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# PROPERTY TAX CAPITALIZATION IN NEW HAMPSHIRE HOUSING VALUES

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**Abstract**

This paper focuses on the capitalization of property tax rate in housing values in towns of New Hampshire. The state lacks income and sales tax revenues, so the property tax is very volatile to the assessor's ambitions for the fiscal year and considerably high for a state that doesn't provide much except rural living. New Hampshire has only three interstates and one West to East highway, minimal coast line, and significant state and national forest areas, so high property tax rates must reflect more state offerings than is blatantly obvious. Test results, of manually gathered and assembled from various New Hampshire state bureaus, show expected inverse relationships between the total property tax rate and housing values as well as an unexpected inverse relationship between local education tax rates and housing values. Through econometric analysis of panel data, this paper tested the effects of a change in the property tax rate and local education tax rate on the Housing Price Index value (HPI) for individual towns for which data is available. For thorough analysis, independent characteristics such as unemployment rate and population density are considered for their effects on property values via the property tax rate. The models find that there is a small and insignificant inverse relationship between the property tax and the HPI appreciation while the other characteristics vary by year. Contradictory to literature, the tests find a negative and significant relationship between the local education tax rate and housing values. Assuming capitalization occurs, the correlation can be interpreted as inefficient use of funds, cross subsidies in cooperative and regional school districts, or data error. This paper discusses bias that may cause insignificance. (JEL Classification Codes: H22, H71, H72, H75)

## 1 Introduction

Property in New Hampshire is relatively highly taxed where people pay similar amounts as residents in California, Connecticut and Massachusetts in some towns. The assessment of a property's value and the property tax rate have a close relationship in simultaneously determining one another rather than a simple x, y relationship. Property values directly affect the amount of revenue each jurisdiction can collect. I suspect that higher assessment values reflect the property tax for the given year as a result of capitalization and changing resident attitudes toward public services. Since these higher rates are reflected in a lower market value of the property, it poses a potential issue of inequality of payments.

Education is highly valued across New Hampshire, and these attitudes influence the property tax rates, specifically the local education tax rate; *residents* of and cooperative school systems vote for increases, decreases, or no changes in tax rates to fund public education. However, according to the town of Brookline meeting results (2019), the majority of residents do not vote, yet are subject to these rate changes, even if the changes may not always create an increase in school quality. This system creates disparities for tax payers with identical homes (in size, land, modernity, etc.) across New Hampshire towns; a tax payer in Amherst pays significantly greater taxes than Deering as a result of local education tax rate voting.

## 2 Literature Review

Sirmans, Gatzlaff and MacPherson (2008) detail the evolution of property tax capitalization through a string of references, which I reference and summarize in this section. The article explains that Hamilton (1976) believes amenities differ among jurisdictions and those differences must be reflected in the property values. In concurrence with such changes, the market should adjust for these capitalization effects; if prices are changed by the tax and benefit capitalization, the effect is a change in supply followed by another price change. The prices and accompanying public services, however, depend on equality of land value if communities are homogenous. In New Hampshire, homogeneity is rare since the three border states (Massachusetts, Maine and Vermont) have such different demographics and influence bordering towns, and a majority of conservation land scattered throughout the state creates a heavy rural influence. The communities are just as heterogeneous within each individual jurisdiction. These external effects create great differences in income classes within towns thus differences in property and fiscal surplus differentials. I will consider the yearly House Price Index (HPI) value mean for each town of which data is available to account for such differences.

Elasticity of demand explains this capitalization effect as theorized by Henderson (1985); elasticity of supply of housing and relevant benefits as well as costs of public services should be reflected in property values. New Hampshire state tax funding and expenditures are highly dependent on property tax revenues and each town (an individual jurisdiction) is responsible for providing services and setting appropriate rates according to the attitudes of their residents. The relevant benefits of public services will be determined by what the population believes is important, so an increase or maintenance of the local education tax rate reflects the higher value to residents.

Henderson (1985) finds that elastic housing stock does not reflect capitalization in house prices while an inelastic demand means an increase in demand raises prices and perhaps decreases taxes (less funding). He writes that people shop for their optimal or preferred bundle of services rather than the independent piece of property. The different levels of received government services directly determine the varying property prices and indirectly determine the costs of the services.

Per Palmon and Smith (1998), the property tax should be treated as an amenity itself. Prospective buyers consider the tax rate in addition to property location, structure and quality of public services. The 1998 amenity model provided that these considerable factors can be analyzed in a linear function and find full capitalization of the property tax in housing values.

Lutz (2008) looks at the housing market bubble and 2007-2009 Great Recession. He claims the housing market "run-up" decreases the dependability of the determined property tax rate, since the market reflects what may be valuable today. However, revenue affects lag by at least one year. Though a useful consideration, New Hampshire residents' present values of housing may not be adequate for predicting revenue, especially after a period with heightened cyclical risk. Lutz studies significant increases in housing values and policymakers' responses. Since these increases affect revenues, they have the potential to skew the tax burden; Lutz models and calculates the average elasticity of the change in revenues to make his conclusion about responsiveness. The elasticity of 0.4 indicates that policymakers respond to large house price increases by implementing offsetting mechanisms to avoid a shift of the burden. In times of normal appreciation, little is done to offset.

In *Property Taxes for Local Finance*, Fisher focuses on finding the burden of property tax payments on homeowners with respect to their personal income. Fisher's (2009) study measured the impact of tax amounts on the type of property compared to the total value of property and total tax amounts to personal income ratios in addition to analyzing American Housing Survey Data. The study found at least 2% of income is dedicated to property tax payments on average. Should Fisher's research be updated, it would be interesting to compare the ratios years later when the recession is not looming over American households.

Property tax rates and HPI per capita is assessed and tested for correlation by Cornia and Walters (2006) under full disclosure of the state assessor. As house prices appreciate state assessors can respond by reducing the property tax rate and vice versa in order to restrain growth in the overall tax bills of property owners at the margin. In Utah, they find that the per capita HPI has a positive relationship with the market value of the property while the effective tax rate on residential property is *negatively* related to changes in the assessed value of property. Fully considering the time stamp of the study, this paper showed that states that witnessed growth in property tax revenues see slower house value appreciation. Allowing full disclosure of the state assessor can offset expenses for the homeowner, thus the inverse relationship between the effective property tax rate and the house value.

Part of this paper's focus is the share of the property tax that is dedicated to local education, specifically it considers the effect of an increase in the local education tax rate on housing values. Henderson, Simar and Wang (2017) researched the relevant inputs of Illinois schooling and their effects on school quality (gauging quality with state test scores) while considering the effects of property tax limits. In Cobb-Douglas and Translog tests the researchers found that fiscal constraints on schools do not make them more efficient, and limits on property taxes don't directly decrease the quality of schooling, but negatively affect them. Simply forcing schools to efficiently manage the preexisting funds by limiting tax revenues will not create increases in state test scores.

Oates and Fischel (2016) consider resident attitudes about access to public education. They posit that public education should depend on the state, rather than the local jurisdiction, for funding since "access to public education should not depend on willingness to pay for it via the housing market". Socio-economic status differs among and within jurisdictions, so it can be inferred that towns with a larger "wealthy" population may be able to fund "better" (assuming more money provides better education) schools at lower property tax rates. Similarly, towns with a larger relatively less wealthy population may have lower quality schools. Funding inherently depends on the resident's willingness to pay higher amounts in housing to provide higher funding for education.

Local tax capacity determines the setup of the school district which defines the algorithm for the corresponding funding mechanism. Giertz and Mcguire (1992), who studied regional and state-wide property tax bases for funding local education in Illinois, find that people with a high demand for education reside in towns with relatively good schools (students score high on standardized tests) and relatively high property tax rates. There is a population that accepts lower school quality for lower rates, however, reorganization into regional or cooperative school districts may help finance better schools at the expense of a population with a higher tax capacity and willingness to pay. In this system, it is easier to level-up expenditures rather than reducing due to teacher contracts and indebtedness of school districts dependent on their local tax capacity, so a decrease in the education tax rate most likely won't happen. Transitioning education financing from local capacity to fixed expenditure, in an effort to prevent exploitative taxation, won't fix the problem; it may make resident preferences harder to satisfy equitably and reduce capitalization of education taxes in housing values. It's best to leave local residents in charge of voting for increases in funding what they value most.

Considering property taxes are subject to the assessor's report, the increase in the property tax, if capitalized, decreases the housing value. Wales and Wiens (1974) found this effect in their test of a single locale to measure differences in similar properties. A 1% increase in the property tax rate decreased housing values by 5%. They do stipulate that the changes in prices can be explained by omitted variables like substantial differences in consumer tastes. In New Hampshire, the differences in tastes considered are education or municipal service quality.

New Hampshire residents might have a higher proportion of disposable income in the absence of income taxes, however, the burden they inherit in property taxes may offset this benefit. The majority of the tax amount residents pay fund local education, or municipal services, depending on their location in the state. These differences reflect the attitudes of residents: some value education more than town services, and vice versa, so their house and the tax amounts they pay inform what is worth it, according to a survey of New Hampshire Residents.

# 3 Hypothesis

A decrease in the log of the average value of housing ($lp_t$) for the year is caused by an increase in the log of the property tax rate ($lt_t$) for the same year. In addition, an increase in the log of the local education tax rate ($lte_t$) should cause an increase in the average value of housing. When the two conditions hold, the property tax is capitalized in the market values of housing and reflect residents' optimal bundles, in a given jurisdiction, in accordance with Henderson (1985).

# 4 Methodology

## 4.1 Survey

In addition to panel data regressions, a survey, created with George Washington University's student access to Survey Monkey, was administered at random via social media groups with only New Hampshire residents as members. By selecting New Hampshire resident exclusive groups, I could control for differences in state property taxation. The limit was set for 100 people to respond to five to six questions about their knowledge of and feelings toward their property tax. Over the course of three days, respondents took the survey, and only 73 were able to answer all six questions; the respondent was asked if he or she knew his or her property tax break down, thus proceeding to ask if the respondent would change the breakdown, if possible. If the answer was no the respondent was prompted to skip the next question. The survey is not entirely representative as residents in various locations do not have access to social media or have internet capabilities. It is a mean of matching real peoples' attitudes about their property taxation with the regression results.

Questions one through five were multiple choice and question six being free response. Eighty-two respondents completed questions one through four. Of the 82, 32.93%, on question one, chose the answer choice stating they pay more than $1000 per month in property tax amounts and another 34.15% pay between $750 and $1000. Unsurprisingly and in accordance with the literature, education is an important consideration for residents; in fact, on question six, 34.15% vocalized that their main reason for moving to, or remaining in, their town of residence was/is for the school system while only 25.61% prioritized location. The tests in section 4.3 below cannot lead to a conclusion on the quality of school systems due to the lack of data and variability in reporting consistency. However, the tests look at the effects that increases in local education tax rates have on housing values, so a positive increase in housing values may quantify the decisions made by the surveyed residents.

Considerably as important to capitalization of property values is the value residents place on their municipal/public services provided by their town of residence. The majority (52.44%) of respondents believe the services they receive are worth the monthly amount in property taxes they pay. On question three, an unanticipated 36.59% of respondents chose "no" indicating they do not believe their town services are worth the amount in property taxes they pay. Though this paper did not test the quality of services (plowing, transfer station/garbage collection, etc.) it tests the effects of an increase in the total property tax, which encompasses the taxes for funding these services, on housing values.

## 4.2 Test

This paper uses a dataset organized into a panel which covers the period of 2010-2018. The panel is comprised of data sourced from the United States FHFA HPI by five-digit ZIP codes dataset, New Hampshire Unemployment Security Office, Department of Revenue Administration and Office of Strategic Initiative. The FHFA offers HPI values with base 2000, 1990, or 100 when first recorded and are not seasonally adjusted: for consistency this paper uses the base 100 HPI values for all years 2010 through 2018. The dataset is incomplete at best with several years of New Hampshire ZIP-code data missing. New Hampshire town unemployment and property tax rate data sets are complete through 2018 for each incorporated town. However, data gaps rendered only 158 towns out of the 221 viable for testing.

The unit of observation is the Housing Price Index (HPI) value in response to town total property tax rate and local education tax rate (dollars per thousand) and unemployment rate (percentage), population density (persons per square mile). There are initially 1,370 observations, however, per restricted use of 2010 US Census data for population estimates, there are 1,213 complete observations that can be used for complete testing. Testing controls for incompleteness through robust differences "aregs" which include fixed effects in the OLS regressions (using STATA 16).

The initial variables: Annual HPI value as *hpi*, town total property tax rate as *tax*, town local education tax rate as *edutax*, town annual unemployment rate as *uerate*, and town estimated annual population density as *popest*. I create new variables for the program to test to control for rates and percentages: log of hpi is *lp*, log of tax is *lt*, log of edutax is *lte*, log of uerate is *lu* (Annex Figure 1.0). We interpret the gathered summary statistics as total tax rates have a mean of $19.88 per $1000 and HPI values have a mean of 172.61 where the highest recorded value is 540.26.

This paper uses differences testing to model HPI appreciation (D.lp). I run multiple linear regressions with large dummy sets being *year*, aregs, so town can be absorbed and we get a clean summary of the relationship I look to find. The areg of lp on lte (Appendix Figure 2.0) has a negative coefficient, but is not significant at the 5% confidence level; this causes concern after reading previous literature and survey of New Hampshire residents which is discussed below. Appendix Figure 2.1 regresses lp on lt in a robust areg. The negative correlation, though insignificant at the 5% confidence level, is expected per the literature. Appendix Figure 2.2 uses every log variable as a regressor in a robust areg, as well as other aregs for individual variables and combinations. The addition of *lpe* to the regression in column 4 actually changes the p-value of the *lte* variable to .082; this is still insignificant; however, it is relatively the most significant variable in this regression. Figure 2.3 provides the full summary statistics for the regression of column 4 in Figure 2.2.

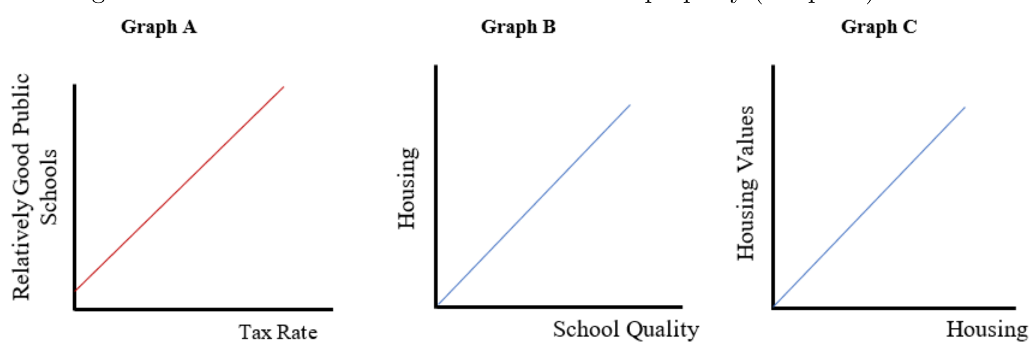
# 5 Interpretations

Market changes, election or business cycles may impact these numbers, but this paper will not make any steadfast conclusions since the regressors are *not* statistically significant in the model. There are three questions this analysis will address: What can we make of the capitalization of the property tax? Is there any bias to consider in these data points? Is there reason to be concerned about education funding?

Increases in the log of *edutax* and the log of *totaltax* decrease the HPI. According to the literature, the effect on HPI of the *totaltax* is unsurprising, however, the effects can be minimized by policymakers' offsetting mechanisms and laws as well as the assessor's report on each property.

What's most surprising, and somewhat concerning, is the negative correlation between the log of the *edutax* and the log of *hpi*. The literature claims that increases in the local education tax rate should increase the value of housing since the education quality should improve with an increase in funds. Figure 2.1 shows that the log of the edutax has an inverse relationship with the log of the HPI. This directly conflicts with the literature and what residents expect of their local government. This might break the confidence that residents have in their local officials' ability to efficiently allocate the funds to the school system. Survey data shows that a shockingly high (>30%) proportion of surveyed residents believe the quality of public services they receive is not reflected by their tax payments.

The public-school system is a public service that every resident, regardless of age or familial status, pays into. When paying over $1000 per month, in some New Hampshire towns, residents should expect increasing public-school quality (Graph A) with increases in funding as more resources can be made available and a greater variety of programs can be offered. The literature expects the same results; as there is an increase public-school quality an increase in demand for attending those schools will follow, hence the increase in the quantity demanded of local housing (Graph B). People move to, and continue to reside in, the towns whose school quality is exceptional so an increase in the quantity demanded for local housing should cause an increase in the local value of property (Graph C).




There are 221 towns in New Hampshire and 167 school districts which means there are cooperative and regional schools that support multiple towns in one system. These setups allow town residents, with relatively lower financial capacity, to send kids to school and receive necessary program support without a significant financial burden. Per Giertz and Mcguire (1992), the formula for local education funding relies on resident willingness to pay and presence of businesses and service providers who also pay taxes. A town with multiple restaurants, a greater population, and pharmacy may facilitate a lower local education tax payment than a town with no restaurants and a smaller population, because of its broader tax base. The benefits of high property taxes in cooperative and regional systems are not exclusive to individual towns whose residents pay them but shared across the system. This local capacity is nondiscriminatory in nature because the proportion of families with children to single households is irrelevant for funding the local school system; however, it could explain the negative effect the test results show. My survey supports a positive correlation between the willingness to pay and education quality of the local school system, regardless of the town of residence. It's possible that homeowners buy into a cooperative or regional school system for the benefits of multiple towns of funding, or do so without prior knowledge or preference.

## 6 Data Analysis

This paper uses manually collected data, so this section will summarize some bias the dataset absorbed. First, with manual collection there's human error bias. There may be data points missing, or mistyped, during consolidation of the information. Second, availability of data is selective in that obtaining some complete data sources requires payment. This paper uses government collected data, so it should be free and available, however, 2010 US Census data is only available upon request. This obstacle made the panel set less wholistic than if free and accurate data were available by the FHFA and Strategic Initiatives Office of New Hampshire.

Finally, HPI values are subjective to state assessor reports. Their assessment of each property is subject to personal bias; there may not have been "enough" home improvement to increase the value, so the HPI average for the town decreases. Definitions for improvement adequacy are ill defined, and the increase or decrease in a property's value is completely subjective to who is purchasing. Officially the assessor decides what the property is worth, but someone's willingness to pay for the property is the deciding factor in the market.

Revenues can't be entirely dependent on the property tax if the relationship is not significant. Lutz (2008) holds that revenues lag by at least a year, so the revenues from an increase in the property taxes and values shouldn't significantly increase until the following year. This indicates that the assessor and policymakers might raise rates in accordance with inflation rather than out of necessity for revenue according to New Hampshire Association of Assessing Officials (2020).

## 7 Conclusion

New Hampshire, as of 2020, has higher average housing values (HPI) compared to Massachusetts, Virginia, Texas and Florida, and the US as a whole, even though most towns in the state lack proximity to major highways, exceptional school systems, and some public services such as water and sewerage. Survey data informs that the value comes from the consumer rather than the town or state itself.

