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To my parents, Bharati Maiti and Anjan Kumar Maiti,

who sacrificed a lot of things in life to bring me here,

and to my little brother Soumyadeep

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ABSTRACT

My doctoral dissertation consists of three empirical investigations in economics. Using dataset from the United States and India, I investigate the impact of law changes on labor market outcomes, effect of early classroom intervention on test scores, and estimate an important measure of elasticity.

In the first chapter, I investigate the effect of joint custody laws on children's future well-being. In a joint custody regime, both parents are given equal preference by the court while granting the custodial rights of their children in the event of divorce. Using 50 years of census data for the United States' population, I show that growing up in a joint custody regime leads to lower educational attainment and worse labor market outcomes. My results are robust to different model specifications and apply to both males and females.

In the second chapter, I explore the impact of corporal punishment on young children's academic outcome. In many parts of Europe and the United States, corporal punishment is banned in schools. However, in many developing countries that is not the case. Even if corporal punishment in schools is banned in a developing country such as, India, the law may not be adequately enforced. It is argued that corporal punishment produces bad outcomes in both the short run and the long run. Instead of instilling good behavioral traits in children, corporal punishment leads to more delinquent behavior. Corporal punishment in schools does not make students more attentive or motivated. However, so far there is no comprehensive empirical study that shows how the application of corporal punishment at schools affects children. Using a dataset from India, I show that corporal punishment in schools has a significantly negative impact on children's academic performance. To tackle the problem of endogeneity, I use an instrumental variables method.

In the third chapter, we use a large panel dataset covering the years 1988 to 2010 to estimate county specific total wage elasticities of labor demand for four highly aggregated industries in the United States. Our industries are construction, finance/real estate/service, manufacturing, and retail trade, which together employ on average over 80% of the U.S. national labor force per year. We use both the conventional constant coefficient panel data model and a random coefficients panel data model to estimate labor demand elasticities in various industries. We find the labor demand curves in all the industries studied to be downward sloping. We also find significant evidence that the total wage elasticity of labor demand exhibits regional variation. The labor demand estimates obtained in this study are useful to investigate the differential impact of various shocks and policy changes on the labor market. As an example, we use the estimated county specific labor demand elasticities to identify the impact of union membership and right to work laws on labor demand. We show that labor demand tends to become less elastic with higher union membership rates. We also find that labor demand becomes more elastic if a right to work law is in place.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

Chapter 1

Effect of Joint Custody Laws on Children's Future Labor Market Outcomes

1.1 Introduction

An almost inevitable byproduct of divorce is the issue of the allocation of custodial rights over a child. In the United States, the divorce rate started to increase sharply in the 1960s (Gruber 2004). According to Rasul (2006), one million children in the United States have to survive the difficult process of divorce proceedings every year. A few decades ago, mothers were typically granted the sole custody of a child in the event of divorce under the argument that maternal care is more important to nurture a child (Brinig and Buckley 1998). With the introduction of joint custody laws in the United States around 1973, both parents were given equal preference for custodial rights. As discussed in Nunley and Seals (2011a), joint custody can either mean joint legal custody or joint physical custody. In either of the cases, important decisions regarding the child have to be agreed upon by both parents.

Arguments have been forwarded both in favor of (e.g., Brinig and Buckley 1998) and against (e.g., Singer and Reynolds 1988) joint custody laws. Proponents of joint custody law suggest that it fosters more emotional and financial involvement on the part of the parents, and this extra involvement is better for children. Opponents of the joint custody law suggest that, following divorce, children are better off being cared for by the primary caregiver, and provision of joint custody might lead to an unhealthy domestic environment for the upbringing of a child.

Rasul (2006) provides a theoretical framework to investigate the economics behind joint custody. In Rasul's model, joint custody is optimal if the parent who attaches more importance to the development of the child keeps the majority of custodial rights. However, this result hinges on the assumption that the preferences for child development are relatively homogeneous. With sufficiently heterogeneous parental preferences for child development, sole custody is optimal. If the allocation of child custodial rights is not optimal, then it distorts the investment incentives for parents, and investment in children might be less than optimal. This is an interesting insight worthy of empirical investigation. Rasul's model provides us with a framework in which joint custody laws may actually harm a child's future prospects. Investment in a child is intended for human capital formation. If, as a consequence of the provision of joint custody, a child has access only to sub-optimal levels of resources while growing up, then it will adversely affect the stock of human capital the child will posses in the future when entering the labor market. Hence, the adoption of a joint custody law could have a significant impact on labor supply and the productivity of the labor force.

This study attempts to explore the impact of growing up in a joint custody law regime on future adult outcomes. In particular, I examine the consequences of children being exposed to a joint custody law regime on both educational outcomes (*years of education*, *high school dropout*, *high school graduate*, *some college*, and *college graduate*) and labor market outcomes (*real total income*, *percentage income over poverty line*, *weeks worked*, *real wage income*, and *employed*). For my analysis, I am using 50 years of census data obtained from the Integrated Public Use Microdata Series and a difference-in-differences (DiD) panel fixed-effect model. My results show that being introduced to joint custody laws as a child adversely affects future educational and labor market outcomes.

1.2 Background

Before the introduction of joint custody laws in the USA around 1973, mothers were overwhelmingly granted custodial rights in case of a divorce (Brinig and Buckley

1998). The logic behind such decisions was based on the argument that maternal care is more important for the development of a child. However, with the introduction of joint custody laws, fathers were also granted partial custodial rights of their children. The joint custody laws have made custodial rights gender neutral and are more focused on the best interests of the child. When divorced parents share the custody of a child, they need to make decisions regarding the child's development jointly. This system is supposed to be more conducive to a child's overall development. The idea is that a decision made by one parent and that may be clearly detrimental to a child's future well being can be blocked by the other parent (Brinig and Buckley 1998).

Rasul (2006) serves as the theoretical background for this paper. According to this study, joint custody laws have both "efficiency and distributional consequences". Each spouse's share of marital surplus is determined by the share of custodial rights. If the allocation of a child's custodial rights are made *ex ante*, then it will maximize investment in the child and minimize the likelihood of divorce. However, it is not feasible for couples to decide beforehand the level of resources that are going to be invested in a child. Hence, it is more than likely that the allocation of a child's custodial rights, conditional upon divorce, is going to be decided *ex post*. Any kind of *ex post* allocation of child custody will maximize *ex ante* investment only if the couples have sufficiently heterogeneous preferences for child development. Here, by '*ex ante*' we mean before the parents get divorced and '*ex post*' identifies the post-divorce situation. If the spouses have sufficiently heterogeneous preferences for child development, then it is optimal for the high-valuation parent to have the sole custody. However, for spouses with relatively homogeneous preferences of child development, joint custody is optimal if the high-valuation parent keeps the majority of custodial rights. Hence, joint custody is not universally optimal and the allocation of the child's custody should ideally depend on parental preferences for child development.

Even in cases where joint custody is preferred, it is in the best interests of a child that the high-valuation parent retains the majority of custodial rights. The problem for the judicial system, however, is the fact that the court does not have all the information. For example, the court does not know how spouses value child development. Even determining the high-valuation parent is riddled with problems. Respective monetary investments in children made by parents may give a distorted view of parent's preferences for child development, since investment can also be non-monetary, such as through the investment of time. This information asymmetry creates a situation where, *the best interests of a child* might not be served by granting both parents an equal share of child custody.

The *Coasian Irrelevance Theorem* holds in Rasul's (2006) model if child custody rights are treated just as other property rights and parents bargain over them simultaneously. Hence, the parent with higher valuation for child development will trade other property rights to gain better custody rights through bargaining. Introduction of a joint custody law marks a shift in the spousal bargaining power within a household. Before the introduction of joint custody laws, mothers were usually expected to receive sole custody of children in case of a divorce. Since joint custody laws made the process of granting child custody gender-neutral, mothers' bargaining position was weakened. This outcome of joint custody laws has important ramifications for the human capital formation of children coming from a separated household. It has been suggested by Lundberg et al. (1997) that a weakened bargaining position for mothers leads to lower investment in children. Hence, joint custody laws, as well-intentioned as they might be, have the ability to hurt the future prospects of a child whose parents have divorced.

Brinig and Buckley (1998), using bonding and monitoring theories, suggest that joint custody laws lead to fewer divorces and higher child support payments. Bonding theories predict that a father will be more emotionally attached to a child if he is expected to keep some ties with the child after divorce. If a state implements joint custody laws, then the fathers living in that state can expect to retain custodial rights of children if and when a divorce takes place. Monitoring theories predict that a parent will be more willing to contribute financially to a child's development if some sort of custodial rights are granted. The key idea is that a parent is willing to invest more if that parent can monitor how the money intended for investment in the child is being spent, then the parent may be willing to invest more. So, even in case of a court mandated child support payment, a parent might be willing to pay more to make sure the child has access to sufficient resources, if the investment can be monitored. Joint custody laws allow for such provisions, and, therefore, are more conducive for the human capital formation of a child coming from a broken household.

However, granting joint custody of a child to both parents also has its pitfalls. Brinig and Buckley (1998) suggest three possible scenarios where granting joint custody instead of sole custody may be harmful for the child. In the first scenario, joint custody may be awarded to unfit fathers. This may prove to be against a child's best interests since it hampers the developmental process of the child. Brinig and Buckley (1998) argue that, since both parents can monitor a child under a joint custody setting, such issues are unlikely to arise. In the second scenario, a parent may need to forgo other property rights in a divorce settlement in order to gain the sole custody rights of a child. However, Brinig and Buckley (1998) suggest that it does not necessarily make joint custody laws a bad initiative. In the third case, joint custody laws might as well become inactive if couples use it as a bargaining chip instead of an effective instrument to serve the best interests of a child whose parents are divorcing. Brinig and Buckley (1998), however, argue that this kind of *Coasian Irrelevance* might not work in reality since people in general might be unwilling to trade their children for assets or those arrangements might not meet the legal requirements. Using data from the Statistical Abstracts of the United States for the years between 1980-1991, and with the help of Ordinary Least Squares (OLS) and Two Stage Least Squares (2SLS) Fixed Effect methods, they find that joint custody laws reduce divorce levels. Child support payments are also positively influenced by the joint custody laws.

The critics of joint custody laws, however, insist that implementing them is a bad idea (e.g., Singer and Reynolds 1988) and the system under which a court assigns a "primary caretaker" is better.

Using the Integrated Public Use Microdata Series (IPUMS) from the United States Census for 1980 and 1990 waves and a Difference-in-Difference (DiD) method, Nunley and Seals (2011a) find that following the implementation of joint custody laws, parental investment in children (e.g., private school attendance) may actually decline. Since joint custody laws weaken the bargaining power of mothers, they tend to develop more market-specific skills to be better placed at the bargaining table in case of a divorce. They interpret the results to mean that fathers give investment in a child lower importance following joint custody law implementations.

In a related study, Nunley and Seals (2011b), with the help of the Panel Study of Income Dynamics (PSID) dataset, find that following the introduction of joint custody laws, there are changes in the within-household dynamics with mothers working outside of the home more often whereas fathers increase the propensity of working at home. According to them, since fathers can expect to see their children more often following divorce, they decide to develop skills more suitable for the upbringing of a child. This leads to a reduction in the amount of time spent on outside work. Mothers, however, need not invest so much time developing skills solely for housework since fathers will also share some responsibilities of household work. Hence, mothers can now spend more time working outside of the house. This is another signal of the changed bargaining dynamics inside a household following the introduction of joint custody laws.

Leo (2008) uses US census data to find evidence that children from divorced or separated households will do better academically if they grow up in a joint custody law state. In another recent working paper, Chen (2013) finds that exposure to joint custody laws during childhood reduces the likelihood of high school graduation by about 2 percentage points.

Halla (2013) suggests that joint custody law implementations are responsible for higher marriage and fertility rates as well as higher divorce rates. He also finds evidence of a declining labor market participation for females. However, he does not take into account whether the respondents were exposed to joint custody law regimes as children, nor does he explore labor market outcomes of the well-being of the population. The main source of Halla's data is the National Vital Statistics System (NVSS) maintained by the National Center for Health Statistics (NCHS). He uses a DiD panel fixed-effects model for the purpose of his investigation.

The main theme emerging from the existing literature is that the overall impact of joint custody laws on children is ambiguous. The empirical literature is completely silent (at least to this researcher's best knowledge) on the long run impact of joint custody laws on children. This paper contributes to the existing literature by investigating how joint custody laws affect the future educational and labor market outcomes of children growing up under joint custody law regimes. As Rasul (2006) suggests, joint custody laws might influence the parental decision-making process of investment in child development. That means children might not have access to the optimal level of resources while growing up. This can hamper their ability to form the optimal level of human capital and, in turn, negatively affect future labor supply and labor force productivity. Hence, it is important to investigate whether joint custody laws indeed have such effects. I also investigate the results for male and female subsamples separately to evaluate any gender-based discrimination in resource allocation. My research design allows me to identify both long run and short run effects.

1.3 Data and Methodology

I use the Integrated Public Use Microdata Series (IPUMS-USA) for the United States census years 1960, 1970, 1980, 1990, and 2000. This database is a collection of high-precision samples obtained from the United States census data (Ruggles et al., 2010). I am using the 1% State sample for the five census years. Following Gruber (2004), I collapse the data into state of residence/state of birth/year/age/sex cells. This setting can be justified as the variations in law come at the state/year/age levels (Gruber 2004). This methodology has also been followed elsewhere in the economics literature (e.g., Wolfers 2006, Alesina and Giuliano 2007). In my modified data, each cell becomes the mean of observations for a particular combination of state of residence, state of birth, year, age, and sex. While obtaining the mean I use personal weights so that my data incorporate the underlying microstructure of the American population. A shortened version of the data is provided in Table 1 for illustrative purposes. Table 2 shows how various laws relevant to our current analysis evolved over the years.

For my analysis, I include only the individuals who were born in the United States, are within the age range of 25-50 years, are not enrolled in school, and are earning a non-negative amount of income. I restrict the maximum amount of income to \$500,000. I also discard the observations for which worker class, weeks worked, and poverty index data are not available. Observations from the prison inmate population are not included in this study either.

There are nine outcome variables which can be broadly classified into two categories: educational outcomes (*years of education*, *high school dropout*, *high school graduate*, *some college*, and *college graduate*), and labor market outcomes (*real total income*, *percentage income over poverty line*, *weeks worked*, *real wage income*, and *employed*). The variable *years of education* is the total number of years a person has been in school. *High school dropout*, *high school graduate*, and *college graduate* are all indicator variables taking a value of 1 if an individual falls into the specified category and 0 otherwise. *Some college* takes the value of 1 if an individual has been to college but never graduated. *Real total income* and *real wage income* are price adjusted income variables, where the adjustment factors are supplied by IPUMS. The price adjustment converts all income variables to the year 2000 level in real terms. *Percentage income over poverty line* is the value of one hundred times a person's income divided by the poverty level income. A value of 200 will therefore mean that the individual's income is 200% above the poverty threshold. This acts as an indicator of well-being in our model. *Weeks worked* is an index for the number of weeks worked. It takes values from zero to four. *Weeks worked* is zero if no work is done by an individual, 1 if 1-13 weeks have been worked, 2 if 14-26 weeks, 3 if 27-39 weeks, and 4 if 40-52 weeks have been worked. *Employed* is an indicator variable taking the value of 1 if an individual is employed. Again, I am collapsing my data by state/year/age levels, for each age from 25-50, for a total of 26 age years, classified into 51 states of residence including the District of Columbia, and further classified into 51 states of births, ordered by year and separated by sex. Hence, each cell of my modified data corresponds to the cell mean for all the observations falling into a particular combination of state of residence, state of birth, year, age, and gender.

The purpose of the study is to see whether growing up in a joint custody law regime has an economically relevant impact on an individual in the future. To capture whether an individual was introduced to a joint custody law regime while growing up, I use the information about an individual's year of birth to calculate whether the joint custody law was implemented in that individual's state of birth by the time she turned 18. I estimate a difference-in-differences (DiD) panel fixed effect model (e.g., Gruber 2004, Wolfers 2006, Halla 2011) for the set of my outcome variables. Following Gruber (2004), the model can be written as follows:

$$\begin{aligned} Outcome_{asbt} &= \alpha + \beta_1 CUSTODY_{st} + \beta_2 KIDCUST_{abt} + \beta_3 RACE_{ast} \\ &\quad + \beta_4 UNILAT_{st} + \beta_5 NOFLT_{st} + \beta_6 EQUIT_{st} + \beta_7 \eta_a \\ &\quad + \beta_8 \sigma_b + \beta_9 \delta_s + \beta_{10} \tau_t + \beta_{11} \eta_a * \tau_t + \epsilon_{asbt} \end{aligned} \tag{1.1}$$

Here, $Outcome$ identifies any of the outcome variables. Subscript a denotes age, s represents current state of residence, b stands for state of birth, and t identifies the year. $CUSTODY$ is an indicator variable taking the value of 1 if a joint custody law is implemented in a state in a given year, $KIDCUST$ takes on the value of 1 if joint custody law was in effect in the state of birth before age 18, $RACE$ include white and black indicator variables. $UNILAT$, $NOFLT$, and $EQUIT$ are binary variables taking the value of 1 if unilateral divorce laws, no fault divorce laws, and equitable property laws are in effect in the current state of residence, respectively, in a particular year. $\eta_a, \sigma_b, \delta_s, \tau_t$ are binary variables for age groups, state of birth, current state of residence, and year, respectively. $\eta_a * \tau_t$ is the set of interaction terms for age groups and year. Gruber (2004) suggests that this interaction term can capture age specific variances over time. I have divided the age range into the following groups: 25-30, 31-

35, 36-40, 41-45, 46-50. The indicator variable $KIDCUST$ is constructed following the standard procedure in labor economics (e.g., Gruber 2004, Wolfers 2006). The information about the state of residence of an individual is only available for the year of birth and the current census year.

