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The Impact of State-Funded Merit Aid on the Retention of College Graduates*

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Abstract: We investigate the relationship between broad-based merit aid programs sponsored by U.S. state-level governments and the retention of college graduates. The impact of these programs is still debated and research in this field is limited. Using a unique data set for the 1988-2015 period and employing a difference-in-differences approach with adjustments for endogeneity bias, we find that: 1) states offering merit aid have higher workforce retention rates of college graduates, 2) the effect is strongest immediately after graduation and decreases over time, and 3) states with larger spending on merit aid have higher average retention rates of graduates that may be weakening faster over time compared with lower-spending states.

Keywords: human capital, education, merit aid, migration, regional development

JEL Codes: I22, I28, J11, J24, O15, R23

1. INTRODUCTION

Knowledge gained through tertiary education has become one of the fundamental economic resources driving modern economic growth and development. The Organisation for Economic Co-operation and Development (OECD) highlights that "knowledge-based economies" are increasingly reliant on college-educated individuals with very specific skills and scientific acumen for their source of innovation and growth (OECD, 2006). Importantly, Faggian and McCann (2009) recognize that agglomeration of college graduates may have two different economic effects. At the national level, tertiary-educated individuals impact the aggregate productivity through various externalities (or spillovers), whereas at the regional level we may observe a significant "spatial reallocation of factors" due to their increased mobility.[1]

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[1]Greenwood (1997) and Bound and Holzer (2000) are among the first researchers to observe the increased mobility of more-educated people.

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Thus, Faggian and McCann observe that regions will grow when the spillover and mobility effects overlap and will stagnate when these two forces go in opposite directions. This is particularly true in the United States, where availability of skilled labor is the primary factor in deciding the location of new businesses (Gambale, 2019).

The increasing importance of tertiary-educated individuals in the regional workforce across the United States is also evident from the growth in the average share of the population with a bachelor's degree or higher from about 20% in the 1990's to over 30% in 2018 (U.S. Census Bureau, 2020). This increase coincides with many states introducing broad-based merit aid programs intended to increase in-state college enrollments and to retain the college graduates in the state's workforce post-graduation (Zhang and Ness, 2010). As a result, there is a growing literature that investigates whether there exists a causal link between merit aid and college enrollment, completion, and ultimately retention of college graduates. Most of this literature emerged at the beginning of the 2000's and has focused mainly on the issues of enrollment and completion based on mostly single-state analyses (Dynarski, 2000, 2004, 2008; Cornwell et al., 2006; Zhang and Ness, 2010; Toutkoushian and Hillman, 2012; Sjoquist and Winters, 2015).

Taking into account the fundamental role of skilled labor availability in keeping existing businesses as well as attracting new ones (Gambale, 2019; Zhang and Ness, 2010), the more stringent question related to these programs is whether they increase the in-state retention of college graduates. Surprisingly, there is a rather limited stream of literature covering this topic. The main reason for this appears to be the lack of data: most studies investigating the retention of college graduates use Census PUMS data, which only covers the geography of the respondents' subsequent residence or employment, but not the geography of the university they graduated from. As a result, the researchers in previous studies, without having an actual location of the university where these individuals studied, simply assume they attend college in the state of their birth. Given these roadblocks, the literature covering the relationship between broad-based state-funded merit scholarships and the migration decisions of college graduates is still nascent and divided on the final effect of such programs.

Our current study fills several major gaps in this existing literature. We use a unique data set of approximately 2.4 million observations acquired from EMSI (Economic Modeling Specialists International) in which we identify both the location of individuals' most current job as well as the location of the university they graduated from. We believe that using both locations leads to a more accurate estimation of the relationship between access to merit aid programs and post-graduation migration. In addition, our data set allows us to carry out the analysis across all the states and the entire 1988-2015 period, which ensures more consistent control groups for estimating the effect of merit aid programs.

The main contribution of our study is the detailed analysis of the effect of merit aid over time, which has been overlooked in the existing literature. We recognize that retention can be defined differently depending on the temporal perspective. In the more general case, retention is defined based on whether the person chooses a job after graduation in the same state where they went to college ignoring the timing of this decision (all of the existing literature carries out the analysis this way). For a more detailed picture, however, retention should be analyzed taking into consideration the time period between graduation and a job start. This is the approach we take in this study; we analyze the effect of merit aid over time based on discrete one-year intervals within the range of 0 to 10 years after college graduation. As a result, our method of deriving the temporal effect of merit aid on retention of college graduates is more nuanced. Not only does it allow us to determine the magnitude of this effect immediately after graduation, but also the strength and direction of this effect over a period of 10 years past graduation. To the best of our knowledge, no other studies employ such a temporal analysis framework.

Additionally, we compute the effect of merit aid on retention for each of the states with such programs. We then order these results according to spending magnitude to determine differences in the merit aid effect across states with higher and lower spending. We run this analysis based on the individual-level as well as on aggregated state-level data. Importantly, we combine the temporal and regional frameworks in a unique way to better explain the effect of merit aid programs across high- and low-spending states. This nuanced analysis of the regional effect taking into account the temporal dynamics of migration is also missing in previous literature.

In our study, we employ established methods used in the existing literature for assessment of the merit aid effect, however we supplement our analysis with in-depth endogeneity bias controls. In particular, we employ the intent-to-treat difference-in-differences (DD) linear probability model (LPM) with state and year fixed effects.[2] An important aspect missing in the majority of the existing literature that we address in the current study is the endogeneity of the "treatment" variable identifying merit aid. The more recent studies investigating the effect of merit aid (Hawley and Rork, 2013; Leguizamon and Hammond, 2015; Whatley, 2019) recognize that without controlling for potential endogeneity bias the results may not be interpreted as causal. In this study, we find that merit aid is indeed endogenous and, as a result, we adjust our empirical approach to control for endogeneity in several different ways. First, we match the control and treatment groups using a propensity score matching (PSM) technique with a common support and then run the DD models using propensity score-weighted observations in the two groups (Whatley, 2019 uses a similar approach). Next, we employ a two-step instrumental-variable (IV) estimation based on the linear potential outcomes (LPO) model with a binary endogenous treatment variable (Cameron and Trivedi, 2010, Chapter 6).

Lastly, we add an original state-level analysis within the general and regional models to check the robustness of the individual-level results. Because the merit aid treatment occurs at the state level (implementation of merit aid is a statewide decision) we collapse the data to the state level and estimate the effect of merit aid on the retention proportion of graduates in each state using binomial models. We use state-level propensity score weights that make the treatment and control states more comparable as well as employ a similar LPO approach as the one at the individual level to control for endogeneity in these models. No other researchers have looked at this modeling approach to study the relationship between merit aid programs and retention of college graduates.

Our general findings confirm that merit aid has a positive and statistically significant effect on retention. The state-level models also strongly support the general positive effect

[2]These results are robust to those obtained from DD logistic regressions, which we do not report but can provide upon request.

of merit aid. Most importantly, we find that the effect of merit aid is highest immediately after graduation (more than double the general effect of merit aid) and has a decreasing trend over time. Specifically, the effect of merit aid on retention becomes insignificant in the fifth to seventh year range and may become significantly negative by the tenth year post college graduation. These results support the observations made in the existing literature about the increased mobility of the highly-educated individuals in the long term.

Finally, when looking at the regional results, we observe heterogeneity in the effects across states and an interesting contrast between individual- and state-level analyses. Studying the data reported by NASSGAP (2020), we have observed that seven states (mainly from the South East) amass almost 88% of the entire national spending on merit aid programs. This interesting fact prompted us to investigate whether there are differences in effects between these high-spending and lower-spending groups of states. Based on the individual-level data, we find that merit aid programs in higher-spending states have a lower average effect on retention relative to the lower spending ones, a somewhat peculiar result. However, after aggregating the data at the state level and running binomial models on retention proportions, we find that higher spending on merit aid is associated with a higher average effect on retention. Although both the individual- and state-level analyses provide fairly consistent insights on the regional heterogeneity of the merit aid program effects, the state-level aggregation and analysis may be better suited for estimating regional effects given the intent-to-treat framework proposed in existing literature.

Additionally, even the state-level results for specific states indicate that three of the highest-spending states (Georgia, Florida, and South Carolina) have insignificant effects on retention. Although these specific results are also counter-intuitive at first, we show that temporal dynamics of the merit aid effect may explain them. Specifically, when running the regional analysis on the sub-sample of individuals who report starting their job within one year after college graduation we find significantly higher results in the top-spending states. We also observe that the top-spending states have much higher merit aid effects on retention relative to comparable low-spending states. Based on the results from the entire data set as well as the results from the "first job only" subset, we conclude that high spending states have indeed a significantly higher merit aid effect on retention. We note, however, that the effectiveness of these programs on retention may weaken faster over time relative to the effectiveness in comparable lower-spending states. Thus, by combining the temporal and regional frameworks we obtain a unique perspective that allows us to better explain the effect of merit aid programs across high- and low-spending states.

2. LITERATURE REVIEW

The main studies that investigate the relationship between broad-based merit aid and retention of college graduates are those by Hickman (2009), Hawley and Rork (2013), Sjoquist and Winters (2013, 2014), Leguizamon and Hammond (2015), Harrington et al. (2016), and Fitzpatrick and Jones (2016). Hickman (2009) uses U.S. Census Bureau's 2000 decennial census as well as the 1% sample of the American Community Survey (ACS) data for the years 2001-2006 to analyze whether "exposure" to Florida's Bright Futures program (introduced in 1997) affects the residence location decision post graduation. Using binomial regressions with a DD approach, Hickman finds that, on average, exposure to Bright Futures increases the probability of Florida college graduates to reside in-state by approximately three to four percentage points.

In their 2013 study, Sjoquist and Winters perform a single-state analysis of Georgia's HOPE program effect on retention of college graduates. Their significant contribution is using University System of Georgia (USG) data to investigate the share of USG graduates getting jobs in-state. In addition to the USG data, Sjoquist and Winters (2013) use ACS data and estimation procedures similar to Hickman (2009). Based on the analysis of the USG data, the authors find that Georgia's HOPE program is generally associated with a reduction in the state's workforce retention of college graduates compared with the cohorts not having access to this program. The results from the ACS analysis, however, are not uniform: two Hickman-style specifications yield a significantly positive effect on in-state residence, whereas other modified specifications result in an insignificant effect for the HOPE variable.

Sjoquist and Winters (2014) extend Hickman's approach to a 25-state analysis based on the 2000 decennial census as well as the 2001-2010 ACS annual data. The authors classify the 25 states with merit aid programs into strong and weak programs and focus mainly on the strong ones. Using an intent-to-treat difference-in-differences LPM approach, they find that exposure to merit aid increases the likelihood of 24 to 30-year olds to reside in their state of birth by approximately 2.8 percentage points, which increases to almost four percentage points after adjusting the analysis framework to account for potential measurement error. The authors also estimate the effects for each of the strong and weak states and find heterogeneous treatment effects (both within and across the two groups of states).

Another study that looks at the effect of merit aid on inter-state migration of college graduates is that of Hawley and Rork (2013). These authors divide their study into two parts: in the first part they investigate whether existence of merit aid affects in-state enrollment and graduation rates and in the second they investigate whether merit aid programs affect state-level migration rates. Using Integrated Post-secondary Education Data System (IPEDS) panel data and employing a first-difference estimation they find that these programs lead to increases in enrollment, but not in graduation rates.[3] In the second part, Hawley and Rork combine Census decennial data with annual ACS data to create four periods of aggregated 5-year migration flows in 1980, 1990, 2000, and 2009 and investigate the relationship between merit aid and migration flows. They also introduce a temporal dimension by dividing the merit aid variable into four indicator variables identifying the amount of time passed since initial introduction of the merit aid program. Importantly, Hawley and Rork are the first in this stream of literature who recognize the presence of self-selection bias associated with the merit aid variable and try to control for it by lagging their covariates to the year before migration occurs. They find that broad-based merit aid programs (particularly those in place for at least six years) significantly increase in-state retention of recent graduates (although merit aid appears to decrease retention of 35 to 65 year-olds by 1.5 percentage points).