The capitalization of the property tax in HPI incentivizes buyers and current residents in New Hampshire to consider individual town characteristics. Understanding the real relationship between the local education tax and local property values is imperative for smart homebuying especially when expectations about the education system are already set. If capitalization is a problem in New Hampshire, then surveys should collect data about resident willingness to pay specific to cooperative and regional school systems. When property taxes are similar among identical homes capitalization is benign; however, if there are differences and they are capitalized into market values then there's potential tax discrimination. This of course depends on the residents' tolerance of differences in taxation and the reflection of personal optimal bundles in their properties.

# 8 Appendices

## 8.1 Appendix A

**Figure 1.0**

| Metric | Mean | Std. Dev. | Low/High |
|---|---|---|---|
| Total tax | 19.87799 | 9.032707 | 0.1/42.66 |
| lt | 2.714721 | 1.011865 | |
| HPI | 172.6102 | 85.19617 | |
| Lp | 4.943313 | 0.8250156 | |

## 8.2 Appendix B

**Figure 2.0**

| lp | Coef. | Std. Err. | t | P> $\|t\|$ | [95% Conf. Interval] |
|---|---|---|---|---|---|
| lt | -0.0273277 | 0.0228702 | -1.19 | 0.232 | (-.0721976, .0175421) |
| | | | | | |
| year | | | | | |
| 2011 | -0.0155966 | 0.0849926 | -0.18 | 0.854 | (-.1823467, .1511536) |
| 2012 | -0.0468934 | 0.085006 | -0.55 | 0.581 | (-.2136698, .119883) |
| 2013 | -0.0506468 | 0.0849848 | -0.6 | 0.551 | (-.2173818, .1160881) |
| 2014 | -0.0790285 | 0.086317 | -0.92 | 0.36 | (-.2483772, .0903202) |
| 2015 | 0.0839698 | 0.0861248 | 0.97 | 0.33 | (-.0850017, .2529412) |
| 2016 | 0.226796 | 0.0859083 | 2.64 | 0.008 | (.0582494, .3953427) |
| 2017 | 0.1650386 | 0.0867172 | 1.9 | 0.057 | (-.0050951. .3351723) |
| 2018 | 0.2057475 | 0.0879298 | 2.34 | 0.019 | (.0332346, .3782603) |
| _cons | 4.965561 | 0.0845573 | 58.72 | 0 | (4.799665, 5.131457) |

**Figure 2.1**

| lp | Coef. | Robust Std. Err. | t | P> $\|t\|$ | [95% Conf. Interval] |
|---|---|---|---|---|---|
| lte | -0.0385674 | 0.0227663 | -1.69 | 0.091 | (-.0832335, .0060986) |
| | | | | | |
| year | | | | | |
| 2011 | -0.0121705 | 0.0910228 | -0.13 | 0.894 | (-.1907516, .1664106) |
| 2012 | -0.0444541 | 0.0911126 | -0.49 | 0.626 | (-.2232113, .1343031) |
| 2013 | -0.0441583 | 0.0940631 | -0.47 | 0.639 | (-.2287043, .1403876) |
| 2014 | -0.0818558 | 0.0955456 | -0.86 | 0.392 | (-.2693104, .1055989) |
| 2015 | 0.077 | 0.0880929 | 0.87 | 0.382 | (-.0958329, .2498329) |
| 2016 | 0.2266908 | 0.0794927 | 2.85 | 0.004 | (.0707311, .3826506) |
| 2017 | 0.1686728 | 0.0862428 | 1.96 | 0.051 | (-.0005303, .3378759) |
| 2018 | 0.2112235 | 0.0895485 | 2.36 | 0.018 | (.0355349, .3869121) |
| _cons | 4.972508 | 0.080416 | 61.83 | 0 | (4.814737, 5.130279) |

**Figure 2.2**

| | areg lp (coefficients / standard error) | | | |
|---|---|---|---|---|
| Variable | (1) | (2) | (3) | (4) |
| lte | -0.0385674 | -0.0369819 | -0.0373641 | -0.0407409 |
| | (0.0227663) | (-0.0226622) | (-0.022557) | (-0.0233921) |
| lt | | -0.0255131 | -0.0258222 | -0.0220444 |
| | | (-0.0206587) | (-0.0205042) | (-0.0217498) |
| lu | | | 0.0312869 | 0.0428565 |
| | | | (-0.0358082) | (-0.0381237) |
| lpe | | | | 0.0714063 |
| | | | | (-0.574429) |
| | | | | |
| 2012 | -0.0444541 | -0.042514 | -0.0426751 | -0.0320749 |
| | (-0.0911126) | (-0.0909946) | (-0.0910327) | (-0.0884744) |
| 2013 | -0.0441583 | -0.0441432 | -0.0397725 | -0.0271571 |
| | (-0.0940631) | (-0.0939008) | (-0.0942631) | (-0.0917512) |
| 2014 | -0.0818558 | -0.0797096 | -0.0692815 | -0.052022 |
| | (-0.0955456) | (-0.0959391) | (-0.0941017) | (-0.0913475) |
| 2015 | 0.077 | 0.0853177 | 0.1016386 | 0.1197824 |
| | (-0.0880929) | (-0.0885655) | (-0.0893217) | (-0.0861927) |
| 2016 | 0.2266908 | 0.2341669 | 0.2539565 | 0.2693818 |
| | (-0.0794927) | (-0.0800822) | (-0.0828988) | (-0.0785759) |
| 2017 | 0.1686728 | 0.1725375 | 0.1961169 | 0.2150768 |
| | (-0.0862428) | (-0.0865308) | (-0.0885941) | (-0.0857934) |
| 2018 | 0.2112235 | 0.2140408 | 0.2432547 | 0.262062 |
| | (-0.0895485) | (-0.0899321) | (-0.0960847) | (-0.0935631) |
| | | | | |
| R-squared | 0.2685 | 0.2693 | 0.2699 | 0.3 |

**Figure 2.3**

| lp | Coef. | Robust Std. Err. | t | P> $\lvert t \rvert$ | 95% Conf. Interval |
|---|---|---|---|---|---|
| lt | -0.0220444 | 0.0217498 | -1.01 | 0.311 | (-.0647228, .0206339) |
| lte | -0.0407409 | 0.0233921 | -1.74 | 0.082 | (-.0866418, .00516) |
| lu | 0.0428565 | 0.0381237 | 1.2 | 0.261 | (-.0319513, .1176643) |
| lpe | 0.0714063 | 0.574429 | 0.12 | 0.901 | (-1.055761, 1.198573) |
| | | | | | |
| year | | | | | |
| 2012 | -0.0320749 | 0.0884744 | -0.36 | 0.717 | (-.2056828, .141533) |
| 2013 | -0.0271571 | 0.0917512 | -0.3 | 0.767 | (-.2071949, .1528806) |
| 2014 | -0.052022 | 0.0913475 | -0.57 | 0.569 | (-.2312676, .1272236) |
| 2015 | 0.1197824 | 0.0861927 | 1.39 | 0.165 | (-.0493482, .2889131) |
| 2016 | 0.2693818 | 0.0785759 | 3.43 | 0.001 | (.1151971, .4235665) |
| 2017 | 0.2150768 | 0.0857934 | 2.51 | 0.012 | (.0467297, .3834239) |
| 2018 | 0.262062 | 0.0935631 | 2.8 | 0.005 | (.078469, .4456551) |
| | | | | | |
| _cons | 4.797908 | 2.905876 | 1.65 | 0.099 | (-.9041143, 10.49993) |

## 8.3 Question Responses

**Question 1: Approximately how much do you pay in property taxes per month?**

| Answer Choices | Responses | |
|---|---|---|
| $1-$500 | 20.73% | 17 |
| $501-$750 | 12.25% | 10 |
| $751-$1000 | 34.15% | 28 |
| $1000+ | 32.93% | 27 |

**Question 2: Do you know the break down of your property tax rate?**

| Answer Choices | Responses | |
|---|---|---|
| Yes | 68.29% | 56 |
| No | 31.71% | 26 |

**Question 3: Do you believe your town's municipal services reflect the amount you pay in property taxes?**

| Answer Choices | Responses | |
|---|---|---|
| Yes | 52.44% | 43 |
| No | 36.59% | 30 |
| Unsure | 10.98% | 9 |

**Question 4: *If answered yes to Q2* Would you change the break down of your property tax rate?**

| Answer Choices | Responses | |
|---|---|---|
| Yes | 13.7% | 10 |
| No | 41.1% | 30 |
| Unsure | 45.21% | 33 |

73 answered, 9 skipped

**Question 5: When home searching, did you consider the property tax rate?**

| Answer Choices | Responses | |
|---|---|---|
| Yes | 71.95% | 58 |
| No | 28.05% | 23 |

**Question 6: What is the top reason you moved to/still reside in the town you live in?**

| Answer | Responses | |
|---|---|---|
| Location | 25.61% | 21 |
| Avoid MA | 4.88% | 4 |
| School | 34.15% | 28 |
| Sentiment | 9.76% | 8 |
| Affordability | 3.66% | 3 |
| Employment | 3.66% | 3 |
| Quality | 9.76% | 8 |
| Other | 4.88% | 4 |

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# MINIMUM WAGE INCREASES BY BALLOT INITIATIVE: EFFECT ON RESTAURANT EMPLOYMENT AND EARNINGS

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**Abstract**

Fourteen states increased their minimum wages by law or by ballot at the start of 2020. In a perfectly competitive labor market, price floors like the minimum wage generate unemployment, but empirical evidence suggests a mixed effect, finding often that minimum wage increases have no significant effect on employment. One explanation for this disconnect could be that legislators avoid unemployment by raising minimum wages at times when the labor market is unlikely to respond with more unemployment. This paper examines this explanation by studying minimum wage increases by ballot initiative, a process that avoids any legislative input. Using a difference-in-differences research design on a 20% increase in the minimum wage in Maine by ballot in 2016, this paper finds that the increase had no significant effect on restaurant industry employment or earnings. These results suggest that minimum wage increases passed by ballot may be inferior to those passed by state legislatures, which similarly tend not to cut employment but generate significant increases in earnings for workers. (JEL Classification Codes: J30, J33, J38)

## 1 Introduction

The federal minimum wage currently lies at $7.25 an hour, unchanged since its increase to that amount in 2009. Viewing that wage as an insufficient floor, many states and localities have taken to increasing their local minimum wage for a variety of reasons, including to adjust for increases in inflation and costs of living over the years and to set a price for labor deemed high enough for a worker to earn a "fair" or "living" wage. According to the National Conference of State Legislatures, 14 states increased their minimum wage via legislation or ballot initiative this year, up from 10 in 2019 and 11 in 2018 (National Conference of State Legislatures, 2020). The textbook view of the imposition of a price floor like a minimum wage in the labor market is that such a control drives a wedge between demand and supply, creating some deadweight loss. In the labor market, an increase in the minimum wage, according to theory, drives perfectly competitive firms to demand fewer employees, increasing unemployment.

But labor markets are not perfectly competitive, and in spite of the theoretical underpinnings, empirical evidence on the impact of minimum wage increases on employment is mixed. Some studies bear out the effect predicted by theory, while others find inconclusive or negative effects on unemployment resulting from minimum wage increases. Card and Krueger (1994) and Neumark and Wascher (1995), two early studies in modern minimum wage literature, make use of a difference-in-differences research design and reach differing conclusions about the direction and size of the elasticity of employment with respect to minimum wage using different datasets related to the same event study, a minimum wage increase in New Jersey in 1993. A third replication study using a changes-in-changes research design found CK's conclusion held for small fast-food restaurants, but reversed for large fast-food chains

(Ropponen, 2011). Clemens and Strain (2018), using American Community Survey data, found that relatively large increases in the minimum wage generate a small but negative elasticity of employment with respect to minimum wage. What might explain this divergence from theory? A few explanations come to mind. The "real world" labor market is not perfectly competitive, and firms enjoy some monopsony power, meaning they are able to pay employees below their marginal productivity, with the remaining revenue accruing to the firm as profit (Caldwell and Naidu, 2020). A minimum wage increase under these conditions could result in higher earnings per worker with no adjustment in overall employment, vindicating the results found in minimum wage literature. Another explanation is that firms push the costs of minimum wage increases onto consumers by increasing prices (Basker and Khan, 2016). Finally, one last explanation could be that legislators, observing inelasticity in the demand for labor in an imperfectly competitive market and expressing a preference for greater equity, increase the minimum wage at opportune times – e.g., during a tight labor market – to avoid any adverse effect on employment and subsequent political backlash, creating the results observed in some studies.

But state legislatures are not the only driving factor behind minimum wage hikes. Some state constitutions allow their citizens to introduce ballot initiatives ushering in minimum wage increases, and they have done so roughly 20 times at the statewide level over the past two decades. This occurred most recently in Florida during the 2020 general election, where state residents voted to raise the hourly minimum wage to $15 an hour. State minimum wage increases enacted via ballot bypass the deliberative process of the state legislature and are instead approved by voters, who often "substitute a variety of cues for detailed policy information" like elite endorsement and identity of initiative sponsors when casting a ballot (Boehmke and Patty, 2007, p. 98). As a result, minimum wage increases passed via the initiative process may not be as carefully timed or coordinated to coincide with appropriate market conditions as are legislatively approved increases.

To investigate whether the results of minimum wage increases by initiative differ from those by legislative statute, I follow in the footsteps of Card and Krueger (1994) and use a difference-in-differences approach to study a similar event. Maine voters approved an hourly minimum wage increase from $7.50 to $9.00 (with subsequent $1.00 annual increases to $12.00) in 2016, and this study examines the effect of that event using New Hampshire counties bordering Maine – which have a state minimum hourly wage set at $7.25 – as a control group. My study focuses on restaurant workers, identified in literature as disproportionately minimum wage earners. I found statistically insignificant evidence that the Maine minimum wage hike had a negative effect on full-service employment and insignificant evidence for a positive effect on partial-service restaurant employment and earnings and full-service restaurant employment in Oxford County, Maine. The data show statistically insignificant evidence that the wage hike had a negative effect on partial-service restaurant employment and insignificant evidence for a positive effect on full-service restaurant employment and earnings and partial-service restaurant earnings in York County, Maine. Thus, Maine voters' attempts to increase the minimum wage led to neither an appreciable uptick in earnings for restaurant workers nor a discernable change in employment for restaurant workers, when comparing against earnings and employment changes in New Hampshire restaurants along the border with Maine in the same time period.

The result merits further investigation before a conclusion can be drawn about the efficacy of ballot-initiated minimum wage increases relative to ones passed by state legislatures. Further data on hours worked or on the distribution of restaurant jobs by earnings would enable a thorough inquiry on what effect, if any, the minimum wage increase had on the labor market by revealing the relevant adjustment mechanism in the labor market for restaurant workers. Given the values for average hourly earnings pre- and post-treatment in Maine counties for restaurant employees and prior literature, it seems plausible that the firm response to an increase in the minimum wage, provided it was an effective price floor, was a change in payroll hours, but more investigation is needed to make a conclusion.

The rest of this article is structured as follows. Section III reviews the literature on the effect of minimum wages on employment and earnings. Section IV discusses the process to determine the treatment and control groups used in this study and provides a summary of the data. Section V lays out the difference-in-differences model used in this study. Section VI discusses the results of the regression and includes a discussion on the results and robustness checks. Finally, Section VII concludes with further lines of inquiry.

## 2 Literature Review

Modern minimum wage studies begin with Card and Krueger (CK, 1994), who conducted surveys of fast-food restaurants in western New Jersey and eastern Pennsylvania and employed a difference-in-differences research design to quantify the effect on employment of New Jersey's 1993 increase in the state hourly minimum wage from $4.25 to $5.05. CK reasoned that Pennsylvania's border counties with New Jersey could serve as a control group, since they remained at a $4.25 hourly minimum wage. They used New Jersey's border counties with Pennsylvania as the treatment group to isolate the effect of the minimum wage specifically by modeling potential outcomes in New Jersey in the absence of the wage increase using Pennsylvania's trends. They ultimately found that average starting earnings in New Jersey fast- food restaurants jumped a statistically significant 10%, while employment in those restaurants increased relative to Pennsylvania's – but not to a statistically significant degree. CK checked the validity of Pennsylvania's counties acting as a control group by examining trends among higher-paying restaurants in the sample counties. They found that those restaurants, largely unaffected by the minimum wage hike, experienced changes in employment in roughly the same size and direction, supporting the idea that the sample counties had similar underlying conditions. This paper uses CK's difference-in-differences methodology to study minimum wage differentials.

Shortly after Card and Krueger (1994) published, Neumark and Wascher (NW, 1995) reevaluated their results and found drastically different results. NW observed that the summary statistics from CK's dataset revealed that fast-food chains experienced surprisingly large swings in full-time equivalent employment, leading them to investigate their conclusions by other means. Using payroll records obtained from fast-food restaurants in CK's sample region rather than telephone interviews, which NW found to be a less variable dataset, NW found that, in contrast to an insignificant increase in employment, New Jersey's hourly minimum wage hike of roughly 18.8% led to a statistically significant decline in employment of 4.6%, and elasticity of employment with respect to minimum wage of -0.24. To avoid the issues presented in CK's dataset, this study will rely on data sourced from employers participating in state unemployment insurance programs, a representative sample of employers surveyed by the U.S. Bureau of Labor Statistics, in place of author-designed surveys.

Dube et. al. (2010) is a critical next development in modern minimum wage studies. They attempt to answer why local event studies, like CK, often yield positive or near-zero estimates of the elasticity of employment with respect to minimum wage increases, whereas national-level aggregate studies of all state borders with minimum wage differences yields a clear negative elasticity of employment. Dube et. al. compile hundreds of local event studies, cross-border county pairs with a minimum wage difference, across the nation in a difference-in-differences design using data from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW). They ultimately find a small positive elasticity of employment, implying that the overall welfare gain of a minimum wage increase is positive, and minimum wage increases boost total earnings among fast-food employees.

Like Dube et. al., this paper will use QCEW data and analyze the same sub-industries identified as disproportionate employers of minimum wage employees, full- and partial-service restaurants, using North American Industrial Classification System codes, two six-digit subcategories (most specific category) under NAICS's five-digit restaurant classification.

According to the NAICS scheme, full-service restaurants are establishments where patrons receive waiter service and pay after eating, while partial-service restaurants are those where patrons typically pay before eating, whether food is delivered, eaten on the premises, or taken out. This paper excludes the remaining two six-digit NAICS codes under the five-digit restaurant classification – Cafeterias, Grill Buffets, and Buffets and Snack and Nonalcoholic Beverage Bars– because rates of self-service among establishments in this category obscure the effect of an increase in the minimum wage on hired labor. This paper will also replicate the cross-border county pair structure by comparing two cross-border county pairs, one pair in the less densely populated northern side of the border between Maine and New Hampshire, and one pair in the southern, more densely populated border region.

