Virtually the same: Using Bayesian methods to investigate the relationships between online course delivery and postsecondary student enrollment, course outcomes, and degree attainment

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Chapter 1

Introduction

1.1 Objective

A number of trends over the past few decades have contributed to the expansion of online coursework at postsecondary institutions across the United States. In one current, the college student population is both growing and changing. As total student enrollments have increased so too have the relative proportions of non-white, older, and low income students (Snyder, de Brey, & Dillow, 2016). While some attend elite institutions, most persons from these historically underrepresented groups enroll in public two- and four-year institutions, many with open admissions policies (Ross et al., 2012). At the same time as this growth in enrollment, the schools they attend have generally witnessed decreases in the amount of direct funding they receive from state and federal sources and have increasingly relied on tuition dollars to meet their budgetary needs (Carlson, Ott, Armstrong, Zaback, & Auer, 2015). Public community colleges and open admissions four-year universities have needed to find ways both to increase enrollments and get the most use out of scarce institutional resources (Allen & Seaman, 2010).

One solution made possible by improvements in technology and infrastructure has been to increase the number of distance education courses in which course material is delivered through online means (Radford & Weko, 2011). In the fall of 2014, 28% of all college students took at least one online course, with 14% completing all coursework through online classes (Snyder et al., 2016). With the promise of flexibility for non-traditional students, which is of key importance to open access institutions with a strong equity mission (Cox, 2006), and a functionally zero marginal cost, college courses that make substantial or total use of online instructional delivery have great potential for expanding access in a cost-effective manner (Bowen, 2013; Selingo, 2013).

But what are these online courses? Are they the same as traditional face-to-face courses or something radically different? For colleges and universities, these courses may allow them to reach students whom they otherwise would not have enrolled, more effectively democratizing higher education (Bowen, 2013; Deming, Goldin, Katz, & Yuchtman, 2015; Selingo, 2013). For producers of educational content, the leasing or selling of online courses could be a way to generate additional revenue (Fisher, 2006; Powell & Owen-

Smith, 1998). For universities that purchase such content, it could be a way to save on labor costs by hiring "paraprofessionals" who can curate prepackaged online courses in the place of full faculty (Paulson, 2002). For faculty, a long history of intellectual property case law that has sometimes supported their ownership of intellectual property over claims by the university and sometimes not (Nemire, 2007), rights and responsibilities in regards to online courses are more fraught. For students, the option to take online courses adds new layers of uncertainty to the college decision process. Does a student still need to choose one college, or can she instead earn skill-based credits or "badges" at a number of institutions (Selingo, 2013)? Will future employers look down on graduates who earned most their credits online (Cottom, 2017)? Will they even care?

Perhaps most immediately important to prospective online students, however, is an answer to the question: *Will I do as well in online courses as I would in more traditional classrooms?* Institutions, faculty, policy makers, and education researchers would similarly like an answer to this question. Unfortunately, findings have been mixed regarding student outcomes in virtual versus face-to-face settings. A meta-analysis of the literature between 1996 and 2008 conducted by the U.S. Department of Education found that among 45 qualifying studies, students taking courses with online components "performed modestly better, on average, than those learning the same material through traditional face-to-face instruction," (Means, Toyama, Murphy, Bakia, & Jones, 2010, p. xiv), though non-equivalence between groups may have favored the online students in many of the studies. Recent studies that employ more rigorous research designs have not firmly resolved the question, with three randomized experiments (Bowen, Chingos, Lack, & Nygren, 2012; Figlio, Rush, & Yin, 2010; Joyce, Crockett, Jaeger, Altindag, & O'Connell, 2014) finding little or no average difference in student outcomes between delivery types, and two studies that utilize quasi-experimental designs finding that online students performed worse (Xu & Jaggars, 2011, 2013).

In the midst of this uncertainty, it becomes a matter of equity that we better understand the effects that online courses have on student persistence, likelihood of passing, and eventual degree attainment. As an increasing number of students attend college, many will choose to enroll in community colleges and open admissions universities. Barring major changes in funding structures, these schools will feel pressure to do more with less and are likely to continue to increase online instruction. Some students may instead choose to enroll in for-profit institutions, which rely heavily on online education (Deming, Goldin, & Katz, 2012; Fisher, 2006; Kinser, 1999). Under either scenario, students from populations historically underrepresented at the postsecondary level—non-white, older, and lower income—as well as those from all populations at the margins of attendance will be the ones most likely to take online and hybrid courses. If it is the case that those who attend college through their computers realize better or simply similar outcomes as those who attend in-person, then the flexibility and potential cost savings of online courses may be a boon to students and schools alike. But if the opposite is true, that online course-takers face lower odds of succeeding than those who learn face-to-face, regardless of their personal preparation or incoming ability level, then an unfettered expansion of online coursework will not only represent a waste of institutional resources but also a blow against the belief that a college education is equitably accessible to all who desire it.

1.2 Research questions

In this dissertation I investigate postsecondary online education with three research questions:

1. What is the connection between broadband access and the number of students who attempt online coursework?
2. What is the effect of online course delivery on course persistence and, conditional on completion, the likelihood of passing the course?
3. How do degree attainment rates differ between students who take online courses and those who take only face-to-face courses within and across states?

1.3 Conceptual framework

Individuals choose whether to enroll in college as well as their particular degree pathways for a wide array of reasons. They may have heard or see that college-educated adults are more likely to be employed, have access to employer-based health insurance and retirement accounts, lead healthier lifestyles, participate in civic activities, and spend more time on education with their children (Baum, Ma, & Payea, 2013). Because there may also be public benefits to living in a society with better educated individuals (Doyle & Skinner, 2017; Kam & Palmer, 2008; Nie, Junn, & Stehlik-Barry, 1996; Pascarella, Ethington, & Smart, 1988; Verba & Nie, 1972; Wolfinger & Rosenstone, 1980), some may choose to enroll, in part, to support such a society. The most commonly cited reasons for choosing to attend college or enroll in certain types of courses, however, are to improve job prospects and increase future wages (Eagan et al., 2014; Fishman, 2015). This is rational as postsecondary degree holders tend to earn higher wages and salaries than those who have only a high school diploma, an outcome that may be attributed to their having gone to college (Card, 1995; Carneiro, Heckman, & Vytlacil, 2010; Doyle & Skinner, 2016; Kane & Rouse, 1993; Mincer, 1962).

Human capital theory states that the college enrollment decision is based on maximizing utility by investing in one's own education (Becker, 2009; Kane, 1999; Manski & Wise, 1983; Paulsen, 2001; Perna, 2006; Toutkoushian & Paulsen, 2016; Turner, 2004). In this dissertation, I conceptualize student decisions to enroll in online courses as being rooted in their desire to improve their human capital in the most cost effective manner. Online courses may offer students who otherwise would not have been able to attend college the opportunity to do so (Cox, 2006; Moloney & Oakley, 2010; Moore & Kearsley, 2011). They may also give non-traditional students more flexibility to work around their existing commitments to employers and family (Jaggars, 2014). They may be less expensive on the margins for institutions to implement, savings that could be passed on to students (Bowen, 2013; Deming et al., 2015; Selingo, 2013). On the whole, online courses may reduce the costs of college—direct and indirect—for students, helping them to earn credits, a degree, and eventually better wages and employment prospects.

The uncertainty surrounding student outcomes in online courses, however, complicates this straightforward conception. Students who lack sufficient broadband access may face difficulties taking online courses (Hurst, 2010; Oyana, 2011), rendering potential cost reductions moot. If students are less likely to successfully complete online courses than face-to-face courses, then cost savings may not be realized if students have to retake courses and take more time to graduate. Lower grades in online courses would mechanically lead to lower overall grade point averages, which could lessen an undergraduate student's chances to transfer or enter a graduate program. It could also hurt her ability to maintain merit-based aid, potentially changing the likelihood of completing a degree at all (Carruthers & Özek, 2016). States like Tennessee that seek to increase state-wide degree attainment through the expansion of online course options (*Drive to 55 Tennessee*, 2014; Tennessee Higher Education Commission, 2015) may end up seeing less return per dollar spent on online courses than face-to-face courses. In the three chapters of my dissertation, I investigate these aspects of online learning—enrollment in terms of broadband access, within course outcomes, and state-level aggregate attainment—so as to better understand how these courses may or may not support students' human capital decisions.

1.4 Methods

Throughout this dissertation, I use Bayesian quantitative methods to answer my research questions (Carlin & Louis, 2009; Gelman et al., 2014; Jackman, 2009). To answer the first question, *(RQ1) What is the connection between broadband access and the number of students who attempt online coursework?*, I regress the number of students who attempt some online courses at public open admissions colleges and universities across the lower 48 states on various measures of broadband connectivity—download speed, upload speed, number of Internet service providers—and institutional and geographic characteristics. To account for the nested nature of the data in which postsecondary institutions are situated in states with their own higher education policy contexts, I also fit multilevel regression models that allow marginal effects of broadband measures to vary across states.

For the second question, *(RQ2) What is the effect of online course delivery on course persistence and, conditional on completion, the likelihood of passing the course?*, I use a quasi-experimental instrumental variables approach with student-course-level data from the University System of Georgia. Specifically, I model the two outcomes of interest, (1) withdrawal from the course, and (2) passing the course conditional on not withdrawing, as functions of whether or not the course section met online or face-to-face. Because the choice to enroll in an online course section is likely endogenous, I instrument that decision using the same measures of broadband access that I use for the first research question, this time assigned at the student level. Due to the binary nature of both outcomes as well as the indicator for online delivery, I employ a recursive bivariate probit model, which jointly fits the outcome of interest and online treatment as a pair of probit equations in which exogenous instruments (broadband connectivity measures) are placed on the right-hand side of the first level equation (online status), and the first-level outcome is placed on the right-hand side of the second level (completion/pass) (Greene, 2012; Hollenbach, Montgomery, & Crespo-Tenorio, 2016).

To analyze the third and final question, *(RQ3) How do degree attainment rates differ between students who take online courses and those who take only face-to-face courses within and across states?*, I use Bayesian multilevel regression with poststratification (BMRP) in order to make state-level inferences with observations from survey data that themselves are only nationally representative. This methodology, which is often used by political scientists to model voter beliefs and likely voting patterns with non-representative data (Gelman & Little, 1997; Lax & Phillips, 2009; Little, 1993; Park, Gelman, & Bafumi, 2004; Wang, Rothschild, Goel, & Gelman, 2015), first uses a multilevel regression to model responses among types of individuals in the non-representative data sample. Predictions from this model are then post-stratified to the area of interest, such as the state, using population counts from Census data to appropriately weight the aggregate response. Specifically, I use student-level data from the Beginning Postsecondary Students Longitudinal Study, 2004/2009, survey to compare aggregate degree outcomes between students who took some online courses and those who took none within each state. As an example application of this method, I discuss the results in the context of Tennessee, which has established degree attainment goals that pertain to online education and therefore serves as a useful case study (Tennessee Higher Education Commission, 2015).

In each of this dissertation's studies, I work within a Bayesian statistical framework rather than a frequentist/likelihoodist framework for two primary reasons. First, the data I use and questions I ask either align with a Bayesian epistemology or do not lend themselves to estimation with frequentist methods. The institutional level data I use to answer the first question represent the population of interest. The repeated sampling assumption of frequentist statistics, therefore, does not apply in this context (Jackman, 2009). Though the multilevel models that I employ for the first and third research question may be fit with maximum likelihood routines, Bayesian estimations of the models may be more likely to converge and directly model uncertainty in the spread of the parameter posterior distributions (Gelman et al., 2014). For the second question, Bayesian estimation of the bivariate probit model proved to be the only way to produce reliable estimates when using binary treatments and outcomes (Chib, 2003; Hollenbach et al., 2016).

Second, I argue that quantitative education policy research conducted under the Bayesian framework produces results that are easier to interpret by policymakers and practitioners than those produced by conventional methods (Gill & Witko, 2013). For each research question, the results that I show represent probabilistic statements about correlations, effects, or outcomes in which the amount of uncertainty is directly addressed by the range of the posterior distribution. Though I often make comparisons with frequentist results as a point of context, I also demonstrate the unique characteristics of Bayesian analyses that allow results to be parsed and presented in wide number of policy-relevant ways (Gelman et al., 2014).

1.5 Results

I find evidence that broadband access is positively associated with the number of students at open admissions colleges and universities who take some of their courses online. For every tier increase in download speed, which roughly translates to rates that are 2-3X times faster, institutions see an average of 41% (single level model) to 56% (multilevel model) more of their student body enrolling in these courses. The 95% credible intervals for each of these estimates are wide, but remain positive. I do not find similarly credible results for changes in upload speed or the number of providers. This finding suggests that students at these schools may be making a rational choice to enroll in online courses, at least as it relates to their technological capacity for doing so.

I do not find, however, that once in online courses students perform as well as they do in face-to-face courses. In agreement with other studies that used quasi-experimental designs (Xu & Jaggars, 2011, 2013), I find that University System of Georgia students are, on average, 2.8 percentage points more likely to withdraw from online courses than face-to-face courses. Among those who persist to the end of the course, online students are, on average, 5.4 percentage points less likely to pass with a C- or better. Among a subset of students within each term who took both online and face-to-face courses, students are an average of 2.5 percentage points more likely to withdraw and 1.5 percentage points less likely to pass online courses, though wider credible intervals suggest more uncertainty about these results. I find that local average treatment estimates and treatment on the treated estimates of the effect are similar to the average treatment effects.

Turning to long-term degree outcomes post-stratified from national survey data to the states, I find that the population of students who took at least one online course were generally less likely to earn a bachelor's degree within six years of initial enrollment than the population who took only face-to-face courses. These differences are reduced slightly when certificates and associate degrees are included. For associate degree attainment within three years of initial enrollment, I find that the average difference shifts slightly in favor of those who took at least one online course. There is less change when certificates and bachelor's degrees are included in these models. Importantly, since multilevel regression with poststratification is new to higher education research, the procedure successfully shows heterogeneity across the states and is able to be validated using data from external sources. Results specific to Tennessee suggest that while increases in online enrollments may support six-year bachelor's degree and three-year associate degree outcomes, they may depress three-year certificate outcomes. Because meeting aggregate degree attainment goals in the state relies on increasing the number of certificate holders, my results suggest that an unmindful online expansion in Tennessee may work at cross purposes to the Drive to 55 initiative (*Drive to 55 Tennessee*, 2014).

1.6 Contributions

My dissertation contributes to the education policy literature both through its empirical findings and its use of Bayesian methodologies. With the first research question, I offer the first robust analysis of the association between measures of broadband access and the number of students who enroll in online courses across the country. Though it is a truism that broadband access should positively affect online enrollments, no prior studies directly estimate the presupposed connection in the national context. In answering my second research question, I provide further evidence of the negative effect that online courses have on student course outcomes. I do so using data from a new state context, Georgia, and using new broadband-derived instruments. Finally, through analysis of the third research question, I produce state-specific predictions of the difference in degree outcomes between sometimes-online and never-online students. The inability of the current literature to offer states these estimates represents a large gap, especially in the context of distance education policy decisions that are made at sub-national levels. With these final results, I fill this gap. For each question, I offer new insights into online courses in higher education that support a better understanding of how they may fit into students' human capital enrollment decisions.

My use of Bayesian statistics represents a contribution in its own right. Though the Bayesian paradigm is not novel to education policy research, it has yet to find widespread application in the field. Across the three chapters, I show how a Bayesian methodological foundation is well suited to education data that are often nested and situated in a particular time and context (Gill & Witko, 2013; Jackman, 2009). With the Bayesian recursive binary probit model, I offer the field a new technique for estimating quasi-experimental instrumental variables equations that may perform better in the face of weaker instruments and can easily return more than one type of treatment effect (Chib, 2003; Hollenbach et al., 2016). Finally, with the Bayesian multilevel regression and poststratification methodology, I fully describe the implementation of a procedure that could be usefully applied to wide range of education policy questions.

1.7 Structure of the dissertation

The investigation of each research question is presented in a self-contained chapter that has its own introduction, literature review, methodology, data, results, and concluding sections. Research questions 1, 2, and 3 are examined in Chapters 2, 3, and 4, respectively. I end the dissertation with overall conclusions and ideas for future research in Chapter 5.

Chapter 2

Estimating the Relationship between Broadband Access and Online Course Enrollments at Open Admissions Public Higher Education Institutions

2.1 Introduction

The number of students who enroll in online courses has increased dramatically since the early 2000s (Radford & Weko, 2011; Snyder et al., 2016). Though the popular press has often focused more on massive open online courses, or MOOCs, (e.g., Pappano, 2012), relatively few college students take only distance education credits. Many instead split their time between face-to-face courses on campus and those offered online through their home institution (Snyder et al., 2016; Snyder & Dillow, 2015). As more people decide to pursue postsecondary education, it is likely that a significant portion of the higher education sector's expansion will occur online.

In the midst of this growth, those studying the outcomes of students who enroll in online course sections have found mixed results. Some studies that compare online students to those who enroll in face-to-face sections have found no average difference in end-of-course grades for online students (Bowen et al., 2012; Figlio et al., 2010; Joyce et al., 2014). Other studies have shown the opposite, that some students who enroll in online courses, particularly those who attend open access institutions such as community colleges, may perform worse and be more likely to withdraw (Xu & Jaggars, 2011, 2013).

Largely unexamined in this literature on outcomes, however, is a key mechanism through which most online students access their work: high speed broadband. Students who do not have access to quality broadband cannot take online courses. Existing disparities in broadband access (Federal Communications Commission, 2016; Grubesic, 2008b; Grubesic & Murray, 2002; Prieger & Hu, 2008) are likely to lead to disparities in online education, which, as more courses become digital, could exacerbate inequities currently seen across the higher education sector as a whole. This especially may be true at public colleges and universities with open admissions policies that educate greater numbers of first-generation and non-traditional students.

In this chapter I investigate the relationship between access to high speed broadband and the number of students who attempt online courses at public universities and community colleges with open admissions policies in the lower 48 states during the period from 2012 to 2014. To do this I rely primarily on enrollment data from the Integrated Postsecondary Education Data System (Department of Education, 2015) and census block-level measures of broadband access from the National Broadband Map (National Telecommunications and Information Administration, 2011). I specifically operationalize broadband access using three key measures: download speed, upload speed, and the number of providers. In order to approximate the download/upload speeds and number of providers experienced by the average enrolled student at each institution, I assign each school broadband measures that are the population/inverse distance-weighted averages of those recorded in surrounding census block groups. Due to the nested nature of the data, with schools located in states that have different postsecondary policy contexts, I fit multilevel Bayesian regression models in addition to single level pooled models.

I find that for every tier increase in download speed, the percentage of an institution's students who enroll in some online courses increases an average of 41% to 56%. I do not find strong evidence of a similar association for upload speeds or the number of Internet service providers in the area. Multilevel models that allow broadband regression parameters to vary by state suggest that a few states—Tennessee, Alabama, and Oklahoma—may have very strong positive relationships between download speed and online course enrollments, with one state, Louisiana, having a very strong negative association. For the majority of states that show no strong positive or negative association (95% credible intervals cross zero), it could be the case that institutional and state-level higher education policies are stronger drivers of online enrollments than student access to faster broadband speeds.

My results offer compelling evidence of a positive relationship between download speed and enrollment in online courses at public open admissions colleges and universities. If the higher education sector continues its move toward increased digital offerings, I argue that student access to quality broadband must be taken into account. The rest of the chapter is structured as follows: section 2.2 reviews the relevant literature on online education; next, I present my theoretical framework in section 2.3, estimation strategy in section 2.4, and data in section 2.5; after discussing my results in section 2.6, I offer a conclusion in section 2.7.

2.2 Literature review

2.2.1 Rise of online education

Over time, a number of technologies have been used to make education at a distance possible. Correspondence courses that took advantage of the postal system in the 19th century mark a more formal start to higher education at a distance (Johnson, 2003). Moore and Kearsley (2011) note five generations of distance education: correspondence, broadcast radio and television, open universities (as seen in the United Kingdom and Australia, among other countries), teleconferencing, and the Internet/Web. With each new generation, improvements in technology meant potential improvements in the delivery of education. While radio and television broadcasts were faster and potentially more inclusive than direct mail, teleconferencing and the Internet once again “allowed a student to answer back,” (Moore & Kearsley, 2011, p. 36).

The United States Armed Forces itself took advantage of distance education during the Second World War. Under the theory that better educated soldiers made better all-around soldiers, the federal government built a robust system of correspondence courses that troops could (and did) take during their downtime (Loss, 2012). Though these courses gave way to temporary place-based universities that soldiers attended before returning home at the end of the war, the idea that higher education was fit for many more than society’s elite found its first application in these correspondence courses.

In the postwar period, a number of other countries similarly decided to make higher education available to new populations of students. The Open University of Great Britain, founded in the late 1960s, was new university model that used distance education technologies to enroll students from all over the country (Moore & Kearsley, 2011). Not a part of existing universities, it instead combined distance learning technologies, distributed brick-and-mortar locations around the country, and a “radical open admissions policy” (The Open University, 2015) to enroll a large number of students. Still operating today, the Open University has seen countries around the world borrow its name and model for their own similarly structured institutions (Keegan, 1996).

Aside from experiments among some state university systems such as the Articulated Instructional Media Project (AIM) at the University of Wisconsin (Moore & Kearsley, 2011), the United States has not attempted to develop an open national university along the same lines as Britain’s Open University. This is in spite of the fact that the number of college-going students in the United States has steadily risen since the end of the Second World War. From the early 1960s to 2010, the number of postsecondary enrollments increased from fewer than 5 million students to over 20 million, a sizable proportion of which came from non-traditional, historically underserved populations (Snyder & Dillow, 2015). Enrollments at large public universities greatly increased to meet this new mass demand for higher education, as did the number of two-year institutions, most often in the form of community colleges (Cohen & Brawer, 2003; Crookston & Hooks, 2012; Thelin, 2011).

One popular solution to the problem of rising enrollments of non-traditional students coupled with declining funding (Carlson et al., 2015; Tandberg, 2010) has been to expand online education offerings through existing colleges and universities[1] (Bowen, 2013; Deming et al., 2015; Goldrick-Rab, 2010). Online learning has been touted as a way to educate students in a flexible and cost-effective manner (Moloney & Oakley, 2010). Such a move is also in line with the equity mission found at open access public institutions (Cox, 2006).

While some researchers have held up massive open online courses, or MOOCs, as models for a new type of higher education (Bowen, 2013; Selingo, 2013), the newest digital revolution in higher education has not been limited to these unique courses. Public institutions in many states, especially those at the two-year level, have also steadily increased the number of online credit hours they offer in the past few years (Allen & Seaman, 2011; Allen, Seaman, Poulin, & Straut, 2016; Radford & Weko, 2011; Southern Region Educational Board, 2013, 2015). In the fall of 2012, 27% of all college students took at least one online course, with 13% completing all coursework through online classes (Snyder & Dillow, 2015). Two years later in the fall of 2014, those same numbers had increased to 28% and 14%, respectively (Snyder et al., 2016). Unlike MOOC students, many of whom take courses anonymously and for no credit, these data show that formally enrolled students often incorporate online courses into their degree pathways, taking them alongside more traditional face-to-face courses.

From correspondence classes meant to replace traditional higher education programs to online course sections as simply another option in the menu of higher education choices, distance education has remained one way to open higher education to a greater number of people than possible at traditional campuses. But as the technology of distance education has improved—from mail, radio and television to the Internet—so too have the requirements for participation increased. Where an address, paper, and pen had once sufficed, a computer and steady Internet connection are now required to take most distance education courses. Students who do not have these technological resources may find themselves effectively shut out. The open access

[1] *cf.* the Open University and its founding as an autonomous institution.

rhetoric of online education, therefore, need first acknowledge the digital divide between those with access to the Internet and those without.

2.2.2 The digital divide: an overview

Scholars and policy-makers have long noted the divide between those who have access to communications technology and those who do not (R. H. Brown, Barram, & Irving, 1995; Irving et al., 1999; McConnaughey & Lader, 1998). Of recent concern is the digital divide between those with access to broadband and those without (Federal Communications Commission, 1999, 2000, 2002, 2004, 2008, 2010, 2011b, 2012a, 2015a, 2016). Because a large majority of persons in the United States have access to some form of broadband, researchers have transitioned from questions that ask *if* persons have access to questions that ask *what kind* of access (T. Brown et al., 2010). This represents an important shift as research has shown that local infrastructure, regardless of the relative affluence of the population, can have a major impact on the availability of service in a particular area (Grubesic & Murray, 2002). Even within a socioeconomically homogeneous local area, topological features can cause the quality of broadband connections to vary substantially across households and neighborhoods (Oyana, 2011).

Local variations notwithstanding, less affluent rural areas have generally had poorer access to broadband than wealthier and more urban areas (R. H. Brown et al., 1995; Copps, 2009; Federal Communications Commission, 2015a). One reason for this disparity lies in prohibitive "last-mile" infrastructure costs that communications firms, local governments, and residents are reluctant to cover (Grubesic & Murray, 2004). In rural, suburban, and urban areas alike, those of lower socioeconomic status usually have the least access to broadband (Horrigan, 2010). Even the near ubiquity of cellular and satellite technology does not close the gap since wireless services still cannot compete with wired services in terms of speed and reliability (T. Brown et al., 2010) or coverage (Grubesic, 2012b).

Due to growing concern over these issues, Congress charged the Federal Communications Commission in 2009 with instituting the National Broadband Plan. This plan represents a concerted national attempt to close the digital divide by making sure that "every American 'has access to broadband capability' " (Federal Communications Commission, 2009, p. XI), a goal that is seen as worthwhile due its economic, civic, and educational benefits (Copps, 2009; Czernich, Falck, Kretschmer, & Woessmann, 2011). Since the inception of the plan, a number of government reports have been issued detailing the status of broadband in the country (Federal Communications Commission, 2011a, 2012b, 2013, 2014). Data collected from ISPs about broadband penetration has also been opened to the public in the form of the National Broadband Map (National Telecommunications and Information Administration, 2011). While these data come with their own limitations mostly owing to the size of the raw data (Grubesic, 2012a, 2012b), they nonetheless represent a vast improvement over previously available data on broadband (Grubesic, 2008a, 2008b, 2008c) and make possible new quantitative research on the effect that being on the wrong side of the digital divide may have on educational access and outcomes.

2.2.3 The digital divide in higher education

Regarding the effect of the digital divide on students, the National Broadband Plan's founding document notes that

> [t]oday, millions of students are unprepared for college because they lack access to the best books, the best teachers and the best courses. Broadband-enabled online learning has the power to provide high-quality educational opportunities to these students—opportunities to which their peers at the best public and private schools have long had access. (Federal Communications Commission, 2009, p. 5)

In concurrence, a number of studies have highlighted the negative effects of the digital divide for communities and students in postsecondary institutions. From the early days of the Internet, scholars have noted that white students were more likely than their African-American peers to have computers in their homes and to have used the Internet (D. L. Hoffman & Novak, 1998). Even in later years as more students gained access to the Internet, differential usage across gender and racial/ethnic groups in terms of communication and academic usage suggest a continued divide (Cotten & Jelenewicz, 2006; S. Jones, Johnson-Yale, Millermaier, & Pérez, 2009).

Two recent studies in particular reveal how infrastructural differences in broadband access among higher education students remain. Using GIS spatial data to examine the availability of broadband in the region around Southern Illinois University Carbondale, Oyana (2011) finds that broadband quality is not uniform in the region and is generally inferior in poorer and more rural areas. Presenting data on the quality of signal as function of land topography, he shows a correlation between signal quality and median household income. Based on his analysis, Oyana concludes that without improvements to broadband infrastructures, much of the southern Illinois area under study could not support the large data requirements of the types of libraries and research labs that area students would need in order to have the same educational advantages as their peers who live in areas with better broadband access.

Hurst (2010) highlights similar concerns for knowledge production in low broadband areas in his dissertation that asks students at Walters State Community College in eastern Tennessee to describe their home broadband access and how it related to and affected their coursework. He finds that while 65% of his survey respondents ($N = 740$) said they felt having broadband access was very important for completing their schoolwork, 20% reported having no Internet access or only dial-up at home and 30% reported feeling dissatisfied with their broadband quality. He also finds a statistically significant relationship between having faster internet speeds at home and the propensity for taking an online course.

Both of these studies provide suggestive evidence of a gap in broadband access that may make it more difficult for rural and low-income students to fully participate in courses that require online coursework. The authors' respective findings are echoed by others who suggest that some areas of the country lack access to the quality broadband, computers, and human capital required to successfully integrate online coursework into higher education (Cejda, 2007). These studies, nonetheless, have limitations. Singly, each considers only a small local area; together, they focus on the mid-southern region of the country. The generalizability of Hurst's findings are further limited by the fact that his analyses do not utilize metrics of broadband connectivity in the area surrounding Walters State Community College, but instead rely exclusively on survey data.

This chapter adds to the literature on online higher education by rigorously investigating the connection between broadband access and the take up of online courses among students at open admissions colleges and universities across the country. With its national scope and novel data, this study further bridges the gap between research on the digital divide and online education. Though it may seem obvious that increased download speeds should be connected to increased online-based distance enrollment, the uncertainty surrounding the demands of online coursework may mean that students do not readily make the connection.

2.3 Theoretical framework

Under the human capital model of college enrollment, the enrollment decision requires a weighing of the potential gains of increased education against its costs (Becker, 2009; Turner, 2004). For many students, gains are realized as improved job prospects or increased wages (Eagan et al., 2014; Fishman, 2015). College costs generally include direct costs such as tuition, books, fees, room and board as well as indirect opportunity costs like forgone wages (Manski & Wise, 1983).

A key selling point of online courses is their potential to lower costs for both the institution and the student (Deming et al., 2015). For the institution, online students do not require physical plant space or the costs associated with using the space: desks, lights, chalk/whiteboards, paper handouts, electricity, support staff to maintain the space, *etc*. For this reason, the marginal cost of the course in terms of an additional student is close to zero. For courses in which grading may be largely automated (*e.g.,* those that rely on multiple choice tests rather than written assessments), the cost of an additional student in terms of instruction is also effectively zero. If most online courses are asynchronous, meaning that students are not required to view course materials at specific class meeting times but rather can do so when they choose, then schools can save money by reducing or eliminating off-hour course sections in which the student-instructor ratio may be comparatively lower and less cost-effective.

Students may also see online courses as personal cost reducers. Because online courses do not require travel to campus, a student can save on travel-related expenses such as gas or public transportation costs. Asynchronous courses do not demand a set block of time in which to work on them. Students in these types of online courses can self-pace and work at times with lower opportunity costs. The flexibility given by the online course may allow for a greater range of options in other areas—work, family—that themselves may not be as time flexible (Jaggars, 2014).

Online courses, however, may increase rather than decrease some educational costs. These additional costs may be difficult to measure due to informational asymmetry and uncertainty on the part of the student. When deciding whether to enroll in an online course, an important consideration is whether one has access to the quality of broadband required to successfully complete the course. Slow speeds, poor connections, and high connection prices may all reduce the likelihood of success by increasing the direct and indirect costs of enrollment. More money spent on gaining quality broadband access is less money to spend on books or other supplies; more time spent completing and submitting online assignments due to low quality broadband is less time to complete other assignments, work, or spend with family.

At the time of enrollment, students may have difficulty connecting perceptions of their broadband connections as experienced through other devices and services (*e.g.,* cellular phone) to the demands of online coursework. Students who are older or who face socioeconomic barriers to their enrollment may have had little experience with broadband upon which to base their enrollment decision. It could be the case that broadband access is moot for students with constrained choice sets that effectively limit their options to online work. For these reasons, it is unclear whether broadband access should be correlated with the decision to enroll in online courses in the aggregate. For the student, this lack of information regarding his ability to take online courses may increase his costs since uncertainty surrounding his ability to successfully complete the course may itself be a cost (Jaggars, 2014).

It is outside of the scope of this chapter to fully investigate whether students perceive online courses as increasing or decreasing the costs associated with their enrollment decision. Instead, I focus on a single aspect—the association between broadband access and the decision to enroll in online courses. The limited prior literature suggests that students with better access to quality broadband connections are more likely to attempt and succeed in online coursework than their peers who lack access (Hurst, 2010). If individuals respond this way, it may also be true that institutions in areas with better broadband are more likely to offer course sections in an online format and/or encourage their faculty to move some work online into a hybrid setting. In either of these situations, I would expect measures of broadband access to be positively correlated with the number of students attempting online courses. If uncertainty surrounding online courses or broadband connectivity is high or schools have other prerogatives regarding online education that outweigh considerations of broadband access, however, then online courses may be perceived as having higher costs. In this scenario, I would not expect a strong connection between broadband measures and online enrollment.

To investigate the relationship between broadband and online enrollment, I rely on a number of assumptions. First, I assume that students attending public open access institutions who split their enrollments between online and face-to-face course sections are likely to live nearby. Both prior research (Mattern & Wyatt, 2009) and national survey data suggest that most students attend schools close to home (U.S. Department of Education, Institute of Education Sciences, National Center for Education Statistics, 2012). Table 2.1 shows that in the years just before the study period, approximately three in four students attended college within 50 miles of their permanent address. Because the assumption of geographic proximity is less warranted for students who enroll only in online courses, I do not include institutions mostly likely to enroll this type of online education student—for-profit two-year colleges and selective public and private universities—in the study sample.

To further support this assumption of proximity, I consider only the number of students who split their coursework between face-to-face and online courses. That these students must be on campus at least part of their time only bolsters my assumption that they live nearby. Finally, I assume that the outcome of interest involves online coursework rather than other methods of distance education (*e.g.*, television, radio, CDs, mail-based correspondence). While the IPEDS variable that I employ allows for many types of distance learning technology, I argue that based on historical trends and the current technological landscape (Allen & Seaman, 2011; Allen et al., 2016; Moore & Kearsley, 2011), online work makes up the vast majority, if not functionally all, of the distance coursework during the years from which my sample is drawn.

2.4 Estimation strategy

I estimate the association between three measures of broadband access—download speed, upload speed, and the number of providers—and the number of students enrolling in some online courses using both Bayesian single and multilevel regression models. The Bayesian paradigm, as opposed to a frequentist paradigm, is warranted in this instance for two primary reasons.

First, the data I have represent the entire population of public open admissions postsecondary institutions that have students who enroll in online courses. Furthermore, these data are situated in their specific historical context, a time in which online-based online courses were growing in popularity and the technological means by which they were made possible were also developing and improving (Allen et al., 2016; Federal Communications Commission, 2016; Snyder et al., 2016). A repeated sampling framework, on the other hand, is predicated on the belief that the analysis data represent a random sample of data from a population. The key assumption is that under repeated samples, which would realize different analysis data, fitted models would produce parameters that would converge to the true and fixed values. Were I to gather these institutional and broadband data again, however, I would not expect differences since I have the population of data (Western & Jackman, 1994). I argue that a Bayesian approach, which instead views the data as fixed and the parameters as random, is more applicable to this situation (Jackman, 2009).

A second reason I employ a Bayesian framework is the nested nature of my data. Because schools are nested in states, hierarchical models that can account for correlation between units in groups may be preferred to single-level linear models (Gelman et al., 2014). The nature of the Bayesian framework is such that it is relatively straightforward to move from single-level to multilevel models in terms of estimation and interpretation.

In the analysis, I first estimate a number of single-level Bayesian linear regression models that take the form

$$log(y_i) \sim N(\alpha + \beta Broadband_i + \mathbf{X}\gamma, \sigma_y^2), \tag{2.1}$$

where y_i is the number of students who enroll in some online courses; α is a constant term; β is the parameter of interest for $Broadband_i$, the institution's measure of broadband; and $\mathbf{X}$ is a matrix of covariate data values with γ as its corresponding vector of parameters. Because the number of students who take some online courses is right skewed, I fit the log transformation of these values. This procedure normalizes the outcome, which allows me to use a normal likelihood function with a variance of σ_y^2. Using the natural log of the outcome has the added benefit of making β represent the percentage change in the number of students who take some online courses for each unit increase in the broadband measure of interest (Greene, 2012).

A key feature of Bayesian estimation is that one must set priors on all non-fixed parameters. These priors reflect the researcher's prior beliefs about what the parameters should be. One way of interpreting Bayesian analysis is that it is simply the use of new data to update prior beliefs. Broadly, priors may be strong or weak. The results of a Bayesian estimation, the posterior, are a compromise between the prior and data that weights each by the strength of its information. Strong priors in the face of weakly-informative data result in posterior beliefs that are not much changed. Weak priors estimated alongside strong data allow the data to "speak for themselves" (Gelman et al., 2014), giving results that are generally similar to those returned in a comparable frequentist analysis. In the single-level models, I utilize weakly-informative priors meaning that my results are driven by the data and may be interpreted much as they would be were they generated by ordinary least squares.[2]

To account for the nested structure of the data, in which institutions are located in states, I also fit two types of Bayesian multilevel models: one that allows state-varying intercepts and a second that allows both intercepts and β to vary across the states. One particular advantage of multilevel models over single-level models is that they allow information to flow between observations within groups. To produce state-specific estimates, which may be warranted due to state-level differences in the higher education policy context, I could conduct separate estimations for each state. Due the small within-state sample sizes, however, this procedure would produce noisy estimates. The multilevel model allows for partial pooling of

[2]When coding the model, I technically use improper priors. Priors are improper when $\int p(\theta)\,d\theta \neq 1$, that is, the probabilities do not sum to one. All regression coefficients are drawn from a uniform distribution with support on $\theta \in (-\infty, \infty)$ and all variances from a positive uniform distribution: $\theta \in (0, \infty)$. Improper priors may combine with a likelihood function, however, to produce proper posterior distributions (Gelman et al., 2014).

the state-specific estimates across states. Institutions in states with few or no other observations can "borrow strength" (Jackman, 2009) from other similar institutions based on their group-level characteristics. With this "bias/variance" trade-off (Carlin & Louis, 2009; Gelman et al., 2014), I am be able to estimate the differential connection between broadband access and the number of students enrolled in online courses even in states with relatively few observations.

In the first multilevel model, each state is allowed to have its own intercept. It takes the form

$$\begin{aligned} log(y_i) &\sim N(\alpha_j + \beta Broadband_i + \mathbf{X}\gamma, \sigma_y^2) \\ \alpha_j &\sim N(\delta_s Region_s + \mathbf{Z}\psi, \sigma_s^2), \end{aligned} \tag{2.2}$$

in which **X** represent a vector of school and county level covariates, **Z** are state-level covariates, and unknown parameters $(\beta, \gamma, \psi, \sigma_y^2, \sigma_s^2)$ are again given diffuse priors. In the second specification of the multilevel model, the effect of download speed is also allowed to vary within each state:

$$\begin{aligned} log(y_i) &\sim N(\alpha_j + \beta_j Broadband_i + \mathbf{X}\gamma, \sigma_y^2) \\ \alpha_j &\sim N(\delta_s Region_s + \mathbf{Z}\psi_s, \sigma_s^2) \\ \beta_j &\sim N(\delta_s Region_s + \mathbf{Z}\psi_s, \sigma_s^2) \end{aligned} \tag{2.3}$$

I fit each model type using each measure of broadband separately and together in a single equation. In each case, measures of broadband include both the level and squared value. For varying slope models, both the level and squared terms are allowed to have state-specific parameters. Both the full sample of institutions and the subsample of two-year institutions are used as data, producing eight sets of results for each of the three model types.[3]

[3]In an alternative specification, I use the proportion of students as the outcome of interest. To properly model the proportion, which is bounded by [0,1], I use a beta likelihood function. The results for these models are qualitatively the same as those given by the log outcome/normal likelihood models, so I present the latter for ease of interpretation. More details about the beta likelihood specification as well results from the models are shown in Appendix 2.A.

2.5 Data

2.5.1 Institution data

Data on the number of students who enroll in online courses were taken from the Integrated Postsecondary Education Data System (Department of Education, 2015). Though the IPEDS survey has asked institutions if they are primarily distance learning schools for a number of years (a binary *yes* or *no* response), it has only asked institutions to break down the total number of students who attempt distance learning coursework since the fall of 2012.

