RESEARCH STATEMENT

Field and Areas of Interest: My *field* of research is applied microeconomics, broadly defined to include forays into labor economics and the economics of education. I am interested in policy evaluation within both fields, especially in education—particularly higher education. Policies generally place individuals into one of three groups: those who stand to gain, those who stand to lose, and those who stand to experience no meaningful impact. I am interested in empirically examining the composition of these groups. Who wins and who loses under various types of education and labor policies, and what might be some potential unintended consequences? I am interested in how certain policies affect student choices in higher education, and how these choices are reflected in early-career labor market outcomes. It is this intersection between the economics of education and labor economics which constitutes the main focus of my research.

I am also broadly interested in measuring inequality in education and labor markets. I am currently exploring research questions surrounding the issue of "intersectionality," a term familiar to most sociologists but relatively few economists. Intersectionality holds that individual-level characteristics are not independent of one another, but interact to produce complex landscapes of inequality. For example, when examining health disparities in the United States, although it is valuable to perhaps first look at disease incidence across gender only, or race only, this approach is not sufficient, and generally masks important differences across more well-defined groups, such as the difference in incidence between low-income, white women and high-income, American Indian men, for example. I am interested in developing more sophisticated econometric methods to measure achievement gaps using intersectionality. What otherwise masked patterns of inequality, whether in education, health, the labor market, *et cetera*, emerge when we view certain individual-level characteristics as interdependent rather than independent?

My dissertation demonstrates my interests at the intersection of higher education and labor in three inquiries. My first chapter, published in *Education Finance and Policy*[1], explores the intended and unintended consequences of college financial aid on student outcomes. Specifically, I focus on broad, low-bar merit-based aid for undergraduate students. I explore this topic by examining completion effects of the New Mexico Legislative Lottery Scholarship, arguably the most generous, easily attainable and renewable merit-based scholarship in the United States. This question interests me because on one hand, if such scholarships ease the financial burdens of college, one could expect such programs to increase accessibility on behalf of economically disadvantaged students, and decrease the need for students to work during school, both widely viewed as improvements in higher education. On the other hand, such programs could have significant negative unintended consequences for students. Because many broad, low-bar merit based scholarships essentially make college "free" or nearly so for a large proportion of high school graduates, they may incentivize more marginally-prepared students to enroll in college who otherwise would have without the program. Many of these students may not be adequately prepared to succeed in college, and may exhibit higher dropout rates than students not receiving aid. Moreover, broad, low-bar merit aid programs remove price signals across in-state institutions which may increase the prevalence of "overmatching" at in-state colleges where degrees would otherwise cost more, and so may be perceived as "worth" more. Such programs may also distort students' choice of major away from more difficult degrees, such as those in STEM fields, in order to increase their expected likelihood of maintaining the scholarship. Because many broad, low-bar merit based scholarships essentially make college "free" or nearly so for a large proportion of high school graduates, this analysis also informs recent proposals which would make college free for the majority of high school graduates across the United States. I would like to look further into these issues, and believe this to be a promising line of future research.

Second, I am interested in the consequences of lengthened time to baccalaureate degree in the United States, a phenomenon that has received much attention in recent years from policymakers. I have been engaged in exploring this topic through searching for evidence that lengthened baccalaureate time to degree serves as a signal of low productivity to prospective employers. If this is the case, one would expect to find some sort of wage penalty in the student's compensation post-graduation. Recent proposals to reward the traditional four-year path to degree completion at both state- and federal-levels may not be in students' best interests if there is no penalty beyond potential loss of earnings from later post-graduation labor market entry, losses that may easily be countered by higher earnings during school for those taking longer to graduate. Researchers examining this relationship using cross-sectional data report a negative relationship between time to degree and earnings, which they attribute to ability. Increases in time to degree, however, cannot plausibly be linked to lower ability over time. When I proxy for ability and instrument for the student's time to degree, I find no labor market penalty for delayed graduation. This work is currently being reviewed at the *Journal of Human Capital*.

My third chapter builds off of the first chapter in that it examines the potential unintended consequences of broad, low-bar state merit aid on college outcomes. In this chapter, I propose that when state merit aid is contingent on meeting relatively rigorous academic benchmarks (e.g., high GPA and credit hour requirements), students may respond by taking less challenging majors in order to increase the likelihood of scholarship retention. On the other hand, low-bar programs such as the University of New Mexico's may not provide such incentives as initial and renewal scholarship eligibility requirements are relatively lenient. In fact, low-bar state merit scholarships may actually incentivize students to try for more difficult majors, especially since more difficult majors tend to pay more (e.g., engineering, business majors, *et cetera*). My findings suggest that in states with generous renewal requirements, such as the University of New Mexico, many students do, in fact, try for more difficult majors, including those classified by the U.S. Census Bureau as STEM. However, despite higher initial engagement in STEM majors, there appears to be no program effect on actually completing a STEM major. The story is plausible: the state pays for a student's college as long as they can maintain a "C+" average and complete 12 credit hours per semester, so why not try for a major with a higher return, provided that graduation seems likely? Marginal students appear to be more willing to take a risk on higher paying, but more difficult, majors either because they are not bearing the direct costs of college or they mistakenly overestimate their ability. Either way, this chapter provides evidence that state merit scholarships distort students' choice of major. This chapter is under review at *Applied Economics Letters*.