It is important to note that Hawley and Rork (2013) and Sjoquist and Winters (2013, 2014) introduce a temporal dimension in the analysis of the effect of merit aid. Their ap-

[3]This result contrasts with the discussion in Sjoquist and Winters (2014) in which the authors argue that they do not observe self-selection issues as merit aid does not lead to increases in in-state enrollment.

proach, however, is different from the way we carry out this analysis in our current study. Sjoquist and Winters (2013) compare the mean shares of USG graduates employed in the state during the 1990-1991 period (pre-HOPE implementation) and 1995-1996 (post-HOPE implementation) and find no difference in retention rates four years after high school graduation. Moreover, the difference becomes negative and ranges between a decrease of 1.4 to 3.6 percentage points when looking at periods more than four years post high school graduation. Hawley and Rork (2013) instead look at four bands of time passed since the initial introduction of the merit aid program: two years or less, three to five years, six to 10 years, or 11 or more years. These authors also split the effect by age group. They find that the effect is larger on individuals within the younger age group (22 to 25 years old), but decreases in the group with older individuals (22 to 34 and 35 to 65 years old). Sjoquist and Winters (2014) seem to follow in the footsteps of Hawley and Rork and also introduce a temporal dimension in terms of amount of time passed since program implementation, although they do not find any difference in the effect of merit aid with time. We, on the other hand, employ the following tools to provide more nuance and improve the accuracy of estimation of merit aid temporal effects: 1) control for both individual- and region-specific characteristics, 2) take into account the effects of endogeneity, and, importantly, 3) estimate the temporal effect in relation to the graduation date regardless of program inception age.

Both Leguizamon and Hammond (2015) and Harrington et al. (2016) use a regression discontinuity estimation approach with administrative databases for the states of West Virginia and Missouri, respectively. Along with Sjoquist and Winters (2013), these three are the only studies that have single-state data on both the location of the university as well as the employment location post graduation. Leguizamon and Hammond (2015) use data only for the 2006-2007 and 2007-2008 cohorts of graduates, which are matched with employment records for 2008 and 2009. In the case of Harrington et al. (2016), the data are based on the ACT test takers between 1999-2002 and their corresponding employment status eight years later in 2007-2010. In order to avoid the self-selection bias of graduates who enroll in the merit aid program due to their high achievement scores, the authors in both studies use the intent-to-treat approach in which anyone with a specific ACT score would be eligible for the scholarship, but the actual receipt of the scholarship is not observed. Thus, both studies assign individuals with an ACT score of 21 (Leguizamon and Hammond, 2015) or 30 or higher (Harrington et al., 2016) to the treatment group and those with a lower score to the control group. Although the approach is very similar in both cases, the two groups of researchers observe opposite results in the two states. Leguizamon and Hammond observe that West Virginia's program is associated with a reduction in the likelihood of college graduates to be employed in-state (the effect varies between a decrease of approximately four percentage points to almost 10).[4] Conversely, the main conclusion of Harrington et al. is that, in the long-term, having access to the Bright Flight program increases the retention of Missouri's highly talented college graduates by approximately 4.3 percentage points.

The last study we have identified to investigate the relationship between broad-based merit aid programs and migration of college graduates is that of Fitzpatrick and Jones (2016). These authors have a similar approach to that of Sjoquist and Winters (2014). They

[4]Interestingly, Sjoquist and Winters (2014) find that the effect in WV is positive (they report that the program is associated with an increase in retention of almost four percentage points).

use decennial census data for 1990 and 2000 as well as ACS data for the 2001-2010 period and employ a similar DD model with fixed effects as the one used in earlier studies. The main finding is that exposure to merit aid has a modest positive and statistically significant effect on the likelihood that college-graduates live in their state of birth after graduation.

We have performed a systematic analysis of the literature[5] that provides a formal list of states with merit aid programs along with their respective start year (and end year, in some cases) and found many discrepancies both in terms of start dates as well as the list of states having a merit aid program at all. Since a number of studies identify some states with merit aid while others do not and given the discrepancies in start years, we decided to formalize the list of merit aid states based on the overall consensus of the literature. In the current study, we classify the states with merit aid programs (and thus assign individuals who graduated from colleges in these states to the treatment group) in two ways: 1) the first classification is based on an overall consensus in the existing literature (at least five of the nine studies identify these states as having a merit aid program) and it covers a total of 17 states and 2) the second classification includes all the states that have been identified by any one of the existing studies as well as by our own research of state agencies' websites (this classification identifies a total of 32 states with merit aid programs). We believe this structure allows a clearer picture of the final effect of merit aid on the retention of college graduates. Although we present the effects of merit aid separately for each of the two classifications mentioned above, we focus mainly on the results from the first classification for several reasons. First, these results are more comparable to those of previous studies that investigate a similar list of states. Second, these states have the largest expenditures on merit aid and amass approximately 97.3% of all the expenditures on merit aid programs in 2017-2018. Third, given the size of expenditures in these states, we believe they most closely reflect the "broad-based" aspect of merit aid programs.

Lastly, none of the studies investigating the relationship between merit-based programs and migration of college graduates discuss the theoretical foundations of human capital migration. We do so in the remainder of this section. According to Faggian and McCann (2006), there are two models explaining individual migration that are based on the assumption that people migrate to maximize their welfare. The first model is the "human capital theory" and the second one is the "job search theory." The human capital model of migration was first discussed by Sjaastad (1962) and states that individuals invest in migration to increase the productivity of their human resources.

Faggian (2014) provides a good overview of the job search theory, which starts with the seminal works of McCall (1970) and Mortensen (1970) and culminates with the contributions of Diamond (1982a,b), Mortensen (1986), and Pissarides (1979, 1984, 2000). The earlier studies concentrate on finding the condition in which individuals stop searching for a job (when the marginal cost of job search equals the marginal return from continuing the search). The subsequent literature contributes by enhancing the job search theory with the development of a framework in which individuals interact with firms (a framework also referred to as the "matching theory").

[5]Dynarski (2004); Heller and Marin (2004); Zhang and Ness (2010); Toutkoushian and Hillman (2012); Hawley and Rork (2013); Sjoquist and Winters (2014); Domina (2014); Fitzpatrick and Jones (2016); and Whatley (2019).

An important extension of the job search model takes into consideration the migration aspect of job search by adding the geographic location element. In this variation of the model, migration occurs in one of the two states: *ex-ante* (also known as "speculative" migration), in which the individual migrates to another region to search for jobs, or *ex-post* (also known as "contracted" migration), when the individual migrates to another region as a result of having already found a job there (Faggian, 2014). The more recent literature (Basker, 2018) tries to reconcile both types of migration options and assumes that individuals optimize the choice of ex-ante or ex-post migration by maximizing their utility. Using binomial and multinomial regressions, Basker (2018) finds several interesting results: first, more educated individuals have higher mobility rates; second, inter-regional migration is more sensitive to education level than intra-regional migration; third, speculative migration is more characteristic of low-skilled (low education) workers, whereas contracted migration is more characteristic of high-skilled workers; and, finally, speculative migration is more likely to occur (compared with contracted migration) due to weaker economic conditions in the origin region. These important findings inform the decision in our current study of: 1) expanding our analysis period over almost three decades (1988-2015) allowing us to capture the economic cyclicality element observed by Basker, 2) assuming that college graduates are more likely to have job offers on hand at the time of graduation (rather than graduating and relocating to other states in search of jobs), and 3) forming our expectation, based on all of the above, that the impact of merit aid programs should increase the probability of tertiary-educated workers to remain in the state where they went to school.

3. EMPIRICAL APPROACH

3.1. Background

We study the causal effect of merit aid policies on retention of college graduates by taking advantage of the natural experiment framework and implement an intent to treat difference-in-differences (DD) analysis. This approach allows the comparison of the difference in pre- and post-implementation outcomes of the policy (whether individuals stay in the same state after graduation) for the treated group with the difference in pre- and post-policy implementation outcomes for the control group. The treatment group is comprised of individuals who studied in states that had implemented the policy while the control group consists of individuals who were not affected by the policy. In other words, the DD framework is designed to measure the causal effect of a policy (in this case, the merit aid program) when it is not administered as a randomized controlled trial.

One of the reasons for this framework is the lack of data identifying individuals who actually receive merit aid funds. As a result, studies investigating the effects of merit aid on educational and labor market outcomes (enrollment, completion, or retention) employ the intent-to-treat approach in which they study the outcomes on cohorts of students who are either *eligible* or *not eligible* for such merit aid programs. Different studies define eligibility in different ways: some studies simply assume that all those who were born in states with merit aid policies are eligible and are thus included in the treated group (Hickman, 2009; Sjoquist and Winters, 2014, 2015; Fitzpatrick and Jones, 2016), whereas others who work primarily with state-agency data use specific academic test (like ACT) cutoff levels to determine eligibility for such funding (Leguizamon and Hammond, 2015; Harrington et al., 2016). In this paper, we also take advantage of an intent-to-treat framework in which we assign individuals to the treatment group based on their eligibility to receive merit aid funds, which depends on the location of their university as well as the year when they graduated. Thus, we estimate the effect of access to the merit aid scholarship rather than the effect of actually receiving merit aid funds.

There are two different levels at which we assess the effect of merit aid programs on retention: 1) the individual and 2) the state level. We carry out analyses at the individual level based on three major model specifications for each of the two merit aid definitions (as explained in sections 2 and 4.3). The first model specification at the individual level is the DD model that measures the effect of merit aid on the migration decision in general, while controlling for additional individual- and region-specific characteristics. We refer to this specification as the *general model*. We then apply a similar framework to study the relationship between merit aid programs and the migration decision accounting for the amount of time passed between graduation and current job start year. We call this second specification the *temporal model*. Finally, we investigate the relationship between merit aid programs and the migration decision of individuals within each of the merit aid states to determine whether there are important differences in the effect of merit aid across states. We refer to this third specification as the *regional model*.

Additionally, since our data set does not contain variables that identify individuals' actual receipt of merit aid funds, our individual-level treatment group may be measured inaccurately.[6] As a result, we collapse the individual-level data to the state level and use binomial models for state retention proportions (similar to the general and regional models at the individual level) to estimate the effect of merit aid programs on states' proportion of retained college graduates. To the best of our knowledge, we are the first to use this approach as a refinement over individual-level data analyses used in earlier literature.

3.2. Individual-level Models

The dependent variable in our study, individual's decision to stay in the same state after college graduation, is binary: the job is either inside or outside the state where they went to school. Using mathematical notation, the dependent variable, $stay_{ij}$, is equal to 1 if person i stays in-state and 0 if they leave within $j \in \{1, 2\}$ merit aid classification version. Thus, we can model a person's decision to migrate by estimating their probability of staying in-state after graduation using the linear probability model (LPM), which is straightforward to implement and interpret.

While there are some drawbacks to the LPM, a logistic regression analysis is significantly more complicated to be implemented with discrete endogenous variables, which we explain in more detail in section 3.4. Nevertheless, we have run logistic regressions in our general and temporal model specifications (explained below) and have obtained very similar results to those from the LPM. Importantly, previous studies in the merit aid program evaluation

[6]There are other sources of measurement error that we discuss in more detail in section 4.4.

literature (Dynarski, 2000, 2004, 2008; Sjoquist and Winters, 2013, 2014, 2015) have also widely employed the LPM approach. Thus, using the LPM approach makes our results more comparable to those reported in earlier literature.

As explained above, we do not observe the person's actual receipt of state merit aid funds, but rather their access to such funds, making this an intent-to-treat framework. Assuming that individuals take four years to graduate and following the approach used in previous literature (Hickman, 2009; Sjoquist and Winters, 2013), we consider that all individuals graduating from college in a state that offered merit aid four years prior to graduation would have had access to such funds. This assumption could potentially lead to a sample selection bias because the treatment group may be inflated with out-of-state students who would not have had access to such funds. This is because most of the merit aid programs were designed for in-state high school graduates only. More specifically, this assumption may create upward bias in our treatment effect estimates if the misidentified treated individuals decide to stay in the same state where they graduated from college. Alternatively, it could also create downward bias if the misidentified treated individuals leave the state; for example, they may be returning to their home states. Sjoquist and Winters (2014) also recognize that some broad-based merit aid programs were initially targeting recent in-state high school graduates but have eventually evolved to allow even out-of-state students to qualify under specific conditions. As a result, we perform our main analysis with the assumption that all individuals who studied in a state that was offering merit aid at the time had access to such funding, but in section 4.4 we discuss further adjustments we make to the treatment group to determine whether this measurement error affects our general results significantly.

In our *general model*, we take the simplest specification of the DD framework and expand it by including a list of individual- and state-specific covariates to control for systematic differences in the treatment and control groups. This improves the estimation of the causal link between the merit aid program and tertiary-educated individuals' migration decision. The general model equation is given as follows:

$$stay_{ijkt} = \beta_{0j} + \beta_{1j} M_{jkt} + \beta_{2j} X_i + \beta_{3j} Z_{kt} + \gamma_{jk} + \tau_{jt} + \phi_{ij} + u_{ijkt}, \tag{1}$$

where β_{1j} is the main parameter of interest that measures the effect of merit aid on the probability of individual i to stay in-state; M_{jkt} is the binary merit aid status variable identified based on state k, graduation year t, and merit aid definition j; γ_{jk} and τ_{jt} represent state and graduation year fixed effects that allow expression (1) to represent a DD framework[7]; X_i and Z_{kt} are the vectors of individual- and state-specific controls, respectively; ϕ_{ij} represents the individuals' academic major fixed effect. We discuss these additional covariates in more detail in section 4.3. Thus, expression (1) represents our main specification, which we implement with minor adjustments in our consecutive models.

