Essays in Economics of Education

by

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Seth Sanders

Dissertation submitted in partial fulfillment of the requirements for the degree of
Doctor of Philosophy in the Department of Economics
in the Graduate School of Duke University
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Abstract

This dissertation consists of three separate essays on the economics of education. The first chapter, co-authored with Esteban Aucejo, studies the relative effectiveness of reducing absences to extending the school calendar on test score performance. Using administrative data for North Carolina public schools, we exploit a state policy that provides variation in the number of days prior to standardized testing and find substantially larger effects for absences relative to additional days of class.

The second chapter, co-authored with Esteban Aucejo, analyzes whether different institutional settings could affect how school administrators and teachers respond to possible extensions of the school calendar. We present a theoretical model in which principals set the date of the test and teachers decide how much effort to exert in the classroom with and without monetary performance bonuses for teachers. Leveraging the removal of monetary bonuses during the sample period, we utilize a difference-in-difference estimation strategy and find that, consistent with the theoretical model, low performing schools are more likely to make extensive use of the testing window when monetary bonuses are in place; this behavior disappears after changes to the scheme of incentives.

In the third chapter, I present joint work with Peter Arcidiacono, V. Joseph Hotz and Arnaud Maurel, utilizing data on subjective expectations of outcomes from counterfactual choices to recover *ex ante* treatment effects as well as the non-pecuniary benefits associated with different treatments. The particular treatments we consider are the choice of occupation. By asking individuals about potential earnings associated with counterfactual choices of college majors and occupations, we can recover the full distribution of *ex ante* monetary returns to particular occupations, and how they vary across majors. We then link subjective expectations to a model of occupational choice, enabling the examination of how individuals tradeoff their preferences for particular occupations with the corresponding monetary rewards. While sorting across occupations is partly driven by the *ex ante* monetary returns, sizable differences in expected earnings across occupations remain after controlling for selection on monetary returns, which points to the existence of substantial compensating differentials.

To my husband Peter, for his constant love, support, and understanding.

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Acknowledgements

I am extremely grateful to the chair of my committee, Professor Peter Arcidiacono, as well as my committee members, Professors V. Joseph Hotz, Hugh Macartney, Marjorie McElroy and Seth Sanders, for their support and guidance. I thank the participants of Duke University's applied microeconomics lunch groups for their helpful comments and suggestions. I am especially grateful to my friends and colleagues for their help and encouragement: Emily Anderson, Amy Hopson, Deborah Rho, Kalina Staub and Yair Taylor.

My coauthor, Esteban Aucejo and I would also like to thank Marta De-Philippis, Marc Gurgand, Alan Manning, Guy Michaels, Steve Pischke, Douglas Staiger, and participants at both the CEP labor seminar and the labor seminar at the Tinbergen Institute for feedback on earlier drafts of this work. Additionally, my coauthors and I would like to thank seminar participants at the London School of Economics, Stanford University, Washington University in St. Louis, as well as participants at the NBER Labor Studies 2014 Spring Meeting for helpful comments on the joint work with Peter Arcidiacono, V. Joseph Hotz, and Arnaud Maurel.

Thanks are also due to the Duke Economics Department, the Duke Graduate School and DuPRI for their financial support during my time at Duke. I am also grateful to the Center for Child and Family Policy for access to the NCDPI dataset.

Finally, thank you to all those who have supported me on this journey. To my husband, Peter Romano, for his patience, encouragement, and sacrifice. To my parents, Bill and Anna Foy, who have always supported and believed in me. To my sisters, Christina and Julia Foy, for always helping me to look on the bright side. To my in-laws, Jerry and Peggy Romano, for all of your encouragement. To my uncle, Professor Charles Foy, for your valuable advice and insight. To Jane Little, for helping put things in perspective when the going got tough.

1

Introduction

This manuscript is composed of three separate essay that focus on the understanding of the human capital accumulation process. The first essay studies the importance of class time on student achievement by examining absences and number of schools days. The second explores the constrained choice that schools face regarding the amount of school days prior to the test and how accountability programs influence that decision. The third essay explores how college students tradeoff their preferences for particular occupations with the corresponding expected monetary rewards.

The first essay, "Assessing the Effect of School Days and Absences on Test Score Performance," co-authored with Esteban Aucejo, constitutes the first attempt to jointly estimate the relative effectiveness of reducing absences to extending the school calendar on test score performance. While instructional time is viewed as crucial to learning, little is known about the effectiveness of reducing absences relative to increasing the number of school days. Using administrative data for North Carolina public schools, we exploit a state policy that provides variation in the number of days prior to standardized testing and find substantial differences between these effects. Extending the school calendar by ten days increases math and reading test scores by only 0.8% and 0.2% of a standard deviation, respectively; a similar reduction in absences would lead to gains of 5.8% and 3% in math and reading. We present several robustness checks. First, we examine the linearity of the effect of absences and incorporate a contemporaneous measure of student disengagement to address the fact that students may lose interest in classroom activities during their schooling career. The dynamic component of this type of behavior cannot be captured with student fixed effect. In addition, we present a family-year fixed effects specification to address concerns of family-year specific shocks driving our results. We also follow an instrumental variables approach where we instrument the number of excused absences with (proxy) data of flu outbreaks at the city level. We find that our results are qualitatively robust to these alternative empirical strategies. Our findings indicate substantial heterogeneity across student ability, suggesting that targeting absenteeism among low performing students would aid in narrowing current gaps in performance.

While the first essay explores the effect of classroom time, the second essay, "Gaming The System? Incentives and Setting High Stakes Testing Dates," co-authored with Esteban Aucejo, notes that the amount of class time is in part dependent on the school's choice of the testing date. In this essay we examine how schools make this decision and analyze whether different institutional settings affect how school administrators and teachers respond to possible extensions of the school calendar. We present a theoretical model in which principals set the date of the test and teachers decide how much effort to exert in the classroom with and without monetary performance bonuses for teachers. Leveraging the removal of monetary bonuses during the sample period, we utilize a difference-in-difference estimation strategy and find that, consistent with the theoretical model, low performing schools are more likely to make extensive use of the testing window when monetary bonuses are in place; this behavior disappears after changes to the scheme of incentives.

The final essay in this manuscript, "Recovering Ex Ante Returns and Preferences for Occupations using Subjective Expectations Data," joint work with Peter Arcidiacono, V. Joseph Hotz and Arnaud Maurel, on uses data on subjective expectations of outcomes from counterfactual choices to recover *ex ante* treatment effects as well as the non-pecuniary benefits associated with different treatments. The particular treatments we consider are the choice of occupation. By asking individuals about potential earnings associated with counterfactual choices of college majors and occupations, we can recover the full distribution of *ex ante* monetary returns to particular occupations, and how they vary across majors. We then link subjective expectations to a model of occupational choice, enabling the examination of how individuals tradeoff their preferences for particular occupations with the corresponding monetary rewards. While sorting across occupations is partly driven by the *ex ante* monetary returns, sizable differences in expected earnings across occupations remain after controlling for selection on monetary returns, which points to the existence of substantial compensating differentials.

2

Assessing the Effect of School Days and Absences on Test Score Performance

2.1 Introduction

During the last decade, the U.S. federal government and many states have taken a series of steps to improve educational outcomes in elementary, middle and high school. In this regard, many programs have been implemented[1] whose primary aim is to hold schools accountable for the performance of their children. More recently, policy makers have (once again)[2] focused on the actual number of days that students spend at school. For example, while the federal government is aiming for an extension of the school calendar,[3] many states and cities have already increased the number

[1] For example, while the program No Child Left Behind has been implemented by the federal government since 2001, North Carolina introduced Accountability for Basic skills and for local Control (ABCs) in 1997.

[2] In 1983, the report "A Nation at Risk" issued by the National Commission on Education Excellence, compared the U.S. school year of 180 days to the longer school calendars in Europe (190 to 210 days) as justification for an increase in school time.

[3] In 2009, President Obama said that the "challenges of a new century demand more time in the classroom" (The New York Times, August 22, 2011). In a similar vein the U.S. Secretary of Education, Arne Duncan has claimed that "the school day is too short, the school week is too short and the school year is too short" (Time Magazine, April 15, 2009).

of school days.[4] Despite these initiatives, little is known about the effectiveness of these type of interventions relative to other competing policies. For instance, reducing absenteeism may constitute a more effective and less expensive intervention as it would target specific students who would benefit the most from being in the classroom. Recent examples of this type of initiative are "NYC Success Mentor Corps,"[5] and "WakeUp! NYC"[6] which were launched in New York City with the goal to reduce chronic absenteeism.[7,8]

The goal of this chapter is to quantify the relative effectiveness of reducing absences to extending the school calendar on test score performance. While most studies have analyzed the importance of absences or days of class separately,[9] this analysis constitutes the first attempt to provide an approach that allows for both effects to be examined simultaneously. We believe that, from a policy perspective this is key, given that extending the school year or reducing absences are likely to affect students at different margins. For example, missing a day of school (due to absence) may be more detrimental to a student's performance since they will need to (later) make up missed work. Moreover, catching up is likely to be more difficult for low performing students, resulting in larger gaps in academic performance within the classroom. To this end, we examine possible heterogeneous effects of absences and

[4] North Carolina recently added 5 days to the public school calendar.

[5] The NYC Success Mentor Corps is a research-based, data-driven mentoring model that seeks to improve attendance, behavior and educational outcomes for at-risk students in low-income communities citywide.

[6] Students receive phone calls with pre-recorded wake up messages from Magic Johnson, Jose Reyes, Mark Texeira, among others.

[7] Chronic absenteeism is typically defined as missing more than 10 percent of school days in an academic year.

[8] According to Balfanz and Byrnes (2013), these programs do constitute cost-efficient strategies. In this regard, they found that students in poverty at schools that were targeted by these initiatives were 15% less likely to be chronically absent than similar students at comparison schools. Moreover, they show that those who exited chronic absenteeism experienced significant improvements in their academic performance, leading to important reductions in dropout rates.

[9] See Section 2.2 for a discussion of the related literature.

days of class. Specifically, we analyze whether children from (relatively) low income families, or those who perform poorly, benefit comparatively more from spending more time at school. Similarly, we try to identify whether the loss of a school day has differential effects depending on the school grade. For example, a fifth grade class is likely to cover more material than a third grade one; the consequence of which is students in higher grades find it more difficult to catch up. We believe that providing a detailed analysis of heterogeneous effects will inform the policy discussion in terms of identifying specific groups of the population that may benefit the most from particular interventions. Finally, we also investigate the effect of teacher and school quality on absences. We study to what extent attending (having) a better school (teacher) leads to a decrease in the number of days absent at school.

