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An Examination of Treatment Effects with a Focus on Postsecondary State Merit Aid Programs

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AN EXAMINATION OF TREATMENT EFFECTS WITH A FOCUS ON POSTSECONDARY STATE MERIT AID PROGRAMS

A Dissertation
Presented to
the Graduate School of
Clemson University

In Partial Fulfillment
of the Requirements for the Degree
Doctor of Philosophy
Economics

by
Sherry Meador Jensen
December 2011

Accepted by:
Dr. Thomas Mroz, Committee Chair
Dr. Michael Maloney
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Dr. John Warner
ABSTRACT

With a focus on the impact of postsecondary merit aid programs, this body of research uses a variety of econometric methods to evaluate treatment effects. Since the introduction of Georgia’s HOPE (Helping Outstanding Pupils Educationally) scholarship in 1993, a large number of U.S. states have introduced merit-based financial aid programs. As a growing emphasis is placed on higher education and an increased level of scrutiny is directed toward postsecondary charges, it is important to formally investigate the impact of such programs on the behavior of higher education institutions and students.

Using multiple state merit aid programs introduced over the period 1993-2000, the reaction of postsecondary institutions to the increased ability of students to pay college costs due to newly available merit-based financial aid is investigated. Empirical evidence from difference-in-difference regressions suggests that public, 4-year colleges and universities have benefited from the implementation of merit aid programs. After the introduction of widespread merit aid in a state, public institutions increase the cost of tuition relative to similar institutions in nearby, non-merit aid states. As predicted, the change in pricing is largest in states where the value of the merit aid is tied to the level of tuition and the merit eligible population is large. The impact, however, is not immediate; sizable tuition increases are typically observed to begin in the third year of a merit program.

Next, the Georgia HOPE program is used to examine the reaction of private, 4-year institutions to the introduction of a merit-based financial aid program. In contrast to
an earlier study (Long 2004), this study reveals smaller relative increases (1%-2.7%) in tuition and fees and smaller relative decreases (4.3%-5.4%) in institutional aid at private, 4-year Georgia institutions after the introduction of HOPE through the use of difference-in-difference regressions. While unable to implement significant increases in tuition and fees, Georgia private institutions may have been successful at reducing internal merit awards to HOPE recipients, partially offsetting the state-provided scholarship.

While the prior two chapters focus on merit aid’s impact on postsecondary institutions, merit aid surely impacts the education-related decisions of college-age individuals. Here, the impact of merit availability on high quality military enlistments is examined. Many factors influence the decision of youth to enlist in the United States Army. Among these factors are the educational benefits provided by the Montgomery GI Bill program, which offers participants substantial funds for postsecondary study. Given that publicly provided merit aid is a substitute for military educational benefits, it is expected that the introduction of a state merit aid program will reduce the number of high quality Army enlistees from that state. Using both aggregate-level and micro-level data, an OLS and probit model confirm that the presence of a state merit aid program reduces the number of black, high quality enlistees.

The final chapter of this work seeks to replicate and summarize findings presented by LaLonde (1986), Dehejia and Wahba (1999, 2002), and Smith and Todd (2005) that evaluate the ability of non-experimental data and propensity score matching to reliably estimate treatment effects in social programs. Additionally, an assessment of their results is provided and general concerns regarding the use of propensity scores to facilitate the
evaluation of treatment effects are discussed. As in the previous research, propensity score estimators are applied to data from the National Supported Work (NSW) Demonstration to estimate the treatment effect on post-training earnings of participants. A number of factors are identified that may weaken the performance of the estimator.
DEDICATION

I dedicate this work to my husband Matthew, who has been a great source of support in all of my endeavors.
ACKNOWLEDGMENTS

I would like to acknowledge the guidance and support of my advisor, Dr. Thomas Mroz.
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CHAPTER ONE

THE IMPACT OF STATE MERIT AID PROGRAMS ON TUITION AND FEES AT PUBLIC, 4-YEAR COLLEGES AND UNIVERSITIES

1. INTRODUCTION

Over the past several decades the United States has witnessed a dramatic increase in the cost of acquiring postsecondary education: from 1981 to 2000, average tuition, in real terms, doubled at four year not-for-profit institutions (The College Board 2001). This cost increase not only strongly outpaced inflation, but also surpassed the recorded median family income growth of 27% (NCES 2002). Over this same time frame, financial aid per full-time-equivalent student grew by 82% (NCES 2002).

In the mid-1980s, U.S. Secretary of Education William Bennett publicly suggested that universities consider the availability and magnitude of federal aid awards when determining tuition and other fees charged to students (Bennett 1987). The idea that schools increase the cost of attendance as additional federal aid is made available was hence known as the Bennett Hypothesis. Researchers have tested Bennett’s proposition but with conflicting results. Responses to changes in federal financial aid can be difficult to determine given that the changes are felt nationwide and a control group is difficult to construct.

Federal aid programs\(^1\) are collectively the largest source of student aid, awarding over $100 billion in new aid each year to 14 million postsecondary students. State financial aid programs distributed $10 billion of aid in the 2007-08 academic year (NASSGAP 2008). Although the majority of state aid is awarded based on financial

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\(^1\) In the context of this paper, federal financial aid programs refer to loans, grants, and work-study.
need, merit-based state aid programs have grown increasingly large over the last two decades. 27 states now offer programs that award financial aid based solely on the merit of the student. The funds awarded through these programs represent 19% of the total aid awarded by states (NASSGAP 2008). The variation in state financial aid programs, both in the size and timing of implementation, provides the opportunity to investigate the impact of the programs on both students and educational institutions where the aid dollars are used. Since the introduction of state merit aid programs in the early 1990s, many researchers have examined the impact of increased merit-based financial support on student behavior in a variety of dimensions, including enrollment, choice of institution, major selection, course load, etc. Less attention has been directed to the responses of educational institutions to the increase in availability of merit-based financial aid.

This paper seeks to determine if public, 4-year, postsecondary institutions in states with large, widespread merit-aid programs behave differently than institutions in states without such programs when determining the level of in-state tuition and fees. Responses to the introduction of merit aid are measured by examining the pricing behavior of institutions in merit aid states both pre- and post- merit aid, and contrasting their behavior with the pricing decisions made by similar institutions in nearby, non-merit aid states using a difference-in-difference technique. One would expect the largest changes in relative pricing to occur in states where the value of the merit scholarship is tied to the level of tuition and the merit eligible student population is large. Evidence from Florida, Georgia, Kentucky, Nevada, and South Carolina suggests that after the introduction of widespread merit aid in a state, institutions in that state increase the cost
of tuition relative to similar institutions in other non-merit aid states. The impact, however, is not immediate; sizable tuition increases are typically observed 3 years post implementation of the merit program. This is to be expected as during the first year of a program only incoming students, or approximately one quarter of total enrollees, have access to the merit aid, but by year three approximately 75% of students are potential merit aid recipients.

2. BACKGROUND

Researchers have addressed a number of questions surrounding both need- and merit-based financial aid at federal and state levels. The following sections present an overview of federal financial aid and state financial aid with a detailed focus on the Georgia HOPE (Helping Outstanding Pupils Educationally) program, the state merit aid program that was the direct inspiration for other state programs and the program most widely examined by the literature.

2.1 Federal Financial Aid

The GI Bill of 1944 marked the beginning of the development of federal financial aid for students in the United States. Since its inception, federal financial aid has experienced a vast expansion. Today Federal Student Aid, an office of the U.S. Department of Education, distributes over $100 billion in new aid each year. Hauptman and Krop (1998) highlighted the tremendous growth in the proportion of educational costs that were met by federal aid over the decades. In 1975, federal aid paid for less than one tenth of the total attendance cost at public universities, but by 1995, federal aid covered almost 50% of this cost. Many have suggested that the growth in availability of
federal loans “has facilitated the ability of both public and private institutions to raise tuition dramatically without threatening enrollment levels” (Hauptman and Krop 1998).

Several studies formally address the impact of federal financial aid on college pricing. However, changes in federal aid can be difficult to analyze given that any change impacts all U.S. colleges and universities. Therefore these studies are based on changes to Pell grant eligibility requirements and increases in the size of Pell grant awards. Li (1999) demonstrated a relationship between increased Pell grants and increased tuition at both four-year public and private schools using a unique data set that tracked individual Pell grant recipients and the tuition price at the institutions they attended. Singell and Stone (2007), however, found “little evidence” of in-state tuition increases by public universities but evidence of price increases by private universities and out-of-state tuition charges at public institutions.

2.2 State Financial Aid

In the 1990s, state financial aid programs made a dramatic shift from awarding aid based on need to awarding aid based on merit (Salingo 2001). It was during that time that Florida, Georgia, Kentucky, Nevada, and South Carolina, along with a number of other states, introduced merit-based aid programs. In general, these merit-based programs award financial aid based primarily on high school GPA and in some cases standardized test scores and class rank. The awards range in value from designated, flat dollar amounts up to 100% of the cost of tuition and fees at public, 4-year postsecondary institutions. The awards are renewalable for up to four years based on satisfaction of eligibility requirements. Although a number of the merit programs offer awards that can
be applied at private institutions and 2-year institutions, this study will focus solely on public, 4-year postsecondary institutions. This focus is driven by the larger value (relative to tuition and fees) of a merit award applied at a public institution compared to an award applied at a private institution.  

2.2.1 Georgia HOPE Scholarship  

To date much of the research on the impact of merit-based financial aid has focused on the Georgia HOPE (Helping Outstanding Pupils Educationally) scholarship, first implemented in the fall of 1993 and funded entirely by the Georgia Education Lottery. Eligibility for the HOPE scholarship is based on a cumulative 3.0 high school GPA. Initially, the scholarship covered two years of tuition at public institutions for students whose family annual income was less than $66,000. However, the scholarship later underwent several modifications. In 1994, the program was expanded to include mandatory fees and a book allowance and extended to cover 4 years of costs. In 1995, all income caps were lifted. As of March 27, 2010, approximately 1.36 million scholarship and grant recipients have collected $5.1 billion in aid for the pursuit of higher education in Georgia.  

HOPE’s Impact on the Student  

Dynarski (2000) examined the impact of the HOPE scholarship on college enrollment in Georgia. Using Current Population Survey data, she found that the

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2 For example, when applied at a public university the Georgia HOPE scholarship is worth 100% of tuition and fees, but when applied at a private university the award is worth $3,000, a relatively small percentage of tuition and fees at a private institution. Therefore, the largest pricing effects from the introduction of a merit aid program are expected in the public sector.  

3 General information describing the HOPE program was taken from the HOPE Scholarship Joint Study Commission Report (2003) and scholarship and grant information provided by the Georgia Student Finance Commission (2010).
scholarship increased the probability that an 18-19 year old Georgia resident would attend college by approximately 25%. Cornwell, Mustard, and Sridhar (2006) also investigated the enrollment effects of merit-based financial aid using Georgia’s HOPE program. Using a difference-in-difference regression with a control group of institutions in other Southern Regional Education Board (SREB) states, they found that freshman enrollment was, relatively, 5.9% higher in Georgia due to the HOPE scholarship; the gain was concentrated at four-year schools and largely explained by a reduction in the number of Georgia students attending out-of-state colleges. In fact, Cornwell and Mustard (2001b) identified that from 1993-1997, 96% of HOPE awards went to students who would have attended college regardless of the HOPE award. The award simply affected where, but not whether, the students pursued higher education.

In addition to enrollment effects, there are a number of interesting issues that have been addressed in the literature. Cornwell and Mustard (2001a, 2002, 2006) and Cornwell, Lee, and Mustard (2005) identified varying effects by race and explore students’ responses (chosen major, number of credit hours per semester, etc.) to the merit scholarship. Henry and Rubenstein (2002) looked for changes in the quality of education due to the increased incentive for strong high school performance provided by the HOPE scholarship. They found that the percentage of Georgia high school students earning a “B” average or better has increased since the HOPE scholarship was made available. This grade point average improvement was further validated by increased SAT scores over the same time period, which they suggested points to a quality improvement instead of mere grade inflation (Henry and Rubenstein 2002).
HOPE’s Impact on the Institution

Much of the research on merit aid has focused on HOPE’s impact on the student and not on the educational institutions serving Georgia’s students. A supply-side reaction is expected given the increased ability of students to pay tuition and the increase in demand for Georgia’s universities as more students remain instate to reap the rewards of the HOPE scholarship.

Long (2004) examined the impact of financial aid policies on colleges. She compared how tuition and fees, room and board charges, institutional aid awards, and instructional expenditures changed over time collectively for 37 Georgia four-year institutions relative to a control group of schools in other southeastern states that lacked similar widespread merit aid programs. Because public and private institutes are believed to behave differently, the analysis was completed separately for public and private institutions in Georgia.

After eliminating specialized colleges (music, art, nursing, etc.) and “competitor” colleges and controlling for both state and school characteristics, Long found that after the implementation of HOPE public, tuition and fees at 4-year, public Georgia colleges fell, relatively, by 3%. This result, however, was based on a simple “before” and “after” analysis where tuition and fee pricing four years prior to HOPE was contrasted to charges four years post-HOPE. Such an analysis does not permit the impact of HOPE to vary over time. Long did examine an additional specification, which retained the grouping of

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4 Alabama, the District of Columbia, Delaware, Florida, Kentucky, Maryland, Mississippi, North Carolina, South Carolina, Tennessee, Virginia, and West Virginia

5 Colleges outside of Georgia with at least 5% of their incoming freshman class from Georgia.

6 Annual per capita income, the percentage of the population holding at minimum a bachelor’s degree, annual unemployment rates, along with the institution’s Carnegie classification and selectivity rating
pre-HOPE years into a collective “before” term but included terms for each individual year post-HOPE. Using this specification, Long found a 7.6% relative increase in tuition and fees in the fourth year of HOPE, which contrasted and contradicted the overall 3% decrease reported in her research. Additionally, the collective grouping of “before” years did not identify if the relative price of Georgia tuition and fees was consistent throughout all of the four years prior to HOPE. This paper will modify Long’s approach and apply the analysis to four additional states to further strengthen general conclusions regarding the impact of merit aid on tuition levels at public, 4-year postsecondary institutions.

2.2.2 Other State Merit Aid Programs

While Georgia’s HOPE scholarship is the merit aid program most examined by the literature, there are numerous works that highlight the impact of merit programs in other states. See for example McDonough, Calderone, and Purdy (2007); Heller and Ramussen (2001); and Ackerman, Young, and Young (2005). The impact of merit aid on enrollments and the distribution of merit aid dollars across race and socioeconomic groups are the most commonly addressed topics. In addition to scholarly articles, almost all states with merit aid programs produce a periodic report, or audit, that provides a detailed account of the respective program and benchmarks it against programs within other states. Appendix A contains details descriptions of each merit aid program examined in this study.

3. THEORY

Almost all educational institutions are non-profit organizations. As such, they are limited by a non-distribution constraint, which dictates that no profits can be
distributed to an individual as a for-profit firm distributes funds to owners (Winston 1999). Given this constraint, an institution’s interest in raising prices may be questioned. However, Hansmann (1980) aptly pointed out that the non-distribution constraint is not as rigid as one might initially presume. For example, profits can be indirectly distributed to individuals through inflated salaries. As an alternative to direct compensation, the potential profits can also be spent on on-the-job consumption of goods preferred by the employee (James 1990). Given that the non-profit produces multiple products (such as institutions of higher learning who supply both undergraduate and graduate educations, athletic teams, etc.), profits from one division of the university may be redistributed to support other divisions that are otherwise unable to self-support (Winston 1999).

The non-distribution constraint ultimately leads the managers of higher education institutions to maximize their own utility from organizational expenditures (James 1990). Numerous researchers agree that pursuit of “excellence” or “prestige maximization” is the ultimate objective of the institution, where excellence is defined as preserving or improving the quality of services provided and prestige exists as a relative position among competitors based primarily on student quality, graduate training, and research (Winston 1999, Clotfelter 1996, James 1990).

It is clear that increased revenue, ceteris paribus, facilitates higher levels of prestige and faculty satisfaction at the institution by permitting additional spending on services that attract high-quality students, salaries, and/or valued on-the-job consumption goods. Further, Rose and Sorenson (1992) found a link between high tuition and
administrative overhead and instructional costs, suggesting that both college administrators and faculty members are important beneficiaries of student revenue.

One way the institution may elect to increase revenue is by increasing tuition and fees. Under normal circumstances a price increase would surely decrease the number of students consuming the institution’s services and potentially impact student quality as other institutions become relatively less expensive. However, in the merit-aid environment, students are sheltered from bearing the full cost of tuition increases due to the substantial value of the merit award. In cases where the award covers the full cost of tuition, students are essentially immune from tuition increases. This decreased sensitivity to changes in price allows institutions to increase tuition without producing large detrimental impacts to enrollment and student quality. Therefore, I predict that the largest increases in tuition and fees will be observed in states where the value of the merit award is tied to the level of tuition, instead of existing as a flat dollar amount that erodes in value as postsecondary prices rise, and where the merit eligible population, which is desensitized to increases in price, is a large percentage of the total student body.

The above theory presumes that each institution is free to determine tuition levels to maximize the institution’s objective function, but such an assumption may not reflect the conditions under which all postsecondary institutions operate. The State Higher Education Executive Officers (SHEEO) reported that a state’s public tuition philosophy can be formalized through one of three channels: the state constitution, state statute, or board rule/policy (Boatman & L’Orange 2006). Such a formalization exists in approximately half of all U.S. states. However, even within this subgroup of states with
formalized philosophies, the role of individuals and/or state entities in determining tuition levels varies greatly. Many states use their governor, legislature, or a statewide coordinating agency as informal consultants when it comes to setting tuition. A small number of states grant full legal authority to such groups. Only in 15 states do individual public institutions have full legal authority to establish tuition without external influence. Of the states examined in this study, only South Carolina falls into this category. However, institutions in the state set tuition and fees under “moderate or limited guidelines established by local or state-level entities” (Boatman & L’Orange 2006).

Georgia, one of the states of interest in this study, falls in the middle of the continuum of tuition autonomy: the Georgia Board of Regents is the governing body that manages public higher education in the state and approves tuition\(^7\) rates for all University System of Georgia (USG) institutions (USG 2010). Although the Board of Regents is technically independent from the legislature, Georgia was one of nine states who reported pressure from the legislature to minimize tuition increases (Boatman & L’Orange 2006). As a group these states suggested to the SHEEO that it is “never wise to seriously upset the General Assembly and/or Governor” as sizable tuition increases can in danger future state appropriations and invite formal restrictions on increases (Boatman & L’Orange 2006). Florida institutions are subject both to a governing board and the legislature, while statewide coordinating agencies oversee tuition and fees in Kentucky and Nevada (Boatman & L’Orange 2006). Increases in tuition may be tempered by the influences outside of any given institution.