Commonly, studies implementing a DD regression framework cluster standard errors to control for serial correlation (Sjoquist and Winters, 2013, 2014; Hawley and Rork, 2013; Fitzpatrick and Jones, 2016). The argument here is that serial correlation can bias the significance of the estimated model coefficients and, thus, clustering reduces the over-rejection of the null hypothesis (i.e. increases the standard errors). On the other hand, the choice of

[7]See Conley and Taber (2011) for more details on the DD framework in which there are multiple treatment groups.

the clustering dimension can lead to overly conservative estimates of the standard errors. We observe that the choice of the clustering dimension varies in the existing literature (Sjoquist and Winters, 2013 use both clustering at the birth year as well as at the state, whereas Sjoquist and Winters, 2014, Hawley and Rork, 2013, and Fitzpatrick and Jones, 2016 cluster at the state). However, no apparent rationale for the choice of clustering dimension is provided in any of these papers.

Interestingly, Abadie et al. (2017) raise several important issues related to clustering. First, there should not be any apparent choice of clustering unit (geographic, temporal, or any other dimension may be equally valid for clustering). Second, the motivation for clustering should be weakened when the regression specification includes fixed effects (which are supposed to control for a common shock). And, third, clustering should matter in situations when the units of analysis are sampled in such a way that there are clusters in the population of interest that are not present in the sample and whether the assignment to treatment was clustered. Cameron and Miller (2015) also argue on page 334 that "*If the model includes cluster-specific fixed effects, and we believe that within-cluster correlation of errors is solely driven by a common shock process, then we may not be worried about clustering. The fixed effects will absorb away the common shock, and the remaining errors will have zero within-cluster correlation.*" Following the recommendations in Abadie et al. (2017) and Cameron and Miller (2015), because we have assigned individuals to treatment both using the geographic as well as the temporal characteristics of the treatment, we decided to cluster the standard errors at the graduation year dimension.

While the general model described above helps us determine the effect of merit aid programs on overall retention, it does not take into consideration the amount of time elapsed between graduation and the start date of the current job listed on the individuals' resume/CV. This is important because a person may have graduated college and relocated to a different state (in which case the state offering the merit aid would not have retained this individual post-graduation) and some years later they may return back to the state where they went to college at which point their resume/CV may be picked up in the data gathering process. In such an example, if we ignore the timing between graduation and job start, we may incorrectly assume that the respective person stayed in the same state where they went to school. However, if we evaluate the outcome variable by properly controlling for the time element then we can capture the effect of merit aid programs on retention over time (with the focus being on individuals' migration decision within the first year post graduation). This nuanced temporal analysis is missing in previous literature.

Traditionally, one is interested in the migration decision based on the location of the person's *first* job post graduation, which usually takes place within a one year period. In our data we limit the employment time-frame post graduation to 10 years (i.e. the individual's start year of the current job[8] is listed in the interval $[0, 10]$ years after having graduated from college) because we want to investigate what happens to the effect of merit aid on retention of college graduates as time passes. Based on the job search and job matching theories (discussed in section 2), it is clear that workers are reevaluating their utilities across time

[8]Note that our data only capture the person's most current job information. We do not have information on all the jobs held by the respective person. However, we assume that for those individuals who indicate obtaining the job within a year after college graduation the current job is the first job.

and constantly searching for opportunities to maximize their welfare. Also, based on the findings in Mincer (1958, 1974), workers gain more experience over time, which allows them to seek better-paid opportunities around the country. On the other hand, it may also be argued that as time passes the social and professional networks that individuals form locally may decrease their likelihood of relocation (Bartel, 1979; Hickman, 2009). Nevertheless, recent literature on human capital migration emphasizes the enhanced mobility of educated people (Basker, 2018), which leads us to believe that, within relatively short periods of time (up to a decade) post graduation, educated workers have a higher regional mobility. Thus, we expect merit aid to be most effective in retaining college graduates within a year of graduation and have a decreasing effect as the number of years since graduation increases.

Based on the above, in our *temporal model*, we adjust expression (1) by fully interacting the time period indicator with the merit aid policy indicator to evaluate the effect of access to a merit aid program on retention over the period of time from graduation to job entry. Mathematically this temporal model can be represented as follows:

$$\begin{aligned} stay_{ijkt} = \;& \beta_{0j} + \beta_{1j} M_{jkt} + \delta_{1j} T_i + \delta_{2j}(M_{jkt} * T_i) + \beta_{2j} X_i + \beta_{3j} Z_{kt} + \gamma_{jk} + \tau_{jt} + \\ & + \phi_{ij} + u_{ijkt}, \end{aligned} \tag{2}$$

where T_i is the categorical variable that identifies the number of years that had passed between graduation and the start of the current job (ranging between 0 and 10 years, inclusive) for person i and δ_{2j} is a vector of the main coefficients of interest within the j^{th} definition of merit aid. Thus, δ_{2j} together with β_{1j} and δ_{1j}, measure the effect of merit aid on retention at specific periods of time of current job entry.

During our research of the existing literature we have determined that not all merit aid programs are the same, especially when it comes to the amount of spending on these programs. Some states spend significantly more than others. In fact, when we matched the 17 states in the first version of merit aid classification with the spending during 2017-2018 reported by NASSGAP (2020), we found that they amass approximately 97% of the entire nation's merit aid spending in this year. Importantly, when looking at a subgroup of seven states (mainly from the South East region of the US, we refer to this as the "*7-state region*"), which were among the first to implement such programs (Arkansas, Florida, Georgia, Kentucky, Louisiana, South Carolina, and Tennessee) we see that they amass approximately 90% of the total spending by the 17 states (or 88% of the national spending). In other words, within the 17 states with a merit aid program, about 41% of them spend 90% of the total merit aid funds. As a result, it is important to determine the differences in merit aid effects on retention across these two high- and lower-spending groups of states.

In our *regional model*, we add the interaction of M with an indicator variable for each specific merit aid state according to classification version j to investigate the effect of this policy within states offering merit aid (Sjoquist and Winters, 2014 also estimate the effect of merit aid within specific states using a similar approach to ours). We specify this model as follows:

$$\begin{aligned} stay_{ijkt} = \;& \beta_{0j} + \beta_{1j} M_{jkt} + \theta_j(M_{jkt} * state_k) + \beta_{2j} X_i + \beta_{3j} Z_{kt} + \gamma_{jk} + \tau_{jt} + \\ & + \phi_{ij} + u_{ijkt}, \end{aligned} \tag{3}$$

where θ_j is the coefficient of interest that, along with β_{1j} and the respective state's dummy coefficient from vector γ_{jk}, measures the effect of merit aid on the migration decision of tertiary-educated workers for each state k offering a merit aid program according to merit aid classification version j.

3.3. State-level Models

The additional step that we take to check the robustness of our results obtained from the individual-level general and regional models is the estimation of the merit aid programs' effect at the state-level. We do this because at the individual level we use an intent-to-treat approach in which we do not observe the actual treatment: whether individuals actually receive any merit aid funds. In this case the clearer assignment to treatment can be done at the state level so that there is a one-to-one relationship between the treatment and the units being treated: the states that choose to either implement a merit aid program or not.

At the state level, we continue to use the DD method with controls (similar to the individual-level models, but without the individual level covariates) to see how the proportion of graduates who stay in state after graduation is affected by the presence of the merit aid program. The dependent variable in this setting is the proportion of college graduates who choose to stay in-state, defined as the count of graduates who stay in state k divided by the total number of college graduates in state k. Assuming normality of the data, one could estimate a linear model based on this setup. However, after testing for normality of the in-state proportion data (using Kolmogorov-Smirnov, Cramer-von Mises, and Anderson-Darling tests) we determined that proportions do not follow a Gaussian distribution. As a result, we employ a binomial regression model as follows:

$$ln(\tfrac{\pi_{jkt}}{1-\pi_{jkt}}) = \beta_{0j} + \beta_{1j}M_{jkt} + \beta_{2j}Z_{kt} + \gamma_{jk} + \tau_{jt}, \tag{4}$$

where π_{jkt} is the proportion of graduates in state k and graduation year t who stay in-state, β_{1j} is the coefficient that measures the effect of merit aid on retention in state k according to treatment classification version j, Z_{kt} is the vector of state-level covariates (which includes *bachelors_pct*, *unempl_rate*, and *violent_cr*; see section 4.3 for details), and γ_{jk} and τ_{jt} represent state and graduation year fixed effects, respectively.[9]

Baum (2008) provides further explanations for this methodology. We implement this analysis using Stata's "*glm*" command, specifying the distribution of the dependent variable to be *binomial* and the link function to be *logit*. Importantly, the GLM framework at the state-level also allows us to adapt expression (4) according to that in (3) so that we can use the state-level data to estimate the regional model for each of the merit aid states. Thus, we are able to directly compare the individual-level results from the general and regional models with those obtained from the state-level GLM general and regional models.

3.4. Endogeneity

In earlier studies of the relationship between merit aid programs and migration decisions of college graduates (Hawley and Rork, 2013; Leguizamon and Hammond, 2015) as well as in a study that investigates the relationship between merit aid programs and students'

[9]We thank an anonymous reviewer for valuable input in refining our state-level analysis approach.

participation in study-abroad programs (Whatley, 2019), the researchers point out that to estimate a causal link effectively one needs to take into account the bias introduced by the endogeneity of the merit aid treatment. Endogeneity may arise either out of simultaneity with other observable factors influencing the migration decision or due to self-selection (or omitted variable bias), in which case there may be an endogenous process with unobservable factors captured by the error term. An example of simultaneity is when the states' decision to offer merit aid may be driven by overall labor market conditions as well as overall level of human capital, which along with merit aid also influence the migration decision of new college graduates. Self-selection bias may arise due to various reasons. For example, we may need to adjust for self-selection if we believe that a merit aid policy is associated with a higher enrollment of in-state high school graduates. This in turn biases the effect on retention because the migration decision of in-state college graduates may be driven by unobserved characteristics, such as the structure of the person's social network, that are fundamentally different from those of out-of-state graduates. Another way in which self-selection may arise is when individuals who self-select to go to college as a result of availability of merit aid are systematically different at a more personal level (e.g. intellectual ability or ambition) from those who go to college when such funds do not exist. As a result, these two groups cannot be compared directly.

Consequently, such situations should be analyzed within a counterfactual framework in which the treated individuals are compared with the treated individuals had they not been treated (the counterfactual, which is not observable but can be estimated based on existing covariates linking the two groups). Given the nature of the data, economic literature corrects for this endogeneity bias using either a type of two-step estimation with instrumental variables (IV), by taking differences in panel data, or by using propensity score matching (PSM). We ran a series of Durbin Wu-Hausman tests for endogeneity in our sample and found that exposure to merit aid exhibits strong and statistically significant endogeneity.

These results call for more complex models than the simple unadjusted LPM with fixed effects to estimate the causal effect of merit aid on the migration decision of tertiary-educated individuals. Importantly, dealing with endogeneity in binomial models (i.e. logit or probit) is generally more complicated than in linear regression models (hence our choice of LPM). For example, Stata's "*ivprobit*" model (the workhorse of binary endogenous models) is not recommended for estimating the effect of exposure to merit aid on the binary outcome because it works consistently only with *continuous* endogenous variables (Baum et al., 2012; Dong and Lewbel, 2015), whereas the variable identifying merit aid treatment is *discrete*. As a result, we turn to two other methods that adjust for endogeneity in linear models: the linear potential outcomes (LPO) model with a binary endogenous treatment variable and the propensity score matching (PSM) model.[10]

We use one variable, *lottery*, to instrument merit aid. This binary instrumental variable equals 1 if the respective state has a lottery earmarked for K-12 or higher education, and

[10]Stata implements the LPO model with the "*etregress*" command. We also use the "*twostep*" option to produce two-step consistent estimates as well as calculate bootstrapped robust standard errors based on 100 replications (in the case of the general model). Stata's sub-manual on *etregress* available at https://www.stata.com/manuals/teetregress.pdf provides more details related to the theoretical underpinnings of the LPO as well as a more thorough explanation of how the two-step estimation procedure takes place.

zero otherwise. We build this instrument based on the observations made in Bell et al. (2020). According to these researchers, there are 25 states that have implemented lotteries earmarked for education purposes (including higher education) as early as 1970. Since the lottery described in Bell et al. (2020) applies to general education purposes, it follows that it is only partially correlated with the merit aid policy (very few states have introduced the lottery with the sole purpose of funding their merit aid programs: Arkansas, Florida, Georgia, Montana, Oklahoma, and West Virginia). In fact, the correlation coefficient between the second classification of merit aid that includes 32 states offering a merit aid program and the lottery variable is 0.37 (and even smaller at 0.34 for the merit aid variable identified based on the first definition).