There can be two possible sources of bias in my analysis. First, bias may come from time invariant omitted variables influencing both my outcomes and the joint custody law implementations. Since we are carrying out the analysis at the state level, state fixed effects should be sufficient to account for this kind of time invariant bias (Angrist and Pischke 2009). I have included current state of residence, state of birth, and time fixed effects in my model. This procedure essentially follows a least squares dummy variable approach (LSDV). Another source of bias may stem from the inability to account for the unobserved trends in the implementation of joint custody laws. May be the states where custody battles are on the rise, are also the states implementing the joint custody laws. Following Gruber (2004), I include linear time trends for current state of residence and state of birth. Gruber (2004) suggests that including trends can sufficiently address the issue of bias coming from unobserved trends. Also, if the directions of results without including trends hold even after the inclusion of trend, then endogeneity through time-varying unobserved heterogeneity is not an issue for our estimates. Nunley and Seals (2011a) and Halla (2011) suggest that there has been no systematic implementation of joint custody laws in the United States over the years. Combining all variables the model can be re-written as:

$$\begin{aligned} Outcome_{asbt} = {} & \alpha + \beta_1 CUSTODY_{st} + \beta_2 KIDCUST_{abt} + \beta_3 RACE_{ast} \\ & + \beta_4 UNILAT_{st} + \beta_5 NOFLT_{st} + \beta_6 EQUIT_{st} + \beta_7 \eta_a + \beta_8 \sigma_b + \beta_9 \delta_s \\ & + \beta_{10} \tau_t + \beta_{11} \eta_a * \tau_t + \beta_{12} \delta_s * Trends + \beta_{13} \sigma_b * Trends + \epsilon_{asbt} \quad (1.2) \end{aligned}$$

To account for autocorrelation within the state of residence/state of birth cells over the years, I cluster over state of residence*state of birth*year (e.g., Gruber 2004, Bertrand et al. 2004). The standard errors are also corrected for possible heteroskedasticity.

As can be seen from Table 2, the joint custody law came into effect in various states in the United States between 1973-2003. This within-states over-time variation allows me to use a DiD panel fixed model. My model is identified by the variation in the timing of joint custody law implementation in different states. I control for unilateral divorce laws, no fault divorce laws, and equitable property laws to make sure that I am calculating the effect of joint custody law itself, and not of any other law changes.

I run the regression for male and female subsamples separately, and also pool the subsamples. In addition, I run the regressions with and without current state of residence and state of birth specific trends.

Sample means of the outcome variables for the whole modified data are provided in Table 3. Means for the male and female subsamples are also provided.

1.4 Results

The model specification allows us to investigate effects of the existence of joint custody laws both during childhood (through the coefficient of $KIDCUST$) and contemporaneously (through the coefficient of $CUSTODY$). We are mainly interested in the coefficient of $KIDCUST$ since we want to measure the effects of growing up under the joint custody laws.

In Tables 4 and 5, I provide the estimation results for all adults (male and female subsamples are pooled). Tables 6 and 7 contain the results for the female subsample. Tables 8 and 9 show the results for the male subsample. The first column in these tables gives the results from the model without trends (equation 1), and the last column gives the results from the model with trends (equation 2).

For the educational outcomes in the aggregate sample (Table 4), if children grow up under joint custody laws, total years of education decreases by 0.074 years when the model doesn't have a trend. This corresponds to a fall by 0.6% of the sample mean. With a trend present, *education* is reduced by becomes 0.081 years, or 0.7% of the mean. We need to note that none of these estimates are statistically significant. Being exposed to joint custody laws as children raises the likelihood of being a high school dropout by 0.028 percentage points without trend (7.8% of the sample mean) and by 0.014 percentage points with trends (3.9% of the sample mean). Both of these estimates are statistically significant. Growing up in a joint custody law regime also raises the odds of being a high school graduate by 0.01 percentage points (8.2% of the sample mean) in the model without trend. When a child is exposed to joint custody laws, it lowers the odds of the child graduating from college by 0.01 percentage points (7% of the mean), and the likelihood of the child attending some college at all by 0.019 percentage points (7% of the mean). These estimates refer to the model without trend and are statistically significant. The estimates retain their signs in the model with trend. The rise in high school graduation with a concurrent fall in college graduation may imply that there is a resource constraint for the children whose parents have divorced. A similar argument is made in Gruber (2004) for unilateral divorce laws.

In the category of labor market outcomes (Table 5), being exposed to a joint custody law regime reduces real total income by $2,396 (6.7% of the sample mean) and real wage income by $1,998 (6% of the sample mean), for the model with trends. The percentage income above the poverty threshold also falls by 10.21 percentage points and weeks worked by 0.023 (0.6% of mean). The likelihood of being employed decreases by 0.013 percentage points (1.5% of the sample mean). We notice that the signs of the coefficients remain the same for our models with and without trend. This is an indication that our model results are robust.

According to Table 6, for females, growing up in a joint custody regime means that the likelihood of being a high school dropout goes up by 0.013 percentage points and the likelihood of being a college graduate falls by 0.021 percentage points, for the model with trends. These estimates are also statistically significant. Years of education falls for growing up under joint custody laws and the odds of graduating high school rise, although they are no longer statistically significant.

Table 7 shows the labor market outcomes of growing up under joint custody laws for the female subsample. Growing up in a joint custody regime leads to a decrease in real total income of $990.20 and real wage income by $760.28. The percentage income above the poverty threshold falls by 7.674 percentage points and weeks worked by 0.04. The likelihood of being employed is also reduced by 0.02 percentage points. Again, in all these instances, the directions (sign) remain the same for the models with trends and the models without trends.

For the male subsample (Table 8), being exposed to a joint custody regime as a child raises the likelihood of being a high school dropout by 0.016 percentage points. The likelihoods of going to college and graduating from college fall by 0.014 and 0.033 percentage points respectively.

In Table 9, the results for the labor market outcomes are consistent with the results in the previous results tables. Being exposed to joint custody laws as a child decreases real income of $4,003.41 and wage income by $3,396.27. The percentage income over the poverty threshold falls by about 13 percentage points and weeks worked by 0.015. The likelihood of being employed also goes down by 0.009 percentage points.

1.5 Discussion

Rasul (2006) lays the theoretical foundations for our present analysis. Rasul argues that sole custody is optimal if parents have sufficiently heterogeneous preferences for child development. If parents have relatively homogeneous preferences for child development, then joint custody is optimal assuming the high-valuation parent retains the majority share of the custodial rights. In practice, the court does not have all the information about parental preferences when making child custody decisions. This kind of information asymmetry may lead to less than optimal outcomes. Hence, joint custody may be granted where sole custody is warranted, and vice versa. If custodial allocations are not efficient, then it distorts the investment incentives of parents. Hence, the investment in a child's human capital development may become inadequate. This inadequacy may have serious consequences for the child's future.

I find that growing up in a joint custody law regime leads on average to worse future outcomes for children. In particular, for individuals growing up under the joint custody law regime, the likelihood of dropping out of high school increases, and the odds of graduating from college decreases. The labor market outcomes are equally depressed. Being exposed to a joint custody regime reduces future real total income, the percentage income above poverty, weeks worked, real wage income, and the likelihood of being employed. These results hold true for the aggregate sample, the female subsample, and the male subsample. The results are robust to the inclusion of trends in the model, which suggests that endogeneity through unobserved heterogeneity changing over time is not driving the results.

The findings of this paper can be reconciled with the findings of the existing literature. Nunley and Seals (2011a) argue (following Rasul 2006) that implementation of a joint custody law leads to a weakened bargaining position for mothers. If fathers give investment in child development lower importance than mothers, then shifting the bargaining power in favor of fathers will lead to a lower investment in children. My results are consistent with this line of thinking. Since, mothers have a weakened bargaining position, the investment in children tends to be lower. Elsewhere in the literature, it has been proposed that an increased bargaining power for mothers will lead to greater investment in children (e.g., Lundberg et al. 1997). A lower investment in children will weaken their ability to acquire human capital during their developmental phase, which will lead to weaker labor market outcomes in the future. In my analysis, I find that exposing children to joint custody laws will lead to a higher likelihood that these children drop out of high school and to a lower likelihood that they graduate from college. These findings provide support to the idea that joint custody laws weaken the bargaining position of mothers, and tend to lower investment in children.

The lower labor market outcomes due to growing up in a joint custody regime can be a result of lower human capital accumulation. Lower total income and lower wage income due to being exposed to joint custody laws as a child also implies earning lower wage rates. A lower likelihood of finding a job and the finding that fewer weeks are worked also support the notion that individuals growing up in a joint custody law regime as children are having a more difficult time later in the labor market.

Gruber (2004) suggests two linkages through which growing up in a unilateral divorce regime might affect the likelihood of graduating college: liquidity constraints and extra stress. I find that for the pooled sample, the odds of graduating high school increases, but the likelihood of attending college and graduating from college decreases. Thus resource constraints may explain lower educational attainment growing up in a joint custody regime.

Another interesting feature of the results in this paper is the large difference between the decrease in future income of males and females. Being introduced to the joint custody regime lowers the future real total income for the female subsample by $990 in the model with trends. However, the decrease in future real total income for the male subsample is far larger at $4,003. Hence, a possible resource constraint affects males significantly more than females. We can provide two reasons for this result. First, the increase in female graduation rates and workforce participation have been relatively recent phenomena. Since we start our analysis in 1960, the effect of being introduced to a joint custody regime as a child may therefore be less severe on females. The second explanation of the lower impact on females is related to the idea of gender-specific discrimination in the allocation of resources in a household. If female children are receiving fewer of the available resources, then a shock in the form of a divorce and the ensuing resource constraint will be less severe for them than for their male counterparts. Since female children already had fewer resources to begin with, a parental divorce affects them less than it does male children. In sum, our finding may provide indirect evidence of gender-based discrimination with regard to resource allocation among children. Further research into this aspect may be of interest.

Overall, I find that growing up in a joint custody regime has detrimental effects on future educational and labor market outcomes. The existing literature suggests that weakening the bargaining power of mothers in a household will lead to lower investment in children's development. My results are fully consistent with this view.

1.6 Conclusion

Before the introduction of joint custody laws, mothers were predominantly given the custodial rights in the event of a divorce. The argument in favor of such a system was the recognition that mothers tended to be the "primary caregivers". Joint custody laws made the awarding of custodial rights gender-neutral. Bonding and monitoring theories suggest that a joint custody regime would be a better option than a sole custody regime because it would provide fathers with an incentive to be emotionally closer to their children, and as a consequence, they would be more willing to support their children financially. However, the literature also suggests that if mothers lose their bargaining power, even if only partially, the investment in children tends to be lower. Rasul (2006) suggests that if parental preferences for child development are sufficiently heterogeneous, then sole custody is a better option. Even when joint custody is optimal (under relatively homogeneous parental preference for child development), investment in a child is maximized if the parent who is giving child development more weight retains the majority share of the custodial rights. Hence, an equal spread of custodial rights after divorce may not be in the best interests of a child. My results support this argument. I do not find growing up in a joint custody law regime to be beneficial for children.

The literature on the economics of divorce has not focused yet on the future outcomes of growing up in a joint custody law regime. My results show that being exposed to a joint custody law regime leads to lower educational attainment (higher likelihood of dropping out of high school, lower likelihood of graduating from college) and worse labor market outcomes (lower real total income, lower real wage income, lower percentage income over poverty line income, lower weeks worked, and lower likelihood of being employed). My results are robust do different specifications and hold for both the male and female subsamples.

I also find indirect evidence of discriminatory resource allocation among children based on their gender. Being introduced to joint custody as children, males are more severely affected than females. If female children already had lower resources to begin with, then the resource constraint after divorce does not hurt them as much as it does male children. This may be interpreted as indirect evidence of within-household gender-based discrimination.

Although a joint custody regime is intended to serve the best interests of a child, it appears that it is working in the opposite direction.

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Table 1.1: Data

Year	Age	Gender	AL	AK	AR	...	bAL	bAK	bAR	...	y1960	y1970	...	CUST	KIDCUST	...	RealInc
1960	25	M	1	0	0		1	0	0		1	0		0	0		19118
1960	25	M	1	0	0		0	0	1		1	0		0	0		18606
⋮	⋮	⋮	⋮	⋮	⋮		⋮	⋮	⋮		⋮	⋮		⋮	⋮		⋮
1960	25	F	1	0	0		1	0	0		1	0		0	0		8846
⋮	⋮	⋮	⋮	⋮	⋮		⋮	⋮	⋮		⋮	⋮		⋮	⋮		⋮
1990	32	F	0	0	1		0	1	0		0	0		0	0		16773
⋮	⋮	⋮	⋮	⋮	⋮		⋮	⋮	⋮		⋮	⋮		⋮	⋮		⋮
2000	50	F	0	0	0		0	0	0		0	0		1	0		11737

Note: Each row identifies a collapsed sample observation, with its unique values for each variable such as Age, Gender, State of Residence (Alabama, Alaska, Arkansas, ...), State of birth (bAL, bAK, bAR, ...), year indicator variables (y1960, y1970, ...), Growing up under joint custody laws as children (KIDCUST), outcome variables (RealInc, ...).

There are as many rows as there are unique combinations of the Census Year (Year), the age of a person (Age from 25 to 50), Gender, State of residence, and state of birth.

There are numerous economic and demographic variables in the data set. The table shows just one: real income (RealInc). The values for these are derived as weighted averages of all those persons in the sample with the same values of age, gender, state of residence, state of birth, and Census year. The weight used is the person specific weight as provided in the original data set.

Table 1.2: Evolution of Joint Custody Law, and Various Divorce Laws

State	Joint Custody	Unilateral	No Fault	Equitable	State	Joint Custody	Unilateral	No Fault	Equitable
AL	1997	1971	1971	1980	MT	1981	1973	1973	1976
AK	1982	1935	1935	pre-1950	NE	1983	1972	1972	1972
AR	2003	-	1937	1979	NV	1981	1967	1931	pre-1950
AZ	1991	1973	1931	pre-1950	NH	1974	1971	1971	1988
CA	1979	1970	1970	pre-1950	NJ	1981	-	1971	1971
CO	1983	1972	1972	1972	NM	1982	1933	1933	pre-1950
CT	1981	1973	1973	1973	NY	1981	-	1967	1962
DC	1996	-	1966	1977	NC	1979	-	1910	1981
DE	1981	1968	1957	pre-1950	ND	1993	1971	1971	pre-1950
FL	1979	1971	1971	1988	OH	1981	-	1974	1990
GA	1990	1973	1973	1980	OK	1990	1953	1953	1975
HI	1980	1972	1965	1955	OR	1987	1971	1971	1971
ID	1982	1971	1945	pre-1950	PA	1981	-	1980	1979
IL	1986	-	1984	1977	RI	1992	1975	1910	1979
IN	1973	1973	1973	1958	SC	1996	-	1969	1979
IA	1977	1970	1970	pre-1950	SD	1989	1985	1985	pre-1950
KS	1979	1969	1969	pre-1950	TN	1986	-	1963	1959
KY	1979	1972	1962	1972	TX	1987	1970	pre-1910	1970
LA	1981	-	1916	1978	UT	1988	1987	1943	pre-1950
ME	1981	1973	1973	1972	VT	1992	-	1969	pre-1950
MD	1984	-	1969	1969	VA	1987	-	1960	1982
MA	1983	1975	1975	1974	WA	-	1973	1921	pre-1950
MI	1981	1972	1972	1983	WV	-	-	1969	1984
MN	1981	1974	1933	1951	WI	1979	1978	pre-1910	1978
MS	1983	-	1978	pre-1950	WY	1993	1977	1977	pre-1950
MO	1983	-	1974	1974					

Source: Brinig and Buckley(1998), Gruber (2004), Halla (2011), Alesina and Giuliano (2007)

Table 1.3: Sample Means of Outcome Variables for Adult Females and Males

	Pooled	Adult Female	Adult Male
Years of Education	12.003	12.130	11.893
High School Dropout	0.357	0.361	0.354
High School Graduate	0.122	0.138	0.109
Some College	0.275	0.277	0.273
College Graduate	0.144	0.140	0.148
Real Total Income ($)	36043.56	20334.5	49600.6
Above Poverty	318.492	319.147	317.926
Weeks Worked	3.623	3.319	3.885
Real Wage Income ($)	31785.82	18189.53	43519.54
Employed	0.890	0.810	0.958
No. of Observations	221303	102515	118788

Table 1.4: All Adults : Educational Outcomes

	KIDCUST	
	Without Trend	With Trend
Years of Education	-0.074	-0.081
	(0.065)	(0.070)
High School Dropout	0.028 ***	0.014 ***
	(0.003)	(0.003)
High School Graduate	0.010 **	0.015 ***
	(0.005)	(0.005)
Some College	-0.019 ***	-0.002
	(0.005)	(0.005)
College Graduate	-0.011 **	-0.027 ***
	(0.005)	(0.005)

Note: Regression results on pooled adult sample
Standard errors in parentheses
*, **, *** significant at 10%, 5%, and 1% respectively