IPEDS defines distance education as "Education that uses one or more technologies to deliver instruction to students who are separated from the instructor and to support regular and substantive interaction between the students and the instructor synchronously or asynchronously." (Department of Education, n.d.) The code book further defines the technologies that may fall under the heading of distance education as including "Internet; one-way and two-way transmissions through open broadcasts, closed circuit, cable, microwave, broadband lines, fiber optics, satellite or wireless communication devices; audio conferencing," (Department of Education, n.d.). While not all of these technologies are strictly broadband-based, it is likely that most distance education students at public open access institutions in the sample period experienced distance education through online portals (Allen & Seaman, 2011; Allen et al., 2016; Moore & Kearsley, 2011). For this reason, I refer to these students as *online* students.

Specifically, I use the IPEDS variable that gives the number of students who took some of their courses online as the primary outcome. This number indicates the number of students who enrolled in both online and face-to-face courses. Because the data do not include the number of online courses that students took, the dosage of online course-taking within and between schools is unobserved. Sometimes-online students need only take one of each type of course to be labeled as such. This means that whereas some students represented in the data could have had balanced course loads, others were predominantly online students who took one face-to-face course or mostly in traditional courses with a single online course enrollment. Though it may be the case that broadband speeds are positively correlated with the number of online courses attempted, these data do not support this particular analysis.

Based on my theoretical framework, I include a number of school-level covariates in some model specifications. Online courses may reduce costs for students by increasing access or it may increase costs by increasing uncertainty. Though the direction of its potential effect is unclear, it is possible that student populations will be differentially affected. To account for potential heterogeneity in response to online coursework, covariate models include, at the institutional level, proportions of students who receive Pell grants, non-white students, women, students who are 25 years old or older, and part-time attenders. Only undergraduate enrollments are considered. Models using the full sample of institutions include an indicator for status as a two-year college. As control measures, all models include the log transformation of the total number of students enrolled and indicators for the survey year of the observation.

Table 2.2 shows means and standard deviations of the numbers of students taking courses for credit, those taking some online courses, and other institutional covariates. I drop schools in Alaska and Hawaii due to their unique spatial situations—Alaska as a large, but sparsely populated state and Hawaii as an island group—that may bias the weights used to construct the broadband measures that I assign to each institution. Because data for all branches of Indiana's public two-year institution, Ivy Tech, were aggregated under a single identification code, these institutions were also dropped from the sample as broadband measures could not be accurately assigned to them. The final estimation samples represent 1,018 unique public open admissions institutions observed across three years.

2.5.2 Broadband data

Broadband data were collected from the National Broadband Map website.[4] Gathered at the behest of the National Telecommunications and Information Administration in partnership with the Federal Communications Commission, these data were collected from Internet service providers (ISPs) within each state by appointed grantee agencies. Each service provider gave information about upload and download rates at the census block level as well as information on the number and types of community anchor institutions (typically libraries, K-12 schools, college, *etc.*) in the area (National Telecommunications and Information Administration, 2011). These data were corroborated against other sources of broadband information and released to the public. Data released in December 2012, 2013, and 2014 were used since they were collected around the same time period as the IPEDS data and represent the best estimates of broadband connectivity surrounding the sample institutions during the time period under study.[5]

Internet service providers reported measures of broadband speed in ordered categories that range from

[4]www.broadbandmap.gov

[5]Though student enrollment data have now been released for the fall of 2015, the National Broadband Map stopped being updated in June 2014. I have not incorporated the newest enrollment data for this reason.

1 (greater than 200 kB/sec and less than 768 kB/sec) to 11 (greater than 1 GB/sec). Past FCC guidelines suggested that the minimum required speed to watch university lectures was 4 MB/sec, which would fall under category 5.[6] Acknowledging the increasing "speeds required to use high-quality video, data, voice, and other broadband applications," (Federal Communications Commission, 2015a, p. 3) as well as the demands placed on broadband connections by multiple users within the average household, the FCC recently updated these benchmarks, arguing that "having 'advanced telecommunications capability' requires access to actual download speeds of at least 25 Mbps and actual upload speeds of at least 3 Mbps," (Federal Communications Commission, 2015a, p. 3). These new benchmark speeds fall under category 8. Figure 2.1 shows the variability in download speeds across the country as of December 2013. County-level values represent the population weighted average of all values within each respective county. The map makes clear the heterogeneity in average download speeds experienced by persons across the country. It also shows that while many counties have broadband-level Internet speeds under the old definition (roughly category 5), many fewer have broadband speeds under the new definition (category 8) (Federal Communications Commission, 2015a, 2016).

A primary question for this analysis: how does one assign a broadband measure to each institution? Were the unit of analysis the student and I knew home locations, I could simply assign each student the broadband measures of his or her home census block group. But because the unit of analysis is the school, I am unable to see where each student lives and must assign broadband measures to the institution that takes into account those experienced by the average student who is enrolled there.

One solution would be to use the measures computed to produce Figure 2.1, assigning each school those of its county. Yet postsecondary students, especially those who are part-time, older, or attend non-residential schools, may not live in the same county as the one in which their institution is located. As shown in Table 2.1, a large portion of students may travel more than 20 miles to attend classes. To assign each institution the average download speed in its county, for example, may neglect the broadband experience of students who live in an adjacent county with very different download speeds.

Rather than rely on somewhat artificial political boundaries, such as county borders, I argue that measures assigned to each school should give more weight to those recorded nearby, with adjustments based on population that up-weight measures recorded in areas with higher population density. To better approximate the most likely distribution of students around institutions, I assign each school broadband measures

[6] www.fcc.gov/guides/broadband-speed-guide

using a weighting process that takes into account both (1) its distance to surrounding census block group-level measures and (2) the populations of those census block groups. The parts of the combined inverse-distance/population weight are discussed in turn below.

The inverse-distance weight was constructed by first computing the Great Circle distance, d_{sc}, from each institution, s, to all census block group centroids, c, in the lower 48 states to produce an $N \times K$ matrix where $N = \#$ schools and $K = \#$ census block groups. For the year 2013, this resulted in a $1,004 \times 217,290$ matrix of distances. So that download speeds of nearby census block groups would count more than those of distant census block groups, these values were inverted and scaled to create inverse distances, id_{sc}:

$$id_{sc} = \frac{1}{(d_{sc})^r}. \tag{2.4}$$

Finally, weights were created by dividing each inverse distance over the row sums of the inverse distances (each row representing the distance between a single institution and all census block group centroids):

$$idw_{sc} = \frac{id_{sc}}{\sum_{c=1}^{C} id_{sc}}. \tag{2.5}$$

Were the data used in this study at the student level, the inverse distance weight would be sufficient to approximate the broadband measures experienced by each student (if I did not wish to simply assign them the values of their census block group). But because the data are aggregated to the institution level, I do not know exactly where the students live in relation to the school. Some may live very close whereas others commute from further away. It is also unlikely that students are evenly distributed around the school. It is more likely that students come from nearby population centers: towns, suburbs, and neighborhoods. The inverse distance-weighted average assigned to the school, therefore, is likely to be different from what would be assigned to the students were their addresses known.

To mitigate this potential bias, I employed a second weight that adjusted each school's inverse distance-weighted broadband measure average back toward values recorded in more highly populated areas. Accounting for population in spatial-weighting schemes has been shown to improve upon estimates in which only inverse distance weights are used (Hanigan, Hall, & Dear, 2006). Using census block group population estimates taken from the 2010 Census, I constructed a second $N \times K$ matrix (again, $N = \#$ schools and $K = \#$ census block groups) in which each column represents a census block group's population, pop_c, repeated in each row. The population weight, pw_c, is simply

$$pw_c = \frac{pop_c}{\sum_{c=1}^{C} pop_c}, \tag{2.6}$$

or the population in the matrix cell divided by the row sums.[7] In applying the second weight, I make the assumption that the likelihood a student lives in a particular census block group around the institution is proportional to that block group's population size. Lest major metropolitan centers unduly skew the average too far away from the institution (*e.g.,* census block groups in Charlotte, North Carolina, affecting averages of schools in the eastern part of the state), I use a quadratic decay ($r = 2$) in the inverse distance weight formula (Shepard, 1968). At this rate of decay, the effect of neighboring census block group broadband values on the institutional average quickly diminishes with distance, even when population sizes are taken into account. Thus the second population weight serves as a slight correction to the inverse distance weight, which dominates in the final computation of each broadband measure average at an institution.

The combined weights, $w_{sc} = pw_c \times idw_{sc}$, were then used to create weighted average broadband measures for each school, $wbroadband_s$,

$$wbroadband_s = \sum_{c=1}^{C} \frac{w_{sc} \cdot broadband_c}{\sum_{c=1}^{C} w_{sc}}, \tag{2.7}$$

in which $broadband_c$ is the average broadband measure—download speed, upload speed, or number of providers—in the census block group. Each weighted average was computed using census block group broadband values reported in the December of the fall term in which the online course enrollment numbers were reported. Schools that appear more than once in the data set (the majority), therefore, have distinct broadband measure averages for each year.

Figure 2.2 offers a stylized visualization of this weighting process. Using Nashville State Community College (NSCC) as an example, dotted lines connect its location in Davidson County, Tennessee, shown by the gold diamond, to population centers in surrounding counties. (For clarity, the black dots represent county-level population centers rather than the census block group centers that were actually used.) The average download speed, for example, assigned to NSCC would be most influenced by the value recorded

[7] Because the census block group population values are constant, it is not strictly necessary build an entire $N \times K$ matrix, which is simply a vector of values of length K repeated N times, or compute the row sums and weights N times. For purposes of computation, however, it is easier to build a full matrix that can be easily combined with the other weighting matrix when computing the final average.

for the Davidson County center, the nearest black dot just the right of NSCC in the center of the map. Because the Davidson County value also represents the highest population density in the area, NSCC's average download value would be even further pulled in that direction.

The distribution of the weighted broadband measures across each year are shown in the second row of Table 2.3. Due to the asymmetric design of most residential broadband networks (Federal Communications Commission, 2016), the average download speed is greater than that of the average upload speed. As might be expected due to continual demand for greater speeds, both download and upload averages increase in subsequent years, though the number of providers remains relatively similar (Federal Communications Commission, 2016).

2.5.3 Geographic and demographic data

When estimating multilevel models, I use additional state level covariates to aid in fitting second level parameters. These include measures of statewide unemployment rates, which were taken from data provided by the Bureau of Labor Statistics (Bureau of Labor Statistics, 2012, 2013, 2014). To account for potential differences that funding structures could have on the availability of online courses, measures of state appropriations per full-time equivalent student in each year were gathered from a report produced by the State Higher Education Executive Officers Association (SHEEO) (Carlson et al., 2015). I also include a measure of the proportion of two-year open admissions public institutions within each state that I computed using data from IPEDS. These variables were intended to account for potential differences across states in the number of students who might be likely to attend open admissions universities and/or attempt some online courses.

Because the likelihood of students enrolling in online courses (or an institution offering more sections) might be correlated with the average distance a student must travel to reach a postsecondary institution (Xu & Jaggars, 2013), I incorporate a measure of the average distance a person would have to travel to get to the nearest open admissions institution. To compute this measure, I first found the distance from each census block group centroid to the nearest public open access postsecondary school. I then averaged these distances to the state level, using the relative population in each block group as the weight. Though a rough measure, it does give an indication of the spread of institutions around each state in terms of its population centers and is variable across the states. I include the log transformation of this measure alongside other state-level measures.

Finally, to account for potential differences between students living in rural and urban areas (Cejda, 2007), I include both a self-constructed measure of population density for each school and an array of indicators for degree of urbanicity/rurality. I computed the first measure by summing the averages of census tract density (tract population divided by land area) to the county level and assigning each institution the value of its county. Indicator variables for degree of urbanicity come from the United States Department of Agriculture, which assigns all counties one of nine rural-urban continuum codes (United States Department of Agriculture Economic Research Service, 2013). As with the population density, I assign each school the value of its county. Unlike the measures above, I incorporate both of these measures into the vector of first-level covariates in all models.

2.6 Results

To generate my results, I utilized a computationally-based Markov chain Monte Carlo algorithm that fit each Bayesian model a repeated number of times. While simple Bayesian models may be solved analytically, non-trivial equations are often too complex or have no closed-form solution (Gelman et al., 2014). To solve these problems, a computer program uses an iterative process to propose, compare, and either accept or reject parameter values. With enough iterations, parameters produced by the process will come from the true posterior distribution (Brooks, Gelman, Jones, & Meng, 2011). Though no tests exist that can determine whether enough samples have been drawn so that the true posterior is effectively summarized by their distribution, there are a number of best practices that support such a conclusion.

First, the algorithm is generally run multiple times to generate multiple chains of results. If these independent chains converge, that is, give distributions of results that are the same, this supports a conclusion that the draws summarize the posterior density distribution. For this analysis, I ran four independent chains with different starting values that appear to converge based on visual inspection of density plots and Rubin-Gelman statistics, which compare within-chain variance to between-chain variance, close to 1 (no significant difference) (Gelman et al., 2014).

Second, each chain should have a large number of draws. Because each chain starts with different values that are unlikely to come from the posterior, a large number of iterations is needed so that chains have time to reach the posterior. To prevent the initial and likely improbable starting values from biasing the results, it is common practice to discard some number of initial draws (Gelman et al., 2014). For all models, each of the four chains was run for 2,000 iterations and the first 1,000 of these values discarded. Combining these chains means that results for each model are a function of 4,000 draws.[8]

2.6.1 Single-level models

Table 2.4 shows the results for the single-level linear models. The Bayesian point estimates represent the mean of the posterior distribution with the accompanying numbers in the square brackets showing the 95% credible interval.[9] Models 1-3 use each broadband measure—download speed, upload speed, and the number of providers—in turn along with its quadratic. Model 4 uses all broadband measures and their quadratics in the same equation. All models include indicators for two-year institutions, sample year, and USDA urban/rural continuum code (not reported); the natural log of the institution's total of student enrollment as well as its proportions of non-white students, women, Pell grant recipients, part-time attendees, and students 25 years and older; and the county-level measure population density, logged. In all models, the dependent variable is the natural log of the number of students who took some online courses. The parameter posterior distributions therefore represent the percent change in the number of sometimes online students for a one unit change in the covariate.

I first consider model (1), which uses download speed as the measure of broadband access. Because all right-hand-side variables were centered, the intercept, α, may be interpreted as the expected log number of students who take some online courses for average institution.[10] At $\alpha = 6.729$, this translates to about 836 students (95% credible range: [817, 856]). Though this number is lower than that shown in Table 2.2 (1391 students), it accounts for different enrollment sizes across institutions as well as changes over time that skew the unconditional average. Indeed, the mean values for the year indicators, $\beta_{2013} = 0.103$ and $\beta_{2014} = 0.157$, show positive growth in the number of students taking online courses on the order of 10 to 15% over that seen in 2012. This aligns with general trends described elsewhere in the literature (Allen et al., 2016; Snyder

[8] All models were estimated using command line version of Stan's No-U-Turn Sampler (NUTS), a variant of the Hamiltonian Monte Carlo sampler that may more efficiently explore the parameter space. To reduce the amount of lagged auto-correlation between successive draws in the chains and improve convergence, all models were estimated using centered data (Lunn, Jackson, Best, Thomas, & Spiegelhalter, 2013) and a QR reparameterization.

[9] All reported 95% credible intervals represent the middle 95% of the posterior distribution, meaning that the lower and upper bound values are the 2.5% and 97.5% quantile values, respectively.

[10] Throughout the rest of the chapter, I will generally use the Bayesian point estimates when referring to the mean of parameter's posterior distribution. They should be understood, therefore, in their proper context as useful summaries of full probability distributions.

et al., 2016).

All else equal, two-year institutions are likely to have around 7% more ($\beta_{two-year} = 0.068$) of their students take some online courses than four-year institutions. Though all schools in this analysis utilize open admissions practices, this finding may reflect the two-year sector's particular focus on access (Cox, 2006). Concerning characteristics of the student body, the results are mixed. On one hand, institutions with greater proportions of Pell grant recipients and students over 25 years old show increases in the percentage of students taking some online courses. These findings are in line with arguments of online courses as means to increase access (Bowen, 2013; Selingo, 2013). On the other hand, schools with greater proportions of women, non-white students, and those attending part time are associated with lower percentages of students taking some online courses, which runs counter to the same arguments. One potential explanation may be that the high correlation between some of these student body characteristics, many of which fall under the "non-traditional student" designation (Snyder et al., 2016), produces counter-intuitive marginal results.

Two other institution-specific characteristics are of interest. First, schools located in areas with higher population density have fewer students who take some online courses. For a 10% increase in the population density surrounding an institution, 0.7% fewer students choose to enroll in some online courses ($\beta_{log(population density)} = -0.074$). Though the relationship is weak (keeping in mind that the degree of urbanicity is also accounted for in the model by the USDA RUCC codes), it does support arguments that online courses may be particularly appropriate in less populated areas that are less likely to have strong transportation networks (Copps, 2009). Second, the model shows that as the total number of enrolled students increases, proportionally greater numbers of them take online courses, with every 1% increase in the former associated with a 1.154% increase in the latter ($\beta_{log(enrollment)} = 1.154$). The positive elasticity of this relationship provides evidence that recent growth in enrollments may be due in part to increases in the number of students taking online courses, which, once prepared, benefit from economies of scale (Deming et al., 2015).

Turning to the parameter of interest in model (1), $\beta_{download}$, Table 2.4 shows a positive association between tiers of download speed and the number of students enrolling in some online courses. For a single-tier increase in speed, akin to roughly a 2 to 3X increase in megabytes/sec (Mbps) download rate, approximately 32% more of an institution's students enroll in some online courses ($\beta_{download} = 0.324$). The credible intervals show much variance in the probable marginal effect, however, ranging from a high point of a 69% increase to a low point that is negative, -5%. Though Bayesian analyses do not utilize null hypothesis significance tests in the way that frequentist analyses do, it nonetheless may be useful to consider the 95% credible intervals as boundaries of interest. Because the lower bound of the credible interval on $\beta_{download}$ is negative, it suggests some uncertainty about the parameter.[11] Yet since fully 96% of the sample draws are greater than zero, $Pr(\beta_{download} > 0 = 0.961)$, the model indicates that there is a greater than 95% probability that the association between download speed and online course enrollment is positive.

Results for models (2) and (3), which operationalize broadband access using upload speed and the number of providers, respectively, are reported in the next two columns of Table 2.4. The posterior distributions of the covariate parameters in each remain much as they were in the first model. But unlike download speed, neither upload speed nor provider count are strongly predictive of online course enrollment numbers. Though both β_{upload} and $\beta_{provider}$ posterior distributions have negative mean values—$\beta_{upload} = -0.075$ and $\beta_{provider} = -0.003$—each has large portions of its distribution above zero: $Pr(\beta_{upload} > 0 = 0.202)$ and $Pr(\beta_{provider} > 0 = 0.480)$.

In the fully specified model (4), download speed once again is the most strongly predictive of the broadband measures. On average, a single tier increase in download speed is associated with a 41% increase ($\beta_{download} = 0.41$) in the number of students who attempt some online courses. The large spread between the 95% credible interval suggests a wide range of possible marginal effects, from 8% to 82% increases in online course takers. In this model, however, the lower bound does not cross zero, which provides stronger evidence that the marginal effect is positive. The credible intervals for both upload speed and provider coefficients once again cross zero and are therefore comparatively uninformative.

In second set of models, I limit the data set to two-year institutions. Results from these models, which are shown in Table 2.5, generally follow the same pattern as those found when four-year institutions are included. None of the broadband parameters of interest, however, are as informative. In all models, the 95% credible intervals indicate greater variance in the posterior distributions as well as cross zero. There are two possible explanations for this finding.

First, it could be that broadband measures are more salient for online course enrollment at four-year institutions (or, alternately, less salient at two-year institutions). If the average student at a four-year institution is less constrained when choosing course sections than the average student at a two-year institution, then broadband speed and/or access may become more important in terms of online course enrollment. Alterna-

[11] If the mean value presented in the table represented a frequentist point estimate, one could not reject the null that $\beta = 0$ under a two-tailed test of significance at conventional levels of significance ($\alpha = 0.05$). This is not to say, however, that it would not be jointly significant with its quadratic term.

tively, students at two-year institutions, who are more likely to be non-traditional (Snyder et al., 2016), may have other factors that limit their effective choice set and make broadband speeds less important (Jaggars, 2014).

Second, it may be the case that the reduced sample simply has less information about a potential connection. Four-year institutions make up approximately 12% of all observations in the data set and contain about 13% of the undergraduate student population represented by the schools. Yet 18.9% of students at four-years took some courses online (an average of 1,524 students per school), while 17.6% of students at two-year institutions did so (1,372 average per school). Dropping four-year institution observations from the analysis may remove enough information that wider, less informative posterior distributions are the result.

2.6.2 Hierarchical models

To account for the nested nature of the data and estimate potentially heterogeneous effects across states, I next fit two sets of multilevel models (Gelman et al., 2014). In the first set, I allow the intercept to vary by state. As with the single-level linear regression models, the dependent variable is the log transformation of the number of students who attempt online courses. In addition to the covariates used in the single-level equations, I include a number of second-level covariates to help predict each state's unique intercept. These include the state average unemployment rate, statewide average appropriations per full time equivalent student, the proportion of open admissions public two-year institutions within the state, and the population-weighted average distance to the nearest public open admissions institution. In interest of space, coefficients for these parameters and the varying intercepts are not reported.

Table 2.6 shows the posterior means for these models. Again, I fit four separate equations in which each broadband parameter was included singly and alongside the others. Broadly the results from the varying intercept multilevel models are similar to those found in the single level models. In models (1), (2), and (3), all of the broadband parameters of interest measures have 95% credible intervals that include zero. In the fully specified model (4), however, the marginal effect of download speed is once again positive ($\beta_{download} = 0.556$) and with a 95% credible interval that does not cover zero [0.166, 0.957]. For each tier increase in download speed, institutions show a 56% average increase in the percentage of students who attempt some online courses, with a 95% probable association between 17% and 96%.

Though the number of providers, as in the single-level models, remains less informative, the marginal effect of upload speed in model (4) becomes more informative. For each tier increase in upload speed, 22% fewer students are expected to take some online courses. This result appears counter-intuitive. Why should improved upload speeds be associated with reductions in online course take-up? One possibility lies in the asymmetric nature of most residential broadband connections. Downloaded and uploaded data travel the same line, so ISPs must decide how to balance the load. Because most Internet services are structured to send audio, video, text, and other files to the end-user who in turn needs only send small files of instructions regarding what to download, most residential broadband connections are asymmetric, meaning they allow download rates to be much higher than upload rates (Federal Communications Commission, 2014, 2016). If increased upload rates come at the expense of download rates and download rates are more salient for online students, then it is reasonable that the average marginal effect of upload speed be negative (see in particular model (2) in Table 2.6).

Another reason for this counter-intuitive finding may lie in the technologies used to serve broadband. Though some people have access to broadband served through fiber optic lines, which use glass fibers and light to send digital signals, many users with wired connections have their broadband delivered through copper wires that also transmit their telephone or cable television signals (National Telecommunications and Information Administration, 2011). Broadband data transmitted over these lines more quickly degrades with distance, making it more difficult for ISPs to separate signal from noise as well as separate the data requests of users who share the line (reduce "crosstalk") (Grubesic & Murray, 2002). Holding download speeds and the number of providers constant as in model (4), it may be that increases in upload speed allow for more traffic on wired lines that degrades the average signal quality for users along the line. The negative sign, therefore, could reflect a decrease in the proportion of students who attempt some online courses due to a less robust broadband connection, not necessarily faster upload speeds. I note, however, that because the marginal increase for download speed in model (4) is larger than the decrease for upload speed, even concomitant increases in both measures (+1 tier increase in both download and upload speeds) is associated with a 33% average increase in the number of students taking some of their courses online.

Table 2.7 shows the results from the varying intercept models fit to the subset of two-year institutions. The posterior means generally follow the pattern set by those found for the full sample. As before the 95% credible intervals become wider and in many cases cross zero. Though the indicator for status as a two-year institution remains positively associated with students taking some online courses when four-year institutions are included (Table 2.6), once again the evidence points to students at four-year institutions being an important part of the overall estimated effect.

In a final set of models, I allowed parameters on broadband measures to vary by state in addition to the intercepts. As with the single-level and varying intercept models, I include each broadband parameter and its quadratic independently (models 1-3) and together (model 4). Rather than report a large table of unique intercepts for each state, I instead show the differences in marginal effect in two figures. In Figure 2.3, only download speed is included in the model (model 1). In each plot, the blue central dot represents the median posterior value. The red and gray error bars show the 68% and 95% (1sd and 2sd) of the distributions. A black vertical line is set where $\beta_j = 0$. The plot on the left shows the results produced for the full set of institutions; the plot on the right shows those for the subset of two-year institutions.

Focusing first on the left plot in Figure 2.3, four states—Tennessee, Colorado, Alabama, and Oklahoma—show positive associations between download speed and enrollment in some online courses and have 95% credible intervals that do not include zero. At the other end of the plot, both Texas and Louisiana show strong negative associations and similarly have credible intervals that do not include zero. The rest of states all have credible intervals that cover zero, with median marginal effects that fall on either side of zero. Looking at only two-year institutions in the right plot, students at two-year institutions in Texas and Louisiana continue to show strong negative reactions to increased broadband speeds whereas only Colorado and Alabama show significant positive marginal effects.

Figure 2.4 shows state-level differences in the marginal effect of download speed on online course enrollment in models that also contained varying slopes for upload speed and the number of providers (model 4). In these models, Tennessee, North Dakota, Alabama, and Oklahoma show positive associations credibly above zero in the full sample; only Alabama also shows a similar association in the two-year subsample. No states in either the full sample or two-year sample show a credible negative association.

Together, Figures 2.3 and 2.4 suggest that students in some states more than others may be driving the results found in the single level and varying intercept models. Though states such as Tennessee and Oklahoma have many rural students who may be more inclined to take some online courses, other equally rural states do not show such a strong association. In fact, Louisiana, a Southern state like Tennessee and Alabama shows credibly negative association between download speeds and online course enrollment. These varying slope estimates should be interpreted with caution, however, due to the reduced sample sizes within each state that increase credible intervals in low population states (*e.g.*, Wyoming), making state-specific estimates less certain.

2.7 Conclusion

This chapter offers empirical evidence of a positive association between broadband access and student enrollment in online courses at open admissions public colleges and universities in the United States. In preferred model specifications, I find that single tier increases in download speeds in the area surrounding an institution correspond to 41% to 56% average increases in the number students who take some online courses. Because the majority of these parameter posterior densities lie to the right of zero, the marginal effect of increased download speeds is almost certainly positive. The precise marginal effect is unclear, however, as the 95% credible intervals across the models indicate a wide range of possible values—as much as 17% to 96% in the varying intercept hierarchical model. Among the subset of two-year institutions, I find similar but less precise results.

I do not find similar correlations for upload speed. Generally, upload speeds appear to be negatively correlated with online enrollment numbers. With the exception of a single model, however, these estimates are less certain than those estimated for download speeds. The negative marginal effect for upload speeds is curious, but as I note above, the asymmetric design of most broadband connections may mean that increases in upload speed come at the expense of download speed. If online courses for most students consist of streaming audiovisual materials and submitting only papers or taking online tests, then download speeds should be comparatively more important than upload speeds. My results support this hypothesis.

The average number of providers in an area, on the other hand, generally has no clear association with the number of students taking some online courses. In all models, the parameter on this measure have 95% credible intervals that cross zero. In many models, the average marginal effect is close to zero. In theory, increases in the number of providers should foster competition that lowers prices and/or improves speeds. That students do not appear to respond to a greater number of providers suggests a market failure due to a lack of real competition. Despite nominal access to the services offered by ISPs, geographic restrictions and costs may prevent some users from actually accessing those services (Grubesic & Murray, 2002; Oyana, 2011). The lack of clear association between the number of providers and the take-up of online courses may reflect this difference between the nominal number of ISPs, which I can observe in the data, and actual number of ISPs reasonably available to users, which I do not.

Finally, I find evidence of heterogeneous effects of broadband access across the states. Schools in some states show strong positive associations between download speed and the number of sometimes online students. Schools in most other states show little connection, and, in a couple of states, credibly negative correlations. The high variability in state-level estimates could occur for a number of reasons. If download speed measures are measured with error, any relationship between broadband and online education should be expected to attenuate. Similarly, the aggregated broadband measures that I construct may smooth over differences and reduce the variation required to untangle clear effects. My study, through its reliance on the assumed proximity of online students to their institutions and many aggregated measures, may suffer from such attenuation. Policy contexts within each state may also drive differences between them. Those with strong positive associations between download speed and online enrollments may have higher education policy contexts conducive to institutions offering online courses. States with little or negative associations, on the other hand, may lack the capacity or interest in expanding online offerings.

As a limitation, I note that the dependent variable I utilize does not measure course completion, only the number of students who enrolled in some online courses. Student persistence within the course is an important outcome to measure since students may make the rational decision to withdraw rather than persist and earn low grade or fail the course (Xu & Jaggars, 2011, 2013). Because the course enrollment decision typically comes in the months prior to the start of the course, it may be that some students only realize broadband access barriers after enrolling. As I note in section 2.3, students may not have a clear idea about the quality of their area broadband or, if they do, how it corresponds to the demands of an online course. Thus while my results speak to the possible effect that broadband access has on students' decisions regarding whether to enroll in online courses (or for institutions to offer them), they do not extend to student performance in online courses. I take up this question in chapter 3 of this dissertation.

Another limitation of this chapter is that I cannot differentiate between student demand for online courses and institutional supply. Increases in broadband speeds may induce students to demand more online course options. Conversely, postsecondary institutions situated in areas with better broadband access may choose to offer more online course options. Under this second scenario, students only respond to increases in broadband quality insofar as their schools do by offering more online courses (or comparatively fewer face-to-face courses). My findings, therefore, should not be interpreted causally, but instead as regression-adjusted measures of the association between broadband speed and online course enrollment.

These potential limitations notwithstanding, my results offer support for the conclusion that access to quality broadband, particularly in the form of faster download speeds, is an important component of the choice to enroll in some online courses. Schools, however, may choose to offer online courses regardless of area broadband speeds based on their institutional needs and goals (Allen & Seaman, 2011; Allen et al., 2016; Moore & Kearsley, 2011). If students are effectively limited in their choice set to these courses, they may be forced to register for them regardless of their broadband connectivity. State-level higher education policies may also support online education to varying degrees based on financial and political rather than technological considerations (Johnstone, 2006; Kinser, 1999). Though my results speak only to enrollments and not completions, it is logical to think that improved broadband would also be associated with improved student outcomes in online courses.

A scenario of infrastructure-irrelevant policy-making, therefore, has important ramifications for the future of online higher education policy. As recent trajectories suggest, distance education delivered through online technology will almost certainly play an important part in the future of higher education (Allen et al., 2016; Bowen, 2013; Selingo, 2013; Snyder et al., 2016). Insofar as it has the potential to be less expensive on the margins and reach more non-traditional students due to its flexibility, online education will be appealing to higher education institutions, especially those without deep endowments who mostly serve non-traditional student populations. But if these populations are also most at risk for lacking access to quality broadband (Cejda, 2007; Hurst, 2010), then the move to online coursework may have negative effects on their educational outcomes.

Should it be true that students respond positively to broadband speeds, public open admissions institutions that desire to increase the number of online learning opportunities may first wish to consider speeds in their service area. They may also find partners with those in other sectors who wish to improve broadband infrastructure for economic reasons. If, conditional on equitable access, students at these institutions realize equal or better educational outcomes, improving broadband infrastructure around public postsecondary institutions may pay triple dividends in the form of improved student outcomes, lower costs, and improved infrastructure that can be shared with all members of the community.

Table 2.1: Distance from student's home (in miles) to NPSAS institution by dependency status

	< 21	21–50	51–100	101–500	> 500
Total	59.9	13.6	7.2	11.8	7.6
Dependency status					
Dep.	54.8	14.1	9.8	16.3	5.1
Ind.	64.7	13.2	4.7	7.4	10.0

Notes. Data come from the U.S. Department of Education, National Center for Education Statistics, 2011-12 National Postsecondary Student Aid Study (NPSAS:12). All numbers are percentages. Computation by NCES PowerStats.

Table 2.2: Descriptive statistics of institution sample

	Mean/(SD)
Total enrollment	7838
	(8406)
Some online enrollment	1391
	(1717)
Two year institution	0.88
	(0.33)
Non-white enrollment	0.42
	(0.24)
Women enrollment	0.42
	(0.07)
Pell grant recipients	0.42
	(0.15)
Part-time enrollment	0.57
	(0.15)
Aged 25 years and older	0.37
	(0.11)
2013	0.4
	(0.49)
2014	0.3
	(0.46)
N (2012)	751
N (2013)	1004
N (2014)	742

Notes. Total enrollment and some online enrollment represent the average number of students rounded to nearest student. Other rows are proportions. Standard deviations are shown in parentheses. Schools included in the sample are public, open admissions postsecondary institutions that report at least one student who took some distance education courses.

Table 2.3: Descriptive statistics of broadband measures

	2012	2013	2014
Download tier	6.96	7.27	7.42
	(0.72)	(0.73)	(0.74)
Upload tier	4.43	4.66	4.95
	(0.81)	(0.89)	(0.94)
Number of providers	3.26	3.43	3.45
	(0.93)	(1.06)	(1.01)

Notes. Values are the average of broadband measures assigned across all schools in the sample in a given year. Each school is given a value that is the population-distance-weighted average of surrounding measures (at the census block level). Download and upload speeds are reported in ordered categorical tiers from 1 to 11. Broadband data come from the National Broadband Map. Standard deviations are shown in parentheses.

Table 2.4: Single level Bayesian regressions of log number of students who enrolled in some distance education courses on broadband measures

	(1)	(2)	(3)	(4)
Download speed	0.324			0.41
	[-0.046,0.687]			[0.008,0.821]
Download speed^2	-0.024			-0.027
	[-0.049,0.002]			[-0.056,0.001]
Upload speed		-0.075		-0.206
		[-0.256,0.103]		[-0.432,0.017]
Upload speed^2		0.003		0.015
		[-0.014,0.021]		[-0.006,0.038]
# Providers			-0.003	0.019
			[-0.11,0.1]	[-0.097,0.131]
# Providers^2			-0.002	-0.003
			[-0.014,0.011]	[-0.016,0.011]
Two year institution	0.068	0.078	0.076	0.068
	[-0.005,0.14]	[0.004,0.151]	[0.001,0.149]	[-0.004,0.139]
log(Total enrollment)	1.154	1.155	1.155	1.157
	[1.119,1.189]	[1.119,1.192]	[1.119,1.19]	[1.121,1.193]
Prop. non-white	-0.64	-0.641	-0.646	-0.626
	[-0.756,-0.524]	[-0.756,-0.523]	[-0.758,-0.533]	[-0.747,-0.507]
Prop. women	-2.205	-2.197	-2.226	-2.199
	[-2.628,-1.782]	[-2.602,-1.804]	[-2.63,-1.817]	[-2.599,-1.798]
Prop. Pell grant	0.658	0.62	0.65	0.627
	[0.457,0.866]	[0.417,0.829]	[0.445,0.854]	[0.414,0.84]
Prop. part-time	-0.461	-0.463	-0.459	-0.463
	[-0.67,-0.247]	[-0.676,-0.243]	[-0.682,-0.236]	[-0.677,-0.252]
Prop. 25 years and older	0.379	0.399	0.396	0.414
	[0.118,0.636]	[0.153,0.645]	[0.151,0.652]	[0.169,0.657]
log(Pop. density)	-0.074	-0.065	-0.069	-0.069
	[-0.099,-0.048]	[-0.091,-0.039]	[-0.095,-0.044]	[-0.093,-0.044]
2013	0.103	0.11	0.101	0.105
	[0.048,0.159]	[0.054,0.166]	[0.044,0.158]	[0.05,0.161]
2014	0.157	0.171	0.15	0.167
	[0.097,0.218]	[0.109,0.234]	[0.089,0.211]	[0.105,0.228]
(Intercept)	6.729	6.729	6.729	6.729
	[6.706,6.752]	[6.707,6.752]	[6.707,6.752]	[6.706,6.752]
Unique institutions	1018	1018	1018	1018
N	2497	2497	2497	2497

Notes. Bayesian point estimates represent posterior mean values. Values in the square brackets are 95% credible intervals. Covariates not reported include indicators for USDA urban/rural community codes. Parameter distributions in each model are the combination of four independent MCMC chains of 1000 draws each (with 1000 initial draws discarded as burn-in) for a total of 4000 total draws. All models were estimated using the Stan NUTS sampler with a normal likelihood sampling statement. The outcome measure in all models is the log number of students at each institution who enrolled in some distance education courses.

Table 2.5: Single level Bayesian regressions of log number of students who enrolled in some distance education courses on broadband measures: two year institutions

	(1)	(2)	(3)	(4)
Download speed	0.188			0.231
	[-0.308,0.697]			[-0.369,0.829]
Download speed^2	-0.016			-0.016
	[-0.051,0.019]			[-0.057,0.026]
Upload speed		-0.111		-0.207
		[-0.329,0.109]		[-0.451,0.041]
Upload speed^2		0.005		0.015
		[-0.017,0.027]		[-0.009,0.04]
# Providers			0.049	0.075
			[-0.07,0.168]	[-0.053,0.202]
# Providers^2			-0.011	-0.012
			[-0.025,0.004]	[-0.028,0.003]
log(Total enrollment)	1.14	1.138	1.143	1.141
	[1.1,1.181]	[1.097,1.178]	[1.103,1.183]	[1.101,1.182]
Prop. non-white	-0.758	-0.746	-0.758	-0.751
	[-0.882,-0.633]	[-0.871,-0.623]	[-0.882,-0.634]	[-0.882,-0.625]
Prop. women	-2.216	-2.205	-2.231	-2.2
	[-2.65,-1.792]	[-2.646,-1.78]	[-2.674,-1.801]	[-2.621,-1.772]
Prop. Pell grant	0.699	0.652	0.708	0.668
	[0.473,0.934]	[0.425,0.873]	[0.477,0.935]	[0.428,0.895]
Prop. part-time	-0.335	-0.357	-0.321	-0.352
	[-0.584,-0.098]	[-0.604,-0.109]	[-0.561,-0.077]	[-0.597,-0.106]
Prop. 25 years and older	0.366	0.391	0.395	0.417
	[0.107,0.63]	[0.121,0.661]	[0.128,0.661]	[0.15,0.685]
log(Pop. density)	-0.064	-0.055	-0.063	-0.058
	[-0.09,-0.037]	[-0.082,-0.028]	[-0.09,-0.036]	[-0.086,-0.031]
2013	0.099	0.104	0.094	0.104
	[0.037,0.16]	[0.043,0.163]	[0.035,0.153]	[0.045,0.162]
2014	0.161	0.176	0.149	0.174
	[0.096,0.225]	[0.11,0.242]	[0.086,0.214]	[0.109,0.239]
(Intercept)	6.746	6.746	6.746	6.746
	[6.722,6.77]	[6.722,6.769]	[6.722,6.77]	[6.723,6.769]
Unique institutions	899	899	899	899
N	2186	2186	2186	2186

Notes. Bayesian point estimates represent posterior mean values. Values in the square brackets are 95% credible intervals. Covariates not reported include indicators for USDA urban/rural community codes. Parameter distributions in each model are the combination of four independent MCMC chains of 1000 draws each (with 1000 initial draws discarded as burn-in) for a total of 4000 total draws. All models were estimated using the Stan NUTS sampler with a normal likelihood sampling statement. The outcome measure in all models is the log number of students at each institution who enrolled in some distance education courses.