It is important to note that: 1) many states that do not offer merit aid scholarships do in fact have lotteries earmarked for education purposes, 2) in many states that do offer merit aid scholarships the lottery earmarked for education purposes has been introduced at a significantly different time than the merit aid program, and finally 3) many major states offering a merit aid program do not have a lottery program earmarked for education purposes. For example, although the merit aid programs in Arkansas and Oklahoma were introduced in 1991, the lottery IV takes value 1 for these two states significantly later in 2009 and 2005, respectively (similar observations can be made about California, Delaware, Idaho, Maryland, Montana, New Jersey, and South Dakota). Also, Arizona, Connecticut, Maine, North Carolina, and Oregon are coded as having a lottery in place even though they do not offer any merit aid programs, whereas Alaska, Louisiana, Michigan, Mississippi, and Nevada that have significant merit aid programs have not implemented a lottery earmarked for education purposes according to Bell et al. (2020). Thus, we believe that these important characteristics make the *lottery* IV a good candidate for our endogeneity adjustment models.[11]

The second popular method of dealing with endogeneity involves some type of matching between the treated and non-treated groups of observations. Using the PSM model is a common way of achieving the balance in these groups by ensuring that they are similar on average across all their covariate values. According to Rosenbaum and Rubin (1983), the propensity score represents a conditional probability that a specific observation in some data set is assigned to some treatment condition given a vector of observed covariates. Having information on the propensity score of assigning to treatment one can match treated and control observations based on several mechanisms (nearest neighbor, kernel, radius, or stratification). Whatley (2019) and Ridgeway et al. (2017) also explain that using propensity

[11] Since this is the only instrumental variable employed, we use the technique described by Cameron and Trivedi (2010, Chapter 6, pp. 197-199) using the "*ivregress*" Stata command to test whether it is a weak instrument. We use this technique across both classifications of the treatment and find that: 1) the R-squared in the first stage is equal to 0.81 in version one and 0.73 in version two, 2) the partial R-squared (between merit aid treatment and *lottery* controlling for the other covariates) is equal to 0.19 and 0.04, respectively, and 3) the F statistic for the joint significance of the instruments excluded from the structural model equals 197,185 in version one and 104,126 in version two of the treatment. All of these statistics indicate that the *lottery* variable is indeed a strong instrument for the merit aid endogenous treatment. Finally, when running the "*etregress*" command in Stata, the vector of variables used in the first stage regression (vector W discussed in the Stata manual referenced in footnote 10) includes the subset of *unempl_rate* and *violent_cr* main covariates along with the *lottery* instrument (see section 4.3 for more details on these variables).

score estimated weights along with control variables in linear regressions provides a "doubly robust" estimation of the treatment effect.

Accordingly, we use the propensity score method to create weights for each observation in our data in order to balance our treatment and control groups in such a way that it mimics the random assignment in a randomized experiment. We then incorporate these weights within the LPM framework (and the binomial one at the state level) for each of the models described above to derive the average treatment effects on the treated (ATT's).[12] We employ similar PSM and LPO endogeneity adjustments in our state-level models (described in section 3.3).

4. DATA AND VARIABLES

4.1. Description of Data

We use an original pooled cross-sectional data set produced by Economic Modeling Specialists International (EMSI), a leading labor market analytics firm[13], to construct our dependent variable as well as several individual-specific covariates (we discuss this process in more detail in section 4.3). EMSI gathers these data from various private and public databases, however, due to proprietary and confidentiality reasons, they do not provide a detailed list of these sources.[14] The entire database comprises the profiles of over 100 million persons in the United States, which typically include information related to their employment location (city/county/state), company information, job details and history, educational information, and skills. Unfortunately, we do not have access to information related to individuals' gender, race, marital status, household size, and other demographics.

After a rigorous data cleaning and filtering, our analysis sample contains 2,396,986 unique observations for persons who have earned a bachelor's degree over the 28-year time period between 1988 and 2015 inclusive.[15] Please note that the data set from EMSI only covers

[12]Due to the disconnect between the level where treatment is assigned (state) and the units of analysis (individuals), we use only the list of individual-specific covariates (T, *uni_type*, *major*, and *occupation*, which are discussed in detail in section 4.3) to determine the propensity scores and their respective weights. We only use the *occupation* variable to derive the propensity score of individuals to be assigned into the merit aid treatment, but we do not use it in any other models due to its limited predictive value.

[13]See www.economicmodeling.com for more details.

[14]Despite the high level of confidentiality, we trust the validity of the data given earlier use in other reputable publications (Florida, 2015; Tsvetkova et al., 2019; Dunn and Dunn, 2019; Matherly and Rodriguez-Garriga, 2020) as well as in popular media (Dougherty et al., 2018).

[15]We start with a sample of approximately 12.3 million observations for all levels of education in which both the IPEDS identification code as well as the location of the current job are present. We then drop: 1) observations with education levels that are different from bachelor's degree (since the main focus of the big majority of broad based merit aid programs is on four-year college education), 2) duplicates (individuals can report several levels of education, including several bachelor's degrees; in the case of multiple bachelor's degrees we work only with the latest degree profile), 3) observations with missing degree level that could not be determined from the textual description of the educational degree, 4) observations with degrees from private institutions in states that do not offer merit aid funds for private colleges (we use information from Dynarski, 2004, Zhang and Ness, 2010, Fitzpatrick and Jones, 2016, and state agency websites to identify states that allow using merit aid funds in private colleges), 5) observations in regions other than the 50 states and the District of Columbia, 6) observations for which the job start year occurs before the

employed persons; however, since we are investigating the relationship between merit aid programs and the job location choice of tertiary-educated *workers*, this is the appropriate sample to use in our analysis. Also, we only have information on the person's current job. Ideally, we would want to have information on all jobs and their specific location since graduation, however, we can make inferences whether the current job can be classified as the first job after college based on the period of time between graduation and job start years (both of these are available in our data set).

We source the data for region-specific covariates from secondary databases as follows. Data on human capital agglomeration come from the Educational Attainment tables reported by the U.S. Census Bureau (Census).[16] Data on the unemployment rate come from the Bureau of Labor Statistics (BLS).[17] Finally, we retrieved data on the violent crime rate from the U.S. Department of Justice, Federal Bureau of Investigation's (FBI) Uniform Crime Reporting Statistics.[18]

4.2. Data Set Limitations

The lack of demographic characteristics (age, gender, race, etc.) in the EMSI data set creates several complications. First, without this information it is impossible to determine how well the data align with the actual labor markets. Second, these can be important controls without which the estimation of the merit aid policy effects may not be entirely accurate.

However, existing Census PUMS as well as state agency data on individuals' education and subsequent employment are still generally messy. Studies employing PUMS data have information on the individuals' current residence and place of work as well as place of birth, but no information about their place of high school and university education. On the other side, researchers who use state agency databases have richer information that includes the individuals' high school, college, as well as employment information, however these data are limited only to public education institutions and local labor markets, making the analysis across various types of institutions and across multiple states impossible. Sjoquist and Winters (2015) provide further limitations of state-agency data. In our EMSI data set, we have information on the individuals' employment as well as university education, including locations, however due to personal information confidentiality we do not know their demographic characteristics as well as where they were born. Clearly, each type of data set has its merits and limitations and estimation of policy effects will not be entirely accurate in any case.[19]

graduation year (we are only interested in the job location choice post graduation; Sjoquist and Winters, 2014 also drop observations for individuals still enrolled in college and argue that these persons' job choice location is predicated by different factors than those of people who have completed their education) or the job start year is beyond 10 years after the graduation year, and 7) observations with IPEDS for colleges that are closed and we can not find information on location or university tier level.

[16] Available at https://www.census.gov/topics/education/educational-attainment.html

[17] BLS Local Area Unemployment Statistics available at https://www.bls.gov/lau/data.htm

[18] Available at https://ucr.fbi.gov/crime-in-the-u.s and http://www.ucrdatatool.gov/

[19] Interestingly, studies employing PUMS Census data do not include a comprehensive list of demographic controls either. For example, Hickman (2009), Sjoquist and Winters (2013), and Sjoquist and Winters (2014) control only for sex, age, and race and lack other important characteristics like household size, parents' education, socioeconomic status, or the person's academic major, which are not normally available in such data sets.

Despite these limitations, we obtain estimates of the general effect of the merit aid policy that are close (and in some cases more conservative) to those estimated by others (Hawley and Rork, 2013; Sjoquist and Winters, 2014; Fitzpatrick and Jones, 2016). Importantly, in our regional model, we obtain policy effects for merit aid states that are highly robust to those reported by Sjoquist and Winters (2013), Leguizamon and Hammond (2015), and Harrington et al. (2016), who use proprietary state-agency data on graduates as well as their employment profiles (i.e. the most accurate labor market data available).

In addition, the individual-specific variables that we employ have not been previously used in other studies. Specifically, we control for individuals' detailed academic major and type of university (high versus low tier institution) as well as the amount of time passed between graduation and current job start (in the temporal models), which are missing in the multi-state studies based on PUMS data. Given this high similarity in the general and regional results from our data and the results in existing literature, particularly the studies based on state agencies in Georgia, Missouri, and West Virginia, we strongly believe in the overall validity of our data.

4.3. Description of Variables

Given the limitations discussed above, all our individual-specific variables are related to the person's quality of education, academic major, year and state of graduation, and the period of time passed between graduation and job start years (please note that we use individuals' academic major, graduation year, and state of their university as fixed effects). We measure the person's quality of education based on the type (or tier) of university they graduate from using the Carnegie 2018 Basic Classification.[20] Abel and Deitz (2012) observe that colleges and universities produce information spillovers that affect positively the demand for human capital in the region. Since the higher tier institutions would generate more R&D (which in turn is one of the sources of the regional information spillovers; a fact also observed by Faggian and McCann, 2006) compared with other higher education institutions, it follows that these institutions have a more significant effect on retention of human capital in a state's workforce. Also, we follow the existing literature in the choice of other region-specific variables: 1) state human capital agglomeration (measured by the share of people with a bachelors degree or higher in the state's population aged 25 or higher); 2) state unemployment rate; and 3) state violent crime rate (used as a proxy for overall level of amenities in the state). Table 1 summarizes the list of these variables.

As explained in section 2, we create two versions of the merit aid classification (or treatment) as follows:

1. Within the first treatment version (overall consensus – at least five out of the nine studies in the existing literature list the state) there are 17 treated states: Alaska (1999), Arkansas (1991), Florida (1997), Georgia (1993), Kentucky (1999), Louisiana (1998),

[20] The EMSI data set provides information on the individual's university IPEDS code, which we use to link with the Carnegie Classification database (available at https://carnegieclassifications.iu.edu/) in order to determine the institution's tier level. According to Carnegie's classification, we identify both the "Doctoral Universities: Very High Research Activity" and the "Doctoral Universities: High Research Activity" as higher tier institutions and all others as lower tier.

Table 1: List of analysis variables

Variable name	Description
stay	Dependent variable (discrete). Equals 1 if the state of the job matches the state where the person went to college.
M_{jkt}	Main independent variable (discrete). Equals 1 if the state k had a broad-based merit aid program at time t using j^{th} definition of merit aid.
bachelors_pct	Control variable (continuous). Measures the school state's share of individuals with a bachelors degree or higher at the time the individual graduated from college.
uni_type	Control variable (discrete). Equals 1 if the university where the individual studied is a higher tier university (as defined by Carnegie Classification).
unempl_rate	Control variable (continuous). Measures the school state's unemployment rate at the time the individual graduated from college.
violent_cr	Control variable (continuous). Measures the state's violent crime rate per 100,000 people at the time the individual graduated from college.
T	Control variable (categorical; 11 time periods ranging from 0 to 10). Measures the amount of time in years between graduation year and the job start year.
major	Control variable (categorical; 45 categories). Measures the academic major fixed effect.
grad_year	Control variable (categorical; 31 years). Measures the year fixed effect.
school_state	Control variable (categorical; identifies the 50 states and the District of Columbia). Measures the state fixed effect.

Maryland (2002), Massachusetts (2005), Michigan (started 2000, ended 2008), Mississippi (1996), Missouri (1997), Nevada (2000), New Mexico (1997), South Carolina (1998), South Dakota (2004), Tennessee (2003), West Virginia (2002).

2. Within the second treatment version (lower consensus) there are 32 treated states (these are all the states that were identified in any of the nine studies in the existing literature plus any new states we were able to identify in our own research of the state agency web sites): Alaska (1999), Arkansas (1991), California (2000), Delaware (2005), Florida (1997), Georgia (1993), Idaho (2001), Illinois (started 1999, ended 2004), Kentucky (1999), Louisiana (1998), Maryland (started 2002, ended 2005), Massachusetts (2005), Michigan (started 2000, ended 2008), Mississippi (1996), Missouri (1997), Montana (2005), Nevada (2000), New Hampshire (1999), New Jersey (1997), New Mexico (1997), New York (1997), North Dakota (1994), Oklahoma (1991), Pennsylvania (2014)[21], South Carolina (1998), South Dakota (2004), Tennessee (2003), Texas (started 2010, ended 2019), Washington (started 1999, ended 2006), West Virginia (2002), Wisconsin (1999), Wyoming (2006).