Table 1.5: All Adults: Labor Market Outcomes

	KIDCUST	
	Without Trend	With Trend
Real Total Income	-2167.121 ***	-2395.961 ***
	(368.942)	(400.175)
Above Poverty	-11.214 ***	-10.212 ***
	(1.461)	(1.491)
Weeks Worked	-0.022 ***	-0.023 ***
	(0.009)	(0.009)
Real Wage Income	-1765.762 ***	-1998.139 ***
	(336.868)	(363.141)
Employed	-0.012 ***	-0.013 ***
	(0.003)	(0.004)

Note: Regression results on pooled adult sample
Standard errors in parentheses
*, **, *** significant at 10%, 5%, and 1% respectively

Table 1.6: Adult Females: Educational Outcomes

	KIDCUST	
	Without Trend	With Trend
Years of Education	-0.133	-0.143
	(0.089)	(0.094)
High School Dropout	0.021 ***	0.013 ***
	(0.004)	(0.004)
High School Graduate	0.002	0.004
	(0.006)	(0.007)
Some College	-0.001	0.009
	(0.007)	(0.007)
College Graduate	-0.007	-0.021 ***
	(0.006)	(0.006)

Note: Regression results on adult females
Standard errors in parentheses
*, **, *** significant at 10%, 5%, and 1% respectively

Table 1.7: Adult Females: Labor Market Outcomes

	KIDCUST	
	Without Trend	With Trend
Real Total Income	-850.895 ***	-990.197 ***
	(336.881)	(367.434)
Above Poverty	-8.632 ***	-7.674 ***
	(2.045)	(2.128)
Weeks Worked	-0.024 *	-0.040 ***
	(0.014)	(0.015)
Real Wage Income	-572.618 *	-760.275 **
	(304.103)	(329.784)
Employed	-0.015 ***	-0.020 ***
	(0.006)	(0.006)

Note: Regression results on adult females
Standard errors in parentheses
*, **, *** significant at 10%, 5%, and 1% respectively

Table 1.8: Adult Males: Educational Outcomes

	KIDCUST	
	Without Trend	With Trend
Years of Education	-0.022	-0.013
	(0.034)	(0.101)
High School Dropout	0.034 ***	0.016 ***
	(0.005)	(0.005)
High School Graduate	0.018 ***	0.027 ***
	(0.007)	(0.007)
Some College	-0.028 ***	-0.014 **
	(0.007)	(0.007)
College Graduate	-0.015 **	-0.033 ***
	(0.007)	(0.007)

Note: Regression results on adult males
Standard errors in parentheses
*, **, *** significant at 10%, 5%, and 1% respectively

Table 1.9: Adult Males: Labor Market Outcomes

	KIDCUST	
	Without Trend	With Trend
Real Total Income	-3317.677 ***	-4003.409 ***
	(612.839)	(670.148)
Above Poverty	-13.918 ***	-12.984 ***
	(1.867)	(1.960)
Weeks Worked	-0.016 **	-0.015 *
	(0.008)	(0.008)
Real Wage Income	-2826.29 ***	-3396.269 ***
	(564.842)	(611.058)
Employed	-0.009 ***	-0.009 ***
	(0.004)	(0.004)

Note: Regression results on adult males
Standard errors in parentheses
*, **, *** significant at 10%, 5%, and 1% respectively

Chapter 2

Effect of Corporal Punishment on Early Childhood Outcomes

2.1 Introduction

The importance of childhood on a young adult's development has been well-documented (Heckman 2008, Heckman et al., 2009; Chetty et al., 2011). These studies acknowledge the importance of a better childhood environment on future educational and labor market outcomes. Growing up in a well-balanced environment, fosters all around growth and development. Disruption to a balanced environment can lead to severe developmental deficiencies in a child, in particular for already disadvantaged children (Heckman et al., 2009).

Corporal punishment has been used for a long time to discipline children. As summarized in Dwyer (2010) and NCPCR Discussion Summary (2008), the proponents of corporal punishment argue that its usage can instill good values in a child and this in turn leads to a balanced and fulfilling adulthood. However, the opponents argue that corporal punishment is not an effective instrument in nurturing a child and it in fact produces counterproductive results (Gershoff, 2002). For example, corporal punishment may make an already unruly child to become even more uncontrollable.

The application of corporal punishment can thus have two opposite effects on a child. On the positive side, corporal punishment can correct the behavioral characteristics not conducive to proper growth in a child. On the negative side, corporal punishment can worsen a child's problematic behavioral characteristics hindering a proper growth process. Since there are potentially two opposite effects coming from the application of corporal punishment on a child, there is a need to investigate the net effect empirically.

Research in economics on early childhood development has so far not focused on the effect of corporal punishment on children. There is no conclusive quantitative study on how the application of corporal punishment at schools affects a young adult. One of the most important reasons behind this absence of evidence is the lack of data. It is very difficult to find data which contains information about whether a child has been physically punished, be that in school or at home. This paper is the first quantitative study investigating the effects of corporal punishment at schools on early childhood outcomes. Using the *Young Lives* dataset for India (Morrow and Singh, 2014), I explore the effects of corporal punishment on the educational outcomes of young children.

In India, the issue of corporal punishment has received much attention lately. Although, corporal punishment has recently been banned in India, the law has not been enforced rigorously. In our data, more than 78% of the children have been physically punished at least to some extent in school, and more than 52% of the children have been physically punished fairly regularly.

One important obstacle that I need to overcome in this study is the problem of endogeneity. One source of endogeneity can be the omitted variable bias. An unstable home environment contributes to the poor academic performance of a child in school, but also turns the child more unruly at school, thereby making the child more likely to receive corporal punishment at school. Simultaneity bias can be another source of endogeneity. Teachers may use corporal punishment to raise students' academic performance. But as a student performs poorly academically, she receives more corporal punishment, and this punishment has an adverse psychological impact on the student, making her to perform even worse.

In this study, the main explanatory variable of interest is *physical punishment* which measures if and to what extent a child is being physically punished. In order to overcome this problem of endogeneity, I implement an instrumental variables approach, using the variable *physical punishment on others* as the instrument. This variable records whether a child has noticed any other student receiving corporal punishment, and to what extent. The outcome variables in my study are the mathematics and EGRA (Early Grade Reading Assessment) test scores.

The empirical results from this study show that corporal punishment has a statistically significant negative impact on a child's academic performance. Hence, being physically punished at school leads a child to perform worse academically.

2.2 Background

Heckman et al. (2009) emphasize the importance of growing up in a well-balanced environment where children can be nurtured properly. However, if a child is being physically punished, either in school or at home, that may negatively impact the development process with potentially long term consequences.

Corporal punishment can have severe consequences on young children both in the short and long run. The majority of European countries and many US states have therefore banned corporal punishment in schools. There are numerous arguments against the implementation of corporal punishment either at school or at home. A key argument against corporal punishment is that it likely generates counter-productive results, that is, it promotes the exact same effects that it is supposed to prevent. Several opponents of corporal punishment have argued in favor of more humane and effective methods to replace corporal punishment (Radin, 1988).

Hyman and Perone (1998) define corporal punishment as purposeful application of pain or confinement by teachers or officials on students. This can lead to delinquent behavior on the part of the punished children. Corporal punishment is also viewed as generating anti-authority views in the minds of physically punished children and this can be a source of alienation and future psychological problems. It has also been argued that educator-induced corporal punishment of students cannot solve the problem of lack of discipline on the part of a student and often acts as a very poor motivational technique.

Youssef et al. (1998) provide evidence from an Egyptian dataset that physical punishment is used as a method to bring student behavior and test performance to the desired range of a particular school. Also, male students in preparatory (middle) schools are more likely to receive corporal punishment than students in secondary (high) schools.

Radin (1988) traces corporal punishment at schools back to the first century in Rome. Corporal punishment leads students to believe that violence is an acceptable form of conflict resolution. Instead of acting as a motivational tool, physical punishment can make the affected students more resigned and less concentrated on learning. Radin goes on to propose several alternatives for corporal punishment, such as in-school suspension, timeout procedures, transfer to an alternative school, behavior contracting, use of peers, use of parents, and social skills training. These methods, when properly applied, can tackle the same problems that physical punishment aims to solve, and they can also be more conducive to student learning and long term character building.

Straus (1971) provides a linkage theory explanation behind the use of corporal punishment by parents. Similar to the previously discussed literature, corporal punishment is argued to be counter-productive and instead of controlling an unwanted behavioral trait of a child, it merely exacerbates it. Higher exposure to physical punishment during childhood leads to more aggression during adulthood, and lower exposure to physical punishment during childhood leads to "stronger internalized moral standards". Working class parents and middle class parents both use this form of punishment and its application is closely related to what kind of situation the child will likely be in after growing up. Male children are physically punished more often than female children, as the boys are perceived by parents to be more likely to encounter physical violence. Parents valuing obedience more than self-sufficiency tend to use corporal punishment more often and this in turn creates a vicious cycle where the physically punished child grows up lacking self-confidence and being over-dependent.

Noguera (2003) argues that corporal punishment is often meted out to those students who have the greatest need for a supportive environment. Instead of taking care of their needs and helping them overcome hardships in life, corporal punishment succeeds in further alienating them and teaches them to take the easy way out using violence and other socially unacceptable actions. There is also significant evidence that children from minority communities often receive corporal punishment more often.

Lytton (1997) argues that, for children who need to behave within the socially acceptable norms, physical punishment will likely keep them under control. However, corporal punishment does not need to be the one and only motivation for them to behave well. For children with behavioral problems, corporal punishment leads to more problems and solves none.

Gershoff (2002) finds that younger parents are more prone to use corporal punishment on their children. Also, mothers are more likely to use physical punishment to bring an unruly child under control. She also finds that being punished by parents leads children to imitate that behavior while facing a conflict.

Ripoll-Nunez and Rohner (2006) cite several meta-analyses and find that data from the United States reveals no impact of parental corporal punishment on academic outcomes, suicidal thoughts, or violent thoughts.

In the context of school participation in India, Dreze and Kingdon (2001) show that school participation depends on a variety of factors such as family resources, parents' motivation, and returns from work for the children. However, school quality, which influences school participation, cannot be easily measured and is likely multi-dimensional. Infrastructure may be important for one school's quality, but day-to-day functioning may be important for another school's overall performance. Caste-based discrimination is also evident in case of scheduled-caste students. Mid-day meal schemes, which are devised in order to bring more children from the poorer section of the Indian society to schools, also have a significant impact on school participation.

Chetty et al. (2011) show that intervention at schools as early as kindergarten has a significant impact on adult outcomes. They find that kindergarten test scores are good predictors of college attendance, adult earnings, home ownership, and retirement savings. Hence, the effects of corporal punishment at schools can also be expected to affect a child's adult outcomes.

From our discussion so far, it clearly appears that parental application of physical punishment on a child is not desirable and it may lead to more problems in the child's later life. So far, research in this field has not investigated the effects of corporal punishment at schools on early childhood outcomes. Corporal punishment at schools may be considered different from corporal punishment at home, since the former has magnified public shame and peer stigma attached to it. To bridge this gap in research in this field, this study investigates the effects of being physically punished at schools on early educational outcomes.

2.3 Analysis

2.3.1 Methodology

In this paper, I intend to identify the impact of corporal punishment on young children's cognitive development. The hypothesis is that corporal punishment can have two opposite effects on a child. On the positive side, a child may get motivated to become disciplined and study well in order to avoid being punished in school. On the negative side, corporal punishment may make a child more aggressive and take the focus away from education. Corporal punishment may be used by a teacher for various reasons. Corporal punishment can be used as an instrument for disciplining a child as well as to force the child to try to do better academically. As the literature suggests, corporal punishment can lead to behavioral problems in a child, which in turn may result in worse academic performance. However, if corporal punishment is successful as a deterrent to anti-social behavior, then it may act as a catalyst for better academic outcome. Thus, the overall effect of corporal punishment on a young adult's academic outcomes is ambiguous. Hence, we cannot find the overall effect of corporal punishment on a child's academic outcomes by theory alone. In order to comment on the comprehensive impact of corporal punishment on a young adult's academic outcomes we need to conduct an empirical investigation using econometric methods.

In this study, I investigate the effect of corporal punishment on two measures of academic outcomes: a mathematics test score and the EGRA (Early Grade Reading Assessment) test score. These two test scores are obtained from the *Young Lives* dataset, along with other demographic and regional information. My main explanatory variable identifies whether a child received corporal punishment from a teacher and to what extent. The variable is called *physical punishment* and can take on three values: 0 (never physically punished at school), 1 (seldom physically punished at school), and 2 (physically punished most of the times, at school). The mathematics test score can vary between 0 to 29, and the EGRA test score can vary between 0 to 14.

The initial model can be specified as:

$$test\,score = f\left(motivation\left(physical\,punishment\right), stress\left(physical\,punishment\right)\right), \tag{2.1}$$

where the child's test score depends on whether the child is being physically punished at school. Here, physical punishment has two opposite effects on a child. On the positive side, being physically punished may motivate a child to perform well academically in order to get on the good side of a teacher. On the negative side, physical punishment may make a child more delinquent, reinforcing bad behavior. To identify the net effect of a unit change in corporal punishment on test score in Equation 1, we need to calculate the partial derivative:

$$\begin{aligned}\frac{\delta test\,score}{\delta physical\,punishment} &= \frac{\delta f\left(\right)}{\delta motivation\left(\right)}\frac{\delta motivation\left(\right)}{\delta physical\,punishment} \\ &+ \frac{\delta f\left(\right)}{\delta stress\left(\right)}\frac{\delta stress\left(\right)}{\delta physical\,punishment}\end{aligned}$$

where the first term on the right hand side is positive and the second term is negative, which makes the overall impact ambiguous.

We can proceed further by writing the initial model in a linear format as:

$$test\,score_i = \beta_0 + \beta_1 physical\,punishment_i + \gamma X_i + \epsilon_i, \tag{2.2}$$

where γ is a vector, and X is the set of controls that may also influence test scores.

The model contained in equation (2) may suffer from endogeneity issues despite a significant number of control variables, including a child's age, health, IQ, type of school the child attends, whether the child is being bullied at school, household wealth, child's caste and religion, and region identifiers. There may be some important variables not included among the controls that affect both the academic outcomes and the corporal punishment a child receives at school. This omitted variables bias can be a source of endogeneity. For example, if a child is coming from an unstable household, the child may be performing poorly in school while also being less conforming to the rules at school. Both behavior patterns make the child more likely to receive corporal punishment. Endogeneity can also arise from a simultaneity bias. If teachers apply corporal punishment to make the students perform better academically, then a vicious cycle may be created where a student receives corporal punishment for not doing well at school and that makes her perform even more poorly due to the likely adverse psychological impact of corporal punishment. To address the endogeneity issues, I employ an instrumental variables approach.

The instrument used in this analysis is a variable measuring to what extent other students in a child's school are also being physically punished, which is represented by the variable *physical punishment on others*. In order to be a good instrument it needs to be a good predictor of the endogeneous explanatory variable *physical punishment*, and it must be uncorrelated to omitted variables affecting the dependent variable (Angrist and Krueger, 2001; French and Popovici, 2011; Imbens, 2014). These conditions are very likely met by our instrument. We can expect it to be directly related to the corporal punishment of a student but not related to the academic outcomes of the student. If a teacher is prone to using corporal punishment then it may be used quite indiscriminately, and all students are likely at risk of receiving it. Empirically, *physical punishment on others* is in fact a good predictor of *physical punishment*. Since we are controlling for a significant number of variables that explain academic outcomes, most influences that affect test scores are being factored in the model. Hence, it is unlikely that the instrument will be correlated with any omitted variable influencing the outcome variables.

2.3.2 Data

For the empirical analysis, I am relying on the *Young Lives* dataset. *Young Lives* is a longitudinal dataset consisting of demographic data for about 12,000 children in India, Ethiopia, Peru, and Vietnam (Kumra, 2008; Morrow and Singh, 2014). The dataset keeps track of two cohorts of children, an older cohort and a younger cohort. The older cohort consists of 1,000 children who were aged between 7.5 to 8.5 years in 2002. The younger cohort consists of 2,000 children who were aged between 6 to 18 months in 2002. This study is initially intended to be conducted for 15 years and 5 waves. So far, three waves have been conducted in the years 2002, 2005, and 2009. The main aim of this dataset is to analyze the causes of childhood poverty.

For India, the *Young Lives* team collects data from the state of Andhra Pradesh. For data collection, the *Young Lives* study follows a method called sentinel site surveillance system. The survey sites (sentinel sites) are selected on the basis of pre-determined criteria, and households are selected randomly within the sentinel sites. The sites come from three distinct agro-climatic regions within the state of Andhra Pradesh. The sites are chosen in such a manner that they well-represent the regional and urban/rural variation. Since the aim of the *Young Lives* dataset is to study childhood poverty, the relatively poor sites are oversampled.

Kumra (2008) uses the 1998-99 Demographic and Health Survey (DHS), a nationally representative dataset, to assess the quality of *Young Lives* data. Using data for Andhra Pradesh, for the households having at least one child of the age between 6 months to 18 months, she finds that the *Young Lives* dataset has similar variation as the more nationally representative DHS dataset and can therefore be well used for the purpose of causal inference.

I am primarily working with the 2009 India wave of the *Young Lives* dataset, but also take the younger cohort into account. In particular, I bring some variables in from the earlier waves in case they are not measured in the 2009 wave. Since the children in the younger cohort of the *Young Lives* data were 6 months to 18 months of age in 2002, in 2009 they are approximately between 7.5 years to 8.5 years.