Table 2.6: Varying intercept Bayesian regressions of log number of students who enrolled in some distance education courses on broadband measures

	(1)	(2)	(3)	(4)
Download speed	0.32			0.556
	[-0.017,0.649]			[0.166,0.957]
Download speed^2	-0.024			-0.04
	[-0.047,-0.001]			[-0.068,-0.013]
Upload speed		-0.122		-0.224
		[-0.314,0.072]		[-0.459,-0.003]
Upload speed^2		0.009		0.022
		[-0.01,0.028]		[0,0.045]
# Providers			-0.068	-0.081
			[-0.173,0.042]	[-0.193,0.03]
# Providers^2			0.006	0.007
			[-0.008,0.018]	[-0.006,0.02]
Two year institution	0.087	0.099	0.1	0.093
	[0.003,0.172]	[0.016,0.186]	[0.014,0.184]	[0.006,0.179]
log(Total enrollment)	1.134	1.133	1.134	1.135
	[1.098,1.17]	[1.097,1.17]	[1.099,1.17]	[1.099,1.171]
Prop. non-white	-0.795	-0.792	-0.787	-0.801
	[-0.946,-0.641]	[-0.948,-0.637]	[-0.938,-0.628]	[-0.954,-0.647]
Prop. women	-2.089	-2.092	-2.1	-2.115
	[-2.476,-1.701]	[-2.501,-1.694]	[-2.509,-1.698]	[-2.526,-1.706]
Prop. Pell grant	0.525	0.505	0.495	0.506
	[0.279,0.772]	[0.268,0.752]	[0.243,0.745]	[0.262,0.75]
Prop. part-time	-0.847	-0.827	-0.846	-0.844
	[-1.095,-0.597]	[-1.078,-0.588]	[-1.099,-0.605]	[-1.092,-0.598]
Prop. 25 years and older	0.221	0.221	0.236	0.237
	[-0.042,0.496]	[-0.04,0.479]	[-0.021,0.499]	[-0.028,0.503]
log(Pop. density)	-0.048	-0.041	-0.041	-0.041
	[-0.076,-0.02]	[-0.069,-0.011]	[-0.07,-0.013]	[-0.07,-0.011]
2013	0.101	0.102	0.097	0.108
	[0.045,0.155]	[0.047,0.157]	[0.043,0.15]	[0.054,0.161]
2014	0.168	0.171	0.159	0.178
	[0.109,0.226]	[0.112,0.231]	[0.099,0.219]	[0.118,0.235]
Unique institutions	1018	1018	1018	1018
N	2497	2497	2497	2497

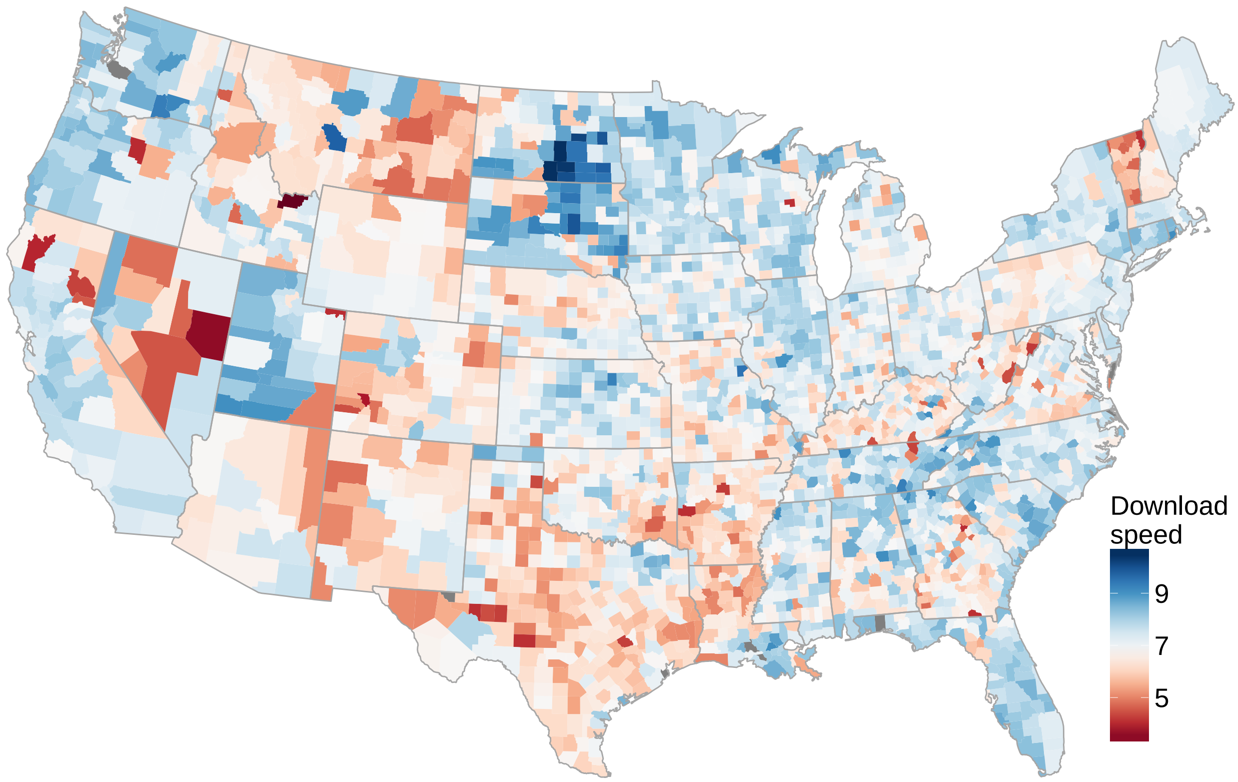
Notes. Bayesian point estimates represent posterior mean values. Values in the square brackets are 95% credible intervals. Intercepts (not reported) were allowed to vary at the state level. First level covariates not reported are indicators for USDA urban/rural community codes. Second level covariates include state unemployment rate, statewide average appropriations per FTE student, the proportion of public open admissions institutions in the state that are two-year institutions, and a population-weighted measure of the average distance to the nearest open admissions institution in the state. Parameter distributions in each model are the combination of four independent MCMC chains of 1000 draws each (with 1000 initial draws discarded as burn-in) for a total of 4000 total draws. All models were estimated using the Stan NUTS sampler with a normal likelihood sampling statement. The outcome measure in all models is the log number of students at each institution who enrolled in some distance education courses.

Table 2.7: Varying intercept Bayesian regressions of log number of students who enrolled in some distance education courses on broadband measures: two year institutions

	(1)	(2)	(3)	(4)
Download speed	-0.002			0.216
	[-0.514,0.508]			[-0.365,0.798]
Download speed^2	-0.004			-0.019
	[-0.039,0.031]			[-0.059,0.022]
Upload speed		-0.222		-0.201
		[-0.438,0.01]		[-0.463,0.059]
Upload speed^2		0.018		0.02
		[-0.005,0.04]		[-0.005,0.047]
# Providers			-0.036	-0.04
			[-0.158,0.086]	[-0.158,0.08]
# Providers^2			-0.002	-0.002
			[-0.016,0.013]	[-0.016,0.013]
log(Total enrollment)	1.133	1.134	1.136	1.138
	[1.091,1.174]	[1.093,1.174]	[1.096,1.176]	[1.099,1.178]
Prop. non-white	-0.859	-0.847	-0.838	-0.866
	[-1.024,-0.691]	[-1.016,-0.682]	[-0.997,-0.678]	[-1.027,-0.705]
Prop. women	-2.022	-2.043	-2.051	-2.053
	[-2.442,-1.603]	[-2.48,-1.618]	[-2.487,-1.623]	[-2.46,-1.628]
Prop. Pell grant	0.532	0.52	0.494	0.497
	[0.277,0.786]	[0.262,0.768]	[0.243,0.746]	[0.242,0.752]
Prop. part-time	-0.752	-0.747	-0.769	-0.774
	[-1.033,-0.468]	[-1.032,-0.467]	[-1.048,-0.492]	[-1.059,-0.501]
Prop. 25 years and older	0.098	0.096	0.129	0.134
	[-0.189,0.384]	[-0.19,0.373]	[-0.152,0.418]	[-0.151,0.422]
log(Pop. density)	-0.048	-0.04	-0.041	-0.039
	[-0.078,-0.017]	[-0.07,-0.009]	[-0.072,-0.009]	[-0.071,-0.009]
2013	0.097	0.094	0.088	0.106
	[0.04,0.152]	[0.04,0.15]	[0.033,0.143]	[0.049,0.162]
2014	0.17	0.17	0.155	0.183
	[0.109,0.233]	[0.11,0.23]	[0.093,0.214]	[0.123,0.244]
Unique institutions	899	899	899	899
N	2186	2186	2186	2186

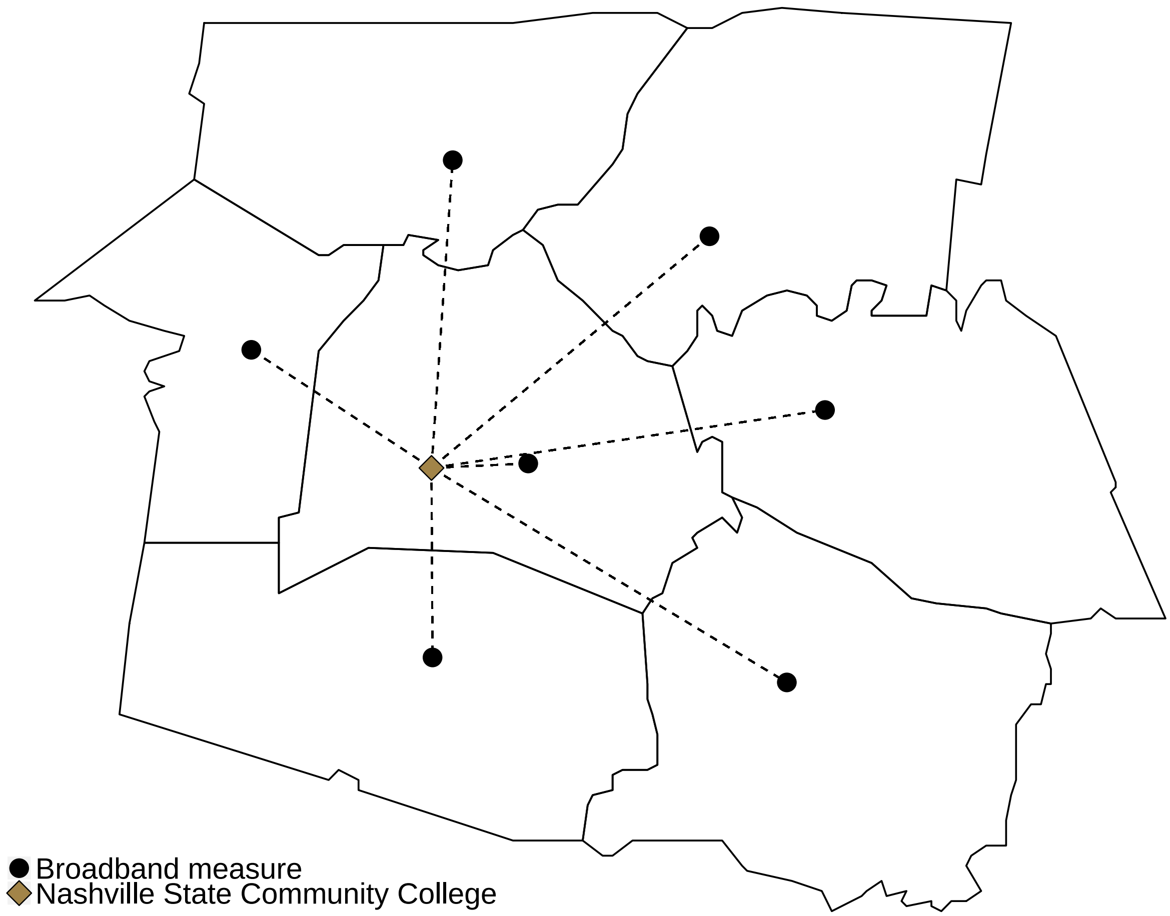
Notes. Bayesian point estimates represent posterior mean values. Values in the square brackets are 95% credible intervals. Intercepts (not reported) were allowed to vary at the state level. First level covariates not reported are indicators for USDA urban/rural community codes. Second level covariates include state unemployment rate, statewide average appropriations per FTE student, the proportion of public open admissions institutions in the state that are two-year institutions, and a population-weighted measure of the average distance to the nearest open admissions institution in the state. Parameter distributions in each model are the combination of four independent MCMC chains of 1000 draws each (with 1000 initial draws discarded as burn-in) for a total of 4000 total draws. All models were estimated using the Stan NUTS sampler with a normal likelihood sampling statement. The outcome measure in all models is the log number of students at each institution who enrolled in some distance education courses.

Figure 2.1: Average county-level broadband download speed, December 2013



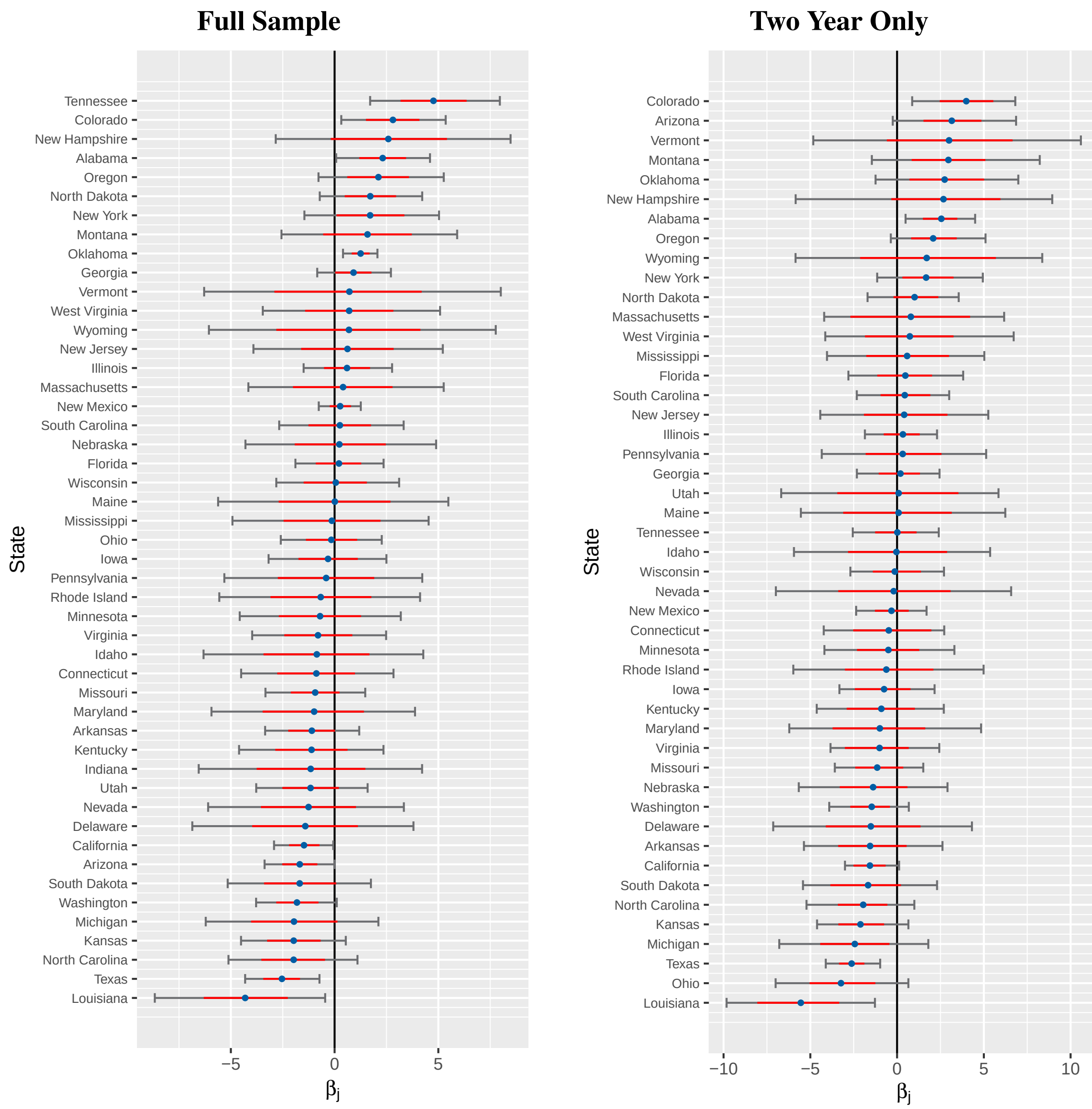
Raw broadband download speeds come from the National Broadband Map (NBM) and are reported at the census block level. For this map and the analysis, these values have been aggregated to the county level using census block group populations as weights. The NBM reports ordinal categories of speeds. Category 5 speeds (approximately 4 megabytes/sec, light red on the map), are what the FCC has historically recommended for watching lectures online. Recently, the FCC upgraded the definition of broadband to download speeds of at least 25 megabytes/sec, which would fall under category 8 (dark blue on the map).

Figure 2.2: Example weighting scheme for download speed at institution



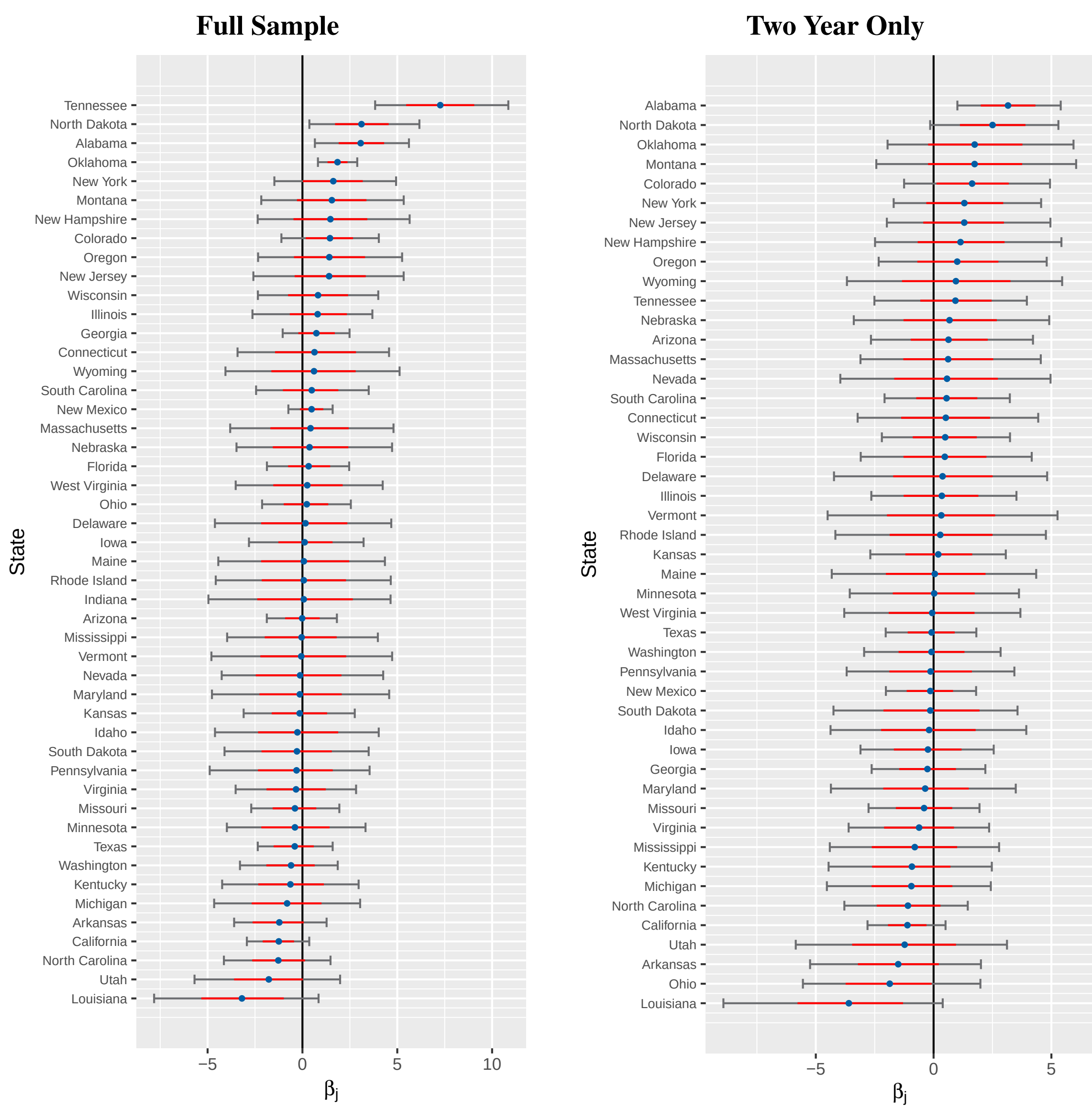
Each postsecondary institution is assigned a download speed that is a weighted version of those calculated for the surrounding areas. Raw broadband download speeds from the National Broadband Map, which are recorded at the census block level, are first aggregated to the census block group level. The distances between each institution and all census block group centers are then computed (represented by the dotted lines). These distances are used as inverse weights in a formula that assigns each institution the weighted average of its surrounding county values, with an adjustment for the population recorded for the census block group. In the example above, Nashville State Community College's broadband measures are most heavily influenced by those computed in Davidson County, center, but is also a function of those computed for surrounding block groups (including all those not shown).

Figure 2.3: State-level differences in the marginal effect of download speed on the number of students taking some online courses



The figure above shows state-level heterogeneity across β_j parameters, the marginal effect of download speed on the number of students who take some online courses. Dots represent the median β_j value. The red bars show the 68% credible intervals; the light gray bars show the 95% credible intervals.

Figure 2.4: State-level differences in the marginal effect of download speed on the number of students taking some online courses in fully specified model



The figure above shows state-level heterogeneity across β_j parameters, the marginal effect of download speed on the number of students who take some online courses. Upload speed and number of providers were also included in both models and allowed to vary at the state level. Dots represent the median β_j value. The red bars show the 68% credible intervals; the light gray bars show the 95% credible intervals.

2.A Alternative specifications

In an alternative specification, I fit equations using the proportion of students who took some online courses as the outcome. To model this outcome, I use a beta likelihood function with dispersion parameter that accurately accounts for the [0,1] bounds of the dependent variable (Gelman et al., 2014):

$$\frac{online}{total} \sim beta(a, b)$$
$$a = \mu \times \phi$$
$$b = (1 - \mu) \times \phi$$
$$\mu = \frac{exp(\mathbf{X}\beta)}{1 + exp(\mathbf{X}\beta)}$$

As with the primarily analysis models, I fit single level as well as hierarchical models in which the intercepts and parameters on broadband measures are allowed to vary at the state level. Results from these models show the marginal effect of broadband on the logged-odds of a percentage increase in the number of students who took some online courses. The results from these models are qualitatively the same as those reported in the main text of the chapter.

Table 2.A.1: Single level Bayesian beta regressions of percentage of students who enrolled in some distance education courses on broadband measures

	(1)	(2)	(3)	(4)
Download speed	0.205			0.224
	[-0.136,0.561]			[-0.182,0.642]
Download speed^2	-0.015			-0.012
	[-0.04,0.009]			[-0.041,0.016]
Upload speed		-0.077		-0.201
		[-0.257,0.106]		[-0.413,0.019]
Upload speed^2		0.002		0.012
		[-0.016,0.02]		[-0.01,0.033]
# Providers			0.004	0.041
			[-0.099,0.111]	[-0.073,0.154]
# Providers^2			-0.003	-0.005
			[-0.016,0.009]	[-0.018,0.008]
Two year institution	0.092	0.101	0.098	0.094
	[0.019,0.162]	[0.027,0.178]	[0.025,0.173]	[0.021,0.168]
log(Total enrollment)	0.149	0.15	0.151	0.151
	[0.112,0.184]	[0.115,0.187]	[0.115,0.187]	[0.114,0.19]
Prop. non-white	-0.609	-0.611	-0.617	-0.58
	[-0.73,-0.493]	[-0.724,-0.496]	[-0.735,-0.501]	[-0.697,-0.46]
Prop. women	-2.36	-2.308	-2.377	-2.313
	[-2.805,-1.925]	[-2.746,-1.872]	[-2.811,-1.958]	[-2.729,-1.896]
Prop. Pell grant	0.696	0.66	0.692	0.648
	[0.492,0.909]	[0.453,0.862]	[0.482,0.897]	[0.44,0.861]
Prop. part-time	-0.474	-0.476	-0.475	-0.47
	[-0.68,-0.267]	[-0.687,-0.261]	[-0.688,-0.257]	[-0.679,-0.26]
Prop. 25 years and older	0.438	0.469	0.462	0.481
	[0.2,0.673]	[0.225,0.714]	[0.219,0.702]	[0.232,0.728]
log(Pop. density)	-0.063	-0.054	-0.059	-0.056
	[-0.088,-0.037]	[-0.079,-0.029]	[-0.086,-0.034]	[-0.083,-0.029]
2013	0.112	0.124	0.113	0.116
	[0.055,0.17]	[0.068,0.181]	[0.057,0.17]	[0.06,0.174]
2014	0.151	0.173	0.149	0.164
	[0.089,0.214]	[0.11,0.235]	[0.091,0.209]	[0.1,0.228]
(Intercept)	-1.436	-1.437	-1.436	-1.437
	[-1.461,-1.41]	[-1.461,-1.412]	[-1.46,-1.412]	[-1.461,-1.413]
Unique institutions	1018	1018	1018	1018
N	2497	2497	2497	2497

Notes. Bayesian point estimates represent posterior mean values. Values in the square brackets are 95% credible intervals. Covariates not reported include indicators for USDA urban/rural community codes. Parameter distributions in each model are the combination of four independent MCMC chains of 1000 draws each (with 1000 initial draws discarded as burn-in) for a total of 4000 total draws. All models were estimated using the Stan NUTS sampler with a reparameterized beta likelihood sampling statement. The outcome measure in all models is the percentage of students at each institution who enrolled in some distance education courses.

Table 2.A.2: Single level Bayesian beta regressions of percentage of students who enrolled in some distance education courses on broadband measures: two year institutions

	(1)	(2)	(3)	(4)
Download speed	0.08			0.034
	[-0.461,0.606]			[-0.537,0.603]
Download speed^2	-0.008			0
	[-0.044,0.029]			[-0.04,0.04]
Upload speed		-0.113		-0.203
		[-0.329,0.104]		[-0.458,0.056]
Upload speed^2		0.004		0.012
		[-0.018,0.025]		[-0.014,0.037]
# Providers			0.05	0.088
			[-0.069,0.174]	[-0.039,0.216]
# Providers^2			-0.011	-0.013
			[-0.027,0.003]	[-0.029,0.002]
log(Total enrollment)	0.136	0.134	0.14	0.136
	[0.096,0.177]	[0.094,0.175]	[0.1,0.18]	[0.096,0.176]
Prop. non-white	-0.739	-0.727	-0.744	-0.718
	[-0.866,-0.612]	[-0.849,-0.606]	[-0.876,-0.612]	[-0.849,-0.588]
Prop. women	-2.451	-2.397	-2.474	-2.398
	[-2.921,-1.988]	[-2.853,-1.94]	[-2.917,-2.04]	[-2.852,-1.95]
Prop. Pell grant	0.752	0.708	0.755	0.704
	[0.529,0.982]	[0.481,0.937]	[0.531,0.982]	[0.474,0.93]
Prop. part-time	-0.364	-0.395	-0.363	-0.375
	[-0.611,-0.123]	[-0.631,-0.154]	[-0.606,-0.114]	[-0.616,-0.138]
Prop. 25 years and older	0.443	0.488	0.494	0.506
	[0.177,0.705]	[0.23,0.754]	[0.232,0.761]	[0.241,0.768]
log(Pop. density)	-0.052	-0.043	-0.052	-0.045
	[-0.079,-0.025]	[-0.07,-0.016]	[-0.08,-0.025]	[-0.072,-0.018]
2013	0.11	0.122	0.109	0.118
	[0.051,0.171]	[0.064,0.18]	[0.05,0.167]	[0.056,0.179]
2014	0.153	0.179	0.146	0.171
	[0.086,0.219]	[0.114,0.245]	[0.081,0.21]	[0.103,0.24]
(Intercept)	-1.441	-1.442	-1.442	-1.443
	[-1.468,-1.414]	[-1.467,-1.417]	[-1.468,-1.416]	[-1.468,-1.418]
Unique institutions	899	899	899	899
N	2186	2186	2186	2186

Notes. Bayesian point estimates represent posterior mean values. Values in the square brackets are 95% credible intervals. Covariates not reported include indicators for USDA urban/rural community codes. Parameter distributions in each model are the combination of four independent MCMC chains of 1000 draws each (with 1000 initial draws discarded as burn-in) for a total of 4000 total draws. All models were estimated using the Stan NUTS sampler with a reparameterized beta likelihood sampling statement. The outcome measure in all models is the percentage of students at each institution who enrolled in some distance education courses.

Table 2.A.3: Varying intercept Bayesian beta regressions of percentage of students who enrolled in some distance education courses on broadband measures

	(1)	(2)	(3)	(4)
Download speed	0.261			0.369
	[-0.069,0.605]			[0,0.759]
Download speed^2	-0.02			-0.027
	[-0.044,0.003]			[-0.054,-0.001]
Upload speed		-0.052		-0.123
		[-0.248,0.139]		[-0.346,0.105]
Upload speed^2		0.001		0.01
		[-0.018,0.02]		[-0.012,0.032]
# Providers			-0.05	-0.062
			[-0.157,0.054]	[-0.177,0.052]
# Providers^2			0.002	0.004
			[-0.01,0.015]	[-0.009,0.017]
Two year institution	0.09	0.1	0.104	0.097
	[0.007,0.173]	[0.015,0.186]	[0.019,0.188]	[0.013,0.183]
log(Total enrollment)	0.122	0.122	0.123	0.125
	[0.085,0.158]	[0.086,0.16]	[0.087,0.158]	[0.089,0.162]
Prop. non-white	-0.746	-0.735	-0.732	-0.74
	[-0.896,-0.595]	[-0.894,-0.58]	[-0.886,-0.579]	[-0.895,-0.585]
Prop. women	-2.291	-2.287	-2.304	-2.301
	[-2.74,-1.875]	[-2.718,-1.844]	[-2.726,-1.878]	[-2.719,-1.873]
Prop. Pell grant	0.472	0.455	0.443	0.456
	[0.225,0.718]	[0.214,0.702]	[0.205,0.692]	[0.216,0.699]
Prop. part-time	-0.922	-0.904	-0.924	-0.918
	[-1.166,-0.672]	[-1.152,-0.664]	[-1.166,-0.686]	[-1.157,-0.683]
Prop. 25 years and older	0.348	0.348	0.37	0.365
	[0.088,0.606]	[0.088,0.62]	[0.119,0.623]	[0.106,0.618]
log(Pop. density)	-0.041	-0.034	-0.035	-0.034
	[-0.071,-0.013]	[-0.064,-0.004]	[-0.065,-0.004]	[-0.064,-0.004]
2013	0.109	0.113	0.107	0.119
	[0.056,0.163]	[0.061,0.167]	[0.053,0.161]	[0.065,0.172]
2014	0.164	0.171	0.157	0.179
	[0.105,0.221]	[0.111,0.228]	[0.098,0.212]	[0.12,0.239]
Unique institutions	1018	1018	1018	1018
N	2497	2497	2497	2497

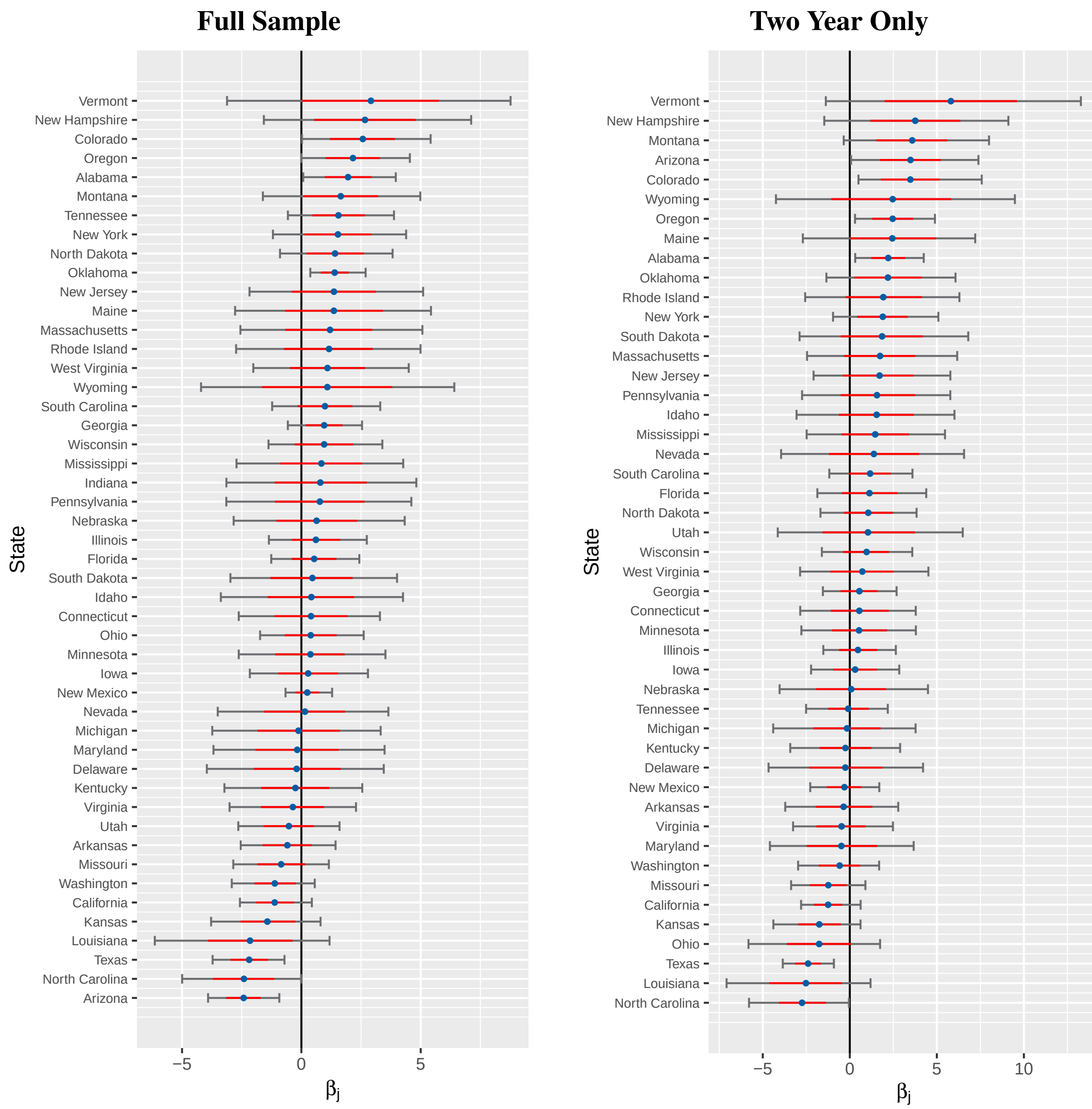
Notes. Bayesian point estimates represent posterior mean values. Values in the square brackets are 95% credible intervals. Intercepts (not reported) were allowed to vary at the state level. First level covariates not reported are indicators for USDA urban/rural community codes. Second level covariates include state unemployment rate, statewide average appropriations per FTE student, the proportion of public open admissions institutions in the state that are two-year institutions, and a population-weighted measure of the average distance to the nearest open admissions institution in the state. Parameter distributions in each model are the combination of four independent MCMC chains of 1000 draws each (with 1000 initial draws discarded as burn-in) for a total of 4000 total draws. All models were estimated using the Stan NUTS sampler with a reparameterized beta likelihood sampling statement. The outcome measure in all models is the percentage of students at each institution who enrolled in some distance education courses.

Table 2.A.4: Varying intercept Bayesian beta regressions of percentage of students who enrolled in some distance education courses on broadband measures: two year institutions

	(1)	(2)	(3)	(4)
Download speed	-0.06			-0.044
	[-0.565,0.449]			[-0.604,0.532]
Download speed^2	0.001			0
	[-0.034,0.036]			[-0.039,0.038]
Upload speed		-0.126		-0.086
		[-0.351,0.1]		[-0.337,0.17]
Upload speed^2		0.007		0.007
		[-0.016,0.029]		[-0.019,0.031]
# Providers			-0.021	-0.025
			[-0.144,0.098]	[-0.157,0.104]
# Providers^2			-0.005	-0.004
			[-0.019,0.01]	[-0.02,0.011]
log(Total enrollment)	0.132	0.134	0.135	0.135
	[0.09,0.173]	[0.093,0.173]	[0.094,0.175]	[0.095,0.174]
Prop. non-white	-0.811	-0.791	-0.786	-0.8
	[-0.964,-0.651]	[-0.956,-0.628]	[-0.955,-0.623]	[-0.961,-0.637]
Prop. women	-2.273	-2.286	-2.323	-2.285
	[-2.728,-1.827]	[-2.745,-1.808]	[-2.778,-1.866]	[-2.762,-1.833]
Prop. Pell grant	0.505	0.488	0.451	0.456
	[0.249,0.753]	[0.225,0.757]	[0.189,0.705]	[0.195,0.719]
Prop. part-time	-0.877	-0.87	-0.904	-0.897
	[-1.174,-0.593]	[-1.157,-0.575]	[-1.197,-0.619]	[-1.188,-0.607]
Prop. 25 years and older	0.178	0.183	0.225	0.223
	[-0.131,0.472]	[-0.105,0.474]	[-0.082,0.526]	[-0.068,0.525]
log(Pop. density)	-0.045	-0.038	-0.038	-0.035
	[-0.076,-0.014]	[-0.07,-0.007]	[-0.069,-0.007]	[-0.067,-0.004]
2013	0.105	0.106	0.1	0.119
	[0.047,0.161]	[0.051,0.161]	[0.043,0.155]	[0.064,0.176]
2014	0.16	0.168	0.149	0.18
	[0.097,0.223]	[0.105,0.232]	[0.09,0.209]	[0.116,0.244]
Unique institutions	899	899	899	899
N	2186	2186	2186	2186

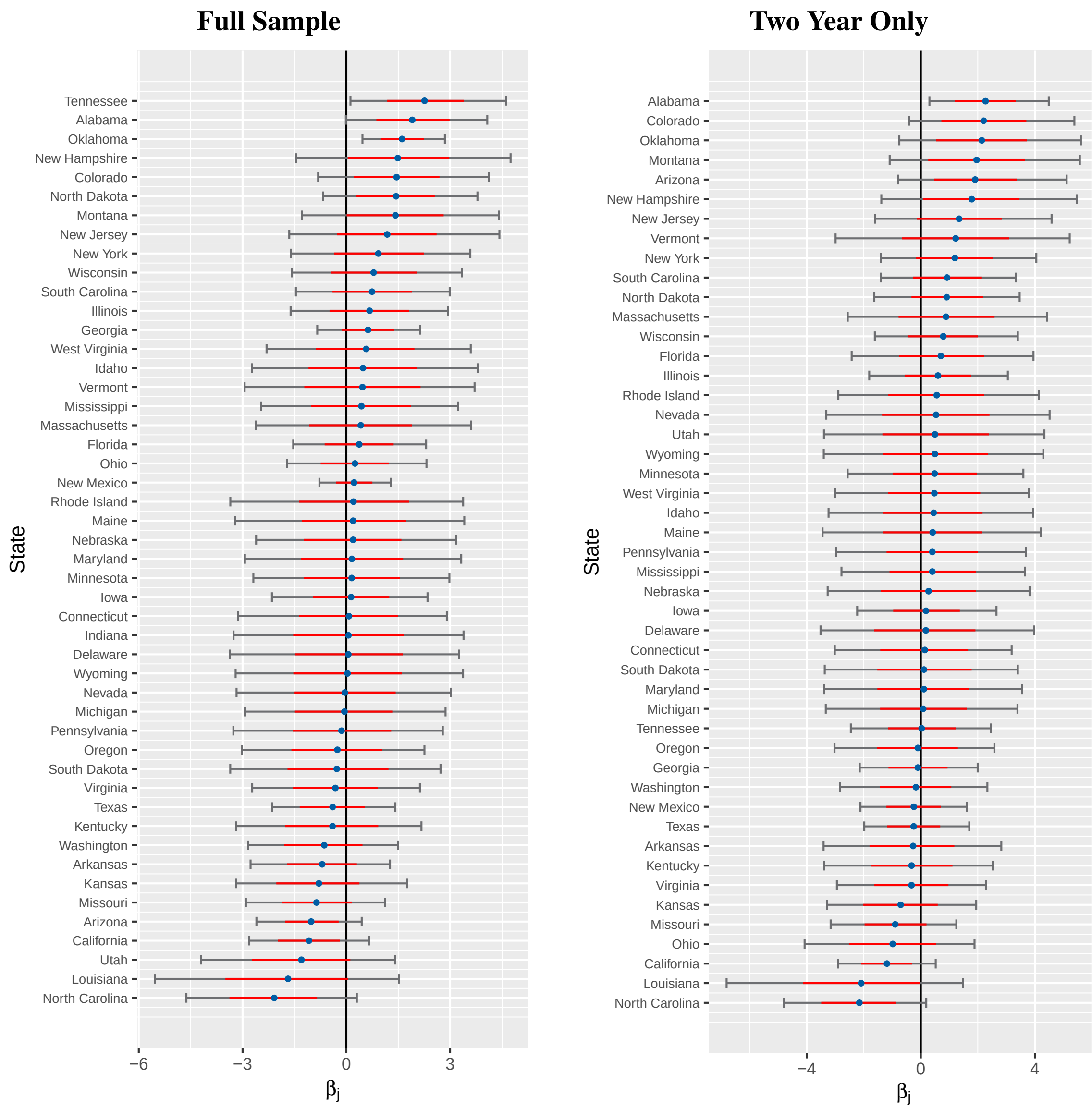
Notes. Bayesian point estimates represent posterior mean values. Values in the square brackets are 95% credible intervals. Intercepts (not reported) were allowed to vary at the state level. First level covariates not reported are indicators for USDA urban/rural community codes. Second level covariates include state unemployment rate, statewide average appropriations per FTE student, the proportion of public open admissions institutions in the state that are two-year institutions, and a population-weighted measure of the average distance to the nearest open admissions institution in the state. Parameter distributions in each model are the combination of four independent MCMC chains of 1000 draws each (with 1000 initial draws discarded as burn-in) for a total of 4000 total draws. All models were estimated using the Stan NUTS sampler with a reparameterized beta likelihood sampling statement. The outcome measure in all models is the percentage of students at each institution who enrolled in some distance education courses.