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\(^7\) Tuition is defined as “payment required for credit-based instruction and related services charged to all students.”
4. METHODOLOGY

I use a difference-in-difference technique to examine the responses of public, 4-year institutions to the introduction of merit aid programs. To determine if colleges and universities in merit aid states behave differently from other public institutions in non-merit aid states, the following specification is estimated using ordinary least squares on data pooled over the academic years of interest (four years pre-merit, four years post-merit) specific to each state’s merit aid program:

\[ y_{it} = \alpha + \beta T_{treatment} + \gamma Year_{it} + \delta (T_{treatment} \cdot Year_{it}) + \lambda X_{it} + \epsilon_{it}. \]  \hspace{1cm} (1)

Here \( y_{it} \), the outcome of interest, is public in-state tuition and fees at institution \( i \) in year \( t \). \textit{Treatment} is a dummy variable indicating if the institution is located in a merit aid state. Colleges and universities in merit aid states compose the treatment group, while the control group is made up of institutions in nearby, non-merit aid states. Underlying assumptions for difference-in-difference estimation require that time effects are common across both groups, treatment and control, and the composition of the two groups is stable prior to and after the introduction of merit aid (Cameron and Trivedi 2006). \textit{Year} denotes a series of dummy variables, one for each academic year of interest, except the year immediately prior to the introduction of the given merit aid program. This specification also includes interaction terms between \textit{Treatment} and each academic year (\textit{Year}), except the excluded year; thus, \( \beta \) identifies how pricing in the merit state differed from pricing in the control states (nearby, non-merit aid states) immediately prior to the introduction of the merit aid program. This coefficient provides a baseline for comparing relative pricing behavior after the introduction of the merit program. The coefficient on each
Treatment\*Year interaction term displays how the merit state outcomes in each year vary from the baseline difference identified by $\beta$. If the coefficients on the interaction terms using post-merit years are near zero, then the data suggest that institutions in the merit state did not react to the new merit aid by altering their tuition and fee setting policies. However, if these coefficients are non-zero values, evidence suggests that the merit-eligible institutions changed pricing behavior after the introduction of the aid. Further, the difference between the merit state outcome and the control state outcome for a given academic year is calculated as the sum of the baseline effect, as captured by $\beta$, and the coefficient for the interaction between Treatment and the respective Year.

$X_{it}$ is a vector of covariates thought to influence tuition and fees. Following Long (2004), these additional controls include the following state level variables: state appropriations per FTE, state annual per capita income, state annual unemployment rate, and the percentage of the state population that holds at minimum a bachelor’s degree. Controls are also present for the institution’s Carnegie classification and selectivity rating. The listed state characteristics may influence the price of colleges within the state as they reflect both attitudes toward higher education and the opportunity cost of attending college. An institution’s Carnegie classification is used to distinguish among different types of institutions that may display different types of pricing behavior. Finally, the institution’s selectivity rating captures its degree of admissions competitiveness. If admission is highly competitive, institutions may be able to

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8 Barron’s Profiles of American Colleges puts institutions into seven categories: most competitive, highly competitive, very competitive, competitive, less competitive, noncompetitive, and special. Ratings are assigned based on median entrance exam scores, percentage of freshmen scoring above particular SAT and
implement price increases with minimal impact on enrollment. Due to the serially correlated outcomes, error terms are clustered by state.

This model is highly similar to the model used by Long (2004), who explored if the reaction to HOPE varies by year using dummy variables for each year in the study after the implementation of HOPE. Key additions in this paper are dummy variables for the individual years prior to the implementation of the merit program, instead of a collective term for “before” years, to establish baseline relative pricing behavior. The inclusion of these terms reveals if the relative relationship between pricing in the merit state and pricing in the control states was consistent before the introduction of the merit aid. It is also important to allow the impact of the merit program to vary over time. A number of the merit programs experienced changes in award values in their initial years. In addition to the changing value of awards, the percentage of enrolled students with access to merit aid is relatively small in a program’s initial year and increases over time. Because the eligibility to receive funds is most often based on the year of high school graduation, in the first academic year a merit award is offered only the entering freshman class contains potential award recipients. In the following year, approximately 50 percent of students have possible access to merit aid. It is not until the fourth year of the program that nearly all enrollees may potentially receive the state merit aid. Therefore, the introduction of a merit aid program may not immediately alter pricing, as only a relatively small population is impacted by the aid in its introductory year.

ACT scores, percentage of freshmen who ranked in the upper fifth and the upper two-fifths of their high school graduating class, minimum class rank and GPA required for admission, and the percentage of applicants to the freshmen class that were admitted.
5. DATA

This study examines the impact of merit aid programs in the states of Florida, Georgia, Kentucky, Nevada, and South Carolina. Although a number of states introduced merit aid programs in the 1990s and early 2000s, these states were selected for study due to the magnitude of their merit aid programs, both in terms of the dollar value of the awards and the size of the eligible population. A timeline and brief description of the programs of interest are found in Table 1.1, while full profiles of each state’s program are located in Appendix A. Georgia’s HOPE scholarship was the first program to be implemented in academic year 1993-94. Florida’s Bright Futures awards emerged in 1997-98, followed by South Carolina’s LIFE scholarship (1998-99), Kentucky’s KEES awards (1999-00), and Nevada’s Millennium Scholarship program (2000-01). When applied at public, 4-year institutions, the awards range in value from approximately $500 to 100% of in-state tuition and mandatory fees over the time period studied. Recipients are limited to state residents, and the merit aid is generally awarded based on cumulative high school GPA and in some cases standardized test scores and class rank. In all cases, the duration of the award is four academic years subject to continuing eligibility requirements.

This paper uses data from academic years 1989-90 to 2003-04 on 194 public, 4-year postsecondary institutions in 17 states taken from the Integrated Postsecondary Education Data System. The states are California, Florida, Georgia, Illinois, Indiana, Kentucky, Louisiana, Maryland, Massachusetts, Michigan, Missouri, New Jersey, North Carolina, Ohio, Pennsylvania, South Carolina, and Texas.

9 Legislative Incentive for Future Excellence
10 Kentucky Educational Excellence Scholarship
11 Only fully accredited 4-year public and 4-year not-for-profit private educational institutes are used in this analysis. Specialty schools, or those with Carnegie classifications such as schools of art, music, and design; theological seminaries; and other specialized institutions, are excluded from the study because they are assumed to have missions that differ than the typical postsecondary institution.
Education Data System (IPEDS), which compiles annual surveys conducted by the U.S. Department’s National Center for Education Statistics (NCES). In this analysis, IPEDS is used to collect data on tuition and fees, state appropriations, enrollment figures, and Carnegie classification. Selectivity ratings\textsuperscript{13} were taken from \textit{Barron’s Profiles of American Colleges} (1994). Data on unemployment rates, per capita income, and educational attainment by state are collected from the Bureau of Labor Statistics, the U.S. Bureau of Economic Analysis, and the U.S. Bureau of the Census, respectively.

The analysis is completed separately for each merit aid state (Florida, Georgia, Kentucky, Nevada, and South Carolina) and its respective control group over an 8-year time period covering both pre- and post-merit aid availability. Each state-specific treatment group is composed of public, 4-year institutions in the merit aid states. (See Appendix B for a complete list of public, 4-year institutions within each treatment state used in this study.) Each control group is constructed of public, 4-year institutions within non-merit aid states nearby the given merit aid state. (A complete list of the institutions composing each control group is found in Appendix C.) Institutions in nearby, typically bordering states, are thought to operate in an educational environment similar to the merit aid state. Table 1.2 shows summary statistics for each treatment and control group pair in the year prior to the implementation of the specified merit aid program. (Note: all dollar

\textsuperscript{12} Alabama, Arizona, California, Florida, Georgia, Idaho, Illinois, Indiana, Kentucky, Nevada, North Carolina, Ohio, Oregon, South Carolina, Tennessee, Utah, and Virginia

\textsuperscript{13} \textit{Barron’s Profiles of American Colleges} puts institutions into seven categories: most competitive, highly competitive, very competitive, competitive, less competitive, noncompetitive, and special. Ratings are assigned based on median entrance exam scores, percentage of freshmen scoring above particular SAT and ACT scores, percentage of freshmen who ranked in the upper fifth and the upper two-fifths of their high school graduating class, minimum class rank and GPA required for admission, and the percentage of applicants to the freshmen class that were admitted.
values are year 2000 dollars.) This highlights the difference in the level of tuition and fees between each treatment and control group prior to the introduction of merit aid. Figures 1.1-1.5 depict the average level of tuition and fees over time for each merit aid state, its control group, and other U.S. institutions. Although they do not control for institution and state level characteristics, these figures allow for the visualization of tuition and fee growth with easy comparison between the relevant groups.

6. ESTIMATION & RESULTS

The analysis is completed separately for the five state merit aid programs of interest (Georgia, Florida, South Carolina, Kentucky, and Nevada) and their respective control groups composed of nearby, non-merit aid states.

6.1 GEORGIA

Note: First awarded in 1993, the Georgia HOPE scholarship awards 100% of the cost of tuition and fees to qualifying students based on cumulative high school GPA. An income cap initially restricted eligibility but was lifted in 1996.

The first column in Table 1.3 highlights the impact of HOPE on tuition and fees at public, 4-year institutions in Georgia. The model includes interaction terms for Georgia (Treatment) and each year studied except the year 1992-93 (Year -1), the year immediately before the introduction of HOPE. This omitted interaction term provides a base line difference between treatment and control group tuition and fees as captured by the coefficient on the Treatment variable in the model. In 1992-93 before HOPE, tuition and fees were 14.2% higher at Georgia public institutions relative to comparable
institutions in the bordering, non-merit aid states of Alabama, Florida\textsuperscript{14}, North Carolina, South Carolina\textsuperscript{15}, and Tennessee. As observed in the coefficients on $\text{Treatment*Year 1}$ (-0.012) and $\text{Treatment*Year 2}$ (-.004), relative decreases in tuition and fees are observed in years one and two of HOPE. However, by year four (1996-97), a 9.2\% relative increase in tuition and fees is apparent. Notably, this is the year when the income cap was lifted, and HOPE recipients grew in number to 46\% of FTE undergraduate enrollment at public institutions. Using a similar technique, Long (2004) identifies a relative increase of 7.3\% in 1996-97. However, she reports an overall decrease in tuition and fees post-HOPE based on a simple “before” and “after” analysis of Georgia pricing, which fails to identify year-specific effects and contradicts her 1996-97 effect.

6.2 FLORIDA

\textit{Note: Introduced in 1997, the Florida Bright Futures program awards 75\% to 100\% of the cost of tuition and fees to qualifying students based on cumulative high school GPA and standardized test scores.}

Because of the value of the Bright Futures Awards is tied to the level of tuition and the merit eligible population is large due to relatively less stringent requirements, a strong effect is predicted in Florida. Analysis reveals a strong pattern of tuition and fee increases that progressively expand over the first four years of the program. See the second column of Table 1.3 for the results of the Florida specification. Historically, Florida is known as a low tuition state (WICHE 2003), and in the base year (1996-97)

\textsuperscript{14} Florida introduced its Bright Futures program in 1997. Thus, it was a non-merit state during the first four years of the Georgia HOPE program.

\textsuperscript{15} South Carolina introduced the LIFE scholarship in 1998. Thus, it was also a non-merit state during the first four years of the Georgia HOPE program.
immediately prior to the program’s implementation, the estimation results reveal that Florida public institutions were on average 36% lower in cost than institutions in nearby, non-merit aid states of Alabama, North Carolina, Tennessee, and Virginia. Further, the coefficients on the years prior to Bright Futures indicate that tuition was consistently lower in Florida across the pre-Bright Futures period. As observed in the coefficients on Treatment*Year 1 (0.035) and Treatment*Year 2 (0.018), small relative increases in tuition and fees are evident in the first two years of Bright Futures. Increases in cost of larger magnitude and of statistical significance are observed in years three and four of Bright Futures: by year 4 (2000-01), tuition and fees at public institutions in Florida have increased 19.4% relative to the pre-Bright Futures baseline.

6.3 SOUTH CAROLINA

Note: In its first year (1998-99), the South Carolina LIFE scholarship awarded $2,000 to qualifying students based on cumulative high school GPA, standardized test scores, and/or class rank. The scholarship was later increased to $3,000 in the 2000-01 academic year, the third year of LIFE.

View column three of Table 1.3 for the results of the South Carolina model. In the year prior to LIFE, charges at South Carolina public institutions, on average, were 35.2% higher than tuition at fees at similar institutions in nearby, non-merit aid states of Alabama, North Carolina, Tennessee, and Virginia. When the pricing behavior of public, 4-year South Carolina institutions is compared to the control institutions after the introduction of LIFE, a consistent pattern of pricing behavior is not apparent. In the initial year, a 1.6 percentage point increase in the baseline gap of 35.2% between South
Carolina tuition and fees and charges at control institutions is observed. There is a 4.3 percentage point decrease from the baseline gap in year 2, a 9-percentage point increase in year 3, and finally a 7-percentage point decrease in the baseline gap in LIFE’s fourth year. The definitive increases in tuition and fees as observed in Georgia and Florida are not present in South Carolina. However, the LIFE award, when compared to HOPE and Bright Futures awards, has a relatively smaller monetary value. The stricter eligibility requirements also result in a relatively smaller merit eligible population: in the fourth year of LIFE, only 26% of FTE public, undergraduate enrollees are award recipients\textsuperscript{16}.

6.4 KENTUCKY

Note: Since 1999, the Kentucky Educational Excellence Scholarship (KEES) program has awarded $500-$2,500 toward the cost of tuition and fees to qualifying students based on high school GPA and standardized test scores.

Introduced in academic year 1999-00, Kentucky’s KEES program appears to have generated relative increases in tuition and fees at the state’s public, 4-year institutions. See Table 1.3, column four for the empirical results. In 1998-99, immediately prior to the introduction of KEES, charges at Kentucky’s public institutions were approximately 13.4% below charges at comparable institutions located in bordering, non-merit aid states of Illinois, Indiana, Ohio, Tennessee, and Virginia. This gap in the level of charges between Kentucky institutions and charges in bordering states is reduced after the implementation of KEES. As revealed by the coefficients on Treatment*Year 2 (.108) and Treatment*Year 3 (.09), charges grow in years two and three of KEES. Tuition

\textsuperscript{16}This is a relatively small merit eligible population when compared to Florida, where 48% of FTE, public, undergraduate students are award recipients.
levels also remain relatively elevated (0.066) in the fourth year of KEES. Although the KEES award is similar in size to South Carolina’s LIFE award, the larger, more consistent increase in tuition and fees in Kentucky may be attributed to the state’s relatively large merit eligible population of 43% of FTE enrollment.

6.5 NEVADA

Note: The Nevada Millennium Scholarship program was introduced in 2000 and awards $2,500 toward the cost of tuition and fees to qualifying students based on cumulative high school GPA.

Reference Table 1.3, column 5 to view the results of the Nevada analysis. In the academic year prior to the program’s implementation (1999-00), Nevada public, 4-year institutions charged tuition and fees approximately 19% below comparable institutions in the bordering, non-merit aid states of Arizona, California, Idaho, Oregon, and Utah. As observed in the coefficients of Treatment*Year 1 (.026) and Treatment*Year 2 (-.01), which are similar to the results observed in the previously examined states, relatively small pricing changes are observed in the first two years of the program. In the third year of the program, Nevada charges rise from the baseline by 7.7 percentage points, reducing the gap between Nevada and bordering states from -19% to -11.3%. However, this relative increase in tuition is not maintained in Millennium’s fourth year; in 2003-04, Nevada tuition and fees increased by only 1.6 percentage points from the baseline difference in the year immediately prior to Millennium’s implementation. Much like South Carolina, Nevada institutions’ ability to increase pricing may be limited by the
relatively small merit eligible population. Over the first years of the Millennium program, merit aid recipients represent only 12-32% of FTE undergraduate enrollment.

7. CONCLUSIONS

Three of the five states examined show consistent trends of tuition and fee increases following the introduction of a merit aid program. Georgia, Florida, and Kentucky all demonstrate relative increases in charges that emerge primarily in the third and fourth years of each merit program’s life. These relative changes in the level of tuition and fees are measured as the difference from the baseline gap (the percentage difference between charges in the merit state and charges in nearby, non-merit states in the year immediately prior to the program’s implementation) and the gap that exists in each year of the program’s life. The observed increases in tuitions and fees (relative to the year before the respective scholarship program) in years 3 and 4 range from 0.012 percentage points to 19.4 percentage points.

The delayed impact of the merit programs on postsecondary pricing is as expected. Because each program is initially available only to the entering freshman class, who graduated from high school after the program’s creation, only one quarter of the college population has potential access to merit dollars in a program’s inaugural year. After two years, approximately half of students may qualify for the merit aid, and only after four years can the full student body potentially receive merit dollars. Therefore, changes in pricing behavior are expected to be greater in years three and four when the eligible population has grown to a substantial proportion of the student body.
The results of the largest magnitude and statistical significance are observed in Florida, where tuition and fees experienced a relative increase of 18.4 percentage points, on average, in years three and four of the Bright Futures program from the baseline in the year before the program. Stated in an alternate way, although tuition and fees at public, 4-year institutions in Florida where 35.5% below charges in nearby states immediately before the introduction of Bright Futures, the pricing gap was reduced to -16.1% by the 2000-01 academic year. The magnitude of the increase, which is the greatest of all merit states examined in this study, can presumably be attributed to the both the nature of the Bright Futures awards and the relatively large size of the merit eligible student population. The award is a percentage of tuition and fee charges, instead of a flat dollar amount, and the merit eligible population represents 48% of FTE enrollment in the fourth year of the program.

Similar to Florida, Georgia exhibits a sizable relative increase in charges in the fourth year of HOPE when the baseline difference (14.2%) between Georgia tuition and fees and charges in the control states is increased by 9.2 percentage points. Like the Florida Bright Futures program, the Georgia HOPE scholarship awards cover a percentage of tuition and fees, specifically 100% of charges, as opposed to a fixed dollar amount, as observed in Kentucky, Nevada, and South Carolina. This type of award coverage makes award recipients less sensitive to increases in tuition and fees, thus enhancing the ability of the institution to raise tuition and fee levels. It is also important to note that Georgia removed the income cap from eligibility requirements year 4, which further expanded the merit eligible student population to 46% of FTE enrollment.
Increases in the cost of tuition and fees are also evident in Kentucky after the implementation of the KEES program. Although tuition and fees in Kentucky were 13.4% below charges in bordering states before KEES, charges increased by 6.6 to 10.8 percentage points from the baseline gap during years 2, 3, and 4 of the program. Unlike Georgia and Florida, the KEES awards are a flat dollar amount with the maximum award valued at $2,500\(^1\), which covers nearly the full cost of tuition and fees. Although the value of the award is not linked to tuition levels, the impact of the KEES program on tuition and fees may be attributed to the relatively large student population (43% of FTE enrollment) that is eligible for the merit award due to the low GPA and standardized test score requirements. Additionally, the KEES program’s continuing eligibility requirements are the least stringent among the five programs studied. Only a 2.5 GPA is required in the first year of postsecondary study to maintain eligibility. The GPA requirement is increased to a 3.0 in the second year of postsecondary, but reduced awards are available to those students who fail to meet the highest level of continued eligibility requirements.

Some of the least consistent results are observed in South Carolina where a relative increase is observed in year three of LIFE but is not maintained in year four. Notably, South Carolina has the most stringent eligibility requirements. They are based on GPA, standardized test scores, and/or class rank. Over the first four years of LIFE, merit award winners represent only 17-26% of FTE enrollment in South Carolina. This relatively smaller merit qualifying population may limit the institutions’ ability to

\(^1\) $2,500 is awarded to a Kentucky student who maintained a 4.0 GPA each year in high school and scored a 28 or above on the ACT.
introduce price increases without incurring detrimental effects on enrollment and student quality. Also, the award is a flat dollar amount, unrelated to the level of tuition and fees, and this characteristic may dampen the award’s impact on postsecondary pricing.