## 3 Data

As stated earlier, this paper will use earnings and employment data from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages. In selecting states and counties to examine in this study, I looked at minimum wage increases passed by ballot in the last two settled election cycles, 2016 and 2018, which yielded six events in Arkansas and Missouri (2018) and Arizona, Colorado, Maine, and Washington (2016). Of these increases, both Arizona's and Washington's included provisions stipulating that employees are guaranteed paid family leave, which is likely to impact firm decisions on employment and earnings. These states were stricken from the sample.

Of the remaining four states, I selected Maine, with border counties in New Hampshire serving as the control. I made this decision based on data availability – to ensure that BLS does not release identifiable information, the agency chooses not to disclose data for low-population or low-density counties where employment and earnings data could be traced back to particular firms. As a result, I chose Maine's 2016 increase, as it is the state with the greatest population density at its border with neighboring New Hampshire. I do not anticipate any sample selection concerns resulting from the need to pick a population-dense border for this study. Given that this study uses a difference-in-differences model, an additional concern that could skew results is the presence of another law that enters effect at the same time as the minimum wage increase that could affect earnings and employment. The highest-profile law that entered effect at the same time was a legal marijuana law, which should not have a discernible effect on earnings and employment. I was unable to find any other laws that could interfere with my results.

**Table 1: Minimum Wages in Sample**

| | State hourly minimum wage ($) | | | | |
|---|---|---|---|---|---|
| | 2015 | 2016 | 2017 | 2018 | 2019 |
| New Hampshire | 7.25 | 7.25 | 7.25 | 7.25 | 7.25 |
| Maine | 7.5 | 7.5 | 9.0 | 10.0 | 11.0 |

Source: U.S. Department of Labor

Data for each county on the border of New Hampshire and Maine (six in total; four in New Hampshire and two in Maine) was obtained by downloading NAICS-Based Data Files By Area for the years 2016 and 2017 and aggregating data labeled with NAICS Codes 722511 and 722513, the six-digit codes for full- and partial-service restaurants respectively. Counties for which employment and weekly earnings data was not available for all quarters – BLS does not disclose data in the event it could compromise anonymity – were discarded from the sample, leaving four counties, two in each state, one pair in the northern part in the state and one in the southern part, remaining in the sample. The remaining employment and earnings data were averaged across the three months of each quarter, and in the case of weekly earnings data, divided by 40 to construct an hourly earnings variable, for ease of comparison to state hourly minimum wages. The data for each county-pair are displayed in Figures 1 and 2 in the Appendix. The data are non-seasonally adjusted, displaying peaks in employment in the third quarter of each year. State hourly minimum wage data was obtained from the St. Louis Federal Reserve Bank's Economic Data.

**Table 2: All-County Sample – Descriptive Statistics**

| Variable | Mean | St. Dev. | Max | Min |
|---|---|---|---|---|
| estabs | 153.64 | 109.54 | 21 | 310 |
| festabs | 175.63 | 107.84 | 48 | 310 |
| pestabs | 131.66 | 108.42 | 21 | 305 |
| femp | 3510.43 | 2705.54 | 513 | 7831.33 |
| pemp | 1516.34 | 1371.1 | 223.33 | 3978.67 |
| fearn | 10.13 | 1.51 | 7.5 | 13 |
| pearn | 8.05 | 1.21 | 6 | 10.98 |

Descriptive statistics calculated across 8 quarters of data, from Q1 2016 to Q4 2017.

Source: Bureau of Labor Statistics Quarterly Census of Employment and Wages

**Table 3: Variable Descriptions**

| Variable | Description |
|---|---|
| estabs | Number of restaurants in county |
| festabs | Number of full-service restaurants in county |
| pestabs | Number of partial-service restaurants in county |
| femp | Number of full-service restaurant employees in county, averaged over the three months of each quarter |
| pemp | Number of partial-service restaurant employees in county,averaged over the three months of each quarter |
| fearn | Hourly earnings among full-service restaurant employees in nominal dollars, constructed by dividing QCEW weekly earnings data by 40 |
| pearn | Hourly earnings among partial-service restaurant employees in nominal dollars, constructed by dividing QCEW weekly earnings data by 40 |

# 4 Model

Following Card and Krueger (1994), I use the difference-in-differences technique to isolate and model the effect of the minimum wage increase passed by ballot in Maine on employment and earnings. This method simulates an experimental research design using observed data by designating a treatment group – two counties on the western border of Maine – and a control group – two counties on the eastern border of New Hampshire. I will compare the change in average employment and earnings from 2016 to 2017, as the minimum wage change entered effect in Maine on January 1, 2017, across both county pairs. By first subtracting the difference in earnings or employment from 2017 to 2016 in the Maine counties and then reducing that amount by the difference in earnings or employment from 2016 to 2017 in the New Hampshire counties, I will isolate the effect of the minimum wage increase on employment and earnings. The regression equation for each cross-border county- pair's partial service restaurant employment is as follows:

$$\begin{gathered}\ln(pemp_{it}) = \alpha + \beta_1 * time_{it} + \beta_2 * treatment_{it} + \\ \beta_3 * time_{it} * treatment_{it} + \epsilon_{it}\end{gathered} \tag{1}$$

Where $time_{it}$ dummy variable that equals 1 for observations after the change in the minimum wage and 0 for all other observations and $treatment_{it}$ is a dummy variable that equals 1 for observations in the treatment group (i.e. counties in Maine) and 0 for observations in the control group (i.e. counties in New Hampshire). In total, I use four regression equations, substituting the $pemp_{it}$ variable above for the variables for partial-service restaurant earnings ($pearn_{it}$) and full-service restaurant employment and earnings ($femp_{it}$ and $fearn_{it}$). These four regressions will be once for the first county-pair, Carroll and Oxford counties, and once for the second pair, Rockingham and York counties. The regression coefficient $\beta_3$ is the coefficient of interest, the difference-in-differences estimator, in each of the models. The null hypothesis for this coefficient is $H_0$: $\beta_3 = 0$, the possibility that the change in employment or earnings resulting from the minimum wage increase is zero, and the alternative hypothesis is $H_A$: $\beta_3 \neq 0$.

# 5 Results

**Table 4: The Effect of Minimum Wage Increases on Employment and Earnings**

| | ln(pemp) | ln(femp) | ln(pearn) | ln(fearn) |
|---|---|---|---|---|
| Carroll and Oxford counties | 0.0627 | 0.0496 | 0.0318 | -0.0044 |
| | -0.533 | -0.66 | -0.832 | -0.966 |
| Rockingham and York counties | -0.0478 | 0.0085 | 0.0155 | 0.0304 |
| | -0.681 | -0.967 | -0.855 | -0.752 |

Robust standard errors in parentheses. Significance levels are ** 0.05 and *** 0.01.

I will begin the analysis by examining the effect on partial-service restaurants hourly earnings and employment of an increase in the minimum wage. According to the difference-in-differences model, the 20% increase in the minimum wage in Maine led to a statistically insignificant 3.18% increase in partial-service worker hourly earnings in Oxford County, Maine and an insignificant 1.55% increase in partial-service restaurant hourly earnings in York County, Maine. In other words, the increase in the minimum wage in Maine had no significant effect on partial-service worker earnings in Maine. The model also shows that the minimum wage increase had a negligible effect on employment – which may be expected given the insignificant elasticity of earnings shown above. According to the model, the 20% increase in the minimum wage affects a statistically insignificant 6.27% increase in employment in Oxford County, Maine, and a statistically insignificant 4.78% decrease in employment in York County, Maine.

Next, I will look at the treatment's effect on full-service restaurant earnings and employment. The model shows that Maine's 20% increase in the minimum wage led to a statistically insignificant 0.44% decrease in earnings and a statistically insignificant 4.96% increase in employment in full-service restaurants in Oxford County. The same increase led to an insignificant 3.04% increase in earnings and a statistically insignificant 0.85% increase in employment in full-service restaurants in York County. For all of these figures, the null hypothesis cannot be rejected – the minimum wage increase did not have a statistically appreciable effect on employment and earnings in full-service restaurants in Maine as well. These figures are less surprising than the previous set of results. Full-service restaurants include more upscale restaurants that offer higher wages than their partial-service counterparts, and one would expect the elasticity of worker earnings with respect to the minimum wage to be lower than that of partial-service restaurants because the minimum wage does not serve as an effective wage floor for a larger subset of full-service restaurants.

## 5.1 Discussion

The data show that, for partial-service and full-service restaurants in Maine, the increase in the minimum wage had no statistically significant effect on employment and hourly earnings statistically when taking New Hampshire border counties as a control. The employment result is not surprising when considering the negligible elasticity of earnings with respect to minimum wage this paper finds, which implies that firms have not experienced any significant changes in their labor costs (worker earnings), as a result of other potential adjustment mechanisms discussed below. With negligible changes in labor costs as a result of the increase in the minimum wage, firms have no cause to reduce the units of labor employed in the face of a minimum wage increase. While these findings are at odds with Card and Krueger (1994) and others, who find a demonstrable increase in earnings for fast-food workers in New Jersey following a minimum wage increase, other literature, outlined below, suggests that these data are well within the realm of previous findings.

One potential adjustment mechanism this study fails to capture for the minimum wage increase beyond employment and earnings is hours worked. In a minimum wage literature review, Brown (1999) suggests "hours per week fall when minimum wages increase, so the effect on hours worked is more pronounced than the effect on bodies employed." Stewart and Swaffield (2008), using an employer-based survey of employment and earnings in the United Kingdom, found that the introduction of a minimum wage reduced total paid working time by one to two hours. Michl (2000) found that the difference in the result Card and Krueger (1994) and Neumark and Wascher (2000) obtain using the same data set may be attributable to the fact that the latter study captures adjustment in hours worked, whereas the former does not, a point that Card and Krueger (2000) conceded as a potential driver of differential results.

The Quarterly Census on Employment and Wages does not provide data on payroll hours, but this variable could explain the discrepancy between my results and other findings in literature.Payroll hours are a compelling explanatory variable, especially considering that the hourly earnings post-treatment in partial-service restaurants in the Maine counties, constructed by dividing the weekly earnings by a fixed 40 hours a week, did not exceed $9.00, the value of the increased minimum wage (and in Oxford County, Maine, the same held true for full-service restaurants). Absent rampant illegal behavior with respect to the minimum wage law on the part of firms, the low hourly earnings indicate an unaccounted change in hours worked. Other explanations, like pushing costs onto consumers through increases in consumer prices, are less compelling. Were that to be the case, one would expect to observe a statistically significant increase in earnings driving the firm to increase prices on consumers to claw back revenue. To test this explanation, one would need to obtain data on payroll hours in the counties used in this analysis and regress it in Equation 1 above.

Another limitation of this study is the inability to examine the distribution of earnings as a result of the limited data available from the QCEW. In addition to payroll hours, a breakdown of employment by earnings would permit a bunching analysis to determine how exactly firms respond to an increase in the minimum wage by adjusting the number of employees working at each level of earnings, from which several more analyses are possible. For example, should this distribution reveal that jobs paying an hourly wage below $9.00 ceased to exist, and assuming restaurants passed on the costs of a higher wage rather than absorbing it, one could determine how the Maine minimum wage increase impacted hours worked.

Finally, demographic data of minimum wage-earners and data on the types of establishments constituting the NAICS codes used in this study, like the data sets used by Cengiz et. al. (2019) and Card and Krueger (1994) contained, may shed light on other adjustment mechanisms for the minimum wage increase. Using a difference-in-differences design on minimum wage increases enacted between January 2013 and January 2015, Clemens and Strain (2018) found that across all low-skilled earners, the effect of the minimum wage increase was statistically insignificant, whereas for teenage workers, the increase led to a statistically significant rise in earnings. Further decomposing the response to the minimum wage increase across several types of partial-service and full-service restaurants – like those in restrictive franchise contracts versus those in mom- and-pop shops – may also yield valuable information (Ropponen, 2012).

### 5.2 Robustness

Following the discussion above, to check the robustness of these results, data on the distribution of jobs at each income level and on hours worked in the restaurant industry in Maine and New Hampshire would permit an analysis to verify the adjustment mechanism for the minimum wage increase in Maine.

An essential component of any difference-in-differences study is analysis of the parallel or common trend assumption, which holds that in the absence of the treatment (e.g. the minimum wage increase), trends in the variables of interest in the treatment group (e.g. employment and earnings) would match those in the control group. There is no formal statistical test to verify this assumption, but this study uses cross-border county-pairs under the assumption that economic trends in border counties are relatively similar in size and direction to one another. Neumark, Salas, and Wascher (2014) dispute that neighboring areas constitute good controls, but Dube, Lester, and Reich (2010) find that border county pairs are more alike than non-adjacent pairs using the county-level Quarterly Workforce Indicators data set. Another common method to verify the common trend assumption is to visually inspect the data, but the relatively low number of periods over which the data in this experiment are observed and seasonality of the data make it difficult to visibly discern any pattern (data displayed visually in Appendix for each cross-border county pair).

With more employment and earnings data related to the restaurant industry that is unaffected by the minimum wage – e.g., data from high-end restaurants that paid employees well above the minimum wage both before and after the treatment period – we may be able to verify the parallel trend assumption. If it is observed that employment and earnings data at high-end restaurants follow a common trend in both New Hampshire and Maine, I would be more certain that the restaurant industry at large in both states followed the same trend, increasing the robustness of these findings.

## 6 Conclusion

This paper examines the effect of minimum wage increases passed by ballot initiative on worker earnings and employment through a difference-in-differences design that modeled a minimum wage hike by ballot initiative in Maine in 2016 as a treatment. I construct two cross- border county pairs between Maine and New Hampshire counties to use as a treatment and control group, respectively, while looking specifically at restaurant workers, who disproportionately constitute minimum wage earners. I find that the minimum wage increase in Maine had no statistically significant effect on full- and partial-service restaurant worker hourly earnings or employment, implying no net change in welfare resulting from the minimum wage increase.

Before making a conclusion about the overall impact of ballot-driven minimum wage increases versus those initiated and passed by lawmakers, further research is necessary to identify where the impact of the minimum wage increase fell. The literature suggests that data on hours worked by restaurant employees is an essential piece of this puzzle – firms often reduce payroll hours to avoid incurring costs imposed by minimum wage hikes, leading a negligible impact on earnings and employment for workers. Examining the distribution of jobs at each level of earnings for evidence of bunching could also provide more information needed to reach a firm conclusion about Maine's minimum wage increase.

Beyond the Maine-New Hampshire data collected in this study, greater availability of data and more time would permit a broader analysis of minimum wage increases passed by ballot. Finding available data on the two remaining New Hampshire counties unavailable through the QCEW data set would permit a comparison across the entire Maine-New Hampshire border, rather than only selected portions of it. Expanding this type of study to other states identified in Section IV as ballot-led minimum wage increasers (Arkansas, Colorado, and Missouri) would enable one to get a broader picture of the impact of these types of minimum wage increases specifically as well.

However, should the results in this paper hold, they imply that minimum wage increases passed by state legislators may be superior to those passed by ballot initiative. In prior case studies examining changes in the minimum wage driven by state legislatures, like Card and Krueger (1994), studies tend to find that such increases had a statistically significant positive effect on worker earnings and a statistically insignificant effect on employment, implying a welfare gain for workers. On the other hand, this paper finds that a ballot-driven minimum wage increase had a significant effect neither on earnings nor employment, implying no real welfare gain for workers. From the perspective of employers, both methods of increasing the minimum wage seem identical, but from the perspective of workers, wage increases passed via state legislatures lead to significant gains in earnings, ergo the superiority of increases ushered through by legislators.

# 7 Appendix

Figure 1: Employment in the restaurant industry in Carroll County, New Hampshire, and Oxford County,

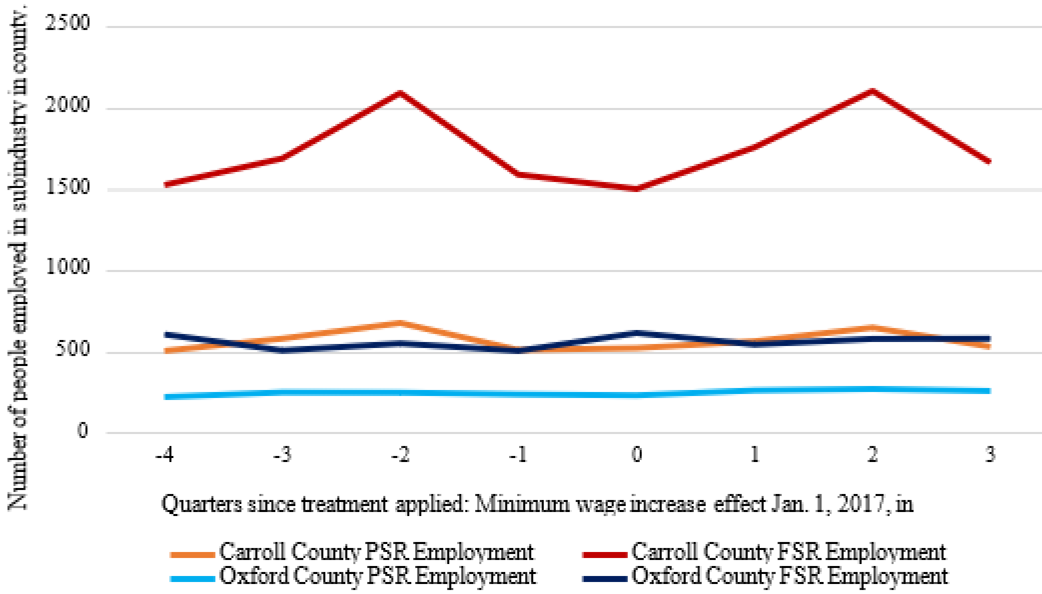
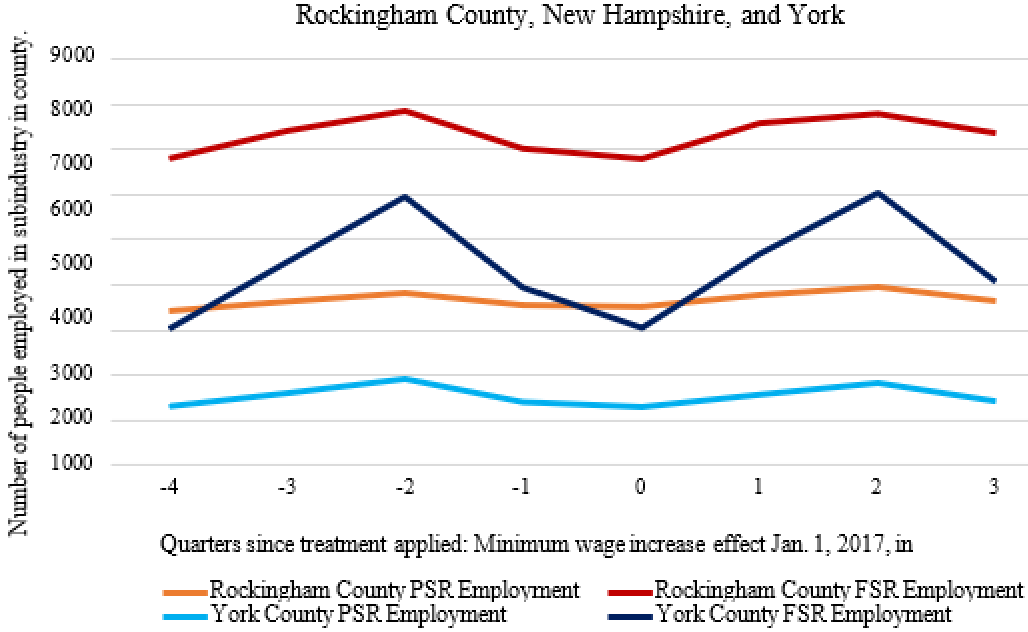



Figure 2: Employment in the restaurant industry in Rockingham County, New Hampshire, and York



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# KURZARBEIT BIS LANG-ARBEITER: DO EUROPEAN SHORT-TIME WORK PROGRAMS PREVENT LONG-RUN UNEMPLOYMENT INCREASES?