By definition, all the states defined as treated (offer merit aid) in version 1 are also included in version 2 treatment group of states. However, because there is an incremental number of states in the second treatment version, the results that we obtain from the analysis within each version differ. Although we run the analysis for both treatment versions, we focus mainly on the first one due to: 1) its consensus within the existing literature, 2) these states

[21] Since we only consider data between 1988 and 2015, all individuals graduating in 2015 or earlier in Pennsylvania are counted as part of the control group.

Table 2: Summary statistics of main analysis variables

	(1) All Mean	(2) *treatment*=1 Mean	(3) *treatment*=0 Mean	t-statistic (2) vs. (3)
Dependent variable:				
stay	0.6017	0.6137	0.5988	18.57***
	(0.4896)	(0.4869)	(0.4901)	(0.000)
Main variable:				
M_1	0.1916	–	–	–
	(0.3936)	–	–	–
M_2	0.4123	–	–	–
	(0.4922)	–	–	–
Control variables:				
T	3.5870	3.3701	3.6384	-55.71***
	(3.0434)	(2.9006)	(3.0741)	(0.000)
bachelors_pct	0.2763	0.2525	0.2820	-420.55***
	(0.0504)	(0.0407)	(0.0508)	(0.000)
uni_type	0.4928	0.5322	0.4835	59.51***
	(0.5)	(0.4990)	(0.4997)	(0.000)
unempl_rate (in %)	6.5078	7.1834	6.3477	225.69***
	(2.1205)	(2.3042)	(2.0420)	(0.000)
violent_cr	456.8922	520.5071	441.8132	318.48***
	(188.5132)	(137.7282)	(195.6473)	(0.000)
N	2,396,986	459,299	1,937,687	

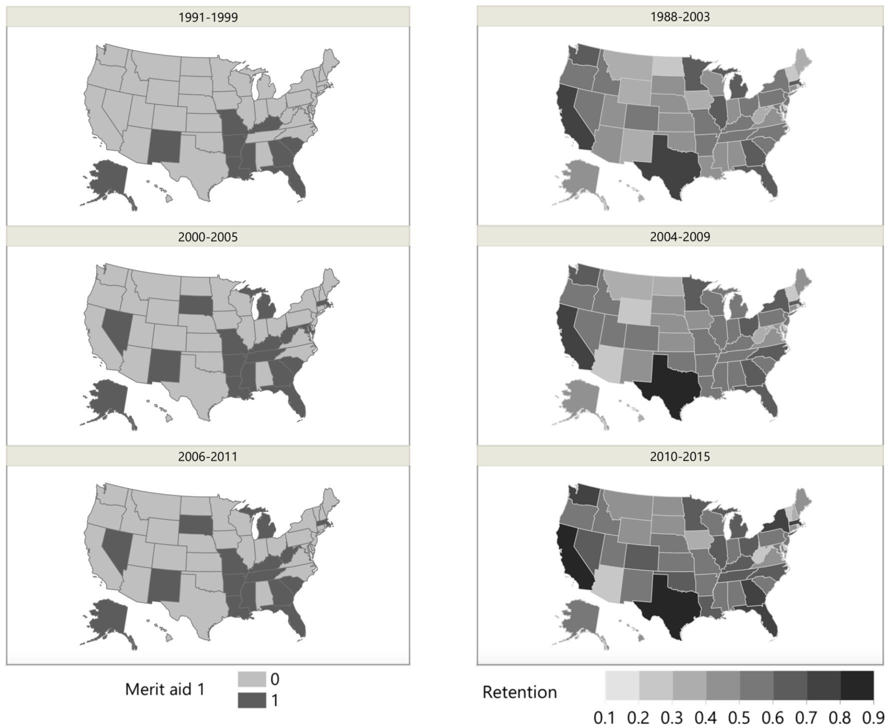
Notes: Standard Deviations in parentheses for means and p-values in parentheses for t-statistics. *** $p < 0.01$.

amass more than 97% of the entire national spending on merit aid programs in 2017-2018, and 3) these states align closer with the "broad-based" aspect of merit aid programs.

Table 2 provides descriptive statistics for our analysis variables. In the second column we show the average values for the entire data set, whereas in the third and fourth columns we present the averages within the treatment and control groups.[22] We compare the summary statistics of the main analysis variables across the control and treatment groups in the last column to determine whether there are any significant differences between the two. All variables present statistically significant differences in means across the treatment and control groups. The raw difference in the outcomes of the two groups indicates that individuals in the merit aid group are approximately 1.5 percentage points more likely to stay in-state compared with the control group. Importantly, these results provide a strong indication of the systematic differences in the two groups: treated individuals graduate in states with a significantly lower human capital agglomeration (approximately 25% for the treated group versus 28% for the control group) although they do so from higher ranked universities (average tier level is 0.53 for the treated versus 0.48 for the control group), the labor markets appear to be much weaker in the treated group (unemployment rate is approximately 7.2%

[22] Please note that we do this only for treatment version 1. Summary statistics and t-statistics for treatment version 2 are available upon request.

Figure 1: Merit aid introduction and retention of college graduates across the United States.



for the treated group compared with 6.3% for the control), and finally the treated individuals graduate in states with much lower amenities (the violent crime rate for the treated group is approximately 521 cases per 100,000 people compared with a rate of 442 cases in the control group). One other conclusion we may infer from these observations is that running simple LPM models controlling for these covariates may not be enough as we may still have endogeneity bias due to the significant systematic differences in the two groups.

Figure 1 provides further details about the regional and temporal heterogeneity of college graduates' retention and introduction of merit aid programs (according to version 1 of the treatment) across the United States. The left side of Figure 1 presents a column of three maps for the merit aid status of states across the following three periods: 1991–1999, 2000–2005, and 2006–2011. The right column of maps shows the average retention rate across the corresponding graduation periods: 1988–2003, 2004–2009, and 2010–2015.[23] Note that the left column of maps is based on the actual year of program inception, whereas the right column is based on the graduation year. Since we assume a four-year lag between inception of a merit aid program and the first time we observe graduates in the respective state, the two sets of maps are equivalent (for example, the year 2000 in which NV introduced its merit aid program corresponds with the graduation year of 2004 in the right column maps).

This set of maps shows that retention has increased across most of the states (particularly

[23]We show the average retention rate by state in the right map starting in 1988 rather than 1995 (the corresponding graduation year for the earliest merit aid implementation year of 1991) to capture the information from all the available data.

in most of the states with established merit aid programs). Taken together, we can see further motivation for our expectation of a positive relationship between merit aid and retention of human capital: it appears that states that have introduced merit aid programs have generally increased their retention of college graduates over time.

4.4. Measurement Error Adjustments

At the beginning of section 3.2 we explain that one of our main assumptions is that *all* individuals studying in a state offering merit aid have access to such funds. However, this assumption may potentially lead to sample selection bias because we are confounding the out-of-state students who should not be counted inside the treatment group, as they would not normally have access to such scholarships, with in-state students who should. If an out-of-state student who is erroneously counted as treated decides to secure a job in the same state as the university this would lead to an upward bias on the estimate of the merit aid effect; conversely, if the same student decides to relocate to another state (for example, they return to their home state) our assumption may in fact bias the estimate of the merit aid effect downward. Thus, to fully account for this bias we also need to control for the individual's in-state status based on the location of their high school.[24]

In our data set, we have identified only 1,706 individuals (out of the 2.4 million) with information on both their high school location as well as the location of the university. We ran our general and temporal models on this significantly smaller subset of the data to determine whether there is an upward or downward bias on the merit aid effect. Based on this sub-sample we observe a significant increase in the effect of merit aid on retention within these two models.[25] However, given the extremely small subset, we decided to consider another type of adjustment. Using data from the U.S. Department of Education, we have identified the share of in-state enrollment in higher education institutions between 1994 and 2015 across all states. We then compute median in-state enrollment shares during this period for each state and use these shares to adjust the number of treated individuals within each state accordingly.

As a result of this adjustment, we are effectively reducing the share of the treated individuals in each state to match as closely as possible the median in-state enrollment rate. The resulting means of the adjusted merit aid variables decrease slightly from 0.1916 to 0.1581 and from 0.4123 to 0.3459 in treatment versions one and two respectively. Please note, however, that since we are randomly adjusting each individual's treatment status there is a need for multiple iterations of this random assignment for a more accurate analysis. Although in

[24] Another potential source of measurement error may be related to our assumption that it takes a person four years on average to complete a college degree. Of course, there may be individuals requiring less or more than four years. However, given the much wider range of possibilities and the lack of data on the share of students graduating at various time frames, it becomes extremely complex to randomly adjust our observations to reflect this highly varying graduation time. This is further complicated by the fact that many programs have time limits for applying. For example, Florida high school graduates have a total limit of five years to apply for the first time to the Bright Futures program, Georgia high school graduates have a 7-year limit for the HOPE program, whereas Louisiana high school graduates must enroll in the TOPS program by the first semester following one year since high school graduation.

[25] We do not report these results in section 5, but they are available upon request.

section 5 we present the results from our main models based on one such random adjustment of the merit aid variables, we also ran 100 simulations in which we randomly adjust the merit aid variables as described above to evaluate the robustness of the adjusted merit aid effect within the general model only. In section 5, we discuss this analysis in more detail.

5. RESULTS

Recall that at the individual-level we propose three models: 1) the *general* model, which ignores the temporal effects; 2) the *temporal* model, which includes control for the amount of time passed after the individual graduates from college; and 3) *regional* models, in which we estimate the effect of the merit aid scholarship in each of the states with such programs as well as investigate difference between the high- and low-spending on merit aid states. The temporal and regional models represent important contributions we make to the existing literature. Finally, the state-level models are intended to provide robustness checks related to potential measurement biases as well as a potential refinement over the intent-to-treat framework at the individual level (for both the general as well as the regional model frameworks).

In all of these models, we estimate the effects using two versions of the merit aid treatment assignment, however we focus our discussion on the results obtained in version 1 of the merit aid. The results in each table are grouped by estimation method (LPM, PSM, and LPO or GLM, PS adjusted GLM, and LPO, for individual- and state-level models respectively) within each merit aid version. Also, the tables are split to show the results using the data without any measurement error adjustment as well as with the adjustment based on the median in-state enrollment discussed in section 4.4.

In the interest of improving readability, we only report coefficients for the M_{jkt} term, which measures the effect of merit aid programs. We present the results in the form of average marginal effects (AMEs), or average treatment effects on the treated (ATT's) in the models adjusting for endogeneity.

5.1. General Model Results

The results of the *general model* shown in Table 3 indicate the overall effect of access to merit aid programs (without taking into consideration the amount of time that has passed after graduation). According to this table, merit aid has a generally positive and statistically significant effect on retention (even after adjusting for measurement error). In particular, based on the regular LPM model, individuals are approximately 1.2 percentage points more likely to stay in-state after graduation in states that offer merit-based scholarships compared with graduates in the control group. This effect drops slightly to approximately 0.8 percentage points in the PSM model and to approximately 1.1 percentage points in the LPO model.