Nevada also exhibits inconsistent changes in pricing similar to South Carolina. Tuition and fees demonstrate a relative increase (0.077) in year 3 of the Millennium Scholarship program, but the relative increase is not maintained in year 4, when Nevada tuition and fees vary from the baseline gap by only 1.5 percentage points. The Nevada merit award, like the South Carolina LIFE award, is also a flat dollar amount, which is less likely to support increases in tuition and fees because students are not immune to the rising costs. Additionally, the proportion of students receiving merit aid is relatively small (12-32% of FTE undergraduate enrollment) compared to the other states in this analysis.18

The empirical evidence suggests that tuition setting policies at public, 4-year postsecondary institutions change after the introduction of a widespread merit aid program. Although increases in tuition and fees are observed across the nation, these charges are growing faster in states with large merit aid programs. Further, the largest increases are present in states where the merit award is linked to the level of tuition and fees and the merit eligible population is relatively large.

18 Georgia: 46% of FTE undergraduate enrollment, Florida: 48% of FTE undergraduate enrollment, Kentucky: 43% of FTE enrollment
TABLE 1.1 Time Line of Merit Program Implementation

<table>
<thead>
<tr>
<th>Year</th>
<th>State</th>
<th>Program Name</th>
<th>Value of Award at 4-year Public Institution&lt;sup&gt;19&lt;/sup&gt;</th>
<th>Primary Criteria</th>
</tr>
</thead>
<tbody>
<tr>
<td>1993</td>
<td>Georgia</td>
<td>HOPE</td>
<td>100% of tuition &amp; fees</td>
<td>GPA</td>
</tr>
<tr>
<td>1997</td>
<td>Florida</td>
<td>Bright Futures</td>
<td>75%-100% of tuition &amp; fees</td>
<td>GPA &amp; standardized test score</td>
</tr>
<tr>
<td>1998</td>
<td>South Carolina</td>
<td>LIFE</td>
<td>1998-99: $2,000 2000-01 &amp; on: $3,000</td>
<td>GPA, standardized test score, &amp;/or class rank</td>
</tr>
<tr>
<td>1999</td>
<td>Kentucky</td>
<td>KEES</td>
<td>$500-$2,500</td>
<td>GPA &amp; standardized test score</td>
</tr>
<tr>
<td>2000</td>
<td>Nevada</td>
<td>Millennium Scholarship</td>
<td>$2,500</td>
<td>GPA</td>
</tr>
</tbody>
</table>

<sup>19</sup> Award value is given for the respective time period studied, which includes the first 4 years of each program’s life. Value is per academic year and each award is renewal for up to 4 years based on continued eligibility criteria.
<table>
<thead>
<tr>
<th>State</th>
<th>Year</th>
<th>Group [# of Institutions]</th>
<th>Tuition &amp; Fees (Std. Dev.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Florida</td>
<td>1996</td>
<td>Treatment(^{20}) 9</td>
<td>$1,970 (163)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Control(^{21}) 51</td>
<td>$2,852 (1,138)</td>
</tr>
<tr>
<td>Georgia</td>
<td>1992</td>
<td>Treatment 18</td>
<td>$2,105 (284)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Control(^{22}) 58</td>
<td>$2,179 (623)</td>
</tr>
<tr>
<td>Kentucky</td>
<td>1998</td>
<td>Treatment 8</td>
<td>$2,533 (397)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Control(^{23}) 69</td>
<td>$3,761 (900)</td>
</tr>
<tr>
<td>Nevada</td>
<td>1999</td>
<td>Treatment 2</td>
<td>$2,233 (34)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Control(^{24}) 47</td>
<td>$2,598 (887)</td>
</tr>
<tr>
<td>South Carolina</td>
<td>1997</td>
<td>Treatment 11</td>
<td>$3,526 (317)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Control(^{25}) 59</td>
<td>$2,839 (1,075)</td>
</tr>
</tbody>
</table>

\(^{20}\) See Appendix B for the composition of each state sample.
\(^{21}\) Public, 4-year institutions in Alabama, North Carolina, Tennessee, and Virginia
\(^{22}\) Public, 4-year institutions in Alabama, Florida, North Carolina, South Carolina, and Tennessee
\(^{23}\) Public, 4-year institutions in Illinois, Indiana, Ohio, Tennessee, and Virginia
\(^{24}\) Public, 4-year institutions in Arizona, California, Idaho, Oregon, and Utah
\(^{25}\) Public, 4-year institutions in Alabama, North Carolina, Tennessee, and Virginia
TABLE 1.3 The Impact of State Merit Aid Programs on Tuition and Fees at Public, 4-Year Colleges and Universities
Treatment: Institutions in Merit Aid States
Control: Institutions in Nearby, Non-Merit Aid States

Dependent variable: \( \ln(\text{Tuition and Fees}) \)

<table>
<thead>
<tr>
<th>Treatment State</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment*Year 4(^{31})</td>
<td>0.092 (0.074)</td>
<td>0.194 (0.044)</td>
<td>-0.074 (0.091)</td>
<td>0.066 (0.117)</td>
<td>0.016 (0.080)</td>
</tr>
<tr>
<td>Treatment*Year 3</td>
<td>0.012 (0.036)</td>
<td>0.174 (0.053)</td>
<td>0.093 (0.056)</td>
<td>0.090 (0.083)</td>
<td>0.077 (0.049)</td>
</tr>
<tr>
<td>Treatment*Year 2</td>
<td>-0.004 (0.042)</td>
<td>0.018 (0.066)</td>
<td>-0.044 (0.048)</td>
<td>0.108 (0.084)</td>
<td>-0.010 (0.044)</td>
</tr>
<tr>
<td>Treatment*Year 1</td>
<td>-0.012 (0.042)</td>
<td>0.035 (0.049)</td>
<td>0.016 (0.020)</td>
<td>0.065 (0.034)</td>
<td>0.026 (0.032)</td>
</tr>
<tr>
<td>Baseline Difference: T vs. C in Year -1(^{32})</td>
<td>0.142 (0.148)</td>
<td>-0.355 (0.048)</td>
<td>0.352 (0.026)</td>
<td>-0.134 (0.246)</td>
<td>-0.195 (0.286)</td>
</tr>
<tr>
<td>Treatment*Year -2</td>
<td>0.219 (0.068)</td>
<td>-0.020 (0.060)</td>
<td>-0.131 (0.035)</td>
<td>-0.018 (0.024)</td>
<td>0.005 (0.013)</td>
</tr>
<tr>
<td>Treatment*Year -3</td>
<td>0.062 (0.061)</td>
<td>-0.014 (0.116)</td>
<td>-0.020 (0.021)</td>
<td>0.010 (0.012)</td>
<td>-0.017 (0.026)</td>
</tr>
<tr>
<td>Treatment*Year -4</td>
<td>0.089 (.104)</td>
<td>-0.022 (0.151)</td>
<td>-0.117 (0.063)</td>
<td>0.016 (0.031)</td>
<td>-0.148 (0.042)</td>
</tr>
</tbody>
</table>

| Number of Obs.   | 517     | 488     | 489     | 619     | 391     |
| Number of Institutions | 76       | 60       | 70       | 77       | 49       |
| R-Squared        | 0.6848  | 0.8601  | 0.8176  | 0.6076  | 0.6719  |

1. Additionally, each model contains state level controls for state appropriations per FTE, annual per capita income, annual unemployment rate, and the percentage of the state population that holds at minimum a bachelor’s degree. Controls are also present for the institution’s Carnegie classification and selectivity rating.
2. Standard deviations are clustered by state.

\(^{26}\) Control: Public, 4-year institutions in Alabama, Florida, North Carolina, South Carolina, and Tennessee
\(^{27}\) Control: Public, 4-year institutions in Alabama, North Carolina, Tennessee, and Virginia
\(^{28}\) Control: Public, 4-year institutions in Alabama, North Carolina, Tennessee, and Virginia
\(^{29}\) Control: Public, 4-year institutions in Illinois, Indiana, Ohio, Tennessee, and Virginia
\(^{30}\) Control: Public, 4-year institutions in Arizona, California, Idaho, Oregon, and Utah
\(^{31}\) Year 4 denotes the 4\(^{th}\) year of the respective merit aid program
\(^{32}\) Year-1 denotes the year prior to the introduction of the respective merit aid program
FIGURE 1.1 Georgia Average Public, In-State Tuition Over Time

FIGURE 1.2 Florida Average Public, In-State Tuition Over Time

FIGURE 1.3 South Carolina Average Public, In-State Tuition Over Time

FIGURE 1.4 Kentucky Average Public, In-State Tuition Over Time

FIGURE 1.5 Nevada Average Public, In-State Tuition Over Time
CHAPTER TWO

THE IMPACT OF THE HOPE SCHOLARSHIP ON TUITION AND FEES AND INSTITUTIONAL AID AWARDS AT PRIVATE, 4-YEAR INSTITUTIONS IN GEORGIA

1. Introduction

Over the past several decades the United States has witnessed a dramatic increase in the cost of acquiring postsecondary education: from 1981 to 2000, average tuition, in real terms, doubled at four year not-for-profit institutions (The College Board 2001). This cost increase not only strongly outpaced inflation, but also surpassed the recorded median family income growth of 27% (NCES 2002). Over this same time frame, financial aid per full-time-equivalent student grew by 82% (NCES 2002).

Many individuals (Bennett 1987, Singell & Stone 2007, Li 1999), both public figures and researchers alike, have suggested that growth in financial aid availability has facilitated the escalation of tuition charges. Long (2004) specifically examined the impact of the Georgia HOPE (Helping Outstanding Pupils Educationally) Scholarship, a statewide merit aid program, on pricing at Georgia postsecondary institutions. She compared how tuition and fees, room and board charges, institutional aid awards, and instructional expenditures changed over time for Georgia four-year institutions relative to a control group of institutions in other southeastern states without widespread merit aid programs.

This paper revisits Long’s analysis of private, 4-year colleges and universities and identifies potential weaknesses in her results. This study (1) correctly links the data from

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33 Alabama, the District of Columbia, Delaware, Florida, Kentucky, Maryland, Mississippi, North Carolina, South Carolina, Tennessee, Virginia, and West Virginia
each fiscal year to the corresponding academic year (2) alters Long’s Georgia private institution sample and (3) amends Long’s specification for modeling the impact of HOPE by introducing additional interaction terms between Georgia and the years studied. In contrast to Long (2004), this study finds smaller relative increases (1-2.7%) in tuition fees and smaller relative decreases (4.3-5.4%) in institutional aid at private, 4-year Georgia institutions after the introduction of HOPE.

2. Background

To date much of the research on the impact of merit-based financial aid has focused on the Georgia HOPE (Helping Outstanding Pupils Educationally) scholarship. Funded entirely by the Georgia Education Lottery, the HOPE program was first implemented in the fall of 1993. The HOPE program has two components: the merit-based HOPE scholarship and the HOPE grant. Eligibility for the scholarship is based on a cumulative 3.0 high school GPA. The HOPE grant has no such GPA requirement and is exclusively for use at a public university non-degree program. Initially, the scholarship covered two years of tuition at public institutions or a $500 annual award toward study at private universities for students whose family annual income was less than $66,000. However, the scholarship underwent several modifications. In 1994, the program was expanded to include mandatory fees and a book allowance. It was also extended to cover four years of expenses. In 1995, all income caps were lifted. HOPE awards for attendees of private 4-year institutions gradually increased each year, reaching $3,000 in 1996.
2.1 HOPE’s Impact on the Student

Dynarski (2000) examined the impact of the HOPE scholarship on college enrollment in Georgia. Using Current Population Survey data, she found that the scholarship increased the probability that an 18-19 year old Georgia resident would attend college by approximately 25%. Cornwell, Mustard, and Sridhar (2006) also investigated the enrollment effects of merit-based financial aid using Georgia’s HOPE program. Using a difference-in-difference regression with a control group of institutions in other Southern Regional Education Board (SREB) states, they found that freshman enrollment was 5.9% relatively higher in Georgia due to the HOPE scholarship, with the gain concentrated at four-year schools and largely explained by a reduction in the number of Georgia students attending out-of-state colleges. In fact, Cornwell and Mustard (2001b) identified that from 1993-1997, 96% of HOPE awards went to students who would have attended college regardless of the HOPE award. The award simply affected where, but not whether, the students pursued higher education.

In addition to enrollment effects, there are a number of issues that have been addressed by previous researchers. Cornwell and Mustard (2001a, 2002, 2006) and Cornwell, Lee, and Mustard (2005) identified varying effects by race and explore students’ responses (chosen major, number of credit hours per semester, etc.) to the merit scholarship. Henry and Rubenstein (2002) looked for changes in the quality of education due to the increased incentive for strong high school performance provided by the HOPE scholarship. They found that the percentage of Georgia high school students earning a “B” average or better has increased since the HOPE scholarship was made available.
This grade point average improvement was further validated by increased SAT scores over the same time period, which they suggested points to a quality improvement instead of mere grade inflation.

2.2 HOPE’s Impact on the Institution

Much of the research on merit aid has focused on HOPE’s impact on the student, not the impact on the educational institutions serving Georgia’s students. A supply-side reaction is expected given the increased ability of students to pay tuition and the increase in demand for Georgia’s universities as more students attend in-state colleges and universities to reap the rewards of the HOPE scholarship.

Long (2004) examined the impact of financial aid policies on colleges, specifically she investigated how the HOPE scholarship may influence college pricing (both tuition and room and board fees), institutional aid awards, and instructional expenditures at 4-year Georgia institutions. Long compared how pricing and expenditures change over time for Georgia institutions relative to a control group of schools in other southeastern states that lack similar widespread merit aid programs. Because public and private institutes are believed to behave differently, the analysis was completed separately for each sector. This study will focus on Long’s analysis of HOPE’s impact on tuition and fees and institutional aid at private, 4-year institutions.

After (1) eliminating specialized colleges (music, art, nursing, etc.) and “competitor” colleges and (2) controlling for both state and school characteristics,

34 Alabama, the District of Columbia, Delaware, Florida, Kentucky, Maryland, Mississippi, North Carolina, South Carolina, Tennessee, Virginia, and West Virginia

35 Colleges outside of Georgia with at least 5% of their incoming freshman class from Georgia.
Long found that private Georgia institutions increased tuition 3.2% faster than private schools outside of Georgia and experienced a simultaneous relative decrease in institutional aid awards by 11.4% after the introduction of the HOPE scholarship. This result, however, was based on a simple “before” and “after” analysis where tuition and fee pricing four years prior to HOPE was contrasted to charges four years post-HOPE. Such an analysis does not permit the impact of HOPE to vary over time. Given the significant changes in the value of the HOPE award at private institutions over the first four years of the scholarship, the impact of HOPE is surely expected to vary. It is likely the impact increased over the initial years as the monetary value of the award increased, and the merit eligible population expanded\(^{37}\). Long did examine an additional specification, which retained the grouping of pre-HOPE years into a collective “before” term but included terms for each individual year post-HOPE. Using this specification, she found a 4.1-4.6% relative increase in tuition and fees in years 3 and 4 of HOPE, and relative decreases in institutional aid of 5.8-13.9% over the first four years of HOPE.

Although Long’s analysis provides interesting insight into the behavior of postsecondary institutions in an environment where merit aid has proliferated, her analysis is potentially weakened by a number of challenges associated with educational data and the timing of strategic financial decisions by educational institutions. The fiscal year for the vast majority of educational institutions does not correspond to the calendar year, but instead runs from August to July (approximately), closely tracking the academic

\(^{36}\) Annual per capita income, the percentage of the population holding at minimum a bachelor’s degree, annual unemployment rates, along with the institution’s Carnegie classification and selectivity rating

\(^{37}\) During the first year of a program only incoming students, or approximately one quarter of total enrollees, have access to the merit aid, but by year three approximately 75% of students are potential merit aid recipients.
year. The IPEDS database, from which data is collected for Long’s analysis, reports financial data by fiscal year that must be mapped to the appropriate academic year. For example, fiscal year 1992 describes the 1991-92 academic year. However, based on the schools excluded from Long’s Georgia private institution sample for a lack of data, it appears that Long interpreted IPEDS financial 1992 data as academic year 1992-93, instead of 1991-92. This apparent interpretation may change the meaning and of Long’s results as her pre- and post-HOPE data may be somewhat mislabeled.

Additionally, IPEDS presents fiscal year institutional and financial data accompanied by enrollment data for fall of the given fiscal year. Thus, if, for example, one associates fall 1992 enrollment data with fiscal year 1992 an incorrect linkage is created: fiscal year 1992 is academic year 1991-1992 and should be associated with enrollment in fall 1991. Given the supposed error in fiscal year to academic year mapping, Long’s use of enrollment data may also be faulty.

In addition to the difficulties noted above, Emanuel College and Reinhardt College should be eliminated from the private sample used by Long. These two institutions experienced a change in accreditation during the time period studied: both colleges transitioned from two-year to four-year status. Atlanta Christian College should

---

38 Long states that she excluded institutions for which less than 7 of 8 years of data were available across the time period studied (1989-90 academic year to 1996-97 academic year, or fiscal years 1990-1997). Clark Atlanta lacks data for fiscal years 1989 and 1996 (academic years 1988-89 and 1995-96). Based on availability of academic year data, Clark Atlanta should appear in Long’s sample, as the 1988-89 school year is outside the scope of her study, but the institution is not included in her analysis. This leads the present author to believe that Long interpreted fiscal year as academic year and excluded Clark Atlanta for missing data in the 1989 and 1996 fiscal years. Additionally, Paine College is not eliminated from Long’s sample for missing institutional aid data but IPEDS does not contain internal aid data for this institution fiscal years 1990 and 1997 (academic years 1989-90 and 1996-97).
also be removed due to its status as a theological seminary. Paine College, which Long excluded due to lack of data, should be introduced into the sample.

This paper (1) correctly links the data from each fiscal year to the corresponding academic year (2) alters Long’s Georgia private institution sample and (3) amends Long’s specification for modeling the impact of HOPE by introducing additional interaction terms between Georgia and the years studied.

3. Theory

Almost all educational institutions are non-profit organizations. As such, they are limited by a non-distribution constraint, which dictates that no profits can be distributed to an individual as a for-profit firm distributes funds to owners (Winston 1999). Given this constraint, an institution’s interest in raising prices may be questioned. However, Hansmann (1980) aptly pointed out that the non-distribution constraint is not as rigid as one might initially presume. For example, profits can be indirectly distributed to individuals through inflated salaries. As an alternative to direct compensation, the potential profits can also be spent on on-the-job consumption of goods preferred by the employee (James 1990). Given that the non-profit produces multiple products (such as institutions of higher learning who supply both undergraduate and graduate educations, athletic teams, etc.), profits from one division of the university may be redistributed to subsidize other divisions (Winston 1999).

The non-distribution constraint ultimately leads the managers of higher education institutions to maximize their own utility from organizational expenditures (James 1990). Numerous researchers agree that pursuit of “excellence” or “prestige maximization” is
the ultimate objective of the institution, where excellence is defined as preserving or improving the quality of services provided and prestige exists as a relative position among competitors (Winston 1999, Clotfelter 1996, James 1990).

It is clear that increased revenue, ceteris paribus, facilitates higher levels of prestige and faculty satisfaction at the institution by permitting additional spending on services that attract high-quality students, salaries, and/or valued on-the-job consumption goods. Further, Rose and Sorenson (1992) found a link between high tuition and administrative overhead and instructional costs, suggesting that both college administrators and faculty members are important beneficiaries of student revenue.