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**Abstract**

This research paper analyzes the impact of short-time work programs on changes in the unemployment rate of workers ages 25-54 in 17 European countries over a 10-year period. Short-time work programs share common characteristics, but differ by country in their eligibility and conditionality requirements and overall generosity. European trends in unemployment and firm-level inefficiencies evidence a need for programs that reduce inefficient separation during economic downturns. Through a differences-in-differences regression analysis that exploits variations in program use and take-up rate across European countries, this paper determines that an increased short-time work program take-up rate during periods of decreased output is associated with a statistically significant decrease in unemployment fluctuations. The research explores regional trends in unemployment and political and demographic trends in short-time work take-up, and concludes by proposing potential paths for future study. (JEL Classification Codes: J08, J64, J65, J68)

## 1 Introduction

After the economies of Europe finally began to stand on shaky legs after back-to-back economic crises, they were shoved under the market-crushing wave of a global pandemic. The new regulations of this uniquely challenging era require social distancing, work from home, and the lowest possible exposure to others– hardly a recipe for an economic boom. Market conditions have worsened as restaurants and small businesses crumble under the weight of the emptied marketplace. As short-term furloughs start looking increasingly like permanent layoffs, aggregate demand has undergone an agonizing recoil. A multi-billion dollar question seems to arise from the chaos: by what means can we smooth this demand shock out and prevent decades of painful recovery?

As economists and politicians alike scramble to find long-term solutions to massive exogenous shocks in the job market, many among them have locked in on a tried-and-true method of job preservation in Europe. Short-time work schemes, such as the Kurzarbeit system in Germany, have been utilized by dozens of European nations to tamper economic blows to the labor market. The short-time work system involves an agreement between the government and a qualifying firm during a time of temporarily decreased demand: the firm decreases the hours of workers rather than laying them off, and the government provides the funds for up to 100 percent of the worker's original paycheck (Hijzen and Venn 2011). The ultimate goal of the German Kurzarbeit system, as well as programs like it, is to keep aggregate demand afloat and prevent inefficient separations in the labor market. Of course, the appeal of such a program under pandemic conditions has not gone unnoticed. Analysts have lauded the program's ability to keep total employment up while keeping the number of on-site workers down in the industries that require physical presence. A number of researchers have analyzed firm-level data on the German Kurzarbeit system, cross-country studies on the swiftness of disaster adjustment, and the particulars of what makes a "good" short-time work system. However, there is a lack of literature addressing the central intention of the system – do short-time work programs actually reduce unemployment in the long run? While the COVID-19 crisis is still ongoing, the data to observe current trends is lacking. Luckily, there is comprehensive documentation of the rapid and dramatic increase in European short-time work use during and after the 2008 financial crisis. Utilizing this, we may explore the long-term effects of program use on fluctuations in unemployment and predict its potential impacts in contemporary applications.

This research paper seeks to determine whether employer take-up of short-time work programs has a statistically significant impact on unemployment in Europe during and directly following a period of economic crisis. The second section addresses the modern history of short time work. Section three utilizes simplified models of firm behavior to explain the labor market deficiencies that short-time work intends to remedy. Section four observes the previous literature on the subject at the firm, country, and international level. Sections five and six examine the construction and results of a panel data analysis utilizing a difference-in-differences model to measure the impact of short-time work take-up rate on percentage changes in the rate of unemployment; this section also features a subsection analyzing the demographic and policy-based trends in take up rate. Finally, section seven concludes with a discussion of the results and the potential extension of this analysis in future studies.

## 2 Key Features of Short-Time Work Programs

National short-time work programs are neither new nor exclusive to Europe. Germany has paid for short-time benefits during economic downturns since 1969, marking over half a century of program use to date (Abraham and Housemann 1992, 3). Since then, the implementation of short-time work has spread far beyond the German borders. The program was utilized heavily during the 2008 recession: at the onset, 18 countries of the 33 OECD countries operated short-time work programs, and that number rose to 25 by the end of 2009 (Cahuc and Carcillo 2011, 134). Each nation set rules, regulations, duration of short-time work, and compensation according to the needs of the market and the capacity of their respective governments. Nevertheless, European programs in particular share a number of features in common: they are intended to operate only in the short term, they require firms to meet particular eligibility requirements, and they provide government coverage of a portion of the employee's original paycheck. The major variances in short-time work programs stem from eligibility and conditionality requirements and program generosity.

According to data summarized by Hijzen and Venn in their seminal research on short-time work programs, a number of countries with short-time work programs have minimums and maximums on the reduction in hours worked (2011). Austria, Belgium, and the Netherlands have maximum reduction rates below 100 percent. The researchers explain that countries may set these limits in order to encourage greater spreading of the burden in hours reductions. In contrast, higher minimum reductions may be set in place by countries who want to prevent overuse of the program; a majority of nations implement a minimum number of hours for short-time workers

**Table 1: Minimum and Maximum Reductions in Weekly Working Hours for European Short-Time Workers**

| Country | Minimum | Maximum |
|---|---|---|
| Austria | 0.1 | 0.9 |
| Belgium | 0 | 1 |
| Czech Republic | 0 | 1 |
| France | 0 | 1 |
| Germany | 0.1 | 1 |
| Hungary | 0.2 | 1 |
| Ireland | 0.4 | 1 |
| Italy | 0 | 1 |
| Netherlands | 0.2 | 0.5 |
| Norway | 0.4 | 1 |
| Poland | 0 | 1 |
| Portugal | 0 | 1 |
| Spain | 0.33 | 1 |

Data obtained from Hijzen and Venn (2011).

Hijzen and Venn also collected data on variations in eligibility and conditionality requirements across countries with short-time work programs. Eligibility and conditionality requirements are the conditions in place that the firm must meet in order to qualify for enrollment in a short-time work program. Common eligibility requirements in OECD countries include a justification of need, a social partner agreement, and that participating workers be eligible for unemployment benefits. Common conditionality requirements include compulsory training, job searches for short-time workers, a firm recovery plan, and limits on firing short-time workers. More stringent eligibility and conditionality requirements are intended to screen candidates more heavily before entry into a short-time work program. Higher requirements necessarily result in less enrollment in short-time work programs, and requirements such as necessitating job searching may incentivize inefficient separation in the labor market (Hijzen and Venn, 2011). However, these requirements also mitigate the possibility of inefficient retention of workers. Firms are incentivized to adjust to decreased demand more efficiently and only use short-time work programs as a last resort. These requirements also have the potential to decrease moral hazard. Firms must commit to structural changes, such as planning for future recovery and training their workers; these costly commitments may serve to eliminate firms who seek to take short-time work funds in order to avoid eliminating inefficient practices or redundant workers during a recession.

In contrast to the limiting factors of eligibility and conditionality requirements, program generosity measures are designed to increase participation. More generous programs tend to be more expensive, as governments utilize primarily monetary incentives to encourage firm use. In Belgium, Denmark, Finland, Ireland, and Spain, firms bear none of the cost of hours not worked under short-time work (Hijzen and Venn, 2011). The cost of the program is also incorporated into the total compensation for short-time workers; countries with well-funded programs require a much smaller decrease in each participating worker's paycheck. Hijzen and Venn additionally note that more generous countries also tend to have longer maximum durations for short-time work. Program duration by country ranges massively, from 3 months in Slovakia to 36 months in Finland, and increases to duration can significantly impact the timeline for firm recovery. When it comes to being generous, however, countries tend to be limited by their pocketbooks. Nations with higher GDPs and more resilient economies are more capable of providing extra perks for firms to prevent unemployment rises. Countries must therefore determine how to best balance program expenditure with expected economic return, as extra spending may cover its own costs in the future through greater long-run output and stability.

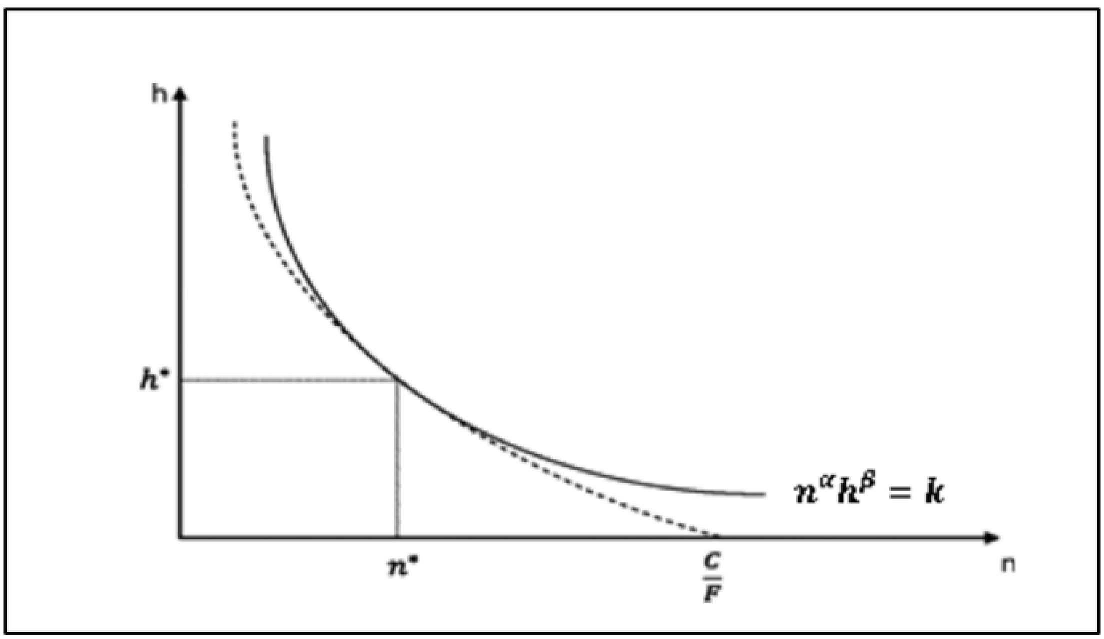
## 3 Understanding and Modeling the Goals of Short-Time Work

National determinations on program expenditures, program rules and regulations, and the various minutiae of design are hefty undertakings. The need for such programs must therefore be present and unaddressed to make such ventures worthwhile. This section undertakes the task of detailing the labor market gaps that short-time work programs are designed to fill, and how they are uniquely suited to doing so.

In Europe, the need for programs that retain workers is readily apparent. Ljungqvust and Sargent (1998) found that labor force participants experience massive losses of human capital over a sustained period of unemployment. This decrease in value decreases their potential earnings in turn, and in countries with robust welfare programs, the monetary impetus to return to the workforce is significantly diminished. Workers who had high previous earnings are especially resistant to returning to the workforce following the diminishment of their skills, and resultantly, their potential future earnings. In the case of a shock, the researchers find that economies with strong welfare programs recover less swiftly than those with lower unemployment compensation, resulting in longer term bouts of unemployment. This is because previously high earners may face such a stark decrease in their potential earnings that they stand to earn more by remaining on unemployment benefits. While the issue is particularly stark in countries with robust welfare programs, the researchers note that the same long-term discouragement trends can be found in nations with weaker social safety nets as the time out of work decreases human capital. The researchers note that former manufacturing workers are disproportionately represented among the long-term unemployed, since their skills diminish rapidly and decrease their potential future earnings. Short-time work programs may offer the solution to this by preventing the loss of human capital. In fact, short-time programs are most heavily utilized by manufacturing firms: Hijzen and Venn (2011) note that during the 2008 financial crisis, the average monthly short-time work take-up rate in the manufacturing sector reached 9 percent in selected years (p. 19). Short-time work is a particularly valuable mechanism, therefore, for firms that specialize in specialized skilled labor with high initial training costs. It is worth noting that while short-time work is disproportionately used by manufacturing firms due to the highly specialized skill sets within the industry, program use is not exclusive to this sector. The short-time work take-up rate in the construction sector and the agriculture sector rose by approximately three and two percent, respectively, during the financial crisis (Hijzen and Venn 2011, 17). Sectors related to services, on the other hand, rose by less than one percent over the duration of the crisis (Hijzen and Venn 2011, 19). This may be because the incentives to engage in short-time work programs are greater in the goods-producing sector, where particular skill sets require tailored training. The following models are based on firm behavior and contract design in the goods-producing sector to model the intended impact of short-time work on the firms most likely to benefit from the program.

The method by which short-time work programs can aid in retaining jobs can be best understood through models of individual firms during a crisis period. Boeri and Bruecker (2011) utilize an isolabor curve to explain the substitution effect, or ability to switch to a cheaper alternative during a time of increased expense, introduced by a national short-time work program (Figure 1). The firm's output can be produced through some number of workers, n, and some number of hours worked, h, in the production function $y = n^{\alpha}\ h^{\beta}$, where both hours and workers exhibit diminishing marginal returns. Labor costs are shown in the isocost function C=*n(F+wh)*, and the firm will obtain its target output level at the intersection of the two curves. In countries without short-time work programs, the researchers found that the optimal number of hours for a firm is independent of the target output; therefore, "changes in the scale of production affect the number of workers, but not the hours of work per employees (707)." In firms that do not utilize short-time work, employment adjustments are therefore concentrated at the extensive margin. Though firms may be indifferent to the form of adjustment, employees are not, and this discrepancy leads to job loss of the efficient outcome.

**Figure 1: Isolabor Curve for Firm Without Short-Time Work**

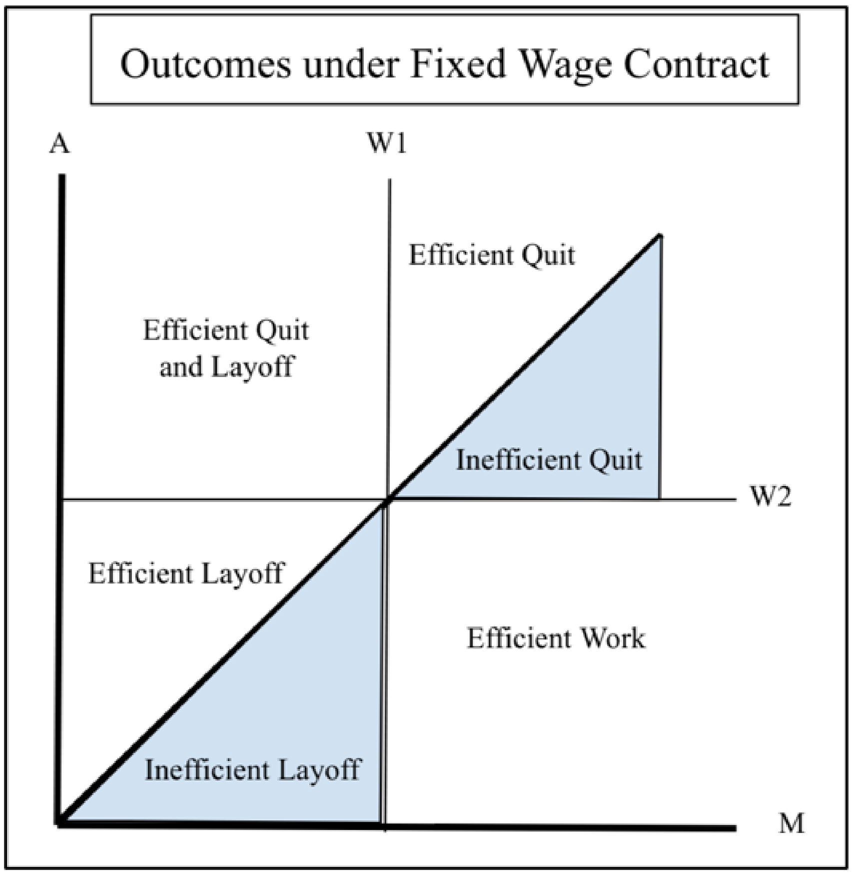



Obtained from Boeri and Bruecker (2011, 706).

In order to remedy this, the researchers derive that employers can be incentivized to reduce hours instead of workers by making labor costs more responsive to decreased hours. Short-time work programs accomplish this by covering more in wages for hours lost than the firm would pay along its isolabor function. Simply put, firms operating under short-time work programs receive a reduction in hourly costs per worker as an incentive to reduce the number of hours worked instead of the number of workers. Short-time work therefore biases the labor adjustment towards the intensive margin. This intensive adjustment is referred to as work-sharing, since the burden of reduced hours and wages is distributed across many employees in order to avoid reducing the number of workers.

Hall and Lazear (1984) further explore the inefficiency of layoffs in their simplified model of fixed-wage work contracts. In this model, long-term contracts are set on the basis of M, the profitability and overall success of the firm, and A, the conditions of the labor market outside of the firm. The model operates on the maximization of the equation E(XM + (1 - X)A), where X is a binary variable that takes the value of 1 when M is large and the value of 0 when A is large. Therefore, firm contracts attempt to ensure employment when the firm is doing well and allow for separation between the firm and the worker when the worker may have higher value opportunities in the greater marketplace. Work should therefore continue only when M¿A, or when the employee has a comparative advantage within the firm. The firm lays off the worker if the wage rate exceeds M, and the worker quits if A, their outside market earnings potential, exceeds their wage. However, this worker contract model holds the potential for both efficient and inefficient quits and layoffs. The lower left triangle represents an inefficient layoff where the worker is dismissed because the firm is doing poorly (M is low), but the market is faring even worse (A is lower than M). Such are the conditions of an exogenous demand shock such as a recession.

