.4 The Structure of Coal, Steel and Manufacturing Industries

Positions in steel production and coal mining are typically male dominated. These positions provided men with high-earning jobs, despite their low-skill set, that their wives could not reasonably attain. In regions heavy in coal and steel production, these jobs represent some of the best-paid positions for those with high-school level educational-attainment. Black, McKinnish and Sanders (2003) found that in regions with a heavy steel-concentration, the employees in the steel industry were 31% more likely to be male, earned almost 53% more and had on average 15% less years of education than their counterparts in non-steel industries. Whereas, in regions of heavy coal-concentration, the employees in the coal industry were almost 35% more likely to be male, earned almost 60% more and had on average 17% less years of education than their counterparts in local non-coal industries[20].

3.3 Data

Using the similar, quasi-natural experiment variation as Black, McKinnish and Sanders (2003), an empirical analysis is presented in this paper to determine how the decline in steel production and coal mining affected divorce rates in regions where these industries were highly concentrated. Analysis is limited to the ten states that employment in steel and coal industries was high in the 1970s: Illinois, Ohio, Michigan, Pennsylvania, Alabama, California, Indiana,

[20] See Black, D. A., McKinnish, T. G., & Sanders, S. G. (2003), Table 1, for more specifics on the authors calculations. Authors calculations used the 1970 Public Use Microdata Sample from the US Census. Steel worker was defined as those working in the Blast Furnace, Steel work, Rolling and Finishing Mills, or in Other Primary Iron and Steel Industries.

New York, Kentucky and West Virginia. Although the states in this sample may have some overlap between industries, many states have a high concentration of only one industry. Table 3.1 displays the industries of interest in the above 10 state sample, as well as the number of counties in each sample.

Table 3.1 - Sample States

State Name	**Steel Production**	**Coal Mining**
Alabama	X	
California	X	
Kentucky		X
Illinois	X	
Indiana	X	
Michigan	X	
Minnesota		
Missouri		
New York	X	
Ohio	X	X
Pennsylvania	X	X
West Virginia		X
Total Number of States	8	a
Total Number of Counties	610	242
Sources for Identifying key states of production in coal and steel were attained from Black, McKinnish and Sanders, 2001		

Measures of the earnings in the steel and coal industries are constructed using an amalgamation of data from the Regional Economic Information Systems reported by the Bureau of Economic Analysis.[21] National-level annual earnings by industry and the 1970s PUMS data on steel-worker concentration are used, as instrumental variables to predict county level steel earnings. Geological information on coal reserves and the global price for coal are used to

[21] This data was used in Black, D. A., McKinnish, T. G., & Sanders, S. G. (2003) and was generously provided by Terra McKinnish for the purposes of this paper.

construct county-level coal earnings estimates. For more detail on these data sources See footnotes 8, 9 and 10 in the following section.

A newly-digitized dataset of all divorces and annulments granted in each county in the sample states, from the years 1965 to 1988, is added to the industry measurements. 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.[22] These data are merged with intercensal county-population from the US Census to create the crude divorce rate, or the number of divorces per 1,000 people.[23]

Figure 3.1, shows the decline in the fraction of the national earnings that could be attributed to steel, as reported in the Bureau of Economic Analysis's Earnings-by-Industry Reports. As can be seen in the top figure, the demand for steel has steadily declined since the mid-1970s. From 1980 to 1987 the fraction of national earnings attributed to steel fell by almost 50%.

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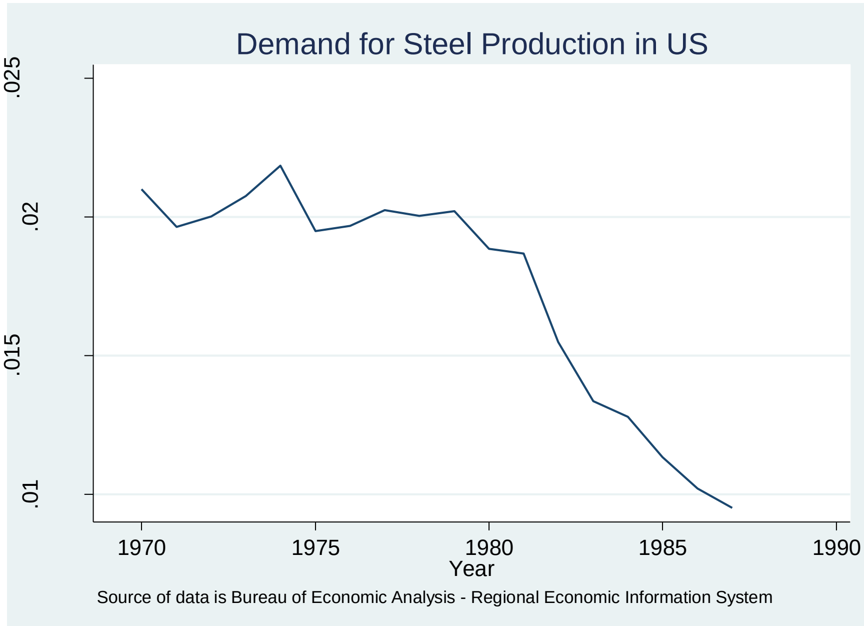
[22] Data obtained from: http://www.cdc.gov/nchs/products/vsus.htm. Note, that no divorce data at the county level is available post 1988 as the federal government stopped collecting data in this year.

[23] 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).