Contrary to most of the literature that has considered countries, states, counties, or schools as the unit of analysis,[10] we make use of detailed longitudinal data at the individual level from North Carolina public schools. This allows us to control for students', teachers', and schools' observable and unobservable characteristics. Therefore, this manuscript is able to analyze the importance of time spent at school from several perspectives (i.e. absences and days of class), as well as implement a rigorous econometric strategy that will deal with problems of endogeneity in several ways. In order to deal with the variety of threats to identification, we employ a number of different identification strategies. First, we use previous year test score/student, teacher and school fixed effects to control for unobserved heterogeneity that is constant over time. Second, we control for contemporaneous measure of student disengagement. Third, we utilize flu data to instrument for excused absences. Fourth, we employ family-year fixed effects to account for any time-varying family specific shocks. Finally, we examine unexcused absences to take into account any illnesses or other

[10] For example, Lee and Barro (2001), Pischke (2007), and Marcotte and Hemelt (2008), among others.

excused events that may effect both absences and grades. Reassuringly, our results are similar in all cases. Estimating models with triple fixed effects when the sample size is large is not a trivial matter. In our case, it requires us to estimate more than 413,351 parameters[11] (i.e. 382,835 students; 29,202 teachers; and 1,305 schools), therefore an iterative algorithm is implemented in order to overcome computational issues.

Results show substantial differences between the effect of absences and days of class on test score performance. Our preferred specification indicates that extending school calendar by ten days would increase math and reading test scores by around 0.8% and 0.2% of a standard deviation, respectively; while a similar reduction in absences would lead to increases of 5.8% and 3% in math and reading. Moreover, estimation results show that absences have even larger negative effects among low performing kids, suggesting that catching up is costly especially among those who show greater difficulties at school. In addition, we analyze whether spending more time at school (i.e. less absences or longer academic calendar) have larger effects on later grades. While being 10 days absent in grade 3 leads to a decrease of 2.5% of a standard deviation in math test scores, in grade 5 the effect is 8.9%. This finding is consistent with the concept that the amount of material covered per day in later years is larger; therefore catching up could become more problematic. Finally, we show that attending (having) a school (teacher) in the 75th percentile decreases absences by 0.21 (0.14) days relative to the 25th percentile; a relatively large result given that the average number of days absent is 6.[12]

Overall, the results point towards the presence of an important asymmetry between the effects of expanding total time spent at school through a reduction of

[11] Given that part of our empirical strategy makes use of all fixed effects in a later analysis, we need to recover all the fixed effect parameters (i.e. demeaning the sample is not a feasible alternative in this case).

[12] These calculations are based on the fixed effects from the math test score regression.

absences or through an extension of the school calendar. Therefore, a successful strategy that decreases absences may have substantially larger effects than that of extending the school calendar. Moreover, the fact that this type of intervention may benefit low achieving students the most, suggests that it may also help to narrow current gaps in academic performance.

Finally, it is important to point out that the financial resources needed to extend the school calendar are undeniably high. Most calculations suggest that a 10 percent increase in time would require a 6 to 7 percent increase in cost [Chalkboard Project (2008), Silva (2007)]. Therefore, the fact that a competing policy, like targeting absenteeism, could lead to large improvements in academic performance at a lower cost suggest an alternative avenue for policy.[13]

The remainder of this chapter is organized as follows. Section 2.2 places our work in context with the related literatures on student absences and school length. Section 2.3 details the data used in the empirical analysis. Section 2.4 outlines the econometric strategy and Section 2.5 describes the results. Section 2.6 presents a series of robustness checks. Section 2.7 will examine the heterogeneous effects of absences and days of class by several student characteristics. Section 2.8 concludes.

2.2 Background

The length of the school year and absences combine to determine the total amount of instructional time for a student in a given year. Despite this, their effects on student performance have largely been examined independently; likely due to the

[13] For example, the Education Act of 1996 in the United Kingdom empowers head teachers to issue Penalty Notices in cases of unauthorized absence from school. This means that when a pupil has unauthorized absence of 5 days or more, in any term (where no acceptable reason has been given for the absence) or if their child persistently arrives late for school after the close of registration, their parents or carers may receive a Penalty Notice of £60 if paid within 21 days rising to £120 if paid within 28 days. In this regard, a report on the effectiveness of fines [Crowther and Kendall (2010)], found that 79% of local authorities said penalty notices were "very successful" or "fairly successful" in improving school attendance.

lack of available data on absences and the limited variation on school year length.

2.2.1 Absences

A common finding in the literature is that students with greater attendance than their classmates perform better on standardized achievement tests, and that schools with higher rates of daily attendance tend to generate students who perform better on achievement tests than do schools with lower daily attendance rates [Roby (2004); Sheldon (2007); Caldas (1993)]. These correlations present a challenge in estimating the effects of absences on student performance; more able and motivated students are both more likely to attend school and to score highly in their courses and on standardized tests. Therefore, without adequate controls for personal characteristics, part of any estimated effects of absences will reflect a downward ability bias due to endogenous selection.

The literature has addressed this in a variety of ways. Devadoss and Foltz (1996) used survey responses to obtain information on student effort and motivation. Dobkins et al. (2010) exploited data generated from a mandatory attendance policy for low-scoring college students. Stanca (2006) and Martins and Walker (2006) also examined college student attendance utilizing panel data to try to control for unobserved characteristics correlated with absence, finding that attendance does matter for academic achievement. Both panel studies utilized student fixed effects to control for unobservable heterogeneity.

Fewer studies have exploited panel data to examine the effects of absences at the elementary school level. Gottfried(2009; 2011) examines the effects for second through fourth graders in the School District of Philadelphia. Gershenson et al. (2014) look at elementary students in North Carolina and find that a one standard deviation increase in absences is associated with decreases in achievement of 0.04 and 0.02 math and reading test-score standard deviation, respectively. Relative to these papers, we additionally control for teacher and school fixed effects. School fixed effects enables us to control for the common influences of a school by capturing systematic differences across institutions. This includes curriculum, hiring practices, school neighborhood, and the quality of leadership. Teacher fixed effects control for the common influences of a given teacher. We will also be able to identify siblings and control for family-year FE combined with previous year test-score.[14] This controls for family-specific shocks such as a death in the family or divorce when estimating the effects of absences. Additionally, we utilize North Carolina flu data to instrument for excused absences and control for a contemporaneous measure of student disengagement.

2.2.2 Length of the School Year

A number of previous studies have examined the effects of length of the school year on student achievement. Various studies on school quality in the United States include term length as one of the regressors (for example, Grogger (1996) and Eide and Showalter (1998)) but typically found insignificant effects. The biggest stumbling block to uncovering the impact of school days on student performance is the lack of variation in the total number of school days in an academic year. We overcome this problem thanks to a specific North Carolina policy that provides variation in the number of instructional days across schools.[15]

Most studies examining the length of the school year use state or country level data, and in some cases, less recent data [for example, Card and Krueger (1992);Betts and Johnson (1998); Lee and Barro (2001)]. Card and Krueger (1992) and Betts and Johnson (1998) found positive and significant effects of length on earnings for birth

[14] Gottfried (2011) also employs a family-year fixed effect approach in analyzing student performance in the School District of Philadelphia.

[15] More specifically, North Carolina allows for flexibility in the setting of the testing date, which is when academic achievement is measured. See Section 2.3 for more information.

cohorts in the first half of the 20th century, which had more variability in the number of school days.[16] Lee and Barro (2001) utilized cross-county data and examined the correlation of student performance and measures of school resources, including the number of class days. They found that longer time in school increased mathematics and science scores, but lowered scores in reading, which is largely consistent with our findings. Differently from other papers, we are able to use within school variation in days of class. Moreover, we use microlevel data at the student level which allows us to explore policy relevant heterogeneous effects of increasing school days.

Other studies have exploited quasi-experimental variation to identify the effect of additional days of class. Marcotte and Hemelt (2008) examined the effect of fewer days of class resulting from snow related school closures on test score performance and found that the pass rate for 3rd grade math and reading assessments falls by more than a half percent for each school day lost to an unscheduled closure. Pischke (2007) utilized variation introduced by the West-German short school years in 1966-67, which exposed some students to a total of about two-thirds of a year less of schooling while enrolled. He found that the short school years increased grade repetition in primary school and led to fewer students attending higher secondary school tracks.[17] Relative to Pischke, we examine a smaller change in the number of days of class; however, it is of the approximate size considered by policy makers.[18] Carlsson et al. (2012) exploits conditionally random variation in the assigned test date for a battery of cognitive tests required for 18 year-old males in Sweden to take in preparation for military service. They find that an additional 10 days of school instruction raises cognitive scores on synonym and technical comprehension tests by approximately

[16] Card and Krueger presented additional results including a state fixed effect; the positive effect of term length vanished within states and conditional on other school quality variables.

[17] Pischke (2007) found no effect on earnings.

[18] In North Carolina, the school year was recently extended by 5 days. In contrast, Pischke's findings are due to a change in about 100 days of schooling.

one percent of a standard deviation, slightly larger than our finding on math scores.

Our paper is most similar to Sims (2008), who studied the effect of days of class, using the implementation of a Wisconsin state law that restricted districts to start dates after September 1st to identify the effects of this extra time on student achievement. He found that an additional week of class was associated with a increase of 0.03 standard deviations in math scores for fourth graders, but he found no effect on average reading and language scores. We find smaller effects of additional days on scores, likely due to our different econometric strategy and individual level data.

2.3 Data and Descriptive Statistics

2.3.1 Data

The North Carolina education data is a rich, longitudinal, administrative data set that links information on students, teachers, and public schools over time. This data is maintained by the North Carolina Education Research Data Center (NCERDC), which is housed at Duke University. This longitudinal database contains mathematics and reading test scores for each student in elementary,[19] middle, and high school. Since the availability of some of the data varies over time, the analysis is restricted to the years 2006 to 2010[20] and grades 3 to 5.[21] Encrypted identifiers make it possible to track the progress of individual student over their educational careers and link students to their teachers[22] and school in each year, provided they stay within the

[19] More specifically, for grades 3 and above. Students in lower grades do not take end of grade (EOG) tests, but a test is administered in September as well as the end of grade 3. All other grades were tested in either May or June of that year.

[20] School years are referred to by the year the school year ended. For example, the 2005/06 school year is year 2006.

[21] Younger students are less likely to skip school without parental knowledge, limiting issues of endogeneity. In addition, students in upper grades can take courses with multiple teachers, making the estimation of teacher fixed effects problematic.

[22] The data does not identify student's teachers directly, but rather identify the individual who administered the end of grade exams. In elementary school, classrooms are largely self-contained with the classroom teacher proctoring the exam.

universe of North Carolina public schools.

NCERDC records also include extensive information on student, teacher, and school characteristics. Data on students include ethnicity, gender, whether or not they participated in the federal free and reduced price lunch subsidy program, geocoded address, days in membership and absences. Days in membership is used to calculate the number of days of class prior to the exam.[23] It is defined as the number of days the student was on the roster in a particular school; a student is in membership even when absent. Absences data includes both the total number of days, as well as disaggregated data by excused and unexcused absences. All absences and days in membership data are collected at the time of end of grade (EOG) testing.