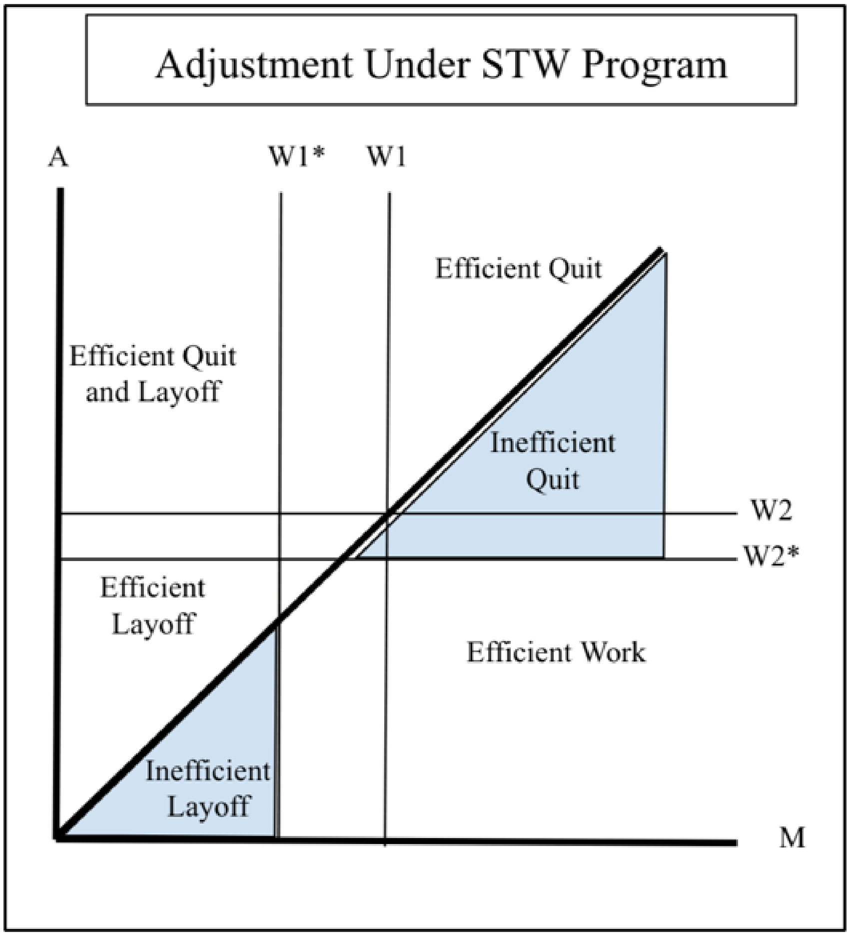
**Figure 2: Firm Outcomes under Fixed Wage Contract, No STW Program**




Model obtained from Hall and Lazear (1984, 12a).

Short-time work programs attempt to adjust the cost of keeping the worker in order to shift the fixed-wage work contract into the quadrant of efficient work. They do so by shifting the wage line leftward from the point of view of the firm. Because the firm must now cover less of the fixed costs of continued employment, they are incentivized to retain the worker until conditions improve. This has the potential to mitigate the losses of inefficient separation. From the point of view of the employee, their wage only decreases marginally. This has the potential to result in an inefficient quit, as the employee's wage may decrease more than their post-recession value to the firm; this depends on program generosity. The impact on reducing inefficient layoffs is designed to outweigh this trend. This design, however, depends on the restoration of productivity following the recession. If the firm is unable to restore itself after the recession, or if workers are preserved in this shift who may have been otherwise efficiently laid off, then short-time work programs may actually cause as many inefficiencies in the post-recessionary period as they solve in the short run.

**Figure 3: Firm Outcomes under Fixed Wage Contract, STW Program**




Model derived from Hall and Lazear (1984, 12a).

# 4 Literature Review

Existing studies of short-time work programs can be divided into two primary categories: those that examine the program of a single country, and those that compare multiple countries' programs. Single-country studies are often conducted at the microeconomic level, as researchers can collect more detailed information from local firms. This additional data can often provide more accurate quantitative conclusions regarding the number of jobs saved, the types of firms that participate in the program, and the direct national expenditures on the program. However, their results are often limited to the country under examination. Further, firm-level studies are often skewed by selection bias, since suffering firms are more likely to apply for short-time work programs; this may make the programs appear less effective than under a more controlled randomized experiment. Comparative analyses are usually much broader economic surveys. They are limited to the data available for all countries in the survey, so the researchers cannot make more particular assumptions about program design, individual firm participation, and national cost. What they lose in detailed analysis, however, they gain in overall applicability. The outcomes and potential caveats of six studies, three at the country level and three comparative analyses are explored below.

## 4.1 Country Level Studies

Balleer, et al. (2016) utilize German microeconomic data and a macroeconomic labor market model to study the stabilization effects of short-time work. The researchers determine through counterfactual analysis of the German labor market that short-time work schemes directly reduce firing, and by increasing the value of a job, short-time work may indirectly increase hiring. The results of the study determine that the presence of a short-time work program in the national economy reduces fluctuations in national unemployment by approximately 21 percent. The study separates the rule-based component of short-time work, which describes the automatic stabilization effect that more firms are eligible for the program during a recession, from the discretionary components, or specific features of eligibility and generosity that change frequently within the program. Particularly, programs tend to become more generous and lower their barriers to entry during economic downturns. The structural VAR and simulation of the labor market jointly determine that discretionary changes such as the introduction of more generous programs and lower barriers to entry for companies do not have a statistically significant effect on changes in unemployment. The final analysis determined that the short-time work system prevented a possible 1.29 percent increase in unemployment. However, the researchers acknowledge that German market pessimism during the bull economy and the relatively low cost of labor up to 2007 likely also played important roles in keeping unemployment low. The researchers further found that higher firing costs and union presence impacted the overall success of short-time work.

Efstathiou et al. (2018) composed a detailed microeconomic analysis on the short-time work trends in Luxembourg. From 2008-2009, and again from 2011-2015, Luxembourg had a sudden and rapid uptick in short-time work participation. Firms within the country heavily utilized the robust system to avoid hefty re-training costs and prevent the inefficient separation of productive workers during the crises. The authors utilized a survey that questioned Luxembourg firms on the qualitative and quantitative effects of short-time work involvement during the crisis period. The survey asked firms to identify how many individuals would have been laid off had the program been unavailable. The survey data determined that the program saved 4.3 percent of overall jobs during the peak months of the 2008 recession, and 4.9 percent of jobs during the Euro Crisis. The firms reported that approximately 25 percent of employees involved in short time work programs in 2008 would have lost their job without the program, and 20 percent of short-time workers' jobs were saved from 2011-2013.

Kopp and Siegenthaler (2017) utilize a differences-in-differences model at the firm level to observe the impact of short-time work take-up rate on unemployment in Switzerland. The authors recognize an inherent bias in firm-level studies towards companies more immediately in need of the program. This can thwart ceteris paribus comparisons by skewing the data towards companies that were struggling prior to the crisis period. Because more layoffs would be observed in these firms regardless, the measured impact of short-time work programs at the microeconomic level may appear lower. Due in part to this, the researchers find that macroeconomic country-comparison models tend to yield significant positive results between short-time work take-up rate and unemployment prevention, whereas firm-level studies often show little to no correlation. The researchers' results, however, provide very significant evidence that the short-time work programs prevented job loss in Swiss firms that were approved for the program. Notably, they discover that the direct fiscal benefits of program use, including reduced expenditure on unemployment benefits, may have been large enough to fully cover the cost of the program. The indication that the program paid for itself directly, excluding the indirect benefits of job security, prevention of inefficient separation, and aggregate demand benefits from the spending of retained employees, is very promising for future program design.

## 4.2 Comparative Studies

Abraham and Houseman (1994) are often credited as founders in the field of short-time work analysis for their utilization of a model of the dynamic demand for labor to compare the rate of structural adjustment in Germany, France, Belgium, and the United States. Their study conducts detailed measures of the impact of short-time work programs on the adjustment rate to shocks in demand. In order to collect the data for the three nations, the authors surveyed firms in the manufacturing sector to determine how the adjustments in hours would have differed without a short-time work program. They find that the German, French, and Belgian short-time systems all played an important role in national labor adjustment changes as a response to exogenous demand shocks. Their final results further suggest that job security protections like short-time work are compatible with labor market flexibility. The adjustment of hours is similar for countries with labor protections in place, though reactions to changes in output are much slower in more protected European countries than in the United States. Hijzen and Venn (2011) provide a thorough systematic evaluation of the impact of short-time work schemes on 19 OECD countries during the recession. The researchers use a differences-in-differences approach in order to gauge short-time work take-up rate impact on employment and hours worked over a two-year period. Hijzen and Venn opt to use full-time equivalents rather than total number of participants, since this gives them the explanatory power to calculate the number of jobs potentially saved by the program. They note a significant bias towards medium and large firms in the take-up rate, as these firms have more resources available to meet eligibility and conditionality requirements. The researchers analyzed the impact of short-time work programs on both employment and average hours. Their results provide statistically significant evidence that short-time work programs played a role in preventing unnecessary unemployment over the recession, though these benefits were almost entirely restricted to full-time workers. However, the researchers were not able to obtain post-crisis data given the time of publication, so they were unable to determine whether the preservation of jobs held out in the long run.

Cahuc and Carcillo (2011) utilize OECD data to determine the consequences of short-time work programs on unemployment, employment, and hours. The researchers extend the Hijzen and Venn model further by accounting more directly for the endogeneity in take-up rates across countries. They use a regression model to estimate the impact of the short-time work take-up rate on the unemployment rate of 25 countries. The research finds that a one-percent increase in the short-time work take-up rate results in a 1.24 percent decrease in percent changes in unemployment, which is statistically significant at the five-percent level. The paper confirms previous studies on the topic which identify the success of short-time work programs in preserving jobs and consistently keeping unemployment down. However, this study was completed before the long-term post-recession effects could be fully measured. As a result, the duration of time under study is unable to display longer-term trends. Because of the scale of the demand shock, it was also challenging for the researchers to isolate short-time work effects while controlling for the numerous and diverse state programs created to counter the crisis. Though the crisis provides a prime case study with regards to program uptake, the researchers warn that the overall effects of short-time work could be easily drowned out by the size of the demand shock.

For the purposes of this study, I have opted to conduct a comparative analysis of the impact of short-time work on unemployment. The comparative analysis allows for wider applicability of the results through observation of a large number of countries. This method will allow for the exploration of greater trends in short-time work implementation, rather than limiting the results to the particular demographic and programmatic features of one country.

# 5 Methodology

The diverse range of techniques and methods for measuring the impact of short-time work on unemployment provides ample resources to extend the methods of study into the long run. As such, I have opted to utilize a comparative differences-in-differences model to observe more widely applicable trends in short-time work. In order to observe the impact of short-time work on unemployment in the years following a period of crisis, I exploit the variations in take-up rate across European countries over the period under observation. The data set utilizes annual observations from 2005 to 2015 in order to capture the crisis and post-crisis effects of the program. Seventeen countries are observed in this regression model. Of these countries, 14 have short-time work programs: Austria, Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Norway, Portugal, Slovenia, and Spain; the Czech Republic, the Netherlands, and Poland all created short-time work programs during the crisis period (Cahuc and Carcillo 2011). Greece, Sweden, and the United Kingdom did not have national short-time work programs over the duration of the study. Hungary, Poland, Switzerland, and Luxembourg were removed from the study due to incomplete or insufficient data on national take-up rates.

For the purposes of this research, the take-up rate in countries without a national short-time work program is assumed to be zero. The United Kingdom did have firm-level programs that qualified statistically as short-time work models, but they applied to a small number of workers, operated as firm-specific schemes, and were not nationally implemented, and were therefore excluded from analysis. The model also includes only the data for workers ages 25-54. This excludes the potentially skewed impacts of both first entry into and final departure from the labor force. During recession periods, those entering the labor market are likely to become discouraged or depart from the labor market to receive more education, and older workers may be pressured to retire early. Elimination of those two groups should mitigate the distortive labor market shrinking effects of the recession.

This differences-in-differences analysis makes the necessary assumption that short-time work take-up rate prior to the crisis was 0. Some countries utilized short-time work programs prior to the crisis period, however these programs were not capitalized upon to a significant degree until the beginning of the crisis. Most use was restricted to particular firms facing circumstantial downturns, and these very small take-up rates did not reflect systemic economic downturns. The only major exception to this is Belgium, where the national short-time work program was utilized heavily prior to the crisis (Hijzen and Venn 2011). I therefore provide numerical analysis excluding Belgium alongside my initial findings.

The impact of short-time work take-up rate is estimated using the following differences-in-differences model, derived from Hijzen and Venn (2011), Cahuc and Carcillo (2011), and Angrist and Pischke (2014):

$$\begin{gathered}\ln(unempl.) = \beta_0 + \beta_1 STW + \beta_2 GDP + \beta_3 CRISIS * STW + \\ \beta_4 STW * GDP + \beta_5 CRISIS * STW * GDP + \\ \sum_{n=17}^{i=1} \beta_i COUNTRY_1 + \sum_{n=10}^{i=1} \beta_i YEAR_i + \sum_{n=17}^{i=1} [\beta_i (COUNTRY) * t] + u\end{gathered} \tag{1}$$

With the null hypothesis that $\beta_5 = 0$. This model is based on a traditional differences-in-differences multi-country analysis, though it has been extended to include interactions with a model of elasticity of unemployment with respect to output. STW denotes the natural logarithm of the annual short-time work take-up rate of workers ages 25-54 by country. CRISIS denotes a dummy variable which equals 1 from 2009 onward, as the financial crisis impacted Europe during the first quarter of 2009 (Hijzen and Venn, 2011). GDP measures the natural logarithm of annual gross domestic product. The coefficient on the interaction between the crisis dummy, the short-time work variable, and output ($\beta_5$) contains the differences-in-differences effect in the equation by allowing comparison between countries with and without a short-time work scheme directly following the crisis period. This interaction term denotes the impact of a short-time work program on the responsiveness of the unemployment rate to an exogenous output shock (Hijzen and Venn, 2011).

In order to ensure that a fixed-effects model is the most consistent and efficient estimator for the parameters, I conducted a Hausman test, which compared the data under fixed- and random-effects models. The Hausman test operates on the null hypothesis that the fixed- and random-effects models are efficient and consistent estimators of the variables. The test resulted in a chi-squared value of 44.54 and a p-value of 0.00, a resounding rejection of the null hypothesis. I therefore utilized a fixed-effects model to control for country and time effects in regression 1. The complete regression outcome for the Hausman test can be found in the appendix.

The model also includes a control for inflation based on annual percent change in national CPI (OECD 2020) in order to control for inflationary impacts on the GDP variable. A control for the percentage of employees with the right to collective bargaining also remains in the model – this is in part because this value was time-variant and thus not fully captured by the fixed-effects model, and in part because its inclusion isolates the impact of the take-up rate increase from the lower unemployment fluctuation trends in countries that have higher labor protections outside of short-time work. The original specification model also included dummy variables for eligibility and conditionality requirements; however, a majority of these variables (5 of the 7 parameters) were perfectly correlated with the controls for country effects. These variables were therefore removed from the final analysis.

# 6 Results

## 6.1 Comparative Analysis of All Countries

Selected results from the regression analysis are included in Tables 2 and 3. These results were selected based on their relevance to the research question. The complete regression outcomes for all variables can be found in the appendix.

**Table 2: Selected Results of the Full Regression**

| Variable | Coefficient | Std. Error | Sig. Level |
|---|---|---|---|
| STW ($\beta_1$) | -0.1905447 | 0.2397206 | 0.428 |
| Output ($\beta_2$) | -1.671641 | 0.1999584 | 0 |
| Interaction between the Crisis Dummy and STW ($\beta_3$) | 0.1936395 | 0.1173714 | 0.101 |
| Interaction between Output and STW ($\beta_4$) | 0.0144601 | 0.0194211 | 0.458 |
| Interaction between Output, Crisis Dummy and STW ($\beta_5$) | -0.0138531 | 0.0089011 | 0.122 |
| R-Squared | 0.9307 | | |

Data obtained from OECD Labor Force Statistics (2020) and Eurostat (2020).

The results in Table 1 indicate a somewhat weak statistical correlation between the short-time work take up rate during a crisis and the percent change in unemployment. The data indicates that a one-percent increase in the short-time work take-up rate in response to an exogenous decrease in output is associated with a 0.013 percent decrease in the rate of change of unemployment. This suggests that countries with short-time work programs may be slightly less sensitive to major shocks to output than countries without such programs. However, we cannot reject the null hypothesis $\beta_5 = 0$ at the 10 percent level. It is worth observing that this coefficient is significantly smaller than the results from shorter time periods. This may be because the longer duration under observation allows for adjustments in the marketplace unrelated to short-time work programs, including economic recovery measures and innovation of new industries in the place of those that did not survive the crisis period. The large and varied marketplace changes have the potential to overwhelm the impact of short-time work, which is designed to help only a small subset of qualifying firms.

It is not particularly surprising that the coefficient on the crisis dummy and the take-up rate ($\beta_3$) is positive. During times of crisis, there is simultaneously higher take-up and higher unemployment. Cahuc and Carcillo (2011) also found a significant positive correlation between unemployment and the take-up rate, and they attribute this outcome to the endogeneity of short-time work. The researchers obtained a negative correlation after adjusting for a number of instrumental variables, including the particulars of national employment policy.

**Table 3: Full Regression Excluding Belgium (STW6)**

| Variable | Coefficient | Std. Error | Sig. Level |
|---|---|---|---|
| STW ($\beta_1$) | -0.7631645 | 0.3219417 | 0.019 |
| Output ($\beta_2$) | -0.5635311 | 0.2382018 | 0.019 |
| Interaction between the Crisis Dummy and STW ($\beta_3$) | 0.31777 | 0.1606079 | 0.05 |
| Interaction between Output and STW ($\beta_4$) | 0.064412 | 0.0259538 | 0.014 |
| Interaction between Output, Crisis Dummy and STW ($\beta_5$) | -0.0239516 | 0.0121748 | 0.051 |
| R-Squared | 0.9156 | | |

Data obtained from OECD Labor Force Statistics (2020) and Eurostat (2020).

Removing Belgium from the observed countries significantly impacts the results. The results in Table 2 are more significant for every statistic besides the coefficient on changes in output. We can now reject the null hypothesis $\beta_5 = 0$ at the 10 percent level. This indicates that Belgium's heavy use of short-time work programs in the period before the crisis may make it incompatible with the necessary assumption that the pre-crisis take-up rate is 0. Because of this assumption, Belgium's take-up rate appears to increase rapidly after the onset of crisis rather than trending upwards for years prior to it, so the per-unit impact of short-time work may appear significantly lower in this country (Hijzen and Venn 2011).

## 6.2 Regional Analysis

In addition to the overall regression, I have included regionally divided regressions based on particular geographic clusters. These are designed to indicate whether certain regions had particularly different experiences with short-time work programs, or whether localized features may have potentially played a role in the outcome of short-time work schemes. These are not comprehensive, and they do not include every country in the original data set. They also do not intend to delineate any geographical or political relationship besides physical proximity. The Nordic and Baltic Countries data set contains the data for Finland, the Netherlands, Norway, Sweden, and Denmark. The Central and Southern Countries data set contains Austria, the Czech Republic, Germany, Italy, and Slovenia. The Northern and Western Countries data set contains Belgium, France, Ireland, Portugal, Spain, and the United Kingdom. Complete regression results can be found in the appendix.