A brief description of the variables used in this analysis are given, in Table 1. Table 2 provides summary statistics for the variables.

The two dependent variables of interest are the scores obtained by a child in the mathematics and the EGRA tests. These are my measures of a child's academic progress. Both of these tests are administered by the interviewer at the time of the interview. The mathematics test measures the numerical problem solving ability of a child. The EGRA tests for reading and oral comprehension, and measures a child's verbal communication ability. Both tests are measures of a child's cognitive ability. For this analysis, I use the corrected versions of the two test scores. The raw scores are corrected for poor psychometric results.

The main independent variable of interest is whether a child received corporal punishment from a teacher and to what extent; this is the variable identified as *physical punishment*. The child can answer the corporal punishment question in three possible ways: never, once or twice, most/all of the time. This variable gives us a measure of whether the child was being physically punished in school and to what extent. The main purpose of this study is to find if this measure of corporal punishment in schools has any significant impact on a child's cognitive development and to what extent.

Since the independent variable is likely to be endogenous, as I discussed in the previous section, I use an instrument to account for the endogeneity issue. My instrument of choice is the variable *physical punishment on others*. The variable *physical punishment on others* measures whether the child in question saw any other other student being physically punished. A child can answer in three ways: never, once or twice, most/all of the time

I include several control variables. The children in the younger cohort are between 7.5 years to 8.5 years of age in 2009, which are not far apart. However, I include age in months as one of my controls because this is one of the most vital times for a child to develop cognitive abilities. Even a year can make a large difference at this age and therefore the failure to control for age may bias the results. I also have two child health indicators in my set of controls: *child health* and *BMI*. The *child health* variable measures how healthy the survey respondent thinks the child is. A respondent can rank a child's health on a scale of one to five, one identifying poor and five very good health. The variable *BMI* measures the body mass index index of a child. Body mass index is calculated as the ratio between a child's weight and height squared.

To control for a child's innate ability, I include the *past Peabody Picture and Vocabulary Test* score from the 2005 wave. PPVT is a standard measure of a child's IQ, and should remove ability bias from my study.

The type of school a child attends, be it public or private, may also influence how much cognitive ability a child develops. I control for it with a dummy variable (*private*) that is one if the child attends a private school, and zero otherwise. It may be possible that in private schools children are taught better, or corporal punishment is applied less often. This dummy variable also takes into account another important factor, the provision of mid-day meals. In India, in order to boost school participation, a scheme called mid-day meal has been introduced in the public school system.

There is evidence that this has boosted school participation among Indian children, especially for female children (Dreze and Kingdon, 2001). However, a mid-day meal is provided mostly in public schools, and almost never in private schools. So, the *private* dummy variable also controls for the availability of mid-day meals at school.

There is also a need to control for school quality and teacher quality. Unfortunately, the available data do not easily permit that. I use several proxies to try to overcome this problem. Firstly, I use a respondent's answer to the question of why a child is being sent to a particular school to construct my first proxy (dummy) variable for school and teacher quality. The respondent can choose three reasons, ranked in the order of preference to specify why a particular school is selected. If "good quality teaching and care" is either the first or second choice, then my dummy variable *good school* is set to one. I also create dummy variables for a child's *region of residence* and *sentinel sites*. Region of residence and sentinel site indicator variables are included to account for area specific differences. As discussed before, these area specific dummies will also account for school and teacher quality, albeit partially. Approximately one hundred children are from each sentinel site in the *Young Lives* data. Hence, controlling for the type of school, the region of residence, and the sentinel site, should at least partially control for teacher and school qualities. Again, this is not the best way to account for school and teacher quality, but given the data available this is best that can be done.

Another important factor in a child's cognitive development, especially during an early stage is bullying. Being bullied in school can often lead to fear and frustration on the part of a bullied child and this is likely to have an adverse impact on the cognitive development of a child. To control for this, I employ the variable *being bullied*.

A child's ability to develop cognitive skills during early childhood will also depend on the resources available to a household. If a household has access to more resources, then more can be invested in a child in the form of better food and study materials, among other things. To control for this, I use an index constructed by the *Young Lives* team. The *wealth index* (wi) is supposed to capture the overall access to resources for any particular household.

I also include controls for the mother's education, the child's caste, and the child's religion. A better educated mother is more likely to encourage her children to get better educated. Controlling for a child caste is important since a possible source of bias can arise from it. If a child from a lower caste is more likely to be punished physically and is also likely to have fewer opportunities to develop cognitive skills due to fewer resources, then a failure to account for caste will bias our results. A similar argument can be presented for including the religion dummy in our analysis.

2.4 Results

As discussed before, I estimate the effect of corporal punishment using two regression techniques: ordinary least squares (OLS) and instrumental variables (IV). The regressions are carried out for both dependent variables, the mathematics score and the EGRA score. The results are given in Tables 3 and 4.

There is statistically significant evidence from our OLS regressions that corporal punishment at school is negatively affecting a young adult's academic performance. In Table 3 (column 1) we find that the application of corporal punishment at school leads to a lower mathematics test score when a child receives corporal punishment. This result is statistically significant. Throughout this analysis, we find that corporal punishment negatively impacts a child's academic performance. There is no evidence of corporal punishment at school acting as a positive catalyst in a child's educational outcome. The coefficient of -0.48 indicates that for a unit rise in the variable measuring the extent of corporal punishment at school (*physical punishment*), the mathematics test score decreases by 0.48 (approximately 4% of the sample mean).

The IV model generates the same direction of change for corporal punishment. In column 3, we find that if a child receives physical punishment in school, that child's mathematics score falls by 0.19 (2% of the sample mean). However, the IV result is no longer statistically significant.

In Table 4, there are similar results for the EGRA (Early Grade Reading Assessment) test scores. The difference is that the results from the ordinary least squares models (columns 1 and 2) and the instrumental variables models (columns) both show that corporal punishment at school leads to lower test scores. The OLS regression results find that physical punishment lowers the EGRA test score by 0.37 (7% of the sample mean), while the IV regression results show that corporal punishment lowers EGRA test score by 0.39. Both these results are statistically significant.

We also find that if a child in a household has access to better resources, then the child is more likely to get higher scores in the two tests. This result is also statistically significant across specifications. Being bullied also lowers test scores, although this result is not statistically significant. Another important finding in this study is the effects of a child's health on test scores; being healthy approximately raises the mathematics test score by 0.66 and EGRA test score by 0.26, and both these results are statistically significant across OLS and IV model specifications.

As a robustness check (columns 2 and 4), I redo the analysis controlling for how a child views the outcome of having a good education. A child's cognitive development might be influenced by the outlook a child has regarding education. This outlook in turn will be conditioned by the child's family and other environmental factors. If a child thinks that doing well in school leads to a better life in the future, then the child is more likely to put extra effort into developing her cognitive skills. In order to control for this, I use a variable *child education outlook*. To construct this variable, a child is asked about the likelihood of getting a better job as an outcome of studying well in school. Based on a child's response from very small likelihood to very high likelihood, the variables can take values from 1 to 5. In our analysis, we find the results in columns 1 and 3 to approximately retain their magnitude and sign even in the modified specification.

As a further robustness check, I rerun the analysis with *physical punishment* and *physical punishment on others* recoded as binary variables, that is if a child is never punished, then the recoded variable for *physical punishment* becomes zero, and one otherwise, and if a child never sees any other child being physically punished, then the recoded variable for *physical punishment on others* becomes zero, and one otherwise. The qualitative results in earlier analysis still hold after recoding the variables.

2.5 Conclusion

Corporal punishment is a debated topic among parents, educators, and policymakers. The opponents and proponents often argue about the potential benefits (better disciplined children, for example) and the potential harmful effects (e.g., adverse psychological impact on a child) of corporal punishment, both at school and at home. However, the empirical evidence is sparse to come by. This paper uses a unique data from India to examine the impact of corporal punishment on a young adult's academic performance. The issue of endogeneity is tackled through an instrumental variables method. This study finds that there is a statistically significant negative impact of corporal punishment at school on a child's academic performance. In our baseline model, this applies in particular to a child's verbal and reading comprehension. The impact on the numerical abilities of a child is also negative, but not uniformly as significant as that for verbal and reading comprehension.

The results from this study show that corporal punishment at schools is counterproductive for the well-being of a child. Hence, stricter measures appear in order to ban corporal punishment at schools in India. It is not only necessary to have laws in place to stop the usage of corporal punishment at schools, but also to enforce them properly.

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Table 2.1: Data Description

Variable	Description
Math Score	Measures the numerical ability of the child
EGRA Score	Measures verbal and reading comprehension
Physical Punishment	Whether the child received corporal punishment in school
Physical Punishment on Others	Whether other children received corporal punishment
Age	Child's age in months
Child Health	How healthy is the child
BMI	Child's BMI
Past PPVT Score	Measure of Child's IQ
Private School	Whether the school is a private one
Good School	The school has good quality teachers and staff
Being Bullied	Whether the child is being bullied in school
Wealth Index	Relative wealth position of the household
Urban Household	Whether the child lives in an urban area
Gender	Gender of the child
Mother's Education	Mothers level of education
Child's Caste	Caste of the childhood
Child's Religion	Religion of the childhood
Region of Residence	Child's region of residence
Sentinel Site	Sentinel site identifier
Child Education Outlook	If child thinks that doing well in school leads to better job

Note: Data Description

Table 2.2: Summary Statistics

	Obs.	Mean	Std.Dev.	Min.	Max.
Dependent Variables					
Math Score	1715	12.24	6.38	0	29
EGRA Score	1715	5.53	3.38	0	14
Independent Variable					
Physical Punishment	1715	1.04	0.70	0	2
Controls					
Age (Months)	1715	95.49	3.83	86	106
Child Health	1715	3.95	0.65	1	5
BMI	1715	13.93	1.64	4.58	41.36
Past PPVT Score	1715	28.00	21.58	3	119
Private School	1715	0.45	0.50	0	1
Good School	1715	0.43	0.50	0	1
Being Bullied	1715	0.44	0.50	0	1
Wealth Index	1715	0.52	0.18	0.01	0.95
Urban Household	1715	0.25	0.43	0	1
Gender	1715	0.53	0.50	0	1
Child Education Outlook	1715	4.32	0.66	1	5
Instrument					
Physical Punishment on Others	1715	1.44	0.63	0	2

Note: Other than the specified controls, dummies for mother's education, child's caste, child's religion, region of residence, and sentinel site are also used in the analysis.

Table 2.3: Effect of Corporal Punishment on Mathematics Test Score

Dependent Variable: Math Score				
	OLS		IV	
	(1)	(2)	(3)	(4)
Physical Punishment	-0.48 (0.19) ***	-0.48 (0.19)***	-0.19 (0.36)	-0.26 (0.35)
Age (Months)	0.20 (0.03) ***	0.19 (0.03) ***	0.20 (0.03) ***	0.19 (0.03) ***
Child Health	0.66 (0.20) ***	0.62 (0.20) ***	0.66 (0.20) ***	0.62 (0.20) ***
BMI	0.16 (0.08) **	0.17 (0.08) **	0.17 (0.08) **	0.18 (0.08) **
Past PPVT Score	0.06 (0.01) ***	0.06 (0.01) ***	0.06 (0.01) ***	0.06 (0.01) ***
Private School	-0.84 (0.38) **	-0.96 (0.38) ***	-0.83 (0.39) **	-0.95 (0.39) **
Good School	0.56 (0.34) *	0.64 (0.34) *	0.57 (0.35) *	0.65 (0.35) *
Being Bullied	-0.05 (0.28)	-0.16 (0.28)	-0.06 (0.28)	-0.16 (0.28)
Wealth Index	6.40 (1.09) ***	6.30 (1.08) ***	6.43 (1.06) ***	6.32 (1.06) ***
Urban Household	0.66 (1.20)	0.59 (1.21)	0.65 (1.07)	0.58 (1.07)
Gender	0.34 (0.26)	0.31 (0.26)	0.29 (0.26)	0.27 (0.26)
Child Education Outlook	×	0.82 (0.20) ***	×	0.82 (0.19) ***
R-squared	0.37	0.38	0.37	0.34
No. of Observations	1715	1715	1715	1715

Note: Standard errors are given in the parentheses.
Other Controls: Indicator variables for mother's education,
child's caste, child's religion, region of residence, and sentinel site.
*, **, *** significant at 10%, 5%, and 1% respectively

Table 2.4: Effect of Corporal Punishment on EGRA Test Score

Dependent Variable: EGRA Score	OLS		IV	
	(1)	(2)	(3)	(4)
Physical Punishment	-0.37 (0.11) ***	-0.38 (0.11) ***	-0.39 (0.20) **	-0.43 (0.20) **
Age (Months)	0.07 (0.02) ***	0.06 (0.02) ***	0.07 (0.02) ***	0.06 (0.02) ***
Child Health	0.26 (0.11) **	0.24 (0.11) **	0.26 (0.11) **	0.24 (0.11) **
BMI	0.05 (0.05)	0.05 (0.05)	0.05 (0.04)	0.05 (0.04)
Past PPVT Score	0.03 (0.01) ***	0.03 (0.01) ***	0.03 (0.01) ***	0.03 (0.01) ***
Private School	-0.54 (0.22) ***	-0.60 (0.22) ***	-0.54 (0.22) **	-0.60 (0.22) ***
Good School	-0.04 (0.20)	-0.01 (0.20)	-0.04 (0.20)	-0.01 (0.20)
Being Bullied	-0.16 (0.16)	-0.22 (0.16)	-0.16 (0.16)	-0.21 (0.16)
Wealth Index	1.77 (0.59) ***	1.72 (0.59) ***	1.77 (0.60) ***	1.72 (0.60) ***
Urban Household	-0.20 (0.63)	-0.23 (0.64)	-0.20 (0.60)	-0.23 (0.60)
Gender	0.14 (0.14)	0.12 (0.15)	0.14 (0.15)	0.13 (0.15)
Child Education Outlook	×	0.39 (0.10) ***	×	0.39 (0.11) ***
R-squared	0.29	0.30	0.29	0.30
No. of Observations	1715	1715	1715	1715

Note: Standard errors are given in the parentheses.
Other Controls: Indicator variables for mother's education,
child's caste, child's religion, region of residence, and sentinel site.
*, **, *** significant at 10%, 5%, and 1% respectively

Table 2.5: Robustness Check: Effect of Corporal Punishment on Mathematics Test Score

Dependent Variable: Math Score	OLS		IV	
	(1)	(2)	(3)	(4)
Physical Punishment	-0.70 (0.31) **	-0.68 (0.31) **	-0.12 (0.68)	-0.24 (0.68)
Age (Months)	0.20 (0.03) ***	0.19 (0.03) ***	0.20 (0.03) ***	0.19 (0.03) ***
Child Health	0.65 (0.20) ***	0.61 (0.20) ***	0.66 (0.20) ***	0.62 (0.20) ***
BMI	0.16 (0.08) **	0.17 (0.08) **	0.17 (0.08) **	0.18 (0.08) **
Past PPVT Score	0.06 (0.01) ***	0.06 (0.01) ***	0.06 (0.01) ***	0.06 (0.01) ***
Private School	-0.82 (0.38) **	-0.94 (0.38) ***	-0.82 (0.39) **	-0.94 (0.39) **
Good School	0.55 (0.34)	0.62 (0.34) *	0.57 (0.35)	0.64 (0.35) *
Being Bullied	-0.02 (0.28)	-0.13 (0.28)	-0.05 (0.28)	-0.16 (0.28)
Wealth Index	6.45 (1.09) ***	6.35 (1.08) ***	6.46 (1.06) ***	6.35 (1.06) ***
Urban Household	0.70 (1.20)	0.63 (1.20)	0.65 (1.08)	0.59 (1.07)
Gender	0.34 (0.26)	0.31 (0.26)	0.27 (0.27)	0.25 (0.27)
Child Education Outlook	×	0.81 (0.20) ***	×	0.81 (0.19) ***
R-squared	0.38	0.38	0.37	0.38
No. of Observations	1715	1715	1715	1715

Note: Physical Punishment, Others' Physical Punishment are binary variables in this model
Standard errors are given in the parentheses. Other Controls: Indicator variables
for mother's education, child's caste, child's religion, region of residence, and sentinel site.
*, **, *** significant at 10%, 5%, and 1% respectively

Table 2.6: Robustness Check: Effect of Corporal Punishment on EGRA Test Score

Dependent Variable: EGRA Score				
	OLS		IV	
	(1)	(2)	(3)	(4)
Physical Punishment	-0.65 (0.18) ***	-0.64 (0.18) ***	-0.55 (0.38)	-0.61 (0.38)
Age (Months)	0.07 (0.02) ***	0.06 (0.02) ***	0.07 (0.02) ***	0.06 (0.02) ***
Child Health	0.25 (0.11) **	0.23 (0.11) **	0.25 (0.11) **	0.23 (0.11) **
BMI	0.05 (0.05)	0.05 (0.05)	0.05 (0.04)	0.05 (0.04)
Past PPVT Score	0.03 (0.01) ***	0.03 (0.01) ***	0.03 (0.01) ***	0.03 (0.01) ***
Private School	-0.53 (0.22) **	-0.58 (0.22) ***	-0.53 (0.22) **	-0.58 (0.22) ***
Good School	-0.06 (0.20)	-0.02 (0.20)	-0.06 (0.20)	-0.02 (0.20)
Being Bullied	-0.14 (0.16)	-0.18 (0.16)	-0.14 (0.16)	-0.19 (0.16)
Wealth Index	1.81 (0.59) ***	1.76 (0.59) ***	1.81 (0.60) ***	1.76 (0.60) ***
Urban Household	-0.16 (0.63)	-0.19 (0.65)	-0.17 (0.60)	-0.20 (0.60)
Gender	0.15 (0.15)	0.14 (0.15)	0.14 (0.16)	0.13 (0.150
Child Education Outlook	×	0.38 (0.10) ***	×	0.38 (0.11) ***
R-squared	0.29	0.30	0.29	0.30
No. of Observations	1715	1715	1715	1715

Note: Physical Punishment, Others' Physical Punishment are binary variables in this model
Standard errors are given in the parentheses. Other Controls: Indicator variables for mother's education, child's caste, child's religion, region of residence, and sentinel site.
*, **, *** significant at 10%, 5%, and 1% respectively

Chapter 3

Regional Variations in Labor Demand Elasticities: Evidence from U.S. Counties

(with Debarshi Indra)

3.1 Introduction

The estimation of wage elasticities of labor demand has attracted significant attention in empirical labor economics. Hamermesh (1993) provides an exhaustive review of the early research that has been done in this area. According to Hamermesh (1993), the absolute value of the constant-output wage elasticity of labor demand for homogeneous labor in the U.S. is between 0.15 and 0.75, with 0.30 being an approximate mean; the absolute value of the estimates for the total wage elasticity of labor demand vary between 0.12 and 1.92. Homogeneous labor implies that we cannot distinguish workers based on their skill level.