Figure 3.1



For additional information on data sources see data section and footnotes 24 & 25.

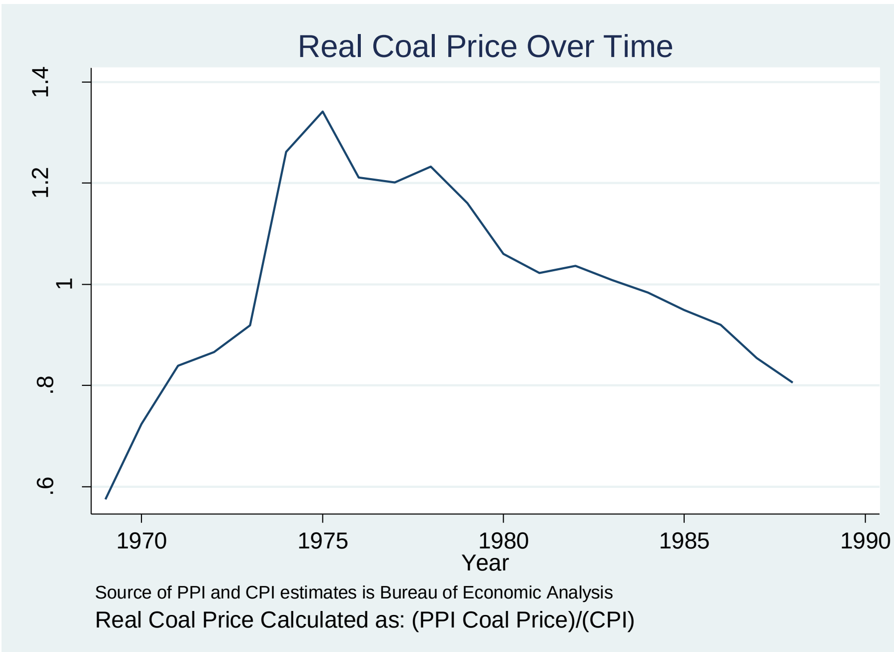
The market for coal mining varies from steel production, as during the same two-decade long decline of steel both the boom and bust of coal is observed. The price of coal was rather stable through the 1950s and 1960s, with less than a one-percent average decline in price per year from 1950-1969. However, the OPEC oil embargo in the early 1970s caused an increase in demand for coal and a 44% increase in the real price of coal from 1973 to 1974[24]. As can be seen in Figure 3.2, the real price of coal peaked, at an all time high, in the early 1970s. The increased demand for coal resulted in an increased demand for miners.

Unfortunately for the miners, the boom lasted for less than 10 years. The price of coal steadily dropped throughout the 1980s, as the price of oil fell. Additionally, new mines opened

[24] From Authors calculations of changes in real coal price – evaluated as : (Coal PPI) /(CPI)

in the Northern Rocky Mountain region during the 1980s, adjusting the national supply for coal, and which used new technology and capital advancements to make mining less labor intensive[25]. The price of coal has never rebounded and the demand for miners has remained consistently low through the 1990s and into 21st century. The structure of the coal mining labor market varied through the period of my analysis, but the bust of coal can be seen as essentially permanent.

Figure 3.2



For additional information on data sources see data section and footnotes 24 & 25.

Table 2 reports descriptive statistics of earnings and coal price from three years in the sample: 1972, 1980 and 1987. Panel A presents averages on the county level industry measures and shows the average percent of total county employment that is in the steel industry, as measured in 1969. The average percent of employment in the steel industry per country was 3.3% with a maximum of 38%, and the average population in the steel counties was approximately 150,782 people in 1980.

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[25] For more information on data sources and coal mining see Black, D. A., McKinnish, T. G., & Sanders, S. G. (2003) and Merrell, D.R. (1999).

The right hand columns present data on the coal sample and measures the average coal reserve per county in 1969, at approximately half a million tons per county. Coal reserves varied greatly, with a maximum coal reserve of 8,426 tons, and a minimum of zero tons. The county's in the coal region of the southern United States tend to be less populous than steel region county's with an average population of 85,912 people in 19080.

Panel B presents demand level measures taken at the national level. Both the levels of, and changes to the share of national earnings in steel are shown, as well as the real coal price, from 1972 to 1987. Steel suffered the most job-loss with over 50% decline during the 15 year period. Coal appears less volatile, but the years in which the minimum of the real coal price (1965) was observed and the maximum (1975) are not used of the key years of comparison. Coal shows a 22% increase in the real coal price from 1972 to 1980 and an almost complete reversal with a 20% decline in price from 1980 to the 1987 observed real price.

Lastly, panel B presents averages of the crude divorce rate in the counties included in each sample. As can be seen in the Table 3.2 , the number of divorces per 1,000 in the steel states in higher at a rate of 5 divorces per 1,000 in the steel region in 1980 and an average of only 4 divorces per 1,000 in the coal sample. Divorce rose in both regions from 1972 to 1980, but fell in the steel counties from 1980 to 1987, while it rose slightly during this period in the coal counties.