One way the institution may elect to increase revenue is by increasing tuition and fees. Under normal circumstances a price increase would surely decrease the number of students consuming the institution’s services and potentially impact student quality as other institutions become relatively less expensive. However, in the merit-aid environment, students are sheltered from bearing the full cost of tuition increases due to the substantial value of the merit award. This decreased sensitivity to changes in price allows institutions to increase tuition without producing large detrimental impacts to enrollment and student quality.

As private institutions are free to set tuition prices without influence from the legislature or other governing bodies, they can directly respond to increases in merit-based aid availability by increasing tuition and/or using merit awards to offset institutional financial aid awards. However, private institutions face several impediments to implementing tuition and fee increases without detrimental impacts on enrollment and
student quality. Unlike, public institutions who have separate list prices for in-state and out-of-state students, private institutions publish a single price for all students, independent of their state of residence. Thus, increasing list tuition and fees at a private institution would impact both potential HOPE scholars and non-Georgia residents, who do not have access to the HOPE scholarship. It might therefore be more effective for private institutions to instead price discriminate by lowering institutional aid offers specifically to HOPE recipients. However, if public education is a close substitute for private education, the competitive relationship between private and public institutions may reduce the ability of private institutions to increase pricing or lower internal aid awards without losing students to their public counterparts, where HOPE scholars receive free tuition. It is also important to remember that in the inaugural year of HOPE only incoming students, or approximately one quarter of total enrollees, had access to the merit aid. Thus, the ability to implement pricing changes due to the introduction of HOPE may be somewhat limited in the earliest years of the program.

4. Methodology

This paper considers two empirical models used by Long (2004), which are modified through a change in composition of the private Georgia institution sample and the inclusion of additional covariates. Impacts on both in-state tuition and fees and institutional aid per FTE\(^\text{39}\) at private, 4-year institutions are explored.

To determine if private colleges in Georgia behaved differently from other, non-Georgia institutions after the introduction of HOPE, the following difference-in-

\(^{39}\) Full-time equivalent student, calculated as the number of full-time students plus 1/3 of part-time students.
difference specification\textsuperscript{40} is completed using ordinary least squares on the data pooled over the academic years of interest, 1989-90 to 1996-97:

\[
y_i = \beta_0 + \beta_1 (\text{Georgia}_i, * \text{After}_i) + \beta_2 \text{Georgia}_i + \beta_3 \text{After}_i + \beta_4 X_i + \epsilon_i. \tag{1}
\]

Here \(y\) is the outcome of interest, and \(i\) is the \(i\)th college observed in time period \(t\). \textit{Georgia} is a dummy variable indicating if the college is located in the state of Georgia, while \textit{After} indicates that the observed academic year is 1993-94 or after (indicating existence of the HOPE program). Private institutions in post-HOPE Georgia compose the treatment group, while the control group is made up of private institutions in nearby, non-merit aid states. \(\beta_1\) is the coefficient of interest, and it will be used to ascertain if institutions in Georgia adjusted pricing and expenditures differently than schools in other states after the creation of the HOPE program. \(X_i\) is a vector of covariates thought to influence the outcomes of interest. These additional controls include the following state level variables: state annual per capita income, state annual unemployment rate, the percentage of the state population that holds at minimum a bachelor’s degree, and the following institution level characteristics: Carnegie classification and selectivity rating. The listed state characteristics may influence the price of colleges within the state as they reflect both attitudes toward higher education and the opportunity cost of attending college (Long 2004). An institution’s Carnegie classification is used to distinguish various types of institutions that may display different types of pricing behavior. Finally,

\textsuperscript{40} Underlying assumptions for difference-in-difference estimation require that time effects are common across both groups, treatment and control, and the composition of the two groups is stable prior to and after the introduction of merit aid (Cameron and Trivedi 2006).
the institution’s selectivity rating captures its degree of admissions competitiveness; presumably, institutions with potential students who compete intensely for admission may be able to implement price increases with minimal impact on enrollment. A year trend is also included. Due to the serially correlated outcomes, error terms are clustered by state.

Long (2004) also explores if the reaction to HOPE varies by year by using dummy variables for each year in the study after the implementation of HOPE. Because some period of adjustment may be necessary, she hypothesizes that reactions may be stronger several years after the initial implementation of HOPE. Also recall that the HOPE program experienced fundamental changes even in its initial years. The income cap was lifted in 1995, and by 1996 the size of the award at private institutions had grown from the initial $500 to $3,000 per year. In order to allow for a more complete analysis of the data, this paper introduces dummy variables for the years prior to the implementation of HOPE, and the following model is used to explore the variation in pricing behavior by year:

\[ y_{it} = \alpha + \beta \text{Georgia}_i + \gamma \text{Year}_i + \delta (\text{Georgia}_i \times \text{Year}_i) + \lambda X_{it} + \epsilon_{it} \]  

(2)

Here, Year denotes a series of dummy variables, one for each academic year of interest, except the year (1992-93) immediately prior to the introduction of HOPE. This

\[^{41}\text{Barron’s Profiles of American Colleges} \] puts institutions into seven categories: most competitive, highly competitive, very competitive, competitive, less competitive, noncompetitive, and special. Ratings are assigned based on median entrance exam scores, percentage of freshmen scoring above particular SAT and ACT scores, percentage of freshmen who ranked in the upper fifth and the upper two-fifths of their high school graduating class, minimum class rank and GPA required for admission, and the percentage of applicants to the freshmen class that were admitted.

\[^{42}\text{As previously noted, although the HOPE scholarship covers the full cost of tuition at public institutions, the scholarship is worth a specified dollar amount at private institutions (1993: $500, 1994: $1,000, 1995: $1,500, and 1996: $3,000), which covers only a portion of tuition costs.}\]
specification also includes interaction terms for Georgia and each academic year studied except 1992-93. \( \beta \) or the “Georgia” coefficient, identifies how pricing in Georgia differed from pricing in other southeastern states in 1992-93, providing a baseline for comparing pricing behavior after HOPE. The coefficient on each Georgia-year interaction term displays how Georgia outcomes in that year differ from the 1992-93 baseline. If the coefficients for the post-HOPE years are near zero, then the data suggests that Georgia institutions did not react to HOPE by altering their pricing. Further, the difference between the Georgia outcome and the control state outcome for a given academic year is calculated as the sum of the 1992-93 effect, as captured by the “Georgia” coefficient, and the coefficient for the Georgia-year interaction term for the given year.

5. Data

This paper uses data from academic years 1989-90 to 1996-97 on 155 schools in 13 states taken from the Integrated Postsecondary Education Data System (IPEDS), which compiles annual surveys conducted by the U.S. Department’s National Center for Education Statistics (NCES). In this analysis, IPEDS is used to collect data on state appropriations, tuition, room and board fees, institutional aid awards, and enrollment figures. Selectivity ratings were taken from Barron’s Profiles of American Colleges (1994). Data on unemployment rates, per capita income, and educational attainment by state are collected from the Bureau of Labor Statistics, the U.S. Bureau of Economic Analysis, and the U.S. Bureau of the Census.
Only fully accredited 4-year public and 4-year not-for-profit private educational institutes are used in this analysis. The treatment group is composed of Georgia 4-year private institutions in academic years 1993-94 through 1996-97. Unlike Long’s (2004) sample, this paper excludes Emmanuel College and Reinhardt College from the private Georgia institution sample. Both of these institutions experienced an accreditation change from 2-year to 4-year accreditation during the time period studied\(^\text{43}\); the change in the scope of these institutions may have contributed to changes in pricing and expenditures independent from HOPE. Specialty schools, or those with Carnegie classifications as schools of art, music, and design; theological seminaries; and other specialized institutions, are excluded from the study because they are assumed to have missions that differ from the standard university. Although Long (2004) states that specialty institutions are excluded, her analysis includes Atlanta Christian College, a theological seminary. This paper removes Atlanta Christian from the sample. However, Paine College is introduced as an addition to Long’s sample. A limited number of schools in control states are also eliminated because they do not charge tuition or have minimal student charges.\(^\text{44}\) Additionally, only schools with at least 7 years of data available from the 8-year time period analyzed are kept. Finally, financial variables with values that exceed 150% of the observed mean by school are coded as missing because these extreme values are suspect of reporting errors (Long 2004).

\(^{43}\) Emanuel College received 4-year accreditation in 1991, Reinhardt College in 1994.

\(^{44}\) These schools include military academies and institutions such as Berea College and Alice Llyod College that do not charge tuition but have required participation in work-study programs.
A control group is constructed of private, 4-year institutions in other southeastern states without state merit aid programs. This group consists of colleges located in Alabama, the District of Columbia, Delaware, Florida, Kentucky, Maryland, Mississippi, North Carolina, South Carolina, Tennessee, Virginia, and West Virginia. Following Long (2004), excluded from the control group are competitor schools, or those non-Georgia schools with at least 5% of their first-time freshmen from Georgia. Long (2004) hypothesizes that these competitor schools may also react to HOPE.

Table 2.1 presents the summary statistics associated with this data before the implementation of HOPE, while Figures 2.1 illustrates the general tuition trends observed from 1989-90 to 1996-97. In 1992-93, tuition and fees at Georgia private institutions are within 1% of tuition and fees at other southern private institutions. Georgia private colleges and universities, however, do offer higher institutional aid per FTE than their southeastern counterparts. It is clear that other southeastern schools more closely match Georgia schools than schools in other regions of the nation, and thus southeastern schools are judged to be the most appropriate control group.

6. Estimation & Results

Columns (1) and (2) of Table 2.2 record the response of private, 4-year Georgia institutions to HOPE through tuition and fee pricing changes. Long (2004) found a 3.2% relative increase in tuition and fees using a simple “before” and “after” difference-in-difference model, and increases concentrated in 1995-96 and 1996-97 (4.6%, 4.1%) when she permitted the “after” effect to vary by year. In comparison to Long, this study finds a

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45 Results remain robust to changes in the definition of “competitor.”
2.3% relative increase in tuition and fees using the simple difference-in-difference with “before” and “after” groupings in specification (1) and yearly effects ranging from 1%-2.7%.

The “Georgia” coefficient in specification (2), which permits variation by year, indicates that in 1992-93 tuition and fees were 7.6% lower at private Georgia institutions compared to other southeastern colleges. The small values (-0.012, -0.011, 0.004) of the coefficients on the years prior to 1992-93 illustrate that the pricing differential between Georgia institutions and the control institutions was fairly consistent across the pre-HOPE time period studied. Post-HOPE increases in tuition and fees relative to the 1992-93 baseline are evident but are not large. As observed in the coefficient of Georgia*1993-94 (0.01), a small relative pricing change is observed in the first year of the program. In the second and third year of HOPE, Georgia tuition and fees rise from the baseline by 2.6 percentage points, reducing the gap between Georgia and bordering states from -7.6% to -5%. In year 4 of HOPE, charges at Georgia private institutions are only 1.1 percentage points higher than the baseline in 1992-93. These results contrast Long’s increases (4.6%, 4.1%) reported in 1995-96 and 1996-97 and suggest that tuition and fee pricing did not grow dramatically in reaction to HOPE.

Long’s results show a strong reaction to HOPE by private Georgia institutions through reductions in institutional aid. Her results reveal an 11.9% relative decrease in institutional aid post-HOPE using the simple “before” and “after” model. This study, however, does not support her findings. As previously recorded, it should be noted that the private institution sample used in this study differs from that used by Long (2004). In
particular, this study uses institutional aid data from 14 Georgia private colleges and universities; Long’s study uses aid data from 16 institutions\textsuperscript{46}. The difference in the impact on institutional giving may then be attributed to this difference and implies that results are not robust to changes in the sample.

Results shown in Table 2.2, column (3) suggests that institutional aid per FTE did decrease post-HOPE (-4.8%), but by less than half the magnitude of Long’s estimate. Results from specification (2) shown in column (4) indicate that in the first 3 years of HOPE, internal aid was reduced from the baseline by, on average, 5 percentage points (-5.4%, -4.3%, -5.3%) relative to the control institutions. However, a 13.8% increase is shown in 1996-97. Investigation of the data exposes that internal aid data for only 7 private Georgia institutions is available in this year. This limited data happens to capture the more generous institutions and thus the 13.8% does not describe the behavior of the full Georgia private sample. Here the empirical evidence suggests that Georgia private institutions did, in fact, decrease institutional aid after the introduction of HOPE. However, the decrease is not of the magnitude (-11.9%) suggested by Long’s (2004) study.

7. Conclusions

Empirical evidence suggests that Georgia private, 4-year institutions altered their pricing and aid strategies in reaction to HOPE. However, the results of this study suggest that the changes are smaller in magnitude than the reactions reported by Long\textsuperscript{46}.

\textsuperscript{46} Unlike Long’s analysis, this analysis uses data from Clark Atlanta College, LaGrange College, Spelman College, which Long eliminated due to missing data, and excludes Emmanuel College and Reinhardt College (due to their change in accreditation during the sample period) and Atlanta Christian College (due to its status as a theological seminary). Paine College is also excluded due to missing data but was also not present in Long’s analysis.
(2004). This difference in the estimated impact of HOPE can be attributed both to the corrected linkage of financial data to the appropriate academic year and the modified composition of the treatment group. While Georgia private tuition and fees were 7.6% below charges at comparable non-merit institutions in the year immediately before HOPE, this gap was reduced slightly to -6.5% in 1996-97. The absence of a large price increase may be attributed to a number of factors. Given that list tuition price is applicable to all students regardless of state of residence and that private institutions face competition from public universities where HOPE scholars receive tuition at no cost, private institutions may be unable to increase pricing without negative impacts on enrollment and student quality. The data suggests that institutional aid awards decreased at private institutions in Georgia after the implementation of the HOPE scholarship. Although these awards were 8% higher in Georgia in 1992-93, this gap was reduced, on average, to 3% in the first three years of HOPE. While unable to implement overall increases in tuition and fees, Georgia private institutions may have been successful at reducing internal merit awards to HOPE recipients.
### TABLE 2.1 Private, 4-year Institutions: 1992-93 Summary Statistics
(Constant year 2000 dollars)

<table>
<thead>
<tr>
<th></th>
<th>Georgia</th>
<th>Competitor</th>
<th>Other Southeastern</th>
<th>Other U.S.</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Number</strong></td>
<td>16</td>
<td>50</td>
<td>139</td>
<td>617</td>
</tr>
<tr>
<td><strong>In-State Tuition &amp; Fees</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>$10,736 (4,131)</td>
<td>$11,420 (3,988)</td>
<td>$10,613 (4,162)</td>
<td>$13,373 (4,667)</td>
</tr>
<tr>
<td><strong>Institutional Aid per FTE</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>$2,694 (1,933) [15]</td>
<td>$2,563 (1,553) [49]</td>
<td>$1,957 (1,445) [134]</td>
<td>$2,755 (1,801) [597]</td>
</tr>
</tbody>
</table>

**Notes:**

1. This sample excluded schools of a specialized nature such as schools of art, music, and design, theological seminaries, medical schools, other health profession schools, schools of law, and teachers colleges.
2. Competitors are non-Georgia schools with at least 5% of first-time freshmen from Georgia.
3. Standard deviations are given in parentheses.
4. When less than the full sample is used in the calculation, the number of observations used are given in brackets.
TABLE 2.2 The Impact of HOPE on Pricing at Private, 4-year Georgia Institutions
Treatment: Institutions in Georgia
Control: Institutions in Nearby, Non-Merit Aid States, excluding competitors

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>ln(Tuition &amp; Fees)</th>
<th>ln(Tuition &amp; Fees)</th>
<th>ln(Inst. Aid)</th>
<th>ln(Inst. Aid)</th>
</tr>
</thead>
<tbody>
<tr>
<td>GA*After</td>
<td>0.023* (0.012)</td>
<td></td>
<td>-0.048 (0.043)</td>
<td></td>
</tr>
<tr>
<td>GA</td>
<td>-0.080* (0.039)</td>
<td>-0.076** (0.040)</td>
<td>0.086 (0.090)</td>
<td>0.080 (0.094)</td>
</tr>
<tr>
<td>After</td>
<td>0.020 (0.042)</td>
<td>0.034 (0.105)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>GA*1996-97</td>
<td></td>
<td>0.011 (0.015)</td>
<td>0.138 (0.133)</td>
<td></td>
</tr>
<tr>
<td>GA*1995-96</td>
<td>0.024* (0.012)</td>
<td></td>
<td>-0.053 (0.075)</td>
<td></td>
</tr>
<tr>
<td>GA*1994-95</td>
<td>0.027** (0.007)</td>
<td></td>
<td>-0.043 (0.034)</td>
<td></td>
</tr>
<tr>
<td>GA*1993-94</td>
<td>0.010* (0.005)</td>
<td></td>
<td>-0.054* (0.027)</td>
<td></td>
</tr>
<tr>
<td>GA*1991-92</td>
<td>0.004 (0.070)</td>
<td></td>
<td>0.075 (0.145)</td>
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</tr>
<tr>
<td>GA*1990-91</td>
<td>-0.011 (0.015)</td>
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<td>0.011 (0.044)</td>
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<tr>
<td>GA*1989-90</td>
<td>-0.012 (0.031)</td>
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<td>-0.018 (0.080)</td>
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<td>R-Square</td>
<td>0.50</td>
<td>0.50</td>
<td>.29</td>
<td>.30</td>
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<tr>
<td>Observations</td>
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<td>1227</td>
<td>898</td>
<td>898</td>
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<tr>
<td>No. of Institutions</td>
<td>155</td>
<td>155</td>
<td>124</td>
<td>124</td>
</tr>
</tbody>
</table>

Notes:
1. ** denotes significance at the 0.05 level. * denotes significance at the 0.10 level.
2. Robust standard errors are clustered by state and shown in parentheses.
3. All models include a year trend and controls for state and institution characteristics.
4. Competitors are those non-Georgia school’s with at least 5% of first-time freshmen from Georgia.
FIGURE 2.1 Georgia Average Private Tuition Over Time
CHAPTER THREE
THE IMPACT OF POSTSECONDARY MERIT AID AVAILABILITY
ON MILITARY ENLISTMENTS

1. Introduction.
Many factors influence the decision to enlist in the United States military. These influences include the quality of civilian opportunities\(^47\) and a variety of other incentives\(^48\), both monetary and non-monetary. Notably, Montgomery GI Bill (MGIB) educational benefits are denoted as “a prime recruiting tool in today’s all-volunteer military” and multiple facts suggest educational benefits provide a strong incentive to join the Army (Simon, Nergrusa, & Warner 2010). In the 1999 Youth Attitudes Tracking Survey (YATS) approximately one third of youth respondents mentioned money for education as a reason for enlisting in the armed forces. In FY1996, 94% of enlistees enrolled in the MGIB program, and evidence from earlier cohorts\(^49\) shows that close to 90% of enlistees utilized their MGIB benefits\(^50\) after exiting the armed forces (Asch, Kilburn, & Klerman 1999). Combined, these findings strongly confirm that enlistees value educational benefits.

In the 1990s, state financial aid programs made a dramatic shift from awarding aid based on need to awarding aid based on merit (Salingo 2001). During this time a number of states introduced statewide, postsecondary merit aid programs that award

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\(^{47}\) The quality of civilian opportunities is measured by the unemployment rate and military pay relative to civilian pay.

\(^{48}\) Enlistment bonuses, patriotism, family tradition, etc. 