**Table 4: Nordic and Baltic Countries**

| Variable | Coefficient | Std. Error | Sig. Level |
|---|---|---|---|
| STW ($\beta_1$) | 0.216865 | 4.295683 | 0.96 |
| Output ($\beta_2$) | -1.250107 | 0.8607548 | 0.158 |
| Interaction between the Crisis Dummy and STW ($\beta_3$) | -1.132582 | 2.029748 | 0.581 |
| Interaction between Output and STW ($\beta_4$) | -0.0230342 | 0.3421998 | 0.947 |
| Interaction between Output, Crisis Dummy and STW ($\beta_5$) | 0.0959497 | 0.1602168 | 0.554 |
| R-Squared | 0.9375 | | |

Data obtained from OECD Labor Force Statistics (2020) and Eurostat (2020).

**Table 5: Central and Southern Countries**

| Variable | Coefficient | Std. Error | Sig. Level |
|---|---|---|---|
| STW ($\beta_1$) | 0.1533106 | 0.236523 | 0.522 |
| Output ($\beta_2$) | 0.0736595 | 0.5790355 | 0.9 |
| Interaction between the Crisis Dummy and STW ($\beta_3$) | -0.1060238 | 0.2013859 | 0.603 |
| Interaction between Output and STW ($\beta_4$) | -0.0085481 | 0.0174208 | 0.627 |
| Interaction between Output, Crisis Dummy and STW ($\beta_5$) | 0.009436 | 0.01483 | 0.53 |
| R-Squared | 0.9564 | | |

Data obtained from OECD Labor Force Statistics (2020) and Eurostat (2020).

**Table 6: Northern and Western Countries**

| Variable | Coefficient | Std. Error | Sig. Level |
|---|---|---|---|
| STW ($\beta_1$) | 0.9220929 | 0.566124 | 0.112 |
| Output ($\beta_2$) | -1.380539 | 0.3375355 | 0 |
| Interaction between the Crisis Dummy and STW ($\beta_3$) | 0.312688 | 0.4833179 | 0.522 |
| Interaction between Output and STW ($\beta_4$) | -0.0645343 | 0.0418049 | 0.131 |
| Interaction between Output, Crisis Dummy and STW ($\beta_5$) | -0.0200633 | 0.0334387 | 0.552 |
| R-Squared | 0.9698 | | |

Data obtained from OECD Labor Force Statistics (2020) and Eurostat (2020).

The coefficient on the interaction term loses its explanatory power with the smaller data sets. No regional data set contains an interaction between output, the crisis dummy, and short-time work that rejects the null hypothesis at even the 50 percent significance level. These very low levels of statistical significance may in part be accredited to the use of annual data. Quarterly or monthly data, if available, may show clearer regional trends over the duration of study. The Northern and Western Countries data set also provides the only negative correlation between short-time work take-up rate and unemployment; this may be related to the statistical significance of output on unemployment in comparison to the other regional data sets.

While the regional analysis offers little information, most likely due to a lack of available data, it is notable that the impact of output on unemployment differs significantly by region. For the central and southern countries in the data set, the null hypothesis that the coefficient on unemployment is 0 cannot be rejected at any significance level; on the other hand, the Nordic and Baltic countries, as well as the Northern and Western countries, have more statistically significant impacts of output on unemployment. This may be due to a number of factors outside of this analysis, such as the reactivity of local and national governments. This information could have an effect on the efficacy of short-time work, as greater impacts of output on employment prior to the introduction of short-time work programs could cause the program-specific impacts to appear minimal in comparison.

### 6.3 Effects of Policy and Labor Demographics on the Take-Up Rate

The results in Table 1 and Table 2 indicate the possibility of a relationship between increases in the short-time work take-up rate and decreases in unemployment rate fluctuation. In this case, it is worth including additional information regarding factors that may influence national short-time work take-up rates. This section measures the impact of selected national policy and labor demographics on the take-up rate. The impact of labor demographics and national policies on the national short-time work take-up rate are measured through the following regression:

$$\begin{gathered} STW = \beta_0 + \beta_1 LABFOR + \beta_2 MANU + \beta_3 AGRO + \beta_4 SERV + \beta_5 UNION \\ \beta_i E_i + \beta_j C_j + u \end{gathered} \tag{2}$$

With the null hypothesis that $\beta_1, \beta_2 \ldots \beta_n = 0$ In this regression, LABFOR indicates the size of the national labor force, MANU, AGRO, and SERV refer respectively to the natural logarithm of percentage of the labor force in manufacturing, agriculture and service, and UNION refers to the percentage of workers with collective bargaining power. The variable Ei is a binary variable indicating the presence of an eligibility requirement, and Ci is a binary variable indicating the presence of a conditionality requirement. The eligibility and conditionality requirements under observation are listed in Table 7.

The eligibility and conditionality requirements were derived from Hijzen and Venn (2011, 10). Program generosity is not included in this regression because countries significantly altered generosity measures over the duration of study. The equation also controls for GDP and inflation differences across countries. Complete regression results can be found in the appendix.

**Table 7: Selected Results for Policy and Labor Demographic Effects**

| Variable | Coefficient | Std. Error | Sig. Level |
|---|---|---|---|
| LABFOR | -8.81e-05 | 5.48e-05 | 0.11 |
| MANU | 0.7740697 | 0.4122748 | 0.062 |
| AGRO | -0.6802087 | 0.3570352 | 0.058 |
| SERV | -9.147639 | 2.475301 | 0 |
| UNION | 0.0082835 | 0.0056844 | 0.147 |
| E1: Justification of Economic Need | -1.099707 | 0.2777664 | 0 |
| E2: Social Partner Agreement | 1.770524 | 0.3178318 | 0 |
| E3: Workers Must be Eligible for Unemployment Benefits | 0.9417821 | 0.2743175 | 0.001 |
| C1: Firms Must Create a Recovery Plan | 3.122205 | 0.3552393 | 0 |
| C2: Firms Cannot Dismiss Workers under STW | 2.016011 | 0.4534536 | 0 |
| C3: Job-Search Requirement for Employees | 1.820068 | 0.2863801 | 0 |
| R-Squared | 0.6785 | | |

Data obtained from OECD Labor Force Statistics (2020) and Eurostat (2020).

The results in Table 7 provide a number of insights. First, the size of the labor force has an extremely small effect on take-up rate. This indicates that countries with smaller populations in the labor force experienced equivalent percentage changes in take-up rate to larger countries. This may be because the take-up rate is far more heavily influenced by the percentage of the labor force in the manufacturing sector than the scale of the labor force overall: the results indicate a 0.77% increase in the take-up rate for every 1% increase in the percentage of workers in manufacturing. This matches the findings of Hijzen and Venn (2011), which indicated a significantly higher take-up rate in manufacturing compared to all other sectors. In contrast, higher percentages of workers in the service industry show a negative correlation with the take-up rate. This is likely because the current design of short-time work programs is far less suited to this industry, as detailed in Section III.

The results further indicate that the first eligibility requirement, justification of economic need, has a negative effect on take-up rate. This may indicate that this eligibility requirement creates a preliminary hurdle of eligibility that causes fewer firms to enroll in short-time work programs. However, the other eligibility and conditionality requirements appear to be positively correlated with the take-up rate. This may be because countries with more robust programs tend to also have well-thought rules and regulations on implementation. More research is needed at the microeconomic level to determine if this result indicates a failure of the requirements to control take-up rate, or whether it is the marker of a successful and well-designed program that efficiently selects suitable firms.

The outcome of this regression heavily indicates that the composition of the labor force with respect to the manufacturing sector and the eligibility and conditionality requirements of short-time work programs have the potential to impact the short-time work take-up rate. Further studies regarding this matter could include more complex measures of program generosity in order to explore the impact of increased expenditure on take-up rate. Such studies could also explore which firms in particular are most impacted by eligibility and conditionality requirements. The current results do not offer much clarity on whether the requirements prevent inefficient retention or serve as a barrier to entry for small firms.

# 7 Conclusion, Analysis, and Potential for Further Research

The results of this data analysis affirm previous researchers' conclusions about the effect of short-time work programs on diminishing fluctuations in unemployment. It appears, however, that the impact of the program is dampened in the long run. This may be because the scale of many countries' short-time work programs is not yet large and comprehensive enough to accomplish the desired results to the fullest extent over the long run. As a result, unemployment impacts that appear larger in the short run are drowned out by the quantity and scale of long-run recovery efforts. However, the statistical analysis indicates that encouraging more widespread take-up of short time work may aid in further smoothing shocks to unemployment. In order to increase the generosity and scale of short-time work programs, states would necessarily need to dedicate more funds and resources to program operation. However, these trends indicate that such investments have a potential payoff in the form of continued employment and prevention of inefficient separations. The analysis in section 6.3 indicates that particular requirements have the potential to decrease short-time work take-up rates, while others trend positively with take-up; however, these statistics fail to indicate whether the requirements are succeeding in efficiently selecting the best-fitting firms. Future studies could explore the impact of eligibility and conditionality requirements on preventing moral hazard. A firm-level study could explore whether these requirements effectively sorted responsible firms from those who would abuse the program to avoid eliminating costly internal inefficiencies. This study could examine whether costly commitments, such as making firms foot a larger part of the bill for hours not worked, succeed in efficiently selecting firms for the program. However, such a study should observe whether the requirements result in an adverse selection of larger firms with more resources to meet them, thus inefficiently eliminating smaller firms that may need more assistance.

There are ample opportunities for further study on this matter. Future researchers could potentially utilize this information to determine the explicit and implicit costs of a short-time work program, such as the potential for inefficient retention and increased expenditure on labor, alongside its explicit and implicit benefits, such as boosting aggregate demand through job retention and improving perceptions of long-term job security. With this information, the researcher could explore optimal program design based on a particular nation's budget, resources, and existing programs. Another interesting extension would involve tracking the spending habits of workers who were preserved under a short-time work program in comparison to socioeconomically comparable workers who were laid off. This research could indicate the real impacts of short-time work on aggregate demand, thus providing more clarity on the potential economic benefits of increasing program generosity. Additionally, someone exploring the possible downsides of the program could examine the difference in unemployment duration between countries with and without short-time work schemes. Keeping employees in their jobs during economic downturns may have adverse effects on the ability of unemployed workers to find new openings after the recessionary period.

In the next few years, the data on short-time work programs during and after the COVID-19 global pandemic should become available. This will provide countless opportunities for analysis, program design, and further extension of the research. Short-time work's focus on decreasing hours, and thus exposure, uniquely fits the health and safety guidelines of the pandemic in companies that cannot conduct operations from home. This will hopefully offer ample opportunity for comparative analysis against the outcomes of the 2008 recession. Though short-time work is not a labor market panacea, it has the potential to become an important tool for nations to endure economic downturns and preserve employee security at a low cost to the employer. Even beyond the unique conditions of a global pandemic, the world economy is becoming both increasingly interlinked and significantly uneven. I therefore believe that national policies that offer protection, stability, and consistency in the face of rapid fluctuations and changes should be at the forefront of research and analysis. In the face of another potential market downturn and instability in every facet of modern life, perhaps potential stability deserves increased attention.

# 8 Appendices

## 8.1 Appendix A: Complete Data Output from Regression Analysis

```
HAUSMAN TEST
hausman fixed5 rand5, sigmamore

                ---- Coefficients ----
             |      (b)          (B)            (b-B)     sqrt(diag(V_b-V_B))
             |     fixed5       rand5        Difference          S.E.
-------------+----------------------------------------------------------------
      CRISIS |    .6012894     .2903416        .3109478         .047289
       NTUR2 |   -.1693652    -.7491683        .5798031        .1696799
      logGDP |   -2.053967    -.5977981       -1.456169        .2225202
 CRISISxNTUR2 |    .0972804     .2588889       -.1616084        .0366504
 CRISISxlog~P |   -1.12e-08     7.57e-08       -8.69e-08        1.46e-08
 NTUR2xlogGDP |    .0165015     .0634578       -.0469563        .0142283
 CRISISxNTU~P |   -.0067164    -.0201934         .013477        .0028873
       UNION |     .002696    -.0021662        .0048621        .0009775
        INFL |   -.0665268    -.0888108         .022284        .0040768
          T1 |    .0649923    -.0418722        .1068645        .0166005
          T2 |    .0849767    -.1183878        .2033644        .0310507
          T3 |   -.4368684    -.3633772       -.0734912        .0115186
          T4 |   -.4710817    -.3821018       -.0889799        .0145996
          T5 |    .4568861     .2198765        .2370096        .0358186
          T6 |   -.0507486    -.0212952       -.0294534         .006497
          T8 |    .6346484     .3013697        .3332787        .0505709
          T9 |    .6144146     .2274978        .3869168        .0586189
         T10 |    .6328261     .1649129        .4679133         .070931
------------------------------------------------------------------------------
                          b = consistent under Ho and Ha; obtained from xtreg
            B = inconsistent under Ha, efficient under Ho; obtained from xtreg

    Test:  Ho:  difference in coefficients not systematic

                  chi2(7) = (b-B)'[(V_b-V_B)^(-1)](b-B)
                          =       44.54
                Prob>chi2 =      0.0000
                (V_b-V_B is not positive definite)
```

```
ALL COUNTRIES
regress logURATE CRISIS NTUR2 logGDP CRISISxNTUR2 CRISISxlogGDP NTUR2xlogGDP CRISISxNTUR2
> xlogGDP BEL CZ DEN FIN FRA GER GRE IRE ITA NET NOR POR SLO SPA SWE UK TBEL TCZ TDEN TFIN
> TFRA TGER TGRE TIRE TITA TNET TNOR TPOR TSLO TSPA TSWE TUK UNION INFL T1 T2 T3 T4 T5 T6 T
> 7 T8 T9 T10

       Source |       SS            df      MS     Number of obs   -      187
-------------+----------------------------------   F(50, 136)      -      59.18
       Model |   44.771975          50  .895439501   Prob > F      -      0.0000
       Residual |  2.05780573       136  .015130924   R-squared           -       0.9561
-------------+----------------------------------   Adj R-squared   -      0.9399
       Total |  46.8297808   186  .251773015   Root MSE          -      .12301

------------------------------------------------------------------------------
       logURATE |     Coef.   Std. Err.     t      P>|t|   [95% Conf. Interval]
-------------+----------------------------------------------------------------
             CRISIS |        0  (omitted)
             NTUR2 |  -.1905447   .2397206 -0.79   0.428  -.6646067       .2835174
             logGDP |  -1.671641   .1999584        -8.36   0.000    -2.067071   -1.276211
       CRISISxNTUR2 |   .1936395   .1173714 1.65   0.101   -.0384697      .4257486
       CRISISxlogGDP |   3.56e-08   4.57e-08        0.78   0.438   -5.49e-08      1.26e-07
       NTUR2xlogGDP |   .0144601   .0194211 0.74   0.458    -.0239463     .0528665
CRISISxNTUR2xlogGDP |  -.0138531   .0089011  -1.56   0.122  -.0314555      .0037493
             BEL |   22.34842   34.52953   0.65   0.519   -45.93583      90.63267
             CZ |  47.97684   33.97843    1.41   0.160   -19.21758      115.1713
             DEN |   -97.3589   33.61778   -2.90   0.004  -163.8401   -30.87769
             FIN |   9.104308   33.47927   0.27   0.786   -57.10297      75.31159
             FRA |  -1.503497       33.7087 -0.04   0.964  -68.16451      65.15752
             GER |   208.7159       33.5957        6.21   0.000   142.2783       275.1534
             GRE |  -130.5053   50.35582   -2.59   0.011  -230.087   -30.92362
             IRE |  -181.5428   34.78894   -5.22   0.000  -250.3401   -112.7456
             ITA |  -53.04505   34.60939   -1.53   0.128  -121.4872      15.39713
             NET |   -45.3418   33.78371   -1.34   0.182  -112.1511      21.46755
             NOR |  -75.65079   34.46601   -2.19   0.030  -143.8094   -7.492158
             POR |  -47.18713   34.89753   -1.35   0.179  -116.1991      21.82486
             SLO |  -114.4928       35.552 -3.22   0.002  -184.799   -44.18656
             SPA |   -139.048   35.54005   -3.91   0.000  -209.3306   -68.76538
             SWE |  -38.62706   33.99816   -1.14   0.258  -105.8605      28.60636
             UK |  10.99146   34.05369    0.32   0.747   -56.35179      78.33471
             TBEL |    -.010742      .017177 -0.63   0.533  -.0447106      .0232265
             TCZ |  -.0241741   .0168917   -1.43   0.155  -.0575785      .0092303
             TDEN |    .0483495   .0167247  2.89   0.004   .0152755       .0814236
             TFIN |  -.0047018   .0166587  -0.28   0.778  -.0376455      .0282419
             TFRA |    .0025338   .0167489  0.15   0.880   -.0305882      .0356558
             TGER |  -.1018528   .0167109  -6.10   0.000  -.1348996   -.0688061
             TGRE |    .0652076   .0250718  2.60   0.010   .0156265       .1147887
             TIRE |    .0903412   .0173198  5.22   0.000   .0560902       .1245921
             TITA |    .0280511   .0171748  1.63   0.105   -.0059131      .0620153
             TNET |    .0231505   .0167971  1.38   0.170   -.0100667      .0563677
             TNOR |    .0375086   .0171543  2.19   0.030   .003585 .0714322
             TPOR |    .0234647   .0173779  1.35   0.179   -.0109012      .0578306
             TSLO |    .0554446   .0177015  3.13   0.002   .0204388       .0904505
             TSPA |    .0707915   .0176489  4.01   0.000   .0358897       .1056934
             TSWE |    .0195246   .0169212  1.15   0.251   -.0139382    .0529873
             TUK |  -.0036933   .0169201   -0.22   0.828  -.0371538      .0297672
             UNION |   .0042221   .0020413        2.07   0.041   .0001852       .008259
             INFL |  -.0327611   .0124937  -2.62   0.010  -.0574681    -.008054
             T1 |   .0294662   .0465479    0.63   0.528   -.0625852      .1215175
             T2 |  -.0000836   .0563717    -0.00   0.999  -.111562       .1113949
             T3 |  -.0422713   .0868135    -0.49   0.627  -.2139503      .1294077
             T4 |    .044737   .0867517     0.52   0.607   -.1268199      .2162938
             T5 |   .3137613   .0799775    3.92   0.000   .155601 .4719216
             T6 |   .3077572        .106301        2.90   0.004   .0975406       .5179739
             T7 |   .3653925   .1158257    3.15   0.002   .1363401       .5944448
             T8 |   .4566178   .1126669    4.05   0.000   .2338123       .6794234
             T9 |   .4309627        .124812        3.45   0.001   .1841395       .677786
             T10 |   .4287798   .1394661   3.07   0.003   .1529771       .7045825
             _cons |   22.02896   2.460826        8.95   0.000   17.16252       26.89539
------------------------------------------------------------------------------
```