Only counts of absence are provided for each student and each academic year; it is not possible to specifically discern when a student was absent. The NCERDC data categorizes absences as either excused or unexcused; excused absence are defined as the ones due to illness or injury; quarantine; medical appointment; death in the immediate family; called to court under subpoena or court order; religious observance; educational opportunity (prior approval is needed); local school board policy; absence related to deployment activities. All other absences are categorized as unexcused.[24] Aside from the distinction between excused and unexcused absences, no other details are provided as to the reasons for the absences.

In addition to the main sample, a subsample of students who are siblings is also employed. Following Caetano and Macartney (2013), the geocoded address data is used to identify students living in the same household to create a family identifier. Students residing at the same address were identified through the geocoded data. Two or more children who share the same home address in a given academic year are

[23] In practice, days of class is the modal days in membership at the school level.

[24] More information on North Carolina's attendance policies can be found at: http://www.ncpublicschools.org/docs/fbs/accounting/manuals/sasa.pdf.

considered to be part of the same household. Even if the address changed between years, as long as the students remain together at the new address, they are considered to be members of the same household. As a result, the ability to observe children's addresses as they progressed through elementary school makes it possible to identify family fixed effects.

Teachers that are matched with less than 5 students are not included in an effort to avoid special education (or other specialty) classes as well as minimize measurement error when estimating fixed effects. Moreover, teachers with more than 30 students in a school year were excluded due to possible data miscoding. The total number of student-year observations for 2006-2010 is more than 1,008,000 while the total number of teachers included is more than 29,000.

2.3.2 Descriptive Statistics

Table 2.1 presents descriptive information on the sample of students in grades 3 to 5. Students are absent on average 6.14 days of school prior to the exam. Figure 2.1 depicts the distribution of absences in the data. While the distribution is centered around 5 days of class, a sizable proportion of students are absent for much longer; 25% percent of students miss nine days (just under two weeks) of class and 10% miss 13 days or more prior to the day of the exam in each year. Interpretation of results typically focuses on the effect of the average number of absences on performance. However, it is important to recognize that for a sizable share of the sample, reducing absences would have a much larger impact.

North Carolina has an ethnically diverse student body with 25.5% black and over 10% Hispanic. Relative to the United States in the 2010 Census, North Carolina has a greater share of black school-age children and a slightly smaller Hispanic population. Males and females are equally represented in the data. Just under half of elementary school students are eligible for the free or reduced price lunch subsidy program, a measure of low-income status. In addition, 14% of students are categorized as special education students and 6.42% are English language learners. Finally the proportion of students that has ever been suspended is 6.72%, where the average number of days suspended is 3.04. Note that North Carolina ranks third nationally in the rate of school suspensions behind South Carolina and Delaware.

Table 2.1: Descriptive Statistics for North Carolina Public School Students

	Mean	s.d.
Days Absent	6.14	5.55
Days of Class	166.32	3.48
Suspensions:		
Ever Suspended (%)	6.72	25.04
Days Suspended*	3.04	4.16
Race (%):		
White	56.75	49.54
Black	25.50	43.59
Hispanic	10.34	30.45
Asian	2.30	14.98
Other	5.11	22.02
Gender (%):		
Male	49.99	50.00
Female	50.01	50.00
Other characteristics (%):		
Free/reduced lunch eligible	46.86	49.90
Special education	14.00	34.70
English language learner	6.42	24.52
N	1,008,575	

Source: NCERDC, 2006-2010. End of grade test scores are standardized by year and grade level. Samples are based on students having two or more observations with required test scores and total absences information, linked to a teacher with at least 5 and no more than 30 students. Final analytical samples also require non-missing information for all included variables. *Conditional on suspension.

As younger students are less likely to skip school without parental knowledge, by limiting the sample of analysis to grades 3 to 5 we are able to minimize issues of endogeneity.[25] In addition, students in these grades are more likely to enjoy self-

[25] In the NCERDC data, middle school students do in fact exhibit slightly more absences, driven largely by a greater number of the unexcused type.

contained classrooms and therefore the link between teachers and students is more reliable as compared to those in higher grades.

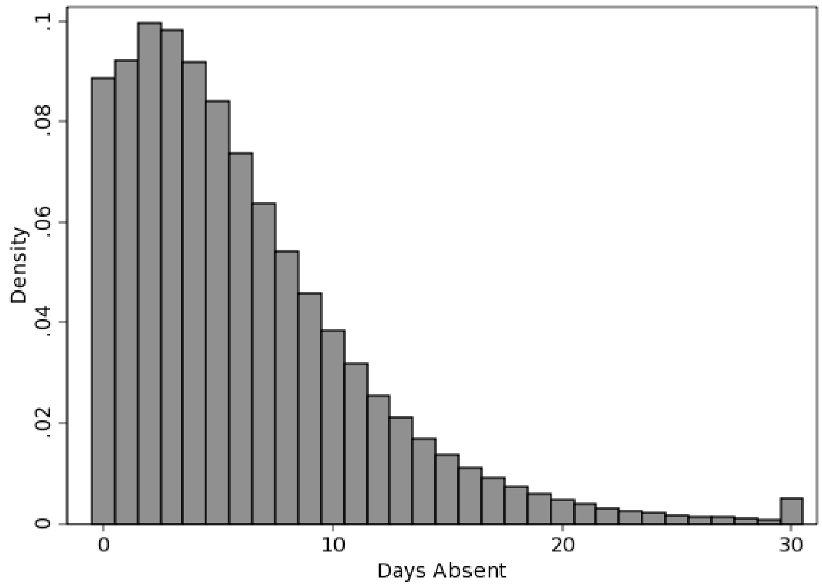

FIGURE 2.1: Distribution of Absences

Researchers have demonstrated that students with greater attendance than their classmates perform better on standardized achievement tests and that schools with higher rates of daily attendance tend to generate students who perform better on achievement tests than do schools with lower daily attendance rates [Roby (2004); Sheldon (2007)]. Table 2.2 examines absences by student characteristics, including quintile of last year's prior math score.[26] Students with lower prior year test scores generally have a greater number of absences. This result is largely driven by unexcused absences which exhibits a stronger negative relationship with test scores.[27] This suggests that students who are less capable are also more likely to miss school.

[26] Scores are comparable across time and grades through the use of a developmental scale. The developmental scale is created from the number of correctly answered questions on the standardized test. Each point of the developmental scale measures the same amount of learning. For example, a student who shows identical growth on this scale in two consecutive grades is interpreted as having learned equal amounts in each year.

[27] This pattern holds when examining absences relative to prior reading score.

Simple ordinary least squares (OLS) will therefore result in biased coefficient estimates; without adequate controls, part of any estimated effects of absences will reflect a downward ability bias due to endogenous selection.

Table 2.2: Average Number of Absences

	Total Absences		Excused Absences		Unexcused Absences	
	Mean	s.d.	Mean	s.d.	Mean	s.d
Grades 3-5						
Average:	6.14	5.55	3.51	4.24	2.31	3.28
Prior Math Score:						
Lowest Quintile	6.80	6.20	3.55	4.44	2.99	3.91
Second Quintile	6.31	5.71	3.54	4.27	2.61	3.49
Third Quintile	6.16	5.50	3.62	4.28	2.32	3.19
Fourth Quintile	5.90	5.21	3.59	4.16	2.06	2.88
Highest Quintile	5.46	4.88	3.38	4.00	1.69	2.51
Sex:						
Male	6.20	5.62	3.51	4.26	2.36	3.33
Female	6.08	5.48	3.51	4.22	2.26	3.23
Race:						
Asian	3.96	4.27	2.10	3.20	1.39	2.44
Black	5.54	5.53	2.53	3.66	2.69	3.72
Hispanic	5.27	4.96	2.55	3.50	2.51	3.36
White	6.60	5.60	4.18	4.50	2.12	3.03
Year:						
2006	6.07	5.50	3.55	4.25	2.26	3.23
2007	6.55	5.76	3.50	4.45	2.13	3.24
2008	6.10	5.59	3.66	4.19	2.43	3.35
2009	5.76	5.30	2.90	3.60	2.62	3.31
2010	6.25	5.54	3.16	3.76	2.55	3.15

Source: NCERDC, 2006-2010. Samples are based on students having two or more observations with required test scores and total absences information, linked to a teacher with at least 5 and no more than 30 students. The sum of excused and unexcused absences do not sum to total absences as absence counts by type are only available for about two-thirds of the student-year observations. Absences by type are generally missing at the school level.

Table 2.2 also highlights racial and gender differences in total number of absences as well as their distribution between excused and unexcused types. White students have a greater number of absences than other racial groups with an average of 6.60 days a year. Blacks and Hispanics are absent 5.54 and 5.27 days respectively. However, a greater share of absences are excused for white students relative to both the other two racial groups. Males have slightly more absences than do females due to a greater number of unexcused absences. There does not appear to be any time trend in absences.

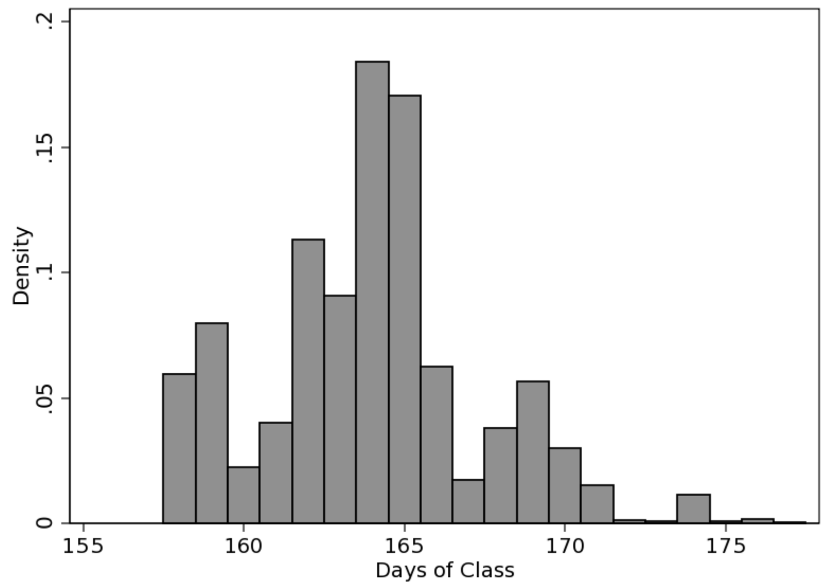

Figure 2.2: Distribution of Days of Class, 2009-2010

Although students may have varying quantities of instructional time prior to end of grade tests resulting from absences, schools also differ in the number of actual class days prior to exam administration. During the sample period, the Department of Education mandated 180 days of class. North Carolina Department of Public Instruction dictates a window of time for exam administration.[28] As a result, students at different schools may have had differing number of instructional days at the time academic performance was measured, as can be seen in Figure 2.2. While schools are not actually extending the school year, they are effectively adjusting their school year length by choosing when to administer the EOG test. This variation in instructional

[28] http://www.ncpublicschools.org/accountability/calendars/archive lists the testing windows for all tests administered in North Carolina since 2001.

days,[29] coupled with data on absences allows for the separate identification of the effect of absences from additional days of schooling. Since schools with more days prior to the exam are those that are more interested in making use of the additional class time as North Carolina schools face pressure from both state and federal accountability policies [Aucejo and Romano (2014)], the estimated effects are likely an upper bound once we control for student and school characteristics.[30]

2.4 Methodology

The data enable us to observe the EOG test score, the number of class days, and the absence of students in each year for grades 3 through 5. Our primary aim is twofold; to estimate the causal effect of both absence and an additional day of instruction on performance. The number of instructional days prior to the exam varies across schools and years and therefore enables the identification of the effect of absences separately from additional instructional time.