\(^{49}\) FY86 & FY90

\(^{50}\) The Montgomery GI Bill (MGIB) program provides college benefits, which vary by length of service, to any recruit who joins the program through a $1,200 contribution in his first year of service. On top of MGIB, the Army and Navy offer recruits in selected skills additional college benefits, called “kickers,” which vary by length of enlistment and skill category (Warner, Simon, & Payne 2001).
substantial scholarship dollars, which in some cases cover the full cost of tuition and fees, to resident youth. These merit programs provide youth with a source of funds for education that do not require military enlistment. Thus, when presented with this alternative form of funding, youth may have reduced interest in joining the armed forces.

Although there has been a significant decrease in accession requirements over time, the military has experienced difficulty fulfilling recruitment goals (Asch, Kilburn, & Klerman 1999; Warner, Simon, & Payne 2003). This study seeks to determine if the presence of a state merit aid program reduces the number of high quality (HQ) Army enlistees (enlistees with a high school degree and AFQT score>49) from that state. The analysis is completed using two data sources. An aggregate-level dataset containing the total number of Army contracts per year over the period 1988-2004 is used to model the impact of merit aid on HQ enlistment supply. A probit model using micro-level survey data from the Youth Attitude Tracking Survey examines the impact on merit aid on the propensity of youth to enlist in the Army. Empirical evidence from both models suggests that the presence of a state merit aid program reduces the number of high quality, black male Army enlistees.

2. Background.

Over the period 1988-2004, the percentage of army enlistees that were HQ ranged from 52% to 75%. Asch, Kilburn, & Klerman (1999) stated that the armed forces have a preference for HQ enlistees because of their superior military performance and lower risk of attrition. However, these HQ individuals are those whom the armed forces may

51 “Across the Department of Defense, total accession requirements declined by 33% between 1989 and 1998” (Asch, Kilburn, & Klerman 1999).
52 Armed Forces Qualification Test
have difficulty recruiting due to the civilian opportunities available to them, notably the returns to attending college (Asch, Kilburn, & Klerman 1999).

Previous studies have identified that HQ enlistment is impacted by economic factors, military recruiting resource variables, and military enlistment incentives (Warner, Simon, & Payne 2004). The literature has established a clear positive relationship between HQ enlistment and pay and unemployment. Warner, Simon, and Payne (2004) found pay elasticities ranging from 0.67 to 0.90, and an unemployment elasticity of approximately 0.25. Their estimated effect of the presence of recruiters per capita was captured by an elasticity of 0.55 (Warner, Simon, & Payne 2004).

Asch, Hosek, and Warner (2007) found that educational benefits do, in fact, attract high quality (HQ) youth as evidenced by an average elasticity estimate of 0.1, which indicates that a doubling of education benefits would increase HQ enlistment by approximately 10%. Wilson (1996) asserted that the propensity for military service is a function of educational prospects: interviews and focus groups reveal that college bound youth with “the intent, academic abilities, and funding, appear to have very little interest in enlistment” but others view the military as a means to education, in part for the funding it can provide. Notably, the interviews also show that the likelihood of enlistment changes as an individual’s circumstances change. For example, “one young man’s interest in military service may evaporate as college funding materializes” (Wilson 1996). This strongly supports the hypothesis that the introduction of a state merit aid program may significantly reduce HQ enlistments.
3. Theory.

In a simplified setting, individuals choosing to complete postsecondary education have two options: attend college or first enlist in the Army and later attend college using MGIB benefits. Each individual will select the most attractive option given the expected utility from each choice as denoted below using an occupational choice framework from Smith, Hogan, Chin, Goldberg, and Goldberg (1990):

\[
E[U_{\text{army}}(A, GI)] = \text{expected utility from enlistment and use of MGIB benefits}
\]

where \( A = \text{Army enlistment} \)
\( GI = \text{use of MGIB benefits} \)

\[
E[U_{\text{civilian}}(C)] = \text{expected utility from college attendance}
\]

where \( C = \text{College attendance} \)

Enlistment occurs when \( E[U_{\text{army}}(A, GI)] > E[U_{\text{civilian}}(C)] \).

A change in some factor that alters the relative values of \( E[U_{\text{army}}(A, GI)] \) and \( E[U_{\text{civilian}}(C)] \) will impact the likelihood of enlistment. It is commonly accepted that pay, available terms of enlistment, education benefits, bonuses, and future job opportunities influence military enlistment. I hypothesize that publicly provided merit aid is a substitute for military educational funding, and thus the presence of a state merit aid program increases the relative value of \( E[U_{\text{civilian}}(C)] \) and negatively impacts the propensity to enlist.

I further hypothesize that the impact of a state merit aid program on enlistments may vary by race. Specific to MGIB benefits, Simon, Nergrusa, and Warner (2010) reported that blacks were 0.8-1.6 percentage points more likely to use education benefits in both the Army and the Navy. Outside of MGIB benefits, a number of studies suggest that postsecondary financial aid has a stronger impact on the education decisions of
blacks than whites, all else equal. Using data from 1980 and 1982 cohorts, St. John and Noell (1989) found that a scholarship package increases that probability that a black individual enrolls in college by 17.7%. However, the influence of such a scholarship package on whites increases the probability of enrollment by only 8.9%, which is about half the size of the effect on blacks (St. John & Noell 1989). Jackson (1990) identified that a black scholarship recipient is 11 percentage points more likely to enroll in college than a non-recipient, even when controls for academic and family background are present. He stated that blacks are “most responsive” to scholarship awards with the effect being almost double that for white students (Jackson 1990). Additionally, Alon (2007) found that the likelihood of graduation is more strongly impacted by financial resources, especially grants and scholarships, for minorities than for whites.

The higher sensitivity of blacks to postsecondary financial aid may be a product of the relatively limited family financial resources available to black students. In 2005, black household median income was $30,858, compared to $50,784 for non-Hispanic white households (DeNavas-Walt, Proctor, & Lee 2006). For blacks, the introduction of merit aid may create new funding opportunities for education that were not previously available. However, for whites, merit aid may simply replace the family resources that would have financed education in the absence of merit aid. Thus, in the previously presented choice model, the introduction of a merit aid program is likely to increase the expected value of non-military funded education, or $E[U_{civilian(C)}]$, relatively more for blacks than whites. As a result of the disproportionate change in $E[U_{civilian(C)}]$, it is likely
that merit aid availability will produce a relatively larger impact on the propensity of enlistment for blacks than whites.

Further, evidence from Warner and Pleeter (2001) identified variation in discount rates by race, with blacks having higher discount rates. Gilman (1976) and Black (1984) also identified a racial difference in discount rates. Therefore blacks may be more likely to accept the merit aid immediately awarded by the state than military educational benefits that are awarded after a period of service.

4. Data.

This study uses two approaches to estimate the impact of a state merit aid program on HQ Army enlistments. Aggregate-level data capturing the number of HQ contracts signed each calendar quarter by state will be used to model Army HQ enlistment supply. I will also exploit micro-level YATS data to determine how the propensity to enlist in the Army is affected by merit aid availability.

Aggregate-level Army Contracts by Quarter.

This aggregate-level dataset consists of the total number of Army contracts each calendar quarter by state for the period 1988-2004. The dataset also includes the following state-level variables: civilian demographics (race, gender, education), unemployment rate, and civilian wages\textsuperscript{53} relative to military pay. See Table 3.1 for the summary statistics describing this data. Combined with information on the youth population (ages 17-21), this enlistment data indicate that on average the youth enlistment rate is 0.46% per year, and the probability that a given individual joins the Army during his youth is 2.3%. Over the observed time period, 62% of enlistees are

\textsuperscript{53} Civilian wages are gathered from CPS data and calculated as the weekly earnings of 18-35 male high school graduates.
classified as HQ. Of these HQ enlistees, 78% are male. Whites represent 73% of HQ enlistees, while blacks are 16% of this population.

**Youth Attitude Tracking Survey (YATS).**

YATS is an annual survey by the Department of Defense that originated in 1975 and was last performed in 1999. Administered to approximately 10,000 American youth between the ages of 16 and 24, the survey asks about “youth attitudes and opinions about future plans, perceptions of the military, military enlistment propensity, contact with military recruiters, and awareness of military advertising” (YATS 1999). I utilize YATS data from survey years 1987 to 1998. I further restrict the YATS data to a subset of male participants that includes high school seniors, juniors, and sophomores self reporting grades primarily composed of A’s and B’s. These participants represent the group of youth most likely to be classified as HQ and also most likely to be influenced by the introduction of a postsecondary merit aid program. The resulting sample size is 4,758 of which approximately 12% expressed a positive propensity\(^{54}\) to join the Army. The survey data permits the identification of each participant’s race, grade level, scholastic performance, and parental education background. State-level data capture the unemployment rate and a measure of military pay relative to civilian pay. Summary statistics for the sample are given in Table 3.2.

**State Merit Aid Programs.**

Since the introduction of the Georgia HOPE (Helping Outstanding Pupils Educationally) scholarship in 1993, a relatively large number of states have introduced

\(^{54}\) A survey participant is said to have a positive propensity to join the Army if he answered “definitely” or “probably” in response to the question “How likely is it you will be serving on active duty in the Army; would you say definitely, probably, probably not, or definitely not?”
merit aid scholarships that provide generous educational funding, and in some cases full
tuition at public universities, to students. In 2008-09, states awarded $2,271.5 million
dollars in non-need based aid, compared to $717.7 million in 1998-99, and $169.8
million in 1988-89 (NASSGAP). Even after adjusting these figures for inflation, the
merit aid dispersed in 2008-09 was over 7 times greater than the aid awarded in 1988-89.

The following is a list of states that introduced a widely available postsecondary
merit aid program during the years addressed by this study:

<table>
<thead>
<tr>
<th>State</th>
<th>Year of Merit Aid Program Introduction</th>
</tr>
</thead>
<tbody>
<tr>
<td>Georgia</td>
<td>1993</td>
</tr>
<tr>
<td>Mississippi</td>
<td>1995</td>
</tr>
<tr>
<td>Florida</td>
<td>1997</td>
</tr>
<tr>
<td>New Mexico</td>
<td>1997</td>
</tr>
<tr>
<td>Louisiana</td>
<td>1998</td>
</tr>
<tr>
<td>South Carolina</td>
<td>1998</td>
</tr>
<tr>
<td>Kentucky</td>
<td>1999</td>
</tr>
<tr>
<td>Nevada</td>
<td>2000</td>
</tr>
<tr>
<td>West Virginia</td>
<td>2002</td>
</tr>
<tr>
<td>Tennessee</td>
<td>2004</td>
</tr>
</tbody>
</table>

In most instances, the programs award merit aid based on high school GPA and
standardized test scores. The awards range in value from $500 to the full cost of tuition
and fees at public institutions. See Table 3.3 for program details. Indicator variables for
the presence of a state merit aid program (as determined by the list above) will be
included in models utilizing both the aggregate-level HQ contract data and the micro-
level YATs data to determine if merit aid availability impacts the decision to enlist in the
Army.
5. Methodology

Two-way Fixed Effects.

To model Army HQ male enlistment per (eligible) capita using aggregate-level HQ contract data, I use a two-way fixed effect model adapted from WSP (2004) that contains (1) the natural log of relative military pay, the civilian unemployment rate, the Army’s total enlistment goal, the number of Army recruiters per youth capita, the percentage of recruits receiving bonuses, and the average bonus amount and (2) controls for state and year effects. Additionally, I introduce a dummy variable (Merit) for the presence of a merit aid program.

\[
\ln(\text{HQEnlistees}_i / \text{EligibleYouthPop}_i, + \text{mean HQEnlistees}_i / \text{EligibleYouthPop}_i) = \\
\beta_0 + \beta_1 \text{Merit}_i + \beta_2 \ln \text{RelPay}_i, + \beta_3 \ln \text{Unemp}_i, + \beta_4 \text{TotalGoal}_i, + \\
\beta_5 \ln(\text{Recruiters}_i / \text{YouthPop}_i) + \beta_6 \ln \text{BonusAmount}_i, + \beta_7 \ln \text{BonusPercent}_i, + \beta_8 X_t, + \epsilon_t
\]

Following Warner, Simon, and Payne (2001), HQ enlistments are scaled by the eligible youth population (those individuals with at minimum a high school degree) aged 17-21. HQ male enlistment is modeled separately by race. Because in a number of observations black, male HQ enlistment is equal to zero, I transform the dependent variable by adding the mean\(^{55}\) of race-specific, male HQ enlistees per eligible youth population to each observed value of enlistees per capita. The use of this transformation allows the full sample to be utilized in the model. However, the interpretation of the coefficients will be altered such that the value of each coefficient is interpreted as .5 *

\(^{55}\) For both races, the mean is calculated across all observations with non-zero values for male HQ enlistees per eligible population. For blacks, the mean is equal to 0.008 and for whites, 0.002.
\[ \frac{d\ln(y)}{dx} \text{ when the dependent variable (} y = \text{Army HQ contracts per eligible capita) is evaluated at its mean}^{56} \]

**Probit Model.**

Using micro-level YATS data, I employ a probit model to estimate the impact of a merit aid program on the probability of having a positive propensity \((Armypos=1)\) to enlist in the Army. A dummy variable \((merit)\) is used to indicate the presence of a merit aid program. The model also includes variables that capture the unemployment rate, relative military pay, the participant’s years of education, parental education levels, and dummies for census divisions\(^57\) and survey years.

\[
\Pr(Armypos_i = 1) = \beta_0 + \beta_1 \text{merit}_i + \beta_2 \text{urate}_i + \beta_3 \text{relpay}_i + \beta_4 \text{yrscd}_i + \beta_5 \text{fhsgrad}_i + \\
\beta_6 \text{mpay}_i + \beta_7 \text{mhsgrad}_i + \beta_8 \text{mcoll}_i + \lambda X_i + \varepsilon_i
\]

Again, white males and black males are modeled separately. As discussed in earlier sections, merit aid is thought to impact potential HQ army enlistees as they are the individuals most likely to attend college. Therefore, I restrict the YATS sample to individuals with self-reported grades of A & B’s, the population most likely to be classified as HQ and most likely to attend college.

The resulting coefficients from the probit model are used to calculate the marginal probabilities evaluated at the mean. Given \(merit\) is a dummy variable, the marginal effect is calculated as the difference in the probability of positive propensity \((Armypos=1)\)

---

\(^{56}\)Given the transformation of the dependent variable \((y + \text{mean of non-zero } y\text{'s})\), the value of the coefficient is interpreted as \(.5 \times \frac{d\ln(y)}{dx}\) when \(y\) is evaluated at its mean. \(\ln(y+y^*)=a+bx \Rightarrow x=1/b[\ln(y+y^*)]-a/b \Rightarrow 1dx=(1/b)d[\ln(y+y^*)] \Rightarrow b=(1/dx)[dy/(y+y^*)] \Rightarrow \text{evaluated at } y=y^* \Rightarrow b=(1/dx)(dy^*/2y^*)=.5(d\ln y^*/dx).\)

\(^{57}\)New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific
given $merit=1$ and the probability of positive propensity given $merit=0$, holding all other variables constant at their means.

6. Estimation & Results.
Two-way Fixed Effects Model.
Again, given the transformation of the dependent variable, the estimated coefficients given in Table 3.4 are interpreted as $0.5 \times \frac{\text{dln}(y)}{\text{dx}}$ when $y$ is evaluated at its mean. Therefore, to ascertain the impact of each variable on HQ enlistment per eligible capita, the reported value of the coefficient is doubled$^{58}$. Using aggregate-level HQ enlistment contract data by state, this model reveals that the presence of a state merit aid program has no apparent effect (-0.004) on white male HQ Army contracts per capita. However, for black males, the presence of a state merit aid program reduces the number of HQ Army contracts per capita by a statistically significant 23%$^{59}$. In both the white and black male models, the estimated effect of recruiters (0.34, 0.40) is roughly consistent with an elasticity of 0.38-0.55 reported by Warner, Simon, and Payne (2004). Additionally, for white males the relationship between the unemployment rate and HQ enlistment per capita (0.22) is consistent with Warner, Simon, and Payne (2004). However, the estimates for black HQ enlistments are not consistent with the white male model. For blacks, an insignificant relationship between the unemployment rate and HQ enlistments exists. However, black male HQ enlistments appear to be significantly impacted by relative pay (2.06) and the percent of enlistees receiving bonuses (.178).

---

$^{58}$ Given the transformation of the dependent variable ($y + \text{mean of non-zero } y\text{'s}$), the value of the coefficient on Merit is interpreted as $0.5 \times \frac{\text{dln}(y)}{\text{dx}}$ when $y$ is evaluated at its mean. $\ln(y+y^*)=a + bx \Rightarrow x=1/[b[\ln(y+y^*)]-a/b] \Rightarrow 1dx=(1/b)d[\ln(y+y^*)] \Rightarrow b=(1/dx)[dy/(y+y^*)] \Rightarrow$ evaluated at $y=y^* \Rightarrow b=(1/dx)(dy^*/2y^*)=0.5(d\text{ln}y^*/dx)$.

$^{59}$ The observed mean value of HQ black enrollees per eligible population is 0.008.
**Probit Model.**

Results from the probit model using micro-level YATS data suggest that the propensity of black male high school students reporting A’s and A/B’s to enlist in the Army is negatively impacted by the presence of a state merit aid program. As seen in the probit coefficients in Table 3.5 and the resulting marginal probability evaluated at the mean in Table 3.6, the presence of a merit aid program reduces the probability that a black male is positively propensed to join the Army by 9.5%. In contrast, the propensity of high achieving white male high school students is positively related to the presence of a state merit aid program, which increases the probability of being positively propensed by 4.8%. However, the results are not statistically significant.

7. **Conclusions.**

Both aggregate-level and micro-level data reveal evidence that the presence of a state merit aid program decreases the number of HQ, black males that enlist in the Army (-23%) and reduces the number of HQ, black high school males that have a positive propensity to enlist (-9.5%). The aggregate-level data indicates that the enlistment decisions of HQ, white males are unaffected by a state merit aid program, while the micro-level YATS data suggests a potential positive impact on the propensity to enlist.