Figure 2.A.1: State-level differences in the marginal effect of download speed on the proportion of students taking some online courses



The figure above shows state-level heterogeneity across β_j parameters, the marginal effect of download speed on the proportion of students who take some online courses. Dots represent the median β_j value. The red bars show the 68% credible intervals; the light gray bars show the 95% credible intervals.

Figure 2.A.2: State-level differences in the marginal effect of download speed on the proportion of students taking some online courses in fully specified model



The figure above shows state-level heterogeneity across β_j parameters, the marginal effect of download speed on the proportion of students who take some online courses. Upload speed and number of providers were also included in both models and allowed to vary at the state level. Dots represent the median β_j value. The red bars show the 68% credible intervals; the light gray bars show the 95% credible intervals.

Chapter 3

Estimating the Effect of Online Courses on Student Persistence and Likelihood of Passing in Georgia Public Postsecondary Institutions

3.1 Introduction

In the fall of 2014, 28% of all college students took at least one online course, with 14% completing all coursework online (Snyder et al., 2016). Online courses hold great potential for democratizing postsecondary education (Bowen, 2013; Selingo, 2013), yet empirical findings have been mixed regarding student outcomes in virtual versus face-to-face settings. Whereas three randomized experiments (Bowen et al., 2012; Figlio et al., 2010; Joyce et al., 2014) showed little or no average difference in course or final exam grades, two quasi-experimental studies (Xu & Jaggars, 2011, 2013) found that online students were generally less likely to complete the course or earn a passing grade. With this chapter, I add to this literature by investigating two key student outcomes in online courses—course withdrawal and passing—using data from the University System of Georgia, a large public higher education system that encompasses a number of two- and four-year colleges and universities across the state.

Producing unbiased estimates of the effect of online delivery on student outcomes is a challenge because students generally self-select into course delivery type. Descriptive or even regression-adjusted estimates of the effect of online delivery on a student's likelihood of passing, for example, may instead reflect differences in the underlying student populations who choose one format over another. To mitigate this bias, I follow prior research (Xu & Jaggars, 2013) and employ an instrumental variables quasi-experimental design. Using a recursive bivariate probit model, I simultaneously estimate the course outcome and endogenous treatment as a pair of linked probit models in which the outcome of the first equation, online status, is placed on the right-hand side of the second equation (Greene, 2012; Hollenbach et al., 2016). Based on the results from chapter 2, which showed an association between broadband connectivity and online course enrollments, I use lagged student-level measures of download and upload speed on the right-hand side of the first equation to instrument the choice to take an online course. I argue that these measures are plausibly exogenous predictors of the treatment and thus allow me to produce less biased estimates of the effect of online course delivery on student persistence and success in a course. Due to the nature of my data and the difficulty of fitting bivariate probit models using maximum likelihood routines, I use a variational Bayes estimation strategy that produces full posterior distributions of regression coefficients with their respective degrees of uncertainty appropriately modeled by their standard deviations (Chib, 2003; Gelman et al., 2014; Hollenbach et al., 2016; Kucukelbir, Tran, Ranganath, Gelman, & Blei, 2016).

To preview the results, I find that students in online courses are an average of 2.8 percentage points more likely to withdraw than those in face-to-face courses. Based on the 95% credible interval of the posterior density, this average treatment effect could plausibly range from 0.6 to 5.3 percentage points. Conditional on completing the course, online students are 5.4 percentage points less likely to pass [-9.6 — -1.6]. At their averages, both of these effects reflect slight attenuation of the differences shown in the unadjusted (+3.8, -4.33) and regression-adjusted (+4.8, -7.6) comparisons, respectively. As a sensitivity check, I compute estimates for the subset of students in each term who take both online and face-to-face courses. Students in this sub group are similarly more likely to withdraw from online courses (+2.5 [0.6 — 4.8] percentage points). They also may be less likely to pass online courses should they complete them (-1.5 [-4.4 — 1] percentage points), but these latter findings lack precision and should be interpreted with caution. All together my results concur with those from prior quasi-experimental studies that find poorer average outcomes among students in online courses (Xu & Jaggars, 2011, 2013). Results from this study should give pause to policymakers who may wish to increase enrollments and attainment rates through an expansion of online-based course options without concomitant increases in supports for online learners and/or investigations into best practices to increase student success in online courses.

The rest of the chapter is structured as follows: after a review of the relevant literature in section 3.2 and conceptual framework in section 3.3, I discuss the Bayesian bivariate probit methodology and estimation strategy in section 3.4; describe both student-level and broadband data in section 3.5; present results in section 3.6, and conclude in section 3.7.

3.2 Literature review

A number of studies using a wide range of methodologies have evaluated the effect of online courses on student outcomes over the years. Many rely on non-experimental designs and therefore offer results that do not account for non-random sorting of students into online courses (Means et al., 2010). A few have utilized experimental and quasi-experimental designs that produce results with greater internal and external validity. Results from this latter group of studies, however, do not agree on the effect that online course delivery has on student outcomes. While three sets of researchers that used true experimental designs in their studies generally agree that students in online courses perform better or at least no worse than students in face-to-face courses (Bowen, Chingos, Lack, & Nygren, 2014; Figlio et al., 2010; Joyce et al., 2014), two quasi-experimental design studies by a pair of researchers find that online students are more likely to drop out of the course and, conditional on completion, less likely to earn a passing grade (Xu & Jaggars, 2011, 2013). After a short discussion of the non-experimental studies, I discuss each of these experimental and quasi-experimental studies more thoroughly in the next section.

3.2.1 Non-experimental evidence

In the decade surrounding the new millennium, scholars completed hundreds of studies that empirically evaluated online learning on a variety of student outcomes. In order to make policy recommendations for K-12 school districts interested in incorporating online education in their curricula, the U.S. Department of Education facilitated a meta-analysis of this vast literature in 2010. Yet from a starting pool of over a thousand potential studies published between 1996 and 2008, Means et al. (2010) found only 176 studies that met their criteria of having an experimental or quasi-experimental design and of these, only 45 studies provided enough information that 50 effect sizes could be calculated. Despite its status as a K-12 focused policy document, most of the analyzed studies concerned undergraduate and graduate student populations.

The authors report that, on average, students in online courses performed better than their peers in face-to-face settings, with "an average effect size of +0.20 favoring online conditions," (Means et al., 2010, p. xiv), a result that is statistically significant. They also found that across studies, students in hybrid learning environments performed even better, with a statistically significant positive average effect size of +0.35. The meta-analysis showed that these effects were generally robust across types of online learning and student populations. The authors acknowledge, however, that though the studies included in the meta-analysis had stronger designs than those not included, many did not account for differences between online and face-to-face courses that were due to factors other than delivery medium. In some cases, pedagogies and curriculum materials could have differed. It also appeared that many of the studies, while incorporating comparison groups, did not account for differential sorting into treatment or control.

Sorting is a problem if students who self-select into online courses are different from those who do not.

If, for example, online students are more academically prepared or self-motivated, results that show positive effects for online courses could reflect these differences rather than the true effect of the delivery medium. Even null findings that show no difference in final GPA or end-of-course grade between online and face-to-face students could occur if students persist at different rates in the two course settings. If academically weaker students are more likely to withdraw from online course sections than those taught face-to-face, then the end-of-term grade point averages and average pass rates for online students will be positively biased. If the bias is large enough, unadjusted averages or results from simple regressions fit to cross-sectional data are likely to suggest no difference in outcomes between online and face-to-face students where instead there would be a significant negative effect for online courses if all students were required to complete their courses (Xu & Jaggars, 2011). Because the studies included in the meta-analysis did not account for sorting, the positive effects reported by Means et al. (2010) may in fact represent underlying differences in the populations or differential attrition and not the unbiased effect of online courses on student outcomes.

3.2.2 Experimental evidence

A few recent studies have improved upon prior literature by employing rigorous experimental designs that randomized students into either online or face-to-face courses, thereby eliminating selection bias and improving the internal validity of the estimated effect. In the first study, Figlio et al. (2010) randomized students in large introductory microeconomics course into either a live lecture course or an online version in which students watched the lectures. Because the course had already incorporated a number of online elements through a course web page, the primary difference between the two groups was the lecture-watching requirement—all other materials were the same. Contrary to the evidence presented by the U.S. Department of Education's meta-analysis, the authors found no statistically significant differences between the two groups in terms of exam grades. They did find significant differences between groups, with male students, low-achievers, and Hispanic students performing better in live courses. Of these, however, only the effect on Hispanic students was practically significant with an 11 point difference in favor of face-to-face sections.

In another study that compared multiple sections of the same course (also microeconomics) at a large urban university, Joyce et al. (2014) found significant differences between those who were randomly assigned to a hybrid course section and those assigned to the traditional format section. The authors report that students in hybrid sections scored 2.5 points lower than their traditional course peers on examinations.

While this result suggests poorer performance for students who completed more work online, the authors note that this difference is the same as the difference in one question on a 40 point test—that is, not practically significant. With a participation rate among eligible students of 96% and attrition of only 10%, the results have high internal validity. The authors argue that the low cost of the hybrid format and ease of implementation more than makes up for its small negative effects on the end of course grade.

A final experimental study by Bowen et al. (2014) expanded the scope of the experiment to include over 600 students across six institutions, this time comparing students in an introductory statistics course that offered both traditional instruction and hybrid sections. Their results showed no significant differences between the groups in terms of test scores, though the authors did find marginally significant differences between groups on ancillary outcomes: ratings of course satisfaction and difficulty. On a 0-4 scale, hybrid students rated the course 0.3 lower (2.0 vs 2.3) and 0.2 more difficult (2.5 vs 2.3). The authors faced challenges in their study design, mostly due to its multi-site nature and their inability to offer both types of course sections at each school simultaneously. With access to administrative records, the authors were able to compare those in the courses who volunteered for the study to non-volunteers. Between these two groups the authors found many statistically significant differences. This, among other potential issues, weakened the internal validity of their findings. Despite these limitations, however, the authors argue that hybrid courses may represent a way to control costs and thus fight Baumol's cost disease (Baumol, 1993).

Together, the results of these experimental studies showed no practically significant average differences between students who took online or hybrid course sections and those who took traditional format sections. Due to randomization into treatment, each of these studies has high internal validity. While heterogeneous effects were found across student populations, none of the authors argue for reductions in the number of online or hybrid courses. As is the case with most experimental designs, however, these studies gain internal validity at the expense of external validity. Each considers a sample of volunteers in a single introductory economics or statistic course at large, unnamed public institutions. With the exception of Bowen et al. (2014), the studies were conducted in a single school. For all of these reasons, it is unclear how the results relate to a larger population of students or those who attend other types of institutions.

3.2.3 Quasi-experimental evidence

Two recent papers by Xu and Jaggars (2011, 2013) provide quasi-experimental estimates for the effect of online courses on student outcomes. In both studies, Xu and Jaggars found that students who took online courses in the Virginia and Washington State community college systems were more likely to withdraw from the class and, conditional on completion, likely to earn a lower grade than their peers who enrolled in traditional classroom settings. I discuss each of these papers in more detail below.

In the earlier paper, Xu and Jaggars (2011) utilized a propensity score model to estimate the effect of online courses on both within-course persistence and end-of-course grade among community college students in Virginia. Their sample included students enrolled in either an introductory English or math course at one of Virginia's 23 community colleges during the 2004-2005 through 2007-2008 academic years. This gave them much larger sample sizes—$N = 13,951$ for English students and $N = 8,328$ for math students—than those in the aforementioned randomized experiments.

Xu and Jaggars first note that students who enrolled in online sections (8% for English, 7% for math) were observationally different from students who did not. Older students, women, students receiving federal financial aid, and those who took a computer literacy course were more likely to enroll in an online course section; black, Hispanic, and Asian students, as well as transfer-track and ESL students were less likely. Because of these differences, the authors note that they could not simply compare outcomes between online and traditional-format students. To account for the bias between the groups, Xu and Jaggars employed a propensity score model that matched treatment observations (those who took online courses) with control group observations (traditional-format students) based on their likelihood of being in an online course as summarized by their propensity score. In one specification, the authors allowed students to be matched across institutions; in another specification, students were only matched with others at the same school. The authors report that in each matching scheme, only a very small number of treatment individuals were dropped due to lack of match (< 20) and that they achieved balance on score-adjusted covariates.

Xu and Jaggars considered two outcomes: mid-semester dropout and success in the course, meaning earning a C or better. As a key part of their argument, the authors discuss how differences in attrition between online and face-to-face students may bias estimates that only consider end-of-course outcomes. If online students who completed the course represent stronger than average online students, then estimates that showed no difference between online and traditional-format students or better outcomes for online students are likely the result of unobserved differences between the groups, not the format of the course. For this reason, the authors argue, course persistence must also be considered in addition to end-of-course outcomes. Because each of their outcomes is binary, the authors fit multilevel logistic regression models that took into account student and institution-level covariates and allowed random intercepts and slope coefficients across institutions. For each outcome, four matching procedures were conducted—two for each content area by two for across- vs within-school matching—which gave a total of eight estimate procedures.

The authors found overall attrition rates of 11% and 13% for English and math, respectively, with a 9% gap between online and face-to-face takers in English (19% vs 10%) and a 13% gap in math (25% vs. 12%). They report that the odds of an online course-taker dropping the course were around double that of those who took a face-to-face section. Results for end-of-course success (measured as a C or better) conditional on completion were qualitatively similar, with the authors reporting that online students faced 33% to 41% lower odds of success than their face-to-face peers in both classes across all sample comparisons.

In a second paper, Xu and Jaggars (2013) present similar findings for students who attended Washington State's community and technical colleges between the fall of 2004 and summer of 2009. For this study, the authors used an instrumental variables (IV) approach to produce their estimates. Arguing that students who lived farther away from their institution should be more likely to take online courses, the authors employed a two-stage model in which distance between the student's home and institution was used to predict the student's likelihood of taking an online course in a first-stage probit equation. Fitted values from this equation were then used in place of actual 0/1 enrollment values in a second-stage probit equation that estimated the effect of online courses on (1) course persistence and (2) end-of-course grade. To mitigate against differences between schools and subjects biasing the estimates, the authors used college-by-course fixed effects along with other student, term, and school covariates.

Xu and Jaggars report statistically significant negative marginal effects for online courses on completion that range from -0.036 to -0.044. These results suggest that the persistence rates of online course-takers were around four percentage points lower than those for face-to-face students. Conditional on finishing the course, student grades for online course-takers were -0.196 to -0.266 points lower than those of their counterparts. IV models produced results that were in the same direction but stronger. In terms of course persistence, the effect of taking an online course was -0.054 to -0.065; for grades, -0.228 to -0.324.

That the IV results were consistently stronger is taken by the authors to "[support] the notion that online courses are more popular among more motivated and academically better prepared students," (Xu & Jaggars, 2013, p. 54). In the scenario that students become more likely to enroll in online courses as their baseline ability and motivation increase yet are expected to perform worse than they would in traditional courses, raw and OLS-adjusted average differences between online and face-to-face students will be biased in favor of online students. Because a valid IV approach removes the endogeneity of self-selection, the second-stage coefficient on enrollment in an online course should reflect the difference attributable to the delivery method independent of other differences between the groups. Thus, if the above scenario is true, one would expect IV coefficients to remain negative but become larger in terms of absolute value than similar OLS coefficients. As this is what Xu and Jaggars found, the authors argue that these results support beliefs that online courses attract students who are likely to have better outcomes, and, by implication, that previous claims in favor of online coursework neglect to account for this bias.

Xu and Jaggars acknowledge that despite their negative findings, online courses are likely to continue being an option or requirement for many students. They argue, therefore, that more work is required to analyze how to improve online course design and faculty and student supports in order to improve outcomes. In a related study, Xu and Jaggars (2014) utilized the same data set to investigate differential outcomes across student populations and course types. With the same fixed effects model used in the baseline specification of their 2013 paper, the authors found that "males, Black students, and students with lower levels of academic preparation had significantly stronger online performance gaps compared with their counterparts," (Xu & Jaggars, 2014, p. 651). A surprising result was that performance gaps for older students were smaller than those of their younger peers. The authors suggest this may be the outcome of a rational choice among older students to self-select into online sections if it allowed them more flexibility to meet work and household demands.

The authors also found evidence that increases in the proportion of less-prepared or at-risk peers more negatively influenced other students in online sections. They posit that this may be the result of more difficulty in group assignments or strong instructor differentiation that focused on struggling students to the detriment of other students. Looking across subject areas, their results indicated that online students in social science and applied professions courses had stronger drops in persistence and grade than students enrolled in other subject areas. Xu and Jaggars suggest this finding may be the result of different "hands-on" (p. 652) or pedagogical practices found in the respective areas. Though the empirical analyses of this study were not causal, they may be understood in the context of the authors' earlier papers as providing robust descriptive evidence that even though all students perform less well in online courses than face-to-face courses, the difference in performance is heterogeneous across students, course assignments, and subject areas.

On the whole, the best evidence of the effect of online course delivery on postsecondary student outcomes is mixed. While randomized control trials, the "gold standard" of educational research, show no or practically insignificant average differences between online and face-to-face students, quasi-experimental studies show that online students are generally less likely to succeed. I situate the work of the current chapter in this ongoing debate. To produce less biased estimates, I follow Xu and Jaggars (2013) in using an instrumental variables approach, but with a new set of instruments and population of students. Though my approach may lack the internal validity of a true RCT, I argue that in addition to supporting external validity, the observational data I use provide a better frame for considering how online courses fit into a student's enrollment decision process.

3.3 Conceptual framework

To conceptualize a student's decision of whether to enroll in an online course, I rely on the human capital theoretical framework (Becker, 2009; Kane, 1999; Manski & Wise, 1983; Paulsen, 2001; Perna, 2006; Toutkoushian & Paulsen, 2016; Turner, 2004). Following Turner's notation, the college enrollment decision is modeled with the following equation:

$$\max_{s,j} \sum_{t=s+1}^{T} \frac{Y_{sji}}{(1+r)^t} - \sum_{t=1}^{s} \frac{F_j}{(1+r)^t} - \sum_{t=1}^{s} \frac{U_{ij}}{(1+r)^t} - \sum_{t=1}^{T} \frac{Y_{0i}}{(1+r)^t}, \tag{3.1}$$

where

- Y := utility
- F := direct college costs
- U := uncertainty
- i := individual
- j := college/college program
- s := length of enrollment
- t := time period

- r := market rate for borrowing and lending

In this utility maximization problem that is modeled akin to a financial investment decision, increases in utility that come from additional education at the postsecondary level are the total of lifetime utility gained from choosing a particular college or program of study, Y_{sji}, less the direct costs associated with the schooling choice, F_j, individualized costs associated with uncertainty in the schooling choice, U_{ij}, and the utility realized without additional education, Y_{0i}, with each term adjusted by the appropriate market discount rate, $(1+r)^t$. In the context of this model, the goal for each individual, i, is to select a college/program of study, j, and length of schooling, s, that will maximum her utility (Turner, 2004). The choice for a student, therefore, is comprised of two interdependent parts: which school/program and for how long to enroll.

The costs of a schooling choice, F_j, are multifaceted. They include direct college costs such as tuition, books, fees, room and board, *etc*. They also include forgone wages that otherwise might be earned during the time spent in school. Because the returns to extra schooling compound over time, it makes more sense to attend college early and consistently until finished (Jepsen & Montgomery, 2012; Turner, 2004). Finally, costs do not have to be financial but can include other factors such as consumption costs. If a student enjoys college, the consumption cost of attendance is lower and F_j is reduced. Conversely, if the student does not like college, then consumption cost is added to F_j.

A few constraints, however, complicate the basic assumptions of the human capital model. First, the "basic human capital model assumes that individuals are able to borrow at a market rate (r) in order to finance college," (Turner, 2004, p. 29). If individuals face credit constraints or have strong aversion to debt, then they may under-invest in college on the whole or attend only sporadically so that they can earn money along the way. Second, an individual's cognitive and non-cognitive skills affect the costs of college in terms of the demands that college-level work places on the student—under-prepared students may find these costs too high. Either of these costs could account for increased enrollment time or shift the cost curve to the extent that the choice to enroll becomes less appealing for some students (Toutkoushian & Paulsen, 2016; Turner, 2004).

Other constraints are structural. The first of these are the supply-side constraints of higher education. Though most models of student choice are predicated on the assumption that all individuals share the same population set of higher education institutions, the reality is that individuals face heterogeneous choice subsets based on where they live, their socioeconomic status, and their hitherto academic achievement and ability (Hillman, 2016; Niu & Tienda, 2007). With such constraints, individuals may not be able to maximize equation (3.1), or, otherwise stated, may only be able to maximize it conditional on a restricted set of options.

Informational asymmetry and uncertainty comprise the second set of structural constraints. Individuals who enroll in college often do so without a clear understanding of either the demands or the ultimate return (Becker, 2009; Winston, 1999). An overly simple version of the human capital model assumes the opposite: perfect information across all parties. Systemic inequality across different populations means that some individuals likely have a better idea of college costs and rewards than others. Individuals from wealthier households, who are more likely to have parents or close family members who have attended college, have reserves of social and cultural capital to draw upon as they navigate admissions processes and the college experience (Bourdieu, 1977; Coleman, 1988). Individuals without these supports, who must rely on their own comparatively poorer information, may "make systematic mistakes by enrolling or persisting in college when it is perfectly predictable, given available information, that the costs of college completion will outweigh the benefits," (Turner, 2004, p. 31). Similarly, it may be that costs are only too high under certain scenarios (*e.g.,* attending a for-profit institution when a similar program at a community college is less expensive), but the lack of information prevents an accurate accounting of all options or a myopic view of what options are available (Manski, 1993).

Thus, the costs associated with uncertainty must be included in the model and understood to be heterogeneous across populations. For individuals with a wealth of social and cultural capital, these costs may be very low; for students without such benefits, the costs may be high. Though costs attributable to uncertainty could be subsumed in the F_j term of the equation, I explicitly model them with the U_{ij} term. I include the i subscript to indicate that uncertainty is individualized. The same college or program may induce different levels of uncertainty across student populations and may even be idiosyncratic at the individual level. Including the U_{ij} term in the model separates uncertainty costs from known college costs, be they financial or based on personal taste preferences. For a cost to be uncertain, a student has to know that he does not have complete information, that the information may exist but is not accessible. By construction, increased uncertainty detracts from the potential payoff of an enrollment choice. As a student's uncertainty about his options and potential outcomes is reduced, U_{ij} moves toward zero and only known college costs, F_j, and non-college utility, Y_{0i}, are considered when making an enrollment decision.

3.3.1 Online courses and human capital decisions

The exact role that online courses play in the human capital decision model is complex. On one hand, online courses could reduce costs, F_j, or increase utility, Y_{sji}. They could do so in a number of ways. They may lower the direct cost of attendance if a student does not have to spend as much time and money traveling to the institution (Bowen, 2013). Because most are asynchronous in structure, online courses may allow students more flexibility in how they spend their time, meaning that online students may be better able to accommodate less flexible work schedules or family obligations (Jaggars, 2014). Online courses may also increase a student's choice set of institutions and programs, mitigating the negative effects of living in an geographically isolated "college desert" (Hillman, 2016). Students may enroll in entirely online programs (Allen et al., 2016), or find that their local institutions, even though they lack the capacity to support a full range of disciplines or the funds to access all available research materials, selectively utilize online material produced by other institutions to offer a wider array of course choices (Baldwin & Chronister, 2001; Fisher, 2006; Paulson, 2002). With more schools, courses, and fields from which to choose, students may find that online courses increase the probability of their having access to the high quality courses that they wish to take and, by extension, their likelihood of success.

On the other hand, online courses may increase costs or reduce utility. Costs for online courses may be higher if students find that they must invest in a better computer, broadband access, and/or technological skills in order to complete the courses (Moore & Anderson, 2003). They may also find that online classes in fact cost more than face-to-face courses. Though one key promise of online courses lies in their ability to reduce per student cost at the margins (Deming et al., 2015; Johnstone, 2006; Kinser, 1999; Selingo, 2013), a recent survey of administrators found that online students at many institutions pay the same or even more for online courses when fees are taken into account (Poulin & Straut, 2017). Online courses may reduce the utility to be gained from college if students do not have a taste for the structure of online courses but must take them anyway. Students who desire the instantaneous feedback of face-to-face courses, have difficulty self-regulating, or trouble learning material with minimal help may not gain as much from online courses (Jaggars, 2014; Moore & Anderson, 2003; Xu & Jaggars, 2013). Because the startup costs of producing a new distance education course can be high (R. Jones, 2003; Poulin & Straut, 2017), an institution is incentivized to use courses it has rather than produce new ones so that it can realize their marginal benefits. There may be further incentive for schools to produce courses in which assignments are amenable to automated grading, meaning that multiple choice quizzes would be preferred over essays. In this scenario, online courses could compress curricular options for students—rather than increase choices—and therefore reduce their overall utility.

Online courses may also reduce the utility of the college enrollment decision by increasing the individual's uncertainty, U_{ij}. Due to the inherent nature of education, students cannot be sure what they are purchasing until they have purchased it (Becker, 2009; Winston, 1999). Online courses reflect a subset of this condition with their own idiosyncratic concerns. Students may consider the costs outlined above, but not have a good sense of how these costs will affect them. Understanding, for example, that online courses may require more self-teaching of class material than face-to-face courses is not necessarily the same as knowing whether *I* will be able to do so with *this* material. In addition, students may also be unsure how future institutions, should they wish to transfer, or future employers will view such courses since many conflate online education with for-profit institutions and their poor reputations (Cottom, 2017).

It could also be true that students do not know whether they have the technological capacity to successfully complete online courses. Specifically, they may not know whether they have access to Internet connections with download and upload speeds that meet the federal definition of broadband (Federal Communications Commission, 2015a, 2016). This uncertainty could lead to increases in U_{ij} and lower the utility of choosing online courses. In chapter 2, however, I find a positive association between area average download speeds and the number of an institution's students who take some of their courses online. These results offer evidence that students respond to broadband access when deciding whether to take online courses, meaning that broadband access may not reflect an unknown component but rather one that students can rationally weigh when making their enrollment decisions. If broadband access represents a known, F_j, rather than unknown, U_{ij}, cost, I argue that higher speeds (particularly download speeds) should be associated with increases in the probability of a student enrolling in an online course. In this chapter, I exploit this connection along with variation in broadband access across Georgia in order to produce less biased estimates of the effect that online delivery has on a student's persistence and success in the course.

3.4 Methodology

3.4.1 Model

To estimate the effect of online course delivery on persistence and course outcome, I utilize a Bayesian instrumental variables estimation strategy (Chib, 2003; Hollenbach et al., 2016) that uses variation in broadband connectivity to predict online enrollment. Variation in Internet access has been successfully used in other quasi-experimental studies as an exogenous predictor of online usage (Bauernschuster, Falck, & Woessmann, 2014; Bhuller, Havnes, Leuven, & Mogstad, 2013). Because both the treatment—online course delivery—and the outcome—withdraw/pass—are binary limited dependent variables, the likelihood function takes the form of a recursive bivariate probit in which the outcome from the first equation is included on the right-hand side of the second equation (Greene, 2012). Unlike a two-stage least squares (2SLS) instrumental variables model, which ignores the limited nature of the outcomes and assumes normality, the bivariate probit model assumes that the discrete treatment indicator and outcome for each individual, i, are functions of normally distributed latent parameters,

$$Y_i = \begin{cases} 1 & \text{if } Y_i^* > 0, \\ 0 & \text{otherwise} \end{cases} \tag{3.2}$$

$$T_i = \begin{cases} 1 & \text{if } T_i^* > 0, \\ 0 & \text{otherwise} \end{cases} \tag{3.3}$$

$$Y_i^* = \mathbf{X}_i\gamma_2 + T_i\beta + \varepsilon_{2i} \tag{3.4}$$

$$T_i^* = \mathbf{X}_i\gamma_1 + \mathbf{Z}_i\psi + \varepsilon_{1i}, \tag{3.5}$$

where Y_i is the outcome of interest, T_i is the endogenous treatment, $\mathbf{Z}_i$ are the exogenous excluded instruments, $\mathbf{X}_i$ are the other covariates included in the model, γ_1, γ_2, and ψ are unknown parameters, and β is the coefficient of interest. Errors are assumed to be distributed bivariate standard normal,

$$\begin{pmatrix} \varepsilon_{2i} \\ \varepsilon_{1i} \end{pmatrix} \Big| \mathbf{X} \sim \mathscr{N}\left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} 1 & \rho \\ \rho & 1 \end{bmatrix} \right), \tag{3.6}$$

with ρ measuring the correlation between the errors of the two equations.

Like 2SLS and limited information maximum likelihood (LIML) instrumental variables models, the recursive bivariate probit model can be estimated through a maximum likelihood routine (Greene, 2012). Compared to each of these frequentist methods, however, a bivariate probit estimated using a Bayesian framework has two properties that make its use in the study particularly warranted: (1) its robustness to weak instruments and (2) the ease with which various treatment effects may be computed.

Weak instruments may severely bias 2SLS estimates in finite samples (Angrist & Pischke, 2008; Stock & Yogo, 2005). While the LIML estimator has been recommended when instruments are weak, it too may perform poorly in some samples (Greene, 2012). In this study, I use student-level measures of broadband connectivity as exogenous instruments. In chapter 2 I find a positive association between average download speeds and the number of students at open admissions colleges and universities who attempted some distance coursework. Among students in the USG sample, I similarly find positive marginal correlations between download and uploads speeds and the likelihood of taking an online course (see Table 3.4, discussed in more detail below). To mitigate potential threats to exogeneity caused by a direct connection between broadband access and course outcomes in the same semester, I use student-level measures of broadband connectivity lagged by one term to instrument enrollment in an online course section.

Hollenbach et al. (2016) argue that the Bayesian framework may be better suited to the estimation of instrumental variable models with weak instruments than a frequentist framework. Through simulations and estimations using real data from randomized control trials, the authors show that bivariate probit models estimated with Bayesian methods were more likely to recover either true or more plausible treatment effects than similar models estimated using maximum likelihood algorithms or other IV estimators. In addition, Chib (2003) shows through simulations how "posterior distributions [of regression parameters] provide accurate inferences about both the strength of the instrument and the degree of confounding [ρ]," (p. 73), meaning that lack of information due to weak instrumentation is represented by increases in the standard deviation of β's posterior distribution.

A second benefit of a Bayesian bivariate probit model over 2SLS is the ability to recover a wider range of treatment effects. With 2SLS the estimated effect, the local average treatment effect (LATE)[1], applies to those induced by the instrument into treatment (Angrist & Pischke, 2008; Gelman & Hill, 2007). A LATE estimate, therefore, is sensitive to the choice of instrument since different instruments may induce different populations into treatment. With the bivariate probit model, average treatment effects (ATE) and the effects of the treatment on the treated (TOT) can be estimated in addition to LATE estimates (Angrist & Pischke, 2008; Greene, 2012; Hollenbach et al., 2016). While these treatment effects can be recovered from bivariate probit models fit using maximum likelihood estimation, they are straightforward to compute using results from MCMC algorithms that approximate the posterior. In the Bayesian framework, treatment effects also have distributions that, in their central tendencies and spread, are easily interpreted (Chib, 2003; Hollenbach et al., 2016) and are more amenable to policy prescriptions since they suggest a range of probable effects (Gill & Witko, 2013).[2]

3.4.2 Identification

Identification when using a Bayesian instrumental variables model, as with frequentist instrumental variables models, relies on the assumption that the excluded instruments affect the second-stage outcome only through the endogenous independent variable. In this study, I argue that measures of broadband download and upload speed affect student course completion and likelihood of passing through their decision to enroll in online courses. This means two things: (1) that broadband measures are correlated with the choice to enroll in an online course and (2) that these measures are uncorrelated with the student course outcomes independent of the enrollment decision. The first assumption concerns the relevance or strength of the instruments, which I discussed in the prior section. I discuss the second assumption in more detail below.

Instrumental variables estimates may retain bias if there exists another channel through which the pro-

[1] In the case of multiple instruments, this effect is the complier average causal effects (CACE) (Angrist & Pischke, 2008).

[2] The key issue for any estimate, regardless of mode of inference, is to provide a range of uncertainty around the estimate. Frequentist approaches regard uncertainty as being related to sampling plus unexplained variance. Bayesian approaches regard uncertainty as being related to the degree to which the likelihood and the prior serve to constrain the posterior distribution. Yet another approach is to generate non-parametric treatment bounds (Imbens & Manski, 2004; Imbens & Wooldridge, 2009; Manski, 1990, 2003), which view the primary source of uncertainty as a function of the modeling choices made by the researcher. For example, uncertainty about an estimate may be reduced by knowing certain features of the social situation, such as that only certain individuals can receive treatment or that the treatment can only work in one direction. With this knowledge in mind and assuming both the worst and best potential outcomes for unobservable treatment conditions, a range of potential treatment effects can be computed along with their accompanying errors. Under some conditions, these treatment bounds may be similar to frequentist confidence intervals and/or Bayesian credible intervals for treatment effects. They remain distinct from both frequentist and Bayesian estimates, however, due to their underlying non-parametric paradigm.

posed exogenous instruments affect the second-stage outcome independent of the treatment (Angrist & Pischke, 2008). One threat to instrument exogeneity may be that broadband speeds are directly associated with course outcomes in both online and face-to-face courses. Internet usage in the United States is nearly ubiquitous[3], and college students in modern day face-to-face courses use the Internet to complete many assignments. Faster download speeds may improve outcomes if students are able to more quickly access research materials and/or class portals such as Blackboard, regardless of course delivery method. Assuming better connected students are more likely to take online courses and no treatment effect for online delivery, then differences in outcomes attributed to online course delivery instead may be due to improved technological infrastructure. Should online courses have negative effects on student outcomes, however, then improved Internet access among online students could cover the true negative effect. To mitigate this particular threat, I instrument online enrollment using broadband measures lagged by one term. Using the lagged versions breaks any direct connection between broadband speed in a given term and course outcomes. The lagged versions also more accurately reflect the timing of the course selection decision, which should occur in the period prior to the current term.

If broadband measures are correlated with an unobserved variable that is also correlated with the student outcomes, then the estimated treatment effect will be biased. For example, students who live in areas with faster broadband connections may have other supports that make them more likely to complete or pass courses regardless of delivery type. If broadband speeds are positively associated with socioeconomic indicators (*e.g.,* income) that predict postsecondary success, then students who are more likely to take online courses because of access to better broadband may also be more likely to succeed due to other supports. Because instruments need only be conditionally exogenous ($\mathbf{Z} \perp \varepsilon_2 \,|\, \mathbf{X}, T$), however, I mitigate this threat by including a number of student-level covariates in the model that may be associated with both broadband access and student outcomes.

A final threat to exogeneity of the instruments occurs when omitted variables correlated with both the treatment and outcomes are unobservable. For example, a student may choose to move to an area that has faster broadband speeds. Here the threat is that unobserved characteristics of the student that motivated her to move to areas with better digital infrastructure (or allow her to do so) may positively affect her likelihood of completion and passing. Because this relocation scenario runs counter to the point of online education—that people can attend from where they live without needing to relocate to be near a brick-and-mortar campus—I

[3] http://www.pewinternet.org/fact-sheet/internet-broadband/

am less concerned about this particular threat to internal validity. The inclusion of large number of covariates may also mitigate the threat of bias due to unobservable variables if observed characteristics serve as proxies (*e.g.,* high school GPA conditional on other socioeconomic factor may account for some part of student motivation).

The exclusion restriction cannot be formally tested. With some instrumental variables models that use more instruments than endogenous regressors, tests of overidentification may be used. Though they do not directly test for instrument exogeneity, they do provide evidence that offers support for or against such claims (Angrist & Pischke, 2008; Greene, 2012). Unfortunately, commonly used overidentification tests such as the Sargan test of overidentifying restrictions (Sargan, 1958) do not apply to the bivariate probit model. I therefore estimate the treatment effect of online courses using a non-Bayesian 2SLS model and compute the Sargan test of overidentifying restrictions. The results from this computation fail to reject the null that the instruments are plausibly exogenous for either outcome (withdraw/pass) and are shown in Table 3.8. While these results do not apply directly to the primarily models I estimate, I argue that they provide evidence of instrument exogeneity in addition to the theoretical support I offer above.

3.4.3 Estimation

To estimate the Bayesian bivariate probit model, I use a data augmentation technique that improves posterior convergence (Albert & Chib, 1993; Hollenbach et al., 2016; Jackman, 2009). Rather than fitting the discrete first- and second-stage outcomes (T_i and Y_i), the model instead fits the transformed continuous latent variables (T_i^* and Y_i^*) using truncated normal distributions that are constrained to be positive when the outcome is 1 and negative when the outcome is 0 (see Albert and Chib (1993) or Hollenbach et al. (2016) for a full description of the marginal distributions).

As is the case with all Bayesian estimations, the posterior distribution of parameters, $p(\theta \,|\, x)$, is a function of prior beliefs, $p(\theta)$, and the likelihood of the data, $p(x \,|\, \theta)$,

$$p(\theta \,|\, x) = \frac{p(x \,|\, \theta) p(\theta)}{p(x)} \tag{3.7}$$

Because the unconditional likelihood of the data can be difficult to compute, most Bayesian estimations assume that the posterior is proportional to the prior times the likelihood up to a proportional constant

(Gelman et al., 2014):

$$p(\theta \,|\, x) \propto p(x \,|\, \theta) p(\theta) \tag{3.8}$$

When the prior and likelihood follow conjugate distributions, the posterior may be computed analytically. For applied problems, however, it is often the case that the posterior has no closed form solution (Gelman et al., 2014). For these problems, the posterior may instead be estimated and sampled from using a Markov chain Monte Carlo (MCMC) algorithm such as a Gibbs, Metropolis, or Hamiltonian Monte Carlo (HMC) sampler. Recently, the `Stan` probabilistic modeling language (Stan Development Team, 2016) has made available its No-U-Turn Sampler (NUTS), an optimized HMC-based algorithm, that efficiently samples from the posterior (M. D. Hoffman & Gelman, 2014).

Complicated models and large data sets, however, make it difficult to sample from the posterior distribution. This is especially the case for HMC samplers that must evaluate the gradient of the likelihood multiple times within each sample (M. D. Hoffman & Gelman, 2014). For these types of problems that may otherwise remain computationally intractable, a third method of Bayesian estimation, variational Bayes, may provide the best and most practicable solution. Rather than attempt to reach and then sample from the posterior distribution directly, variational Bayes involves deciding upon and then drawing from an approximation of the posterior that is more tractable.