Table 3.2 - Descriptive Statistics Over Time

Panel A -- Measurement of Variable Of Interest			
Percent Of County Employees Employed in Steel Industry	3.30% (4.90)	**Average Coal Reserve in Thousand**	560.633 (1,260.736)

	Steel Sample		**Coal Sample**	
Panel B	Levels	**Percent Change**	Levels	**Percent Change**
Demand Measure in 1972	0.020		0.866	
Demand Measure in 1980	0.019		1.061	
		-6.00%		+22%
Demand Measure in 1987	0.010		0.855	
		-49.95%		-19.4%
Panel C	Levels	**Percent Change**	Levels	**Percent Change**
Divorce Rate in 1972	3.69		2.81	
	(1.54)		(1.13)	
Divorce Rate in 1980	5.04		4.01	
	(1.90)		(1.62)	
		+ 3.66%		+4.30%
Divorce Rate in 1987	4.62		4.32	
	(1.67)		(1.56)	
		-0.83%		+0.77%

Demand measures Are those shown in Coal and Steel figures above. The measure of Steel Demand in Steel States is the percentage of national earnings attributed to steel earnings. The measure of demand of Coal is the real coal price. Percentage changes are taken from the previous period listed.

3.4 Methodology

An instrumental variable technique is used to estimate the effects of changes in local earnings on changes in local divorce rates, using the national-level demand changes for coal and steel, as the excluded instruments. Although one could regress county level divorce rates on county level earnings, as many have done, this variation is identified off of transitory fluctuations and structural changes in labor market conditions. In order to estimate non-transitory exogenous shocks to county level earnings a measure of the national-level demand for the industry's production is interacted with a measure of the industry concentration at the county level. The national earnings or national estimate of demand, reported by the BEA capture large factors that influence the demand for steel, such as: foreign trade, competition, and changes in production technology. Using variation from changes in national level earnings and prices, interacted with county level measures, provides an explanatory variable that is identified off of longer-term variation in the opportunities for low-skilled men.

The IV methodology requires estimating a two-stage model that is presented, in its generic form, in equations 3.1 and 3.2 below. Equation 3.1 reports the second stage equation in which the outcome of interest, *Y*, is the log of divorce rates and *E* is the estimated log of real earnings in the county. The first stage, shown in equation 3.2, regresses the log of real-earnings per- county on the instrumental variable *Z* of interest, which are summarized below in variables Z_s *and* Z_c, for steel and coal states respectfully.

$$Y_{c,y} = \theta_0 + \theta_1(E_{c,y}) + \sigma_c + \sigma_{yr} + (State * Year)\varphi + \varepsilon_{c,y} \tag{3.1}$$

And

$$E_{c,y} = \beta_0 + \beta_1(Z_{c,y}) + \delta_c + \delta_{yr} + (State * Year)\phi + \upsilon_{c,y} \tag{3.2}$$

<u>Instrumental Variables</u>

Steel region Instrumental Variable = Z_s === (Fraction of Men employed in Primary metals in the county$_{1969}$) * (Fraction of Total US earnings attributed to Steel)

Coal region Instrumental Variable = Z_c === (ln(Coal Reserve in county in $_{1969}$)) *(ln(real Coal Price $_t$))

The estimated county-level earnings in steel states is created by interacting the current share of national earnings attributed to steel with a static measure of county-level industry concentration taken in 1969.[26] Industry concentration, or capacity, is measured by the percent of all county employees that are employed in steel in 1969.

The instrumental variable for coal is the log of a given county's coal reserve interacted with the log of the current real price of coal. Coal reserves are the tonnage of coal still available to be mined in the county.[27] This variable is intended to capture the capacity, or potential for production, at the county level. The opportunity cost to a county, or firm, of not using the reserves in their land varies along with the price of coal, drastically so, during the coal boom. Additionally, there was tremendous variation in the amount of coal reserves found across counties in the four-state sample. Half of the counties have close to no coal, approximately 20 percent had up to a million tons in reserves and approximately 18% of counties had over one million tons of reserves. The interaction between the reserve and the current real-price of coal captures the potential value to mining in a given county[28].

In addition to the instrumental variables of interest, both regressions contain controls for year dummies, state-specific linear trends and county-level fixed effects. The individual county fixed–effects demean the data of any time invariant elements that determine divorce rates across counties and states. The year dummies demean the data of any annual variation in the divorce rate, which is affected by trends at a national level, such as business-cycle or federal law changes. Lastly, the State-Year controls demean the data for variation caused by state-year specific conditions, such as changes in divorce laws, welfare generosity or state-time specific business -cycle fluctuations.

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[26] Data on county-level steel concentration was generously provided by McKinnish. For further details on thedata source, the 4th County Population File C from 1970 census, see : Black, D. A., McKinnish, T. G., & Sanders, S. G. (2003).

[27] Coal reserve data was generously provided by Terra McKinish. Please see Black, D. A., McKinnish, T. G., & Sanders, S. G. (2003) for more details on coal reserve data.

[28] The real price of coal is measured on an annual basis as : (The PPI value of Coal)/(CPI) PPI of coal was found from ftp://ftp.bls.gov/pub/time.series/wp/wp.data.6.Fuels where coal category is : WPUO51 ,

3.5 Results

3.5.1 First Stage Regression Results

The results from the first stage regression, equation 2, that regresses the logged real income on the instruments, show that both instruments are positively related to real earnings and are highly significant. The instruments are strongly predictive of real income in the sample counties, and estimate the logged real earnings at the county level with a high degree of precision. Depending on if the lagged instruments are included, the instrument's F scores range from approximately 100 to over 200. Note the scale of the coefficients varies greatly across samples. This is due to the different values of the Z variables. The steel interaction is bound between (0, 0.005) while the coal interaction variable is bound between (-4.0 , +2.0).