These results suggest that HQ, black males may view state merit aid as a substitute for MGIB benefits, and for them, the need to enlist to fund education is eliminated when a state merit program is introduced. These results are consistent with previous research indicating that enrollment decisions of blacks are more sensitive to scholarship dollars than white students. It may be plausible that as a reduced number of HQ, black males enlist recruiters compensate by focusing on HQ, white males to reach
enlistment goals. It is also possible that educational funding is not a strong incentive for HQ, white males to enlist due to availability of family financial resources, and therefore the alternative funding provided by a state merit aid program has no impact on the decision to enlist. Given the Army’s focus on attainment of enlistment goals and racially representative troops, this study’s identification of a factor that reduces HQ, black male enlistments is of significant value.
TABLE 3.1 Summary Statistics: Aggregate-level Data

Total No. Observations=3450
Unit of Observation=State quarter

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
</tr>
</thead>
<tbody>
<tr>
<td>Army HQ Contracts</td>
<td>264 (275)</td>
</tr>
<tr>
<td>Army HQ Contracts per capita</td>
<td>0.0008 (0.003)</td>
</tr>
<tr>
<td>Army HQ White Male Contracts</td>
<td>160 (154)</td>
</tr>
<tr>
<td>Army HQ White Male Contracts per Capita</td>
<td>0.001 (0.001)</td>
</tr>
<tr>
<td>Army HQ Black Male Contracts</td>
<td>29 (37)</td>
</tr>
<tr>
<td>Army HQ Black Male Contracts per Capita</td>
<td>0.002 (0.004)</td>
</tr>
<tr>
<td>Army Total Recruitment Goal</td>
<td>483 (515)</td>
</tr>
<tr>
<td>Army Bonus Amount</td>
<td>1,523 (1,532)</td>
</tr>
<tr>
<td>Army Bonus, Percent Receiving</td>
<td>0.21 (0.17)</td>
</tr>
<tr>
<td>Army Recruiters</td>
<td>106 (107)</td>
</tr>
<tr>
<td>Unemployment Rate</td>
<td>5.21 (1.47)</td>
</tr>
<tr>
<td>Relative Pay</td>
<td>1.05 (0.07)</td>
</tr>
</tbody>
</table>
TABLE 3.2 Summary Statistics: Youth Attitude Tracking Survey

Total No. Observations= 4,758

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
</tr>
</thead>
<tbody>
<tr>
<td>White</td>
<td>.92</td>
</tr>
<tr>
<td>Black</td>
<td>.08</td>
</tr>
<tr>
<td>Army Positive Propensity</td>
<td>.12</td>
</tr>
<tr>
<td>Grade A</td>
<td>.19</td>
</tr>
<tr>
<td>Grade A/B</td>
<td>.81</td>
</tr>
<tr>
<td>HS Sophomore or Junior</td>
<td>.50</td>
</tr>
<tr>
<td>HS Senior</td>
<td>.40</td>
</tr>
<tr>
<td>Age</td>
<td>16.7 (0.77)</td>
</tr>
</tbody>
</table>
### TABLE 3. 3 State Merit Aid Programs

<table>
<thead>
<tr>
<th>Year</th>
<th>State</th>
<th>Program Name</th>
<th>Value of Award at 4-year Public Institution</th>
<th>Primary Criteria</th>
</tr>
</thead>
<tbody>
<tr>
<td>1993</td>
<td>Georgia</td>
<td>HOPE&lt;sup&gt;61&lt;/sup&gt;</td>
<td>100% of tuition &amp; fees</td>
<td>GPA</td>
</tr>
<tr>
<td>1995</td>
<td>Mississippi</td>
<td>TAG&lt;sup&gt;62&lt;/sup&gt; &amp; ESG&lt;sup&gt;63&lt;/sup&gt;</td>
<td>$500-$1,000 &amp; $2,500, respectively</td>
<td>GPA &amp; standardized test score</td>
</tr>
<tr>
<td>1997</td>
<td>Florida</td>
<td>Bright Futures</td>
<td>75%-100% of tuition &amp; fees</td>
<td>GPA &amp; standardized test score</td>
</tr>
<tr>
<td>1997</td>
<td>New Mexico</td>
<td>Lottery Success Scholarship</td>
<td>100% of tuition</td>
<td>GPA</td>
</tr>
<tr>
<td>1998</td>
<td>Louisiana</td>
<td>TOPS&lt;sup&gt;64&lt;/sup&gt;</td>
<td>100% of tuition</td>
<td>GPA, standardized test score, &amp; required high school curriculum</td>
</tr>
<tr>
<td>1998</td>
<td>South Carolina</td>
<td>LIFE&lt;sup&gt;65&lt;/sup&gt;</td>
<td>1998-99: $2,000 2000-01 &amp; on: $3,000</td>
<td>GPA, standardized test score</td>
</tr>
<tr>
<td>1999</td>
<td>Kentucky</td>
<td>KEES&lt;sup&gt;66&lt;/sup&gt;</td>
<td>$500-$2,500</td>
<td>GPA &amp; standardized test score</td>
</tr>
<tr>
<td>2000</td>
<td>Nevada</td>
<td>Millennium Scholarship</td>
<td>$2,500</td>
<td>GPA</td>
</tr>
<tr>
<td>2002</td>
<td>West Virginia</td>
<td>PROMISE&lt;sup&gt;67&lt;/sup&gt;</td>
<td>100% of tuition</td>
<td>GPA &amp; standardized test score</td>
</tr>
<tr>
<td>2004</td>
<td>Tennessee</td>
<td>HOPE</td>
<td>$4,000</td>
<td>GPA &amp; standardized test score</td>
</tr>
</tbody>
</table>

<sup>60</sup> Value is per academic year and each award is renewal for up to 4 years based on continued eligibility criteria.

<sup>61</sup> Helping Outstanding Pupils Educationally

<sup>62</sup> Tuition Assistance Grant

<sup>63</sup> Eminent Scholars Grant

<sup>64</sup> Taylor Opportunity Program for Students

<sup>65</sup> Legislative Incentive for Excellence

<sup>66</sup> Kentucky Education Excellent Scholarship

<sup>67</sup> Providing Real Opportunities for Maximizing In-State Student Excellence
### TABLE 3.4 The Impact of Merit Aid Availability on HQ Army Contracts

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>ln(Army HQ Contracts per Elig. Capita + Mean Value)</th>
<th>ln(Army HQ Contracts per Elig. Capita + Mean Value)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Sample</strong></td>
<td>White Males</td>
<td>Black Males</td>
</tr>
<tr>
<td><strong>Merit</strong></td>
<td>-0.004 (0.009)</td>
<td>-0.117** (0.039)</td>
</tr>
<tr>
<td>ln(Unemployment Rate)</td>
<td>0.110** (0.011)</td>
<td>-0.032 (0.049)</td>
</tr>
<tr>
<td>ln(Relative Pay)</td>
<td>0.136** (0.058)</td>
<td>1.03** (0.264)</td>
</tr>
<tr>
<td>ln(Army Total Goal)</td>
<td>0.072** (.008)</td>
<td>0.014 (0.037)</td>
</tr>
<tr>
<td>ln(Army Recruiter per capita)</td>
<td>0.172** (0.012)</td>
<td>0.200** (0.053)</td>
</tr>
<tr>
<td>ln(Army Bonus Amount)</td>
<td>0.005 (0.008)</td>
<td>-0.064* (0.037)</td>
</tr>
<tr>
<td>ln(Army Bonus Percent Receiving)</td>
<td>-0.001 (0.009)</td>
<td>0.089** (0.042)</td>
</tr>
<tr>
<td><strong>Number of Obs.</strong></td>
<td>3408</td>
<td>339</td>
</tr>
<tr>
<td><strong>R-Squared</strong></td>
<td>.81</td>
<td>0.21</td>
</tr>
</tbody>
</table>

**Note:**
1. ** denotes significance at the 0.05 level.
2. * denotes significance at the 0.10 level.
TABLE 3.5 The Impact of Merit Aid Availability on the Propensity to Enlist

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Prob(ArmyPos=1)</th>
<th>Prob(ArmyPos=1)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sample</td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>White Males</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Black Males</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Merit</td>
<td>0.242* (0.141)</td>
<td>-0.389 (0.310)</td>
</tr>
<tr>
<td>Unemployment Rate</td>
<td>-0.006 (0.025)</td>
<td>0.032 (0.074)</td>
</tr>
<tr>
<td>Relative Pay</td>
<td>-0.038 (1.43)</td>
<td>-3.066 (3.904)</td>
</tr>
<tr>
<td>Years of Education</td>
<td>-0.294** (0.036)</td>
<td>-0.109 (0.097)</td>
</tr>
<tr>
<td>Father-HS grad</td>
<td>0.109 (0.074)</td>
<td>-0.419 (0.237)</td>
</tr>
<tr>
<td>Father-College</td>
<td>-0.087 (0.072)</td>
<td>-0.462* (0.248)</td>
</tr>
<tr>
<td>Mother-HS grad</td>
<td>-0.125 (0.079)</td>
<td>0.373 (0.249)</td>
</tr>
<tr>
<td>Mother-College</td>
<td>-0.200** (0.078)</td>
<td>0.190 (0.260)</td>
</tr>
<tr>
<td>Number of Obs.</td>
<td>4,374</td>
<td>384</td>
</tr>
<tr>
<td>-Log Likelihood</td>
<td>-1.426</td>
<td>-185</td>
</tr>
<tr>
<td>Pseudo R²</td>
<td>0.048</td>
<td>0.111</td>
</tr>
</tbody>
</table>

Note:
1. ** denotes significance at the 0.05 level.
2. * denotes significance at the 0.10 level.
TABLE 3.6 Changes in Propensity to Enlist: Marginal Probabilities Evaluated at the Mean

<table>
<thead>
<tr>
<th>Variable</th>
<th>White Males Reporting A’s &amp; B’s</th>
<th>Black Males Reporting A’s &amp;B’s</th>
</tr>
</thead>
<tbody>
<tr>
<td>Merit</td>
<td>0.048 (0.032)</td>
<td>-0.095 (0.065)</td>
</tr>
</tbody>
</table>
CHAPTER FOUR
AN EVALUATION OF THE ABILITY OF NON-EXPERIMENTAL DATA
AND PROPENSITY SCORE MATCHING TO ESTIMATE
TREATMENT EFFECTS

1. Introduction

Given that many social programs are not performed as randomized experiments, the ability of econometric estimation methods to accurately evaluate treatment effects in non-experimental settings has long been a topic of debate in the literature. Beginning with LaLonde (1986) and continuing through more recent publications, many authors have addressed the estimation of treatment effects in observational studies. While LaLonde’s original evaluation of econometric estimators indicated general poor performance by a wide range of techniques, Dehejia and Wahba (1999) suggested that particular estimation methods, namely the use of propensity score matching, do in fact provide reliable estimations of treatment effects. Such an assertion drew much focus on propensity score matching in the literature and garnered subsequent publications from additional authors who sought to highlight weaknesses in the work of Dehejia and Wahba. These additional publications served to temper the view that propensity score matching provided a general solution to the difficult problem of estimating non-experimental treatment effects. This paper seeks to replicate and summarize findings presented by previous researchers on this topic, provide an evaluation of their results, and discuss general concerns regarding the use of propensity scores to facilitate the evaluation of treatment effects.
Using the unique setting of the National Supported Work (NSW) Demonstration to evaluate the ability of econometric estimators to correctly assess treatment effects using non-experimental data, this paper identifies a number of factors which may negatively impact the performance of the estimator, including (1) sizable standard errors of both the training effect and the estimated treatment effect, (2) sensitivity of the logit estimates to the number of comparison units used in the estimation, (3) incorrect selection of the propensity score specification and stratum size, and (4) use of a control sample from a different labor market.

2. Background

Because true, large-scale randomized experiments are often both prohibitively costly and difficult to implement, much focus in the econometric literature has been placed on the ability of constructed, non-experimental comparison groups and econometric estimators to accurately evaluate treatment effects. Notably, LaLonde (1986) was one of the first to perform a comprehensive evaluation of econometric techniques and their ability to correctly estimate the treatment effect of a randomized job training field experiment. LaLonde’s evaluation was implemented using data from the National Supported Work Demonstration (NSW), which was a temporary employment program devised to transition disadvantaged workers into the general labor force. (The program is described in more detail in Section 4, NSW and Sample Background Information.) The program was put into practice in the mid-1970s, and for LaLonde the outcome of interest was the impact of the training program on real earnings in 1978. A number of estimators were evaluated for their ability to accurately assess the benefit
received from program participation by comparing the econometric estimate to the actual results from the field experiment, which were measured as the simple difference in mean real earnings in 1978 between the treatment and control group. The treatment effects were calculated for two groups: (1) Aid to Families with Dependent Children (AFDC) participants and (2) male participants. Overall, LaLonde concluded that the majority of the econometric methods utilizing constructed comparison groups to evaluate the NSW program produced inaccurate or imprecise measures of the treatment effect. Furthermore, even the successful completion of typical specification tests did not guarantee that the estimators provided correct assessment of the treatment impact.

Ultimately, LaLonde’s investigation attracted the interest of many researchers, and his work and the NSW data have since become widely known in the field of labor economics. Dehejia and Wahba (DW 1999) were the first to re-examine and extend the work of LaLonde. DW estimate the treatment impact of the NSW Demonstration on post-program earnings through the use of propensity score methods. The authors assert that their propensity score estimates, unlike the econometric estimates presented by LaLonde, more accurately assess the treatment effect. DW first reproduce LaLonde’s results before applying their own methods to LaLonde’s data. It is important to note that DW use only a subset of LaLonde’s data in order to include several years of pre-intervention earnings judged necessary to ascertain the true impact of the job training program; both the treatment and control groups are reduced by approximately forty percent, but DW assert that LaLonde’s message is not altered. The work concludes that when common support between the treatment and comparison groups exists and the
variables that determine participation in the treatment are known, propensity scores are an appropriate and high quality method to estimate the impact of the treatment. As stated directly by DW (1999), the propensity score allows emphasis to be placed on the comparability of the control group to the treatment group and thus functional form and treatment effect heterogeneity can be addressed with less difficulty. The use of matching on the propensity score and stratification methods produce estimates that are merely 5-10% below the true estimate produced using the simple difference in mean real earnings between the treatment and control groups.

As a response to the work of DW, Smith and Todd (2005) also address the debate to determine if non-experimental data and econometric techniques can reliably evaluate social programs and find that the propensity score methods used by DW are “highly sensitive to both the set of variables included in the scores and the particular analysis sample used in the estimation” (ST 2005). They go on to assert that the difference-in-difference matching estimator is the superior estimator among those they examine. Overall, ST suggest that propensity score matching, although a useful tool, is not a universal resolution for all evaluation problems.

ST begin by citing earlier works by Heckman, Ichimura, and Todd (1997) and Heckman, Ichimura, Smith and Todd (1996), which precede the work of DW, and use experimental data from a different program, the U.S. National Job Training Partnership Act (JTPA) Study. As summarized by ST, the respective works by HIT and HIST conclude that

“in order for matching estimators to have low bias, it is important that the data include a rich set of variables related to program participation and
labor market outcomes, that the non-experimental comparison group be drawn from the same local labor markets as the participants, and that the dependent variable (usually earnings) be measured in the same way for the participants and non-participants” (ST 2005).

ST highlight the fact that none of these conditions are true for the data used by LaLonde and later DW. Ultimately, ST question DW’s assertion that propensity score matching is generally more effective than other traditional econometric tools and suggest that difference-in-difference matching estimators perform superior to propensity score estimators. Finally, ST found that although cross-sectional and difference-in-difference matching produce significantly different estimated biases, the type of matching selected for use (nearest neighbor matching, local linear matching, etc.) does not have a significant impact on estimations.

Following ST’s initial publication, a series of comments and responses commenced between DW and ST that continued the debate regarding the ability of propensity score matching to facilitate the estimation of accurate treatment effects. DW’s response to ST’s analysis suggests that ST misinterpreted their claims and made several critical missteps when completing their own estimations. DW stress that they did not claim that matching estimators were a universal solution to the evaluation problem but a potentially reliable tool for evaluation. DW cite their previously published statement that a different specification of the propensity score must be created for each different blend of treatment and comparison groups. Because ST failed to specify a new propensity score that correctly balances the covariates between the treatment and control groups for
their unique sample and misapply DW’s specification to their own sample, DW found
ST’s inability to accurately replicate the experimental treatment effects expected.

ST provided a rejoinder to the comments of DW in which they do not passively
accept DW’s assertion of the power of propensity score matching. In particular, they
question DW’s use of a subsample of the data without a clearly stated justification. The
idea that the propensity score specification must be altered due to simply restricting the
dates of assignment to the program and choosing to exclude some observations with zero
earnings is troubling to ST: all three samples seek to answer the same question, and
therefore it seems more intuitive that the propensity score should not require major
alteration for use with each of the samples. In this rejoinder, ST again stress that DW’s
low-bias estimates are highly sensitive to the composition of the sample and the
propensity score specification.

This paper seeks to replicate and summarize findings presented by these previous
researchers, evaluate the validity of their claims, and provide further points of discussion.

3. Methodology

3.1 Identification of Treatment Effect

Evaluation of a treatment seeks to assess the impact of the treatment on a
particular outcome. Cameron and Trivedi (2005) provide extensive coverage of
treatment evaluation that serves as a general reference for the discussion developed here.
In the context of this paper, the treatment is participation in the NSW program and the
outcome of interest is post-training earnings in 1978. Given that treatment assignment
occurs as a randomized experiment, as in the case of the NSW Demonstration, the following conditional independence assumption holds:

\[ y_{i0}, y_{i1} \perp D|x \]  

Here at the individual level, \( y_{i0} \) is the potential value of the outcome given unit \( i \) was unexposed to the treatment regime (0) and \( y_{i1} \) is the potential value of the outcome given unit \( i \) was exposed to the treatment regime (1). Thus, \( y_{i0} \) belongs to the control group and \( y_{i1} \) is a member of the treatment group. Because of the random assignment, the outcomes are independent from \( D \), where \( D \) assumes a value of 1 if the treatment is applied and a value of 0 otherwise. The random assignment process also generates treatment and control groups with similar distributions of both observed and unobserved characteristics, and it is this similar distribution that plays a vital role in the treatment effect estimation.

Additionally, estimation of the treatment effect requires the satisfaction of the overlap or matching assumption such that

\[ 0 < \Pr[D = 1|x] < 1. \]  

(2)

Thus, for each value of \( x \), or set of covariates, there are both treated and control cases, or overlap between the two subsamples. This ensures that each program participant is matched to an analogous nonparticipant, allowing identification of the treatment effect on a randomly selected individual.

Finally, the conditional independence assumption necessitates that the untreated outcome does not determine participation:

\[ E[y_0|D = 1,x] = E[y_0|D = 0,x] = E[y_0|x]. \]  

(3)
Given these assumptions hold, the average treatment effect (as indicated by the random experiment) can be calculated as follows:

\[ \tau = E(y_{i1}) - E(y_{i0}). \] (4)

Using the conditional mean independence assumption, the following treatment effect emerges:

\[ \tau = E(y_i \mid D_i = 1) - E(y_i \mid D_i = 0) = E(y_i \mid D_i = 1) - E(y_i \mid D_i = 0). \] (5)

In contrast to the experimental setting, an observational study does not provide treatment and control units from the same population; instead a non-experimental comparison group must be formed from a separate population, and here this population is crafted from PSID or CPS data. In this setting, identification of the treatment effect on the treated is given by

\[ \tau_{D=1} = E(y_{i1} \mid D_i = 1) - E(y_{i0} \mid D_i = 1). \] (6)

However, \( y_{i0} \) is clearly not observed for treated units, but given \( \{y_{i0}, y_{i1} \perp D_i\} \mid X_i \) the following is produced:

\[ E[y_j \mid X_i, D_i = 1] = E[y_j \mid X_i, D_i = 0] = E[y_j \mid X_i, D_i = j] \] (7)

for \( j=0,1 \) and the treatment effect for the treated (ATT) is then identified as

\[ \tau_{D=1} = E \left\{ E(y_i \mid X_i, D_i = 1) - E(y_i \mid X_i, D_i = 0) \mid D_i = 1 \right\}. \] (8)

### 3.2 Propensity Scores

DW focus on the ability of propensity score matching to provide accurate treatment evaluation when a true experimental control group is unavailable. As defined
by Rosenbaum and Rubin (1983), the propensity score is the conditional probability of receiving treatment based on pre-treatment characteristics,

\[ p(x) = \Pr[D = 1|X = x]. \]  

(9)

The balancing condition states that \( D \perp x|p(x) \), or that assignment to treatment is random for individuals with the same propensity score and that individuals with the same score have \( x \) vectors with similar distributions for \( D=0 \) and \( D=1 \). This testable hypothesis is critical for correct treatment evaluation; conditioning on a propensity score that satisfies the balancing condition replicates a randomized experiment by ensuring that each individual has the same probability of receiving treatment.

Given the data \((D_i, x_i)\), this score can be calculated using numerous parametric or semi-parametric methods. Following DW and ST, a logit regression is used to produce the propensity score within this paper.