Specifically, the variational Bayes algorithm attempts to minimize the Kullback-Leibler (KL) divergence between a proposed posterior distribution approximation and the exact posterior (Gelman et al., 2014; Kucukelbir et al., 2016):

$$\varphi^* = \arg\min_{\varphi \in \Phi} \mathrm{KL}(q(\theta;\varphi) \,||\, p(\theta \,|\, x)). \tag{3.9}$$

Because the true posterior, $p(\theta \,|\, x)$, is unknown, the KL divergence cannot be directly computed. Instead, the evidence lower bound (ELBO), which is a function of the prior and likelihood functions, is computed:

$$\mathscr{L}(\varphi) = \mathbb{E}_q[log\, p(x|\theta)p(\theta)] - \mathbb{E}_q[log\, q(\theta;\varphi)] \tag{3.10}$$

When maximized, the ELBO is equal to the KL divergence as long as the support for the proposed approximation distribution, q, is contained within the support of the true posterior. By choosing hyperparameters,

φ^*, that maximize the ELBO,

$$\varphi^* = \arg\max_{\varphi \in \Phi} \mathscr{L}(\varphi) \text{ s.t. } \operatorname{supp}(q(\theta;\varphi)) \subseteq \operatorname{supp}(p(\theta \,|\, x)), \tag{3.11}$$

variational Bayes becomes an optimization problem akin to expectation maximization and one that can be solved efficiently even with complicated models and large data sets (Gelman et al., 2014; Kucukelbir et al., 2016).

Because variational Bayes requires choosing a correct candidate distribution as well as a number of analytic computations to be sure that support for the approximating distribution is contained within that of the true posterior, computation can be difficult and historically has required problem-specific solutions. `Stan`, however, has incorporated the Automatic Differentiation Variational Inference (ADVI) method, which uses `Stan`'s existing capabilities to (1) select a candidate distribution, (2) correctly transform parameters into a common supported space, (3) approximate gradients using MCMC methods, and (4) optimize the variational distribution until convergence is achieved (Kucukelbir et al., 2016; Stan Development Team, 2016). The ADVI method has the additional benefit of using the same modeling language and data structures as other `Stan` routines.

To compute the posterior distributions of the Bayesian bivariate probit model, as well as other single level logistic models, I initialize and run four separate variational Bayesian chains using the same respective model and data.[4] I assume weakly informative priors ($\mathscr{N}(0, 2.5)$) for regression parameters in all models. Each chain optimized the ELBO and drew 1000 samples from the approximating distribution. If sample posterior values from the chains converge, it is evidence that the computed posterior distribution accurately reflects the true posterior distribution. For all models, key parameter estimates did converge so results across estimation chains within each model were combined. Each mean value and 95% credible interval reported across tables, therefore, is the result of 4000 posterior draws.

[4]The only exception were the models of zip code income regressed on the measures of broadband connectivity that I ran as a check on this potential source of bias. These models were estimated using the `rstanarm` R package (Gabry et al., 2017). Because the data set only included 707 observations and the model six parameters, the posterior quickly converged using the NUTS MCMC sampler.

3.5 Data

3.5.1 University System of Georgia

The primary source of student-level data for this study come from the University System of Georgia (USG), which is one of the largest postsecondary systems in the United States. During the period of the study, from 2011 to 2014, the university system enrolled around 300,000 students, approximately 70% of which attended full time.[5] USG is comprised of four research universities, four comprehensive universities, ten state universities, and twelve state colleges.[6] Located throughout the state, these institutions educate a wide range of students from rural, suburban, and urban areas. While the University of Georgia at Athens represents the state's flagship institution, drawing students from around the state and country, the state colleges and universities have missions that include supporting the educational and economic development goals of their local areas.[7]

USG has had an active policy environment during the past number of years that makes it an interesting system to study. One major policy begun in 2011 has been to consolidate a number of its institutions. Following the "Principles of Consolidation" established by the Board of Regents, USG administrators have combined a number of state universities and state colleges with the goals of (1) increasing educational attainment, (2) improving accessibility and regional identity, (3) reducing redundancies in educational offerings, (4) realizing benefits of economies of scale, (5) improving economic development within the state's regions, and (6) optimizing administrative roles.[8] Another key policy that is also in line with many of these principles has been to increase online educational offerings through its eCore and Georgia ONmyLINE systems.[9] Because of its large size, location, active policy environment, diverse student population, and the fact that Georgia students are increasingly likely to take online courses due to their greater availability, the USG system represents a relevant context for the study of online course outcomes.

To construct the analysis data set, I use system administrative data which provide a rich set of student- and course-level covariates. With student transcript data, I am able to determine both primary outcomes: whether a student withdraws from the course and, conditional on completion, whether s/he passes with a C-

[5] Enrollment numbers provided by USG and available at http://www.usg.edu/research/enrollment_reports.

[6] USG had a few institutional consolidations in the time period of the study. To differentiate between physical institutional locations, I used the old unique school names when determining where students attended.

[7] http://www.usg.edu/institutions/

[8] http://www.usg.edu/news/release/regents_approve_principles_for_consolidation_of_institutions

[9] http://www.usg.edu/information/online_students/

or better. The transcript data also include an indicator for whether the course is online, the CIP code for the course, and an indicator for whether the course is considered developmental.

Student-level characteristics gathered from the administrative data files include information on the student's gender, race/ethnicity, and age as well as military status and whether the student received any financial aid. For the students who attended high school in Georgia, administrative data files also include high school GPA. Because most of the students in the administrative data set attended high school in Georgia, I limit the analysis sample to freshmen students from Georgia who matriculated in the fall of 2010 so that I could use this key measure of prior ability in my models. I further limit the sample to those students who completed the Free Application for Federal Student Aid (FAFSA) because these data provide indicators for whether the student's expected family contribution (EFC) is \$0, their mother had completed any college, and they planned to live in campus housing. Importantly, the administrative data files also provide student addresses at each term, which I use to assign each student his or her average broadband measures for each term. Data on student outcomes cover the period from the spring of 2011 to the fall of 2014. Because of the structure of the broadband data (discussed in the next section) and the comparatively small number of students who take them, I drop summer courses from the sample. Finally, a few of the USG institutions reported no students (or in the case of one institution, a single student) taking an online course during the period under study. Observations from these schools are also dropped since students did not have the opportunity to choose online course sections.[10] The final analysis data set is thus made up of approximately 767,000 student-course observations for 40,000 unique students.

Descriptive statistics of the full sample are shown in Table 3.1. The first column of the table shows that only 5,695 courses, less than 1% of the total number, are online. Comparing the first two columns, online courses have greater percentages of female and Black/African American students but comparatively fewer Hispanic/Latino/a and Asian students. Online students are on average a year older than face-to-face students (20.44 vs 19.45 years) and are much more likely to be a member of the military (0.51% vs 0.09%). Slightly fewer online students have mothers who have completed some college or receive any financial aid, even though slightly more online students have EFCs of \$0. As might be expected, many fewer online

[10]Omitted institutions include two research universities: Augusta State University (which later merged with Georgia Health Sciences University to form Georgia Regents University) and the Georgia Institute of Technology; one state university: North Georgia College and State University (which later merged with the University of North Georgia); and five state colleges: Gainsville State College (which later merged with the University of North Georgia), Georgia Gwinnett College, Middle Georgia College (which later merged with Macon State College to form Middle Georgia State College), South Georgia College, and Waycross College (which, along with South Georgia College, later became South Georgia State College).

students indicate a plan to live in campus housing (37.24% vs 57.75%). In terms of institution type, online students are more likely to attend comprehensive and state universities than their face-to-face peers. The opposite is true for research universities and state colleges. Comparatively fewer online courses are listed as developmental (0.39% vs 2.84%).

Unadjusted differences show that students are 3.8 percentage points more likely to withdraw from online courses than face-to-face courses (9.96% vs 6.16%). Conditional on completing the course and earning a grade, online students are 4.33 percentage points less likely to earn a passing grade of a C- or better (80.36% vs 84.69%). As a point of comparison, the raw difference in persistence rates is almost exactly the same as the 3.4 percentage point difference for students in the Washington State Community College system (Xu & Jaggars, 2013), but smaller than the 11 to 13 percentage point difference for remedial students in the Virginia Community College System (Xu & Jaggars, 2011).[11]

In the middle two columns labeled "Both", only those courses taken by students who, in the same term, took at least one online and one face-to-face course are included. These courses represent the analytic subsample used in secondary sensitivity analyses. Descriptive patterns for this subsample largely follow those seen in the full sample. Like the full sample of students, subsample students are more likely to withdraw from courses (9.22% vs. 5.53%) and less likely to pass completed online courses (83.6% vs 87.78%). Though they are not used for other sub-analyses, descriptive statistics for two other behavioral subgroups are shown for comparison: courses taken by online-only students (5th column) and those taken by face-to-face-only students (6th column). Compared to the subgroup of students who take only online courses, students who take both types of courses are much less likely to withdraw from their online courses (14.5% vs 9.22%). They are also slightly less likely to withdraw from their face-to-face courses than students who only take face-to-face courses (5.53% vs 6.18%). For course completion, students who take both in the same term are much more likely to pass their online courses than online-only students (83.6% vs 59.14%) and slightly more likely to pass their face-to-face courses than students who only take face-to-face courses (87.78% vs 84.6%). These differences suggest that students who take both types of courses may do so strategically or be different from never- and always-online students, further justifying a quasi-experimental

[11] Both Xu & Jaggars papers use different samples than I do for this study. Whereas I use all USG students (less sample restrictions noted above) and include an indicator for a course's status as developmental, Xu and Jaggars (2011) focus solely on those students taking developmental courses in the Virginia Community College System. On the other hand, Xu and Jaggars (2013) drop developmental courses from their sample of students from the Washington State community and technical college system. In both papers, the authors include summer course sections, which I drop from my analyses. Finally, both student populations considered by Xu & Jaggars were enrolled slightly earlier than those in my USG sample, taking their courses in the mid to late 2000s. Comparisons between their findings and mine throughout this chapter should be understood in the context of these differences.

design to reduce bias caused by self-selection into the treatment.

3.5.2 National Broadband Map

Broadband data used to compute student-level download and upload speeds were collected from the National Broadband Map website.[12] At six month intervals between 2011 and 2014, Internet service providers (ISPs) within each state gave information about upload and download rates at the census block level (National Telecommunications and Information Administration, 2011). After corroboration against other sources of broadband information, these data were released to the public as both interactive maps and complete datasets.

ISPs reported measures of broadband speed in ordered categories that ranged from 1 (less than 200 kB/sec) to 11 (greater than 1 GB/sec). Table 3.2 shows the expected download time of three common digital objects—a book (1 MB), an MP3 music file (4 MB), and a movie (6 GB)—at various tiers of Internet service. Past guidelines have suggested that watching a university lecture requires a minimum download speed of 4 MB/sec, which translates to tier 5 speeds (Federal Communications Commission, 2015a). At this minimum, a student might require more than 30 minutes to download a lecture of similar length.[13] The Federal Communications Commission (FCC) updated its definition of the threshold for broadband, however, requiring download speeds of at least 25 MB/sec (tier 8) and upload speeds of 4 MB/sec (tier 5 up from tier 3) to qualify as such (Federal Communications Commission, 2015a, 2016).

Students were assigned distance-weighted averages of download and upload speeds based on their zip code in each term. Inverse distance weights were used when computing the zip code average so that nearby broadband measurements would be more heavily weighted than those far away, thereby more accurately reflecting the average download and upload speeds experienced in the area. These weighted averages were constructed by first computing the Great Circle ("as the crow flies") distance, d_{nk}, between each zip code centroid, n, and all census block group centroids, k, in the state of Georgia.[14] This resulted in an $N \times K$ matrix where $N = \#$ zip codes and $K = \#$ census block groups. These values were next squared (Shepard,

[12] www.broadbandmap.gov

[13] Assuming an average movie length of 2 hours.

[14] Grubesic (2008c) makes a compelling argument that zip codes are not always the best unit of analysis for spatial data analyses due to their non-uniform structure. IRB requirements, however, prevented my being able to geocode student street addresses using online tools such as the Google Maps API.

1968) and inverted:

$$w_{nk} = \left(\frac{1}{d_{nk}^2}\right). \tag{3.12}$$

Inverse distance weighted averages of each zip code's download and upload speeds were then computed by applying the weights, w_{nk}, and summing:

$$wbroadband_{zip} = \left(\frac{\sum_{k=1}^{K} w_{nk} \times measure_k}{\sum_{k=1}^{K} w_{nk}}\right). \tag{3.13}$$

Finally, students were assigned their respective measures of download and upload speed by merging computed zip code averages on the zip code of the address they reported for the term.

Figure 3.1 shows average download and upload speeds experienced across the state of Georgia based on data released in June 2013. To make the visualization clearer, mapped averages were computed at the census tract level using the same inverse distance weighting technique. Each map uses a diverging color scheme in which the central values in white roughly correspond to the updated 2015 threshold for broadband (approximately tier 8 for download speeds and tier 5 for upload speeds). Red hues show averages below the threshold while blue hues show those above. Darker hues represent increased distance from the threshold in absolute terms. These maps show the heterogeneity in broadband speeds across the state of Georgia during this period. While areas in the northern, western, and coastal area of the state had higher average download and upload speeds, the southern part of the state generally experienced lower speeds. Though less clear on the state-wide map, the Atlanta metropolitan area itself shows a fair amount of variation. Other data collection periods show similar results and provide evidence that students did not experience the same broadband connectivity across the USG system during this time.

Figure 3.2 shows the distribution of download (top) and upload (bottom) speeds assigned to students in the full sample. These combined histogram and density plots are generally normally distributed, with download and upload averages centered at tier 7 and tier 5, respectively, but with variation across the sample. The vertical lines on each plot show both the original and updated thresholds for broadband designation. While almost all students during the sample period had average download speeds under the old definition, the majority did not under the updated definition. About half experienced broadband upload speeds under the new definition, but as with download speeds, almost all did under the old definition. Along with Figure 3.1, Figure 3.2 suggests that broadband speeds experienced by students across the sample is sufficiently varied that it can exploited to predict online enrollment, and through it, student course completion and success.

3.6 Results

Unadjusted means reported in Table 3.1 show that online students are both more likely to withdraw from their courses and, conditional on completion, less likely to pass than students in face-to-face courses. Before fitting the bivariate probit models, I first estimate potential differences in withdrawal and pass rates using logistic regressions in which I regress the binary outcomes on a number of student and course characteristics. In each model, all covariates shown in Table 3.1 are included on the right-hand side of the equation: indicators for a student's gender, race/ethnicity, military status, and receipt of financial aid; indicators that equal one if a student's mother has at least some college, expected family contribution is \$0, and student planned to live in campus housing; indicators for course status as developmental, the term, USG institution type, and urbanicity of the surrounding area; high school GPA and student age. Also included are indicators for course subject that are based on five binned categories of CIP codes, the number of courses the student took in the semester, and squares of student age and high school GPA.

Table 3.3 reports the posterior mean and 95% credible interval of the coefficient of interest, β_{online}, for each outcome as well as the non-causal average treatment effect (ATE) of taking an online course. Because Bayesian estimation does not rely on the frequentist framework of null hypothesis significance testing, no significance-related statistics (*e.g., p*-values) are reported. Instead the 95% credible intervals may be directly interpreted as the range in which 95% of estimated posterior coefficient values will be located. In regression models, credible intervals that cross zero suggest greater uncertainty about the likely value of the coefficient; this includes the possibility that the coefficient is in fact centered around zero (or, in the case of treatment effects, there is no effect). Posterior distributions also lend themselves to interpretation across a range of decision points (Gelman et al., 2014). For example, in a logistic model in which the outcome is an indicator for passing the course, a β_{online} posterior for which 80% of draws are less than zero would indicate an association between passing the course and online delivery that was negative 80% of the time and positive 20% of the time. If 25% of posterior draws were less than -0.69, evidence would suggest that students in online courses were half as likely to pass their courses as face-to-face students one quarter of the time. The ability to consider multiple decision points such as these may be particularly relevant for administrators and policy-makers and represents a key benefit of Bayesian estimation over frequentist estimation (Gill & Witko, 2013; Jackman, 2009).

Turning to Table 3.3, results from the logistic regression indicate that students in online courses have a 0.651 mean increase in the logged odds of withdrawing from the course or, ($e^{\beta_{online}}$) 92% average increase in the relative odds of withdrawing. These values reflect an average treatment effect of 0.048, which translates to a 4.8 percentage point increase in the likelihood of withdrawing from the course if it is conducted online.[15] The 95% credible intervals on the ATE posterior distribution range from 3.5% to 6.4%. With the addition of student and course-level controls, the average (non-causal) effect of taking an online course is larger than the 3.8 point difference seen in the raw data but could credibly contain it. Similarly, the Bayesian point estimate of -0.567 ($e^{\beta_{online}} = 0.567$) in the second column shows that the odds that online students who complete their courses will pass are 43% lower than those of their peers in face-to-face courses. This translates to an average effect of -7.6 percentage points [-9.1 — -6.0], which is nearly double the unadjusted difference of -4.33 and, based on the credible interval, almost always likely to be larger.

Results of the logistic regressions are similar when fit to the subset of students who took at least one online and one face-to-face course in a term. The mean estimated ATE of online course-taking on withdrawing, reported in the first column of the second panel, is 0.044 [0.029 — 0.059]—nearly identical to that of the full sample. The average is also larger than the 3.69 point difference shown in the unadjusted averages, but because the 95% credible interval includes the raw difference, the regression-adjusted estimate may be the same much of the time. In terms of passing the course, the ATE of online courses among the subset of students is less than that estimated for the full sample. Among these students, the average estimate is -0.041, with a credible range of -0.058 to -0.026. The posterior mean translated to the percentage point scale is nearly identical, however, to the -4.18 percentage point difference shown in the unadjusted subsample data.

These logistic regression results provide further evidence that, on average, students may not perform as well in online courses as they do in face-to-face courses. When controlling for a host of covariates, estimates show the same or larger absolute differences than what are found in the unadjusted sample. In terms of passing the course, the difference in estimates between the full sample of students and the subset provide some evidence of self-selection into online courses that may be attributable to unobserved student characteristics. Though the association between online course-taking and withdrawal is the same for both groups, the subset of students who take both online and face-to-face courses in a semester appear less

[15] All treatment effects (ATE, TOT, and LATE) reported in this chapter estimate the effect at observed values. Formulas used to compute the treatment effects are shown in Appendix section 3.B

negatively affected by online delivery in terms of passing. As I note above, these sometimes-online students pass their online courses more often than their only-online peers (84% vs 59%) and pass their face-to-face courses more often than their face-to-face-only peers (88% to 85%). In fact, the sometimes-online students pass their online courses only slightly less often than their face-to-face-only peers pass their courses. Thus it may be that the subset of students who are willing to take both types of courses in given semester—the majority of online enrollees—enroll strategically, choosing online courses that they are more likely to pass, are more motivated to complete, or have other unobserved characteristics that support their eventual success. For this reason, the results from the logistic regression models and the average treatment effects they produce may retain bias in the estimated effects that online courses have on student success.

To produce less biased estimates, I fit the recursive bivariate probit model that jointly models selection into treatment and the outcome of interest. The outcome of the first equation in each model is an indicator for online course delivery. This same indicator for online delivery is included on the right-hand side of the second equation, which places on the left-hand side either an indicator for withdrawing from the class or, conditional on course completion, an indicator of passing the course with a C- or better. I include on the right-hand side of each equation the same student, course-level, and geographic covariates as in the logistic regression models. Lagged values of download speed, upload speed, their squares and interaction are included on the right-hand side of the first equation for a total of five excluded instruments.

This model, like all instrumental variables models, requires that proposed exogenous instruments be predictive of selection into treatment. I therefore first report results from a logistic regression of the indicator for online delivery regressed on all instruments, which include measures of broadband speed as well as the same covariates used in prior models. Results from this estimation are shown in Table 3.4. As before, only the Bayesian point values (mean posterior density) and 95% credible intervals for the coefficients of interest are reported. These include the student's download speed and upload speeds, both at level and squared, as well as the interaction between the two measures. The first column reports the results of the estimation when broadband measures from the same term as the course were used. The primary mean coefficient values for download speed, upload speed, and the interaction between the two are all positive (0.535, 0.35, and 0.145) with 95% credible interval lower bounds above zero. Negative quadratic terms suggest a nonlinear relationship and are similarly estimated with precision. A one unit change in broadband tier for both download and upload speeds[16] predicts that a student is approximately 0.7 to 1.6 percentage

[16]Roughly commensurate with purchasing the next level of service from an ISP.

points more likely to be enrolled in an online course, with an average increase of 1.1 percentage points.

Fitting the model with broadband values lagged by one term produces similar but comparatively less informative results. These are shown in the second column. Once more, the associations between download and upload speeds and online enrollment remain credibly positive and non-linear. The probable effect that these measures have online enrollment, however, is attenuated. A single tier increase in both download and upload speeds is associated with a 0.5 to 1.3 point increase in the odds of taking an online course in the next semester, a 0.8 point increase on average. These results, while predictive, are only weakly so. When fitting a frequentist 2SLS, a common test of instrument relevance is an F-test of their joint significance. For the withdraw and passing outcomes, Table 3.8 shows critical values of 12.25 and 10.98, respectively. Though these values are above the heuristic value of 10 often used to indicate instruments of sufficient strength, they fail to reach the updated critical value of 18.37 recommended in the case of five exogenous instruments and one endogenous regressor (Stock & Yogo, 2005). As they pertain to a 2SLS model, these instruments may produce effect estimates that retain the bias found in those estimated more efficiently with ordinary least squares (Greene, 2012).

Performing a similar test in a Bayesian framework, I compare these models with those that use only other covariates to predict online enrollment using the Deviance Information Criterion (DIC). The benefit of the DIC statistic over a more straightforward comparison of model fit using, for example, values of the log likelihood, is that whereas the log likelihood will almost always increase as more predictors are added to the model, the DIC adds a penalty for additional predictors (Gelman et al., 2014).[17] Table 3.5 shows the results of these computations. Using the data samples for both outcome models, the DIC decreases slightly with inclusion of the broadband predictors. When using the full set of student-course observations, $\text{DIC}_{\text{no broadband}} = 53396.88$ whereas $\text{DIC}_{\text{broadband}} = 52242.08$. Other model/data sample comparisons follow a similar pattern. As expected, the log likelihood for all models increases with the inclusion the broadband predictors, but the lower DIC values agree with other evidence indicating that lagged broadband measures may be weakly informative of online enrollment decisions when other student, course, and geographic characteristics are taken into account. These results support the choice of a Bayesian bivariate probit spec-

[17]The formula for DIC is:

$$\text{DIC} = -2\log p(y \mid \hat{\theta}_{Bayes}) + 2p_{\text{DIC}}$$

where $\hat{\theta}_{Bayes}$ is the posterior mean and p_{DIC} is

$$p_{\text{DIC}} = 2\big(\log p(y \mid \hat{\theta}_{Bayes}) - \text{E}_{\text{post}}(\log p(y \mid \theta))\big).$$

ification over an easier to estimate, but potentially biased 2SLS model since (1) the bivariate probit model has been shown to perform better with weak instruments and (2) the Bayesian estimation framework allows the degree of predictive power that broadband instruments have on online enrollment to be reflected in the spread of the second-stage posterior distributions (Hollenbach et al., 2016).

Table 3.6 shows the results from the Bayesian bivariate probit model estimations. Again, though all covariates are included in the model, only the posterior mean of the coefficient of interest, the indicator for online course delivery in the second-stage equation, β_{online}, is reported. In addition to estimates of the average treatment effect (ATE), the table reports treatment-on-the-treated (TOT) effect and local average treatment effect (LATE). Finally, the table also gives the posterior mean of ρ, the measure of correlation between the errors of the first- and second-stage equations in each model. In the context of the bivariate probit model, ρ may be used in place of the frequentist Wu-Hausman test used to determine whether a less efficient but more consistent IV estimator is preferred over a more efficient, but less consistent estimator. When $\rho = 0$, this suggests that the errors of the two equations are uncorrelated, and it would be more efficient to estimate them as separate probit models (Greene, 2012). Across the four models, the mean posterior of ρ is in the same direction as the treatment effect, but in only one, where $y_i = withdraw$ using the full set of observations, does ρ have 95% credible intervals that do not cross zero. Nevertheless, theory and other evidence, including that found in the prior literature and when fitting other models (see Table 3.8), strongly suggests that the choice to enroll in an online course is endogenous and should be instrumented.

Turning to the outcomes of the models, the first column of the top section of Table 3.6 reports $\beta_{online} = 0.16$ [0.042 — 0.278], which indicates that students in online courses are more likely to withdraw than those in face-to-face courses. This result translates to an ATE of 0.028 [0.006 — 0.053], or a 2.8 average percentage point increase in the likelihood of withdrawing from the course. Because of the quasi-experimental design of the model, this value represents a less biased estimate of the effect of the online delivery mechanism. This value is smaller than both the unadjusted mean and logistic regression adjusted values ($\text{ATE}_{IV} = 2.8$, $\text{ATE}_{logit} = 4.8$, $\text{Diff} = 3.8$), but includes both in its 95% credible interval. The two other estimated treatment effects are similar: TOT = 0.03 [0.007 — 0.055] and LATE = 0.029 [0.007 — 0.054].

Like the ATE estimates, both the TOT and LATE estimates were computed at the observed values in the data. Specifically, the LATE estimates were computed by giving every student in the sample a one tier increase in both download and upload speed from their observed values. Whereas ATE estimates represent the treatment effect averaged across all observations, TOT estimates apply to those actually treated (take an online course in this study). LATE estimates, on the other hand, apply to students who were induced in online courses by improvements in broadband speed. ATE, TOT, and LATE estimates need not be the same if the treated and marginal subgroups are different in their response to the treatment than the average across the full sample. Agreement between the three treatment posteriors on the effect of online course delivery on withdrawal suggests a homogeneous range of probable treatment effects among USG students during this time period.

I find a similar pattern for the effect of online courses on the likelihood of passing the course with a C- or better. In the second column of Table 3.6, $\beta_{online} = -0.201$ [-0.337 — -0.065], which translates to an ATE of -0.054 [-0.096 — -0.016] or an average 5.4 point reduction in the likelihood of students successfully passing their courses when they are online. While smaller in absolute terms than the logistic regression-adjusted estimate, $\text{ATE}_{logit} = -7.6$, this effect is larger than the unadjusted difference, -4.33, observed in the data. As with the model on withdrawal from the course, the other treatment effect estimates for this model are largely similar—and the same within rounding error of each other: TOT $= -0.056$ [-0.097 — -0.017] and LATE $= -0.056$ [-0.097 — -0.017]. Even accounting for attrition by removing those students who withdraw from their courses, I find USG students to be 1.6 to 9.6 percentage points less likely to pass courses that they take online.

I turn finally to the subset of courses taken by students who, in the same term, take at least one online and one face-to-face course. The results for bivariate probit models fit to these data are reported in the second panel of Table 3.6. Overall, they are similar to those reported for the full set of observations. Students in this group are 0.6 to 4.8 percentage points more likely to withdraw from online course sections (ATE = 2.5 [0.006 — 0.048]). The effect for those treated and those induced into treatment by increases in broadband speed is similar: TOT = 0.026 [0.007 — 0.047] and LATE = 0.024 [0.006 — 0.046]. These students are also less likely to pass their online courses on the whole (ATE $= -0.015$ [-0.044 — 0.01], TOT $= -0.016$ [-0.044 — 0.011], LATE $= -0.017$ [-0.045 — 0.008]), but with less certainty as the 95% credible intervals for each treatment effect cross zero. Yet even though these estimates are less precise, it is important to note that more than 87% of each effect posterior lies to the left of zero, meaning that for this subset of students, online course delivery negatively affects their likelihood of passing the course nine out of ten times.

3.7 Conclusion

In summary, I find students enrolled in the University System of Georgia between 2011 and 2014 to be more likely to withdraw from a course and, conditional on completion, less likely to earn a passing grade if that course is delivered online. Because of the quasi-experimental design of the study, these findings more closely speak to the effect of the online delivery mechanism itself than differences between students who do and do not take online courses. In direction, my findings concur with those from other quasi-experimental studies (Xu & Jaggars, 2011, 2013). The credible range of effects I estimate for withdrawing, 0.6 to 5.3, while smaller than those reported among students taking introductory courses in the Virginia Community College System (9 to 13), contain those reported for students in the Washington State community college system (3.6 to 4.4). Thus this study both supports previous findings and adds to the literature by providing results estimated on a new state-wide population of students.

This chapter also adds to the literature by using a new model and estimation technique as well as a novel set of instruments that were constructed with spatial methods and new data sources. With the recursive bivariate probit, I am able to recover plausible estimates and a range of treatment effects even with potentially weak instruments (Greene, 2012; Hollenbach et al., 2016) that are both internally consistent and agree with those found in prior studies that use quasi-experimental designs. Using variational Bayes with the `Stan` programming language, I am able to fit the complex bivariate probit model to a large sample of data (Gelman et al., 2014; Kucukelbir et al., 2016; Stan Development Team, 2016). Though they require a little more effort on the part of the researcher to set up the data and code the model, I believe that the benefits of the Bayesian bivariate probit model and variational Bayes outweigh the costs for difficult models and may be fruitfully applied in other education policy studies for which standard frequentist or Bayesian sampling algorithms fall short.

A few potential limitations to my findings apply. First, while analytic models include the number of courses each student takes in a given semester in order to control for the differences between part- and full-time students, they do not include a measure of the number of those courses that are online. My results, therefore, do not speak to any potential dosage effect for online courses. Furthermore, though I broadly control for the subject area group of each course, I do not control for the specific discipline of each course. Similarly, I cannot control for the respective difficulties of courses since these measures are unobserved in the data. The quasi-experimental design of my analysis should mitigate bias due to students selecting into or out of online courses based on their difficulty. If the lagged measures of broadband access that I use as excluded instruments are not exogenous, however, then the estimates I report may retain bias due to endogenous selection into treatment.

A specific critique of the instruments may be that students with faster Internet connections simply hail from relatively wealthier areas. Even controlling for whether a student accepted financial aid or if s/he had an expected family contribution may not completely account for the advantage that living in wealthier area imparts or its correlation with positive course outcomes. Broadband access, however, can be highly variable, even within relatively small, homogeneous areas due to physical boundaries and last-mile infrastructure restrictions (Grubesic & Murray, 2002; Oyana, 2011). To investigate potential correlations across Georgia's zip codes in terms of income and broadband access, I regressed the log of zip code median incomes reported during the period of the study on the same average measures of broadband speed I used as instruments. The results of this regression, given in Table 3.7, show that the mean posterior values are generally close to zero with wide 95% credible intervals that cross zero. Though not a definitive test, I do not find evidence that broadband speeds are strongly correlated with median incomes in the student's zip code, at least during this time period.

More formal tests of instrument exogeneity unfortunately do not exist. While 2SLS estimation has a number of well-described tests for instrument overidentification (Angrist & Pischke, 2008), the bivariate probit does not. The nonlinear nature of the probit link function means that common tests of overidentification such as the Sargan test that assume homoskedastic errors (Greene, 2012) are not appropriate. In defense of the plausible exogeneity of my instruments I offer by proxy the non-significant results of the Sargan test computed for 2SLS estimates and reported in Table 3.8. I also note the theoretical arguments in favor of the instruments and the fact the reported effects are both internally consistent with unadjusted sample differences and non-causal results as well as other findings in the literature (Xu & Jaggars, 2011, 2013).

Finally, it may be that USG students who attended college during my study period react differently to online course delivery than other populations of students. Though the public system contains many institutions and educates students from a wide range of socioeconomic and educational backgrounds, the higher education context in Georgia may be idiosyncratic, meaning that my results do not apply to students in other postsecondary contexts. My choice to limit the sample to freshmen who filled out the FAFSA likely leaves out many older and non-traditional students who may experience online courses differently than students in my sample (Jaggars, 2014). This could also affect the generalizability of my results to other populations who may be more likely to take online courses (Cottom, 2017; Cox, 2006; Radford & Weko, 2011).

These potential limitations notwithstanding, this chapter's results add to the weight of evidence that says online courses may inhibit student success. Yet in spite of this negative finding, I concur with the authors of prior studies in that I do not recommend abandoning online courses wholesale (Xu & Jaggars, 2013). My data only indicate whether a course is online. Even though I broadly control for the subject area of the course, I do not see characteristics of different online courses that may support or hinder student success. Online courses have great potential for expanding the reach of higher education to students who have historically had difficulty in accessing it (Bowen, 2013; Selingo, 2013). More work, both quantitative and qualitative, needs to investigate best practices in online learning environments so that they may be scaled up and out. Based on recent trends as well as the joint historical arc of technology and education, it appears that greater numbers of students will complete at least some of their postsecondary education online in the future. The task for researchers and policymakers alike will not be to prevent them from doing so, but to understand the mechanisms that will most support their success.

Table 3.1: Descriptive statistics of the student-course sample

	Delivery		Behavior			
			Both			
	Online	Face to Face	Online	Face to Face	Online only	Face to Face only
Student Characteristics						
Female	61.9	58.81	61.97	61.32	61.41	58.76
	(48.57)	(49.22)	(48.55)	(48.7)	(48.71)	(49.23)
Black/African American	34.94	31.22	33.68	32.61	42.75	31.19
	(47.68)	(46.34)	(47.27)	(46.88)	(49.5)	(46.33)
Hispanic/Latino/a	4.69	5.14	4.9	5.2	3.4	5.14
	(21.14)	(22.08)	(21.58)	(22.2)	(18.15)	(22.08)
Asian	2.81	3.94	2.92	3.09	2.14	3.95
	(16.53)	(19.45)	(16.83)	(17.31)	(14.49)	(19.49)
Other race/ethnicity	8.62	7.3	9.16	8.78	5.3	7.27
	(28.07)	(26.01)	(28.85)	(28.3)	(22.41)	(25.96)
Age (years)	20.44	19.45	20.52	20.52	19.97	19.43
	(1.47)	(0.99)	(1.41)	(1.19)	(1.73)	(0.97)
High School GPA (4.0 scale)	3.11	3.13	3.11	3.13	3.1	3.13
	(0.53)	(0.55)	(0.53)	(0.54)	(0.5)	(0.55)
Military	0.51	0.09	0.55	0.34	0.25	0.09
	(7.12)	(3)	(7.4)	(5.79)	(5.02)	(2.92)
FAFSA						
Mother some college or above	55.49	57.78	56.26	57.82	50.69	57.78
	(49.7)	(49.39)	(49.61)	(49.39)	(50.03)	(49.39)
EFC of $0	30.2	29.32	27.76	23.97	45.27	29.43
	(45.92)	(45.52)	(44.79)	(42.69)	(49.81)	(45.57)
Received any financial aid	87.81	89.86	88.07	88.07	86.25	89.89
	(32.72)	(30.19)	(32.42)	(32.42)	(34.45)	(30.14)
Campus housing	37.24	57.75	37.41	42.46	36.19	58.06
	(48.35)	(49.4)	(48.39)	(49.43)	(48.09)	(49.35)
USG Institution Type						
Research university	12.77	16.87	14.65	19.14	1.13	16.82
	(33.37)	(37.45)	(35.36)	(39.34)	(10.6)	(37.41)
Comprehensive university	46.08	36.83	44.17	44.95	57.88	36.66
	(49.85)	(48.23)	(49.66)	(49.75)	(49.41)	(48.19)
State university	26.29	25.89	27.11	26.31	21.19	25.88
	(44.02)	(43.8)	(44.46)	(44.03)	(40.89)	(43.8)
State college	14.87	20.41	14.08	9.6	19.8	20.63
	(35.59)	(40.31)	(34.78)	(29.46)	(39.87)	(40.47)
Course Type						
Remedial/developmental course	0.39	2.84	0.35	0.63	0.63	2.88
	(6.2)	(16.61)	(5.88)	(7.93)	(7.92)	(16.74)
Broadband						
Download speeds	7.26	6.98	7.29	7.31	7.05	6.97
	(0.62)	(0.66)	(0.61)	(0.6)	(0.64)	(0.66)
Upload speeds	5	4.92	5.03	5.07	4.84	4.92
	(0.61)	(0.68)	(0.6)	(0.6)	(0.61)	(0.68)
Course outcomes						
Withdraw	9.96	6.16	9.22	5.53	14.5	6.18
	(29.94)	(24.05)	(28.93)	(22.85)	(35.23)	(24.07)
Pass (C- or better)	80.36	84.69	83.6	87.78	59.14	84.63
	(39.73)	(36.01)	(37.04)	(32.76)	(49.19)	(36.07)
N	5695	761238	4902	15162	793	746076

Notes. Numbers are percentages unless otherwise indicated. Standard deviations are in parentheses. Data are for students admitted as freshmen between the spring of 2011 and fall 2014 and cover the same time period. *EFC:* expected family contribution. Broadband measures are ordered categories from 1 to 11. The first two columns split courses by delivery type. The last three columns first split the sample by student behavior within term (students who take: both online and face-to-face courses, only online, and only face-to-face). The passing course outcome is conditional on not having withdrawn.

Table 3.2: Average time to download files of various size based on category of broadband speed

	Rate/sec range	Book (1MB) [HH:MM:SS]	MP3 (4MB) [HH:MM:SS]	Movie (6GB) [HH:MM:SS]
Tier 1	x <= 200 KB/sec	00:00:40	00:02:40	68:16:00
Tier 2	200 KB/sec <= x < 768 KB/sec	00:00:10	00:00:42	17:47:00
Tier 3	768 KB/sec <= x < 1.5 MB/sec	00:00:05	00:00:21	09:06:00
Tier 4	1.5 MB/sec <= x < 3 MB/sec	00:00:03	00:00:11	04:33:00
Tier 5	3 MB/sec <= x < 6 MB/sec	00:00:01	00:00:05	02:16:00
Tier 6	6 MB/sec <= x < 10 MB/sec	< 00:00:01	00:00:02	01:22:00
Tier 7	10 MB/sec <= x < 25 MB/sec	< 00:00:01	00:00:01	00:33:00
Tier 8	25 MB/sec <= x < 50 MB/sec	< 00:00:01	00:00:01	00:16:00
Tier 9	50 MB/sec <= x < 100 MB/sec	< 00:00:01	< 00:00:01	00:08:00
Tier 10	100 MB/sec <= x < 1 GB/sec	< 00:00:01	< 00:00:01	00:00:49
Tier 11	x > 1 GB/sec	< 00:00:01	< 00:00:01	< 00:00:49

Notes. Values taken from the National Broadband Map: https://www.broadbandmap.gov/classroom/speed. *KB:* kilobyte; *MB:* megabyte; *GB:* gigabyte. 1 MB = 1,000 KB; 1 GB = 1,000 MB. In 2015, the FCC raised the minimum download speed to qualify as broadband from 4 MB/sec (Tier 5) to 25 MB/sec (Tier 8).

Table 3.3: Logistic regression of withdraw or pass on indicator for online delivery and other covariates

	Withdraw	Pass
Full sample		
β_{online}	0.651	-0.567
	[0.554, 0.761]	[-0.664, -0.481]
ATE	0.048	-0.076
	[0.035, 0.064]	[-0.091, -0.06]
N	766933	719443
Subsample		
β_{online}	0.617	-0.34
	[0.506, 0.724]	[-0.448, -0.239]
ATE	0.044	-0.041
	[0.029, 0.059]	[-0.058, -0.026]
N	20064	18774

Notes. Bayesian point estimates are the mean values of 4000 draws from the posterior distribution with 95% credible intervals in square brackets. The subsample reflect courses taken by students who enrolled in both online and face-to-face courses in the same term. The pass outcome is conditional on not withdrawing from the course. Covariates included in all models, but not shown: indicators for student's gender, race/ethnicity, military status, and receipt of financial aid; indicators that equal one if student's mother has at least some college, expected family contribution is $0, and student planned to live in campus housing; indicators for course status as developmental, the term, and USG institution type; and high school GPA and student age, each at level and squared.