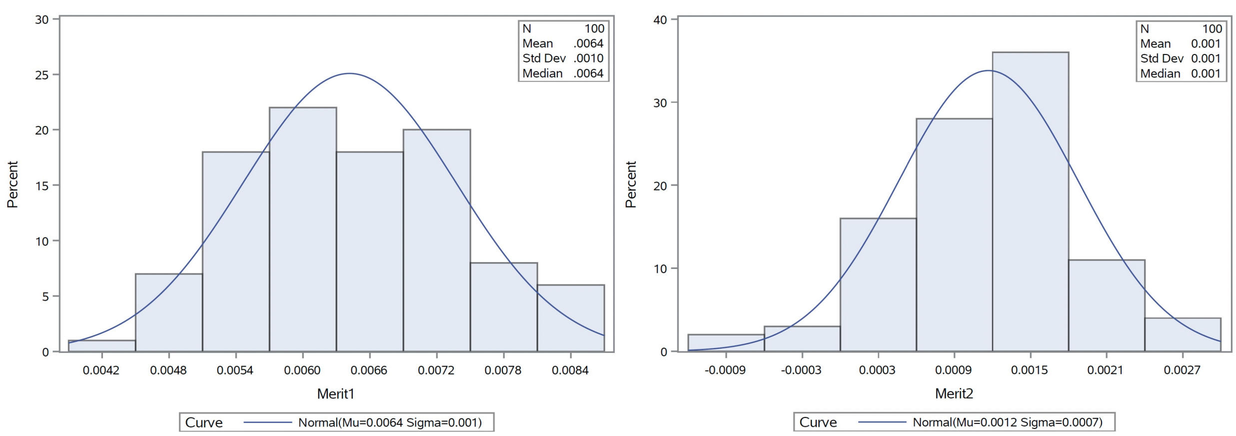
Importantly, after adjusting for measurement error in the treatment group, we observe a slight decrease in the results of the LPM and PSM models and a slight increase in the results obtained from the LPO model. The LPO model results increase from 1.1 to 2 percentage points in the case of treatment version one and from 0.9 to 1.1 percentage points in the second version of the treatment. As explained in section 4.4, we ran 100 simulations in which we

Table 3: Individual-level general model

In-state Adjustment	Merit aid version 1			Merit aid version 2		
	LPM	PSM	LPO	LPM	PSM	LPO
No	0.0116**	0.0079*	0.0114**			
	(0.005)	(0.004)	(0.005)			
				0.0036	-0.0001	0.0093**
				(0.005)	(0.005)	(0.004)
R^2	0.0927	0.0740	–	0.0927	0.0898	–
N	2,396,986	2,396,986	2,396,986	2,396,986	2,396,986	2,396,986
Yes	0.007*	0.0051	0.0197***			
	(0.004)	(0.003)	(0.006)			
				0.0021	-0.0001	0.0112*
				(0.004)	(0.003)	(0.006)
R^2	0.0927	0.0740	–	0.0927	0.0898	–
N	2,396,986	2,396,986	2,396,986	2,396,986	2,396,986	2,396,986

Notes: Clustered standard errors in parentheses (bootstrapped robust standard errors for LPO columns). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 2: Distributions of the merit aid effect based on 100 iterations of random in-state status adjustment.



(a) Merit aid version 1 **(b)** Merit aid version 2

re-estimated the effect of merit aid within the general model (using the LPM model) after randomly adjusting for in-state status. We show the distribution of merit aid estimates from these simulations in Figure 2. Notice that our estimates of the merit aid effect in both versions of the treatment, 0.007 ($p < 0.1$) and 0.002 ($p > 0.1$) respectively, are very close to the means of the simulated distributions: 0.0064 and 0.001 respectively. Consequently, we conclude that the measurement error-adjusted results reported in Table 3 as well as in those that follow, are robust.

Table 4: State-level general model

In-state Adjustment	Merit aid version 1			Merit aid version 2		
	GLM	PSM	LPO	GLM	PSM	LPO
	0.0198*** (0.006)	0.0147*** (0.005)	0.0135 (0.018)			
No						
				0.0139*** (0.004)	0.0063 (0.004)	0.0049 (0.011)
N	1,428	1,428	1,428	1,428	1,428	1,428
	0.0210*** (0.006)	0.0149*** (0.006)	0.0108 (0.008)			
Yes						
				0.0152*** (0.005)	0.0060 (0.005)	-0.0003 (0.020)
N	1,428	1,428	1,428	1,428	1,428	1,428

Notes: Robust standard errors in parentheses for *GLM* and *PS adjusted* models. Bootstrapped robust standard errors for *LPO*. Results based on controlling for all fixed effects. *** $p < 0.01$.

In Table 4 we present the results of the *state-level general model* based on the framework in expression (4). Recall that our intent is to check the robustness of our results from the individual-level general model by comparing them with those at the state-level based on the more direct relationship between treatment assignment and treated units. We also implement the in-state adjustment at the state level, by collapsing the individual-level data adjusted according to in-state median enrollments. Overall, we see a similar picture in Table 4 as that in Table 3. The results within the first version of the treatment are positive and statistically significant. The binomial GLM model as well as the one adjusted with propensity score weights reveal that, on average, the proportion of retained college graduates is between 1.5 and 2 percentage points higher ($p < 0.01$) in states offering merit aid than in those without such programs. We do not see any significant changes in these results after adjusting for in-state enrollment status. The LPO model's result is similar to that of the PSM without in-state enrollment adjustment and slightly lower when we adjust for in-state enrollment; the proportion is approximately 1.4 and 1 percentage points higher in merit aid states, in analyses without and with in-state adjustment respectively. Although the LPO results are insignificant, we observe that when we adjust for in-state enrollment the result is significant at the 10 percent level for a one-sided test.

5.2. Temporal Model Results

In order to identify the impact of merit aid on retention more accurately one should consider the location of the college graduate's *first* job, which normally is observed within a year after graduation. This is important because ignoring the timing of the migration decision may understate the true effect of the merit aid policy on retention of graduates. Table 5 contains the results of the *temporal model* in which we take into account the effect of merit aid at

Table 5: Temporal model

Time	Without In-state Adjustment						With In-state Adjustment					
	Merit aid version 1			Merit aid version 2			Merit aid version 1			Merit aid version 2		
	LPM	PSM	LPO	LPM	PSM	LPO	LPM	PSM	LPO	LPM	PSM	LPO
0	0.030***	0.026***	0.024*	0.017**	0.013**	0.026***	0.024***	0.022***	0.045***	0.013***	0.011**	0.026***
1	0.020***	0.017***	0.014	0.013**	0.009	0.022***	0.015***	0.013**	0.035***	0.010**	0.007*	0.023***
2	0.020***	0.017**	0.014	0.018***	0.014***	0.027***	0.015**	0.014**	0.036***	0.015***	0.013***	0.028***
3	0.015***	0.013**	0.009	0.011**	0.007	0.021***	0.011**	0.010**	0.032***	0.010***	0.008**	0.023***
4	0.011**	0.008	0.005	0.005	0.002	0.015***	0.005	0.005	0.026**	0.005*	0.003	0.018***
5	0.006	0.005	0.0003	0.0001	-0.003	0.009*	0.003	0.002	0.024**	-0.0005	-0.003	0.013*
6	-0.002	-0.004	-0.008	-0.011	-0.014**	-0.001	-0.006	-0.007	0.014	-0.010*	-0.012**	0.003
7	-0.008	-0.010**	-0.014	-0.017***	-0.020***	-0.008	-0.013***	-0.013***	0.008	-0.017***	-0.019***	-0.004
8	-0.008	-0.009	-0.014	-0.020***	-0.023***	-0.010**	-0.012*	-0.012*	0.009	-0.020***	-0.021***	-0.007
9	-0.016	-0.020**	-0.022	-0.028***	-0.031***	-0.018***	-0.021***	-0.022***	-0.0001	-0.028***	-0.029***	-0.015**
10	-0.027***	-0.029***	-0.033**	-0.041***	-0.044***	-0.032***	-0.030***	-0.031***	-0.009	-0.040***	-0.041***	-0.027***

Notes: Results based on controlling for all fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

different intervals of time after graduation.

According to the temporal model, the effect of merit aid immediately after graduation ranges between 2.4 and 3 percentage points in the unadjusted models and between 2.2 and 4.5 percentage points in the adjusted models. Importantly, the effects of merit aid immediately after graduation are more than double the general effect of 0.8 to 1.2 percentage points (or 0.7 to 2 percentage points in the adjusted models), where timing of the migration decision post graduation is ignored. This observation emphasizes the importance of controlling for timing as it reveals much higher and statistically significant immediate effects of such policies. None of the studies in the existing literature have investigated such a temporal framework, which highlights another important contribution of our paper.

In addition, we confirm our hypothesis that this effect has temporal heterogeneity: it starts high immediately after graduation and slowly drops as more time passes (even potentially going into negative territory by the 10^{th} year post graduation). According to Table 5, all three estimation methods reveal that the effect of merit aid drops to zero by the fifth to seventh year range after graduation and eventually reaches lows of -3.3 to -2.7 percentage points by year 10 (statistically significant at least at the 5 percent level). When more states with smaller merit aid programs are included (the broader version 2 of the treatment) we observe that the effects get slightly smaller immediately after graduation: approximately 1.7 percentage points ($p < 0.05$) in the LPM model, 1.3 percentage points ($p < 0.05$) in the PSM case, and approximately 2.6 percentage points ($p < 0.01$) in the case of LPO, which also get into negative territory by year 10 (-4.1, -4.4, and -3.2 percentage points for LPM, PSM, and LPO respectively and in all cases statistically significant at the 1 percent level).

One might expect the effect of such policies to attenuate over time, but not necessarily to become negative. In our measurement error-adjusted models, specifically in the case of the LPO selection correction approach, we do show that the effect essentially drops to zero by the sixth year and stays at this level all the way into the 10-year range. In the regular LPM and PSM approaches within the adjusted-sample analysis, we still observe the effect becoming significantly negative by the 9^{th} year post graduation and reaching lows of approximately -3 percentage points in year 10. Existing literature also indicates that merit aid may be associated with increased mobility in the longer term. For example, Hawley and Rork (2013) find that the effect of such policies on retention becomes negative for individuals aged 35-65, particularly in states with more established merit aid programs. Sjoquist and Winters (2013) also show negative long-term effects of the merit aid policy in Georgia. Although, the existing literature does not provide specific explanations for why the effect would become negative in the long term, we believe that access to merit aid may be picking up the unobserved long term impact of higher education in general. Since this program is usually available to high achieving students, these same individuals would be more mobile in the long term as they enhance their human capital (a core observation also made by Basker, 2018).

5.3. Regional Model Results

To evaluate the differences across states that offer merit aid programs we turn our attention to the *regional model* that assesses the effect of merit aid implementation in each of the merit aid states based on the two treatment definitions. We present the results of this model based on individual-level data in Table 6. Here, we can see that there is a variation in signs as well as magnitudes of effects. Within the first version of the treatment, the regular LPM model reveals that 11 out of 17 states have positive coefficients (with nine of them statistically significant), whereas within the six states with negative coefficients only two are statistically significant. The PSM analysis does not demonstrate any major deviations from the LPM results. Turning to the LPO model, we notice that the estimates are generally slightly larger in magnitude than those of the regular LPM, except for Tennessee where the effect gets smaller.[26] We do not report the regional state effects after the measurement error adjustment due to space considerations, however they are available upon request.

An important observation in Table 6 is that we get results that are very close to those in Sjoquist and Winters (2013), Harrington et al. (2016), and Leguizamon and Hammond (2015) who use actual data on location of studies and employment post graduation for the states of Georgia, Missouri, and West Virginia respectively. Specifically, our effect in Georgia is negative and not statistically significant which matches the results obtained by Sjoquist and Winters (2013) using USG and PUMS data. In another study, Harrington et al. (2016), using actual agency data on education and employment of college graduates in Missouri, find that the effect of the state's merit aid program ranges between approximately four and five percentage points. Our results for Missouri show that this effect ranges between approximately three and four percentage points. Finally, Leguizamon and Hammond (2015) also use actual state agency data on education and employment of college graduates in West Virginia and find that the state's merit aid program is associated with a decrease in the retention between approximately four and 10 percentage points, whereas we find a decrease

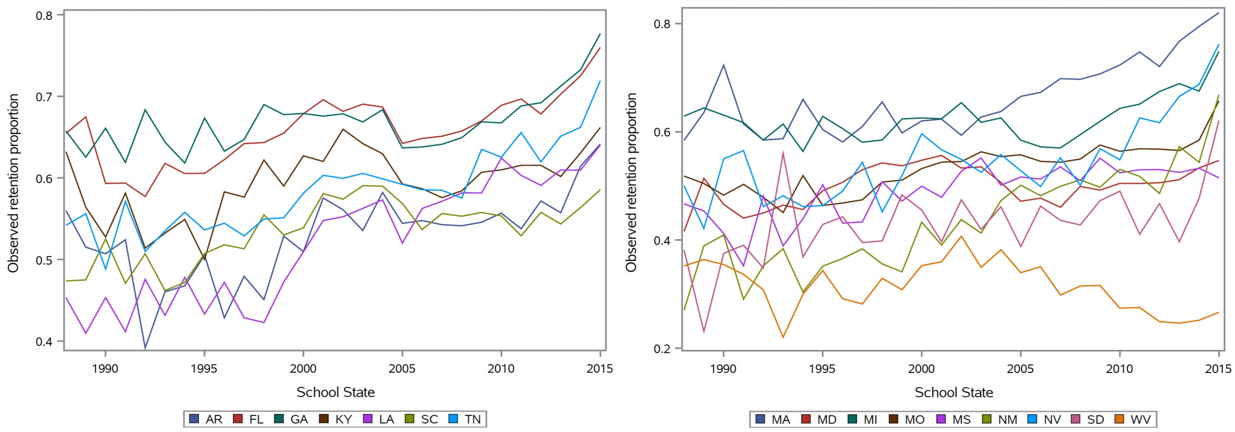
[26] Please note that Michigan's LPO model coefficient (within version one of the treatment) is extremely low at -0.54 ($p < 0.01$); however, this model may be misidentified because according Stata output the log-pseudolikelihood function is not concave for 126 out of the 131 iterations it took to converge. This problem does not occur in the second version of the treatment, where the LPO coefficient for Michigan is -0.0061. Since the regional effects across states in version two do not seem to deviate significantly from those in version one, we believe that the LPO coefficient for Michigan in version one is insignificant (similar to how it behaves in version two of the treatment).