**NORDIC BALTIC**

```
. regress logURATE CRISIS NTUR2 logGDP CRISISxNTUR2 CRISISxlogGDP NTUR2xlogGDP CRISISxNTUR2
> xlogGDP DEN SWE NET NOR TDEN TSWE TNET TNOR UNION INFL T1 T2 T3 T4 T5 T6 T7 T8 T9 T10
note: T3 omitted because of collinearity

       Source |       SS            df      MS      Number of obs   =      55
-------------+----------------------------------   F(26, 28)       =      16.15
       Model |  7.28251682          26  .280096801   Prob > F        =      0.0000
    Residual |  .485697483          28  .017346339   R-squared       =      0.9375
-------------+----------------------------------   Adj R-squared   =      0.8794
       Total |  7.76821431          54  .143855821   Root MSE        =      .13171

------------------------------------------------------------------------------------
       logURATE |      Coef.   Std. Err.      t      P>|t|   [95% Conf. Interval]
-------------------+----------------------------------------------------------------
              CRISIS |  -.7530304   .7189456        -1.05   0.304  -2.225724      .7196629
              NTUR2 |.216865   4.295683    0.05   0.960   -8.582443    9.016173
              logGDP |  -1.250107   .8607548        -1.45   0.158  -3.013283      .5130695
       CRISISxNTUR2 |  -1.132582   2.029748  -0.56   0.581  -5.290332      3.025169
       CRISISxlogGDP |   6.55e-07   1.85e-06        0.35   0.726   -3.14e-06    4.45e-06
       NTUR2xlogGDP |  -.0230342   .3421998  -0.07   0.947  -.7239987      .6779303
CRISISxNTUR2xlogGDP |   .0959497   .1602168  0.60   0.554  -.2322395      .4241389
              DEN |  -118.5237   36.93069   -3.21   0.003  -194.1728   -42.87458
              SWE |  -28.13249   79.31772   -0.35   0.725  -190.6075      134.3425
              NETH |  -118.5237   36.93069  -3.21   0.003  -194.1728   -42.87458
              NOR |  -33.21239   60.49063   -0.55   0.587  -157.1218      90.69704
              TDEN |   .0589518   .0183729  3.21   0.003  .0213166       .096587
              TSWE |   .0142978   .0394693  0.36   0.720  -.0665515      .095147
              TNET |   .0589518   .0183729  3.21   0.003  .0213166       .096587
              TNOR |   .0163312   .0301868  0.54   0.593  -.0455038      .0781661
              UNION |  -.0054732   .0134243 -0.41   0.687  -.0329717      .0220252
              INFL |   .0542031   .0395047  1.37   0.181  -.0267186      .1351248
              T1 |  -.1841067       .113993 -1.62   0.118  -.4176107      .0493973
              T2 |  -.3125494   .1422915   -2.20   0.036  -.6040203   -.0210786
              T3 |          0  (omitted)
              T4 |   .4806426       .156196       3.08   0.005   .1606896      .8005955
              T5 |   .1400906   .1891849   0.74   0.465  -.2474371      .5276183
              T6 |   .547212   .1002566    5.46   0.000  .3418457       .7525782
              T7 |   .6187539   .1185247   5.22   0.000  .3759671       .8615407
              T8 |   .239769   .2406714    1.00   0.328  -.253224       .732762
              T9 |   .2418028       .26455  0.91   0.369  -.3001033      .7837089
              T10 |   .2796147   .2652687   1.05   0.301  -.2637637      .8229931
              _cons |   17.61919   10.37919       1.70   0.101   -3.641627      38.88
------------------------------------------------------------------------------------
```

**CENTRAL SOUTHERN**

```
regress logURATE NTUR2 logGDP CRISISxNTUR2 CRISISxlogGDP NTUR2xlogGDP CRISISxNTUR2xlogGDP
>  CZ GER ITA SLO TCZ TGER TITA TSLO UNION INFL T1 T2 T3 T4 T5 T6 T7 T8 T9 T10

      Source |       SS           df       MS      Number of obs   =        55
-------------+----------------------------------   F(26, 28)       =     23.61
       Model |   4.7232499        26  .181663458   Prob > F        =    0.0000
    Residual |  .215456913        28   .00769489   R-squared       =    0.9564
-------------+----------------------------------   Adj R-squared   =    0.9159
       Total |  4.93870682        54  .091457534   Root MSE        =    .08772

------------------------------------------------------------------------------------
       logURATE |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
--------------------+---------------------------------------------------------------
              NTUR2 |   .1533106    .236523        0.65   0.522   -.3311849      .6378061
             logGDP |   .0736595   .5790355        0.13   0.900   -1.112441      1.25976
       CRISISxNTUR2 |  -.1060238   .2013859 -0.53   0.603  -.5185441       .3064964
      CRISISxlogGDP |  -5.11e-08   5.99e-08 -0.85   0.401  -1.74e-07       7.15e-08
       NTUR2xlogGDP |  -.0085481   .0174208 -0.49   0.627  -.0442329       .0271368
CRISISxNTUR2xlogGDP |  .009436        .01483 0.64   0.530   -.0209419      .0398139
                 CZ |   82.33134   28.69333    2.87   0.008   23.55571       141.107
                GER |   192.7497     25.4361       7.58   0.000   140.6462      244.8532
                ITA |  -122.3213   32.79892   -3.73   0.001  -189.5069   -55.13575
                SLO |  -129.1904   40.40677   -3.20   0.003  -211.9599   -46.42086
                TCZ |  -.0406527   .0141792   -2.87   0.008  -.0696974   -.0116079
               TGER |  -.0957109   .0126357   -7.57   0.000  -.1215939   -.0698279
               TITA |   .0610735   .0159921   3.82   0.001   .0283152       .0938317
               TSLO |   .0646569   .0200915   3.22   0.003   .0235014       .1058124
              UNION |   .0050627   .0040627        1.25   0.223   -.0032593      .0133847
               INFL |  -.0611437   .0350469   -1.74   0.092  -.1329341       .0106466
                 T1 |  -.0724733   .0713467   -1.02   0.318  -.2186205       .0736738
                 T2 |  -.1737677   .0990699   -1.75   0.090  -.3767031       .0291678
                 T3 |  -.2137727   .1387956   -1.54   0.135  -.4980827       .0705372
                 T4 |  -.2557965        .170387 -1.50   0.144  -.6048184      .0932255
                 T5 |  -.0905396   .1409912   -0.64   0.526  -.3793469       .1982677
                 T6 |  -.0850442   .1631506   -0.52   0.606  -.4192431       .2491546
                 T7 |  -.0044973   .1648933   -0.03   0.978  -.3422659       .3332712
                 T8 |   .0418313   .1691804   0.25   0.807   -.3047192       .3883817
                 T9 |  -.0212102   .1979698   -0.11   0.915  -.4267329       .3843126
                T10 |  -.1108712   .2304809   -0.48   0.634        -.58299 .3612476
              _cons |   .1661936   7.127105        0.02   0.982   -14.43302      14.76541
------------------------------------------------------------------------------------
```

**NORTHERN WESTERN**

```
regress logURATE CRISIS NTUR2 logGDP CRISISxNTUR2 CRISISxlogGDP NTUR2xlogGDP CRISISxNTUR2
> xlogGDP TFRA TIRE TPOR TSPA TUK FRA IRE POR SPA UK

      Source |       SS           df       MS   Number of obs   =      66
-------------+----------------------------------   F(17, 48)       =     36.79
       Model |  13.5785493          17  .798738195   Prob > F        =    0.0000
    Residual |  1.04206993          48   .02170979   R-squared       =    0.9287
-------------+----------------------------------   Adj R-squared   =    0.9035
       Total |  14.6206193          65  .224932604   Root MSE        =    .14734

------------------------------------------------------------------------------------
      logURATE |      Coef.   Std. Err.      t      P>|t|    [95% Conf. Interval]
-------------------+----------------------------------------------------------------
            CRISIS |   -.110143    .2683323       -0.41   0.683  -.6496614      .4293753
             NTUR2 |   1.405406    .6515388        2.16   0.036   .0953993      2.715412
            logGDP |  -1.148658    .2951428       -3.89   0.000  -1.742082    -.5552334
      CRISISxNTUR2 |   .2335512    .6165224 0.38   0.706   -1.00605       1.473153
     CRISISxlogGDP |   6.41e-08    1.41e-07        0.46   0.651   -2.19e-07      3.47e-07
      NTUR2xlogGDP |  -.0923733     .047962 -1.93   0.060  -.1888074       .0040608
CRISISxNTUR2xlogGDP |  -.0154281   .0428518  -0.36   0.720  -.1015875       .0707312
              TFRA |   .0604195   .0149504  4.04   0.000   .0303596       .0904793
              TIRE |   .1330448   .0160844  8.27   0.000   .100705 .1653846
            TPOR |   .0523188   .0164399    3.18   0.003   .0192642       .0853734
              TSPA |   .1375144   .0142884  9.62   0.000   .1087856       .1662431
               TUK |   .0458918   .0146363  3.14   0.003   .0164635       .0753201
               FRA |  -119.0853   29.89755  -3.98   0.000  -179.1984   -58.97229
               IRE |  -268.0481   32.41307  -8.27   0.000  -333.2189   -202.8772
               POR |  -105.9333   33.06675  -3.20   0.002  -172.4184   -39.44815
                 SPA |  -274.4665   28.67121 -9.57   0.000  -332.1139   -216.8192
                UK |    -90.089   29.29248  -3.08   0.003  -148.9855   -31.19252
             _cons |   16.12078   3.847475    4.19   0.000   8.384917       23.85665
```