Table 3.3
First Stage Results

	Coal Sample		**Steel Sample**	
	1	2	3	4
Variables	ln(Real Earnings)	ln(Real Earnings)	ln(Real Earnings)	ln(Real Earnings)
ln(Coal Reserve)*ln(Coal Price)	0.044***	0.036***		
	(0.003)	(0.007)		
lagged Coal Instrument		0.013*		
		(0.006)		
(Percent employed in steel in 1969 in county)				
* (Current Percent of all US earnings from			83.045***	34.204+
			(5.845)	(17.611)
Lagged Steel Instrument				60.852**
				(20.587)
Observations	6,365	6,174	10,744	10,059
R-squared	0.993	0.994	0.995	0.995
F Tests for Excluded instruments in First stage	F(1, 5960) = 172.08	F(2,5768) = 96.23	F(1,9997) = 201.84	F(2,9319) = 119.68

Standard errors in parentheses, *** p<0.001, ** p<0.01, * p<0.05, + p<0.10
All regressions also contain year dummies, county fixed-effects and linear state-time trends

3.5.2 Two- Stage-Least Squares Regression Results

The relationship between earnings and divorce rates in steel regions, presented in the right hand panel, is strongly positive and significant at a 1% level. Regression specification 5, estimates that for a given 1% increase in steel earnings the divorce rate will increase by 0.56%. Therefore given the average divorce rate in steel counties of approximately 5 divorces per 1,000 in 1980 a 10% decrease in earnings will decrease the divorce rate by 5.6%, or the equivalent of 0.28 percentage points, to an average of 4.72 divorces per thousand. Given the average population of 305,000 per county in the sample for steel states a decrease in divorce rates from 5 to 4.72 divorces per thousand will decrease the total number of divorces by approximately 85 couples.

The left hand variable of interest is the number of divorces per thousand, or the crude divorce rate, therefore the regression is implicitly controlling for changes in population. However, if people, who migrated out of the region, as plants began to close, were not representative of the general community, the implicit control may bias the results. For example, if younger, single individuals were more prone to move out of the area than those who were married, the divorce rate per-capita may increase even if the rate of divorce per the remaining married population did not change. Therefore as a robustness check to the per-capita specification, regressions 5 and 6 transform the left hand variable to the log of the raw number of divorces (or divorce levels) and the population is directly controlled for on the right hand side of the equation with the log of the current county population level. The data is obtained from the census Bureau's Population Estimates Program (PEP) and is not a linear extrapolation of decennial census. Therefore, the data is highly detailed and should provide enough frequency to identify net migration patterns during this approximate 20 year sample.

Table 3.4

Two-Stage-Least-Squares Results								
	Coal Sample				**Steel Sample**			
	1	2	3	4	5	6	7	8
VARIABLES	Y = Log of Divorces Per Population		Y = Log of Raw Divorce Numbers		Y = Log of Divorces Per Population		Y = Log of Raw Divorce Numbers	
ln(Estimated Real Earnings)	-0.156	-0.157	-0.228	-0.205	0.558***	0.519***	0.879***	0.779**
	(0.194)	(0.186)	(0.251)	(0.220)	(0.135)	(0.135)	(0.242)	(0.237)
ln(Population)			1.427***	1.396***			0.153	0.261
			(0.358)	(0.313)			(0.272)	(0.266)
Lagged Instument Included	.	X	.	X	.	X	.	X
Observations	6,174	6,174	6,174	6,174	10,059	10,059	10,059	10,059

Robust standard errors in parentheses, *** p<0.001, ** p<0.01, * p<0.05, + p<0.10
All regressions also contain year dummies, county fixed effects and linear state-time trend

Including population, as an explicit control, does not appear to affect the significance or magnitude of the coefficient on earnings in the steel sample. Additionally, a non-significant effect of population changes on divorce levels are observed; which is consistent with relatively minimal net-migration in the rust belt during the time. Interpreting the estimate in regression 6, which controls for population, as a right hand variable, as well as the contemporaneous and lagged steel instrumental variables, a 10% decrease in real earnings, while holding population constant, will decrease the level of divorce by 7.8%.

Inversely, a statistically insignificant relationship between earnings and divorce is found in coal regions. The instrumental variable regressions in columns 1-4, show the opposite findings of those found in steel regions. Counties that experienced the coal boom and bust in the Appalachian Mountains were historically depressed economies. Relative to the steel counties, which historically had higher wages, coal regions tended to have lower production and earnings. Additionally, as was seen in Table 2, the coal regions divorce rates were consistently below that of steel regions, regardless of coal prices. Therefore it is not entirely surprising that the impacts from shocks to earnings may be quite different.

As with any instrumental variable technique, the instruments identify a Local Average Treatment Effect (LATE). Because the instruments only use a portion of the variation in real earnings the identification is specific to the region, time and the individuals most affected by the shock. If regions or households respond differently to shocks to earnings, whether temporary or permanent, the estimated effects of these shocks would also vary. Because the coal region was quite different from the steel region, even before the industry decline, the two regions appear to have responded differently to similar shocks in expected earning potential.

Similar to concerns discussed previously, migrant workers may also undermine the precision of the estimated effects. In the coal sample, migration in and out of the region is more of a concern than in the steel states, as many of the mining towns were smaller and flourished and floundered with the boom and bust of coal. It is also possible that many of the workers migrating to the mining town did not relocate their entire families. Instead, the wife may have maintained property and citizenship in a different county and/or state while the husband relocated for work. Additionally, if entire families relocated to the mining town it is possible the family also left, as a unit, after the job opportunities dried up. Therefore, even if a divorce did occur, it may be recorded in either the family county of residence (if the worker was a temporary migrant) or in the new county, post a family relocating from a mining town.