Table 6: Individual-level regional model

	Merit aid version 1			Merit aid version 2		
	LPM	PSM	LPO	LPM	PSM	LPO
7-state Region						
M*Arkansas	0.0311* (0.0167)	0.0272* (0.0159)	0.0315 (0.0238)	0.0299* (0.0167)	0.0263 (0.0164)	0.0360* (0.0194)
M*Florida	0.0073 (0.0056)	0.0012 (0.0056)	0.0114 (0.0109)	0.0078 (0.0067)	0.0079 (0.0064)	0.0111** (0.0052)
M*Georgia	-0.009 (0.0110)	-0.0073 (0.0108)	-0.0058 (0.0119)	-0.010 (0.0114)	-0.0124 (0.0111)	-0.0047 (0.0080)
M*Kentucky	-0.0101 (0.0084)	-0.0173* (0.0096)	-0.0077 (0.0125)	-0.0111 (0.0088)	-0.0138 (0.0092)	-0.0056 (0.0076)
M*Louisiana	0.0780*** (0.0112)	0.0745*** (0.0111)	0.1072*** (0.0120)	0.0779*** (0.0115)	0.0751*** (0.0114)	0.0886*** (0.0080)
M*South Carolina	0.0031 (0.0094)	-0.0002 (0.0095)	0.0056 (0.0112)	0.0025 (0.0093)	-0.0024 (0.0096)	0.0069 (0.0073)
M*Tennessee	0.0312*** (0.0085)	0.0222*** (0.0079)	0.0163 (0.0195)	0.0306*** (0.0087)	0.0259*** (0.0086)	0.0305*** (0.0058)
All other states						
M*Alaska	0.0823*** (0.0298)	0.0814*** (0.0295)	0.0833*** (0.0304)	0.0815*** (0.0299)	0.0757*** (0.0291)	0.0880*** (0.0272)
M*Maryland	-0.0148* (0.0081)	-0.0077 (0.0089)	-0.0188 (0.0148)	-0.0149* (0.0080)	-0.0173** (0.0084)	-0.0093 (0.0068)
M*Massachusetts	0.0671*** (0.0130)	0.0704*** (0.0138)	0.1041*** (0.0123)	0.0671*** (0.0132)	0.0647*** (0.0129)	0.0776*** (0.0076)
M*Michigan	-0.0097 (0.0124)	-0.0101 (0.0121)	-0.5387*** (0.0055)	-0.0103 (0.0127)	-0.0136 (0.0122)	-0.0061 (0.0058)
M*Mississippi	0.0239* (0.0130)	0.0196 (0.0133)	0.0279 (0.0192)	0.0227* (0.0134)	0.0171 (0.0137)	0.0296*** (0.0109)
M*Missouri	0.0380*** (0.0058)	0.0386*** (0.0062)	0.0277* (0.0156)	0.0371*** (0.0058)	0.0358*** (0.0058)	0.0373*** (0.0067)
M*Nevada	0.0613*** (0.0199)	0.0518*** (0.0187)	0.0637*** (0.0222)	0.0598*** (0.0201)	0.0528*** (0.0194)	0.0667*** (0.0178)
M*New Mexico	0.0941*** (0.0157)	0.0908*** (0.0158)	0.0893*** (0.0179)	0.0939*** (0.0154)	0.0923*** (0.0150)	0.0967*** (0.0143)
M*South Dakota	-0.0014 (0.0186)	-0.0042 (0.0202)	-0.0022 (0.020)	-0.0019 (0.0185)	-0.0062 (0.0192)	0.0035 (0.0154)
M*West Virginia	-0.0873*** (0.0167)	-0.0849*** (0.0183)	-0.0812*** (0.0125)	-0.0876*** (0.0165)	-0.0903*** (0.0170)	-0.0783*** (0.0084)
M*California				-0.0045 (0.0072)	-0.0051 (0.0071)	-0.0008 (0.0053)
M*Delaware				-0.0507*** (0.0115)	-0.0578*** (0.0115)	-0.0449*** (0.0108)
M*Idaho				-0.0226 (0.0158)	-0.0273* (0.0162)	-0.0168 (0.0143)
M*Illinois				-0.0434*** (0.0111)	-0.0391*** (0.0112)	-0.0452*** (0.0055)
M*Montana				0.0633*** (0.0175)	0.0612*** (0.0176)	0.0697*** (0.0130)
M*New Hampshire				-0.0235** (0.0095)	-0.0240** (0.0100)	-0.0174 (0.0146)
M*New Jersey				-0.0770*** (0.0117)	-0.0857*** (0.0118)	-0.0750*** (0.0072)
M*New York				0.0623*** (0.009)	0.0614*** (0.0089)	0.0840*** (0.0060)
M*North Dakota				0.0493** (0.0242)	0.0452* (0.0235)	0.0566*** (0.0209)
M*Oklahoma				0.0680*** (0.0177)	0.0665*** (0.0179)	0.0732*** (0.0149)
M*Texas				-0.0121 (0.0094)	-0.0127 (0.009)	-0.0082 (0.0058)
M*Washington				-0.0155* (0.0081)	-0.0155* (0.0081)	-0.0094 (0.0066)
M*Wisconsin				0.0526*** (0.0078)	0.0496*** (0.0073)	0.0651*** (0.0074)
M*Wyoming				0.1029*** (0.0250)	0.107*** (0.0267)	0.1099*** (0.0241)

Notes: Clustered standard errors in parentheses (unconditional standard errors for LPO columns). Results based on controlling for all fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3: Observed retention proportions over time using merit aid 1 definition.



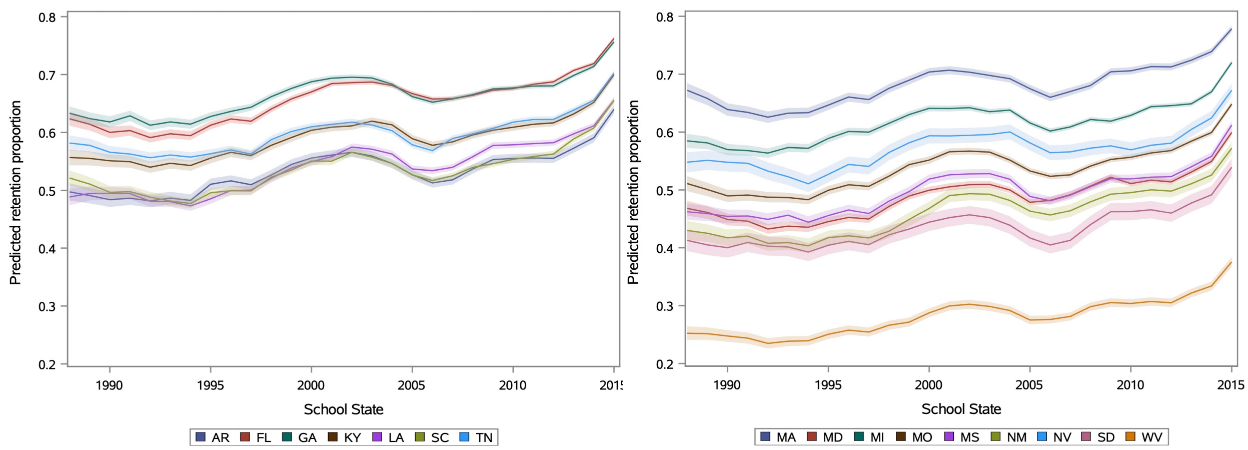
(a) 7-state region **(b)** Other states

in West Virginia's retention of college graduates that ranges between approximately eight and nine percentage points. These extremely robust results reinforce our strong belief in the quality of data used in our study.

Another important observation from Table 6 is related to the *7-state region*. Recall from section 3.2 that roughly 41% of the states in treatment version 1 spend approximately 90% of the total amount. The natural question that arises is whether the impact of this increased spending is higher than in other states with lower levels of spending. As a result, we separate Table 6 into two sections: the top section shows the estimated effects within the 7-state region and the bottom section shows the effects for all other states. Based on the individual-level regional model results, we observe that, on average, the effect of merit aid programs in the 7-state region is smaller than that in the other states. Within the regular LPM model the average effect of the merit aid program inside the 7-state region is approximately 1.88 percentage points and in the other 10 states with merit aid scholarships it is approximately 2.54 percentage points (almost 35% larger). This difference fluctuates after adjusting for endogeneity, but the average effect outside the 7-state region still remains larger than that inside this region (about 71.5% larger within the PSM model and 27% larger within the LPO model). If we ignore the effect of Alaska's program (the characteristics of the educational institutions and labor market in Alaska may not compare well with the rest of the continental U.S.) the average effect in the 7-state region gets closer to that of the other states, although it is still slightly smaller (by approximately 1%, 27%, and 0.3% in each of the three models, respectively). This may potentially raise concerns related to the continued efficiency of merit aid programs in these high-spending states compared with their lower-spending counterparts.

To address this peculiar result, we looked closer at the retention rates in each of these states using state-level data. Figure 3 shows the observed retention proportions over time for the 7-state region vs. all other states using the first (main) definition of merit aid. This comparison shows that the seven states that spend larger amounts on merit aid have a

Figure 4: Predicted retention proportions with 95% confidence bands over time using merit aid 1 definition.



(a) 7-state region **(b)** Other states

generally steady growth in retention proportion over time. There appears to be two periods of growth in this region: the one between the mid 1990s and early 2000s and the second one that started after 2010. During the early 2000s till approximately 2010 all these seven states registered an overall drop in retention. In contrast, all the other states spending less on merit aid show slower growth in retention proportion of graduates over this entire time frame and in some cases, such as West Virginia, even a decrease in retention relative to the early 1990s.

Figure 4 presents predicted retention proportions over time for the 7-state region versus other states using the first merit aid definition. Predicted proportions are obtained based on the model in expression (4) that includes state-level covariates. We can see that the predicted proportions overall show a more unified trend over time, however it is also clear that the high-spending states in the 7-state region generally achieve higher retention proportions of graduates compared to their lower-spending counterparts.

Finally, in Table 7 we replicate the regional model using the state-level aggregated data and the GLM, PS adjusted GLM, and LPO estimation models based on adjusting expression (4) to the specification represented in expression (3). This table further supports our general observations from Figure 4. According to this table, the average effect of merit aid programs on retention proportion across the 7-state region is approximately 2.03 percentage points, whereas that across all other states is approximately 1.89 percentage points higher than in states without such programs (an almost seven percent difference). The average effect across all lower spending states gets even lower when considering the results from the PS adjusted GLM and the LPO (approximately 18 and 33 percent lower, respectively).

Clearly, the regional model results based on state-level aggregate data are more consistent with normal economic expectations. Nevertheless, when comparing individual states in tables 6 and 7 there is relatively little difference. For example, within the 7-state region, the only two states with significant effects (at the five percent level or better) are Louisiana and