Fuchs et al. (1998) survey sixty five labor economists and confirm Hamermesh's findings. They report mean absolute values for constant-output and total wage elasticity of labor demand equal to 0.42 and 0.63, respectively. More recently, Slaughter (2001), using the NBER productivity database, estimates absolute values of the total wage elasticity of labor demand for the manufacturing sector in the U.S. in the range of 0.24 to 0.70. Hasan et al. (2007), using small industry panel data, estimate the absolute value of the total wage elasticity of labor demand in India's manufacturing sector to be around 0.40. In Table 3.1 we provide a list of studies that estimate total wage elasticities of labor demand from a variety of different data sets.

Most studies cited in Table 3.1 estimate wage elasticities of labor demand for one sector or industry, in particular the manufacturing sector, and assume no regional variation in the wage elasticity of labor demand. While regional variation in the labor demand elasticity may be safely neglected for smaller countries such as New Zealand, for a large country, such as the United States, this may not be a reasonable assumption. In the U.S., for example, history, geography, and politics vary considerably across counties, and all these factors are likely to induce regional variation in the wage elasticity of labor demand. Using the County Business Patterns (CBP), we address the issues of regional and industry heterogeneity for labor demand estimates. In particular, we estimate county specific labor demand elasticities for multiple industries located in the U.S. This makes our study unique in the empirical labor demand literature.

Our use of a single data source makes comparing elasticities across industries easier than comparing elasticities from different studies that vary in methodology and data. Our elasticity estimates can therefore be used to calibrate local labor markets that may be part of larger regional economic models. These models can be used, for example, to study how external shocks might have asymmetric effects on different local labor markets based in part on variations in their labor demand elasticities.

To obtain county specific total wage elasticities of labor demand we follow a two-step procedure. In step-one, we specify a canonical log linear labor demand function. Then we use the traditional first-difference panel data estimator to get the following estimates for the absolute values of industry specific total wage elasticities of labor demand: 0.32 for construction, 0.11 for finance-insurance-real estate-service, 0.23 for manufacturing, and 0.23 for retail. Our industry specific total labor demand elasticities fall within the range mentioned in Hamermesh (1993).

In step-two, we assume that the total wage elasticity of labor demand for an industry is not a constant but a random variable, distributed log-normally in the population of counties with unknown parameters. The log-normal distribution ensures that the absolute value of the labor demand elasticity is always positive. We then estimate the parameters of the log normal distribution by the method of maximum simulated likelihood.

The means and standard deviations of the log-normal distribution for the four industries are as follows: (0.08, 0.01) for construction, (0.34, 3.26) for finance-insurance-real estate-service, (0.38, 3.97) for manufacturing, and (0.35, 0.98) for retail trade. For all four industries, the variance parameter is statistically significant, which suggests the presence of regional variation in the total labor demand elasticity. In addition, the means of the labor demand elasticity distributions all fall within the range mentioned in the literature. Our results are also in line with evidence by Revelt and Train (1998) that treating a parameter as a random variable usually increases its mean estimate; this can be seen by comparing the elasticity estimates from step one and step two. An exception is the construction sector.

Once we have information regarding the distributions of the wage elasticities of labor demand, it is possible to get elasticity estimates for each county. Using these estimates we find evidence of a negative relationship between the total wage elasticity of labor demand and the incidence of union membership among workers. This result makes intuitive sense since unions probably make firms less flexible in hiring and firing workers thereby driving down labor demand elasticities. We also find that the presence of a right to work law makes labor demand more elastic. This is also consistent with intuition since a right to work law will reduce the influence of unions at the workplace and firms will become more flexible in their hiring and firing decisions.

The paper proceeds as follows. In section 2 we discuss briefly the theory behind the labor demand function. In section 3 we describe the dataset. In sections 4 and 5 we present the results from the linear and random parameter panel data models . In section 6 we explain how we obtain county specific labor demand elasticity estimates. In section 7 we discuss the relationship between labor demand elasticity and union membership. Finally, in section 8 we conclude by pointing to some applications and possible extensions of our work.

3.2 Theory

Following Hamermesh (1993), the total industry labor demand elasticity (η'_{LL}) can be written as,

$$\frac{\delta \ln L\,(w, Y)}{\delta \ln w} = \eta'_{LL} = -\underbrace{[1 - s_L]\sigma}_{substitution\,effect} - \underbrace{s_L \eta_D}_{scale\,effect} \tag{3.1}$$

where, s_L is the share of labor in total revenue, σ is the elasticity of substitution, and η_D is the own-price elasticity of demand for the industry, L is the quantity of labor demanded, w is the wage rate, and Y is output.

The first part of the total labor demand elasticity can be interpreted as the constant-output labor demand elasticity, or the "substitution effect". As the price of labor rises, firms substitute away from labor in favor of other inputs. The substitution effect captures this adjustment on the profit maximizing firm's part. The higher the substitutability of labor with respect to other factors of production, the larger is the constant-output labor demand elasticity. The second term captures the "scale effect". As the cost of hiring labor rises, output price increases, which in turn lowers the demand for the industry's output, and hence lowers the industry's labor demand. Hence, the total labor demand elasticity can be viewed as the weighted average of the constant-output labor demand elasticity and the own-price product demand elasticity.

As Hamermesh (1993), Slaughter (2001), and Hasan et al. (2007) point out, the choice of Y will determine whether we are estimating the constant output labor demand elasticity or the total own price labor demand elasticity. If the measure of output embodies the overall industry demand conditions, then we will be estimating the total labor demand elasticity.

3.3 Data

We use the County Business Patterns (CBP) data set to get data from 1988 to 2010 on the number of establishments, total mid-March employees, and total first quarter payroll by industry for counties in the conterminous U.S. In our dataset an observation refers to an industry-county-year combination.

According to the Census Bureau, in the CBP, "An establishment is a single physical location at which business is conducted or services or industrial operations are performed." An establishment is different from a company or enterprise in that a company might control multiple establishments. A company is controlled by a single organization. In the CBP, the Standard Industry Classification (SIC) system was used to categorize establishments by their primary activity for the period leading up to 1997. From 1998 onwards, the CBP switched to the North American Industry Classification System (NAICS). Even between 1998 and 2010 there were periodic changes made to the NAICS.

In the CBP, data are available at various industry aggregation levels. For this study, we use the 2-digit SIC and 2-digit NAICS industries to create four major industry groups: construction, finance-insurance-real estate-service, manufacturing, and retail. These four industries account on average for 87% of annual total employment in the sample. Table 2 provides our industry aggregation scheme.

Even at the 2-digit industry classification level the census bureau suppresses data for confidentiality reasons. In such cases the census bureau provides an interval for the industry employment level but sets payroll data equal to zero. Such data suppression causes an average annual loss of 1% of total workers in the sample spread across the different industries. Because of this small size we choose to drop observations subject to data suppression.

We calculate the industry wage rate by dividing first quarter payroll by the total number of mid-March employees. The exact formula is shown below. In our notation i, c, t denote industry, county and year, respectively, and s indexes the state in which the county is located,

$$w_{ict} = \left(\frac{CPI_{2010}}{CPI_t} \times \frac{Total\ First\ Quarter\ Payroll_{ict}}{Employees_{ict}} \right) \div 480 \tag{3.2}$$

where the division by 480 indicates that we assume that an average worker is employed for 480 hours during the first quarter, and CPI is the consumer price index series obtained from the Bureau of Labor Statistics (BLS).

We obtain state level industry GDP from the Bureau of Economic Analysis (BEA). We assume that a county's share in a state's industry GDP (SGDP) is proportional to the county's share in the total number of industry establishments located in that state. Using this assumption, we impute county industry GDP, which gives us a measure of industry demand conditions. The exact formula is shown below,

$$Y_{ict} = \left(\frac{PPI_{2010}}{PPI_t} \times \frac{Establishments_{ict}}{\sum\limits_c Establishments_{ict}} \times Gross\ State\ Product_{ist} \right) \tag{3.3}$$

where PPI is the producer price index obtained from the BLS.

In our sample the count of workers from all industries increased from 86,791,257 in 1988 to 108,831,971 in 2010, a growth of approximately 25%. In 1988, the distribution of workers among the different sectors was given as follows: construction 5%, finance-insurance-real estate-service 36%, manufacturing 22%, retail 21%, and others 16%. In the next 23 years the employment levels in the construction, finance-insurance-real estate-service, and retail sectors registered growth rates of 8%, 69%, and 47% respectively. The manufacturing sector during the same period experienced a fall in employment of around 46%. This means that in 2010 the distribution of workers among the different sectors was: construction 5%, finance-insurance-real estate-service 47%, manufacturing 9%, retail 25%, and others 13%. In other words, in the 23 year period the finance-insurance-real estate-service and retail sectors increased their share in total employment mainly at the expense of the manufacturing sector. During the same time period, real output of the construction, finance-insurance-real estate-service, manufacturing, and retail sectors grew by 27%, 133%, 2%, and 80%, respectively. This implies that even though the manufacturing sector lost workers, the remaining workers became more productive. Figures 3.8 and 3.8 present yearly values of total national employment and total national real output for the four industries.

The real wage rate ($/hour) in 1988 in the construction, finance-insurance-real estate-service, manufacturing, and retail sectors was 13.78, 11.85, 18.03, and 8.32, respectively. In 2010, the real wage rate in the construction, finance-insurance-real estate-service, manufacturing, and retail sectors increased to 15.72, 15, 19.87, and 8.60, respectively. This means that the real wage rate across the construction, finance-insurance-real estate-service, manufacturing, and retail sectors had growth rates of 14%, 26%, 10%, and 3%, respectively. Figure 3 shows yearly values of the real wage rate.

Table 3 presents some more descriptive statistics for the data at the county level. It reveals that on average the finance-insurance-real estate-service sector dominates county employment followed by the retail and manufacturing sectors. The construction sector employs on average the least number of workers in a county. Table 3 also shows that on average the wage rate is highest in the manufacturing sector and lowest in the retail sector. In fact, the retail wage rate is pretty close to the U.S. federal nominal minimum wage rate of $7.25.

Because of data suppression and natural changes in the employment distribution across counties we end up with an unbalanced panel data set. The construction, finance-insurance-real estate-service, manufacturing, and retail sectors are present in 3075, 3099, 2952, and 3106 distinct counties, respectively. However, only 2037, 2889, 1839, and 2857 counties appear every year in our dataset for the construction, finance-insurance-real estate-service, manufacturing, and retail sectors. The remaining counties appear infrequently.

3.4 Constant Parameter Panel Data Model

We denote industry, county and year by i, c, t , respectively, and s indexes the state in which the county is located. We specify the labor demand function following Hamermesh (1993), Slaughter (2001), and Hasan et al. (2007), as

$$\ln\left(L_{ict}\right) = \beta_{0is(c)t} + \beta_{1i}\ln\left(w_{ict}\right) + \beta_{2i}\ln\left(Y_{ict}\right) + \vartheta_{ic} + \varepsilon_{ict} \tag{3.4}$$

where L is employment, w the real wage rate, Y real output, ϑ a time invariant industry specific county fixed effects, and ε is the error term. $\beta_{0is(c)t}$ is a constant that varies by state and year. In the above specification β_{1i} gives the industry specific total wage elasticity of labor demand.

From a purely statistical viewpoint identification of the parameters in equation (4) requires that $\ln(w_{ict})$ and $\ln(Y_{ict})$ be uncorrelated with ϑ_{ic} and ε_{ict}. If this condition fails, we can still identify the parameters by first differencing equation 3.4 which gets rid of the time invariant county fixed effects. The first differenced version of the labor demand function is given in 3.5. Now, as long as $\Delta\ln(w_{ict})$ and $\Delta\ln(Y_{ict})$ are uncorrelated with $\Delta\varepsilon_{ict}$, we can use the OLS estimator to estimate the parameters $\Delta\beta_{0is(c)t}, \beta_{1i},$ *and* β_{2i} . First differencing also implies that the term $\Delta\varepsilon_{ict}$ is less likely to be serially correlated. Note that, by using 3.5 we cannot estimate the state specific trends, but only the change in the trends,

$$\triangle\ln(L_{ict}) = \triangle\beta_{0is(c)t} + \beta_{1i}\triangle\ln(w_{ict}) + \beta_{2i}\triangle\ln(Y_{ict}) + \triangle\varepsilon_{ict} \tag{3.5}$$

In economic terms, to claim that β_{1i} measures the total wage elasticity of labor demand we are in fact assuming that market labor supply is perfectly elastic. If this is not the case, then our model will suffer from simultaneity bias since market outcomes are determined by both demand and supply. We believe that a perfectly elastic labor supply is a reasonable assumption given that our unit of observation is an industry at the county level. Slaughter (2001) makes the same assumption in his time series study of 4-digit SIC national manufacturing industries. Slaughter (2001) argues that his industries are disaggregated enough to support his assumption, and points to the fact that almost all the studies cited in Hamermesh (1993) make a similar assumption regarding labor supply. Figure 3.8 presents our assumption regarding labor supply graphically.

In the labor demand equation, $\beta_{0is(c)t}$ captures the combined state level effects which may drive labor demand in the counties located in that state. For example, among other things, $\beta_{0is(c)t}$ may include state level labor market regulations. Moreover, by allowing the state level constant to vary over time, we can capture changes in such labor market regulations.

The estimation results are presented in Table 3.4, where we present models with and without $\beta_{0is(c)t}$. We report cluster robust standard errors, where clustering is done at the state level to account for possible correlation of employment across counties within a state (Dube et al., 2010). Based Table 3.4, the absolute values of the estimates of the total wage elasticity of labor demand for our four industries fall in the interval 0.11-0.32. This is consistent with the estimates presented in Hamermesh (1993). As specification 2 shows in Table 4, the wage elasticity of labor demand does not change much when we drop $\beta_{0is(c)t}$ from the labor demand equation but, as expected, the R^2 drops significantly. In both specifications we see that the construction sector has the highest labor demand elasticity, followed by manufacturing and retail trade. The finance-insurance-real estate-service sector has the lowest labor demand elasticity.

The coefficient for real output is positive and less than one across industries and specifications. We infer from Table 3.4 that the retail sector is the most sensitive to changes in output followed by the construction, finance-insurance-real estate-service, and manufacturing sectors.

3.5 Random Parameter Panel Data Model

In the labor demand equation presented in the previous section the coefficient of log wage, β_{1i}, is a constant. This means that there is no variation in the wage elasticity of labor demand across counties and/or over time. In the constant parameter linear panel data framework discussed earlier we cannot estimate a β_{1i} for each county-year combination, since then, the number of parameters to estimate will be greater than the number of observations in the data. To incorporate regional variation in the wage elasticity of labor demand across counties, we can estimate a β_{1i} for each county. The problem with this approach is that there is no guarantee that all the β_{1i} s' will have the correct sign.

An alternative approach to incorporate heterogeneity in the wage elasticity of labor demand across counties would be to interact $\ln(w_{ict})$ with some variable which we believe affects the wage elasticity of labor demand and which itself varies across counties. However, there are two drawbacks with this approach. One, since multiple factors may influence the wage elasticity of labor demand, the result will crucially depend on the choice of the interaction variables. Two, theory provides little guidance on the choice of the interaction variables.

We believe that a more robust approach is to assume that the parameter β_{1i} is a random variable. Under this approach, we cannot estimate β_{1i} , but we can estimate the parameters which describe the distribution of β_{1i} in the population of counties. In this paper, we assume for simplicity that β_{1i} varies over counties but not over time[1]. In equation 3.6, the log linear labor demand equation now includes β_{1ic} to incorporate heterogeneity in the wage elasticity of labor demand at the county level. We assume that β_{2i} is a constant.

$$\ln\left(L_{ict}\right) = \beta_{0i} - \beta_{1ic}\ln\left(w_{ict}\right) + \beta_{2i}\ln\left(Y_{ict}\right) + \vartheta_{ic} + \varepsilon_{ict} \tag{3.6}$$

As Table 3.4 shows, the results from the constant parameter linear panel data models are not greatly different with or without the inclusion of the state-year dummy interaction variables. Therefore, to simplify our estimation, we choose the log linear labor demand function without the state-year dummy interaction variables.