In analyzing the effect of absences on performance, there are potential problems of endogeneity bias. As shown in Table 2.2, more able and motivated students appear more likely to both attend school and score highly in their courses and on standardized tests. Therefore, without adequate controls part of any estimated effects of absences will reflect a downward bias due to endogenous selection. This bias could be minimized with good proxies for ability, engagement/motivation, or other individual characteristics. The data contains information on the students' prior year test score which is also included in some specifications of the model.

Our first strategy for dealing with the potential problem of ability bias is to use the panel properties of the data. Student fixed effects are employed for control of all

[29] The number of days of class prior to the EOG exam varies between 158 and 180 days.

[30] All schools should not be expected to set the same testing date as administering the test later in the year is more costly.

observed and unobserved student characteristics that are constant over time. This potentially includes student effort, motivation and ability, as well as familial factors such as parental willingness for their child to miss school or their efforts to help with school work at home.

School fixed effects are also included in the model to control for the common influences of a school by capturing systematic differences across institutions. This includes curriculum, hiring practices, school neighborhood, and the quality of leadership. These effects are identified off of students who switch schools during grades 3 through 5. Teacher fixed effects are included to control for the common influences of a teacher. Finally, fixed effects for grade and year will parse out the effect of schools and teachers from other common influences that occur across the population in a given year and for a given cohort.

The main estimating equation is:

$$y_{igkst} = \beta_0 + \beta_1 a_{it} + \beta_2 d_{ist} + \beta_3 X_{it} + \beta_4 G_{ig} + \beta_5 T_t + \alpha_i + \theta_k + \delta_s + \epsilon_{igkst} \tag{2.1}$$

where y_{igkst} denotes the test score of student i, in grade g, teacher k, school s, and year t where the test score is standardized by grade, year and subject. The main explanatory variables of interest are a_{it} and d_{ist}; a_{it} is the number of absences over the course of the school year up to the day of the exam. d_{ist} is the number of days of instruction prior to the end of year examination. X is a vector of student covariates, G are grade fixed effects, and T are school fixed effects. α_i, θ_k, and δ_s denote student, teacher, and school fixed effects respectively.

A value-added model of student achievement is also implemented. The feature of including a lagged achievement score at the individual level means, that under the assumptions of the model, it is no longer necessary to incorporate additional measures of ability or a full historical panel of information on any particular student.

Estimating Equation 2.1 by ordinary least squares solves

$$\min_{\beta,\alpha,\theta,\delta} \sum_{i=1}^{N}\sum_{k=1}^{K}\sum_{s=1}^{S}(y_{igkst}-\beta_0-\beta_1 a_{it}-\beta_2 d_{ist}-\beta_3 X_{it}-\beta_4 G_{ig}-\beta_5 T_t-\alpha_i-\theta_k-\delta_s)^2 \quad (2.2)$$

Given the large number of students (382,835), teachers (29,202) and schools (1,305) in our data, after using student fixed effects to control for individual heterogeneity, incorporating a dummy variable for each teacher and for each school would be infeasible. We employ an iterative fixed-effects estimator introduced by Arcidiacono et al. (2012a) to reduce the computational cost of estimating the multi-level fixed effects model of student achievement. This method yields OLS estimates of the parameters of interest while circumventing the dimensionality problem. The algorithm begins with an initial guess of the parameters $\alpha_i^{(0)}, \theta_k^{(0)}, \delta_s^{(0)}$. It then iterates on the following steps with the m^{th} iteration:

- **Step 1:** Using the initial guesses of the student, teacher and school fixed effects, calculate $Z_{igkst}^{(m)} = y_{igkst} - \alpha_i^{(m)} - \theta_k^{(m)} - \delta_s^{(m)}$ and solve the least squares problem:

$$\left\{\beta_0^{'(m)}, \beta_1^{'(m)}, \beta_2^{'(m)}, \beta_3^{'(m)}, \beta_5^{'(m)}, \beta_6^{'(m)}\right\} =$$

$$\arg\min_{\beta} \sum_{i=1}^{N}\sum_{k=1}^{K}\sum_{s=1}^{S}(Z_{igkst}^{(m)} - \beta_0 - \beta_1 a_{it} - \beta_2 d_{ist} - \beta_3 X_{it} - \beta_4 G_{ig} - \beta_5 T_t)^2$$

- **Step 2:** Using $\left\{\beta_0^{'(m)}, \beta_1^{'(m)}, \beta_2^{'(m)}, \beta_3^{'(m)}, \beta_4^{'(m)}, \beta_5^{'(m)}, \theta_k^{(m)}, \delta_s^{(m)}\right\}$ calculate $\alpha_i^{(m+1)}$ based on the following expression ($k \in i$ denotes teachers of student i)

$$\alpha_i^{(m+1)} = \frac{\begin{array}{c}\sum\limits_{k \in i}(y_{igkst} - \beta_0^{'(m)} - \beta_1^{'(m)} a_{it} - \beta_2^{'(m)} d_{ist} \\ - \beta_3^{'(m)} X_{it} - \beta_4^{'(m)} G_{ig} - \beta_5^{'(m)} T_t - \theta_k^{(m)} - \delta_s^{(m)})\end{array}}{\sum\limits_{k} I(k \in i)}$$

where the previous expression avoids the minimization over all the $\alpha_i' s$. Notice that this expression is obtained from the first order condition of the least squares problem with respect to α_i

- **Step 3:** Using $\left\{\beta_0^{'(m)}, \beta_1^{'(m)}, \beta_2^{'(m)}, \beta_3^{'(m)}, \beta_4^{'(m)}, \beta_5^{'(m)}, \alpha_i^{(m+1)}, \delta_s^{(m)}\right\}$ calculate $\theta_k^{(m+1)}$ in an analogous way to step 2.

- **Step 4:** Using $\left\{\beta_0^{'(m)}, \beta_1^{'(m)}, \beta_2^{'(m)}, \beta_3^{'(m)}, \beta_4^{'(m)}, \beta_5^{'(m)}, \alpha_i^{(m+1)}, \theta_k^{(m+1)}\right\}$ calculate $\delta_s^{(m+1)}$ in an analogous way to step 2.

- **Step 5:** Repeat steps 1 to 4 until convergence of the parameters.

2.5 Baseline Results

Table 2.3 presents the regression results for math and reading, based on Equation 2.1. Specification (1) is a simple OLS regression of standardized test scores without any fixed effects or controls for student ability.[31] The coefficients on absences for both math and reading are negative, significant and large in magnitude. However, since there are no controls for unobserved individual characteristics which is likely to be negatively correlated with absences, the coefficient is biased downward; we expect that once adequate controls are included, the coefficient on absences will increase. Similarly, the coefficient on days of class is the opposite sign from what was hypothesized and likely also suffers from omitted variable bias.

Specification (2) includes student fixed effects, thereby controlling for observed and unobserved student characteristics that are constant over time. An additional absence results in math (reading) scores declining by 0.66% (0.35%) of a standard deviation. Therefore, the average student's math (reading)[32] score declines by 4.05%

[31] In addition to the regressors specified in Equation 2.1, controls for gender and ethnicity are also included.

[32] The average student in grades 3-5 is absent 6.14 days of school.

Table 2.3: Baseline Regression

	Math Test Score					Reading Test Score				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Days Absent	-0.0198***	-0.0066***	-0.0065***	-0.0058***	-0.0078***	-0.0107***	-0.0035***	-0.0034***	-0.0030***	-0.0040***
	(0.0002)	(0.0002)	(0.0001)	(0.0002)	(0.0001)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0001)
Days of Class	-0.0081***	0.0002	0.0000	0.0008**	0.0011**	-0.0059***	0.0003	0.0001	0.0002	0.0016***
	(0.0003)	(0.0003)	(0.0003)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0002)	(0.0004)	(0.0005)
Student FE	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes	No
School FE	No	No	Yes	Yes	Yes	No	No	Yes	Yes	Yes
Teacher FE	No	No	No	Yes	Yes	No	No	No	Yes	Yes
Lagged Student Score	No	No	No	No	Yes	No	No	No	No	Yes
N	1,001,032	1,001,032	1,001,032	1,000,896	872,860	1,001,032	1,001,032	1,001,032	1,000,896	872,860

Source: NCERDC, 2006-2010, grades 3-5. Dependent variable is standardized by grade and year. All specifications include dummy variables for grade, year, and free/reduced-price lunch status. Specifications without student fixed effects also include dummy variables for race. Bootstrapped standard errors are reported in parenthesis. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%.

(2.15%) of a standard deviation. Additional days of class has a positive, but insignificant effect on both math and reading performance. The addition of school fixed effects (specification (3)) has little effect to the magnitude of the coefficient of interest for either subject.

Specification (4), our preferred specification, includes triple fixed effects and finds significant, although slightly smaller coefficients on days absent relative to the previous specifications. Reducing absences by the average number of absences (6.14 days) results in scores declining 3.56% and 1.84% of a standard deviation respectively for math and reading. Additionally, the effect of days of class on math test scores is now significant, although smaller in magnitude relative to absences, with test scores increasing 0.8% of a standard deviation for an extra ten days of school. Specifications (5) examines a model with lagged achievement, and teacher and school fixed effects. The results are slightly larger in magnitude relative to the preferred specification; reducing absences by the average number of absences results in scores declining by 4.78% and 2.46% of a standard deviation for math and reading respectively. An additional ten days of class would increase math scores by 1.1% and reading scores by 1.6% of a standard deviation.