Table 7: State-level regional model

	Merit aid version 1			Merit aid version 2		
	GLM	PSM	LPO	GLM	PSM	LPO
7-state Region						
M*Arkansas	0.0227 (0.0176)	0.0208 (0.0179)	0.0137 (0.0189)	0.0235 (0.0177)	0.0181 (0.0181)	0.0143 (0.0186)
M*Florida	-0.0027 (0.0074)	-0.0039 (0.0083)	-0.0051 (0.0080)	-0.0019 (0.0074)	-0.0031 (0.0082)	-0.0053 (0.0080)
M*Georgia	-0.0057 (0.0105)	-0.0081 (0.0105)	-0.0130 (0.0116)	-0.0056 (0.0104)	-0.0052 (0.0110)	-0.0132 (0.0114)
M*Kentucky	0.0010 (0.0105)	-0.0045 (0.0118)	-0.0051 (0.0115)	0.0012 (0.0106)	-0.0069 (0.0116)	-0.0055 (0.0114)
M*Louisiana	0.0792*** (0.0126)	0.0794*** (0.0134)	0.0791*** (0.0148)	0.0803*** (0.0128)	0.0744*** (0.0128)	0.0783*** (0.0143)
M*South Carolina	0.0072 (0.0143)	0.0034 (0.0160)	0.0052 (0.0135)	0.0077 (0.0143)	0.0095 (0.0147)	0.0045 (0.0136)
M*Tennessee	0.0402*** (0.0089)	0.0324*** (0.0087)	0.0323*** (0.0096)	0.0409*** (0.0089)	0.0312*** (0.0087)	0.0321*** (0.0096)
All other states						
M*Alaska	-0.0039 (0.0594)	-0.0097 (0.0608)	-0.0154 (0.0605)	-0.0033 (0.0594)	-0.0192 (0.0607)	-0.0151 (0.0607)
M*Maryland	-0.0238** (0.0111)	-0.0262** (0.0118)	-0.0307*** (0.0114)	-0.0231** (0.0111)	-0.0236** (0.0116)	-0.0304*** (0.0114)
M*Massachusetts	0.0690*** (0.0123)	0.0688*** (0.0120)	0.0551*** (0.0129)	0.0705*** (0.0123)	0.0664*** (0.0121)	0.0559*** (0.0127)
M*Michigan	-0.0174* (0.0102)	-0.0284*** (0.0099)	-0.0280** (0.0114)	-0.0172* (0.0101)	-0.0244** (0.0097)	-0.0279** (0.0112)
M*Mississippi	0.0396** (0.0165)	0.0347** (0.0173)	0.0320* (0.0173)	0.0397** (0.0165)	0.0322* (0.0171)	0.0315* (0.0171)
M*Missouri	0.0302*** (0.0078)	0.0266*** (0.0075)	0.0257*** (0.0084)	0.0308*** (0.0078)	0.0252*** (0.0076)	0.0254*** (0.0083)
M*Nevada	0.0617*** (0.0196)	0.0400** (0.0188)	0.0519** (0.0202)	0.0620*** (0.0194)	0.0416** (0.0195)	0.0513** (0.020)
M*New Mexico	0.1016*** (0.0189)	0.1014*** (0.0190)	0.0957*** (0.0193)	0.1026*** (0.0191)	0.0928*** (0.0198)	0.0958*** (0.0194)
M*South Dakota	0.0145 (0.0216)	0.0130 (0.0217)	0.0062 (0.0226)	0.0164 (0.0216)	0.0054 (0.0218)	0.0083 (0.0225)
M*West Virginia	-0.0824*** (0.0190)	-0.0801*** (0.0220)	-0.0906*** (0.0216)	-0.0809*** (0.0190)	-0.0873*** (0.0211)	-0.0892*** (0.0215)
M*California				0.0054 (0.0095)	0.0006 (0.0095)	-0.0163 (0.0112)
M*Delaware				-0.0629*** (0.0133)	-0.0710*** (0.0153)	-0.0795*** (0.0154)
M*Idaho				-0.0308 (0.0189)	-0.0375* (0.0198)	-0.0371* (0.0199)
M*Illinois				-0.0752*** (0.0146)	-0.0688*** (0.0152)	-0.0836*** (0.0150)
M*Montana				0.0460** (0.0204)	0.0397* (0.0207)	0.0383* (0.0215)
M*New Hampshire				-0.0260 (0.0169)	-0.0327** (0.0164)	-0.0426** (0.0174)
M*New Jersey				-0.0541** (0.0211)	-0.0566*** (0.0216)	-0.0575*** (0.0216)
M*New York				0.0451*** (0.010)	0.0426*** (0.0109)	0.0434*** (0.0111)
M*North Dakota				0.0461** (0.0211)	0.0403* (0.0210)	0.0305 (0.0216)
M*Oklahoma				0.0621*** (0.0169)	0.0540*** (0.0167)	0.0537*** (0.0179)
M*Texas				0.0234*** (0.0072)	0.0166** (0.0072)	-0.0286* (0.0155)
M*Washington				-0.0070 (0.0084)	-0.0104 (0.0081)	-0.0134 (0.0094)
M*Wisconsin				0.0272** (0.0124)	0.0190 (0.0116)	0.0160 (0.0137)
M*Wyoming				0.0951*** (0.0257)	0.0887*** (0.0256)	0.0889*** (0.0271)

Notes: Robust standard errors in parentheses (unconditional standard errors for LPO columns). Results based on controlling for all fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Tennessee with effects of approximately 8 to 10 and 2 to 4 percentage points, respectively. Within the lower-spending states, the states with the biggest effects are Massachusetts, Missouri, Nevada, New Mexico, and West Virginia. Specifically, the effects in Nevada, New Mexico, and West Virginia at the individual and at the state levels are quite similar.

The biggest difference moving from the individual- to state-level data sets is the significant drop in the effect of Alaska's merit aid program, which registers a decrease from an effect of approximately eight percentage points at the individual level to zero at the state level. In general, as mentioned above, the LPO model results at the individual level seem slightly higher than those obtained using either the LPM or PSM methods. However, this is generally not the case in the state-level results, where the LPO model effects are overall lower than those obtained in either the GLM or PSM approaches.

Another important observation from Table 7 is that, although the average effect of merit aid scholarships in the 7-state region as a whole may be bigger than that in the other states, three of the biggest merit aid states (Florida, Georgia, and South Carolina) still report rather insignificantly low effects for their programs. According to NASSGAP (2020), during the entire period of 2004-2017 Florida spent approximately \$4.45 billion, Georgia spent approximately \$5.97 billion, and South Carolina spent approximately \$2.82 billion on merit aid. On the other hand, in the same period, the state of Missouri spent much lower on its merit aid program (approximately \$0.55 billion) and our results show that this program had a significantly higher average effect of approximately three to four percentage points. Even Mississippi has a much higher average effect of approximately two to four percentage points, despite spending only approximately \$0.27 billion during 2004-2017.

These results could be explained by taking into consideration the timing of the job entry after graduation. As we discussed above, an individual may find employment out of state post graduation, but many years later they may come back to their school state; alternatively, a person may stay in-state immediate after graduation but decide to move out in the longer term. Thus, it is important to keep track of individuals' migration timing when analyzing the effect of such policies. In our opinion, the effect of the policy immediately after graduation is the most relevant, as there may be other factors (individuals' family, career, health characteristics, etc.) influencing the migration decision in the long term. However, because we use the data on all individuals in the regional analysis, we are in fact ignoring the timing of the first job post graduation.

To address this, we have also run our regional analysis on the first definition of merit aid using the subsample of individuals who report starting their current jobs within a one year period post graduation (we assume this is the first job after college). We know from our temporal analysis that the effect of merit aid policies is strongest immediately after graduation and, as a result, we expect to see the most clear regional effects for this specific subgroup of individuals. We report the results of this analysis in Table 8. As expected, the average effect inside the 7-state region is considerably higher now than that obtained using the entire data set and it is also higher now than that in the group of states with lower spending. The average effect in the 7-state region is approximately 3.96 percentage points according to the LPM model, 3.2 percentage points using the PSM, and 8.03 percentage points based on the LPO. These levels are approximately 39%, 31%, and 9% higher than the corresponding levels in the other lower-spending states. Importantly, however, we now also

Table 8: Individual-level regional model (first job subsample)

	Merit aid version 1		
	LPM	PSM	LPO
	7-state Region		
M*Arkansas	0.0009 (0.0476)	-0.0099 (0.0469)	0.0497 (0.0553)
M*Florida	0.0483*** (0.0114)	0.0410*** (0.0122)	0.0745*** (0.0173)
M*Georgia	0.0687*** (0.0165)	0.0691*** (0.0164)	0.1032*** (0.0245)
M*Kentucky	0.0006 (0.0143)	-0.0106 (0.0146)	0.0440** (0.0179)
M*Louisiana	0.084*** (0.0213)	0.0794*** (0.0203)	0.1514*** (0.0185)
M*South Carolina	0.0342* (0.0199)	0.0267 (0.0202)	0.0671*** (0.0167)
M*Tennessee	0.0407*** (0.01)	0.0286*** (0.0099)	0.0720*** (0.0149)
	All other states		
M*Alaska	0.0815 (0.0590)	0.0787 (0.0582)	0.1347** (0.0652)
M*Maryland	0.029** (0.0114)	0.0355*** (0.0116)	0.0748*** (0.0171)
M*Massachusetts	0.08*** (0.0128)	0.0829*** (0.0142)	0.1704*** (0.0160)
M*Michigan	-0.0093 (0.0130)	-0.0145 (0.0126)	0.0028 (0.0118)
M*Mississippi	0.0125 (0.0339)	0.0094 (0.0339)	0.0759** (0.0301)
M*Missouri	0.0386*** (0.0096)	0.0364*** (0.01)	0.0744*** (0.0170)
M*Nevada	0.0639* (0.0364)	0.0533 (0.0363)	0.1175*** (0.0424)
M*New Mexico	0.0850*** (0.0229)	0.0828*** (0.0216)	0.1205*** (0.0339)
M*South Dakota	-0.0217 (0.0377)	-0.0237 (0.0415)	0.0286 (0.0316)
M*West Virginia	-0.1167*** (0.0248)	-0.1181*** (0.0262)	-0.0702*** (0.0192)

Notes: Robust standard errors in parentheses (unconditional standard errors for LPO columns). Results based on controlling for all fixed effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

see considerably higher effects in the major merit aid states like Florida, Georgia, and South Carolina, which are considerably higher than the effects in comparable states but with lower spending on merit aid (for example, Maryland, Missouri, or Mississippi).

Finally, the results from Tables 6-8 can be linked to the results from Table 5 in the sense that we see a much higher effect of merit aid programs on the retention rate immediately after college graduation (up to one year), Table 8, for high spending states, and this rate may be going down slightly faster for these states in the longer term (when we consider the results based on all the data, Table 6). In other words, the accelerated decrease in the effect over time explains why some of the top spending states report overall insignificant effects (these results are averaged across all individuals at all time frames in their career). This is an important finding that further supports the need of incorporating temporal dynamics when evaluating regional effects of merit aid programs, something that has been missing in the previous literature.

6. CONCLUSION

This paper provides important contributions to the literature evaluating the effect of broad-based merit scholarship programs on retention of college graduates. We use unique data covering a 28-year span from 1988 to 2015 that allow for a larger scope of analysis and inference. In addition, our data contain both the individuals' location of the university as well as of the job post-graduation; this is an important detail that allows an accurate determination of migration, but which is missing in previous literature. We employ intent-to-treat DD linear models at the individual-level as well as DD generalized linear models at the state-level paying special attention to in-depth endogeneity bias control which is not common in the previous literature on this topic.

Our analysis shows that there is a significant positive effect of merit aid on the in-state retention of graduates. The results from our general model, in which we ignore the timing of the migration decision, indicate that, on average, merit aid programs increase the retention probability of college graduates by approximately one percentage point (even after adjusting for potential sample selection bias). These results are further supported by an original approach in which we collapse the individual-level data to the state level to estimate retention proportions using binomial models. At the more aggregated state level, we analyze the relationship between the share of college graduates who retained jobs in the same state (out of the state's total number of graduates) and the state's merit aid status. According to the results of binomial models, the share of retained college-educated workers in states with broad-based merit aid is approximately 1.5 to 2 percentage points larger than that of states without such programs.

Another contribution we make in the current study is related to the regional analysis of the merit aid effect across states with such programs. Within the states with broad-based merit aid programs, we notice the presence of significant heterogeneity in effects. More importantly, when grouping the states according to their spending levels on such programs, we notice an interesting pattern in the individual-level model results. The individual-level results suggest that low-spending states have a much larger average effect of their programs compared to high-spending states (mostly from the South Eastern region of the United States). However, when running a similar analysis using state-level data, the results are better aligned with normal expectations. We observe that higher-spending states demonstrate, on average, a higher proportion of retained college graduates compared to lower-spending states. This is an important refinement that could supplement future studies of merit aid programs.

The major contribution of our paper to the existing research, however, is the detailed analysis of temporal heterogeneity of the merit aid effect. Evaluating this effect over time is important because human capital migration literature informs us that more educated individuals have a higher regional mobility. As a result, it is not surprising that earlier studies that did not include the time consideration find low effects of the broad-based merit aid programs on retention of college graduates. Our approach, on the other hand, is unique because we analyze the effect across specific discrete time periods since college graduation (one-year increments between 0 and 10 years after college graduation). This important nuance allows us to shed more light on the full potential of merit aid programs in retaining new college graduates.

Based on this original framework, we find that the immediate average effect on the probability to stay in-state can be as high as 4.5 percentage points (more than double the general average effect in which the role of time is ignored). Also, we observe that as time passes this effect drops steadily (as expected) such that by the fifth to seventh year range it becomes insignificant and by the tenth year post graduation the policy may be associated with a negative effect on the probability to remain in-state (a reduction of as much as three percentage points); which supports the observations made in earlier literature on the increased mobility rates of individuals with high levels of human capital.

Importantly, we can further apply the temporal dynamics in the regional model to better identify the average effect across states and across time since graduation. Thus, our regional analysis results based on the subgroup of individuals starting jobs within one year after graduation further confirm the significantly higher average effect inside the high-spending states relative to all the others. Importantly, we can see now that the effect in the high spending states is significantly higher in general as well as relative to comparable lower spending states when considering first jobs out of college, but this effect may be weakening over time (when we include all the data) faster in high spending states relative to the lower-spending ones.

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