3.3 Estimation using Single Matching

Given the estimated propensity score, each treated unit is matched with the comparison unit that has the closest propensity score. Within this paper, matching occurs with replacement, and if a treated unit is matched equally well by comparison units equidistant in absolute value, the program randomly draws the forward or backward match. This procedure creates the treatment and comparison units necessary to evaluate the treatment effect.

3.4 Estimation using Stratification

The stratification method divides the range of variation of the propensity score into intervals in which treated and comparison units have the same average score. Within
each interval that contains both treated and comparison units, the difference between the average treated outcome and the average untreated outcome is calculated. Finally, the average treatment effect on the treated is computed as the average of the average treatment on the treated (ATT) effect of each block weighted by the distribution of the treated units across the blocks.

4. Data

The National Supported Work (NSW) Demonstration originated to examine the ability of a 12 to 18 month training program to enable traditionally difficult to employ individuals to secure and sustain employment.\(^\text{68}\) The “guiding principle of the supported work experiment is that by participating in the program, a significant number of people who are severely handicapped for employment may be able to join the labor force and do productive work, cease engaging in socially destructive or dependent behavior, and become self-supporting members of society” (MDRC 1980).

The program focused on four “hard-to-employ” groups including women on Aid to Families with Dependent Children (AFDC); former addicts and offenders; and youth who had failed to complete high school. Eligibility for participation in the program was determined using the following primary criterion: (1) participants were currently unemployed as indicated by working no more than 40 hours in the 4 weeks prior to acceptance into the program and (2) participants had an irregular work history.\(^\text{69}\) It is important to note that participants were thus vastly different from the general public in

\(^{68}\) Detailed information regarding the NSW Demonstration as summarized in this section can be found in the published text *Summary and Findings of the National Supported Work Demonstrations* (1980) by Board of Directors, Manpower Demonstration Research Corporation.

\(^{69}\) During the 6 months prior to the program, the participants at maximum spent 20 hours per week on one regular job for 3 months.
many regards; most were non-Caucasian, less than a third held a high school degree, and less than 25% were married.

The supported work demonstration took place in the mid-1970s at 15 sites across the country and employed 10,043 participants. Local agencies in the 15 sites recruited eligible workers and developed work opportunities for participants that paid at or above minimum wage. These work environments were meant to provide a realistic work experience, and after maximum participation in the program was reached at 12 or 18 months, the local agencies were to aid the supported workers in securing normal employment.

A major focus of the demonstration was a comprehensive research effort to enable the Manpower Demonstration Research Corporation (MDRC) to evaluate the program’s effectiveness relative to its cost. Because of the research-oriented nature of the program, the program was operated as a randomized experiment at 10 of the 15 locations. In accordance with the principles of a randomized experiment, qualified applicants were arbitrarily assigned to participate in the program (receive treatment) or serve as a member of the control group. As a result, a research sample of 6,616 individuals was composed of 3,214 participants and 3,402 controls. Each person in the sample participated in a baseline interview and potentially four successive interviews, each at 9-month intervals. Additionally, the information collected from these interviews was verified using the appropriate government records. Because of this experimental design, the simple difference between the mean post-training earnings of the treatment and control groups is
used as an unbiased estimator of the training effect, and it is against this difference that econometric estimates of the treatment effect are compared for accuracy.

The research effort produced vast amounts of data that have now been utilized by various researchers in an attempt to evaluate the impact of the training program. As previously discussed in Section 2, a number of authors have used the data from the NSW Demonstration to evaluate the effectiveness of various econometric methods used to measure treatment effects. This paper first discusses LaLonde’s original subsample of the NSW data but will ultimately focus on the two experimental subsamples used by DW and ST, along with two non-experimental comparison groups.

The first experimental sample is that originally used by LaLonde. LaLonde concentrated on male participants in the NSW sample and further focused on those who were assigned to treatment post-December 1975 and exited the program prior to January 1978. These qualifications for inclusion in the sample ensure that “sufficient” pre-intervention and post-intervention earnings data are available for analysis. These guidelines reduce LaLonde’s sample size to 297 treated observations and 425 control observations due to limited availability of pre-intervention earnings for the entirety of the NSW sample. Additionally, the difference in size of the treated and control observations is due to (1) the exclusion of treated individuals still in “Supported Work” in January 1978 and (2) the design of the program that used more controls than treatments.

The second experimental sample is taken from the work of DW. DW suggest that additional years of pre-intervention earnings are necessary to establish the effect of the job training program, and thus the DW sample is a subset of the original sample used
by LaLonde that contains only individuals for which earnings information is available in 1974. This requirement allows two years of pre-intervention earnings to be used in the propensity score calculation but reduces the sample size to 185 treated observations and 260 control observations.

ST created the third experimental sample examined in this paper. This subset of DW’s sample (and thus also a subset of LaLonde’s sample) rejects inclusion of individuals randomized into the program post April 1976, resulting in a reduced sample size of 108 treated observations and 142 control observations. ST assert that DW made errors by choosing to include individuals randomly assigned post April 1976 with zero earnings in months 13-24 and failing to include individuals randomly assigned post April 1976 with positive earnings during this period due to their desire to use 1974 earnings as a control variable. ST readily identify that for these individuals randomized post April 1976, earnings in months 13-24 before randomization do not directly correspond to the calendar year 1974.

The two non-experimental comparison groups used here are the original comparison groups created by LaLonde and used by both DW and ST. LaLonde drew from the general population to create these two groups. The first group is drawn from Westat’s Matched Current Population Survey—Social Security Administration File (CPS-SSA), while the second group is created from the Panel Study of Income Dynamics (PSID). Both groups are produced through randomized sampling of populations of households and head of households. Finally, in his original work LaLonde considers four
other comparison groups that are subsamples of the PSID and CPS groups that will not be addressed in this paper.

Summary statistics for all samples are found in Table 4.1. (Note: all samples use 1982 dollars as the unit of measure.) It is important to note that while the treatment and control groups are similar in composition, the means of several key variables greatly differ between the experimental samples and the non-experimental comparison groups. It is apparent that both the CPS and PSID comparison groups are older, have more education, contain fewer minorities, and demonstrate higher earnings in comparison to the treatment group. Thus, it is clear that these two populations are significantly different from the treatment group.

5. Estimation

To first attempt replication of DW’s results, the propensity score is calculated using a logit model and following the specification given within DW (1999) for the DW subsample of treated units and PSID and CPS comparison groups.\(^{70}\) The specification varies for each particular treatment and comparison group combination such that the pre-intervention covariates are balanced between each treatment and comparison group conditional on the propensity score. DW (2005) notes that the search for a specification that satisfies the balancing condition is not indicative of data mining because the search procedure does not examine outcomes, only covariates. Histograms of the estimated

\(^{70}\) PSID: \(\text{Prob}(T=1)=F(\text{age}, \text{age}^2, \text{education}, \text{education}^2, \text{married}, \text{no degree}, \text{black}, \text{Hispanic}, \text{RE74}, \text{RE75}, \text{RE74}^2, \text{RE75}^2, u74*\text{black})\) Note: \(u74*\text{black}\) is an interaction term between black and a dummy for zero earning in 1974.

CPS: \(\text{Prob}(T=1)= (\text{age}, \text{age}^2, \text{age}^3, \text{education}, \text{education}^2, \text{no degree}, \text{married}, \text{black}, \text{Hispanic}, \text{RE74}, \text{RE75}, u74, u75, \text{education*RE74})\) Note: \(u74\) and \(u75\) are dummies for zero earnings in each respective year. Education*RE74 is an interaction term between education and 1974 real earnings.
propensity scores for the DW subsample of NSW treated units and the PSID/CPS comparison units are shown in Figures 4.1 and 4.2. These figures illustrate that although the comparison groups are quite large relative to the treatment group the pre-intervention covariates are quite different and have little overlap. When these figures are compared to the histogram of the estimated propensity score (Figure 4.3) for the treatment and control units, it is clear that the true control group, relative to the non-experimental comparison group, has a distribution of covariates much more similar to the treatment group.

Next stratification and nearest neighbor matching on the propensity score (as described in Sections 3.3 and 3.4) are used to estimate the treatment effect. Table 4.2 presents the results. The estimations within this paper failed to reproduce the previously published results by DW. For the PSID sample, the matching estimate of the ATT effect is $1,447 and the stratification estimate is $2,210, compared to $1,608 and $1,691 as estimated by DW and compared to the randomized-experiment estimate of $1,794. In the case of the stratification estimate, the deviation from the DW estimate is not surprising; one must know the number of stratum used by DW within the estimation process to replicate their result, and this information is not available. In the case of the matching estimation, it is possible that a difference in nearest neighbor determination or weighting techniques produced the estimate below that of DW’s. In both cases, general propensity score techniques fail to replicate the results estimated by DW and produce estimates that vary more greatly from the randomized-experiment estimate.

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71 This randomized-experiment estimate is a simple difference in mean real earnings between the treatment and control groups in DW’s subsample of the NSW data.
The propensity specifications for DW’s subsamples are next applied to the subsample created by ST. It is important to note that this step, although originally executed by ST, is incorrect: as previously indicated, each unique combination of a treatment and comparison group requires a distinctive propensity score specification. This incorrect step is completed here to demonstrate a common error and its impact on estimation. The results from stratification and nearest neighbor matching on the propensity score are given in Table 4.3. Matching on the propensity score produces estimates of $2,034 (PSID) and $781(CPS), while stratification estimates are $1,305(PSID) and $1,373(CPS). In all cases, the estimates fail to closely match the randomized-experiment estimate of $2,717. Again, stratification estimations are not expected to exactly replicate the previous researchers’ results given that the number of stratum used in the original procedure is unknown. However, this is a point of discussion to be addressed later as part of evaluation of the estimation techniques.

As DW assert in their reply to ST, the ST samples require a different propensity score specification from the specification devised for the DW samples. If the propensity score specifications devised specifically for the ST samples as given in DW’s (2005) reply to ST are employed, the performance of the nearest neighbor matching estimation conditional on the propensity greatly improve.

Results are presented in Table 4.4. Use of the PSID and CPS comparison groups with the NSW treated units and nearest neighbor matching estimation now produce average treatment effects on the treated of

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72 This differs from the previously stated randomized-experiment estimate because the subsample used by ST is different than the DW subsample.
73 PSID: Prob(T=1)=f(RE74, RE75, married, black, Hispanic, education, age, married*1(Re75=0), no degree*1(RE74=0)
CPS: Prob(T=1)=f(RE74, RE75, married, black, Hispanic, education, age, black*age)
$2,626 and $2,678 respectively, which are both similar to the benchmark value of $2,717 from the randomized-experiment. The impact of this specification change is not examined for stratification, again due to inability to replicate specific technique used by prior authors.

6. Evaluation of Estimates

All of the prior authors focus on the ability of the non-experimental data and the econometric methods to replicate the training effect, which is calculated as the difference between the mean post-training earnings of the experimental treatment and control groups. However, none of these authors first examine the validity of using this estimate as the benchmark to which the econometric estimates should be compared. For LaLonde’s subsample the training effect is calculated to be $886 with a standard error of $476. Training effects for the DW and ST subsamples are presented to be $1,794 and $2,717, with respective standard errors of $633 and $956. In all of these subsamples, the standard errors are at minimum 35% of the estimated effect, and in the case of LaLonde’s subsample, over 50% of the estimated effect. The large standard errors, due in part to the small sample sizes, should cause researchers to question the use of these figures as benchmarks to which meaningful comparisons can be made.

Standard errors are also of concern when examining the treatment effect estimations produced through the use of stratification and matching on the propensity score. Ignoring the inability of this paper to replicate the original stratification and matching results of DW and instead examining the results in DW (1999), the estimated treatment effects from using the PSID and CPS comparison groups have standard errors
that range in value from 65% to over 130% of the estimated effect. Standard errors of similar magnitude also exist for the ST estimations and all estimations within this paper. When considering first the standard error of the benchmark effect estimation and additionally the standard errors of the estimated effects, one must question if the comparison between the benchmark treatment effect value and the estimated treated effect value is meaningful.

In this particular case, the large imbalance between the number of treated units (DW: 185; ST: 108) and the number of comparison units (PSID: 15,992; CPS: 2,490) may also introduce another problematic issue. The logit model may estimate a significantly different propensity score in circumstances where the size discrepancy between the two groups is reduced.

Even if ignoring the above issues, there still remain some difficulties with the strength of the propensity score as a tool for treatment effect estimation, many of which are highlighted by ST. This paper suggests that these difficulties indicate that propensity score matching is not a universal solution and that the circumstances under which propensity score matching is applied should be carefully considered. First, as demonstrated when the DW propensity score specification is applied to the ST sample, the use of propensity scores necessitate that the score is correctly specified to balance the covariates for the given sample of treated and comparison units. Application of an incorrect specification not exclusively designed for the given sample can lead to inaccurate results. The econometrician must therefore be careful to ensure that the best specification has been used, but this task may be quite challenging. Some of the
interaction terms created for use in the specifications created by DW suggest a process of specification selection that is not clearly obvious. This paper also agrees with criticisms as stated in the Rejoinder by Smith and Todd (2005): it is troubling that minor changes in the sample require major alterations in the propensity score. For example, the ST sample used treated units that were a subsample of the DW sample and identical comparison units, but the propensity score specifications are quite different.

Another issue arises with the use of the propensity score and the stratification technique. It appears that the number of stratum, or intervals, into which the variation of the propensity score is divided can significantly impact the estimation of the treatment effect. Researchers must be careful to ensure that the optimal interval selection is made. This further suggests that a highly generalized stratification method cannot be applied to all cases but must be case specific.

In addition to the concerns discussed above, the criteria as published by HIT (1996) and HIST (1997) suggest that it is critical that (1) the non-experimental comparison groups are drawn from the same labor market as the treatment group and (2) information is collected from the comparison units in the same manner in which treated individuals are questioned. These criteria were not met in the DW and ST samples and may have decreased the quality of their results. These requirements also suggest that the circumstances in which propensity score matching is appropriate are even further limited.

7. Conclusions

The work by LaLonde, DW, and ST makes a meaningful contribution to the econometric literature, and even in light of several difficulties discussed within this
paper, the work warrants careful consideration by readers. The NSW data provided a unique setting in which to evaluate the ability of econometric estimators using non-experimental data to assess treatment effects. Given that the benchmark, or true treatment effect as calculated from the experimental data, is accepted as legitimate, the publications of DW and ST illustrate that the propensity score method may have the potential to closely replicate the actual treatment effect, but the method requires the satisfaction of a number of criteria. Variations in the application of the method may lead to differing results, and the method may also be outperformed by other techniques. Thus, matching on the propensity score is not always the superior estimation method. Ultimately, propensity scores, not unlike other econometric techniques, cannot be applied in all non-experimental settings; researchers must proceed with discretion.
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<th>No Degree</th>
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<th>RE75</th>
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<td></td>
<td></td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treated</td>
<td>108</td>
<td>25.37</td>
<td>10.49</td>
<td>0.82</td>
<td>0.07</td>
<td>0.20</td>
<td>0.71</td>
<td>3,590</td>
<td>2,596</td>
<td>7,357</td>
</tr>
<tr>
<td>Control</td>
<td>142</td>
<td>26.01</td>
<td>10.27</td>
<td>0.82</td>
<td>0.11</td>
<td>0.19</td>
<td>0.80</td>
<td>3,858</td>
<td>2,277</td>
<td>4,609</td>
</tr>
<tr>
<td>Comparison$^{77}$:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>PSID</td>
<td>2,490</td>
<td>33.23</td>
<td>12.03</td>
<td>0.07</td>
<td>0.07</td>
<td>0.71</td>
<td>0.30</td>
<td>14,024</td>
<td>13,643</td>
<td>14,856</td>
</tr>
<tr>
<td>CPS</td>
<td>15,992</td>
<td>34.85</td>
<td>12.12</td>
<td>0.25</td>
<td>0.03</td>
<td>0.87</td>
<td>0.31</td>
<td>19,431</td>
<td>19,063</td>
<td>21,553</td>
</tr>
</tbody>
</table>

Note: Standard deviations are given in parentheses. RExx=earnings in calendar year 19xx expressed in 1982 dollars.

$^{74}$ NSW sample as constructed by LaLonde (1986)

$^{75}$ The DW subset of the LaLonde sample for which real earnings in 1974 (RE74) are available.

$^{76}$ The ST subset of the LaLonde sample for which real earnings in 1974 are available. The subset excludes persons randomized after April 1976.

$^{77}$ Comparison groups as constructed by LaLonde (1986). PSID: All male household heads under age 55 who did not classify themselves as retired in 1975. CPS: All CPS males under age 55.
Table 4.2 Estimated Training Effect: DW Treatment Subsample & DW Propensity Score Specification

<table>
<thead>
<tr>
<th>Comparison Group</th>
<th>Observations</th>
<th>NSW Treatment Earnings Less Comparison Group Earnings</th>
<th>NSW Treatment Earnings Less Comparison Group Earnings, Conditional on the Estimated Propensity Score</th>
</tr>
</thead>
<tbody>
<tr>
<td>NSW DW Subsample</td>
<td>445</td>
<td>1,794</td>
<td>1,447</td>
</tr>
<tr>
<td>PSID</td>
<td>1,339</td>
<td>-15,205</td>
<td>1,420</td>
</tr>
<tr>
<td>CPS</td>
<td>4,042</td>
<td>-8,498</td>
<td>1,239</td>
</tr>
</tbody>
</table>

Note: Shown above in parentheses, standard errors are calculated using bootstrapping techniques.

78 The number of observations denotes the actual number of comparison and treatment units used in the calculations.
Table 4.3 Estimated Training Effect: ST Treatment Subsample & DW Propensity Score Specification

<table>
<thead>
<tr>
<th>Comparison Group</th>
<th>Observations&lt;sup&gt;79&lt;/sup&gt;</th>
<th>NSW Treatment Earnings Less Comparison Group Earnings</th>
<th>NSW Treatment Earnings Less Comparison Group Earnings, Conditional on the Estimated Propensity Score</th>
</tr>
</thead>
<tbody>
<tr>
<td>NSW DW Subsample</td>
<td>250</td>
<td>2,717 (956)</td>
<td></td>
</tr>
<tr>
<td>PSID</td>
<td>1,101</td>
<td>-15,205 (1,154)</td>
<td>2,034 (1,460)</td>
</tr>
<tr>
<td>CPS</td>
<td>3,382</td>
<td>-8,498 (712)</td>
<td>781 (1,320)</td>
</tr>
</tbody>
</table>

Note: Shown above in parentheses, standard errors are calculated using bootstrapping techniques.

<sup>79</sup> The number of observations denotes the actual number of comparison and treatment units used in the calculations.
Table 4.4 Estimated Training Effect: ST Treatment Subsample & ST Propensity Score Specification

<table>
<thead>
<tr>
<th>Comparison Group</th>
<th>Observations</th>
<th>NSW Treatment Earnings Less Comparison Group Earnings</th>
<th>NSW Treatment Earnings Less Comparison Group Earnings, Conditional on the Estimated Propensity Score</th>
</tr>
</thead>
<tbody>
<tr>
<td>NSW DW Subsample</td>
<td>250</td>
<td>2,717 (956)</td>
<td></td>
</tr>
<tr>
<td>PSID</td>
<td>1,298</td>
<td>-15,205 (1,154)</td>
<td>2,626 (1,528)</td>
</tr>
<tr>
<td>CPS</td>
<td>5,362</td>
<td>-8,498 (712)</td>
<td>2,678 (1,110)</td>
</tr>
</tbody>
</table>

Note: Shown above in parentheses, standard errors are calculated using bootstrapping techniques.