In summary, additional days of class seems to have a positive effect on math scores, although on reading the effects are much smaller. Similarly, Lee and Barro (2001) and Sims (2008) also find small or no effect on reading scores. However, the magnitude of the effect is smaller than that of an absence. That both absences and additional days of class have a greater effect on math achievement than on reading achievement is consistent with the general finding that educational inputs and policy have relatively larger impact on math achievement [Hanushek and Rivkin (2010); Jacob (2005); Rivkin et al. (2005)], perhaps because children are more likely to be exposed to reading and literacy outside of school, particularly at home where parents may be more apt to help their children learn and develop reading skills [Currie and

Thomas (2001)].

2.6 Robustness Checks

Despite the set of controls that have been included in Table 2.3 (i.e. student, teacher and school fixed effects, previous year test score and free-reduce price lunch status), our results on absences may still be driven by confounding effects. For example, dynamic student disengagement or family/health shocks could affect absences and test score performance in a way that may not be captured by our extensive set of controls. In this regard, this subsection provides a series of robustness checks.

2.6.1 Student Disengagement

The fact that students may lose interest in classroom activities during their schooling career, suggests that the dynamic component of this type of behavior cannot be captured by the addition of student fixed effects. This may cause concern that our results on absences are in fact driven by a correlation between "lack of interest in school" and the decision to not attend class. To this end, we present several pieces of evidence that assess the importance of this potential threat to our identification strategy.

First, recall that our sample corresponds to students from grades 3 to 5. Therefore, the decision to be absent from school needs to be (at least tacitly) supported by their parents. This suggests that endogeneity issues should be of a less concern relative to a sample of high school students.

Second, if student disengagement is the main driver of our results, then it is likely to have a nonlinear effect on absences. For example, the effect of being absent 10 days at school during the academic year due to (for instance) disengagement is expected to be proportionally larger than the effect of being absent just 2 days at school. To explore this, we saturated the variable absences with dummies for each day absent from 1 to 30 and another for 31 or more. The coefficients on each of the days absent dummies are plotted in Figure 2.3. The pattern of the coefficients indicates that the effect on test scores is in fact roughly linear through 30 absences. The lack of nonlinear effects suggests that disengagement effects are not likely to be driving our results on absences.

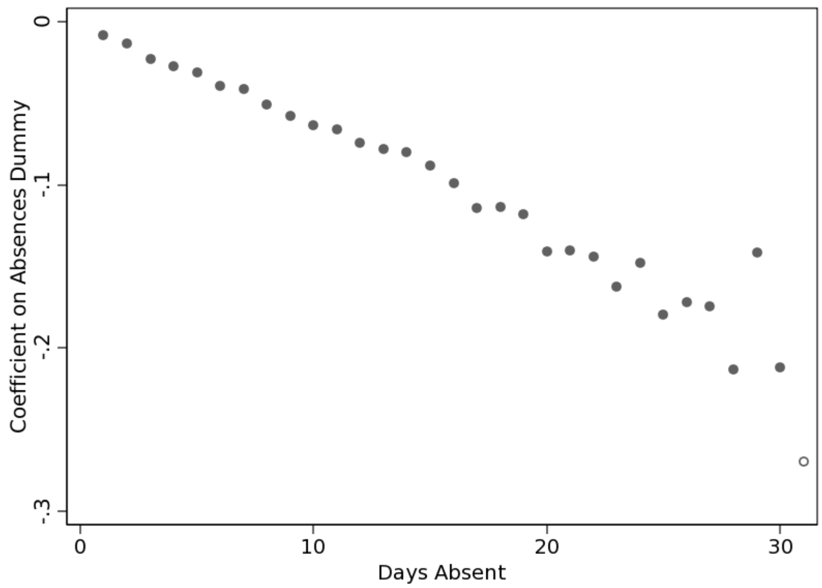

FIGURE 2.3: Coefficients on Days Absent Dummy Variables

Third, we include a proxy for student disengagement to our baseline specifications. More specifically, a measure of misbehavior, total days suspended, is added. The total days suspended is the sum of in-school and out-of-school suspensions.[33] If student disengagement is affecting our results, we should expect a large decline in the effect of absences once controlling for suspensions. Table 2.4 shows that the coefficient on absences are in fact fairly constant across specifications. The first column for both math and reading corresponds to the baseline specification in Table 2.3.[34]

[33] In-school suspensions are usually served in an in-school suspension classroom. When a school does not have an in-school suspension program or when offenses are more serious or chronic, they may be dealt with through short-term, out-of-school suspensions. Long-term suspensions are more than ten days in length may be used for more serious offenses and are served out-of-school.

[34] The sample size is smaller as not all schools report suspensions.

Table 2.4: Student Disengagement Regression

	Math Test Score		Reading Test Score	
Days Absent	-0.0051***	-0.0048***	-0.0032***	-0.0025***
	(0.0003)	(0.0003)	(0.0003)	(0.004)
Suspensions		-0.0058***		-0.0048***
		(0.0014)		(0.0015)
Student FE	Yes	Yes	Yes	Yes
Teacher FE	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes
N	386,776	386,776	386,776	386,776

Source: NCERDC, 2006-20008, grades 3-5. Dependent variable is standardized by grade and year. All specifications include days of class and dummy variables for grade, year, and free/reduced lunch participation. Bootstrapped standard errors are reported in parenthesis. The sample is smaller than our main estimating sample as suspensions are only available for approximately two-thirds of the student-year observations. Suspensions are generally missing at the school level. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%.

For math (reading), the effect on absences only changes from -0.0051 (-0.0032) in our baseline specification to -0.0048 (-0.0025) after controlling for suspensions (see columns 2 and 4).

Finally, we follow an instrumental variable approach, where we instrument number of excused absences with (proxy) data of flu outbreaks at the city level. The data were obtained from Google Flu Trends[35] which uses aggregated Google search data to estimate current flu activity in cities across the United States, including the North Carolina cities of Cary, Charlotte, Durham, Greensboro and Raleigh.[36] The flu is a contagious respiratory illness that affects all ages, however, school-aged children are the group with the highest rates of flu illness.[37] In order for a measure of flu activity to be a appropriate instrument for absences, it must be correlated with absences and only impact test score performance through missing days of schooling. The correlation between the influenza-like illness (ILI) and excused absences for stu-

[35] Data Source: Google Flu Trends (http://www.google.org/flutrends)

[36] The data contains the estimated number of influenza-like illness (ILI) cases per 100,000 population.

[37] Center for Disease Control, http://www.cdc.gov/flu/school/guidance.htm

dents attending school in the five cities is 0.2578.[38] While one may be concerned that the flu has a direct impact on EOG scores, the flu season commonly peaks in January or February, well before the EOG tests are administered. For the years in our sample containing excused and unexcused absences (2006-2008), Google Flu Trends data shows low flu activity from April through the end of the school year.

We define the instrument to be

$$IV_{flu} = \left(\frac{\widehat{ILI}}{100,000} \right) N$$

where $\widehat{ILI}$ is the estimated number of influenza-like illness cases per 100,000 population and N is the student population in grades 3 through 5 in the county.

A possible concern with this instrumental variable is that flu outbreaks may also affect teacher attendance, and therefore may confound the effects of student and teacher absences. In order to address this concern, we control for number of teacher absences due to sickness.[39] Table 2.5 shows the results from the IV regression specification with the sample restricted to schools that are in one of the five cities with flu data with standard errors clustered at the city level.[40] A 10 day reduction in excused absences would lead to increases of 24.5% and 11.6% of a standard deviation in math and reading respectively. This is approximately a four-fold increase over our baseline specification results.

2.6.2 Family Shocks

Our baseline specifications thus far have been assuming that family shocks/inputs that are correlated with absences and affect performance are constant across time

[38] The correlation between city ILI and days absent is, as expected, lower than that for excused absences at 0.0160. This is because, provided a parent reports the absence, illness is an excused absence.

[39] Teacher absences are coded by reason. We sum all illness related absences to create a absences due to sickness measure.

[40] The first stage regression reports a positive coefficient on the instrument.

Table 2.5: IV Regression

Score:	Math	Reading
Excused Absences	-0.0245	-0.0116
	(0.0526)	(0.0273)
Teacher Sick Days	Yes	Yes
Student FE	Yes	Yes
Teacher FE	No	No
N	53,827	53,827

Source: NCERDC, 2006-2008, grades 3-8. Dependent variable is standardized by grade and year. All specifications include dummy variables for grade, year, and size of student population in the county. Standard errors are reported in parenthesis. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%.

and therefore taken care of with the inclusion of student fixed effects. However, these estimates may still be biased if there are potentially time-varying unobserved family factors that may be influencing both student absences and testing performance. As mentioned previously, we follow Caetano and Macartney (2013) in utilizing the geocoded address data to construct a family ID variable. Table 2.6 incorporates a family-year fixed effect, which captures all observed and unobserved characteristics that are common to a family-year and is identified off of different incidence of absences within that year for a family.[41] Lagged test score is incorporated in the specification as a proxy for ability as that is likely to be different even across siblings. The family-year fixed effects specification controls for any family shock, such as parental divorce or a death in the family, that impacted both absences and test scores. Since siblings attend the same school, the coefficient on days of class cannot be well identified. The coefficient on absences in Table 2.6 indicates that an additional absence decreases scores by 0.76% and 0.42% for math and reading respectively, which is similar to our previous findings. This suggests that family specific shocks are not driving the results.

[41] The information in the data does not provide the biological relationship between children living in the same household. Regardless, since the students are residing in the same household and are therefore exposed to shared family characteristics, children living at the same address will be considered family.

Table 2.6: Siblings Fixed Effects Regression

	Math Score	Reading Score
Days Absent	-0.0076***	-0.0042***
	(0.0007)	(0.0007)
Sibling Year FE	Yes	Yes
Lagged Student Score	Yes	Yes
N	659,805	658,456

Source: NCERDC, 2006-2009, grades 3-5. Dependent variable is standardized by grade and year. All specifications include dummy variables for grade and year. Standard errors are reported in parenthesis. The sample is smaller than our main estimating sample as identification relies on observing at least two children from the same family in a given year. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%.

2.6.3 Health Shocks

Even after all of the controls to guard against endogeneity concerns, fixed effects do not guard against absences that are, for example, the result of a major illness; which is an excused absence and might be expected to have a direct effect on test scores. If this was driving our results, then after disaggregating absences into the two types, excused absences would be expected to be more negative relative to unexcused absences.[42] Specification (1) of Table 2.7 is our preferred specification from Table 2.3. Specifications (2) and (3) examine the effect of absences and days of class independently of the other; there is little difference in the magnitude of the effects when comparing to specification (1). The final two columns utilize the sample for which absences disaggregated by types are available.[43] Specification (4) examines specification (1) but with the sample of students for which there is data on absences disaggregated by type. The results are similar for absences, but becomes insignificant for days of class.[44] Specification (5) presents results disaggregating absences by type. An additional excused absence lowers math (reading) scores by 0.45% (0.22%)

[42] Gottfried (2009) also examines disaggregated absences and finds that students with a higher proportion of unexcused absences places them at academic risk, particularly in math achievement.