Again, first differencing removes the county fixed effects and yields the following equation,

$$\triangle\ln\left(L_{ict}\right) = -\beta_{1ic}\triangle\ln\left(w_{ict}\right) + \beta_{2i}\triangle\ln\left(Y_{ict}\right) + \triangle\varepsilon_{ict} \tag{3.7}$$

[1]To incorporate time variation in the wage elasticity of labor demand we could split the data into different time periods and estimate separate models for each time period.

We assume that the distribution of $\Delta\varepsilon_{ict}$ conditional on β_{1ic} , $\Delta\ln(w_{ict})$ and $\Delta\ln(Y_{ict})$ is i.i.d $N(0,\sigma^2_{(\varepsilon_i)})$. If the independence assumption for the error terms fails, our estimator is still consistent. However, the standard errors would need to be adjusted for serial correlation.

We assume that β_{1ic} is distributed i.i.d $\ln\left(N\left[\beta_{1i},\exp\left(\gamma_i\right)\right]\right)$ in the population of counties, where β_{1ic} and $\exp(\gamma_i)$ are the mean and variance of β_{1ic} 's natural logarithm. The log normal distribution assures that β_{1ic} is always positive. Note also that $\exp(\gamma_i)$ guarantees a positive value for the shape parameter of the log-normal distribution. The mean and variance of β_{1ic} are given by the following formulas,

$$\bar{\beta}_{1i} = E\left[\beta_{1ic}\right] = \exp\left[\beta_{1i} + \frac{\exp\left(\gamma_i\right)}{2}\right] \tag{3.8}$$

$$\sigma^2_{\beta_{1i}} = Var\left(\beta_{1ic}\right) = \left[\exp\left(\exp\left(\gamma_i\right)\right) - 1\right]\exp\left[2\beta_{1i} + \exp\left(\gamma_i\right)\right] \tag{3.9}$$

The log-likelihood function for the model is presented in Equation 10

$$\ln L\left(\theta_i\right) = \sum_c\sum_t \ln\left[\int_0^{+\infty} \phi\left(\triangle\varepsilon_{ict}\left(\beta_{1ic}\right)\right)\phi_{Ln}\left(\beta_{1ic}\right)d\beta_{1ic}\right] \tag{3.10}$$

where θ_i is the vector of parameters we estimate, ϕ is a normal density function with mean zero and variance $\sigma^2_{(\varepsilon_i)}$, and ϕ_{Ln} is a log-normal distribution with mean β_{1i} and variance $\exp(\gamma_i)$.

The log-likelihood function in equation 3.10 is evaluated by simulation since the integral in the log-likelihood function cannot be computed analytically. The simulation is performed as follows. Given θ_i , we draw a value for β_{1ic} from the log-normal distribution. The draws of β_{1ic} are independent across counties. We then compute the normal density ϕ_{ict} for that draw. We repeat this process R times and find the average ϕ_{ict} . The simulated log-likelihood function is,

$$\ln\ SLL(\theta_i) = \sum_c \sum_t \left(\frac{1}{R} \sum_r \phi_{ictr} \right) \tag{3.11}$$

where r indexes a draw from the log-normal distribution.

The simulated maximum likelihood estimator is the vector of parameters $\hat{\theta}_i$ that maximize the SLL function. To reduce our computational burden we set the values for β_{2i} and $\sigma^2_{(\varepsilon_i)}$ at those obtained from the linear panel data result presented in Table 4, where $\sigma^2_{(\varepsilon_i)}$ takes the value equal to the variance of the first difference residuals. Given that the number of draws (R) increases faster than $\sqrt{N}$ (the number of cross sectional units), the simulated maximum likelihood estimator retains all the properties of the traditional maximum likelihood estimator (Train, 2009). We use a sample of 1000 random draws for each county to simulate the log likelihood function. We then use the 'Nelder-Mead' algorithm to maximize the simulated log likelihood function[2]. The simulated maximum likelihood estimates are presented in Table 3.5.

Comparing Tables 3.4 and 3.5 we find that for all the industries except construction the random parameter model yields a higher value for the average wage elasticity of labor demand than the estimates from the linear panel data model. Our finding is consistent with Revelt and Train (1998), who conclude that the mean coefficients in a mixed logit model are consistently bigger than that the fixed coefficients from a standard logit model. This happens because the random parameter model explains some of the variation in the unobserved component of the linear panel data model which arises due to the randomness of the parameter.

Table 3.5 also shows statistically significant spatial variation in the labor demand elasticity. The manufacturing sector has the highest spatial variation, followed by finance-insurance-real estate-service, retail trade, and construction sectors.

[2]The 'Nelder-Mead' technique is a search algorithm which does not require computations of derivatives. Given the size of our dataset and the need for simulation in computing the integral, we choose the 'Nelder-Mead' algorithm over other commonly used algorithms.

3.6 County Specific Labor Demand Elasticity

In the previous section we presented estimates of the mean and standard deviation of the log normal distributions which describe the wage elasticity of labor demand for four different industries in the U.S. From these estimates we can calculate, for example, for every industry the proportion of counties which have a wage elasticity of labor demand greater than one. However, we can do better and estimate an average wage elasticity of labor demand for each county. We describe this procedure below based on Train (2009).

Consider Equation 3.12,

$$\hat{\phi}_{Ln}\left(\beta_{1ic}|\triangle\varepsilon_{ict}\right) \times f\left(\triangle\varepsilon_{ict}\right) = \phi\left(\triangle\varepsilon_{ict}|\beta_{1ic}\right) \times \phi_{Ln}\left(\beta_{1ic}\right), \tag{3.12}$$

which states that the joint density of β_{1ic} and $\triangle\varepsilon_{ict}$ can be written as the product of the probability of $\triangle\varepsilon_{ict}$ and the probability of β_{1ic} conditional on $\triangle\varepsilon_{ict}$ (left-hand side), or with the other direction of conditioning, as the product of the probability of β_{1ic} and the probability of $\triangle\varepsilon_{ict}$ conditional on β_{1ic} (right-hand side).

Rearranging equation 3.12 we get,

$$\hat{\phi}_{Ln}\left(\beta_{1ic}|\triangle\varepsilon_{ict}\right) = \frac{\phi\left(\triangle\varepsilon_{ict}|\beta_{1ic}\right) \times \phi_{Ln}\left(\beta_{1ic}\right)}{f\left(\triangle\varepsilon_{ict}\right)} \tag{3.13}$$

Note that the conditional probability of β_{1ic} will vary over the years because $\triangle\varepsilon_{ict}$ changes from year to year. This implies that we can get $\bar{\beta}_{1ict}$, the average wage elasticity of labor demand for industry i located in county c at year t, using equation 3.14.

$$\bar{\beta}_{1ict} = \int \beta_{1ic} \hat{\phi}_{Ln} \left(\beta_{1ic}; \triangle\varepsilon_{ict} \right) d\beta_{1ic}, \tag{3.14}$$

which can be rewritten as

$$\bar{\beta}_{1ict} = \int \beta_{1ic} \frac{\phi \left(\triangle\varepsilon_{ict}; \beta_{1ic} \right) \times \phi_{Ln} \left(\beta_{1ic} \right)}{f \left(\triangle\varepsilon_{ict} \right)} d\beta_{1ic} \tag{3.15}$$

The simulated counterpart of $\bar{\beta}_{1ict}$ is $\check{\bar{\beta}}_{1ict}$ which is described by the formula given in equation 3.16,

$$\check{\bar{\beta}}_{1ict} = \sum_r w^r \beta^r \tag{3.16}$$

where

$$w^r = \frac{\phi \left(\triangle\varepsilon_{ict}; \beta_{1ic} \right)}{\sum\limits_r \phi \left(\triangle\varepsilon_{ict}; \beta^r_{1ic} \right)}. \tag{3.17}$$

Since we assume a time invariant wage elasticity of labor demand, we modify equations 3.16 and 3.17 to get $\check{\bar{\beta}}_{1ic}$ as shown below.

$$\check{\bar{\beta}}_{1ic} = \sum_t \sum_r w^{r\prime} \beta^r \tag{3.18}$$

$$w^{r\prime} = \frac{\phi \left(\triangle\varepsilon_{ict}; \beta^r_{1ic} \right)}{\sum\limits_t \sum\limits_r \phi \left(\triangle\varepsilon_{ict}; \beta^r_{1ic} \right)} \tag{3.19}$$

We map the county specific total wage elasticity of labor demand for each industry using ArcGIS© (Figures 3.8, 3.8, 3.8 and 3.8). As the color in the maps changes from yellow to red, it indicates an increasing wage elasticity of labor demand. The white spots in the map are counties for which we have no estimates available.

3.7 Effect of Union Membership on County Specific Labor Demand Elasticity

In section 5 we mentioned that various factors might induce variation in the wage elasticity of labor demand across counties in the U.S. One such factor might be the incidence of union membership among workers. Intuitively, unions should make firms less flexible in hiring and firing workers in response to wage changes, and, therefore, should exert a negative impact on the absolute value of the total wage elasticity of labor demand. In other words, if there is a significant presence of unions in a state, then following an increase in employment the firms might not be able to reduce employment as much as in a state with lower union presence.

We use an alternative measure of union power by introducing a dummy variable measuring whether a state has implemented a right to work law. If a union is certified at a place of work, then an employee might be required to join the union or pay membership dues. This practice deals with the free rider problem where a worker does not pay the cost of negotiation (membership fee, wage loss during the negotiation period if a strike is called), but enjoys the benefits made possible by negotiations between management and union. A right to work law removes the requirement of being a union member in order to gain employment, or paying membership fees even if the non union member worker will enjoy the benefits arising from the union's negotiations with the management. Hence, in a right to work law state, employers will have more flexibility in changing their hiring pattern following a wage movement. As a consequence we will expect the total elasticity of labor demand to be higher in a county that belongs to a state that has the right to work law in place.

To test this hypothesis we obtain data for the years 2001 to 2010 on the percentage of workers in a state belonging to unions from the Bureau of Labor Statistics. We average the union membership data for the ten year period for the lower 48 states and the District of Columbia. The averages are shown in Table 3.6. According to Table 3.6, over the ten year period, New York State had the highest average union membership among workers at 26.26%, more than twice the overall average of 11% in the conterminous U.S. during this period; North Carolina had the lowest at 4.26%.

We specify our model as follows:

$$\begin{aligned} \ln\left(\breve{\bar{\beta}}_{1ic}\right) &= \sum_{k=1}^{K} \gamma_k X_{ick} + \delta \ln\left(Average\ Union\ Membership_{s(c)}\right) \\ &\quad + \xi Right\ to\ Work\ Dummy + \varepsilon_{ic} \end{aligned} \tag{3.20}$$

where average union membership gives us the extent of unionization in a state and X is a set of controls (K) (average total county employment between 2001 to 2010, industry dummy variables, urban dummy). In a different specification, instead of including average union membership as the main independent variable of interest, we include a dummy indicating whether the state has a right to work law in place or not. The right to work dummy variable has the value of 1 if the state where the county is in has a right to work law in effect. Table 3.7 shows the right to work states and the year when the statue was enacted and/or the constitution amended. We treat the dummy for right to work having the value 0 for Indiana and Michigan as they became right to work states in 2012. We then specify the model with both the average union membership and right to work dummy included.

All the three models are then estimated with dummy variables for state included in order to account for state fixed effects.

In Table 3.8 we present regression results where the dependent variable is the log of the absolute value of the county wage elasticity of labor demand in an industry and the independent variable(s) of interest is the log of average state level union membership among workers and/or the right to work dummy. The regression sample pools across all industries as can be seen from equation 3.20. In the regression equation, we include average total county employment over the ten year period, and a dummy variable to indicate if the county was designated rural or urban in the 2000 U.S. decennial census. We also include industry fixed effects in the regression. In addition to these covariates, specifications 4, 5, 6 in Table 8 include state fixed effects. Across all specifications except specification 3 (with both average union membership and the right to work dummy included, but state dummies excluded) we find that higher union membership among workers in a state tends to lower the absolute value of the county wage elasticity of labor demand. We find that raising union membership by 10% among workers will reduce county wage elasticity of labor demand by 0.05% according to specification 1 (with average union membership included, but the right to work dummy and state dummies excluded). In addition, we find that counties designated urban in the 2000 U.S. decennial census, usually have a lower wage elasticity of labor demand. Counties which have more workers on average, tend to have a more elastic labor demand. In specification (3), where we have both average union membership and right to work binary variable included in our model, but exclude state indicator variables, the effect of union membership becomes positive but not statistically significant.

With the right to work dummy included in our model, we find that the absolute value of the county specific total wage elasticity of labor demand will go up (or the demand for labor will become more elastic) if the state, where a specific county is in, has a right to work law in place. When we include only the right to work dummy in our model and exclude average union membership and state dummy variables, as in specification (2), we find that the total wage elasticity of labor demand is about 0.7% higher in counties belonging to states with a right to work law. When we include only the average union membership variable but not state identifiers, as in specification (3), we find that counties in states with right to work laws have about a 1.1% higher labor demand elasticity. In specification (5), including just the state dummy variables, but not the average union membership tells us that, if a county is in a state with the right to work law in place, then it will have a 5.2% higher labor demand elasticity. If we include average union membership, the right to work dummy, and state indicator variables in our model (specification 6), we find that a 10% rise in average union membership will lower the total wage elasticity of labor demand in a county by 0.18%, and, if the state where the county is situated in has enacted a right to work law, then it will increase the labor demand elasticity by 7.2%.

To summarize, we find in all the specifications except one (not statistically significant) that, with a higher extent of union membership, the county-specific total wage elasticity of labor demand decreases. This implies that, as union penetration rises, the total wage elasticity of labor demand becomes less elastic, or employers become less flexible in their hiring and firing decisions. We find in all specifications that, if a county belongs to a state that has enacted a right to work law, then the county-specific total wage elasticity of labor demand is higher in that county. In other words, if union membership or payment of union membership dues are not mandatory, then the total wage elasticity of labor demand will be higher, or employers will have more flexibility in the hiring and firing decisions.

3.8 Conclusion

The main goal of this study is to provide a benchmark analysis for the estimation of labor demand elasticities by classifying the United States labor market into different industries. One advantage and rationale for pursuing this study is to be able to investigate and comment on the effects of different external shocks and policy changes on the elasticity of labor demand for different industries, without being restricted to any particular industry or sector within an industry. We estimate the elasticity of labor demand by dividing the entire United States economy into various industry groups. Our unit of observation in this study becomes an industry-county pair in any given year. Using the County Business Patterns (CBP) dataset for the years 1988 to 2010 we provide county[3] specific estimates of the total wage elasticity of labor demand for four industries: construction, finance-insurance-real estate-service, manufacturing, and retail trade. Our estimates are based on a two-step procedure. In step one we estimate linear, constant parameters, panel data models for each industry. Using the results from step one, in step two we estimate again, for each industry, a linear panel data model, but, where the total wage elasticity of labor demand parameter is a random variable having a log normal distribution in the population of counties. We find statistically significant evidence that the total wage elasticity of labor demand exhibits spatial variation within each of the four aggregated industries.

Our estimates of the county specific total wage elasticities of labor demand can be utilized to investigate the effects of a policy shock, such as a minimum wage law, or of a labor market feature, such as the extent of union membership on the elasticity of labor demand. Our methodology enables us to compare not only the absolute changes in

[3]2943 Counties located in the conterminous U.S.

the labor demand elasticity in an industry after a policy change or a change in a labor market feature, but also the relative changes in the labor demand elasticity across industries. We show this by analyzing the effect of union membership and the right to work law on the labor demand elasticity. We find that higher union membership makes the county-specific total wage elasticity of labor demand less elastic, and the presence of a right to work law makes it more elastic.