---

80 The number of observations denotes the actual number of comparison and treatment units used in the calculations.
FIGURE 4.1 Propensity Score – DW Subsample of NSW Treated Units and PSID Comparison Units

![Histogram of Propensity Scores for NSW Treated Units and PSID Comparison Units](image1)

FIGURE 4.2 Propensity Score – DW Subsample of NSW Treated Units and CPS Comparison Units

![Histogram of Propensity Scores for NSW Treated Units and CPS Comparison Units](image2)

FIGURE 4.3 Propensity Score – DW Subsample of NSW Treated Units and NSW Control Units

![Histogram of Propensity Scores for NSW Treated Units and NSW Control Units](image3)
Appendix A

Merit Program Profiles

FLORIDA

**Program Name:**
Bright Futures Scholarship Program

**Year of Implementation:**
Academic year 1997-98

**Award at Public Institutions during study period:**
- Florida Academic Scholars Award: 100% of tuition & fees + $600
- Florida Medallion Scholars Award: 75% of tuition & fees
- Florida Gold Seal Vocational Scholars Award: 75% of tuition & fees

**Average Public Instate Tuition & Fees Year Prior to Implementation:**
$1,970

**Award Value Relative to Average Tuition & Fees:**
75%-100%

**Duration of Award:**
4 years

**Initial Eligibility:**

<table>
<thead>
<tr>
<th>Award</th>
<th>Minimum GPA</th>
<th>Minimum Test Score</th>
<th>Service</th>
</tr>
</thead>
<tbody>
<tr>
<td>Academic Scholars Award</td>
<td>3.5</td>
<td>1270 SAT (28 ACT)</td>
<td>75 hrs</td>
</tr>
<tr>
<td>Medallion Scholars Award</td>
<td>3.0</td>
<td>970 SAT (20 ACT)</td>
<td></td>
</tr>
<tr>
<td>Gold Seal Vocational Scholar</td>
<td>2.75 GPA</td>
<td></td>
<td>various</td>
</tr>
</tbody>
</table>

**Income Restrictions:**
None

**Continued Eligibility:**
- Academic Scholars Award: 3.0 GPA
- Medallion Scholars Award: 2.75 GPA
- Gold Seal Vocational Scholar: 2.75 GPA
Number of Awards:

<table>
<thead>
<tr>
<th>Year</th>
<th>#Awards used at % of Public Institutions</th>
<th>Students Receiving Awards</th>
<th>% of Public, 4-Yr Undergraduate FTE</th>
</tr>
</thead>
<tbody>
<tr>
<td>1997-98</td>
<td>27,744</td>
<td></td>
<td>20%</td>
</tr>
<tr>
<td>1998-99</td>
<td>40,636</td>
<td></td>
<td>29%</td>
</tr>
<tr>
<td>1999-00</td>
<td>56,838</td>
<td></td>
<td>39%</td>
</tr>
<tr>
<td>2000-01</td>
<td>73,212</td>
<td></td>
<td>48%</td>
</tr>
</tbody>
</table>

Expenditure on Awards:

<table>
<thead>
<tr>
<th>Year</th>
<th>Dollar (current year)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1997-98</td>
<td>$69.6M</td>
</tr>
<tr>
<td>1998-99</td>
<td>$93.3M</td>
</tr>
<tr>
<td>1999-00</td>
<td>$132.1M</td>
</tr>
<tr>
<td>2000-01</td>
<td>$164.9M</td>
</tr>
</tbody>
</table>

Funding Source:
Lottery

Notes:
The Bright Futures Scholarship Program replaced the Florida Undergraduate Scholars Fund (FUSF), which was created in 1981. Bright Futures was implemented to combat the deflating value of the FUSF awards, which were flat in nature, and lowered the standards required to receive a merit award.

Sources:


---

81 This figure includes Academic Scholars Awards and Medallion Scholars Awards.
Program Name: Helping Outstanding Pupils Educationally (HOPE)

Year of Implementation: Academic year 1993-94

Award at Public Institutions during study period:
1993: 100% of tuition at public Georgia institutions
1994 & on: 100% of tuition plus fees and a book allowance

Average Public Instate Tuition & Fees Year Prior to Implementation: $2,105

Award Value Relative to Average Tuition & Fees: 100%

Duration of Award:
1993: 2 years subject to eligibility
1994: 4 years subject to eligibility

Initial Eligibility:
3.0 cumulative high school GPA

Income Restrictions:
1993: Income cap of $66,000
1994: Income cap of $100,000
1995: Income cap lifted

Continued Eligibility:
3.0 GPA

Number of Awards:

<table>
<thead>
<tr>
<th>Year</th>
<th>#Awards used at Public Institutions</th>
<th>% of Public, 4-Yr Undergraduate FTE Students Receiving Awards</th>
</tr>
</thead>
<tbody>
<tr>
<td>1993-94</td>
<td>9,736</td>
<td>10%</td>
</tr>
<tr>
<td>1994-95</td>
<td>28,967</td>
<td>28%</td>
</tr>
<tr>
<td>1995-96</td>
<td>42,334</td>
<td>40%</td>
</tr>
<tr>
<td>1996-97</td>
<td>48,471</td>
<td>46%</td>
</tr>
</tbody>
</table>
Expenditure on Awards:
Year          Dollar (current year)
1993-94      $21.4M
1994-95      $83.8M
1995-96      $133.9M
1996-97      $153.4M

Funding Source:
Lottery

Source:

Georgia Student Finance Commission. 2011. Scholarship and Grant Award History.
http://www.gsfc.org/gsfnew/SandG_facts.CFM.
**Program Name:**
Kentucky Educational Excellence Scholarship (KEES) Program

**Year of Implementation:**
Academic year 1999-00

**Award at Public Institutions during study period:**
Incremental based on criteria up to $2,000 for each year at any in-state institution
1999-00: Max $1,000
2000-01: Max $1,500
2001-02: Max $2,000
2002-03: Max $2,500

**Base Award Amount**
The base scholarship amount is calculated using the students GPA from each year of high school. For example, a student earning a 3.0 in each year of high school would receive base award valued at $1,000 (4 x $250).

<table>
<thead>
<tr>
<th>GPA</th>
<th>Amount</th>
</tr>
</thead>
<tbody>
<tr>
<td>2.50</td>
<td>$125</td>
</tr>
<tr>
<td>2.60</td>
<td>$150</td>
</tr>
<tr>
<td>2.70</td>
<td>$175</td>
</tr>
<tr>
<td>2.75</td>
<td>$187</td>
</tr>
<tr>
<td>2.80</td>
<td>$200</td>
</tr>
<tr>
<td>2.90</td>
<td>$225</td>
</tr>
<tr>
<td>3.00</td>
<td>$250</td>
</tr>
<tr>
<td>3.10</td>
<td>$275</td>
</tr>
<tr>
<td>3.20</td>
<td>$300</td>
</tr>
<tr>
<td>3.25</td>
<td>$312</td>
</tr>
<tr>
<td>3.30</td>
<td>$325</td>
</tr>
<tr>
<td>3.40</td>
<td>$350</td>
</tr>
<tr>
<td>3.50</td>
<td>$375</td>
</tr>
<tr>
<td>3.60</td>
<td>$400</td>
</tr>
<tr>
<td>3.70</td>
<td>$425</td>
</tr>
<tr>
<td>3.75</td>
<td>$437</td>
</tr>
<tr>
<td>3.80</td>
<td>$450</td>
</tr>
<tr>
<td>3.90</td>
<td>$475</td>
</tr>
<tr>
<td>4.00</td>
<td>$500</td>
</tr>
</tbody>
</table>

**Supplemental Award Amount**
The supplemental award is based on the student's highest ACT score attained by the date of graduation from high school. Values of the supplemental award are given below:

<table>
<thead>
<tr>
<th>Score</th>
<th>Amt</th>
<th>Score</th>
<th>Amt</th>
<th>Score</th>
<th>Amt</th>
<th>Score</th>
<th>Amt</th>
</tr>
</thead>
<tbody>
<tr>
<td>15</td>
<td>$36</td>
<td>19</td>
<td>$179</td>
<td>23</td>
<td>$321</td>
<td>26</td>
<td>$428</td>
</tr>
<tr>
<td>16</td>
<td>$71</td>
<td>20</td>
<td>$250</td>
<td>24</td>
<td>$357</td>
<td>27</td>
<td>$464</td>
</tr>
<tr>
<td>17</td>
<td>$107</td>
<td>21</td>
<td>$250</td>
<td>25</td>
<td>$393</td>
<td>28+</td>
<td>$500</td>
</tr>
<tr>
<td>18</td>
<td>$143</td>
<td>22</td>
<td>$286</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**Average Public Instate Tuition & Fees During Year Prior to Implementation:**
$2,533

**Award Value Relative to Average Tuition & Fees:**
20%-100%

**Duration of Award:**
4 years
Initial Eligibility:
The amount of a KEES award is determined by a student’s high school GPA and highest ACT score.

Income Restrictions:
None

Continued Eligibility:
Full Award
1\textsuperscript{st} year 2.5 GPA
2\textsuperscript{nd} year & on 3.0 GPA

Reduced (50%) Award
2\textsuperscript{nd} year & on 2.5-2.99 GPA

Number of Awards:

<table>
<thead>
<tr>
<th>Year</th>
<th>#Awards used at Public Institutions\textsuperscript{82}</th>
<th>% of Public, 4-Yr Undergraduate FTE Students Receiving Awards</th>
</tr>
</thead>
<tbody>
<tr>
<td>2002-03</td>
<td>33,378</td>
<td>43%</td>
</tr>
</tbody>
</table>

Expenditure on Awards:

<table>
<thead>
<tr>
<th>Year</th>
<th>Dollar (current year)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1999-00</td>
<td>$8M</td>
</tr>
<tr>
<td>2000-01</td>
<td>$22M</td>
</tr>
<tr>
<td>2001-02</td>
<td>$38M</td>
</tr>
<tr>
<td>2002-03</td>
<td>$58M</td>
</tr>
</tbody>
</table>

Funding Source:
Lottery

Notes:
For the academic year 1999-2000, eligible college freshmen received KEES awards based only on the GPA from their senior year in high school. The next year, eligible first-year college students received larger base KEES awards, calculated from two years of high school grades. Only in the academic year 2002-03 did first-year college students begin to receive KEES awards based on four years of high school grades.

\textsuperscript{82} This figure includes Academic Scholars Awards and Medallion Scholars Awards.
Source:


**Program Name:**
Millennium Scholarship Program

**Year of Implementation:**
2000

**Award at Public Institutions during study period:**
$2,500

**Average Public Instate Tuition & Fees:**
$2,233

**Award Value Relative to Average Tuition & Fees:**
100%

**Duration of Award:**
4 years

**Initial Eligibility:**
Cumulative 3.0 high school GPA

**Income Restrictions:**
None

**Continued Eligibility:**
2000  2.0 GPA
2002  2.6 GPA

**Number of Awards:**

<table>
<thead>
<tr>
<th>Year</th>
<th>#Awards used at Public Institutions</th>
<th>% of Public, 4-Yr Undergraduate FTE Students Receiving Awards</th>
</tr>
</thead>
<tbody>
<tr>
<td>2000-01</td>
<td>2,559</td>
<td>12%</td>
</tr>
<tr>
<td>2001-02</td>
<td>4,732</td>
<td>20%</td>
</tr>
<tr>
<td>2002-03</td>
<td>6,594</td>
<td>26%</td>
</tr>
<tr>
<td>2003-04</td>
<td>8,304</td>
<td>32%</td>
</tr>
</tbody>
</table>

---

83 The Nevada Office of the State Treasurer reports only the total number of awards without distinguishing if the award is used at a 4-year or 2-year institution. However, the Office does report the number of 2-year and 4-year degrees earned with use of the awards. Here, the number of awards used at public, 4-year institutions is calculated assuming 60% of total awards are applied at 4-year institutions, and 20% of recipients lose eligibility each year.
Funding Source:
Tobacco Settlement

Sources:


Program Name: Legislative Incentives for Future Excellent (LIFE) Scholarship

Year of Implementation: Academic year 1998-99

Award at Public Institutions during study period:
- 1998-99: $2,000
- 1999-00: $2,000
- 2000-01: $3,000
- 2001-02: $3,000

Average Public Instate Tuition & Fees in Year Prior to Implementation: $3,336

Award Value Relative to Average Tuition & Fees: 60%

Duration of Award: 4 years

Initial Eligibility: Two of the three following criteria must be met:
- 3.0 cumulative high school GPA
- 1100 SAT
- Rank in top 30% of graduating class

Income Restrictions: None

Continued Eligibility: 3.0 GPA

Number of Awards:

<table>
<thead>
<tr>
<th>Year</th>
<th>#Awards used at Public Institutions</th>
<th>% of Public, 4-Yr Undergraduate FTE Students Receiving Awards</th>
</tr>
</thead>
<tbody>
<tr>
<td>1998-99</td>
<td>10,246</td>
<td>17%</td>
</tr>
<tr>
<td>1999-00</td>
<td>11,478</td>
<td>23%</td>
</tr>
<tr>
<td>2000-01</td>
<td>11,383</td>
<td>23%</td>
</tr>
<tr>
<td>2001-02</td>
<td>13,103</td>
<td>26%</td>
</tr>
</tbody>
</table>

84 This figure includes Academic Scholars Awards and Medallion Scholars Awards.
SOUTH CAROLINA (CONT’D)

**Expenditure on Awards:**

<table>
<thead>
<tr>
<th>Year</th>
<th>Dollar (current year)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1998-99</td>
<td>$29.8M</td>
</tr>
<tr>
<td>1999-00</td>
<td>$26.5M</td>
</tr>
<tr>
<td>2000-01</td>
<td>$26.5M</td>
</tr>
<tr>
<td>2001-02</td>
<td>$47.0M</td>
</tr>
</tbody>
</table>

**Funding Source:**
Initially State Appropriations, later the SC Education Lottery

**Notes:**
The South Carolina Palmetto Scholars program has existed since 1988. However, the program was relatively small, awarding only approximately 42 scholarships per year between 1988-95 and 697 awards per year between 1996-2001.

The South Carolina HOPE scholarship was later introduced in 2001. This lower level award was designed for students with a minimum 3.0 cumulative high school GPA that do not meet the qualifications for the Palmetto or LIFE scholarships.

**Source:**

### Appendix B

**Public, 4-year Institutions in Each State Sample**

#### ALABAMA
- Athens State University
- Auburn University at Montgomery
- University of Alabama at Birmingham
- University of Alabama in Huntsville
- University of Montevallo
- University of North Alabama
- University of South Alabama
- University of West Alabama

#### ARIZONA
- Arizona State University
- Northern Arizona University
- University of Arizona

#### CALIFORNIA
- California Polytechnic State University
- California State Polytechnic University
- California State University-Bakersfield
- California State University-Chico
- California State University-Dominguez Hills
- California State University-East Bay
- California State University-Fresno
- California State University-Fullerton
- California State University-Long Beach
- California State University-Los Angeles
- California State University-Northridge
- California State University-Sacramento
- California State University-San Bernard
- California State University-San Marcos
- California State University-Stanislaus
- Humboldt State University
- San Diego State University
- San Francisco State University
- San Jose State University
- Sonoma State University
- University of California-Berkeley
- University of California-Davis
- University of California-Irvine
- University of California-Los Angeles
CALIFORNIA (CONT’D)
University of California-Riverside
University of California-San Diego
University of California-Santa Barbara
University of California-Santa Cruz

FLORIDA
Florida Agricultural and Mechanical University
Florida Atlantic University
Florida International University
Florida State University
The University of West Florida
University of Central Florida
University of Florida
University of North Florida
University of South Florida-Main Campus

GEORGIA
Albany State University
Armstrong Atlantic State University
Augusta State University
Clayton State University
Columbus State University
Fort Valley State University
Georgia College & State University
Georgia Institute of Technology-Main Campus
Georgia Southern University
Georgia Southwestern State University
Georgia State University
Kennesaw State University
North Georgia College & State University
Savannah State University
Southern Polytechnic State University
University of Georgia
University of West Georgia
Valdosta State University

IDAHO
Boise State University
Idaho State University
Lewis-Clark State College
University of Idaho
ILLINOIS

Chicago State University
Eastern Illinois University
Governors State University
Illinois State University
Northeastern Illinois University
Northern Illinois University
Southern Illinois University Carbondale
Southern Illinois University Edwardsville
University of Illinois at Chicago
University of Illinois at Springfield
University of Illinois at Urbana-Champaign
Western Illinois University

INDIANA

Ball State University
Indiana State University
Indiana University-Bloomington
Indiana University-East
Indiana University-Kokomo
Indiana University-Northwest
Indiana University-Purdue University-Fort Wayne
Indiana University-Purdue University-Indianapolis
Indiana University-South Bend
Indiana University-Southeast
Purdue University-Calumet Campus
Purdue University-Main Campus
Purdue University-North Central Campus
University of Southern Indiana

KENTUCKY

Eastern Kentucky University
Kentucky State University
Morehead State University
Murray State University
Northern Kentucky University
University of Kentucky
University of Louisville
Western Kentucky University
NEVADA
University of Nevada-Las Vegas
University of Nevada-Reno

NORTH CAROLINA
Appalachian State University
East Carolina University
Elizabeth City State University
Fayetteville State University
North Carolina A & T State University
North Carolina Central University
North Carolina State University at Raleigh
University of North Carolina at Asheville
University of North Carolina at Chapel
University of North Carolina at Charlotte
University of North Carolina at Greensboro
University of North Carolina at Pembroke
University of North Carolina-Wilmington
Western Carolina University
Winston-Salem State University

OHIO
Central State University
Cleveland State University
Kent State University Kent Campus
Miami University-Oxford
Ohio State University-Lima Campus
Ohio State University-Main Campus
Ohio State University-Mansfield Campus
Ohio State University-Marion Campus
Ohio State University-Newark Campus
Ohio University-Chillicothe Campus
Ohio University-Eastern Campus
Ohio University-Lancaster Campus
Ohio University-Main Campus
Ohio University-Zanesville Campus
Shawnee State University
University of Akron Main Campus
University of Cincinnati-Main Campus
University of Toledo
Wright State University-Main Campus
Youngstown State University
OREGON
Eastern Oregon University
Oregon Institute of Technology
Oregon State University
Portland State University
Southern Oregon University
University of Oregon
Western Oregon University

TENNESSEE
Austin Peay State University
East Tennessee State University
Middle Tennessee State University
Tennessee Technological University
The University of Tennessee
The University of Tennessee-Martin
University of Memphis

SOUTH CAROLINA
Citadel Military College of South Carolina
Clemson University
Coastal Carolina University
College of Charleston
Francis Marion University
Land University
South Carolina State University
University of South Carolina-Aiken
University of South Carolina-Columbia
University of South Carolina-Upstate
Winthrop University

UTAH
Southern Utah University
University of Utah
Utah State University
Utah Valley University
Weber State University

VIRGINIA
Christopher Newport University
College of William and Mary
George Mason University
James Madison University
Longwood University
Norfolk State University
Old Dominion University
Radford University
The University of Virginia's College at Wise
University of Mary Washington
University of Virginia-Main Campus
Virginia Commonwealth University
Virginia Military Institute
Virginia Polytechnic Institute and State University
Virginia State University
REFERENCES