Empirical Methods: I completed a major field in econometrics, and enjoy taking care to ensure that the empirical strategies I employ in my research are both sound and appropriate given the data and research question(s) at hand. For example, in my paper examining the effect of broad, merit-based student aid programs, I first begin by estimating a difference-and-differences model with qualified resident students serving as the treatment group, and nonresident (and thereby unqualified) students as the control group. A visual check of the common trends assumption suggests the identification appears to hold for our outcome of interest, six-year completion rates. This is also empirically examined using a flexible difference-in-differences model following Autor (2003) which verifies the joint insignificance of year dummies prior to the implementation of the scholarship. To mitigate any observable differences between resident and nonresidents, I appeal to difference-in-differences matching estimation on the propensity score. Following rigorous methods in Imbens and Rubin (2015), I check the success of the match along different regions of the propensity score distribution. I also investigate whether the high school GPA requirement (the only requirement beyond being an in-state high school graduate) may be exploited in a regression discontinuity design. As I suspected, threshold manipulation tests following Cattaneo, Jansson, and Ma (2017) provide evidence that the high school GPA threshold for scholarship eligibility is manipulable by high school students—perhaps by taking easier courses, retaking courses, or some other mechanism outside the scope of the paper.

In other working papers, I appeal to a variety of empirical methods. In my research on whether time to degree serves as a productivity signal to prospective employers, I appeal to Two Stage Least Squares (2SLS) in addressing endogeneity in the wage equation (i.e., institution quality and ability both being related to the student's time to degree as well as their post-graduation earnings), evidenced by results of a Hausman test. I address the endogeneity issue by including a proxy for student ability, controlling for institution selectivity, and instrumenting the student's own time to degree with the average from their institution. Ordinary Least Squares (OLS) estimates are similar to previous research that does not address endogeneity in the college graduate's wage equation. 2SLS estimates reveal a very different pattern, suggesting that previous estimates suffer from significant bias.

I recently co-authored and published a paper on gender gaps in higher education.[2] My contribution to the paper, which I also believe to be a significant contribution to the literature, was to offer a novel, but simple, method of examining achievement gaps using regression. The method I developed can be characterized as a sort of accounting tool rather than answering any research questions related to causality. We obtained administrative data at a major public university in the southwest, seeking to explore the complex landscape of achievement gaps in developmental course taking and six-year graduation rates. The empirical model developed in the paper estimates an achievement gap for each race-ethnicity-gender-class combination in the data set. Because we had student-level data on gender (male or female), class (highest or lowest family income quartile), and race-ethnicity (white, black, Hispanic, American Indian, or Asian), we had 2 X 2 X 5 = 20 groups of students with a unique combination of these characteristics. White, high-income, women were designated as the baseline group. Achievement gaps were estimated by taking linear combinations of marginal effects from fully saturated mixed-effects logit models. A hierarchical linear model was chosen to account for the natural clustering of students within feeder high schools. We report full model results (expressed as marginal effects) along with the 19 estimated achievement gaps (relative to the baseline group). This method of reporting makes interpreting achievement gaps across very specific groups easy to assess. For example, we find that Hispanic students are no less likely to graduate relative to white students, all else equal. However low-income, Hispanic men are approximately 17 percent less likely to graduate compared to high-income, white, women.

My post-dissertation research has been mainly focused on employing regression discontinuity design (RDD) to approximate local average treatment effects. In fact, I recently revisited the first chapter of my dissertation using regression discontinuity. In the published version in *Education Finance and Policy*, I find that there is manipulation of the qualifying semester GPA by students, thus invalidating RDD as an identification strategy. Since then, I realized that special freshmen advisement aimed to help students qualify for New Mexico's scholarship was instituted in 2000, so it seems reasonable that such a program would advise students to take easier course loads or easier courses in order to qualify for the program. When I restrict the analysis to pre-2000 cohorts, I find no evidence of student manipulation of scholarship rules. Because some students below GPA and credit attainment cutoff successfully petition to receive to scholarship, I resort to fuzzy RDD. New results reveal positive impacts on degree completion for students just above the qualifying semester GPA threshold of 2.5 at four, 4.5, and five years, with no statistically meaningful program effects at later semesters. This suggests that New Mexico's low-bar merit scholarship reduced time to degree for marginally-qualified students without affecting overall completion rates.