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Table 3.1: Elasticity Measurements in the Literature

Study	Description	Data	Time Period	$-\eta'_{LL}$
Nadiri ('68)	U.S. Manufacturing, K held constant	Aggregate, Quarterly, Time Series	1947-64	0.12
Messe ('80)	U.S. private production-worker, KL prices, K held constant	Aggregate, Quarterly, Time Series	1947-74	1.73
Layard & Nickell ('86)	U.K. Aggregate, K held constant	Aggregate, Quarterly, Time Series	1957-83	1.19
		Aggregate, Annual, Time Series	1954-83	0.93
Andrews ('97)	U.K. Aggregate, KLEM prices, K held constant	Aggregate, Annual, Time Series	1950-79	0.51
Burgess ('88)	U.K. Manufacturing, EM prices, K held constant	Aggregate, Quarterly, Time Series	1964-82	1.85
Harris ('90)	New Zealand private worker, K held constant	Aggregate, Quarterly, Time Series	1965-87	0.24
Nickell & Symons ('90)	U.S. Manufacturing, K held constant	Aggregate, Quarterly, Time Series	1962-84	1.92
Symons & Layard ('84)	OECD Manufacturing, LM prices, no Y or K	Aggregate, Quarterly, Time Series	1956-80	1.54
Wadhwani ('87)	U.K. Manufacturing, KLM prices, no Y or K	Aggregate, Quarterly, Time Series	1962-81	0.38
Kennan ('88)	U.S. Manufacturing production-worker, no Y or K	Aggregate, Monthly, Time Series	1948-71	11.58
Begg et al. ('89)	U.K., import prices, no Y or K	Aggregate, Annual, Time Series	1953-85	0.40
Caruth & Oswald ('85)	U.K.Coal Mining, KLE prices, no Y or K	Small Industry, Annual, Time Series	1950-80	1.4
Wadhwani & Wall ('90)	U.K. Manufacturing, ML prices, K held constant	Firms, Panel Data	1974-82	0.53
Benjamin ('92)	Java Farm Labor, L held fixed	Farms, Cross Section	1980	0.30
Blanchflower et al. ('91)	U.K. plants, no Y or K	Plants, Cross Section	1984	0.93
Slaughter ('01)	U.S. Manufacturing Non-production Labor, no K	Aggregate, Annual, Time Series	1961-91	0.65
Hasan et al. ('07)	India Manufacturing, no K	Small Industry, Panel Data	1980-97	0.40

Notes: *Source*- Hamermesh (1993), authors

Table 3.2: Industry Aggregation Scheme

Name	Industries	SIC	NAICS
Construction (CONS)	Construction	15	23
Finance, Insurance, Real Estate, Service (FISE)	Finance and Insurance; Real Estate and Rental and Leasing; Information; Professional, Scientific, and Technical Services; Management of Companies and Enterprises; Administrative and Support and Waste Management and Remediation Services; Educational Services; Health Care and Social Assistance; Other Services (except Public Administration)	60, 70	51, 52, 53, 54, 55, 56, 61, 62, 81
Manufacturing (MANU)	Manufacturing	20	31-33
Retail Trade (RETA)	Retail Trade; Arts, Entertainment, and Recreation; Accommodation and Food Services	52	44, 45, 71, 72

Table 3.3: Summary Statistics

	mean	sd	iqr	min	max	count
Construction						
Employment	2031.9	6779.1	1070	1	178869	64826
Wage	14.6	4.73	5.75	0.61	111.0	64826
Output	192.4	560.3	116.8	1.13	17412.1	64826
Finance, Insurance,Real Estate, Service						
Employment	15087.9	65559.1	5488	1	1987415	70162
Wage	13.5	4.40	4.22	0.63	103.2	70162
Output	1772.4	7631.0	747.0	1.05	287127.8	70162
Manufacturing						
Employment	6068.5	19922.0	4369	2	909836	60298
Wage	18.9	6.18	7.36	1.54	77.4	60298
Output	684.0	2278.5	403.1	3.91	88393.7	60298
Retail						
Employment	7839.7	25965.9	4338	1	853649	70484
Wage	8.39	1.60	1.67	0.99	50.2	70484
Output	403.0	1465.7	217.6	1.09	64728.4	70484

Note: IQR- Interquartile Range

Real Wage in terms of 2010 dollars: Real Wage(t)= Wage(t)*(CPI(2010)/CPI(t))

Table 3.4: Results from Constant Parameter Panel Data Model

Industry	β_{1i} $\triangle log(wage)$ (1)	β_{1i} $\triangle log(wage)$ (2)	β_{2i} $\triangle log(output)$ (1)	β_{2i} $\triangle log(output)$ (2)	R^2 (1)	R^2 (2)	Observations
Construction	-0.32	-0.29	0.59	0.58	0.24	0.16	59615
	[0.02]	[0.02]	[0.03]	[0.02]			
	(-13.46)	(-12.73)	(21.75)	(25.86)			
Fin./Ins./Real Est./Service	-0.11	-0.13	0.48	0.47	0.36	0.32	66707
	[0.03]	[0.03]	[0.01]	[0.01]			
	(-3.91)	(-4.82)	(32.71)	(32.84)			
Manufacturing	-0.23	-0.22	0.42	0.27	0.16	0.06	55961
	[0.03]	[0.03]	[0.02]	[0.02]			
	(-7.26)	(-7.13)	(19.21)	(11.88)			
Retail Trade	-0.23	-0.20	0.88	0.69	0.50	0.37	66945
	[0.04]	[0.04]	[0.02]	[0.02]			
	(-5.68)	(-5.06)	(45.08)	(30.07)			
State Dummy $\times$Year Dummy	$\surd$	$\times$	$\surd$	$\times$	$\surd$	$\times$	

Notes: Dependent Variable: $\triangle log\left(L_{ict}\right)$
(1): Includes state dummy and year dummy interactions
(2): Doesn't include state dummy and year dummy interactions
Cluster Robust Standard Errors in brackets; T statistics in parentheses
Cluster ID is State

Table 3.5: Results from Random Parameter Panel Data Model

Industry	$\overline{\beta}_{1i}$	$\sqrt{\sigma^2_{\beta_{1i}}}$	β_{1i}	γ_i	β_{2i}	$\sigma^2_{\varepsilon_i}$
Construction	0.08	0.01	-2.47	0.48	0.59	0.04
			(-52.98)	(30.81)		
Fin./Ins./Real Est./Service	0.34	3.26	-3.34	0.75	0.48	0.04
			(-38.98)	(41.76)		
Manufacturing	0.38	3.97	-3.32	0.77	0.42	0.03
			(-40.17)	(42.53)		
Retail Trade	0.35	0.98	-2.15	0.39	0.88	0.01
			(-50.79)	(27.09)		

Notes: $\overline{\beta}_{1ic}$= Mean of log normal distribution for β_{1ic}= $exp\left[\beta_{1i} + 0.5exp\left(\gamma_i\right)^2\right]$
$\sqrt{\sigma^2_{\beta_{1i}}}$ = Standard deviation of log normal distribution for β_{1ic}
$= exp\left[exp\left(\gamma_i\right)^2 - 1\right] exp\left[2\beta_{1i} + exp\left(\gamma_i\right)^2\right]$
T statistics in parenthesis
β_{2i} and $\sigma^2_{\varepsilon_i}$ are fixed during estimation

Table 3.6: Average Union Membership Among Workers by State

FIPS State	State Name	Mean Union Mbrshp (%)	FIPS State	State Name	Mean Union Mbrshp (%)
1	ALABAMA	10.83	31	NEBRASKA	10.41
4	ARIZONA	7.83	32	NEVADA	16.80
5	ARKANSAS	6.05	33	NEW HAMPSHIRE	11.63
6	CALIFORNIA	18.13	34	NEW JERSEY	20.00
8	COLORADO	9.15	35	NEW MEXICO	9.96
9	CONNECTICUT	16.97	36	NEW YORK	26.26
10	DELAWARE	12.76	37	NORTH CAROLINA	4.26
11	DISTRICT OF COLUMBIA	14.01	38	NORTH DAKOTA	8.66
12	FLORIDA	7.56	39	OHIO	16.36
13	GEORGIA	6.23	40	OKLAHOMA	8.10
16	IDAHO	7.64	41	OREGON	16.70
17	ILLINOIS	17.73	42	PENNSYLVANIA	16.14
18	INDIANA	13.20	44	RHODE ISLAND	17.43
19	IOWA	13.73	45	SOUTH CAROLINA	5.18
20	KANSAS	9.66	46	SOUTH DAKOTA	7.01
21	KENTUCKY	11.29	47	TENNESSEE	7.46
22	LOUISIANA	7.57	48	TEXAS	6.45
23	MAINE	14.10	49	UTAH	7.27
24	MARYLAND	14.68	50	VERMONT	12.65
25	MASSACHUSETTS	15.68	51	VIRGINIA	6.13
26	MICHIGAN	20.41	53	WASHINGTON	20.56
27	MINNESOTA	17.05	54	WEST VIRGINIA	15.10
28	MISSISSIPPI	7.50	55	WISCONSIN	16.00
29	MISSOURI	12.73	56	WYOMING	9.39
30	MONTANA	14.93			

Notes: Source- Bureau of Labor Statistics, authors' calculations

Table 3.7: States with Right to Work Laws

FIPS State	State Name	Statue Enactment	Constitutional Amendment
1	ALABAMA	1953	
4	ARIZONA	1947	1946
5	ARKANSAS	1947	1944
12	FLORIDA	1943	1968
13	GEORGIA	1947	
16	IDAHO	1985	
18	INDIANA	2012	
19	IOWA	1947	
20	KANSAS		1958
22	LOUISIANA	1976	
26	MICHIGAN	2012	
28	MISSISSIPPI	1954	1960
31	NEBRASKA	1947	1946
32	NEVADA	1951	1952
37	NORTH CAROLINA	1947	
38	NORTH DAKOTA	1947	1948
40	OKLAHOMA	2001	2001
45	SOUTH CAROLINA	1954	
46	SOUTH DAKOTA	1947	1946
47	TENNESSEE	1947	
48	TEXAS	1993	
49	UTAH	1955	
51	VIRGINIA	1947	
56	WYOMING	1963	

Notes: Source- National Conference of State Legislatures

Table 3.8: Effect of Union Membership and Right to Work Law on Labor Demand Elasticity

Independent Variables	(1)	(2)	(3)	(4)	(5)	(6)
Log of Average State Union Membership	-0.005		0.005	-0.016		-0.018
	[0.004]		[0.007]	[0.001]		[0.001]
	(-1.15)		(0.74)	(-16.11)		(-31.52)
Right to Work Dummy		0.007	0.011		0.052	0.072
		[0.003]	[0.005]		[0.001]	[0.002]
		(2.08)	(2.00)		(43.23)	(31.10)
Log of Average Employment	0.020	0.020	0.020	0.020	0.020	0.020
	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
	(14.02)	(14.59)	(14.27)	(14.80)	(14.80)	(14.80)
Urban Dummy	-0.008	-0.008	-0.008	-0.006	-0.006	-0.006
	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]	[0.003]
	(-2.55)	(-2.66)	(-2.60)	(-2.02)	(-2.02)	(-2.02)
State Dummy	$\times$	$\times$	$\times$	$\surd$	$\surd$	$\surd$
R^2	0.415	0.415	0.419	0.419	0.419	0.419
Observations	11772	11772	11772	11772	11772	11772

Notes: Dependent Variable: log of absolute value of labor demand elasticity. Industry fixed effects are included
Columns 1,2,3: Don't include state dummy
Columns 4,5,6: Include state dummy
Cluster Robust Standard Errors in brackets; T statistics in parentheses; Cluster ID is State

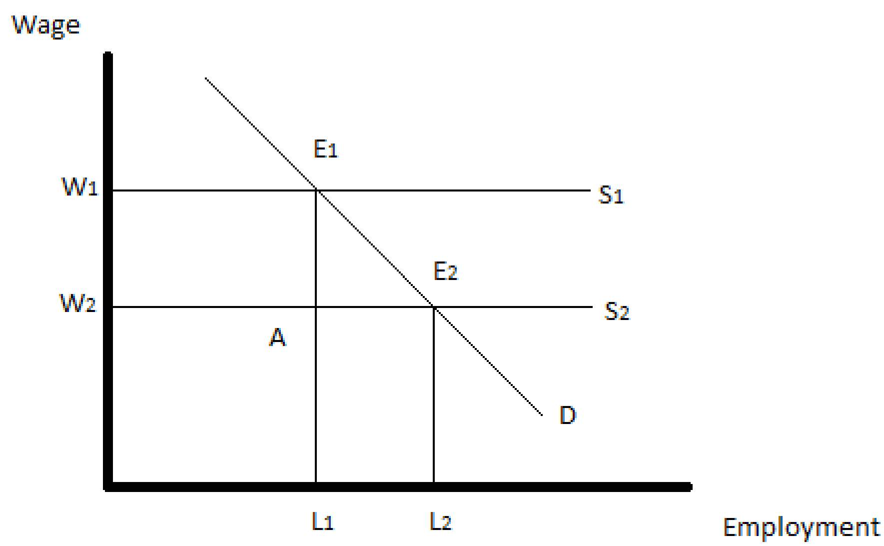

Figure 3.1: Infinitely Elastic Labor Supply (Hamermesh, 1993)