Because population of the counties in the coal region was inversely related to the price of coal, and therefore coal earnings, a separate regression is again specified. This specification, shown in columns 3 and 4, controls for population directly and regresses the logged level of divorces (instead of divorce per capita rates) on the instrumented earnings estimates, while controlling for population on the right-hand side.

If migrants did not have a randomly distributed marital status, but instead were biased towards being single or having a high rate of marriage, but not at risk for being divorced in the county, they would distort the crude divorce rate. Their migration to the county would artificially drive the divorce rate per-capita down even when the risk of divorce for the at-risk population, married citizens in the county who were eligible for divorce in the county, did not change. As can be seen in columns 3 and 4, migration or emigration, strongly affect the local divorce levels in the coal-heavy states, which indicates there was migration to the region and consequently more divorces. However, it did not affect the relationship between earnings and divorce. Controlling for changes in county population does not affect the estimated effect of earnings on divorce, which in still insignificant and inversely related.

3.6 Discussion

In this paper I specifically sought a source of variation that was not temporary, as structural changes have the potential to affect couples differently then business-cycle fluctuations to earnings, the primary variable studied in the literature to date. Two labor markets that underwent major structural changes in the last 50 years were coal and steel. Although the regions and population where these industries were concentrated varied greatly from each other, the permanence and intensity of the negative shocks to earnings of the low-skilled men in these industries was very similar.

The strong and positive effects found in the regions of steel production support that divorce was suppressed in the communities that experienced the greatest decline in earning potential. The findings also support that community macroeconomic conditions, and general equilibrium effects on marital alternatives, may further suppress divorce rates in the presence of permanent shocks versus temporary shocks.

Statistically insignificant, and negative, results are observed in the coal region. This finding highlights that the treatment, a decline in earnings, may be experienced differently by these different regions and individuals. Coal regions do not appear to have experience a suppression of divorce during the bust of the coal mining, the way steel counties did. Additional work in the future will attempt to analyze additional base level differences in the two regions and explore variation in the demographics. Future research will question if there exists a primary demographic difference between the two regions, which may help explain the different county level responses to the losses of work.

3.7 Conclusion

The findings in the steel sample provide interesting insight into the effects of the “disappearance of work” on the marital survival of thousands low-skilled couples in that region. A positive relationship between divorce and earnings is in line with the existing findings on the relationship between business-cycle employment fluctuations and divorce. The high responsiveness to changes in earnings in the steel subsample, which is identified off of changes in the earnings of low-skilled couples, is supported by the existing work that shows low-skilled couples’ marriage decisions are influenced more heavily by job opportunities and the economy than those of the college educated.

Additionally, the findings shed light on the theory of how individuals assess the expected value of a marriage and the expected value of their outside alternatives. The effects found in this paper show that for any given percent-shock to the national-level demand for a steel, the more concentrated the industry is in the county the more disproportionally that shock will be felt. Therefore, the findings support that individuals assess the value of their current relationship relative to the value of the alternatives available outside their marriage, particularly during times of economic turmoil. In the case of low-skilled couples, as work disappears for the current husband, and the majority of other extramarital options available to a low-skilled woman, divorce rates are driven further down in the county. Therefore, the findings demonstrate that as the expected value of marital-alternatives decline, the pressure to stay in the current relationship rose for those in the rust belt. However, similar earnings shocks had no statistical effect in the coal mining region of the Appalachian Mountains.

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Appendix

Figure A.1.1- Creation of CNGs in a state with four sides.