**POLICY AND DEMOGRAPHIC IMPACT**

```
. regress NTUR2 LABFOR logPERMANU logPERAGRO logPERSERV UNION INFL GDP E1 E2 E3 C2 C3 C4

      Source |       SS           df       MS      Number of obs   =       187
-------------+----------------------------------   F(13, 173)      =     28.09
       Model |  425.885612        13  32.7604317   Prob > F        =    0.0000
    Residual |   201.78447       173  1.16638422   R-squared       =    0.6785
-------------+----------------------------------   Adj R-squared   =    0.6544
       Total |  627.670082       186  3.37457033   Root MSE        =      1.08

------------------------------------------------------------------------------
       NTUR2 |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
      LABFOR |  -.0000881   .0000548    -1.61   0.110    -.0001963    .0000201
  logPERMANU |   .7740697   .4122748     1.88   0.062    -.0396664    1.587806
  logPERAGRO |  -.6802087   .3570352    -1.91   0.058    -1.384915    .0244972
  logPERSERV |  -9.147639   2.475301    -3.70   0.000    -14.03332   -4.261962
       UNION |   .0082835   .0056844     1.46   0.147    -.0029362    .0195033
        INFL |   .0202941   .0586164     0.35   0.730    -.0954013    .1359894
         GDP |   1.04e-06   8.25e-07     1.27   0.207    -5.83e-07    2.67e-06
          E1 |  -1.099707   .2777664    -3.96   0.000    -1.647954   -.5514593
          E2 |   1.770524   .3178318     5.57   0.000     1.143197    2.397851
          E3 |   .9417821   .2743175     3.43   0.001      .400342    1.483222
          C2 |   3.122205   .3552393     8.79   0.000     2.421044    3.823366
          C3 |   2.016011   .4534536     4.45   0.000     1.120998    2.911025
          C4 |   1.820068   .2863801     6.36   0.000     1.254819    2.385317
       _cons |   37.92422   10.94822     3.46   0.001     16.31493     59.5335
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# SMALL BUSINESS LENDING AND BANK CONSOLIDATION

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**Abstract**

This paper examines the relationship between market concentration in banking and the quantity of loans available to small businesses. Using data on origination of small business loans and deposit market share from the Community Reinvestment Act (CRA) and the Federal Deposit Insurance Corporation (FDIC) respectively, I evaluate the relationship between concentration and small business lending. This analysis uses panel data across all 50 states and D.C with entity and time fixed effects to control for variation across jurisdictions and business cycles. The results show a statistically significant negative relationship between the small business lending measure and bank market concentration. However, further research and data collection is needed to better understand this relationship. (JEL Classification Codes: G21, G28, G34, G38)

## 1 Introduction

Small business lending is an important engine for economic growth, and any threat to the availability of credit for small businesses could have negative consequences for the American economy. The negative consequences of limited capital access for small businesses include, but are not limited to, reduced business birth and survival rates, weaker competition among businesses, and higher local and national rates of unemployment. Many small businesses have historically relied upon their relationships with loan officers at local and community banks to secure access to credit. Therefore, when rules preventing banks from operating across state lines were relaxed and a wave of bank consolidation kicked off in the mid-1990s, researchers worried that it would lead to credit rationing affecting small businesses. As a result, this became a major empirical question for economists, many of whom feared that market power in finance and a decline in institutional relationships in lending would deprive American small businesses of capital.

In this paper, I employ a panel data analysis, examining data across all 50 states and the District of Columbia for every third year between 1997 and 2018. For this analysis, my dependent variable is the quantity of funds lent out in small business loans in thousands of real 2012 dollars (chained). They key independent variable is market concentration measured by the Herfindahl-Hirschman Index looking at deposit market share in banking. This novel approach allows me to examine the macroeconomic implications of trends towards market concentration at the state level with implications for small business capital access at national level. This approach differs from most of the existing literature because it is not limited to the microeconomic implications of mergers and acquisitions for a given bank's clients. For the purposes of this paper, I use the Community Reinvestment Act definition of small business loans, which is loans in amounts of 1 million dollars or less. This paper benefits from additional data and a longer period of study than much of the existing literature.

My findings lend support to the concerns raised above since they show a statistically significant negative relationship between higher levels of market concentration and small business lending. This may be because small banks are unable to sufficiently expand their small business lending operations to cover the gaps left by larger banks, or because there are barriers to new banks entering the market.

## 2 Literature Review

The bulk of the existing literature was written in the late 1990s when reforms to banking regulations kicked off a surge in mergers and acquisitions, both of which generated a great deal of interest in the effects of market concentration on lending practices. Researchers were concerned that bank consolidation would lead to credit rationing, a situation wherein the demand for credit outstrips the supply, driving higher interest rates and tighter lending standards, because larger banks that replaced small banks might not lend as much to small businesses as their predecessors. This could lead to negative impacts on economic growth because small businesses are crucial sources of employment and provide much needed competition for larger firms. Because small businesses are known to be dependent on bank capital to survive and grow, small business credit rationing could have severe consequences to the creation and growth of American small businesses. On the other hand, if large banks are able to achieve economies of scale and if a level of competition is maintained, bank consolidation could potentially have positive consequences by lowering rates and expanding lending.

## 2.1 Organizational Economies of Scale

The potential for bank consolidation to sustain small business lending is examined by Strahan and Weston (1996), who found that a decline in the presence of small banks will not adversely impact the availability of credit for small businesses. They based this conclusion on the fact that while small, independent banks maintained a larger portion of their loan portfolio in small business loans, many larger banks also engaged in significant small business lending. They also found that small banks involved in mergers expanded their small business lending portfolios, rather than reducing them. Additionally, some of the data examined during the period where banks were consolidating, found that small non-merging banks were expanding their portfolio of small business loans. These findings imply that bank consolidation does not result in a significant withdrawal from the small business credit markets by consolidating banks, and while some consolidating banks do reduce their participation in the market for small business credit, other small banks make up for the difference. It should be noted that the researchers in this article only examine the amount of small business lending, not differential loan interest rates or the specific loan recipients. This conclusion was also reached based on a relatively new data set and the researchers cautioned against taking too strong a message from their findings.

Research from Strahan and Weston (1998) examines the possible effects of economies of scale on small business lending using data covering the whole of the U.S. These researchers theorize that although the complexity of the organization might introduce diseconomies of scale, the superior ability to diversify loan portfolios will exert a countervailing force. They hypothesize that banks with more branches will have greater turnover among their loan officers as people are promoted or move between branches, disrupting the relationships upon which much of small business lending depends. Their findings indicate that as banks consolidate, they expand their small business lending portfolios until they reach a certain asset value threshold. After reaching this threshold, small business lending declines as a portion of total lending. However, this does not necessarily indicate that larger banks withdraw from small business lending, but rather that they expand lending to larger enterprises once they grow beyond a certain size and have greater lending capacity. Small banks are generally prevented from lending to larger firms because the size of the loans big companies need would strain regulatory requirements on bank portfolio diversification. Thus, while these researchers conclude that bank consolidation is not a threat to small businesses, they caution that their results consider only the actions of merging institutions. They do not analyze the response from the market as a whole.

Other researchers try to find evidence for diseconomies of scale by looking at the organizational structure of banks and how this influenced their lending practices. Similar to reasoning on the study of relationship lending, this research was based on the hypothesis that organizationally complex banks suffer a competitive disadvantage in small business lending and would therefore reduce their participation in this lending segment. A study by Keeton (1995) asked if large, multi-office banks would be less willing to lend to small businesses than the pre-existing small banks prior to the mergers. They found that branch banks and banks headquartered out of state tended to reduce the portion of their portfolios allocated to small businesses lending compared to the smaller banks. This finding is in line with the conclusions of Hendrickson and Rauch (2004) and Udell (1995), who found that larger banks do not engage in small business financing to the same degree as smaller banks. Their conclusion also has similar limitations in that it does not prove that bank consolidation will lead to reduced credit availability for small businesses. Because only consolidating and small banks were analyzed, many banks and financial institutions were not studied in this work. The researchers leave open the possibility that as banks continue to consolidate and reduce their role in the small business lending market, new entrants will rise to fill the void. However, if this does not occur, then it is likely smaller businesses would suffer from a lack of access to credit.

### 2.2 Relationship lending

Another issue examined in the literature is the types of loans being offered. It was suspected that small banks were better able to maintain a lending relationship with the businesses which borrowed from them, with firms being evaluated both on the objective information available and on the subjective judgement of the loan officer. This process is dependent on loan officers and other relevant bank officials serving for long periods of time so a relationship can develop. These more embedded relationships generate higher-quality information on lending risks and lending terms for a larger range of business types. Policymakers were concerned that larger banks would be unable to or uninterested in establishing strong lending relationships due to their moving and promoting employees more frequently. Larger banks would instead make "ratio" loans based on a set of predefined objective criteria, which tends to exclude newer and smaller businesses lacking a consistent track record or standard form of collateral.

The first type of lending popular among smaller banks, relationship lending, is beneficial to small business lending because it helps overcome the information asymmetries associated with small business lending. Udell and Berger (1995) found that borrowers with longer relationships with their lenders had better terms on their lines of credit because of these reduced information asymmetries. These researchers were careful, however, to caution that their findings did not prove that bank consolidation would limit credit availability to small businesses, even though small banks had a competitive advantage in small business lending relative to larger banks. This would suggest that as larger banks merged with or acquired smaller banks, they would reduce their participation in the small business lending market, but their place might be filled by other small banks.

This concern was validated by research from Hendrickson and Rauch (2004), who found evidence that consolidation led larger banks to reduce relationship loans with small businesses. This finding was drawn from data taken from the Federal Reserve's Quarterly Survey of the Terms and Bank Lending and was based upon changes in the interest rate markups charged by large and small banks. The researchers found that, as banks consolidated, they abandoned relationship loans in favor of ratio loans, and this was reflected in diverging interest rates by type of loan. As large banks abandoned relationship lending, demand for this service was driven to smaller banks and interest rates at those institutions rose as new loan demand exceeded supply of capital available at small banks. Meanwhile interest rates on ratio loans trended downward. This study did not rule out the possibility that given enough time, new small banks would enter the market or existing small banks would grow to fill the gaps and revivify relationship lending.

My study aims to look beyond the implications for individual firms and instead look at how trends in bank consolidation in each state have been associated with small business lending. While most of the literature on the subject was written before the 2008 financial crisis, this study examines a much longer time series and see how trends in small business lending have advanced since banking deregulations. Additionally, there were no studies found which aggregated data at the state level, focusing either on the national or local levels.

## 3 Data Description

### 3.1 Independent and Dependent Variables

This analysis makes use of data aggregated at the state level available through the Community Reinvestment Act and the FDIC. For small business lending, I chose to use total funds lent in a given year in each of the 50 states and the District of Columbia in real (chained) 2012 dollars. This data is available through the Community Reinvestment Act, which mandates that certain data on small business lending be reported and available through the Federal Financial Institutions Examination Council (FFEIC). This data is only available in thousands of current dollars and had to be converted to thousands of chained 2012 dollars for the purposes of this analysis. In order to study market concentration, I used data from the FDIC on deposit market share by institution.

The data in the FFEIC is not aggregated in a way which is easy to study. These data are reported on an annual basis and are available by MSA or county. The data was aggregated manually to derive state level totals. Previous studies on this topic have avoided using data from the CRA, preferring instead to use firm level survey data on mergers and acquisitions during a given period or national data on interest rates to infer the effects of market consolidation. These data allowed researchers to study the microeconomic consequences of mergers and to study the effect of market concentration on interest rates for small business loans. However, firm level data make it difficult to study the impact of consolidation on the quantity supplied in the market for small business credit. However, by using the data available through the CRA one can directly study the quantity of funds supplied on an annual basis.

I used the data described above for every third year starting in 1997 and ending in 2018. I chose to begin in 1997 because it is the first year data is available on the FFEIC website, and it also marks the start to a period of financial deregulation, which allowed many banks to consolidate across state lines. I aggregate the data to reflect the total quantity of funds lent in each state in each of the years over the time period, measured in thousands of real 2012 dollars.

My dependent variable, market concentration, is generally measured using the Herfindahl-Hirschman index (HHI), which is computed by adding the squares of the market share percentage of each firm in the market. To compute this value for each state, I use data available through the FDIC describing the market share of each FDIC insured financial institution in a given state and year. I chose to use deposit market share rather than share of loans extended for several reasons. The primary reason is feasibility, data on total assets and total lending by financial institution are not available at the state level. As such I am unable to compute measures of market share based on either of these alternatives. Data on lending is available at the state level only for certain kinds of lending such as small business lending. While it would be possible to compute market concentration in small business lending specifically, measuring market concentration this way would fail to provide insight on some of the issues discussed above in the review of the literature. It is suggested by Hendrickson and Rauch (2004) and by Strahan and Weston (1998) that as banks merge and grow, they will devote a smaller proportion of their funds to small business finance. If this is the case, then measures of market concentration based in small business lending will fail to capture the shift in overall market concentration. For these reasons, I choose deposit market share over the only other measure of bank market concentration available at the state level.

### 3.2 Control Variables

To control for other factors, I include a number of control variables in my analysis to better isolate the effect of market concentration on lending. In selecting my control variables, I chose factors which account for variation in small business lending between the states during the period of study. In service of this goal, I examine variables which impact economic conditions between the states and those which would account for some of the other differences between states and over years examined.

It is likely that demographic factors such as urbanization would influence lending amounts with rural areas potentially having different market conditions than urban ones. For this reason, state population density is included in order to capture the effect this might have on lending markets. This is done on the suspicion that population concentration might impact small business credit demand or supply in ways not captured by state economic figures alone. As shown in table 1 below population density varies considerably between states and this is likely to affect market dynamics. For instance, it is likely that dense cities have faster small business turnover or higher failure rates, which would lead to higher lending amounts as startups replace older firms. This data comes from RAND state statistics and by way of the Census Bureau and records the number of persons per square mile in each state.

I also include several variables linked to the health and size of the economy. To this end, I acquire State unemployment figures from RAND state statistics, and State GDP figures from the Bureau of Economic Analysts. These are included in order to isolate the impact that the overall size and health of the economy would have on lending, with larger economies having larger credit markets. In order to avoid endogeneity problems, I use State GDP from the previous year, and as such, I collect State GDP data every third year from 1996-2017. The percentage change in state GDP from the previous year is also included because I am interested in isolating the effect that economic growth would have on small business lending.

Unemployment is an important factor to include when examining small business lending markets because the price of labor is one of the largest costs faced by a small business. Thus, we would expect changes in the labor market to impact the expansion possibilities of small businesses, and therefore their demand for credit. There are several possible impacts that labor market conditions might have on small business, a high unemployment rate might mean reduced wage costs, improving the prospects for a given firm. On the other hand, high unemployment might result in reduced demand for the services of a given small business. I use the rate of unemployment as a percentage to avoid having my analysis confounded by the natural variation in the size of the workforce between states.

I also include other measures of local economic health related more specifically to small business. One such variable is the survival rate for businesses in a given state founded in the previous year. This data comes from the Department of Labor Statistics. The other variable of this type I include is the total non-performing loans for commercial banks in each state in the previous year, measured in thousands of 2012 dollars. I am including these variables to help isolate variations in economic conditions which specifically affected the small business lending market.

**Table 1: Summary Statistics**

| Variable | Mean | Std. Dev. |
|---|---|---|
| Quantity of Funds Lent in thousands of 2012 $ | 5009431 | 5494771 |
| HHI | 1062.51 | 862.33 |
| Population Density | 381.5719 | 1383.704 |
| State GDP in the Previous year in millions of 2012 $ | 290119.8 | 359560.5 |
| % Change in State GDP from previous year | 2.50925 | 3.7594 |
| Survival rate of businesses founded the previous year | 78.7069 | 3.0081 |
| State unemployment rate | 5.42598 | 2.018711 |
| Total value of non-performing loans at commercial banks in the state in thousands of 2012 $ | 2171740 | 6615299 |

# 4 Methodology

Previous analyses on the impacts of market consolidation in banking either rely on survey data covering firms engaged in mergers and acquisitions, as in the study by Strahan and Weston (1996) or use interest rates to make inferences about market dynamics as in Hendrickson and Rauch (2004). This carries several significant limitations, as discussed above in the literature review, since studying firms engaged in mergers cannot tell us how other firms react to changes in competitor behavior. In addition, studies which rely on interest rates cannot concretely infer the impact on the supply of credit. Although each of these methodologies are useful in analyzing different aspects of the lending market, they do not inform conclusions regarding the impact which market concentration has on the lending market as a whole. As outlined above, I decided to use a dataset which would allow me to study the relationship between market concentration and quantity of lending more directly than aforementioned methods. To this end, I employ a panel data analysis across all 50 states and DC in the period from 1997-2018 to determine the extent of the relationship between the variables identified.

## 4.1 Tests

The assumptions underlying panel regressions require the use of Heteroskedasticity and Autocorrelation robust standard errors. This is because the entities in the regression naturally correlate with themselves, and will otherwise confound the regression. I apply a test of overidentifying restrictions which is a test used to detect correlation between any of the regressors and the error term. It is used in panel data analysis to determine if fixed entity and time effects are necessary. The null hypothesis is that random effects should be used because there is no correlation between the regressors, and the error term and the alternative hypothesis is that fixed effects should be used to isolate this variation. This test strongly indicates that the fixed effects model is appropriate, finding that there is no systematic difference in coefficients with over 99% confidence. Thus, I use entity and time fixed effects by state and year for my regression model.

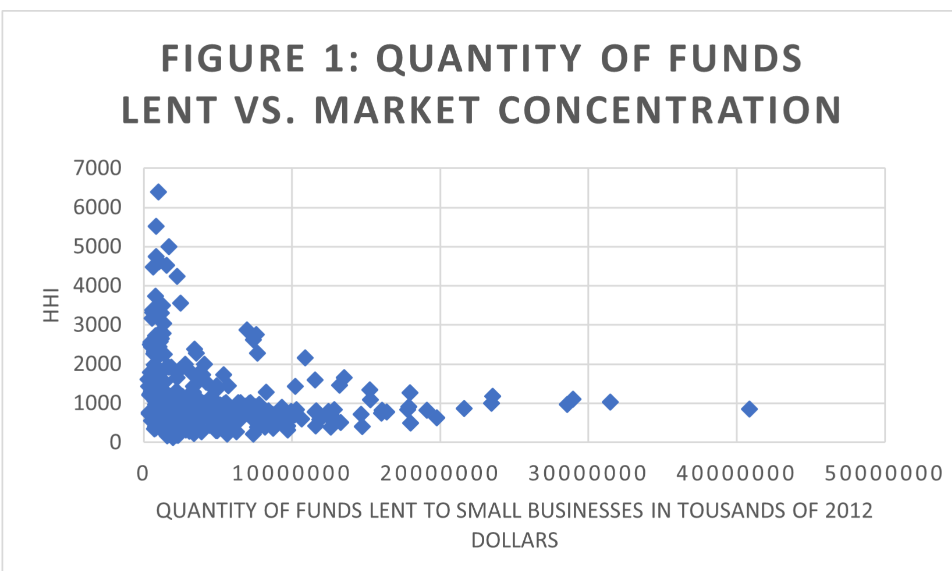
$$\begin{gathered}\text{Test of overidentifying restrictions: fixed vs random effects}\\ \text{Cross-section time-series model: xtreg re robust cluster(st)}\\ \text{Sargan-Hansen statistic 137.678 Chi-sq(7) P-value} = 0.0000\end{gathered} \tag{1}$$

## 4.2 Model Specification

Using the fixed effects regression model, my null hypothesis is that market concentration has no impact on lending to small businesses. The alternative hypothesis is that greater market concentration reduces the amount of funds available to small businesses. The equation for this regression is outlined below.

$$\begin{gathered}\ln(\text{Total Dollar amount of loans issued to small businesses in each state in a given year}) =\\ \beta_0 + \beta_1(HHI)_{it} + \beta_2(\ln StateGDP)_{it} + \beta_3(\%\text{Change in State GDP})_{it} +\\ \beta_4 + (\text{Population Density})_{it} + \beta_5(\text{State Unemployment})_{it}\\ +\beta_6 + (\text{Survival Rate of Buisinesses founded in the previous year})\\ +\beta_7(\ln \text{Total \$ value of Nonperforming Loans for Commercial Banks in each state}) +\\ +Y_1E_1 + Y_2E_2 + ... + Y_{n-1}E_{n-1} + \sigma_1 T_1 + \sigma_2 T_2 + ... + \sigma_{t-1}T_{t-1} + \epsilon_{it}\end{gathered} \tag{2}$$

The left-hand side of my model includes the amount of small business lending by state in real 2012 dollars, while the right-hand side will include the HHI of bank deposit market-share in that state. The right-hand side of the equation will also include the control variables: state GDP in the previous year, change in state GDP, state population density, and state unemployment rate. I chose to linearize my values for State GDP, quantity of funds lent, and value of nonperforming loans. This involves taking the natural log of each of these values before computing the regression. I am doing this in order to flatten the exponential curve observable in the data as shown in figure 1, so this will allow me to compare these variables more easily with others in my regression which did not exhibit this relationship. The difference can be seen in Figure 2 where the HHI is compared with the linearized data on quantity of funds lent. The beta values on each of these variables indicate the degree of influence each of them has on small business lending during that period, with a 1 unit change in one of the independent variables equating to a $\beta$ change in the dependent variable.




The entity fixed effects variables on the right-hand side of the equation will allow me to control for factors specific to each state which might influence small business lending, these entity fixed effects are denoted by $E_{1-(n-1)}$. State-to- state variations in public policy choice would naturally impact the market for small business credit given that much of bank regulation remained under state control to some degree. An example of state policy which we would expect to influence small business lending is that the state of North Dakota has a state-owned bank which supports lending to small businesses. These sorts of variations are naturally difficult to capture, the benefit of the fixed effects model is that it allows me to employ catchall entity fixed effect variables which capture these time invariant differences between the states.

The time fixed effects, denoted by $T_{1-(n-1)}$, allow me to control for variations in economic conditions between years which are uncorrelated with the regression terms. For instance, the global financial crisis of 2008 likely had a much larger effect on small business lending than what would be predicted based on the change in GDP given that the recession was driven by a collapse of the financial system. Indeed, since the economy varies significantly from year to year, it is important to make sure that the entity invariant changes in annual economic conditions do not impact the results of the analysis. By controlling for these variables, this model will analyze the variables of interest more effectively than previous studies. The results of this analysis are shown in Table 2 and discussed below.

# 5 Results and Analysis

**Table 2: Panel Regression Results for Linearized Data**

| Variable | OR (95%) CI | p-Value |
|---|---|---|
| Dependent: Linearized Quantity of Funds Lent Population Density | .0002151 (.00012003 .0003098) | **0.000 |
| Linearized State GDP in The Previous year | -.6317271 (-.9234497, -.3400045) | **0.000 |
| Percentage Change in State GDP from the previous year | -.0018497 (-.0129039, .0092045) | 0.738 |
| Herfindahl-Hirschman Index | -.000065 (-.0001248, -5.25e-06) | *0.034 |
| Business Failure rate | .0101588 (-.0006941, .0210118) | 0.066 |
| Linearized Real loan failures | -.0657994 (-.1239737, -.007625) | * 0.027 |
| State Unemployment Rate | -.01087 (-.0326639, .0109239) | 0.321 |
| Constant | 22.65695 (19.249, 26.06486) | **0.000 |

In my regression of HHI values over loan quantities between 1997-2018, controlling for: linearized state GDP in the previous year, change in state GDP, state population density, and state unemployment rate, business survival rate from the previous year, and quantity of non-performing loans, I find that the market concentration does have a statistically significant negative impact on quantity of funds lent at $\alpha = .05$. with a P $> |z|$ of .034. As shown in Table 2, the coefficient on the HHI term was -.000065, while the standard error was .0000298. The model explains a relatively high degree of the variation in the model with an $R^2$ of .73. This result is a rejection of the null hypothesis and lends support to the alternative hypothesis that a more concentrated banking sector causes a contraction in lending to small businesses. Although it is important to note that the effect I found is relatively small, with a shift from a competitive market with an HHI value of 1,500 to a more concentrated one around 2,500, resulting in only a 65$ drop in lending. This indicates that, while market concentration does affect small business lending, the effect is negligible.

Additionally, as shown in Table 2, my findings indicate a statistically significant positive relationship between population density and small business lending, significant at the 99 percent level of significance. This may indicate that urban areas have better access to small business lending than less densely populated areas. The results shown in Table 2 also show that while the business survival rate does not have a statistically significant impact on lending the quantity of non-performing loans does. The quantity of non-performing loans has a significant negative relationship with small business lending. This is relatively expected; indeed, it would be strange if past loan failures bore no relationship to new lending activity.

This result may indicate that as larger banks reduce their role in the small business credit market, smaller banks are unable to step up their lending operations to fill the void. Much of the literature I examine above finds that mergers and other actions which increase market concentration lead to those institutions reducing their small business lending. These studies are clear to point out, however, that economic theory would indicate that new firms would enter the market to replace those that exited. Whatever the reason, my finding does not seem to show this to be the case.

## 6 Conclusion

These findings raise questions about arguments advanced in some of the existing literature on concentration and small business lending. Strahan and Weston (1996) concluded that small business borrowers would not face credit constraints because consolidating institutions would not reduce their lending, and if they did, others would fill the void. My finding is more in line with the research of Keeton (1995), who found that larger banks would be less willing to lend to small businesses. This basic conclusion was shared by Hendrickson and Ruach (2004) as well as Udell and Berger (1995). Where my result differs is that these researchers were unable to draw conclusions regarding the market for small business lending as a whole because of their methodological focus on a few firms in specific situations. By looking at aggregate state lending data over time, I am able look at more general associations. My findings provide interesting evidence in support of the hypothesis that a more concentrated banking sector reduces the quantity lent to small businesses. This result may support the notion that there are diseconomies of scale in banking arising from organizational complexity, which reduces large bank's ability to engage in relationship lending, as suggested by Keeton (1995).

An important caveat is that these results do not conclusively prove that higher market concentration reduces small business lending. There are several variables which I would have liked to include where the data was not available. For instance, the existing level of debt held by small businesses in each state would likely have an impact on annual lending. However, these data were aggregated at the national level and not available at the state level. Importantly, while my findings show that bank concentration has a significant negative statistical association with small business lending, the coefficient value does not indicate a substantive effect.

Future research should build upon the work done in this paper by refining the model and adding more relevant variables to the regression. One of the major factors which limits the scope of the regression model employed in this paper is the lack of data available at the state level. There are many useful data sources maintained by the federal government which aggregate data only at the national level, particularly data relating to other measures of market concentration. This is unfortunate, both because a complete analysis of this issue would consider more variables, like interest rates, which are only aggregated nationally, and because many decisions regarding bank regulation are still controlled at the state level. State policy makers should have access to all of the data necessary to make decisions about issues which affect their states.

Future analyses should also consider using survey data and other sources to study the behavior of smaller banks during the late 1990s and 2000s. This would help to confirm the suspicion raised in this paper and by other researchers such as Hendrickson and Rauch (2004) that smaller banks will expand their lending in order to fill the void left as other banks consolidate. Studies may also focus on documenting the number of new banks started each year, in order to document the propensity for new entrants to fill the vacant niche left by merging banks.

# 7 Appendices

## 7.1 Appendix A: Stata Output

```
. xtreg lnrloan popd lnsgdpt pdsgdp hhi bfr sue lnrlf, fe vce(robust)

Fixed-effects (within) regression               Number of obs      =       408
Group variable: st                              Number of groups   =        51

R-sq:                                           Obs per group:
     within  = 0.2242                                         min =         8
     between = 0.8925                                         avg =       8.0
     overall = 0.7332                                         max =         8

                                                F(7,50)            =     15.34
corr(u_i, Xb)  = -0.9657                        Prob > F           =    0.0000

                                  (Std. Err. adjusted for 51 clusters in st)
------------------------------------------------------------------------------
             |               Robust
     lnrloan |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
        popd |   .0002151   .0000472     4.56   0.000     .0001203    .0003098
     lnsgdpt |  -.6317271   .1452397    -4.35   0.000    -.9234497   -.3400045
      pdsgdp |  -.0018497   .0055035    -0.34   0.738    -.0129039    .0092045
         hhi |   -.000065   .0000298    -2.18   0.034    -.0001248   -5.25e-06
         bfr |   .0101588   .0054033     1.88   0.066    -.0006941    .0210118
         sue |    -.01087   .0108505    -1.00   0.321    -.0326639    .0109239
       lnrlf |  -.0657994   .0289632    -2.27   0.027    -.1239737    -.007625
       _cons |   22.65695   1.696696    13.35   0.000     19.24903    26.06486
-------------+----------------------------------------------------------------
     sigma_u |  1.7315156
     sigma_e |  .29543002
         rho |  .97171252   (fraction of variance due to u_i)
------------------------------------------------------------------------------
```

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