Table 3.4: Logistic regression of online delivery on measures of broadband speeds plus covariates

	Contemporaneous	Lagged
Download	0.535	0.373
	[0.457, 0.632]	[0.3, 0.458]
Download^2	-0.085	-0.07
	[-0.098, -0.076]	[-0.078, -0.064]
Upload	0.35	0.376
	[0.279, 0.436]	[0.284, 0.481]
Upload^2	-0.165	-0.148
	[-0.175, -0.156]	[-0.158, -0.139]
Download $\times$ Upload	0.145	0.125
	[0.138, 0.153]	[0.109, 0.14]
% point Δ for +1 down/upload tier	0.011	0.008
	[0.007, 0.016]	[0.005, 0.013]
N	766933	766933

Notes. Bayesian point estimates are the mean values of 4000 draws from the posterior distribution with 95% credible intervals in square brackets. Contemporaneous broadband measures come from the same term as the outcome; lagged measures are those from the term prior. Broadband speeds are measured in ordered categories from 1 to 11, with a unit increase approximating the next level tier of service. Covariates included in all models, but not shown: indicators for student's gender, race/ethnicity, military status, and receipt of financial aid; indicators that equal one if student's mother has at least some college, expected family contribution is $0, and student planned to live in campus housing; indicators for course status as developmental, the term, and USG institution type; and high school GPA and student age, each at level and squared.

Table 3.5: Comparison of first-stage instrument strength using the Deviance Information Criterion

	Withdraw		Pass	
Full sample				
DIC	53396.88	52242.08	48301.77	47589.14
Broadband measures included		X		X
Subsample				
DIC	22918.78	22648.21	21075.79	20895.04
Broadband measures included		X		X

Notes. Outcome in all models is a 0/1 indicator of whether the course is online. All models use the same covariates as included in the logistic and bivariate probit models. The DIC values were computed using 4,000 draws that were combined from four separate chains.

Table 3.6: Bivariate probit regression with withdraw/pass as second level outcome and endogenous online indicator as first level outcome

	Withdraw	Pass
Full sample		
β_{online}	0.16	-0.201
	[0.042, 0.278]	[-0.337, -0.065]
ρ	0.02	-0.019
	[0.002, 0.043]	[-0.04, 0.002]
ATE	0.028	-0.054
	[0.006, 0.053]	[-0.096, -0.016]
TOT	0.03	-0.056
	[0.007, 0.055]	[-0.097, -0.017]
LATE	0.029	-0.056
	[0.007, 0.054]	[-0.097, -0.017]
N	766933	719443
Subsample		
β_{online}	0.133	-0.064
	[0.034, 0.24]	[-0.174, 0.046]
ρ	0.028	-0.026
	[-0.011, 0.065]	[-0.064, 0.012]
ATE	0.025	-0.015
	[0.006, 0.048]	[-0.044, 0.01]
TOT	0.026	-0.016
	[0.007, 0.047]	[-0.044, 0.011]
LATE	0.024	-0.017
	[0.006, 0.046]	[-0.045, 0.008]
N	20064	18774

Notes. Bayesian point estimates are the mean values of 4000 draws from the posterior distribution with 95% credible intervals in square brackets. The subsample reflect courses taken by students who enrolled in both online and face-to-face courses in the same term. The pass outcome is conditional on not withdrawing from the course. Second level includes indicator for online status. First level includes five exogenous instruments: download speed, at level and squared, upload speed, at level and squared, and interaction between download and upload speed. All broadband measures are lagged by one term. Covariates included in all models, but not shown: indicators for student's gender, race/ethnicity, military status, and receipt of financial aid; indicators that equal one if student's mother has at least some college, expected family contribution is $0, and student planned to live in campus housing; indicators for course status as developmental, the term, and USG institution type; and high school GPA and student age, each at level and squared.

Table 3.7: Logistic regression of median zipcode income on measures of broadband speeds

	Contemporaneous
Download	0.192
	[-0.631, 1.036]
Download^2	0.008
	[-0.075, 0.094]
Upload	0.409
	[-0.153, 0.967]
Upload^2	0.041
	[-0.032, 0.113]
Download $\times$ Upload	-0.078
	[-0.213, 0.057]
Intercept	8.657
	[6.395, 10.959]
N	707

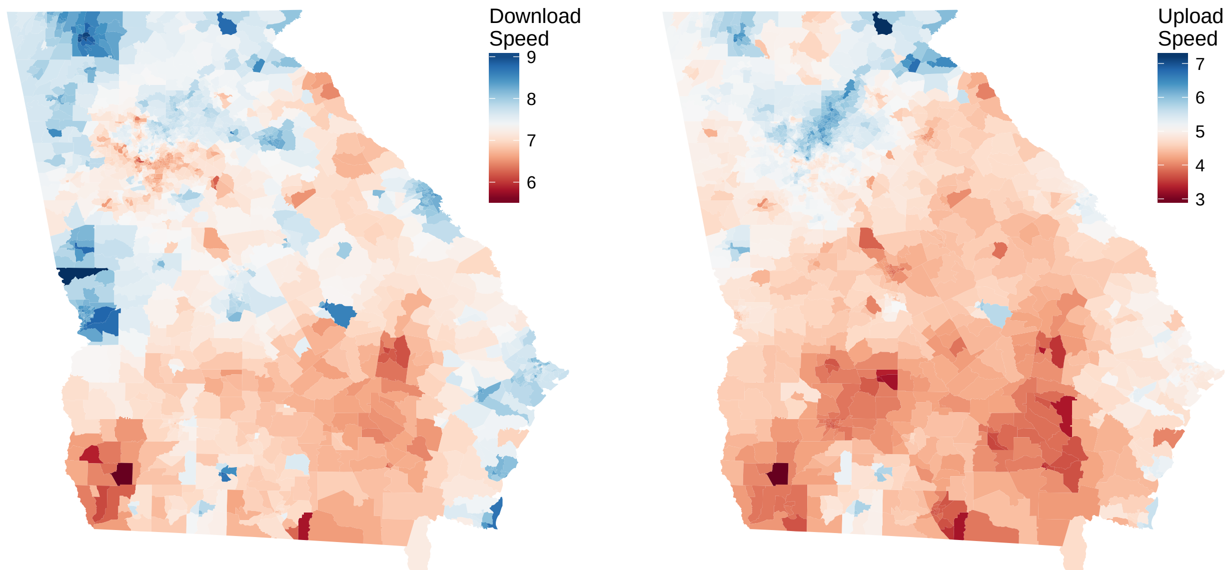
Notes. Bayesian point estimates are the mean values of 4000 draws from the posterior distribution with 95% credible intervals in square brackets. Median income estimates for zip code tabulation areas (ZCTAs) are 5 year rolling estimates from the American Community Survey. Broadband measures were those published in December 2013 and inverse-distance weighted to each ZCTA center in the same manner as the primary analyses.

Table 3.8: Results from 2SLS regression of withdraw/pass on endogenous indicator for online delivery and covariates with lagged broadband excluded instruments

	Withdraw		Pass	
Online_{2SLS}	0.88		-6.06	
	(0.41)		(1.073)	
Endogeneity: Wu-Hausman (F)	4.56	$p = 0.033$	100.44	$p < 0.000$
Instrument Strength: F	12.25	$p < 0.000$	10.98	$p < 0.000$
Overidentification: score (χ^2)	8.39	$p = 0.078$	8.67	$p = 0.070$

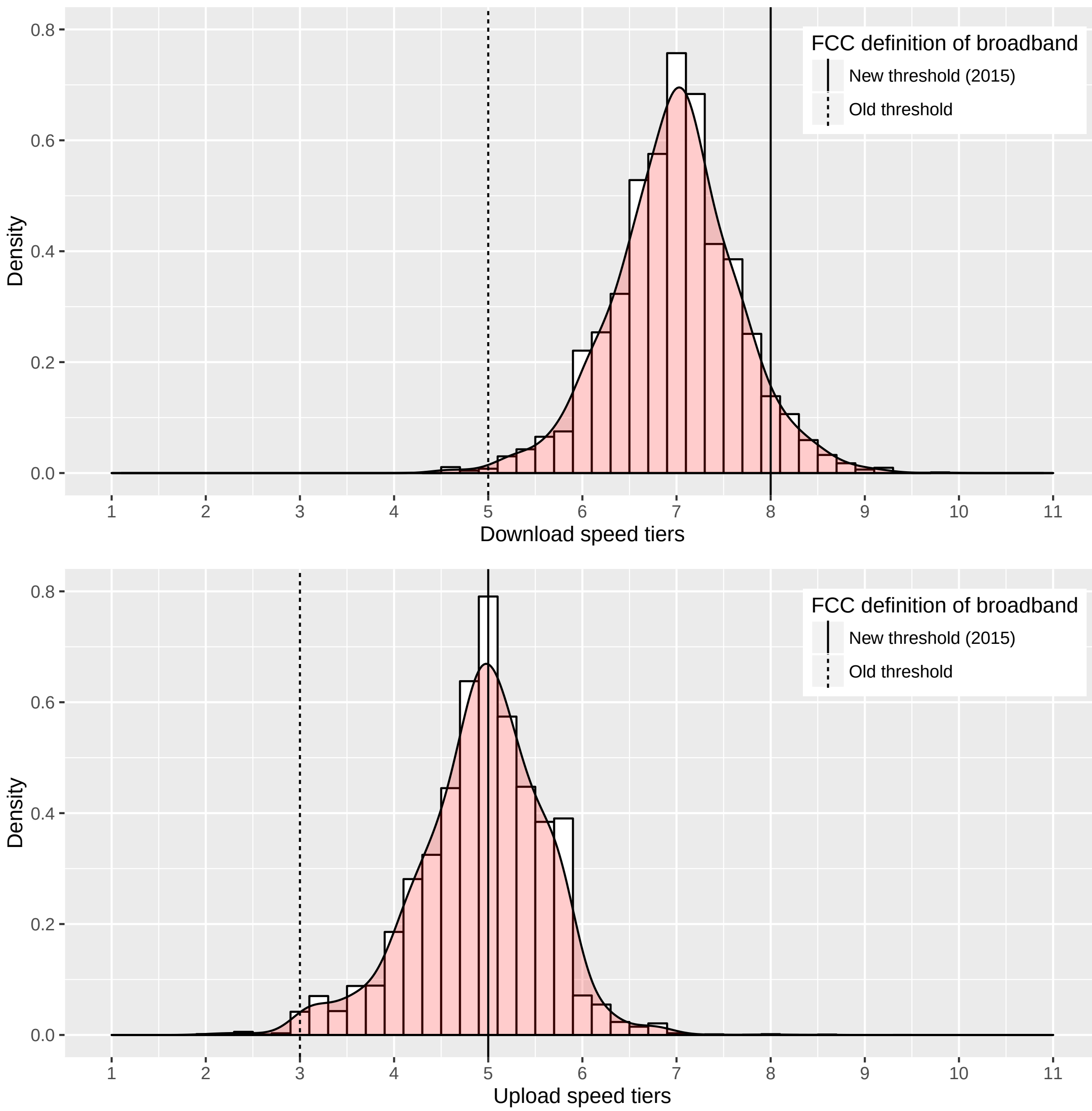
Notes. Robust standard errors in parentheses. Covariates included in all models, but not shown: indicators for student's gender, race/ethnicity, military status, and receipt of financial aid; indicators that equal one if student's mother has at least some college, expected family contribution is $0, and student planned to live in campus housing; indicators for course status as developmental, the term, and USG institution type; and high school GPA and student age, each at level and squared. Excluded instruments are: download speed, at level and squared, upload speed, at level and squared, and interaction between download and upload speed.

Figure 3.1: Census tract-level differences in average broadband speeds in Georgia, June 2013



Census tract values computed by author using data taken from the National Broadband Map, which gives multiple readings at the census block level. Speeds represent the inverse distance-weighted average of measures taken around each census tract's centroid. Speeds are given in ordered categories from 1 to 11. White values for each map are approximately located at the FCC's recent threshold of broadband (roughly tier 8 for download speeds and tier 5 for upload speeds). For each map, red hues represent averages below the threshold and blue hues for those above.

Figure 3.2: Distribution of student download and upload speeds across the sample



Students are assigned a download and upload speed based on the zip code of their listed address. Speeds are assigned to each zip code based on the inverse distance weighted-average of surrounding download speed measures that are taken from the National Broadband Map. For each plot, the dotted vertical line shows the broadband designation threshold prior to 2015; the solid vertical line shows the new definition of the threshold.

3.A Bivariate probit Stan script

```
/*
 * <PROJ> Dissertation: RQ2
 * <FILE> biprobit_vec.stan
 * <AUTH> Benjamin Skinner
 * <INIT> Fall 2016
 *
 * NOTES
 *
 * Based on code by:
 *
 * Bob Carpenter @ Stan
 * <web> http://tinyurl.com/horbxuu
 * Hollenbach, Montgomery, Crespo-Tenorio (2016)
 * <web> https://pages.wustl.edu/montgomery/articles/25282
 *
 */

functions {
  int sum(int[,] a)
  {
    int s;
    s = 0;
    for (i in 1:size(a))
      for (j in 1:size(a[i]))
        s = s + a[i,j];
    return s;
  }
}
data {
  int<lower=0> N; // # of observations in dataset
  int<lower=1> D; // # of equations (whatever as long as D == 2)
  int<lower=1> J; // # of instruments
  int<lower=1> K; // # of covariates (excluding _constant)
  int<lower=0, upper=1> y[N,D]; // 1st and 2nd stage outcome
  matrix[N,K] x;  // covariates (excluding column of 1s)
  matrix[N,J] z;  // exogeneous instruments
}
transformed data {
  vector[N] t;
  int<lower=0> N_pos;
  int<lower=1,upper=N> n_pos[sum(y)];
  int<lower=1,upper=D> d_pos[size(n_pos)];
  int<lower=0> N_neg;
  int<lower=1,upper=N> n_neg[(N * D) - size(n_pos)];
  int<lower=1,upper=D> d_neg[size(n_neg)];
```

```
  // function to sort 0/1s into -/+ vectors per Albert & Chib (1993)
  N_pos = size(n_pos);
  N_neg = size(n_neg);
  {
    int i;
    int j;
    i = 1;
    j = 1;
    for (n in 1:N) {
      for (d in 1:D) {
        if (y[n,d] == 1) {
          n_pos[i] = n;
          d_pos[i] = d;
          i = i + 1;
        } else {
          n_neg[j] = n;
          d_neg[j] = d;
          j = j + 1;
}
      }
    }
  }
  // peel off first stage outcome (treatment) into vector
  t = col(to_matrix(y), 1);
}
parameters {
  // parameters
  real alpha1;
  real alpha2;
  vector[K] gamma1;
  vector[K] gamma2;
  vector[J] psi;
  real beta;

  // using cholesky factor rather than full correlation matrix
  cholesky_factor_corr[D] L_Omega;

  // placing constraints on -/+ latent outcome parameters
  vector<lower=0>[N_pos] l_pos;
  vector<upper=0>[N_neg] l_neg;
}
model {

  // temporary vectors (non-monitored) to hold fitted RHS values
  vector[N] mu_1;
  vector[N] mu_2;
  vector[D] mu[N];
```

```
  // latent (y_star) parameters to be fit
  vector[D] l[N];

  // latent vector reassembled in order,
  // but w/ proper constraints as set above
  for (n in 1:N_pos)
    l[n_pos[n], d_pos[n]] = l_pos[n];
  for (n in 1:N_neg)
    l[n_neg[n], d_neg[n]] = l_neg[n];

  // using vectorization to compute first/second stages
  mu_1 = z * psi + x * gamma1 + alpha1;
  mu_2 = t * beta + x * gamma2 + alpha2;

  // b/c sampling statement below requires certain data types/shapes,
  // must reassemble matrices into vector[] type
  /*
   *         | mu_fs_1 |         | mu_ss_1 |           |/ mu_fs_1 \|
   *  mu_1 = | mu_fs_2 |  mu_2 = | mu_ss_2 |    mu    = |\ mu_ss_1 /|
   * (1 X K) |   ...   | (1 X K) |   ...   | (2 X K)   |/ mu_fs_2 \|
   *         | mu_fs_n |         | mu_ss_n |  X N      |\ mu_ss_2 /|
   *                                                   |    ...    |
   *                                                   |/ mu_fs_n \|
   *                                                   |\ mu_ss_n /|
   */
  for (i in 1:N) {
    mu[i,1] = mu_1[i];
    mu[i,2] = mu_2[i];
  }

  // priors
  alpha1 ~ normal(0,2.5);
  alpha2 ~ normal(0,2.5);
  gamma1 ~ normal(0,2.5);
  gamma2 ~ normal(0,2.5);
  psi    ~ normal(0,2.5);
  beta   ~ normal(0,2.5);
  L_Omega ~ lkj_corr_cholesky(D);

  // likelihood function
  l ~ multi_normal_cholesky(mu, L_Omega);

}
```

3.B Formulas for treatment effects

The formulas below closely follow those presented in Hollenbach et al. (2016) and Greene (2012). Note that Φ represents the cumulative distribution function of the standard normal distribution, and $\mathbf{\Phi}^{(2)}$ represents the cumulative distribution function of the standard bivariate normal distribution.

3.B.1 Average treatment effect (ATE)

$$ATE = \frac{\sum_{i=1}^{N} \Delta_{ATE_i}}{N} \tag{3.14}$$

where,

$$\Delta_{ATE_i} = \mathbf{\Phi}(\mathbf{X}_i\gamma_2 + \beta) - \mathbf{\Phi}(\mathbf{X}_i\gamma_2) \tag{3.15}$$

3.B.2 Treatment on the treated (TOT)

$$TOT = \frac{\sum_{i=1}^{N} \Delta_{TOT_i}}{N} \tag{3.16}$$

where,

$$\Delta_{TOT_i} = \frac{\mathbf{\Phi}^{(2)}\left(\begin{pmatrix}\mathbf{X}_i\gamma_1 + \mathbf{Z}\psi \\ \mathbf{X}_i\gamma_2 + \beta\end{pmatrix}, \Omega\right) - \mathbf{\Phi}^{(2)}\left(\begin{pmatrix}\mathbf{X}_i\gamma_1 + \mathbf{Z}\psi \\ \mathbf{X}_i\gamma_2\end{pmatrix}, \Omega\right)}{\mathbf{\Phi}(\mathbf{X}_i\gamma_1 + \mathbf{Z}\psi)} \tag{3.17}$$

3.B.3 Local average treatment effect (LATE)

$$LATE = \frac{\sum_{i=1}^{N} \Delta_{LATE_i}}{N} \tag{3.18}$$

where,

$$\Delta_{LATE_i} = \frac{M - O}{\mathbf{\Phi}(\mathbf{X}_i\gamma_1 + \mathbf{Z}^*\psi) - \mathbf{\Phi}(\mathbf{X}_i\gamma_1 + \mathbf{Z}\psi)} \tag{3.19}$$

$$M = \boldsymbol{\Phi}^{(2)}\left(\begin{pmatrix}\mathbf{X}_i\gamma_1 + \mathbf{Z}^*\psi \\ \mathbf{X}_i\gamma_2 + \beta\end{pmatrix}, \Omega\right) + \boldsymbol{\Phi}^{(2)}\left(\begin{pmatrix}-(\mathbf{X}_i\gamma_1 + \mathbf{Z}^*\psi) \\ \mathbf{X}_i\gamma_2\end{pmatrix}, -\Omega\right) \tag{3.20}$$

$$O = \boldsymbol{\Phi}^{(2)}\left(\begin{pmatrix}\mathbf{X}_i\gamma_1 + \mathbf{Z}\psi \\ \mathbf{X}_i\gamma_2 + \beta\end{pmatrix}, \Omega\right) + \boldsymbol{\Phi}^{(2)}\left(\begin{pmatrix}-(\mathbf{X}_i\gamma_1 + \mathbf{Z}\psi) \\ \mathbf{X}_i\gamma_2\end{pmatrix}, -\Omega\right) \tag{3.21}$$

$$-\Omega = \begin{bmatrix}1 & -\rho \\ -\rho & 1\end{bmatrix}, \mathbf{Z}^* = \mathbf{Z} + 1 \,\text{tier increase in down/upload speeds} \tag{3.22}$$

Chapter 4

Predicting Degree Outcomes of Online Course Enrollment at the State Level using Bayesian Multilevel Regression and Poststratification

4.1 Introduction

To support educational research and policy-making, the federal government makes available a number of data sources. Many of these represent surveys that provide detailed longitudinal information on panels of students and, due to their design, allow for inferences at the national level. Their sampling methods, however, seldom allow for inferences at the state or sub-state level, where many educational policies are set. At the state-level, a number of states have developed rich administrative databases. Some of these databases are able to follow students from pre-Kindergarten through college and even beyond into the work force. Yet these data sources are not without their own limitations, particularly in their general inability to track students who leave the public school system or the state. Administrative systems are also expensive and their use often restricted, leaving many education researchers interested in sub-national policy limited to studying outcomes in a few states.

This chapter proposes a solution to this fundamental inferential problem in education policy research that is borrowed from political science. Bayesian multilevel regression with poststratification (BMRP) is two-step statistical procedure often used by political scientists to make state-specific inferences about election outcomes or public opinion using nationally or even non-representative data. It is well-described in the political science literature (Gelman & Hill, 2007; Gelman & Little, 1997; Lax & Phillips, 2009; Little, 1993; Pacheco, 2011; Park et al., 2004; Wang et al., 2015; Warshaw & Rodden, 2012), but has yet to find widespread application among researchers in the field of education. Because conducting representative surveys can be both expensive and time consuming, I argue that education policy researchers and policymakers would be well served by using BMRP and the rich nationally representative survey data already at their disposal to investigate sub-national questions of interest.

In this chapter I first explain the method and its general application. I next apply it to a current educational question of interest: degree outcomes among postsecondary students who take online courses compared to those who do not. Increasing numbers of students choose to take online courses (Allen &

Seaman, 2011; U.S. Department of Education & National Center for Education Statistics, 2016), but the literature remains split on the effect of course delivery on student outcomes (Bowen et al., 2012; Figlio et al., 2010; Joyce et al., 2014; Xu & Jaggars, 2011, 2013). In chapter 3, I find that students in online courses are more likely to withdraw and less likely to pass the course. But these outcomes, like those offered in other papers on the topic, speak only to a specific state context and do not consider longer-term outcomes.

Using student-level data from the Beginning Postsecondary Students Longitudinal Study, 2004/2009, (BPS) and population data from the American Community Survey (ACS), I produce state-level estimates of the difference in degree attainment between students who took no online courses and those who took at least one such course. With ACS population data covering the period of the BPS survey, I am able to produce estimates of degree attainment that are now historical (for 2009) and that can be validated against external sources as a test of the method. With more recent ACS data that represent population counts in 2015, I am able to forecast differences in bachelor's degree attainment between online and non-online students for the year 2021.

The results from this chapter, while not causal, offer estimated degree attainment outcomes for each state and the District of Columbia that are unavailable from other sources. They may be used to make state-by-state comparisons as well as inform state-specific policy as it regards the use of online education to increase postsecondary access and meet degree attainment goals. As an example of the latter, I discuss my results in the context of Tennessee, a state that has established degree attainment goals that include expansion of online educational course offerings and therefore provides a useful case study. For other researchers not focused on issues of online learning in higher education, I present these results as an example of what is possible with BMRP, and conclude by discussing how the procedure may be usefully employed in education research more generally.

4.2 Bayesian multilevel regression with poststratification

4.2.1 Procedure

Various procedures for post-stratifying survey responses—described in varying degrees of detail—may be found in the statistical and political science literature (Gelman & Hill, 2007; Gelman & Little, 1997; Lax & Phillips, 2009; Little, 1993; Pacheco, 2011; Park et al., 2004; Wang et al., 2015; Warshaw & Rodden, 2012), with the primary difference across particular methods being in the estimation technique used to fit survey-level response parameters. In this chapter, I follow most closely to the fully Bayesian approach outlined by Park et al. (2004). I outline my particular approach below.

To produce state-level estimates using survey data that are not representative at the state level, two data sets are required. The first is a primary data set that contains binary outcomes of interest linked to individuals with known demographic characteristics and state of residence. Demographic characteristics may include race/ethnicity, gender, age, income, and/or education level. An outcome of interest may be bachelor's degree attainment within six years of matriculation, such as is used in this study. The second data set is a poststratification data set, usually produced using census data, that contains population counts for matching demographic cells by state. To improve model fit and poststratification results, state-level characteristics can be added in the second level (Park et al., 2004).

The primary steps of the procedure are as follows:

1. Using the primary survey data set, model the outcome, y_i, as a Bayesian multilevel logistic regression in which demographics and state of residence are placed on the right-hand side. For these analyses, I use indicators for low income status (== 1 $<$ \$25,000/year), gender, race/ethnicity (Black, Hispanic, other, [White omitted]), age category (15 to 17, 20 to 29, 30 to 49, 50 and over [18 to 19 omitted]), and interactions between gender and income, gender and race/ethnicity, and income and race/ethnicity. I also include state-level characteristics in the second level. This leads to a total of 80 unique demographic cells, j, within each state, s. With 50 states plus the District of Columbia, this model allows for 4,080 unique predictions across the country.

2. Compute predicted probabilities for each demographic cell in each state, π_j. Because demographic parameters do not vary across the states, predicted values for groups that are not represented in the original survey data may nonetheless be produced. For example, even if the primary survey data source has no low income Hispanic women respondents between the ages of 20 and 29 who live in Kentucky, a predicted probability for this group can be computed using the regression parameters and the Kentucky state-specific intercept. For states with no observations (and therefore no estimated intercept), the state-level grand mean may be used. To make sure that all demographic cells are included in each state, a design matrix of 0s and 1s covering all categories should be used when making the predictions rather than the original model matrix, which is likely to have missing categories within most states. If state-level covariates are used to improve model fit, these must be included in the de-

sign matrix (repeating across demographic cells within each state) and aligned with their respective parameters.

3. Within each state, s, sum the predicted probabilities using the population counts, N_j, for each respective demographic cell, j, as weights to produce the post-stratified state-level outcome probability, θ_s:

$$\theta_s = \frac{\sum_{j \in s} N_j \pi_j}{\sum_{j \in s} N_j}. \tag{4.1}$$

Using this procedure, representative state-level estimates may be recovered from survey data that alone are only nationally representative. In some cases, even non-representative survey data may still produce reasonably representative estimates once post-stratified (Wang et al., 2015). With this technique and an extension discussed in the following subsection, I am able to show state-level differences in degree attainment between students who take no online courses and those who take at least one using panel data that is only representative at the national level.

Broadly, the multilevel logistic regression may be estimated in one of two ways. In many papers on the topic, authors have estimated the models using maximum likelihood algorithms (Gelman & Hill, 2007; Lax & Phillips, 2009). Other authors have employed a fully Bayesian approach (Park et al., 2004). I choose the latter Bayesian approach for two key reasons. First, modern computing power and improved probabilistic programming languages such as `Stan` (Stan Development Team, 2016) have made Bayesian analysis more practicable. Second, Bayesian analyses produce full posterior distributions that directly reflect uncertainty in their standard deviations, with less certain predictions having a larger range of plausible values (Gelman et al., 2014; Jackman, 2009). Rather than produce state-specific point estimates of bachelor's degree attainment with some accompanying measure of error, I am able to show distributions of plausible attainment levels in each state. With more certain predictions, the difference between the 5% and 95% percentiles become smaller; with less certain estimates, larger. These posterior distributions have the benefit of being directly interpreted as statements, for example, of the probable proportions of first time students in the state who will attain a bachelor's degree within six years of starting college.

The key limitation of BMRP is the requirement that demographic cells used to fit the primary response model (step (1) above) must be available in the poststratification data set. Covariates that might help predict eventual bachelor's degree attainment such as parental education level cannot be used because they are not available at the same level of demographic detail in the poststratification data set. BMRP also assumes that once state-level effects have been accounted for using varying intercepts and/or state-level covariates, outcomes are stable within demographic cells across the states. In other words, once state-specific contexts are taken into account, non-low income White males in their twenties who live in Wyoming are assumed to have generally the same posterior probability of bachelor's degree attainment conditional on online course-taking behavior as their demographic counterparts who live in Idaho, Iowa, and Illinois.[1] State-level covariates which account for state contexts, therefore, may be a very important determinant of BMRP's ability to make accurate predictions (Park et al., 2004).

4.2.2 Extension

The BMRP procedure can be extended to account for multi-stage outcomes by fitting a series of nested equations. Park et al. (2004) describe a situation in which the state-level Republican vote share in the 1992 presidential election is first a function of a voter's probability of voting, then his or her probability of voting for one of the two major parties, and finally the probability of voting Republican. Using this extension of the procedure, I am able to account for online course-taking behavior on the probability of degree attainment, even though the poststratification data set does not have an the indicator for taking online courses. I can also account for the probability that a student will attend school in state, which is likely to be of interest to state-level policymakers who wish to bolster aggregate degree attainment among their own residents.

I fit the multi-stage nested BMRP model in the following way. First, I model the probability that students will attend school in the same state as their primary residence, π^1. Next, I subset the sample to those students who attend in-state institutions, and model the probability that they will take at least one online course, π^{2a}, and the opposite, the probability that they will take no online courses, π^{2b}, giving two sets of results. Finally, splitting the second data set of in-state attendees into those who took at least one online course (a) and those who took no online courses (b), I calculate the probability of degree attainment, π^3 for each. These three outcomes are then multiplied together and weighted with population cell sizes,

$$\theta_s^g = \frac{\sum_{j \in s} N_j \pi_j^1 \pi_j^{2(a|b)} \pi_j^3}{\sum_{j \in s} N_j \pi_j^1 \pi_j^{2(a|b)}}, \tag{4.2}$$

[1] In political science, it might be the same posterior probability for voting Republican or supporting same-sex marriage.

to produce θ_s^g, where $g \in \{a, b\}$. For each degree attainment outcome, I end up with two distinct post-stratified posterior predictions, one each for online and never-online student populations.

Because the BMRP procedure requires binary outcomes, I split students into only two groups based on their online course-taking behavior. This is despite the fact that BPS 2004/2009 includes counts of the number of online courses that students reported taking. I separate the groups into never-online (0 courses) and any-online (1+ courses) groups for two reasons. First, a relatively small proportion of sample students took any online courses during this period. Of those who did, most took less than a full-time semester's worth of online courses (4). Breaking the students into groups based on a higher number of online courses taken (*e.g.,* code 1 for students who took 4 or more courses and code 0 for everyone else) reduced the number of positive responses and made the post-stratified results less certain. Setting the classification rule at any online courses, 0/1+, gave the best split between the groups and improved the BMRP results. Second, drawing the classification line between none and any online courses represents a more logical break than between two positive numbers, especially if zero is included in the smaller group. These rationales notwithstanding, students who take only one or two online courses may be different from those who take many more. As constructed, the BMRP procedure that I employ does not speak to predicted outcomes based on dosage.

The final estimate directly reflects both the multiple decision points and the corresponding uncertainty around them through the spread of the posterior predictions, θ_s^g. Using the nested logistic models, the resulting post-stratified posterior distributions represent the probability, for example, that in-state students who choose to take some of their courses online will earn a bachelor's degree within six years of their initial enrollment. These probabilities of attainment are different from those directly estimated by the non-nested model and more relevant for state education officials seeking to improve postsecondary pathways for their own residents.[2] Finally, because each θ_s^g is a full Bayesian posterior distribution, estimating the difference in degree outcome probability between the groups is as straightforward as subtracting one set of values from

[2] I note here that while I can observe whether students attend school in-state and whether they take any online courses, I cannot say definitively whether students who take online courses take them in-state. BPS 2004/2009 does not provide these indicators at the transcript level, so it is possible that the student (1) took online courses at another institution out-of-state, (2) took online courses at an institution that counted as in-state but was not physically located in the same state (*e.g.,* branch campus of a national institution), or (3) attended their in-state school online while living out-of-state. My results, therefore, compare in-state students who take no online courses to those who take any, some or all of which may be out-of-state.

the other, producing a new posterior distribution of the potential range of differences:

$$Pr(\theta_s^{difference}) = Pr(\theta_s^{online}) - Pr(\theta_s^{no\,online}). \tag{4.3}$$

4.3 Model

For this chapter, I estimate both the single poststratification model and the nested model. With the first, I predict state-level bachelor's degree attainment within six years of matriculation and compare the results to published estimates for 2009. This serves as a validation of the BMRP procedure and covariate selection. I next fit three intermediate outcomes and four degree outcomes models. These are:

- (**Intermediate 1**) Attend in-state institution
- (**Intermediate 2a**) Take at least one online course | attending in-state institution
- (**Intermediate 2b**) Take no online courses | attending in-state institution
- Bachelor's degree attainment within six years | online group (g), in-state
- Any degree attainment within six years | online group (g), in-state
- Associate degree attainment within three years | online group (g), in-state
- Any degree attainment within three years | online group (g), in-state

Again, the four primary outcomes, y_i, are estimated for two groups each: in-state attendees who took no online courses and in-state attendees who took at least one online course. In total, I perform eleven separate estimations to produce all of the nested model results.

Each outcome, y_i, is fit using the same multilevel logistic regression model:

$$\begin{aligned} Pr(y_i = 1) = logit^{-1}(&\beta^{LowIncome} \cdot LowIncome_i + \beta^{Female} \cdot Female_i \\ &+ \beta^{Female \cdot LowIncome} \cdot Female \cdot LowIncome_i + \beta^{Black} \cdot Black_i \\ &+ \beta^{Hispanic} \cdot Hispanic_i + \beta^{OtherRace} \cdot OtherRace_i + \beta^{age}_{age_i} \cdot Age_i^{cat} \\ &+ \beta^{LowIncome \cdot Race}_{LowIncome_i, Race_i} \cdot (LowIncome \times Race) + \beta^{Female \cdot Race}_{Female_i, Race_i} \cdot (Female \times Race) \\ &+ \beta^{state}_{state_i}) \end{aligned} \tag{4.4}$$

The model matrix, **X**, contains 0s and 1s that indicate each survey respondent's demographic cell. The primary categories are low income status (< \$25,000 a year), gender, race/ethnicity (Black, Hispanic, and other race/ethnicity), age category, and interactions between low income, gender, and race/ethnicity. Age_i^{cat} includes four categories of age: 15 to 17, 20 to 29, 30 to 49, and 50+, with 18 to 19 as the omitted category. The general omitted demographic category within each state are non-low income White males between 18 and 19 years of age. Each respondent is also given an indicator for his or her state of residence, which includes the District of Columbia for purposes of estimation.

The estimated models also include a number of state-level covariates:

$$\begin{aligned}\beta_j^{state} \sim \mathcal{N}(\gamma_j^0 + \gamma^{Unemployment\,Rate} \cdot Unemployment\,Rate + \gamma^{Median\,Income} \cdot Median\,Income \\ + \gamma^{UGEnrollment} \cdot UGEnrollment + \gamma^{Public4yrCost} \cdot Public4yrCost \\ + \gamma^{Ed.Attain}_{Ed.Attain_k} \cdot Ed.Attain^k + \gamma^{Con.Effect}_{Con.Effect_k} \cdot Con.Effect^k \\ + \gamma^{Popper4yr} \cdot Pop.per4yr + \gamma^{Popper2yr} \cdot Pop.per2yr,\, \sigma^2_{state}), \text{for } j = 1, \ldots, 51\end{aligned} \tag{4.5}$$

These covariates include: the state unemployment rate and median income; fall undergraduate enrollment and public four-year university cost (tuition plus fees) for 2004; the relative percentages the adult population in the state with a high school diploma or equivalent, some college, associate degree, and bachelor's degree; and the population per four-year and two-year institution. These last two measures are meant as an approximation of access to postsecondary institutions in the state and were computed by dividing the number of people over 18 years old in the state by the number of postsecondary institutions at that level in the state.

Each state has its own intercept, γ_j^0, which is modeled hierarchically. This allows for the partial pooling of the data in which information from states with more observations can "flow" to those with comparatively fewer and help improve predictions (Carlin & Louis, 2009; Gelman et al., 2014; Jackman, 2009). Many studies that use (B)MRP to model public opinion choose to model state intercepts as functions their respective census regions, which are in turn given a hyper prior (*e.g.,* Gelman & Little, 1997; Lax & Phillips, 2009; Park et al., 2004). While the separation of states into their respective regions are likely to apply to many educational outcomes of interest, they were not particularly informative in the context of these particular analyses. Therefore, I model each state intercept as begin normally distributed with a single location parameter, $\mu_{country}$, and variance, $\sigma^2_{country}$. The variance parameters are given truncated standard normal priors that are bounded to be positive and $\mu_{country}$ a standard normal prior. In the context of logistic regression, none of these priors is particularly informative:

$$\gamma_j^0 \sim \mathcal{N}(\mu_{country}, \sigma^2_{country}), \tag{4.6}$$

$$\mu_{country} \sim \mathcal{N}(0,1), \tag{4.7}$$

$$\sigma^2_{state} \sim \mathcal{N}(0,1) \in (0,\infty), \tag{4.8}$$

$$\sigma^2_{country} \sim \mathcal{N}(0,1) \in (0,\infty). \tag{4.9}$$

The models also include contextual effects, which are the state-level averages of demographic cells. These include percentages of state residents who are low-income, women, Black, Hispanic, and other race/ethnicity (non-White, non-Black, and non-Hispanic). Contextual effects are important as models with random or varying intercepts can violate Gauss-Markov assumptions and underestimate variance if the varying intercepts correlate with the unit-level observed values. By including state-level averages of the demographic cells, however, the correlation between the two is removed, and the resulting estimates are free of this potential source of bias (Bafumi & Gelman, 2006).

To estimate the model, I use the `Stan` probabilistic modeling language (Stan Development Team, 2016) and its No-U-Turn Sampler (NUTS), a variant of the Hamiltonian Monte Carlo (HMC) sampling algorithm (M. D. Hoffman & Gelman, 2014). To speed convergence, I modified the data and BMRP model slightly. First, I aggregated outcomes, y_i, within each demographic cell in each state as well as the total number of observations in that cell. For example, if there were a state with 10 non-low income Black male respondents between the ages of 20 and 29, 8 of whom attended college in-state, then I aggregated that group to a single row in the data set (**X**) and add two columns: total (10) and positive outcome (8). This process transformed the individual yes/no responses, which follow a Bernoulli distribution, into a Binomial distribution of "successes" out of "trials" with no loss of generality. The result is a smaller model matrix and decreased computation time (Stan Development Team, 2016).

Second, I used a QR reparameterization of the data. Briefly, the model matrix can be decomposed into two parts, $\mathbf{X} = QR$, where Q is an orthogonal matrix of size $n \times k$, R is an upper right-triangle matrix of size $k \times k$, and n is the number of rows and k the number of columns in $\mathbf{X}$ (Eubank & Kupresanin, 2012). Rather than fit β and γ parameters directly to $\mathbf{X}$, I instead fit ϑ parameters to Q^*, which is Q scaled by the size of the data: $Q \times \sqrt{n-1}$. To recover β and γ, I multiplied the posterior of ϑ by the inverse of the scaled R^*: $R/\sqrt{n-1}$. Using the QR reparameterization improves the efficiency of the NUTS sampler primarily due to the fact that columns of Q^* are orthogonal and on the same scale, neither of which is true for $\mathbf{X}$ in general nor for these models (Stan Development Team, 2016). All ϑ parameters were given unbounded improper priors. Improper priors may still result in proper posterior distributions and in the context of the `Stan` modeling language are acceptable provided the model converges (Gelman et al., 2014).

Finally, I standardized all state-specific covariates. Because the demographic cells range only from 0 to 1, the values representing average median income and undergraduate enrollment have a much larger scale. Standardizing the state-level covariates removes this difference of scale and supports convergence. Because BMRP is mostly concerned with model predictions rather than individual parameter values, the accompanying change in interpretation for these parameters is relatively unimportant. The main consideration is that when post-stratifying the responses, the state-level covariates must once again be standardized so that the proper predictions are computed.

Following standard practice, I fit multiple (4) separate chains of 2000 iterations each, the first 1000 of which were discarded as burn-in. Each posterior distribution therefore contains 4000 samples. Using the `ShinyStan` R package, I visually inspected plots of the chains for proper mixing and parameter density plots for normality. I also checked the $\hat{R}$ statistic, which compares the ratio of between and within chain variance and should be less than 1.1. All models presented in this chapter converged with good chain mixing and $\hat{R} < 1.1$. Similarly, posterior parameter densities were normally shaped, again suggesting good model fit.[3]

4.4 Data

4.4.1 Beginning Postsecondary Students Longitudinal Study, 2004/2009

Student-level data used to construct the primary data set come from the Beginning Postsecondary Students Longitudinal Study, 2004/2009, (BPS), a nationally-representative survey of first-time postsecondary students. This BPS cohort contains around 16,700 students and was drawn from those participating in the National Postsecondary Student Aid Study (NPSAS), itself a nationally representative sample of students. The BPS participants were first interviewed at the end their first year in college in the spring 2004. They were interviewed again three and six years later, in 2006 and 2009. Student demographic characteristics

[3] In some models, the Stan sampler reported a few divergent transitions, meaning that the sampler may have had trouble exploring parts of the posterior distribution, which may lead to biased estimates (Stan Development Team, 2016). Tuning a few parameters, specifically the *adapt_delta* and *stepsize* parameters reduced the number divergences to fewer than 5 or eliminated them entirely.

