Research Statement

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My research estimates the impacts of different government policies on regional economic growth, with a primary focus on firm entry, along with the development of new econometric methods for answering problems in regional economics. Entrepreneurial activity is incredibly common throughout the US, and researching how policies impact regional variation and local selection on when and how to start businesses helps local, state, and federal governments understand what policies do and do not work in improving regional welfare and growth. While the average new firm has relatively small impacts- is on average small, short lived, and has no competitive advantage- the sum of small entrepreneurial activity is a major source of long run innovation, employment growth, and tax revenue in the US economy. As a result, firm entry allows individuals to understand shifts in opportunity costs presented to entrepreneurs, such as taxes, credit access, or changes in demand for consumer goods or new product types. Driving through small and large towns throughout the United States shows how important firm entry is to many of these communities, particularly in rural and sub-urban communities, where firm entry can improve long term sustainability of these communities.

My job market paper, titled *The Capital Purchase Program's Effect on Firm Dynamics over the Business Cycle* estimates the impact of the Treasurys Capital Purchase Program (CPP) on local firm dynamics- entry, exit, employment expansion, and employment contraction between 2008 and 2015 in regions that had banks receive CPP funds. This project uses information on local bank and household financial health while accounting for dynamic treatment assignment and local spillovers using synthetic control and difference-in-differences estimation techniques. The direct effect of a county having a bank receive CPP funds on entry was zero, but created long run improvements in firm exit, employment expansion, and employment contraction. The effect of being adjacent, defined as having a county centroid within 50 miles of a treated county, is shown to be non-existent across a variety of different model specifications. When looking at log entry, log exit, and share of firms that expanded or contracted employment, even direct effects of having a bank receive CPP funds on a county go away. Finally, explicit pre-trend tests on difference-in-differences are rejected, showing that there are significantly different trends trends in county firm entry rates prior to treatment even after conditioning on a full set of fixed effects including individual, federal reserve branch area, urban to rural code with time trends and interaction effects.

The results show that the CPP impacts on local firm dynamics was small and close to zero. Actually causes of this null result lie in data limitations. Most small firm entry is not capital constrained, and I am unable to gain explicit measures of firm entrants by number of employees or other firm level characteristics. However both the now-defunct National Survey of Small Bank Finances and the newer Small Business Credit Survey show that up to 30% of firms do not have any loans or credit needs at any point in the firm life cycle. A similar share of firms never grow.

My second paper, *"Linear Hypothesis Tests over Fixed Effects with Serially Correlated Panels"* generates Wald-style joint hypothesis tests over inconsistently estimated parameters in settings where individual (or group) specific heterogeneity exists in both intercepts and slope coefficients. I show that under two several different data generating process assumptions and type

of hypothesis researchers can consistently estimate underlying variance-covariance matrices for use in Wald-style tests. I then show the existence of feasible tests. An example of this test is in estimating the impacts of taxes on regional economic growth. A sub-question might be whether or not estimating the model for each Census region and testing whether or not all region specific tax coefficients are the same as the pooled model as a robustness check. A second application lies in teacher value added, where researchers might want to test whether or not teacher value added is actually the same for all teachers are a specific grouping level- all teachers in a particular grade, at a particular school, or district.

My last two papers, *"Do state business climate indicators explain relative economic growth at state borders?"* and *"Impacts of Taxes on Firm Entry Rates along State Borders"* both use the difference in outcome variables in counties along state borders to show whether or not state business climate indices predict wage and employment growth, and the impacts of 7 top marginal tax rates on relative firm entry rates. The underlying identification strategy claims that state government's do not set policies conditional on how specific counties along the borders were doing. Therefore the differences in outcome variables in counties along the border removing shared economic conditions, such as labor markets, credit markets, and agglomeration economies, such that the difference in the variable is driven primarily by differences in state policies. This allowed me to estimate whether or not state tax business climate indices actually predicted future employment and wage growth.

The output showed that most business climate indices do a very bad job of predicting future economic conditions, concluding that state and local governments should generally put very low value on these indices when it comes to understanding what policies actively predict wage and employment growth in local communities. Many business climate indices provide unique weights to a mix of state tax and expenditure programs when creating weights, often driven primarily for political agendas. These indices performed notably worse than indices created by consulting groups, which even long after their discontinuation had strong power in predicting wage and employment growth.

Tax results showed that property, sales, and income taxes have the largest negative effect on relative firm start up rates. However, even these taxes had often economically insignificant effects. Robustness checks showed that the differences in tax rates became insignificant away from the border. At the time Kansas had significantly cut all taxes under the claim that firm and employment growth would offset the initial revenue losses. My paper showed that even along the border, whether the relative importance of tax rates is the highest, that the expected rise in firm entry was small. Therefore, similar plans going forward, particularly at the state level, should not be enacted.

Looking forward, I plan to explore topics related to regional firm entry. The first project will examine how displaced New Orleans residents impacted firm entry in counties they migrated to. This research will explore how large unexpected changes in local population, such as from international refuges, or large changes in immigration policy, impacts demand side preference for consumer goods in communities. Many of the displaced residents moved to larger urban areas such as Houston, TX, but also smaller communities throughout the broader southern USA. Previous research in this area has focused predominately on wage-effects of immigration from displaced New Orleans residents or the Mariel boat lift, without looking at the demand for consumer products, housing, and other necessities of a growing population. I further wish to explore how capital constraints impact medium sized new entrants that still do not generally have access to equity and venture capitalists. Gaining access to New Establishment Time Series data might improve analysis for both my job market paper, but also might open up areas to explore how changes in credit markets and market concentration have impacted the overall lowering rate of new firm entrants across the US.