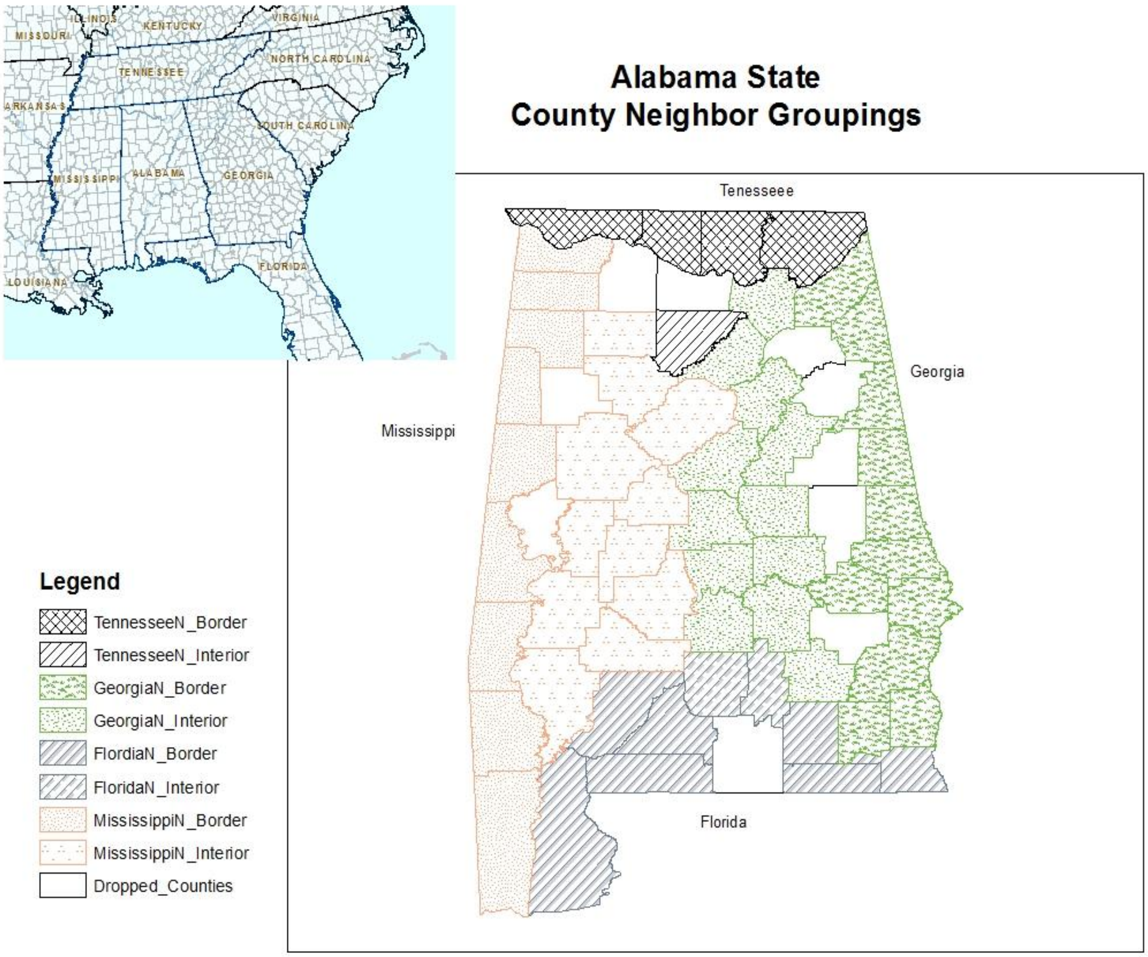

Figure A.1.2 - Test for Variation in Data

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

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 A.1.2 – Variation in Data

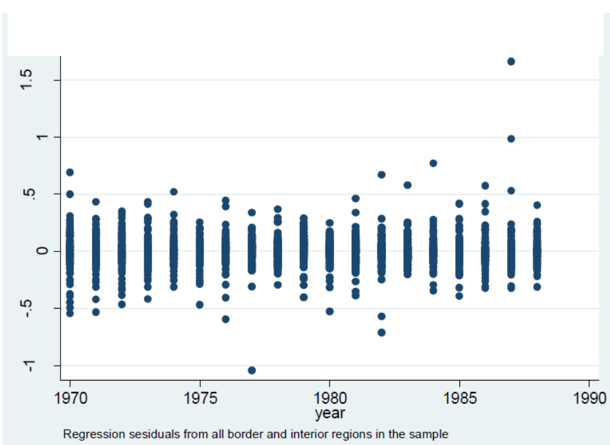

Table A.1.1 – List of Missing Observations

Certain county observations may be missing from the data set due to missing historical data, and outliers, which indicated historical accident in the recording of records, have been removed.

In addition to these missing observations, the primary limits to the sample counties and the resulting loss of observations, which is discussed in the data section of the paper, are covered in Table A.1.1

Table A.1.1	
List of Observations Lost to Sample Selection Criteria	
Counties with a recorded divorce rate over 10%, extreme outlier	98
Interior Counties	342
Counties more than 300 miles from border	27
Counties Lost to the elimination of CNGs where both bins do not exists. Primarily border counties that have no matching interior counties)	337
Counties in an LMA with a population density below 5 people per square mile	136

Of the 3033 continental counties in the United States 1,893 counties remain in the sample. The above sample limitations account for approximately 83%, or 940 of the missing 1140 observations.

Appendix Table A.1.2 - Primary Results Shown with Stepwise Controls

Recreation of Table 1.3, presented *with* step-wise addition of time varying controls

Non-Parametric Specification						
	1	2	3	4	5	6
Variables	lndivrate	lndivrate	lndivrate	lndivrate	lndivrate	lndivrate
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.
The omitted category in regressions 3-6 is CNGratio < 0.40 *** p<0.001, ** p<0.01, * p<0.05, + p<0.10

Table A.1.3 - Primary Results of Table 1.3 with SE clustered at State Level

Table A.1.3 recreates the primary findings in the paper, which are presented in Table 1.3, but clusters the standard errors at the state level. Divorce laws, which can cause serial correlation over time, are directly controlled for. Additionally, conditions, such as long term policy changes, which create concerns for difference and difference estimates are not present in this specification (Bertrand et al., 2004).

Recreation of Table 1.3 *with* SE clustered at State Level

Non Linear & Heterogeneous Treatment Specifications				
	1	**2**	**3**	**4**
Variables	lndivrate	lndivrate	lndivrate	lndivrate
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 A.1.4 - Primary Results of Table 1.3 with SE clustered at State Level & Lagged Own-Divorce rate controls

Table A.1.4 presents the standard errors clustered at the modal state level, but also shows specifications which include the previous year's own divorce rate as a right hand side control.