(income, gender, race/ethnicity, and age) that can be post-stratified upon as well as degree outcomes are contained in the data set.

Importantly for this study, BPS has an indicator for whether a student attended a postsecondary institution in the state of their primary residence. The survey also has information on the number of distance education courses that the student took. While these indicators are not linked to specific course outcomes, they do indicate the number of distance courses that were specifically online. Because comparatively few students took any courses online, I code each student as being an online student if s/he took any online courses during the survey period. Such students should not be understood, therefore, as primarily online degree seekers. Many took only one or two online courses and the rest in a face-to-face format during the survey window.

4.4.2 American Community Survey

Posterior parameter estimates were post-stratified using population cell sizes, N_j, taken from the American Community Survey (ACS) Public Use Micro Sample (PUMS). Earlier studies have used population weights from the decennial census, either 1990 or 2000 depending on the time of the analysis. With ACS yearly estimates, which began in 2004, more timely population counts may be used. Though the sample sizes in the ACS single year estimates are smaller than the 1% or 5% sample sizes in the decennial census, they are generally sufficient at the state level to return values for most demographic cells. For the primary analyses, I post-stratified using cell counts computed from the 2004 ACS estimates. For forecasting models, I used ACS 2015 single year estimates to compute cell counts.

4.4.3 State-level predictors: miscellaneous sources

Contextual effects at the state-level were computed using the BPS respondent values. State unemployment rates and median income come from tables provided by the Bureau of Labor Statistics (Bureau of Labor Statistics, 2004). Undergraduate enrollment totals and public four-year university tuition and fees come from the Integrated Postsecondary Education Data System (IPEDS) (Department of Education, 2015). The proportion of the population over 18 in each state with various levels of educational attainment—high school diploma or GED, some college, associate degree, and bachelor's degree—were taken from ACS summary tables. The population per four- and two-year postsecondary institution were computed using institutional counts and population totals from IPEDS and ACS, respectively. Most ACS values come from 2004 except educational attainment, which are not available for that year. These measures come from 2005. In forecasting models, all values come from 2015.

4.5 Results

4.5.1 Validation

As a validation of the BMRP method, I first predict state-specific bachelor's degree attainment within six years (150% time) unconditional of attending college in-state or whether the student took any online courses. The non-varying model parameters, β and γ, are shown in Figure 4.1. Though earlier studies of public opinion that use BMRP have often found first level demographic characteristics to be somewhat uninformative (*e.g.,* Park et al., 2004), demographic characteristics are much stronger predictors of eventual bachelor's degree attainment in my model. It is important to note here that, by design, the predictive model is lightly specified and serves merely to separate BPS respondents into different demographic cells that can be matched to those in the poststratification data set. Differences across demographic subgroups in factors that support bachelor's degree attainment—parental education, access to quality high schools, financial supports—are absorbed into the demographic indicators.

With these caveats in mind, the model shows that compared to the omitted group (non-low income White men between 18 and 19 years old), low income students across all race/ethnicity categories were less likely to have earned a bachelor's degree by 2009. Similarly, women of color were also less likely to have earned a bachelor's degree by the end of the study. Among non-White, non-low income men, however, the model shows little difference in rates of attainment. Similarly, both younger and older students show slightly higher probabilities of attainment, but generally the differences are small. Compared to the first-level parameters, state-level non-varying parameters (which are fit to standardized covariate values) show less variation and are not generally as informative of bachelor's degree attainment.

Figure 4.2 shows the post-stratified posterior distributions for each state. These distributions can be interpreted as the probability that a first-time postsecondary student will earn a bachelor's degree within six years of matriculation. They can also be interpreted historically and in the aggregate: as the proportion of bachelor's degree holders in each state in 2009 who started their postsecondary education in the fall of 2003. Accordingly, the x axis in each plot has been scaled up by 100 to become a percentage. To validate the method, each state's plot also includes two vertical lines. The dashed line represents the percentage of the state's population over 25 years old with a bachelor's degree (not including graduate or professional degrees). The solid line represents the first percentage with addition of the percentage of 18 to 24 year olds with a bachelor's degree or higher. Both values come from the ACS single year estimates for 2009. I include both as neither quite captures the same sample as that post-stratified using the BPS data set. Because the BPS data set includes students between the ages of 15 and 19 year old—who would be only 24 years old at the oldest in 2009—the first estimate likely offers an underestimated comparison. Including the 18 to 24 year old age range has the opposite problem: half of this population would have been less than 15 at the time of the first BPS survey.[4] The posterior densities of most states should predominantly fall somewhere between these two values if the BMRP procedure has been successful.

For most states, this is the case. Some states, however, have posterior densities that predominantly fall far outside both estimates. These include Arkansas, New Hampshire, and Vermont, as well as the District of Columbia. For the three states, the mean of the posterior is too high; for Washington D.C., it is too low. While it may be the case that the small population size of these areas makes it harder to accurately predict their state-level attainment, a more subtle difference between the BMRP sample and ACS samples may be at work. Whereas the ACS estimates measure attainment among *current* residents, the BMRP estimates extrapolate based on the *initial* state of residence. If states experience differential net migration based on education (brain drain vs. brain gain), the ACS estimates and BMRP estimates could be very different (Hernández-Murillo, Ott, Owyang, Whalen, et al., 2011; Rosenbloom & Sundstrom, 2004). If this is the case, then Washington D.C.'s twin status as a geographically small urban area and the nation's capitol may mean that, on average, bachelor's degree attainment among its residents—which include many highly educated government and private-sector employees transplanted from around the country—may be higher than that of those initially from the District. Similarly, if Arkansas, New Hampshire, and Vermont lose many of their college-educated citizens or experience differential growth patterns than other states (Brome, 2007), then the BMRP estimates would be higher than ACS estimates.

These few states aside, the BMRP estimates appear reasonable: no values are improbably high ($> 70\%$); the posterior distributions of larger states such as California, New York, Texas are narrower than those of less populous states, reflecting the differing degree of information about students available in the primary

[4]This second percentage also includes those with graduate and professional degrees, though the number of those in the 18 to 24 year old population is unlikely to be large enough to significantly skew the results.

data set samples (Carlin & Louis, 2009; Gelman et al., 2014; Jackman, 2009); and most are generally in line with their ACS counterparts. In the cases they are not, as with the District of Columbia, it may be due to differences in the underlying comparison groups. Though the BMRP estimates are not entirely without error, this validation procedure suggests that the method performed well, producing state-level estimates that show reasonable variation in bachelor's degree attainment that largely agrees with estimates given by other sources.

4.5.2 Bachelor's degree attainment within six years

To compare bachelor's degree attainment between online and non-online students, I first estimate three intermediate models: the probability that a student will attend an in-state postsecondary institution, $y_{samestate}$, the probability that an in-state student will take at least one online course in college, y_{online}, and the probability that an in-state student will take no online courses, $y_{not\,online}$. As I explain above, I fit the model for $y_{samestate}$ using the full sample of students in the primary data set. For the two online outcomes, I subset the primary data sample to only those students who attended college in the same state as their initial residence.

Figure 4.3 shows the β and γ parameters for the first model predicting in-state enrollment. Compared to the baseline group, low income status once again has the strongest predictive power. Looking at the interaction terms in addition to the primary effect, low income students across all race/ethnicity demographic were more likely to attend school in-state. Though low income White women may have been no more or less likely than non-low income White men to attend in-state institutions, low income White men were more likely to do so. Similarly, non-White women were much more likely to stay in state while White woman were only slightly so. As with the first non-nested model on bachelor's degree attainment, the second-level parameters are generally less informative, though I include them to support overall model fit.

Posterior parameter distributions for the next nested outcomes, online and never online among in-state students, are shown in Figures 4.4 and 4.5, respectively. Because these models are fit to the same primary sample with only a flip of the outcome value—where $y_{online} = 1$, $y_{not\,online} = 0$, and vice versa—the parameter distributions should be mirror images of one another. Visual inspection of the plots shows this to be the case. In general, both individual demographic characteristics and state-level covariates are less informative about the likelihood of a student taking an online course. The top plot in Figure 4.4, however, does show a few differences among demographic groups (reversed in Figure 4.5): 20 to 29 year old students were generally less likely than 18 to 19 year olds to take any online courses during this time, while low income Black and Hispanic students were more likely than their White male peers.

The post-stratified posterior predictions of bachelor's degree attainment—the key outcome of interest—for both online and never-online groups of students in each state are shown in Figure 4.6. These posterior predictions were produced using the nested BMRP formula and represent the following probabilities within each demographic cell multiplied together, weighted by population size, and summed to the state level: (π^1) being a first-time postsecondary student who went to an in-state college or university, ($\pi^{2(a|b)}$) took (or not, as the case may be) at least one online course, and (π^3) earned a bachelor's degree within six years of enrollment. Population weights come from the ACS 2004 single year estimates. Red densities represent students who never took an online course; blue densities those who took at least one. With the Bayesian framework, these predictions may be directly interpreted as the probability that a first-time postsecondary student who chose to stay in-state earned her bachelor's degree, conditional on whether she ever took any online courses. On average, the probabilities for both groups should be lower than those shown in unconditional bachelor's degree model since these model precondition on choosing to enroll at an in-state institution.

Figure 4.6 shows that across most states, online students appear less likely to have earned a bachelor's degree than never-online students. In some states such as Illinois, Iowa, Massachusetts, Nebraska, and Vermont, the difference is clear as each posterior density is sufficiently tight enough to show their separation. For other states, the difference is either too small (*e.g.,* Georgia and Idaho) or the densities too wide (New Jersey, Washington, and West Virginia) to make an easy comparison in the context of a small multiples plot. The left plot in Figure 4.7 facilitates the comparison through a rank order plot of the differences between the two distributions, $Pr(BA \mid Online) - Pr(BA \mid Not\,Online)$. Blue dots represent the mean value of the difference between online and never online post-stratified posterior distributions. The thick red lines show the 50% credible interval (25% to 75% quantile range) and the gray bars the 95% credible interval (2.5% to 97.5% quantile range). Negative values represent online course takers being less likely than their never-online counterparts to earn a bachelor's degree within six years. Positive values mean they were more likely to earn this degree.

For all but seven states and the District of Columbia, online course takers were less likely to earn a bachelor's degree within six years of enrollment. For only a handful, however, does the 95% credible interval not cross zero: Illinois, Nebraska, Iowa, Pennsylvania, Indiana, and Oregon. In these states, online course takers were on average 14% (Illinois) to 7% (Oregon) less likely to earn a bachelor's degree. For the rest of the negative-leaning states, the 95% and, in some cases, 50% credible interval cross zero and signal less surety about the difference. Another interpretation is that while, on average, students who took one or more online courses were less likely to earn a bachelor's degree within six years, they may have been more likely to do so a non-insignificant proportion of the time. The few states that show positive differences on average, ranging from less than 1% in Wyoming to 9% in the District of Columbia, have wide credible intervals that cross zero. Again, the interpretation is that the difference between the groups is not clear, and is often likely to be negative even though it is on average positive. Washington D.C., commensurate with its small size and wide posterior for online students shows a wide range of potential differences [-8% to 56%] that are mostly uninformative.

It is important to reiterate that due to the Bayesian framework underlying the BMRP procedure, a statement of significance regarding the state-specific predictions does not apply. Therefore, it is more appropriate to say that online students in New Hampshire, for example, were less likely to earn a bachelor's degree within six years than their never online counterparts 89.5% of the time:

$$Pr(\theta^{difference}_{NewHampshire} < 0) = 0.895.$$

The difference in interpretation is not merely semantic, but may align more closely with how the results from a BMRP model may be used by policymakers. Instead of asking, *Are the differences significant?*, they may instead determine their own state-specific decision points and make direct comparisons. If policymakers in New Hampshire determined that based on their unique state needs and infrastructure, a $\pm 5\%$ difference in attainment between online and never-online students were acceptable, then the formula and result would become

$$Pr(\theta^{difference}_{NewHampshire} < -0.05) = 0.745,$$

indicating that three out of four online course-takers had lower odds of successful bachelor's degree completion even given the extra allowable difference. Under this scenario, these results suggest that policymakers would either have to change their expectations or consider state-level policy intended to close the gap.

4.5.3 Other degree outcomes

For many states, degree outcomes other than six-year bachelor's degree attainment may be more relevant in the comparison of online and never-online students. In the following subsections, I briefly discuss the findings for (1) any degree (certificate / associate / bachelor's) within six years of enrollment, (2) associate degree attainment within three years of enrollment and (3) any degree within three years of enrollment. In interest of space, I will only present plots of rank order differences between the post-stratified posterior predictions of the two groups. To produce the results for each outcome, I employed the same multi-step, nested logistic BMRP procedure. Accordingly, the β and γ posterior distributions for the two intermediate outcomes ($y_{samestate}$ and $y_{online}/y_{not\,online}$) were exactly the same as those estimated and used in the prior model on bachelor's degree attainment. Parameter distributions for the final degree outcome models follow generally the same pattern as seen in both bachelor's degree attainment models in terms of the differences that demographic characteristics and state-level covariates have on eventual degree attainment.

4.5.3.1 Any degree attainment within six years

The right plot in Figure 4.7 shows state-specific differences between the groups in terms of any sub-graduate degree attainment: certificate, associate degree, and/or bachelor's degree. Compared to those in the left plot, absolute mean differences between student groups are wider at the extreme ends: -17% to 9% for bachelor's degree versus -29% to 24% for any degree attainment. When any degree is considered, a few states show larger mean differences in outcome probabilities between the online and never-online student populations. The difference in bachelor's degree attainment for Maine's two student populations, for example, was approximately -7%; for all degree types, it jumps to -29%. South Carolina also moves from -6% difference between bachelor's degree attainment to one that is -22% for all degrees.

Most states, however, both retain their relative position in the rankings and generally uninformative differences between online and never-online students in terms of any degree attainment within six years. A few more states than before (fourteen plus the District of Columbia) show positive average differences, where online students became more likely to earn a degree. This suggests that for sub-baccalaureate degrees, online courses may have supported completion. Considering the differences in degree length (four years for a BA/BS; two or fewer for an AA/AS or certificate), however, improved probabilities of an associate degree or certificate attainment for online students may be less policy relevant if students took as long to complete them as non-online students took to complete bachelor's degrees.

4.5.3.2 Associate and any degree attainment within three years

Because the associate degree is generally structured as a two year degree, three years post enrollment may represent a more appropriate time to compare online and never-online students for this outcome. The left plot in Figure 4.8 ranks the differences in post-stratified predicted probabilities by state as before. Unlike for six-year bachelor's degree attainment, the mean difference in outcomes is positive, meaning that in most states, students who took at least one online course were more likely to have earned an associate degree. For Wyoming and West Virginia students, the mean difference was large at 32% and 35%, respectively. For most states with positive differences, however, the difference was more moderate: less than 15% in favor of online students. Only six states—South Carolina, Hawaii, Washington, Illinois, Maryland, and Michigan—show negative average differences. Yet even in these states, much of posterior density falls to the right of zero, meaning that online course takers may still have done better or at least no worse than never-online students in terms of associate degree attainment much of the time.

The right plot in Figure 4.8 ranks the differences for any degree attainment within three years. Though bachelor's degree earners were also included as positive outcomes, very few students earned this degree in three years. The second model outcome, therefore, practically includes certificate earners alongside associate degree earners. Across the country, average attainment differences between online and never-online students shift to be more negative. Most states retain their relative rankings, though a few move more substantially. Whereas the difference in associate degree attainment among Tennessee students was positive, meaning online students were slightly more likely to obtain the degree, the difference becomes negative when certificates were included as positive outcomes. Hawaii also flipped, but in the opposite direction (from negative to positive on average) and in a less extreme fashion. Why might this be the case? Perhaps certificate programs in Tennessee required more hands-on work and put online course-takers at a disadvantage whereas the opposite was true for Hawaii. It also could be the case that the interactions between demographics, course-taking behavior, and degree goals in each state simply resulted in different outcomes. The point is that neither state-specific result is right and the other wrong in terms of the effect of online course-taking on earning a certificate. Instead, each state has its own prediction that policymakers may consider alongside their contextual knowledge when crafting future policy for the state.

4.6 Forecasting

The results presented so far have used population counts from the same year as the initial survey, 2004. Because state-level differences in degree outcomes between online and never-online course-taking populations is not readily available for all states, the results are still of interest even if they are now historical. For public opinion and elections, however, many political scientists are interested in state-specific forecasts, wondering how the current state of public opinion may sway future policy and elections. So too may education policy scholars and state policymakers be interested in future forecasts.

BMRP predictions are easily extensible in this manner. Simply post-stratifying using different population counts and state-level covariates, from 2015 instead of 2004, for example, means that predictions for six-year bachelor's degree attainment apply to 2021 rather than 2009. The plots in Figure 4.9 compare state-specific difference rankings post-stratified using 2004 ACS counts (left) and 2015 ACS counts (right). Each set of predictions used the same underlying BPS data. The only difference lies in the poststratification weights. Comparing the two plots, there are relatively few changes between the historic and the future predictions in terms of state rankings and average difference. Online students who began in the fall of 2014 are, like their fall 2003 counterparts, less likely on average to earn a bachelor's degree by 2021. In Maryland and West Virginia, however, current online students are forecast to be slightly more successful in obtaining a bachelor's degree than their never online peers, a flip in the difference from their earlier counterparts.

The major assumption in this forecast is that more recent first time students who choose to take at least one online course will perform like demographically similar students in the early 2000s. Same-state differences in post-stratified outcomes between earlier and current student populations are only attributable to changes in the state's demographic composition and relative changes in state-level covariates included in the model (*e.g.,* median income, undergraduate population size, public four-year tuition and fees). In the context of online education, this may be a heroic assumption. With increases in the proportions of students who take online courses (Allen & Seaman, 2011; U.S. Department of Education & National Center for Education Statistics, 2016) and technological changes that may make online courses more appealing to students (see chapter 2), current online students may have different propensities for eventual bachelor's degree attainment.

This particular caveat, nonetheless, does not mean that BMRP should not be used to forecast educational policy outcomes or that forecasts regarding online learning in particular are inappropriate. I only note the method's limitations. It remains true that BMRP results may provide researchers and policymaker with more reasonable forecasts and sub-national inferences than they would otherwise be able to produce. In the next section, I discuss the BMRP results in regards to a single state, Tennessee, as a case study of how they may be used to inform education policy decisions in a particular state context.

4.7 Case study: Tennessee

Under the governorship of Bill Haslam, Tennessee has undertaken a number of initiatives meant to bolster postsecondary degree attainment in the state. In addition to the Tennessee Promise Scholarship, which provides recent high school graduates last-dollar support to attend one of the state's two-year colleges for free, the state recently initiated Tennessee Reconnect, which gives the same aid to adults in the state who do not have a postsecondary credential (*Tennessee Promise*, 2017; *Tennessee Reconnect*, 2017). Together with Tennessee LEAP (Labor Education Alignment Program), these two scholarship programs form the core of the state's Drive to 55 campaign, which has set a goal that 55% of all Tennessee adults will have a postsecondary credential by 2025 (*Drive to 55 Tennessee*, 2014; Tennessee Higher Education Commission, 2015).

Online learning represents one component of a multi-armed plan to reach this goal. In addition to supporting its own statewide online system through the Tennessee Board of Regents, TN eCampus, the state has partnered with Western Governors University, the fully-online multi-state compact institution, to form WGU Tennessee (Kinser, 1999; *TN eCampus*, 2017; *WGU Tennessee*, 2017). Governor Haslam has also been a strong proponent of public-private partnerships to improve the state's broadband infrastructure, seeing them as a means to boost both online postsecondary enrollments and economic development (Connected Tennessee, 2015).

In a recent publication, the Connected Tennessee group outlined the state of broadband access throughout the state. The organization's survey showed that as of 2014, "70% of Tennesseans [had] adopted broadband at home, and 81% of businesses in the state utilize[d] broadband," (Connected Tennessee, 2015, p. 3). While these percentages indicate increases in broadband usage across the state since 2009, the authors note that access remained uneven across the state, with rural areas reporting lower rates of access (88.54%) than urban areas (93.46%). They further report that under the updated FCC guidelines for broadband, which set 25 Mbps download speeds and 3 Mbps upload speeds as the benchmark, nearly 17% of Tennessee residents were without broadband access, most them, again, living in rural areas. All together, the survey results showed that 1.4 million adults in the state did not have access to broadband (Connected Tennessee, 2015).

Based on Tennessee's higher education and infrastructural context, how well might online learning support its degree attainment goals? In chapter 2, I provide evidence that students respond to improved download speeds when choosing whether to take some of their courses online. This finding lends credence to Haslam and Connected Tennessee's argument that improvements to the state's broadband infrastructure will help students take online courses. Yet in chapter 3, I provide evidence that online students are less likely to persist or successfully pass their courses than students in face-to-face courses. These findings support others found in the literature (Xu & Jaggars, 2011, 2013). It is a logical extrapolation to argue that degree outcomes for online students may be depressed if, on average, they must retake courses they either dropped or failed. Yet despite these negative findings, Tennessee's student population may respond differently to online coursework than those in Georgia, Virginia, and Washington state. It also may be the case that negative effects of online course delivery are minor (Joyce et al., 2014) and in the aggregate, online courses support degree attainment by increasing access to those who otherwise would not be able to enroll (Bowen, 2013; Cox, 2006; Deming et al., 2015; Selingo, 2013). In the absence of Tennessee-specific survey data, BMRP provides a means of answering these questions.

Figure 4.10 shows density plots of post-stratified predictions of degree attainment for online and never-online students in Tennessee. The top-left plot reproduces the one shown in Figure 4.6, probable bachelor's degree attainment among in-state first-time students within six years, at an increased size. To its right is a plot showing the difference between any degree attainment after six years. The two plots on the bottom show differences in associate degree attainment and certificate attainment after three years. In all plots, predictions for students who took no online classes are shown in red, those who took at least one in blue.

In terms of six year attainment, the two top plots show only small differences between the two groups. The average predicted bachelor's degree attainment for both groups was 18.4%; for any credential attainment, 46.3%. These numbers, which pertain most directly to 2009, are respectively lower and higher than the 37.8% attainment among Tennessee working-age adults reported for 2013 (Tennessee Higher Education Commission, 2015). As with the prior ACS validation, these numbers may not be entirely commensurate due to differences in the underlying sample populations and time frame. The range of most plausible values for the second outcome, however, does contain the published estimate as its 95% credible interval ranges from 35.6% to 54.3%.

On average, Tennessee in-state students who took at least one online course had a 19% chance of earning a bachelor's degree in six years compared to the 18% chance among never-online students. The numbers flip when all degrees are considered: never-online students show a 48% chance of earning a credential whereas online students show a 45% chance. Because of the wide overlap between the two distributions in each plot, however, these average point estimate difference may not seem very compelling. Fortunately, the Bayesian framework allows for better and more policy-relevant comparisons. One is the probability that online students will have a higher likelihood of eventual success,

$$Pr(\theta_{online} > \theta_{never\,online}),$$

which may be obtained by simply evaluating the proportion of draws from online post-stratified posterior that are larger than those from the never-online post-stratified posterior. Using this metric, online students in Tennessee had a higher likelihood of earning a bachelor's degree than never-online students 60% of the time: $Pr(\theta^{BA}_{online} > \theta^{BA}_{never\,online}) = 0.6035$. In terms of any degree attainment, online student had higher probabilities of success only 34% of the time: $Pr(\theta^{any}_{online} > \theta^{any}_{never\,online}) = 0.345$.

Because the Drive to 55 campaign specifically focuses on helping working adults return to school, much emphasis has been placed on associate degree and certificate attainment rates (*Drive to 55 Tennessee*, 2014; Tennessee Higher Education Commission, 2015). These degree outcomes, therefore, are likely more relevant to the state's overall attainment goals. The bottom two plots in Figure 4.10 make it clear that, compared to six-year bachelor's degree attainment rates, differences between online and never-online students were more pronounced for one and two-year degree outcomes after three years. On average, online course takers were more likely to earn an associate degree than never-online students (9.8% versus 3.9%). More importantly, they had a higher probability of earning an associate degree 96% of the time: $Pr(\theta^{AA}_{online} > \theta^{AA}_{never\,online}) = 0.964$. On the other hand, online students were more likely to earn a certificate than never-online student less than 1% of the time: $Pr(\theta^{cert}_{online} > \theta^{cert}_{never\,online}) = 0.0005$.

Considering these four degree outcome comparisons, the role that online coursework can play in helping Tennessee meet its degree attainment goals is not straightforward. While the BMRP results indicate that students willing to take online courses were more likely than their never-online peers to earn a bachelor's or associate degree, this population of students was less likely to earn a certificate. Improvements to broadband infrastructure or direct pushes to increase the number of students who take online courses, therefore, may support four- and two-year degree attainment to the detriment of certificate attainment. Tennessee policymakers should consider these results in context of their twin goals of increasing online enrollment and the number of certificate holders since the findings indicate that they may not be commensurate. Of course, these outcomes are not destiny. Institutional administrators, researchers, and policymakers could also work together to further unpack the gaps between the groups and offer modifications to existing online course offerings that make them more amenable to certificate programs. Whatever the steps moving forward, these results show the potential of BMRP to use nationally representative survey data to provide policy-relevant inferences at the sub-national level.

4.8 Conclusion

Bayesian multilevel regression with poststratification represents a methodology that may be fruitfully applied to questions of education policy. In this chapter, I was able to produce and validate representative estimates of six-year bachelor's degree attainment across the states using survey data that are only representative at the national level. I also was able to use a multi-stage nested BMRP model to produce separate state-level degree outcome predictions for students who took at least one online course and compare them to those of students who took no online courses. On the whole, the results show that online course takers were generally less likely to earn a bachelor's degree than never-online students in six years and slightly more likely to earn an associate degree within three years. The results for all models show a fair amount of variation across the states, however, justifying the use of BMRP over a simple national-level pooling of the data.

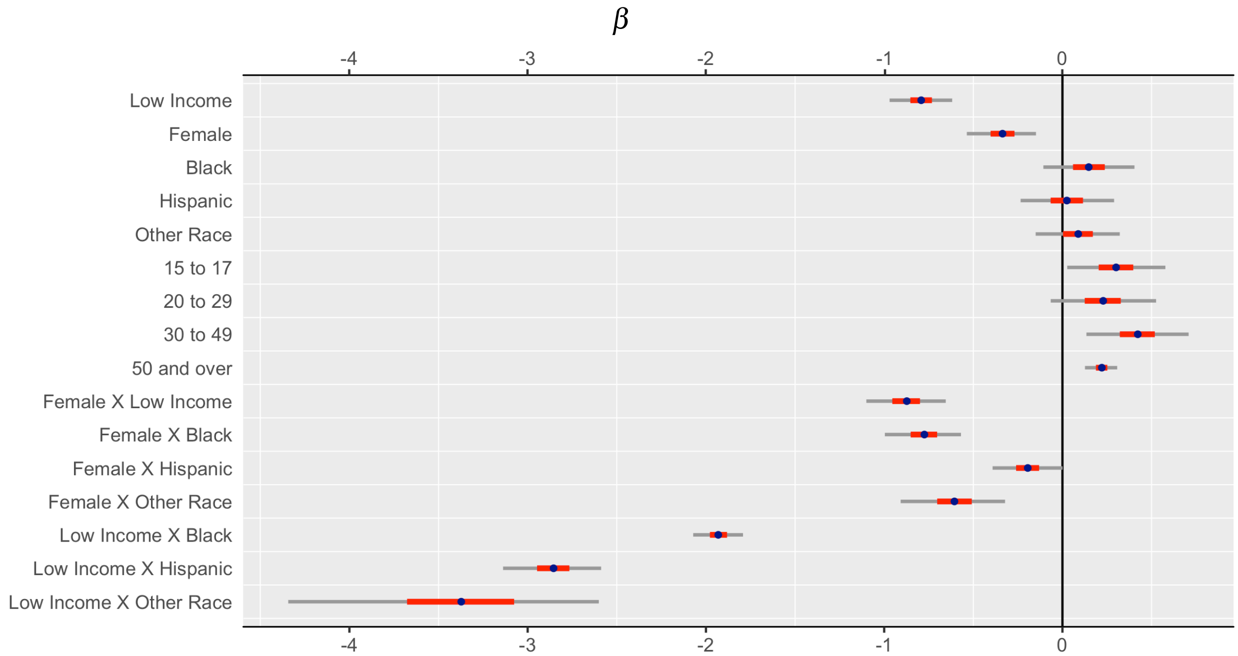
It is important to note that none of the predictions in the chapter are causal. They say nothing about whether students are more or less likely to earn a degree due to their having taken (or not) online courses. Instead, the primary driver of variation in post-stratified outcomes across the states is variation in state demographics. This is by the design of the BMRP procedure, which weights responses from non-representative subjects by state-level proportions of persons who are demographically similar. For some questions to which BMRP has been applied in the past, state-level covariates used in the second level of the hierarchical model were an important determinant of the outcomes (Park et al., 2004). For degree outcomes due to online course enrollments, however, the intersection of respondents' gender, race/ethnicity, and age group were comparatively more important. Once the state-level context is taken into account through the varying intercept and state covariates, students of a certain demographic category are assumed to generally behave the same as their counterparts in other states. In the context of online learning, I argue that this assumption holds well enough to make useful predictions about state-level average degree attainment.

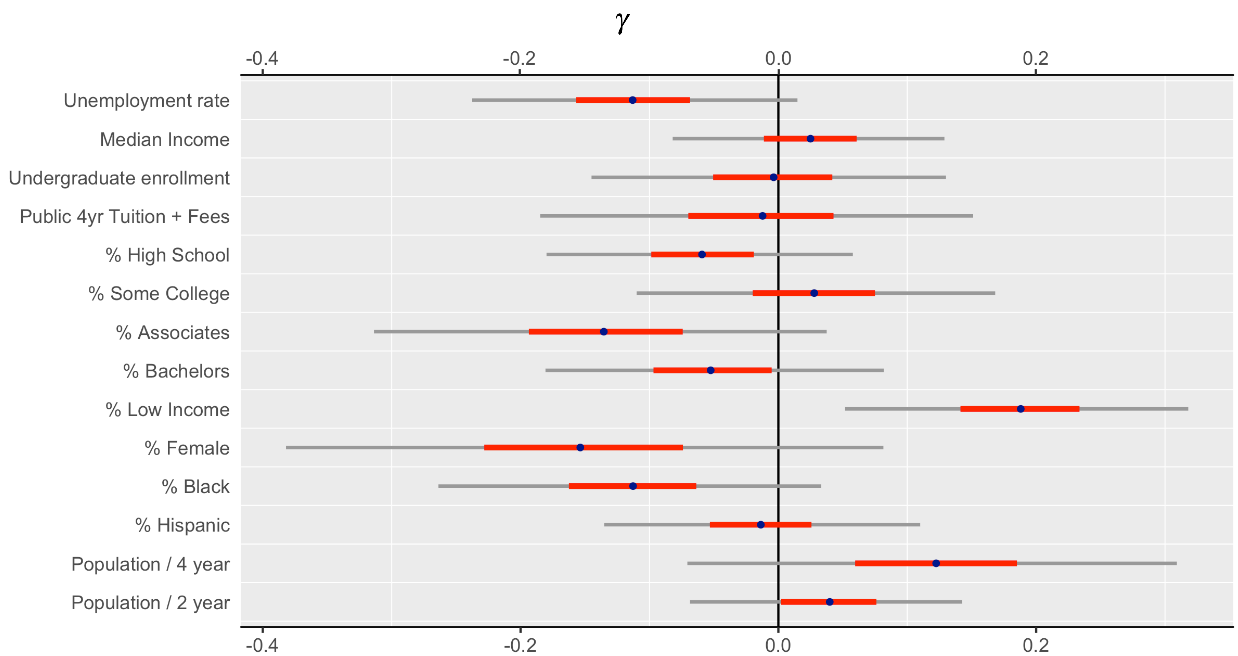
As another point, the criterion for being an online student is quite liberal. Taking only one online course during the entire survey window qualifies a student as being online in this study. These results, therefore, do not speak to any outcome differences among various dosages of online course-taking or consider the differences between students who enroll in fully online degree programs and those in traditional programs. This represents a limitation of the method and chapter. I argue, however, that the never-online and any-online course-takers represent relevant groups for the majority of postsecondary students in a state as relatively few enroll in entirely-online programs (Snyder et al., 2016). States may wish to know how the availability and selective use of online courses will help their population meet their education attainment goals, even if many of their students will still take face-to-face classes as part of their higher education degree pathways.

In this chapter, I focused on Tennessee to offer an example of how BMRP may be used in a specific state context. A similar analysis could be performed for other states. More broadly, my results offer researchers and policymakers a benchmark for comparing their state's degree attainment outcomes among online students to those of other states that they consider demographically, culturally, or politically similar. Policymakers may also use these results, particularly in the absence of state-level data or analyses, to ask whether their current higher education attainment goals and plans as they relate to online learning are supported by underlying population trends. As I show above, BMRP supports both retroactive predictions (past data with current outcomes) and future forecasts. If population and degree outcome forecasts are not well aligned—meaning that current plans are not well supported should the state population change—policies may have to be adjusted to take them into account. My results and implementation of the BMRP procedure allow policymakers to quantify such adjustments.

As a final methodological point, BMRP need not use national data to produce state-level estimates. It may also be used to post-stratify to the county, legislative district, or MSA-level provided that population count data are available for the desired area (Warshaw & Rodden, 2012). States with representative or even non-representative survey data (Wang et al., 2015) may be able to successfully employ BMRP to make predictions of important education policy outcomes for sub-state areas of interest. State policymakers may also find it useful to compare their state to others, and use BMRP in the nation-to-state level used in this chapter. For education policy researchers, an important takeaway of this study is that BMRP is sufficiently flexible to conduct a range of education policy analyses at the sub-national level that otherwise would require the expensive collection of new survey data and produces results that may be more easily imparted to policymakers and administrators than those generated by other methodologies.

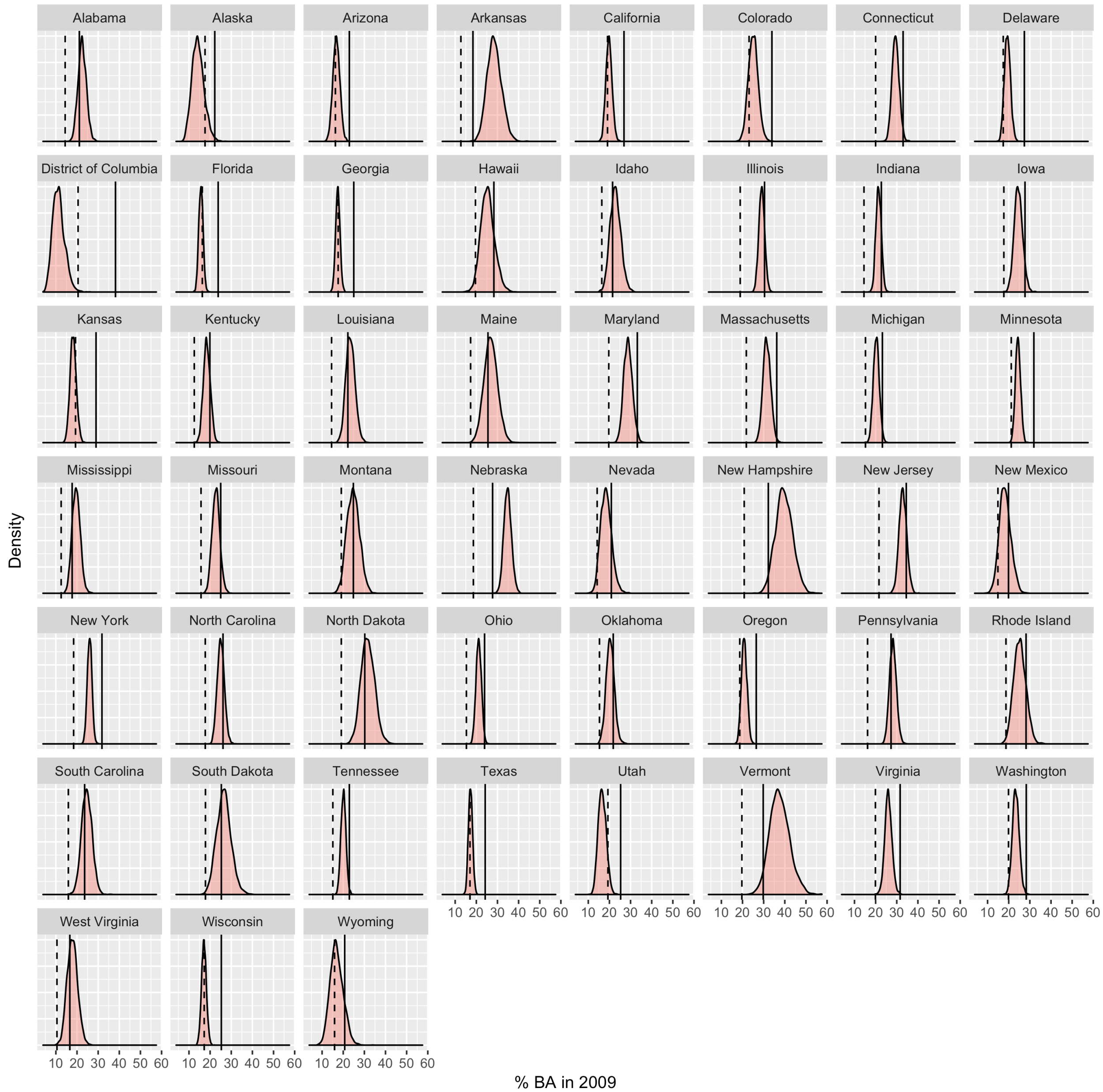
Figure 4.1: Model parameter posterior distributions for binomial model predicting bachelor's degree attainment within six years of matriculation





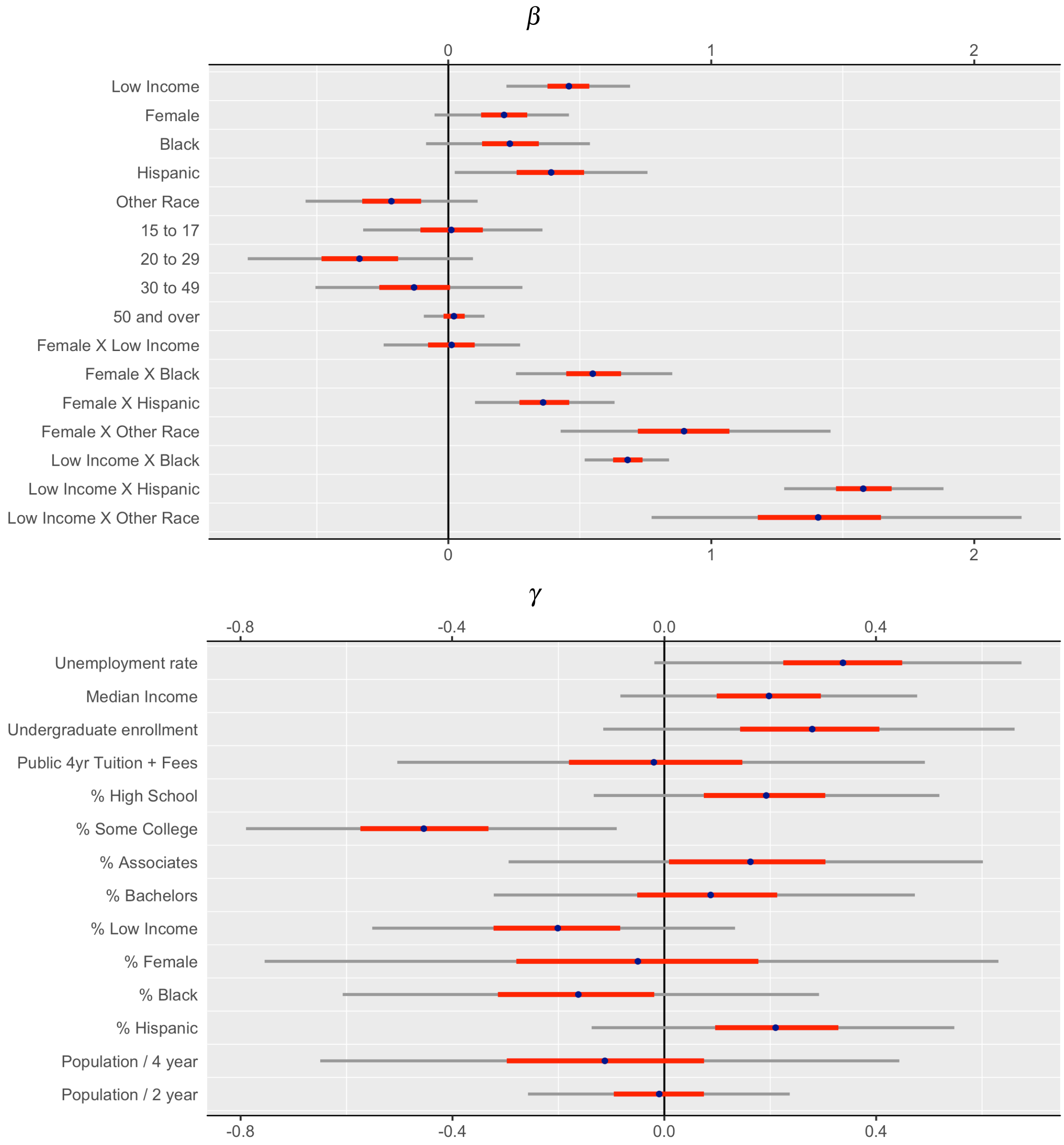
β parameters in the top plot are compared to the omitted category of non-low income 18-19 year old white males. γ parameters are estimated using standardized versions of the variables to help convergence.