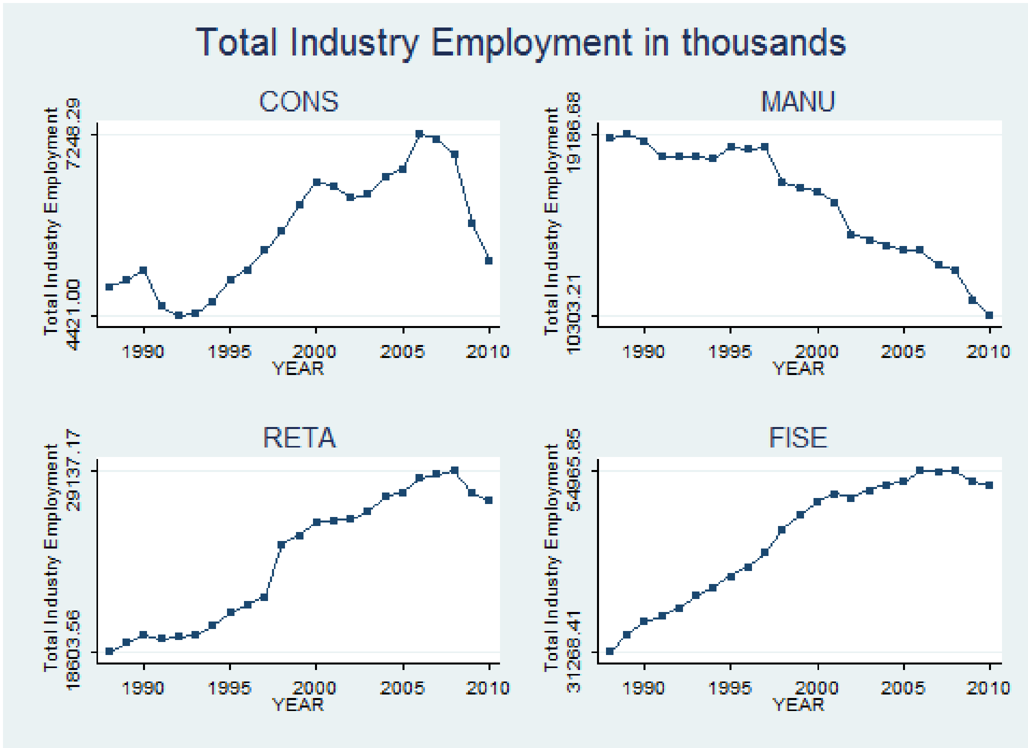

Figure 3.2: Total Industry Employment in Thousands

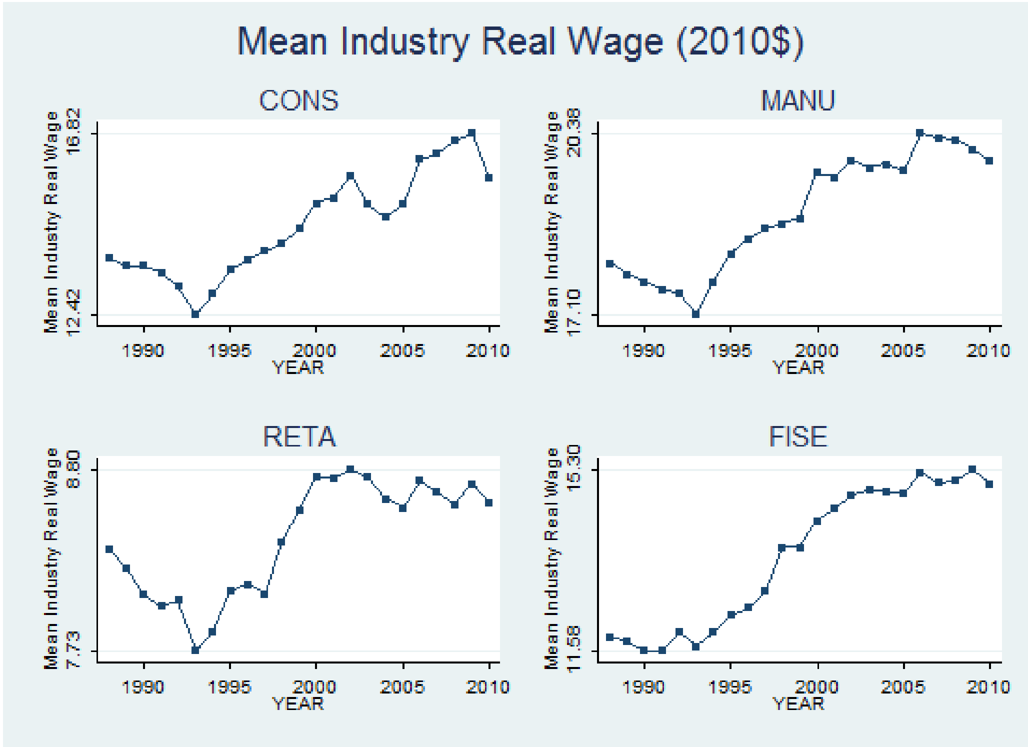

Figure 3.3: Mean Industry Wage Rate for the United States

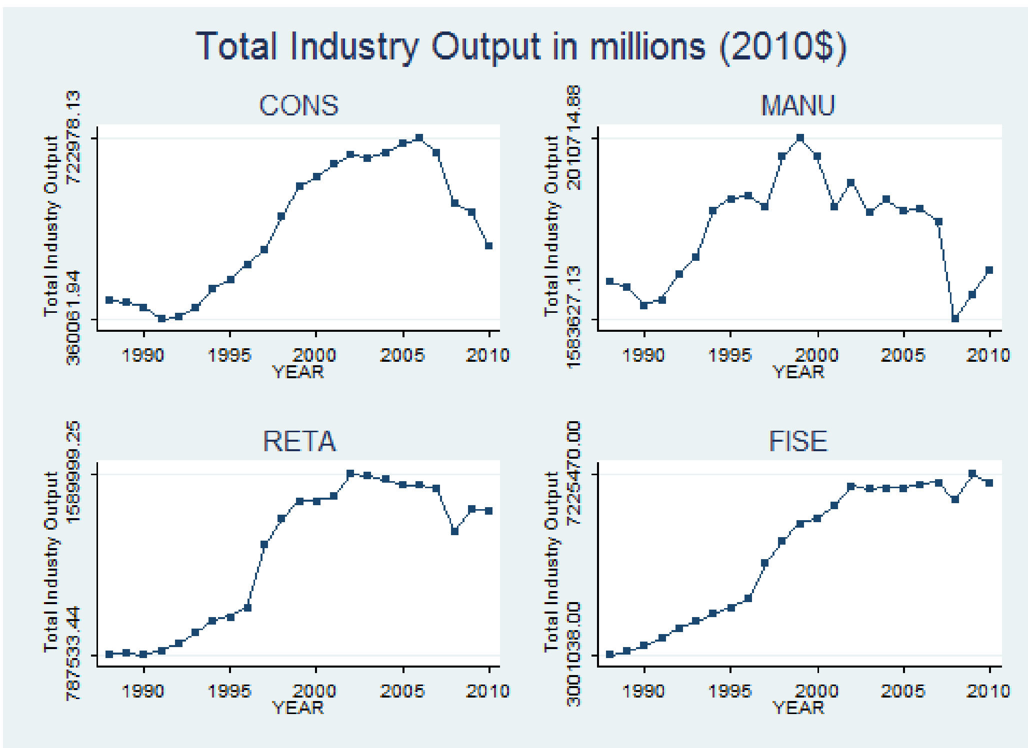

Figure 3.4: Mean Industry Output for the United States

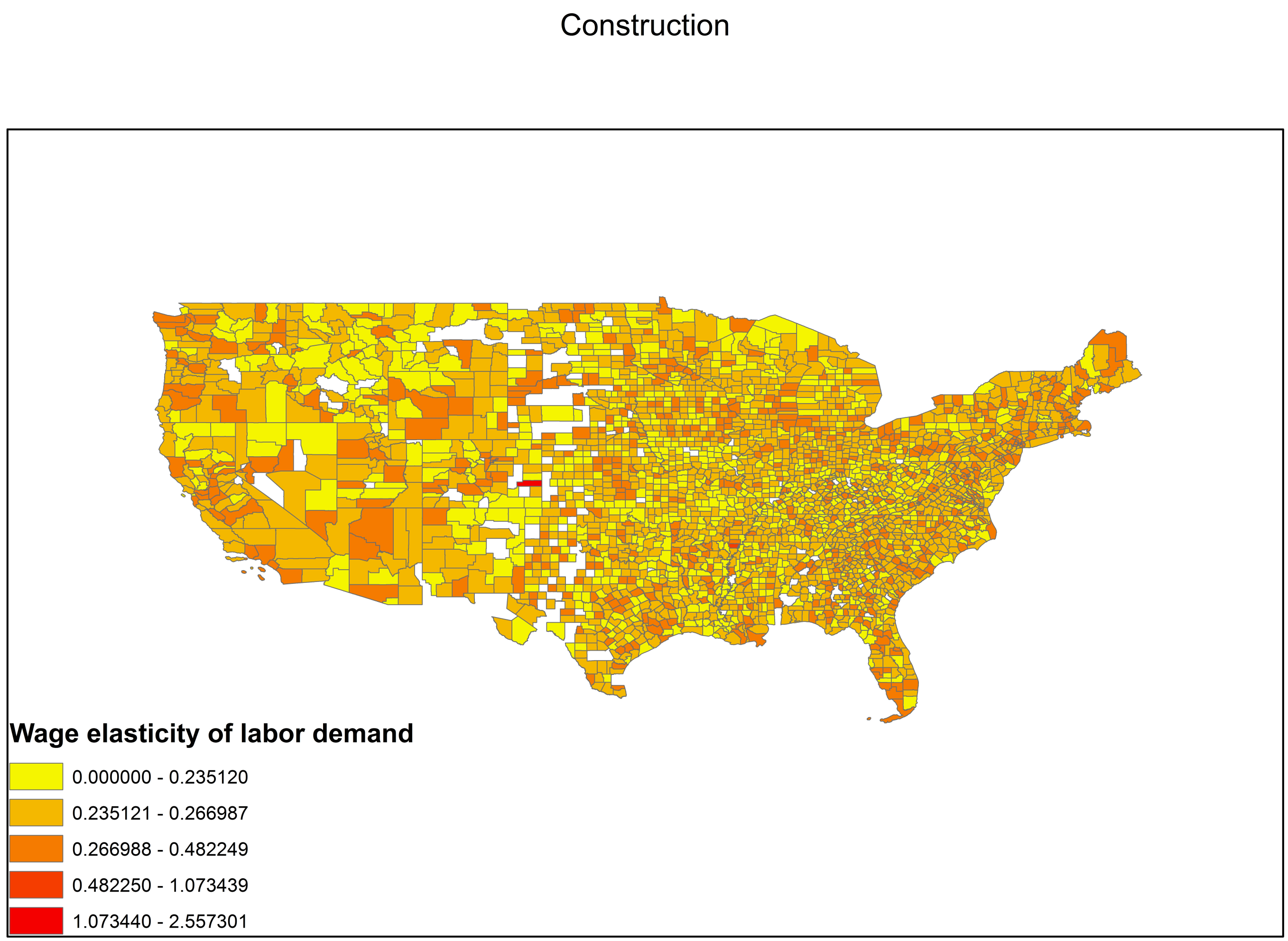

Figure 3.5: County-wide Distribution of Labor Demand Elasticity in Construction

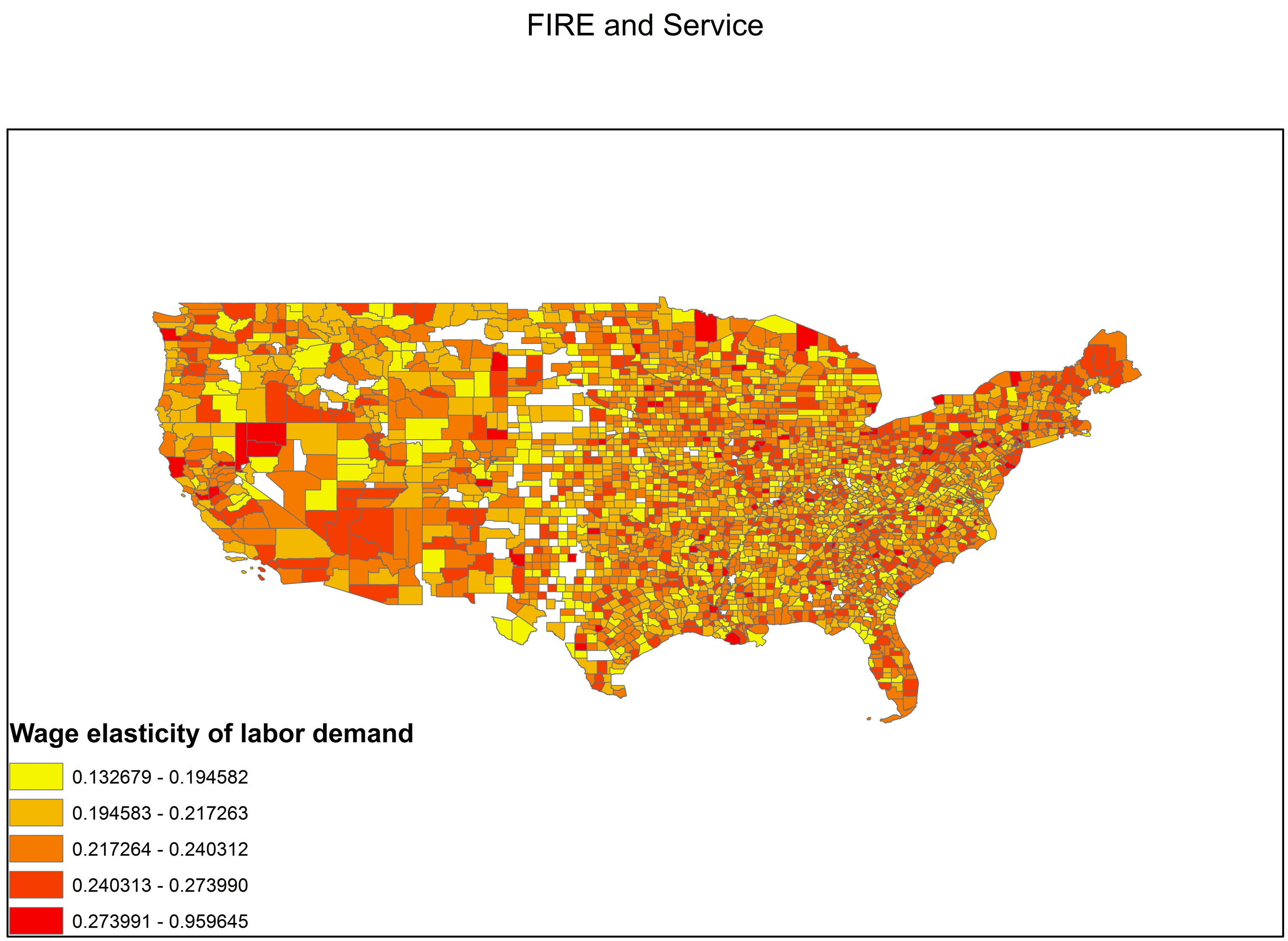

Figure 3.6: County-wide Distribution of Labor Demand Elasticity in Finance, Insurance, Real Estate, Service

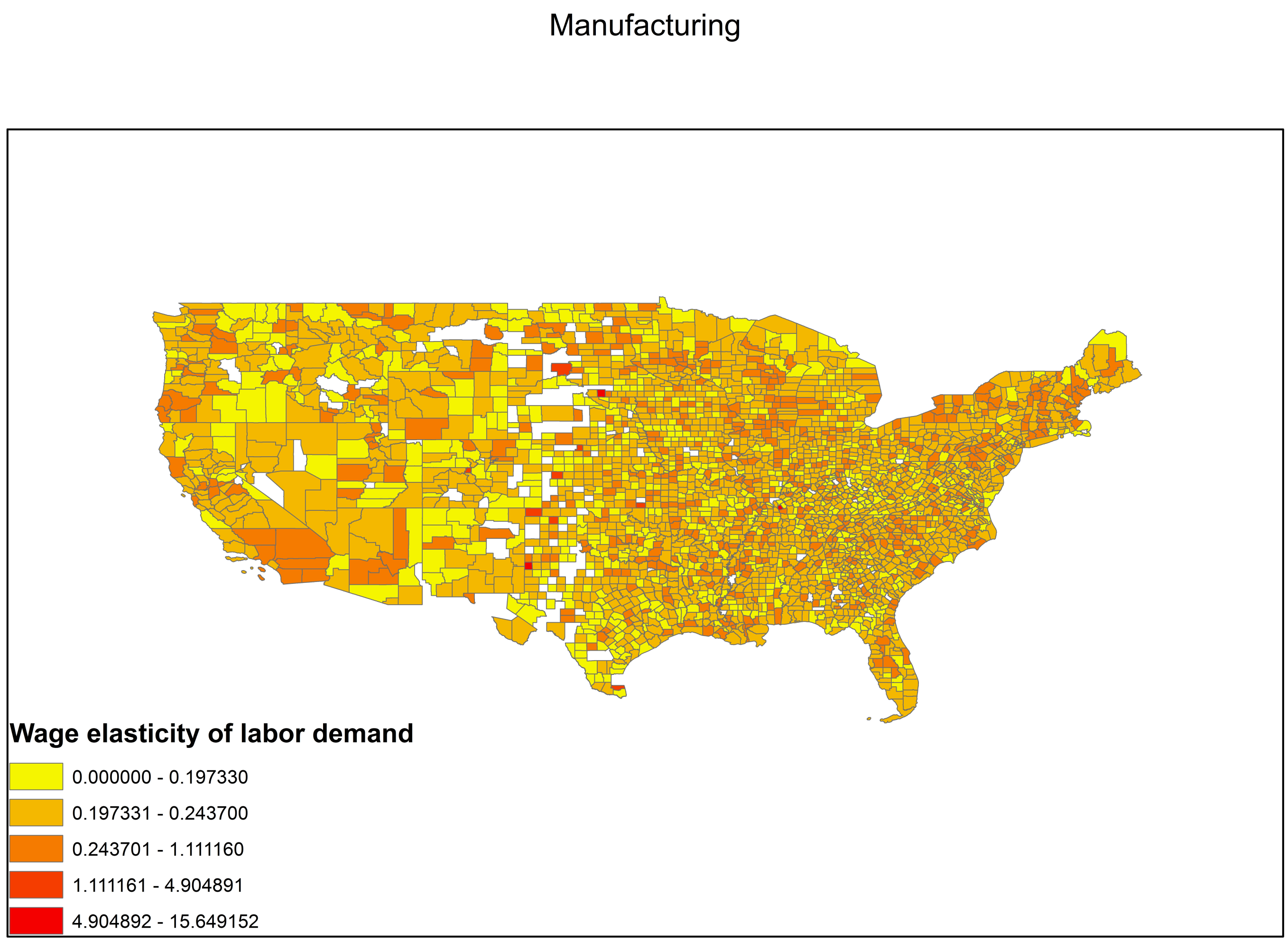

Figure 3.7: County-wide Distribution of Labor Demand Elasticity in Manufacturing

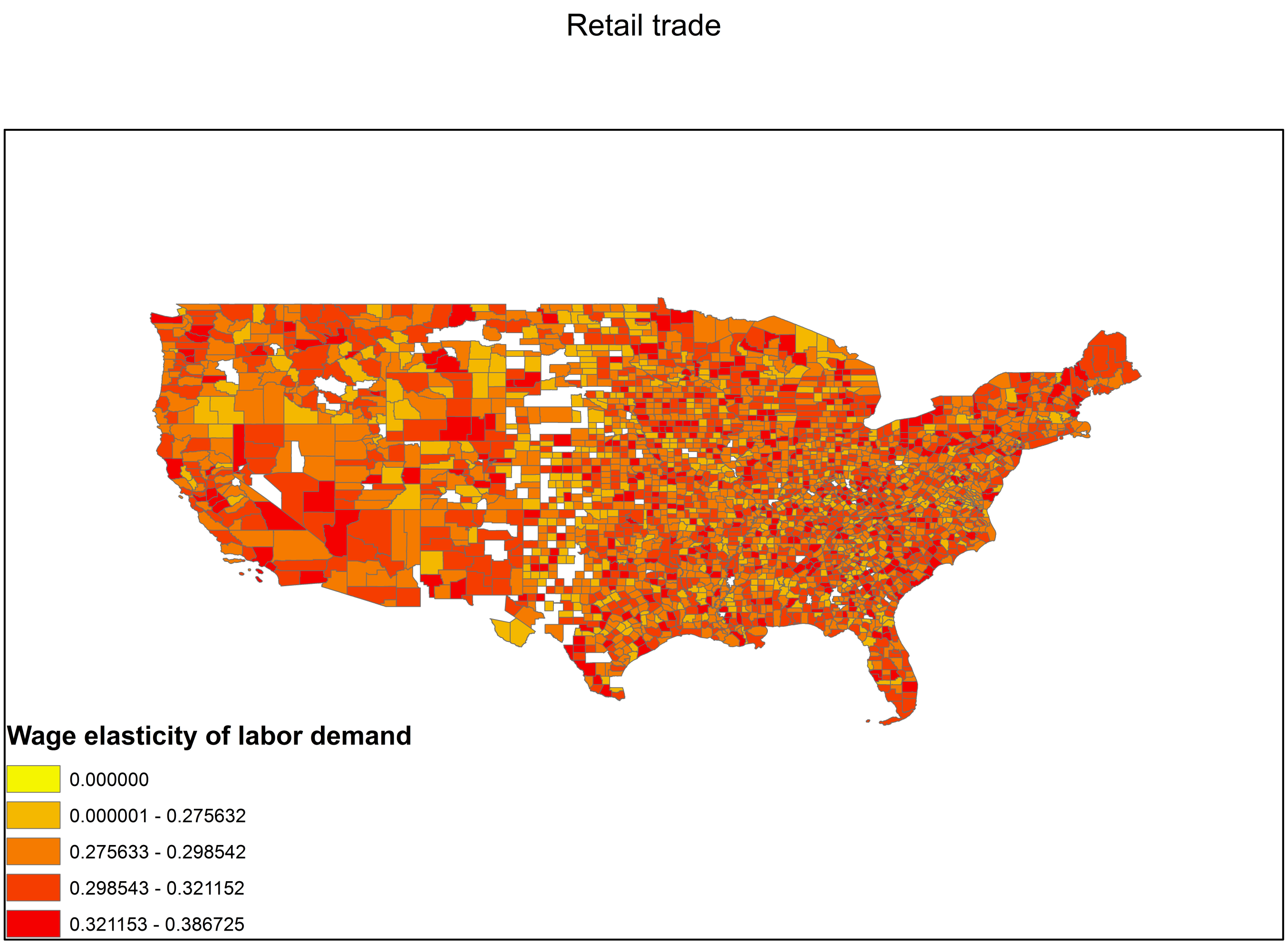

Figure 3.8: County-wide Distribution of Labor Demand Elasticity in Retail