Recreation of Table 1.3 *with* SE clustered at State Level & Own Lagged-Divorce Rate as added control

Non-Parametric Specification						
	1	2	3	4	5	6
Variables	lndivrate	lndivrate	lndivrate	lndivrate	lndivrate	lndivrate
Ln(Neighbor divrate)yr-2	0.0199	0.0188	0.0181	0.0203	0.0199	0.0192
	(0.0365)	(0.0378)	(0.0371)	(0.0368)	(0.0383)	(0.0377)
Ln(Neighbor divrate)$_{yr-2}$* border	-0.0372	-0.0356	-0.0285	-0.0390	-0.0375	-0.0306
	(0.0294)	(0.0286)	(0.0296)	(0.0301)	(0.0291)	(0.0304)
Ln(Neighbor divrate)$_{yr-2}$*(0.40<CNGratio<0.60)*border	0.0604	0.0542	0.0428	0.0689	0.0635	0.0516
	(0.0500)	(0.0484)	(0.0461)	(0.0513)	(0.0494)	(0.0474)
Ln(Neighbor divrate)$_{yr-2}$*(0.60<=CNGratio<1.00)*border	0.1664*	0.1601*	0.1487*			
	(0.0752)	(0.0764)	(0.0697)			
Ln(Neighbor divrate)$_{yr-2}$* (0.60<= CNGratio<.78) * Border				0.2415**	0.2411**	0.2261**
				(0.0708)	(0.0684)	(0.0645)
Ln(Neighbor divrate)$_{yr-2}$*(0.78 <= CNGratio<=1.0)*border				-0.1026	-0.1314	-0.1281
				(0.1561)	(0.1409)	(0.1236)
Ln(Own Divorce Rate)$_{y-1}$			0.0548			0.0513
			(0.0536)			(0.0526)
Ln(Own Divorce Rate)$_{y-1}$ * Border			0.1053			0.1063
			(0.0762)			(0.0727)
State Divorce Law Controls	.	X	X	.	X	X
Population Density Controls	.	X	X	.	X	X
Employment Rate controls	.	X	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 State level.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10

Table A.1.5 – Falsification Test of Primary Results

The following table presents a falsification of the primary specification of the paper. Regressions from Table 1.3 are presented with past own border-region divorce rates regressed on current neighboring state divorce rates. As expected, no statistically significant results are found.

Falsification Test of Table 1.3

Heterogeneous Treatment & Non-Linear Specification Results Regressing Two Years Ago Own rate on Current Neighbor Rate			
	1	**2**	**3**
Variables	**Y = lndivrate $_{yr-2}$**		
Ln(Neighbor divrate)yr	0.0386	-0.0148	-0.0150
	(0.0973)	(0.0703)	(0.0703)
Ln(Neighbor divrate)$_{yr}$* border	-0.0411	0.0000	0.0027
	(0.1103)	(0.0764)	(0.0751)
Ln(Neighbor divrate)$_{yr}$*(CNGratio)*border	0.1420		
	(0.2030)		
Ln(Neighbor divrate)$_{yr}$*(0.40<CNGratio<0.60)*border		0.0452	0.0485
		(0.0850)	(0.0830)
Ln(Neighbor divrate)$_{yr}$*(0.60<=CNGratio<1.00)*border		0.0221	
		(0.1177)	
Ln(Neighbor divrate)$_{yr}$* (0.60<= CNGratio<.78) * Border			0.0481
			(0.1193)
Ln(Neighbor divrate)$_{yr}$*(0.78 <= CNGratio<=1.0)*border			-0.1303
			(0.2199)
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
Year Dummies	X	X	X
Linear State Time Trends	X	X	X
Observations	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

Table A.1.6 - Squared Interaction Model Results

The primary specification in the paper uses a non-linear model. However, a squared interaction model could also be chosen. A square interaction model allows for diminishing effects in regards to neighboring states size.

Table – A.1.6.

Linear Interaction Model, allowing for diminishing effects				
Variables	**2** **lndivrate**	**2** **lndivrate**	**3** **lndivrate**	**4** **lndivrate**
Ln(Neighbor divrate)yr-2	0.0412	0.0383	0.0412	0.0383
	(0.0562)	(0.0545)	(0.0563)	(0.0545)
Ln(Neighbor divrate)$_{yr-2}$* border	-0.0731	-0.0669	-0.1429*	-0.1232+
	(0.0520)	(0.0519)	(0.0569)	(0.0627)
Ln(Neighbor divrate)$_{yr-2}$*(CNGratio)*border	0.1862	0.1688	0.4992*	0.4188+
	(0.1482)	(0.1495)	(0.2128)	(0.2350)
Ln(Neighbor divrate)$_{yr-2}$*(CNGratio)*border^2			-0.1127+	-0.0890
			(0.0607)	(0.0607)
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.
The omitted category in regressions 3 & 4 is CNGratio 0.40 . *** p<0.001, ** p<0.01, * p<0.05, + p<0.10

Table A.2.1 – Relationship between the Unemployment Rate and the Employment Rate

Table A.2.1

VARIABLES	1 Unemployment
Percent of Population Employed	-0.693***
	(0.049)
Constant	39.429***
	(2.337)
State FE	X
Year FE	X
Observations	624
R-squared	0.887

Clustered Standard Errors in parentheses
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10

To more cleanly map the results presented in this paper to the unemployment rate, used as the primary variable of interest in previous research, I first converted the employment rate from in terms of number employed per thousand to terms of percent population employed out of 100. This conversion makes the domain of the employment measure equal to that of unemployment.

Results, from a regression of the employed percent of the population in a county on the unemployment rate, presented in Table A.2.1, indicates that a 1% increase in the employment rate translates to approximately a 0.7% decrease in the unemployment rate.