[43] As mentioned previously, absences by type are not available for the full sample. They are generally missing the school level.

[44] This is likely due to less variability in number of school days.

of a standard deviation, while unexcused absences have an effect of 0.73% and 0.46% of a standard deviation for math and reading respectively.

The evidence indicates that our baseline findings on the effect of students' absences on test scores performance are robust to several specifications, suggesting that possible threats to our identification strategy are not driving the results.

2.7 Heterogeneous Effects

On average, absences have a negative effect on test scores, while the positive impact of an additional day of class within the observed range is much smaller. However, these effects may differ based on student characteristics. As noted earlier, catching up after an absence is likely to be more difficult for a low performing student. Understanding the heterogeneous effects of an absence will help to inform the policy discussion by identifying groups of the population that are likely to disproportionately benefit from particular interventions.

To examine how the effect of attending school differ by student ability, students are grouped based on their test score from the prior year. Table 2.8 shows the regression results with absences and days of class interacted with a dummy for the quartile of the prior year's score. Score 1 denotes the lowest quartile and score 5 the highest. These results indicate that students in the lowest quartile are most adversely affected by an additional absence in both math[45] and reading; consistent with the hypothesis that lower ability students have a harder time making up missed work. A similar pattern can be found when considering days of class, i.e. low achieving students benefit the most from spending more time at school. Our findings also show the same pattern as before, when comparing the effect of an absence relative

[45] The lowest quartile interacted with absences is significantly different from the middle two quartiles. The top of the distribution has similarly negative effects on math scores. The highest quartile is not significantly different from the lowest quartile, but is from the others. Notice that this specification is controlling for quartile of previous year test score performance, instead of using a continuos measure of performance as in Table 3.

Table 2.7: Absences by Type Regression

	Math Test Score					Reading Test Score				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
Days of Class	0.0008**	0.0006		-0.0003	-0.0004	0.0002	0.0001		0.0002	0.0001
	(0.0004)	(0.0004)		(0.0008)	(0.0009)	(0.0005)	(0.0005)		(0.0010)	(0.0009)
Days Absent	-0.0058***		-0.0057***	-0.0055***		-0.0030***		-0.0030***	-0.0031***	
	(0.0002)		(0.0002)	(0.0002)		(0.0002)		(0.0002)	(0.0002)	
Excused Absences					-0.0045***					-0.0022***
					(0.0002)					(0.0003)
Unexcused Absences					-0.0073***					-0.0046***
					(0.0003)					(0.0004)
Student FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Teacher FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,000,896	1,000,896	1,000,896	583,112	583,112	1,000,896	1,000,896	1,000,896	583,112	583,112

Source: NCERDC, 2006-2010, grades 3-5. Dependent variable is standardized by grade and year. All specifications include dummy variables for grade, year, and free/reduced-price lunch status. Bootstrapped standard errors are reported in parenthesis. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%.

to an extra day of class within achievement level. Namely, absences have larger effects than days of class. In order to provide a robustness check, Table 2.9 estimates Equation 2.1 with absences and days of class interacted with student fixed effects instead of using prior scores to proxy for ability. The estimation outcomes show that the effects of both absences and additional days of class are muted for higher ability students which is consistent with our earlier results. For example, the effect of an absence in math (reading) performance for a student in the 25th percentile of the student fixed effect distribution[46] is 18.6% (69.4%) larger than for a student in the 75th percentile. To sum up, the findings in Tables 2.8 and 2.9 suggest that policies aiming to extend time spent at school are likely to have larger impact on low achieving students, helping to close current gaps in performance.

Table 2.10 further explores the relationship between absences and the quality of students, teachers and schools by regressing days absent on the three fixed effects from our preferred specification of the baseline regression (specification (4) in Table 2.3).[47] As expected from our previous results, lower ability students have more absences than their higher ability peers. However, we also find that worse schools (teachers) have a positive relationship with absences. More specifically, an increase in school (teacher) quality from the 25th percentile to the 75th percentile is associated with 0.21 (0.14) fewer days absent[48]. This is a relatively large effect given the sample average of 6 absences. In this regard, policies aiming to improve the quality of schools and teachers could not only benefit students by providing them with a better educational environment, but also by reducing the detrimental effects from

[46] The 25th and 75th percentile of the math (reading) student fixed effect distribution are -0.938 (-0.997) and 0.997 (1.108), respectively.

[47] Given that the fixed effects from the math and reading regressions are different, we present two set of results (i.e. the first column includes FE's from the math specification while the second column includes the FE's from the reading specification).

[48] This result corresponds to the specification that includes the fixed effects obtained from the regressions that use as dependent variable math test score.

Table 2.8: Differences by Ability

	Math Test Score	Reading Test Score
Days Absent x Score 1	-0.0092***	-0.0059***
	(0.0003)	(0.0003)
Days Absent x Score 2	-0.0080***	-0.0040***
	(0.0003)	(0.0003)
Days Absent x Score 3	-0.0079***	-0.0038***
	(0.0002)	(0.0003)
Days Absent x Score 4	-0.0080***	-0.0036***
	(0.0003)	(0.0003)
Days of Class x Score 1	0.0067***	0.0062***
	(0.0007)	(0.0007)
Days of Class x Score 2	0.0036***	-0.0027***
	(0.0008)	(0.0008)
Days of Class x Score 3	-0.0002	0.0005
	(0.0006)	(0.0007)
Days of Class x Score 4	-0.0035***	-0.0005
	(0.0006)	(0.0007)
School FE	Yes	Yes
Teacher FE	Yes	Yes
N	705,784	705,784

Source: NCERDC, 2006-2010, grades 3-5. Dependent variable is standardized by grade and year. All specifications include dummy variables for each quartile of prior year test score, grade, year, free/reduced-price lunch status, and ethnicity. Bootstrapped standard errors are reported in parenthesis. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%.

absences.

As a student advances in their educational career, it is likely that an increasing amount of material is covered in a given school day. For example, one might expect that more subject matter is taught in grade 5 than in grade 3. As a result, catching up could be more difficult in higher grades. Table 2.11 examines heterogeneous effects by grade. Indeed, absences appear to have a larger negative effect on both math and reading test scores at higher grades. While each additional absence decreases math (reading) scores by 0.25% (0.15%) of a standard deviation in grade 3, by grade 5 each absences has about three times the impact. This indicates that spending more time at school in later years may have larger effects on test score performance (at

Table 2.9: Differences by Ability: Student Fixed Effect

	Math Test Score	Reading Test Score
Days of Class	0.0007*	-0.0000
	(0.0004)	(0.0005)
Days of Class x Student FE	-0.0005**	-0.0011***
	(0.0002)	(0.0002)
Days Absent	-0.0057***	-0.0029***
	(0.0002)	(0.0002)
Days Absent x Student FE	0.0005***	0.0007***
	(0.0001)	(0.0001)
N	1,000,896	1,000,896

Source: NCERDC, 2006-2010, grades 3-5. Dependent variable is standardized by grade and year. All specifications include dummy variables for grade, year, and free/reduced-price lunch status. Bootstrapped standard errors are reported in parenthesis. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%.

Table 2.10: Days Absent

	Math Test Score	Reading Test Score
Student FE	-0.2653***	-0.1338***
	(0.0055)	(0.0045)
School FE	-0.6304***	-0.6691***
	(0.0291)	(0.0356)
Teacher FE	-0.5004***	-0.5216***
	(0.0245)	(0.0325)
N	1,000,896	1,000,896

Source: NCERDC, 2006-2010, grades 3-5. Dependent variable is days absent. Bootstrapped standard errors are reported in parenthesis. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%.

least in the short run).

Lower income students may also experience different effects relative to their wealthier classmates. This may be due to parents not having the same amount of time/resources to help their child with homework. Examining the effects by free/reduced price lunch subsidy program status in Table 2.12, we find that an additional absence has larger deleterious effects on test scores for low income students; an additional absence decreases math (reading) test scores by 0.13% (0.11%) of a standard deviation. Additional days of class also has a bigger impact on math (reading)

Table 2.11: Differences by Grade

	Math Test Score	Reading Test Score
Absences x Grade 3	-0.0025***	-0.0015***
	(0.0002)	(0.0003)
Absences x Grade 4	-0.0054***	-0.0030***
	(0.0002)	(0.0002)
Absences x Grade 5	-0.0088***	-0.0042***
	(0.0002)	(0.0002)
Days of Class x Grade 3	-0.0008	-0.0015**
	(0.0007)	(0.0007)
Days of Class x Grade 4	0.0010**	0.0004
	(0.0005)	(0.0005)
Days of Class x Grade 5	0.0019***	0.0012
	(0.0005)	(0.0007)
Student FE	Yes	Yes
School FE	Yes	Yes
Teacher FE	Yes	Yes
N	1,000,896	1,000,896

Source: NCERDC, 2006-2010, grades 3-5. Dependent variable is standardized by grade and year. All specifications include dummy variables for grade, year, and free/reduced-price lunch status. Bootstrapped standard errors are reported in parenthesis. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%.

achievement of low income students, with an extra day of class increasing scores by 0.18% (0.29%). The larger effect on reading for additional class days suggests that these students may not be getting the same reading enrichment at home as their wealthier peers.[49]

Overall, the results from this section show the presence of important heterogeneous effects of time spent at school. Low achieving students or those coming from less wealthy families would benefit the most from having less absences or attending school more days during the year. Therefore, this findings indicate that increasing instructional time (mainly by decreasing absences) most likely will contribute to close gaps in performance.

[49] We also studied heterogeneous effects across race and gender, but did not find statistically significant results.

Table 2.12: Differences by Free/Reduced Price Lunch Status

	Math Test Score	Reading Test Score
Absences	-0.0051***	-0.0024***
	(0.0002)	(0.0002)
Absences x FRL	-0.0013***	-0.0011***
	(0.0003)	(0.0003)
Days of Class	-0.0001	-0.0013***
	(0.0004)	(0.0005)
Days of Class x FRL	0.0018***	0.0029***
	(0.0003)	(0.0004)
Student FE	Yes	Yes
School FE	Yes	Yes
Teacher FE	Yes	Yes
N	1,000,896	1,000,896

Source: NCERDC, 2006-2010, grades 3-5. Dependent variable is standardized by grade and year. All specifications include dummy variables for grade, year, and free/reduced-price lunch status. Bootstrapped standard errors are reported in parenthesis. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%.