Future Research: I am interested in expanding my research into merit-based financial aid programs. Because state-level programs generally provide caps on how long a student qualifies for aid, I am interested in whether caps serve as an efficient mechanism for reducing student time to degree. It is reasonable to think that some students' time to degree would coincide with the semester/quarter cap placed on their aid (i.e., they take as long as they are funded for). Do some students "overshoot" these caps and still manage to graduate? For those that overshoot, what do their patterns of attrition look like in those later, unfunded terms compared to students not receiving such aid? In other words, is losing merit aid a "death sentence" in that graduation likelihoods fall to near-zero? Do students which would have otherwise graduated in four or less years respond to such programs by lengthening their time to degree? I believe there are several promising research papers within this line of questioning.

Related to these questions, I am interested in studying whether state merit aid programs affect high school graduation in the United States. As of now, over half of U.S. states have large-scale, generous state merit aid programs intended to subsidize tuition and fees for in-state students. Although there is considerable heterogeneity in these programs at the state-level, they each share one common requirement: high school completion. As such, the promise of a shot at "free" college tied to completing high school should be expected to increase high school completion in participating states. I plan on using the timing of program launches and use Current Population Survey estimates to examine whether states that implement such programs see an increase in the proportion of residents whose highest schooling was at least a high school education. This paper will serve to fill an important gap in the literature on the externalities associated with generous state merit scholarships.

Moving away from higher education, I have two working papers which examine aspects related to crime. My interest in the economics of crime was spurred by the extensive administrative data made available to me in New Zealand. Through Statistics New Zealand, I have access to a census of investigated criminal incidents going back to 2008. For the vast majority of investigations, a unique identifier for the alleged offender is included. Unique identifiers are linked to other administrative datasets in New Zealand, including mental health referrals, hospitalizations, public benefit receipt, earnings, *et cetera*. Notably, there is a comparable victim database with unique identifiers, enabling the study of victim-offender relationships over time. The data are truly remarkable. These data have given rise to two papers I am currently working on. First, I am linking criminal and victimization data sets in order to better understand the overlap between committing crimes and being the victim of a crime. Anecdotally, we expect a positive tetrachoric correlation between the two: drug dealers, for example, often get caught dealing drugs, and often deal drugs in neighborhoods where they are more likely to become victims of crimes themselves. I am estimating these relationships using a bivariate probit system, which by construction, does not require any instrumental variables for the simultaneous system to be identified. Crimes are broken down into several sub-categories, including gun violence, family violence, intimate partner violence, theft, and so forth. Preliminary results are promising, and data are superior to previous research in this area, which have generally been survey-based, self-reported, and targeted at narrow populations, such as prisoners.

The second crime working paper examines how adolescents respond to legal access to purchasing alcohol. Since December 1999, the minimum legal purchase age has been 18 years of age in New Zealand. Because data include individual-level crimes and birth dates for nearly all five million New Zealand residents, we are able to conduct sharp RDD around the age threshold to estimate how gaining legal access to purchasing alcohol affects drunk driving, public intoxication, and other alcohol-related offenses. I believe both crime-related working papers are destined for top-tier economics journals.

In yet another vein of research, I have a working paper that examines how the Legal Arizona Workers Act (LAWA), also known as SB 1070, impacted wages and employment levels in Arizona after its initial passing in 2008. LAWA was one of the first—and most aggressive—policies attempting to deter illegal immigrants from residing in a particular region. Employers were required to check the immigration status of certain new employees using the E-verify system, and businesses found noncompliant were subject to permanent revocation of their right to do business in the state. Our initial findings agree with the literature in that undocumented immigrants are sensitive to such policy changes. LAWA appears to have led immigrants to flee the state, thereby reducing labor supply in certain industries, resulting in a temporary increase in wages and decrease in overall employment in immigrant-rich sectors. Labor market conditions in the same sectors in neighboring states appeared to demonstrate an increase in the supply of such labor. Shocks appear to stabilize after 1-2 years, however, suggesting a relatively quick correction period. Although numerous policy implications can be formed from these results, the paper has its flaws: the policy change occurred during a major recessionary period, so additional controls for broad economic conditions may be needed. People tend to go where job openings exist. After incorporating said controls, and relying on a recent related article published in *American Economic Journal: Economic Policy*, I believe this paper will be appropriate for a reputable general interest journal in economics.

I also plan on further developing the empirical approach to intersectionality which I discussed above. I would like to generalize results at the national level using the National Education

Longitudinal Study of 1988 (NELS:88) and the ELS:2002. These data are much richer than those used in my article in *Race, Ethnicity and Education* and would allow me to explore expanding the use of intersectionality to other areas of applied microeconomics, including estimating health disparities, pay gaps, and offender recidivism using various individual-level characteristics, for example. I feel this study will be well-received by leading journals in education, sociology, and economics.

[1]Erwin, Christopher, and Melissa Binder. (2020). Does Broad-Based Merit Aid Improve College Completion? Evidence from New Mexico's Lottery Scholarship. Education Finance and Policy 15(1), 164-190.

[2]Nancy López, Christopher Erwin, Melissa Binder & Mario Javier Chavez (2018): Making the invisible visible: advancing quantitative methods in higher education using critical race theory and intersectionality, *Race Ethnicity and Education* 21(2), 180-207.