Both the sign and the magnitude of the coefficient make intuitive sense when one thinks about the differences and relation between unemployment and the employment rate. A simple back of the envelopes calculation supports the logic of the following results:

Assume ten people are actively looking for work in a community and they are considered in the labor force and population. If these ten people find a job, the employment rate changes by ((Previously Employed +10)/population) and unemployment changes by ((Previously unemployed -10)/Labor Force) where the population and the labor force are unchanged in this example. In absolute terms the magnitude of the change in unemployment with respect to the labor force is greater than the magnitude of the change with respect to the population considering the labor force is less than the population. Therefore, it would take a greater than ten person change in the employment rate to be equivalent to a 10 person change in the unemployment rate. This regression finds it would take approximately 14 individuals to change status to employ to be equivalent to a 10 person decrease in the unemployed stock.

Table A.2.2 – Robustness of State Level Regression Results to the Inclusion of Lagged Divorce Rates

The following table shows Table 2 recreated, but including own lagged divorce measures as a control in each specification.

Table A.2.2 - Robustness of State level Regressions to the Inclusion of Lagged divorce Rates

	Panel A				Panel B	
State-Level Regressions	Sample Years 1976-1988				Sample Years 1971-1988	
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	y= divorce rate		y= ln(divorce rate)		y= divorce rate	y =ln(divorce rate)
Unemployment Rate yr-1	-0.076***					
	(0.023)					
ln(Unemployment Rate) yr-1			-0.041			
			(0.048)			
Employment Rate yr-1		0.010***			0.005**	
		(0.003)			(0.002)	
ln(Employment Rate) yr-1				1.075***		0.229
				(0.315)		(0.225)
	576	576	576	576	862	862

Robust standard errors in parentheses
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10
All regressions contain state FE, year dummies and linear state- time trends

Table A.2.3 – Robustness of table 2 to use of Current Employment Measures

Table A.2.3 recreates Table 2.2 in the paper but uses a current measure of employment instead of a lagged measure of employment. As can be seen, by comparing this table to Table 2.2, the results are remarkably similar regardless of which measure is used.

Table A.2.3 - Robustness of State level Regressions to use of Current Employment Measures

	Panel A				Panel B	
State-Level Regressions	Sample Years 1976-1988				Sample Years 1971-1988	
	(1)	(2)	(3)	(4)	(5)	(6)
Variables	y= divorce rate		y= ln(divorce rate)		y= divorce rate	y =ln(divorce rate)
Unemployment Rate	-0.048*					
	(0.019)					
ln(Unemployment Rate)			-0.090+			
			(0.050)			
Employment Rate		0.008**			0.003+	
		(0.003)			(0.002)	
ln(Employment Rate)				0.937**		0.236
				(0.299)		(0.229)
	576	576	576	576	862	862

Robust standard errors in parentheses

*** p<0.001, ** p<0.01, * p<0.05, + p<0.10

All regressions contain state FE, year dummies and linear state- time trends

Table A.2.4 – Robustness of Primary LMA results to Clustering SE at State Level

Table A.2.4 is a replication of Table 2.3 in the dissertation, but with standard errors clustered at the modal state level. I argue it is unnecessary to cluster at this aggregate of a level as the regressions already control for the observational-state's divorce laws that would cause correlation amongst the error terms of the counties in a state over time. Additionally, as LMAs can cross state lines, this level of clustering requires that any LMA that crosses state lines be assigned to a state, somewhat arbitrarily. An inter-state LMA, with counties in more than one state, are assigned to the state that is the modal state of the counties in the LMA.

Table A.2.4 Robustness of Primary LMA results to Clustering SE at State Level

LMA Sample	**Panel A**		**Panel B**	
	Sample Years 1977-1988		Sample Years 1971-1988	
	1	2	3	4
	divorce rate	ln(divorce rate)	divorce rate	ln (divorce rate)
Employment Rate yr-1	0.001		0.001	
	(0.002)		(0.001)	
ln(Employment Rate) yr-1		0.214+		0.144
		(0.114)		(0.089)
Observations	4,548	4,548	6,820	6,820
Rsquared	0.849	0.913	0.880	0.918

Standard errors are clustered at the modal state-level are presented in parentheses.
All regressions contain controls for state divorce laws, LMA FE, year dummies and linear state- time trends.
*** p<0.001, ** p<0.01, * p<0.05, + p<0.10

Table A.3.1. - Reduced form Results

Table A.3.1 presents the reduced form specification of chapter 3.

Table A.3.1
Reduced Form Results

	Coal Sample		Steel Sample	
	1	2	3	4
Variables	ln(div rate)	ln(div rate)	ln(div rate)	ln(div rate)
ln(Coal Reserve)*ln(Coal Price)	-0.008	-0.005		
	(0.010)	(0.021)		
lagged Coal Instrument		-0.002		
		(0.016)		
(Percent employed in steel in 1969 in county) * (Current Percent of all US earnings from Steel)			48.403***	84.488+
			(11.540)	(47.861)
Lagged Steel Instrument				-41.738
				(55.213)
Observations	10,059	10,059	6,174	6,174
R-squared	0.701	0.701	0.566	0.566

Standard errors in parentheses, *** p<0.001, ** p<0.01, * p<0.05, + p<0.10
All regressions also contain year dummies, county fixed-effects and linear state-time trends