2.8 Conclusions

This chapter is the first attempt to jointly estimate the relative effectiveness of reducing absences to extending the school calendar on test score performance. Despite the fact that many policy makers have focused their attention on extending the school calendar, the evidence presented in this manuscript indicates that targeting absenteeism could constitute a more effective intervention. First, our empirical strategy shows that the effect of reducing absences relative to extending the number of school days is substantial. Our preferred specification indicates that extending school calendar by ten days would increase math and reading test scores by only 0.8% and 0.2% of a standard deviation, respectively; while a similar reduction in absences would lead to increases of 5.8% and 3% in math and reading. Second, results point to the presence of important heterogeneous effects. Missing a school day due to absence in grade 5 is three times more detrimental than in grade 3, and more importantly, low performing kids benefit the most from additional instructional time. The fact that reducing absenteeism can target specific students who would benefit the most from being in the classroom, not only suggests that initiatives targeting absenteeism could be more effective than just extending the school calendar, but also could contribute to narrowing current achievement gaps.

Estimation results also show that improving both school and teacher quality from the 25th percentile to the 75th percentile would decrease the average number of absences by about 6%. Therefore, policies aiming to improve the quality of schools and teachers could not only benefit students by providing them with a better educational environment, but also by reducing the detrimental effects from absences.

To conclude, the financial resources needed to extend the school calendar are undeniably high. Most calculations suggest that a 10 percent increase in time would require a 6 to 7 percent increase in cost [Chalkboard Project (2008), Silva (2007)]. This type of policy is even more difficult to implement in a context of decrease in per student public education spending.[50] Therefore, the fact that a competing policy, like targeting absenteeism on specific groups of students, could lead to large improvements in academic performance at a lower cost,[51] points towards an avenue of policy that requires far greater attention.

[50] Public Education Finances: 2011, U.S. Census Bureau, http://www.census.gov/govs/school/

[51] For example, the program "WakeUp! NYC" has been implemented using media tools (i.e. SchoolMessenger) that has already been incorporated in large number of schools for other purposes.

3

Gaming The System? Incentives and Setting High Stakes Testing Dates

3.1 Introduction

While the results from Chapter 2 show small effects of extending the school calendar, it remains an open question as to whether different institutional settings could affect how school administrators and teachers respond to possible extensions of the academic year.

North Carolina provides a testing window (last three/four weeks of class) during which schools administer the end of grade exams, providing schools with some flexibility in when their students are tested. Depending on the incentives that are in place (e.g. monetary bonus based on students performance), school administrators may act strategically by increasing/decreasing the total number of school days prior to the date of the test.[1] Elimination of teachers' incentive pay in the later years of the sample provides an opportunity to analyze school administrator and teacher

[1] Test scores must be submitted before a given deadline established by the North Carolina Testing and Accountability Programs, potentially generating a cost of delaying the day of the exam (i.e. schools will have less time to grade the exams). Therefore, it is expected that not all schools will make full use of the testing window.

behaviors before and after the removal of monetary bonuses. In order to formalize how incentives may shape the behavior of educators, we present a simple theoretical framework. The model provides two main conclusions. First, teachers effort and the number of days of class (before the day of the exam) determined by the school principal are negatively related to the performance of the students in the preceding year. Second, removal of the financial incentives leads to a decrease in teacher effort and fewer days of class (before the exam). Consistent with these conclusions, the empirical evidence shows that low performing schools are more likely to make extensive use of the testing window when monetary bonuses are in place; this behavior disappears after changes to the scheme of incentives (e.g. elimination of monetary bonuses). Overall, these results suggest that different institutional settings will affect how educators make use of available school time.

The remainder of this chapter is organized as follows. Section 3.2 details the data used in the empirical analysis. Section 3.3 provides a brief description of accountability programs in North Carolina. Section 3.4 presents a theoretical framework with which to analyze strategic behavior in the setting of the testing date by schools. The results of an empirical specification examining this behavior are presented in Section 3.5. Section 3.6 concludes.

3.2 Data

The North Carolina education data is a rich, longitudinal, administrative data set that links information on students, teachers, and public schools over time. It is the same data as was utilized in Chapter 2. This longitudinal database contains mathematics and reading test scores for each student in elementary,[2] middle, and high school.

[2] More specifically, for grades 3 and above. Students in lower grades do not take end of grade (EOG) tests, but a test is administered in September as well as the end of grade 3. All other grades were tested in either May or June of that year.

The NCERDC records also include extensive information on student, teacher, and school characteristics. Data on students include ethnicity, gender, number of days in membership, and whether or not they participated in the federal free and reduced price lunch subsidy program. The days in membership for a student is the number of days the student was on the roster in a particular school; a student is in membership even when absent. The day of the EOG exam is discerned from the data by using the modal days in membership for the school. School data includes the name, district, overall performance in fulfilling NCLB requirements as well as demographics of the student and teacher body.

Since the availability of some of the data varies over time and we are interested in the impact of a change in the structure of incentives on setting of the test date which occurred in between 2008 and 2009, the analysis is restricted to the years 2006 to 2010[3] and grades 3 to 8. Encrypted identifiers make it possible to track the progress of individual student over their educational careers and link students to their school.

3.3 Accountability in North Carolina

North Carolina has a long history with accountability programs which have altered the incentives for teachers and schools over time. In 1997, ABCs (Accountability for Basic skills and for local Control) was introduced with the aim of holding schools accountable for their value added. The main objective of this policy is to quantify how much children improve while being enrolled in a given school. To this end, teachers and staff at schools that raise student achievement above a certain threshold receive salary bonuses.[4] In the 2002-2003 academic year, No Child Left Behind (NCLB) was layered on top of the ABCs program. NCLB mandates that all students be proficient

[3] School years are referred to by the year the school year ended. For example, the 2005/06 school year is year 2006.

[4] Bonuses range from $500 to $1,500.

by 2014, and that each school must make Adequate Yearly Progress (AYP) towards meeting this objective, not only overall, but also for a set of demographic subgroups within each school. Schools failing to achieve AYP for two consecutive years begin to face sanctions, where their severity can increase depending on past history. Two main differences distinguish these accountability programs. ABCs focuses on average gains in test scores, and its structure of incentives affects directly teachers behavior (i.e. monetary bonus). In contrast, NCLB evaluates schools based on proficiency levels, and the scheme of incentives is designed to mainly affect school principals' behavior.

North Carolina uses end of grade (EOG) testing in both math and reading for grades 3 through 8 to quantify student's improvement and determine whether or not a school has met its expected growth and proficiency levels. The state provides a testing window during which schools administer the end of grade exams. The EOG tests were required to be administered during the last three or four weeks of classes,[5] providing schools with some flexibility as to when they test their students.[6] However, making full use of the testing window may be costly, given that test scores must be submitted before a given deadline established by the North Carolina Testing and Accountability Programs. Therefore, this generates a cost of delaying the day of the exam given that schools will have less time to grade the exams. It is therefore expected that those schools who could benefit the most from an extra day of class prior to the exam are the ones who will set a later testing date.

Beginning in 2009, two main changes to the scheme of incentives were introduced. First, ABCs discontinued incentive pay to teachers. Second, students that performed

[5] http://www.ncpublicschools.org/accountability/calendars/archive lists the testing windows for all tests administered in North Carolina since 2001.

[6] Superintendents and principals were responsible for setting the testing dates for an individual district and school.

below, but close to proficiency levels in a given subject,[7] were required to retake the test. The higher of the two grades is considered for accountability purposes. The combination of no monetary bonuses with the fact that schools may want to have extra time to focus on those students who need to retake the exam may have substantially changed how schools set the exam day within the testing window. Therefore, changes in the structure of incentives are likely to affect how schools value an extra day of class before the exam. In this regard, the aim of this section is to analyze this type of strategic behavior in order to shed some light on how schools may respond to possible extensions of the school calendar when different institutional settings are in place.[8] Next, we present a simple theoretical framework that intends to formalize these concepts.

3.4 Theoretical Framework

Consider the following scenario where a school principal has to set the number of schools days before the day of the exam, and a teacher that has to decide the amount of effort that she will exert conditional of the number of instructional days and the scheme of incentives that are in place. Moreover, assume that test score production function of student i, with teacher k, in grade g, in school s, during year t is given by the following expression:

$$Test_{igkst} = s_i + e_{gkt} d_{st} + \varepsilon_{igkst}$$

where s_i denotes student ability, e_{gkt} level of effort per unit of time exerted by the teacher, d_{st} total number of school days in school s, and ε_{igkst} denotes an error term. For simplicity, we impose that e_{gkt} and $d_{st} \in (0, 1]$.

[7] NCLB divides student performance into 4 categories. Levels 3 and 4 denote proficient or more, while levels 1 and 2 indicate a student is not proficient in that subject. Since 2009, students who achieve level 2 have been required to retake the test.

[8] Other papers examining strategic behavior of schools in the presence of accountability include Jacob and Levitt (2003) and Macartney (2013).

3.4.1 Teachers Maximization Problem

We assume that teachers derive utility from the average performance of the students in their classroom, and from a monetary bonus that depends on the gains in student performance:

$$U = \underbrace{\overline{Test}_{gkst}}_{\text{Classroom average performance}} + \underbrace{\alpha_1 \left[\frac{\overline{Test}_{gkst} - \overline{Test}_{gkst_{-1}}}{\overline{Test}_{gkst_{-1}}} \right]}_{\text{Bonus}} \tag{3.1}$$

where $\overline{Test}_{gkst}$ denotes the average test performance in the classroom in year t, and $\alpha_1 \left[\frac{\overline{Test}_{gkst} - \overline{Test}_{gkst_{-1}}}{\overline{Test}_{gkst_{-1}}} \right]$ represents the bonus that a teacher gets if her students improve their performance. If we replace $\overline{Test}_{gkst}$ by its definition in Equation 3.1, then we have:

$$U = \underbrace{\frac{1}{N} \sum_{i=1}^{N} [s_i + e_{gkt} d_{st} + \varepsilon_{igkst}]}_{\text{Classroom average performance}} + \underbrace{\alpha_1 \left[\frac{\frac{1}{N} \sum_{i=1}^{N} [s_i + e_{gkt} d_{st} + \varepsilon_{igkst}] - \overline{Test}_{gkst_{-1}}}{\overline{Test}_{gkst_{-1}}} \right]}_{\text{Bonus}}$$

Notice that teachers can only choose the level of effort per unit of time, and days of class are taken as given. Finally, we assume the following functional form for the teachers effort cost function:

$$C(e_{gkt}) = \gamma_1 \left[e_{gkt} d_{st} \right] + \gamma_2 \left[e_{gkt}^2 d_{st} \right] + \gamma_3 \left[e_{gkt} d_{st}^2 \right]$$

Therefore, teachers' problem can be written as follows:

$$\max_{e_{gkt}} U = \max_{e_{gkt}} \underbrace{\frac{1}{N}\sum_{i=1}^{N}[s_i + e_{gkt}d_{st} + \varepsilon_{igkst}]}_{\text{Average performance of classroom}}$$

$$+\underbrace{\alpha_1 \left[\frac{\frac{1}{N}\sum_{i=1}^{N}[s_i + e_{gkt}d_{st} + \varepsilon_{igkst}] - \overline{Test}_{gkst_{-1}}}{\overline{Test}_{gkst_{-1}}}\right]}_{\text{Bonus}}$$

$$-\underbrace{\left[\gamma_1 \left[e_{gkt}d_{st}\right] + \gamma_2 \left[e^2_{gkt}d_{sy}\right] + \gamma_3 \left[e_{gkt}d^2_{st}\right]\right]}_{\text{Effort cost function}}$$

The first order condition w.r.t. e_{gkt} is given by:

$$\frac{1}{2\gamma_2}\left[1 + \frac{\alpha_1}{\overline{Test}_{gkst_{-1}}} - \gamma_1 - \gamma_3 d_{st}\right] = e^*_{gkt}$$

where $\gamma_n \geqslant 0$ with $n = \{1, 2, 3\}$, $\alpha_1 > 0$, and $1 + \frac{\alpha_1}{\overline{Test}_{gkst_{-1}}} > \gamma_1 + \gamma_3 d_{st}$. Therefore, the optimal level of effort is increasing in the "price" (i.e. α_1) of the monetary bonus, but decreasing on the total instructional time, and the average performance of the class in the previous year.