Figure 4.2: Validation of BMRP model predictions of state-level bachelor's degree attainment in 2009 against estimates from the American Community Survey



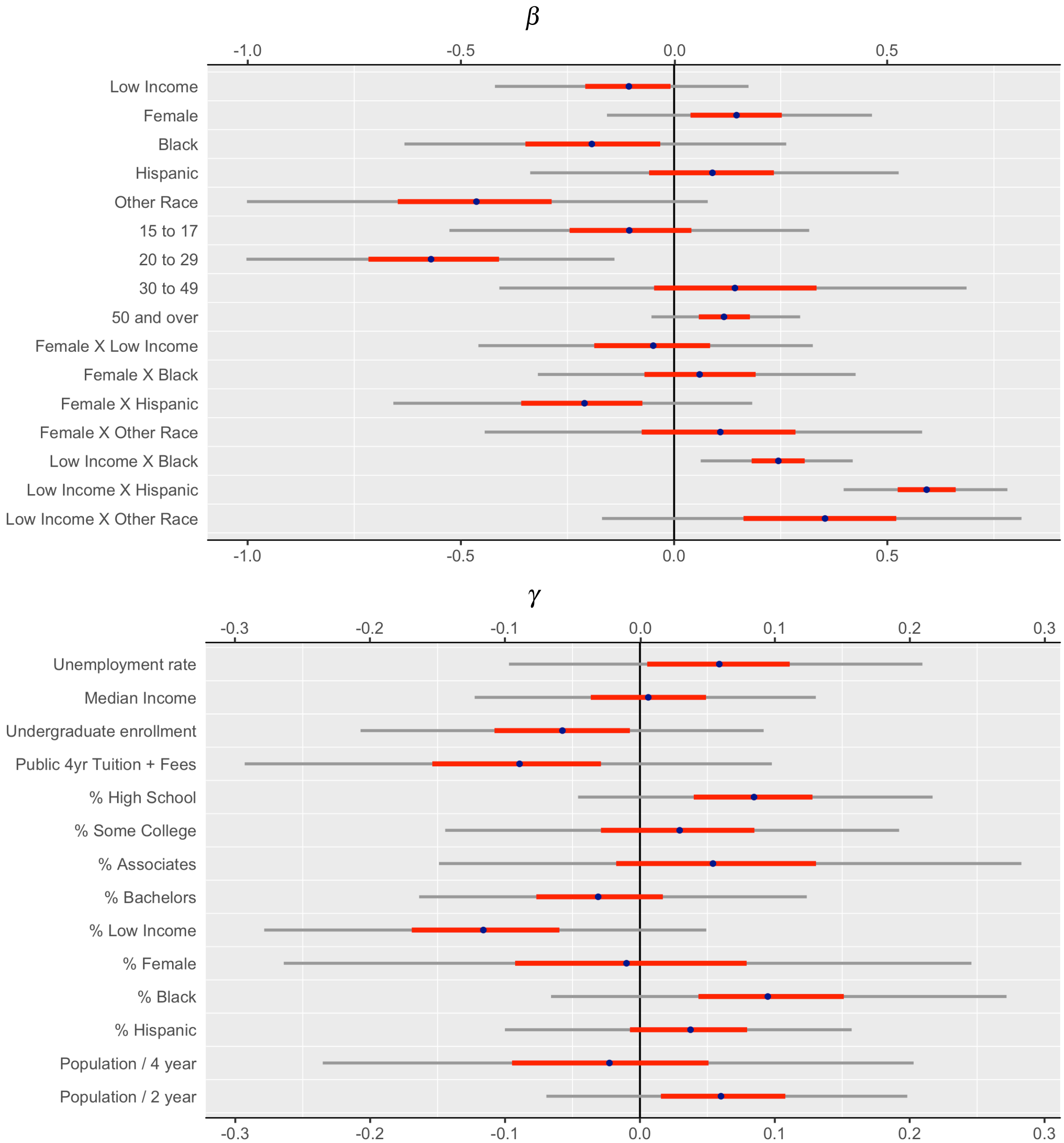
Red densities show post-stratified (ACS 2004) posterior predictions of bachelor's degree attainment in each state. Because these may be interpreted as the percent of the population with a bachelor's degree, the proportions have been scaled up by 100. The dashed line shows bachelor's degree attainment among the population 25 years and older in 2009 (ACS 2009). The solid line adds to the prior number the percentage of 18 to 24 year olds with a bachelor's degree or higher. For most states, the BMRP prediction falls within the two. Larger differences, such as seen for Washington D.C., could reflect comparatively poorer model performance for small areas. Because the BMRP posteriors reflect students *from* the state and ACS reflect those *living in* the state, it may also be due in part to differences in state migration patterns across age and education levels.

Figure 4.3: Model parameter posterior distributions for binomial model predicting postsecondary enrollment within same state



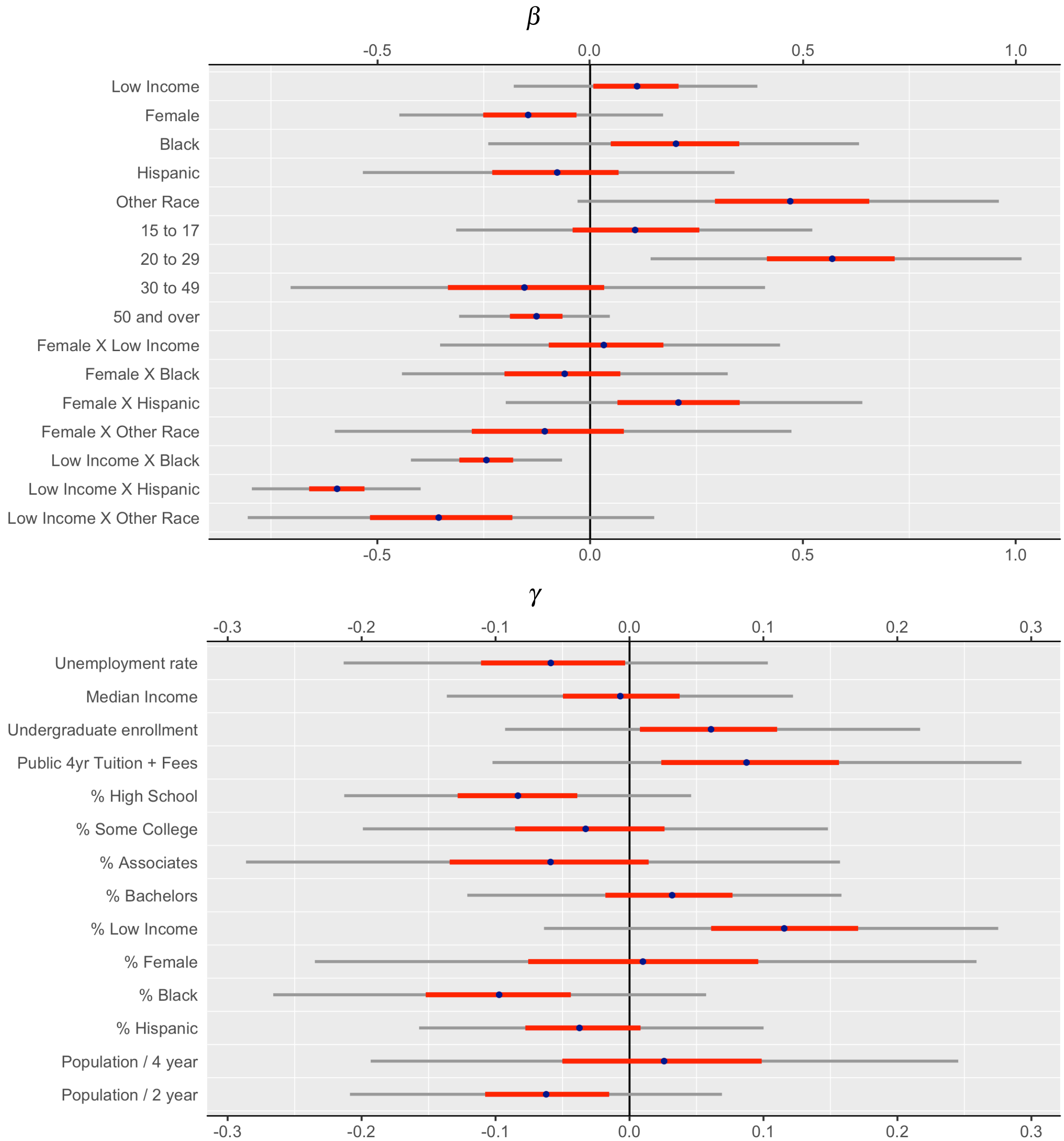
β parameters in the top plot are compared to the omitted category of non-low income 18-19 year old white males. γ parameters are estimated using standardized versions of the variables to help convergence.

Figure 4.4: Model parameter posterior distributions for binomial model predicting online course taking ($online >= 1$) conditional on enrolling in school within same state



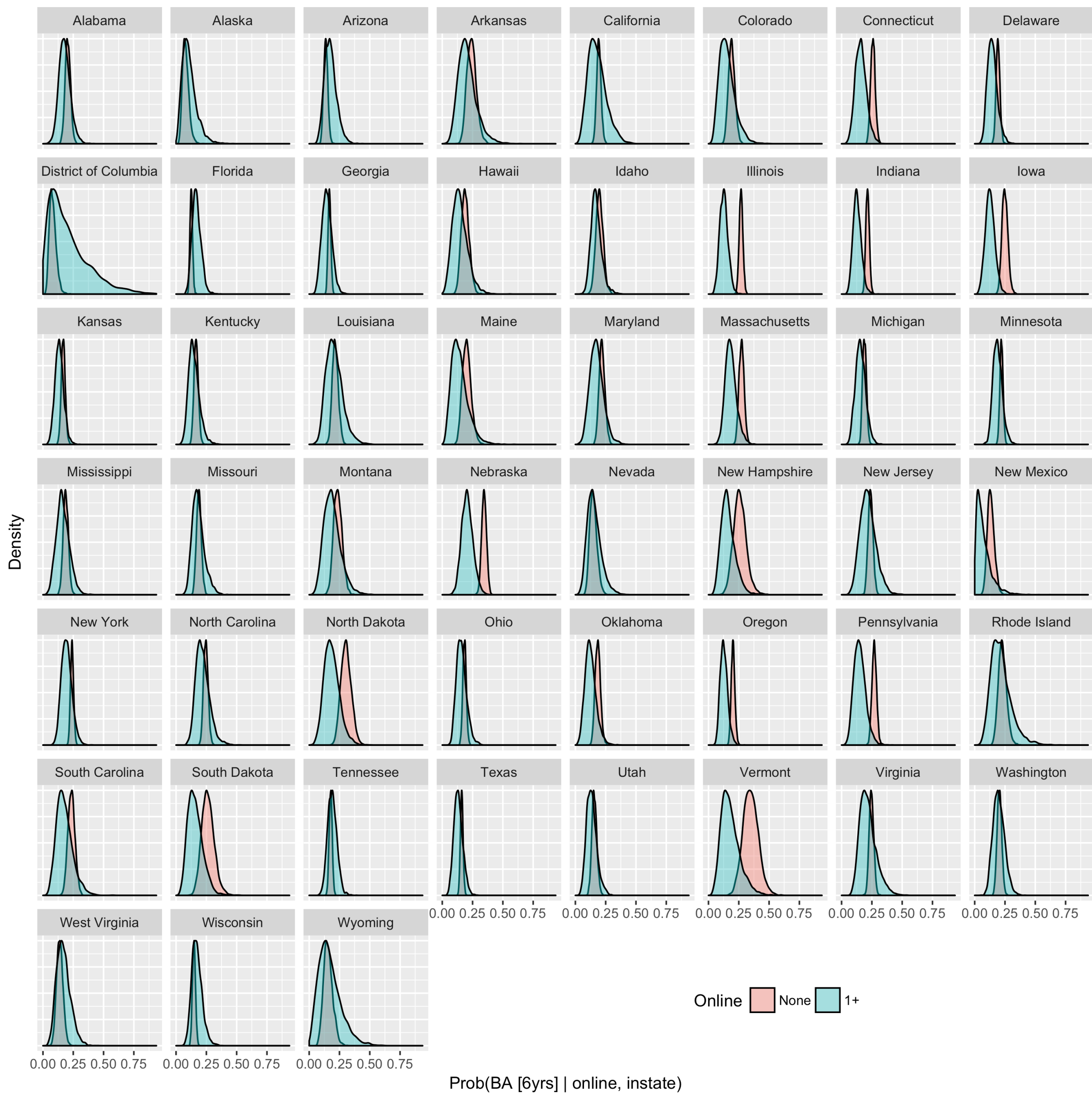
β parameters in the top plot are compared to the omitted category of non-low income 18-19 year old white males. γ parameters are estimated using standardized versions of the variables to help convergence.

Figure 4.5: Model parameter posterior distributions for binomial model predicting online course taking ($online = 0$) conditional on enrolling in school within same state



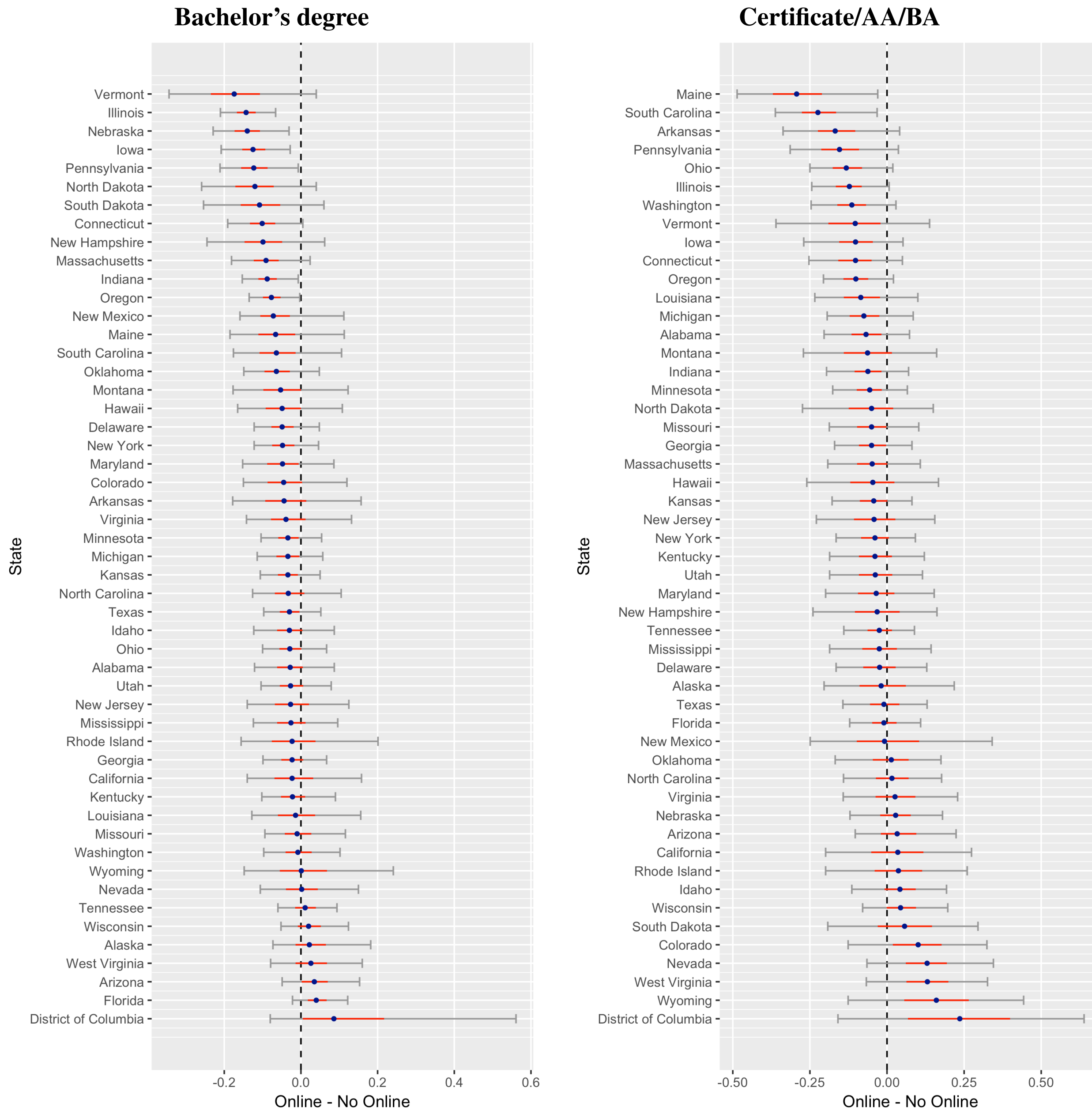
β parameters in the top plot are compared to the omitted category of non-low income 18-19 year old white males. γ parameters are estimated using standardized versions of the variables to help convergence.

Figure 4.6: Bachelor's degree attainment within six years of graduation, no online courses versus at least one online course



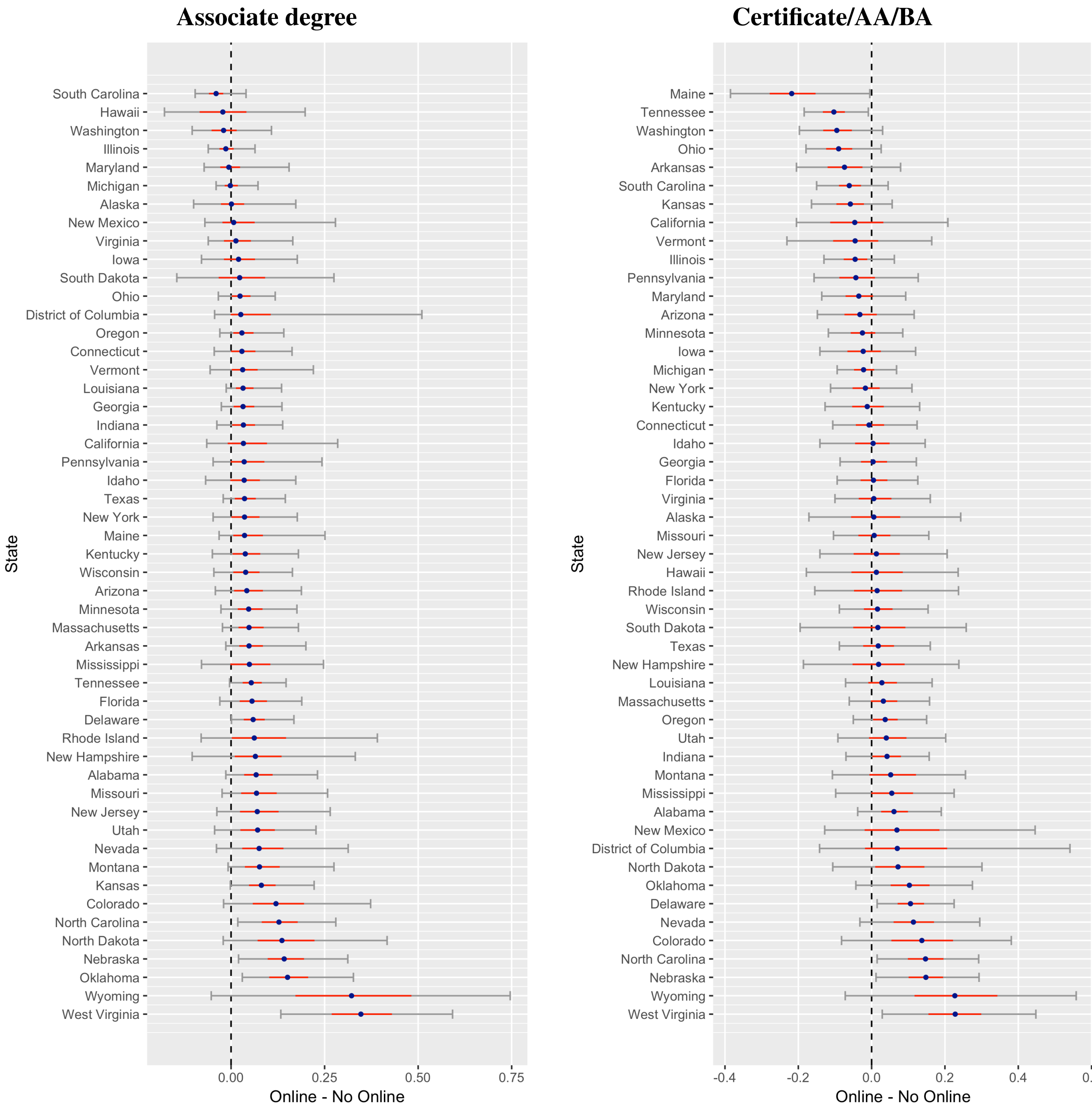
Posterior predictions are post-stratified using demographic population counts from the ACS 2004 single year estimates. The posterior probability that in-state online students will earn a bachelor's degree within six years of matriculation are shown in blue; for never online students, red. The spread of the distributions represents the uncertainty around the predictions and is generally wider for the online student predictions and either prediction within smaller states.

Figure 4.7: Ranked difference in bachelor's degree and any degree attainment within six years of matriculation between online and no online course takers



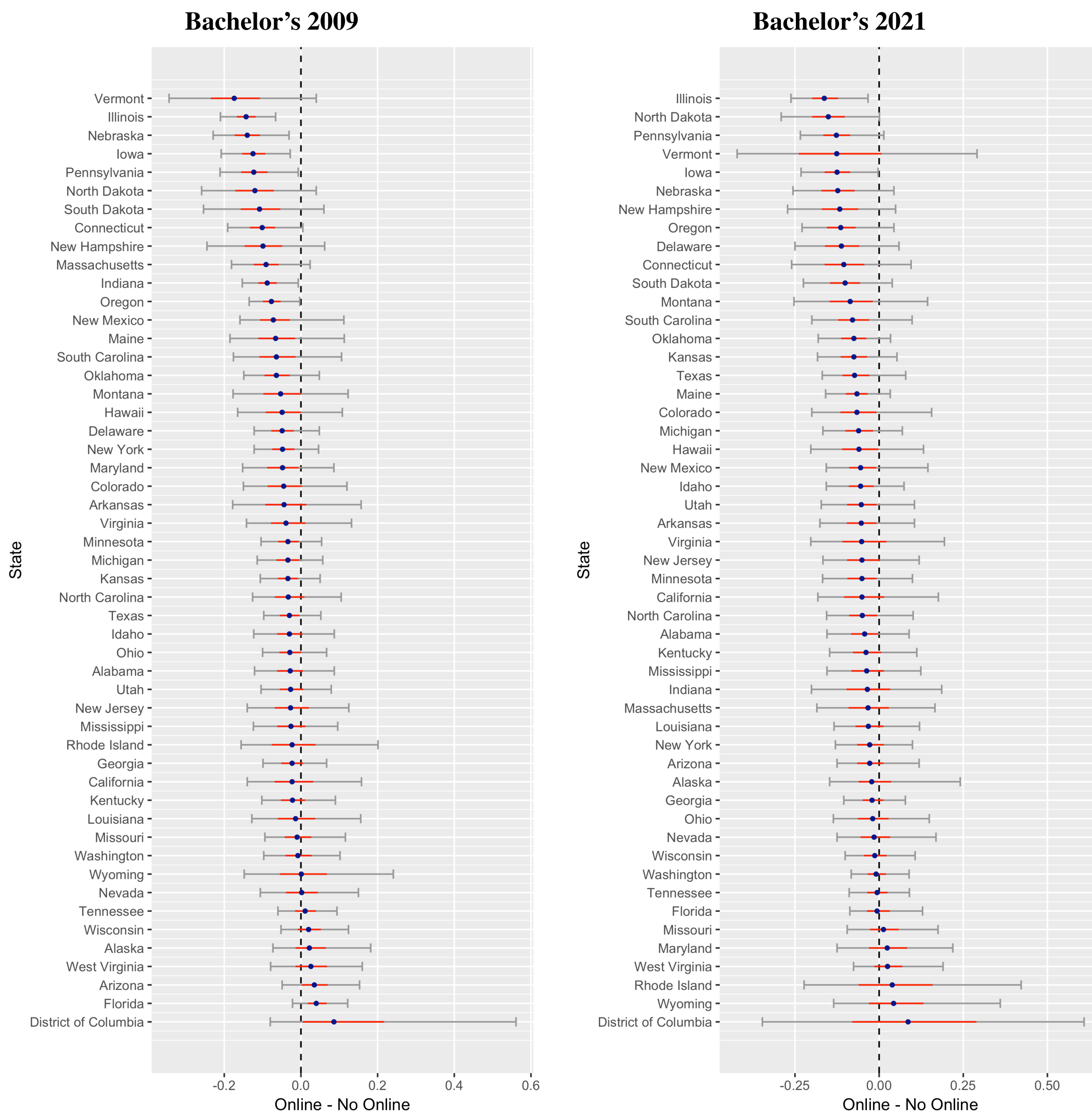
Blue dots represent the mean value of the difference between online and never online post-stratified posterior predictive distributions. The thick red lines are the 50% credible interval (25% to 75% quantile range) and the gray bars the 95% credible interval (2.5% to 97.5% quantile range). Negative values represent online course takers being less likely than their never online counterparts to earn a bachelor's degree (left) or any degree (right). Positive values mean they are more likely.

Figure 4.8: Ranked difference in associate degree and any degree attainment within three years of matriculation between online and no online course takers



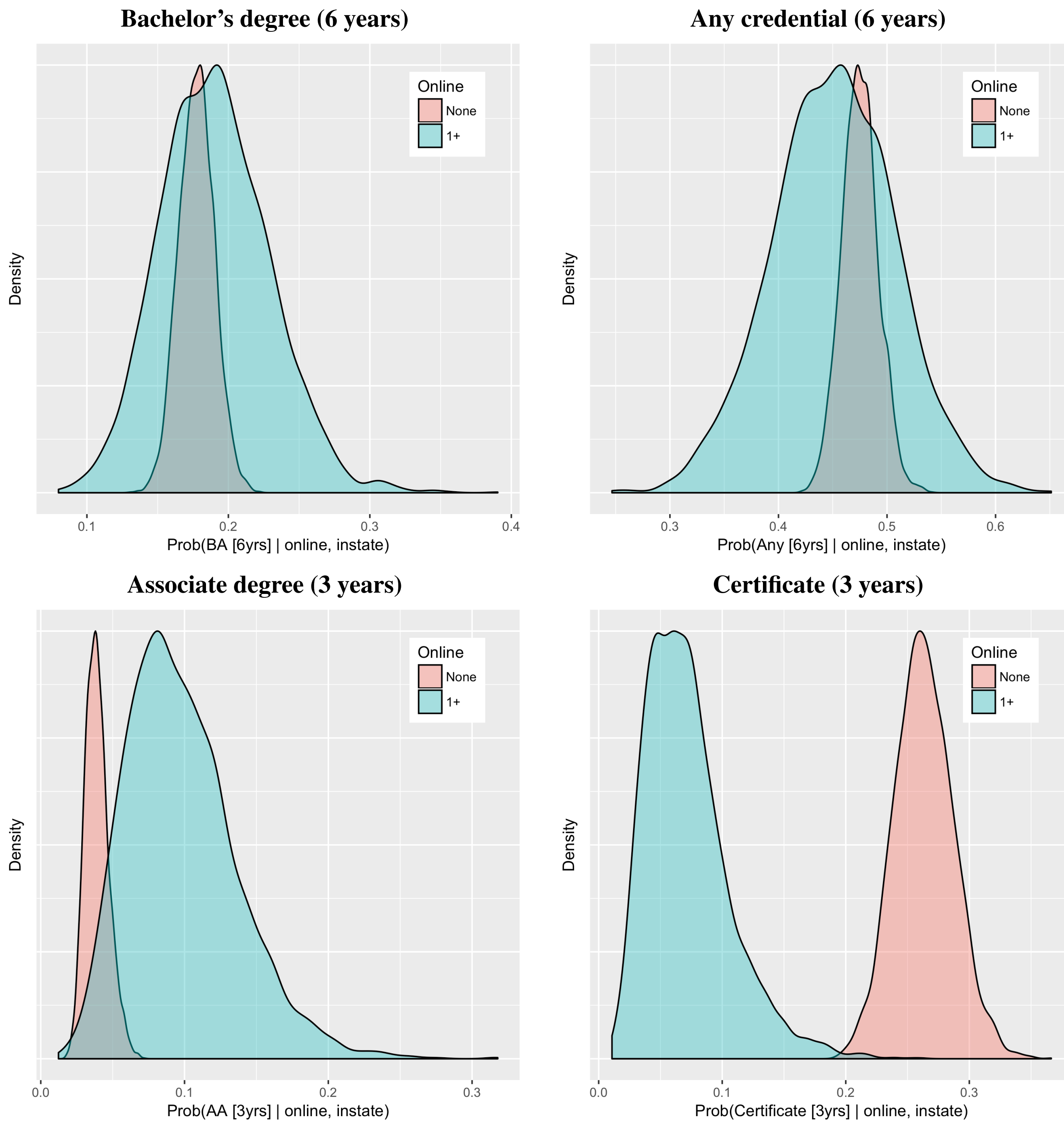
Blue dots represent the mean value of the difference between online and never online post-stratified posterior predictive distributions. The thick red lines are the 50% credible interval (25% to 75% quantile range) and the Gray bars the 95% credible interval (2.5% to 97.5% quantile range). Negative values represent online course takers being less likely than their never online counterparts to earn an associate degree (left) or any degree (right). Positive values mean they are more likely.

Figure 4.9: Ranked difference in bachelor's degree attainment within six years of matriculation between online and no online course takers: historic vs forecast



Blue dots represent the mean value of the difference between online and never online post-stratified posterior predictive distributions. The thick red lines are the 50% credible interval (25% to 75% quantile range) and the gray bars the 95% credible interval (2.5% to 97.5% quantile range). Negative values represent online course takers being less likely than their never online counterparts to earn a bachelor's degree (left) or any degree (right). Positive values mean they are more likely.

Figure 4.10: Differences in predicted postsecondary degree attainment across credential type in Tennessee



Each density represents the post-stratified posterior probability for online students (blue) and never-online student (red) in Tennessee who remain in state and to attain a credential within the specified time. Note that the *x* axes have not been aligned so that separation between the two groups in each plot can be more clearly seen. ACS 2004 estimates were used as weights.

Chapter 5

Conclusion

The chapters of this dissertation increase understanding about postsecondary online education through their investigation of the following research questions:

1. What is the connection between broadband access and the number of students who attempt online coursework?
2. What is the effect of online course delivery on course persistence and, conditional on completion, the likelihood of passing the course?
3. How do degree attainment rates differ between students who take online courses and those who take only face-to-face courses within and across states?

In this final chapter, I conclude by briefly recapitulating the findings of each chapter separately and then in context of one another. I end by suggesting pathways for future research in this policy area.

5.1 Discussion of results

In chapter 2, my results indicate that students who attend open admissions colleges and universities may indeed respond to broadband infrastructure when deciding whether to take some of their courses online. Specifically, this response is associated with increases in download speeds. With the pooled data, single-level regression models show that for every tier increase in download speed (approximately 2-3X faster download rates), institutions see an average of 41% more students choose to take some classes online. Using the multilevel model on partially-pooled data in which each state is given its own intercept (Gelman et al., 2014), the average increase is 56%. Though the 95% credible intervals for both models are wide, indicating a wide range of probable effects, the lower bound (2.5% quantile) remains above zero, meaning that the association is almost certainly positive. I do not find similar results for either upload speed or the number of Internet service providers in the area. Among the subset of two-year institutions, results are qualitatively similar, but posterior distributions are wider and less informative overall.

In chapter 3, I find that students attending schools in the University System of Georgia do not perform as well as in online course sections as they do in traditional face-to-face settings. On average, students in online courses are 2.8 percentage points more likely to withdraw and, conditional on completing the course, 5.4 percentage points less likely to pass with a C- or better end-of-course grade. Because I use a quasi-experimental instrumental variables approach, in which I instrument the endogenous choice to enroll in an online course with lagged measures of broadband access, these values represent less biased estimates of the treatment effect (Chib, 2003; Greene, 2012; Hollenbach et al., 2016). Treatment on the treated and local average treatment effect estimates are generally similar in size and direction. As a sensitivity test, I estimate the same model for the subset of students who, in a given semester, took at least one online and one face-to-face course. Results showed that they are, on average, 2.5 percentage points more likely to withdraw and 1.5 percentage points less likely to pass. Wider 95% credible intervals on the average treatment effect distribution that crossed zero, however, suggest more uncertainty for the second outcome.

In chapter 4, I am able to successfully post-stratify comparisons of degree attainment between students who never take any online courses and students who take at least one from nationally-representative survey data to each of the 50 states as well as the District of Columbia. I show that while never-online students are more likely to earn a bachelor's degree within six years of first enrollment, students who take one or more online courses are generally more likely to earn an associate degree within three years. These averages differ between states, however, with some states showing better average degree outcomes for sometimes-online students and others showing worse. Average differences notwithstanding, large overlaps between the post-stratified posterior degree attainment distributions within most states suggests that there may be little difference in eventual attainment between the groups.

Taken together, these findings offer a complex, but not incoherent narrative of online learning among postsecondary students. Using human capital theory to frame the college enrollment decision (Becker, 2009; Kane, 1999; Manski & Wise, 1983; Paulsen, 2001; Perna, 2006; Toutkoushian & Paulsen, 2016; Turner, 2004), my results offer evidence of a connection between an area's technological capacity for supporting online coursework and the number of students who enroll in online courses. Because the data that support this finding represent student responses aggregated to the institutional level, I cannot definitively say whether the association is due to changes in demand by students or changes in supply by institutions (Toutkoushian & Paulsen, 2016). Very possibly, it is a combination of both institutional desire to reduce costs (Bowen, 2013; Deming et al., 2015; Selingo, 2013) and student desire for increased flexibility and choice (Jaggars, 2014; Selingo, 2013).

Yet once students enter into online course sections, they do not generally perform as well as they do in traditional face-to-face courses. The differences are small, but they do exist. Returning to the first research question, it may be that these results point to it being institutional prerogatives dictating the supply of online courses rather than student demand. For the particular measure I use to answer the first question, the institutional data provided by IPEDS neither indicates the number of students who take more than one (but not all) distance education courses nor the number of students who are taking these courses for the second, third, *etc.*, time. A student who rationally responds to download speeds when deciding whether to take an online course may subsequently make the rational decision not to take another if he does not do as well as he hoped. I cannot see this in the aggregate IPEDS data.

It also may be that institutions respond to area download speeds when deciding to offer online courses rather than students demanding them. Considering that broadband measures were not shown to be especially strong predictors of online enrollment among University System of Georgia students, this may be the case. In this scenario, students may not have much effective choice of whether to enroll in online course sections. This could explain why students continue to enroll in online courses in increasing numbers (Allen et al., 2016; Snyder et al., 2016) even though they may not perform as well (Xu & Jaggars, 2011, 2013).

Results from the chapter 4, however, complicate this story. While it is true that, on average, students who take online courses may be less likely to earn a bachelor's degree within six years, they may be slightly more likely to earn an associate degree within three. Though the state context also appears to matter, for most states the differences in attainment rates are unclear and may actually be close to zero. I note here that the bar for inclusion in the sometimes-online group was very low: taking only one online course placed a student in that group. With more differentiation in dosage between students who took many online courses and those who took none, results may have shown starker differences. Nonetheless, some students may be making a rational decision to take one or more online courses, even with their increased risk for failure, if they have a slightly better chance of eventually earning a degree. The enrollment decision for an individual in regards to online courses remains, if less uncertain, still idiosyncratic.

State policymakers wishing to use these results have a more difficult time since they must make policy amenable to a range of individual needs and goals. I again use Tennessee as an example, which hopes to increase its population's postsecondary degree attainment level to 55% by 2025 and use improvements to its broadband infrastructure to help it meet this goal (*Drive to 55 Tennessee*, 2014). My results show that students (or their institutions) are likely to respond to these improvements by taking more online courses with the outcome that those who do may not perform as well. If students who choose to take these online courses are similar to those in the past, Tennessee may be able to slightly increase associate degree attainment if more take online courses. Forecasting to 2021, they may see relative increases in bachelor's degree attainment as well. But because post-stratified estimates assume that future students will be much like those of the past, changes in the higher education population may mean degree attainment forecasts represent upper bound estimates since students induced into postsecondary education by new incentives are likely to be less prepared than those who would have attended regardless. In sum, a state like Tennessee can very reasonably use my findings to inform their decisions regarding postsecondary online courses—in fact, the Bayesian framework makes it easier for them to do so (Gill & Witko, 2013; Jackman, 2009)—but policymakers will need to consider their particular state context and their taste for the remaining uncertainty regarding online course delivery and student success.

5.2 Future research

Future research should continue to explore the relationship between broadband access and student engagement with online courses. Only recently have data of sufficient granularity to answer my first research question been available. As more years of data are released, researchers will be able to assemble panel data sets that can better show institutional changes over time and support fixed effects estimation techniques that can produce causal results. One hindrance to this plan lies in the fact that the National Broadband Map (National Telecommunications and Information Administration, 2011), from which I gathered the raw broadband data that I used to answer both the first and second research questions, is no longer being updated.

Other data sources are available, however, that not only may be used in place the National Broadband Map data, but also may represent improvements. One source, from the FCC's Measuring Broadband American program has user-specific data that measures numerous aspects of broadband performance under a wide range of conditions (Federal Communications Commission, 2011a, 2012b, 2013, 2014, 2015b). These data may be able to provide more nuanced understanding of how students' broadband specifications affect their ability not only to watch recorded lectures, but also fully interact in synchronous audio/visual conferences that represent the promise of future online courses (Bowen, 2013; Moore & Kearsley, 2011; Selingo, 2013). That these data are large and may require more sophisticated models to apply to online students represents new challenges, but ones that I believe would be worthwhile to overcome.

Future research relevant to policymakers would also do more to look inside the black box of online courses. Attention has been given to student behaviors in MOOCs, and more research in this area is warranted due to the richness of the data. Yet these courses are idiosyncratic in the higher education sector and may not support direct inferences to degree-seeking students who take an online course as part of their degree pathway. More and better data on degree-seeking student behavior within their courses, even down to the clickstream level, would be a boon to better understanding how online courses may be designed so that the negative effects of online delivery may be mitigated or even eliminated.

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