3.4.2 School Principal Problem

The school principal has to determine the number of school days prior the exam (i.e. d_{st}), where the optimal effort exerted by the teachers (i.e. e^*_{gkt}) is taken as given. We assume a benevolent principal who only cares about the average performance of the students in the classroom. Therefore, the objective function is given by:

$$\max_{d_{st}} U = \max_{d_{st}} \underbrace{\frac{1}{N}\sum_{i=1}^{N}[s_i + \frac{1}{2\gamma_2}\left[1 + \frac{\alpha_1}{\overline{Test}_{gkst_{-1}}} - \gamma_1 - \gamma_3 d_{st}\right] d_{st} + \varepsilon_{igkst}]}_{\text{Average performance of classroom}}$$

where $\frac{1}{2\gamma_2}\left[1+\frac{\alpha_1}{\overline{Test}_{gkst_{-1}}}-\gamma_1-\gamma_3 d_{st}\right]=e^*_{gkt}$. The first order condition w.r.t. d_{st} is given by:

$$\frac{1}{2\gamma_3}\left[1-\gamma_1+\frac{\alpha_1}{\overline{Test}_{gkst_{-1}}}\right]=d^*_{st}$$

This implies that the optimal number of school days before the exam is negatively correlated with the performance of the classroom in the previous year. Moreover, the model shows that optimal instructional time would decrease and would be similar across schools if monetary bonuses were eliminated (i.e. $\frac{\alpha_1}{\overline{Test}_{gkst_{-1}}}=0$).

Two main conclusions can be obtained from the model. First, teacher effort and the number of days of class (before the day of the exam) determined by the school principal are negatively related to student performance in the previous year. Second, removal of financial incentives leads to a decrease in teacher effort and fewer days of class (before the exam). While the model is not able to capture the role that re-testing may have on teachers and school administrators' behavior, it is expected that this may lead to a further decrease in the instructional time.

3.5 Empirical Strategy

In order to test whether low performing schools act strategically by making a more extensive use of the testing window when monetary bonuses were in place, we exploit the fact that beginning in 2009, ABCs discontinued incentive pay to teachers and re-testing results were allowed to be use for accountability purposes. In this regard, we estimate the following difference-in-difference specification:

$$D_{ust}=\beta_0+\beta_1\overline{Test}_{ust_{-1}}+\beta_2 Post+\beta_3 Post\times\overline{Test}_{ust_{-1}}+\beta_4 X_{st}+\epsilon_{ust} \qquad (3.2)$$

where D_{ust} is the percentile rank of total number of class days prior the EOG exam in subject u at school s in year t. *Post* is a dummy variable equal to one for years 2009 and 2010; the years incentive pay was not in place. $\overline{Test}_{ust_{-1}}$ is the average school test score in the previous year, X_{st} is a vector of school covariates. If schools were strategically setting testing dates so as to increase the likelihood of improved student performance and enable monetary rewards for the teachers and staff, then schools with the lowest test scores would be most likely to increase the number of instructional days prior to the test, suggesting a negative coefficient on lagged test score. Barring the incentive, schools would then be expected to have fewer instructional days; implying a negative coefficient on the post dummy variable.[9]

Table 3.1 presents the results from the difference-in-difference specifications with school percentile rank of the number of class days prior the EOG exam as the dependent variable. As hypothesized, columns (1) and (2) of Table 3.1 show that the signs on lagged scores for both math and reading are negative and significant. More specifically, schools with a lagged math score 1 standard deviation below the mean, would increase their percentile rank by 8.5 percentage points; where the results are even stronger for reading. However, estimation results show that after the elimination of monetary bonuses high and low achieving schools do not show substantial differences in their number of school days ranks. These results are consistent with the predictions of our theoretical framework, suggesting that low performing schools value an extra day of class more when monetary bonuses are binding.

In order to provide a robustness check, specification (3) examines a modified version of Equation 3.2. Specification (3) examines how last year's accountability status affects the number of school days before the exam. High status schools are

[9] However, schools still face sanctions. If a school misses AYP for two consecutive years in the same subject, the district must offer transfers (with transportation) to higher-performing public schools in the same district. After three years, schools must offer supplemental education services. Subsequent failure to make AYP results in changes to leadership and/or staffing and restructuring of the school.

Table 3.1: Gaming: Difference-in-Difference

	(1) Math	(2) Reading	(3)
Post	0.0329***	0.0334***	-0.0062
	(0.0081)	(0.0081)	(0.0128)
Lagged Score	-0.0852***	-0.0978***	
	(0.0188)	(0.0216)	
Lagged Score x Post	0.0785***	0.1016***	
	(0.0219)	(0.0233)	
High Status			-0.0917*
			(0.0496)
High Status x Post			0.0881
			(0.0636)
Middle Status			0.0252**
			(0.0122)
Middle Status x Post			0.0328*
			(0.0181)
N	5,413	5,413	5,988

Source: NCERDC, 2006-2010. All specifications include percent hispanic, percent black, percent white, percent free/reduced price lunch and school size. Standard errors are reported in parenthesis. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%

those that received a status of Honor school of excellence or school of excellence in the prior year. Middle status are schools that received a status of school of distinction or school of progress in the previous year. The excluded category are schools that received a status of no recognition, low performing or priority school in the prior year.[10] Teacher at both high and middle status schools received a bonus for having met or exceeded the expected growth goal. Consistent with our earlier findings, higher status schools had on average fewer days of class in the subsequent year, but this effect is offset with the change in the incentives. The evidence indicates that different institutional settings do affect how schools value an extra day of class.

[10] Honor School of Excellence, School of Excellence, School of Distinction, and School of Progress are schools that make or exceed expected growth. A School of Excellence and Honor School of Excellence also has at least 90%, of students' score at or above achievement level 3, School of Distinction has between 80% and 89% at level 3 or above, and School of Progress has 60% to 79% of students proficient. All other categories have not met expected growth. Full definitions of each status category is available at http://www.ncschoolreportcard.org/src/performance.jsp

Table 3.2: Gaming: Retesting

	Post		Pre	
	Math	Reading	Math	Reading
D^{-}	-0.2219***	-0.2389***	0.0112***	0.0059
	(0.0034)	(0.0030)	(0.0035)	(0.0038)
School FE	Yes	Yes	Yes	Yes
N	136,775	154,517	389,060	299,604

Source: NCERDC, 2009, grades 3-5 conditional on the first test being +/- 5 points from the proficiency cutoff in year t. All specifications include dummies for hispanic, black, white, asian, male, free/reduced lunch and grade. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%

Therefore, policies aimed at extending the school calendar would likely benefit by providing schools the right incentives to make each extra day more effective.

Previously, we mentioned that beginning in 2009, students who perform below but close to the proficiency threshold in a given subject (i.e. level 2)[11] were required to retake the test. Then only the highest grade is considered for accountability purposes. Teachers may decide to act strategically by concentrating their efforts on those students with a higher probability of becoming proficient, particularly between the time of the original testing and the retesting; they are teaching to the test. To explore this hypothesis, we analyze a sample of students that are +/- 5 points from the proficiency cutoff for their grade and subject. If teachers are teaching to the test in the period between testings, one would expect that students that passed the test on the first try (those just above the proficiency threshold) would do better relative to their peers just below the threshold in the subsequent year's test. This is because those that passed initially likely learned the material, while those that took the retest only retained the information for the next exam. To test this hypothesis, we run the following specification:

$$y_{igks,t+1} = \beta_0 + \beta_1 \max(y_{igkst}) + \beta_2 D^{-}_{igkst} + \beta_3 X_{it} + \beta_4 G_{ig} + \delta_s + \epsilon_{igkst} \tag{3.3}$$

[11] NCLB divides student performance into 4 categories. Levels 3 and 4 denote proficient or more, while levels 1 and 2 indicate a student is not proficient in that subject. Since 2009, students who achieve level 2 have been required to retake the test.

where y_{igkst} denotes the test score of student i, in grade g, teacher k, school s, and year t where the test score is standardized by grade, year and subject. The explanatory variable is the maximum of the test scores in year t; this could either be the score on the original test or the retest. The main variable of interest is D^{-}_{igkst} which is a dummy variable for those students who were just below the proficiency cutoff in year t. X is a vector of time-varying student covariates, G are grade fixed effects, and δ_s denotes school fixed effects. If teachers are in fact teaching to the test, we would expect that the dummy variable D^{-}_{igkst} would have a negative coefficient; that students just below the proficiency threshold on the original test, would do relatively worse in the subsequent year.

Columns 1 and 2 of Table 3.2 shows that after controlling for the prior year's performance, students who were not proficient on the original test in year t do relatively worse than their peers who were just above the proficiency threshold.[12] In contrast, before retesting was used for accountability purposes, there was no difference in the growth in reading scores, and those below the threshold, saw a greater gain in math scores. Therefore, these results suggests that teachers may concentrated their efforts between the original examination and the retest when the retest counted for accountability purposes on "teaching to the test." This is consistent with the findings of Neal and Schanzenbach (2010) who show that NCLB is likely to increase scores for marginal students.

3.6 Conclusion

This chapter examines how the change in North Carolina's accountability program affect how school administrators and teachers respond to possible extensions of the academic year. We present a theoretical model in which principals set the date of the

[12] The sample size is smaller because only year 2009 was used since the retesting only counted for accountability measures in 2009 and 2010 and our specification requires year-ahead scores.

test and teachers decide how much effort to exert in the classroom with and without monetary performance bonuses for teachers. The model provides two main conclusions.First, teachers effort and the number of days of class (before the day of the exam) determined by the school principal are negatively related to the performance of the students in the preceding year. Second, removal of the financial incentives leads to a decrease in teacher effort and fewer days of class prior to the EOG test.

Leveraging the removal of monetary bonuses during the sample period, we show that low performing schools seem to value an extra day of class more when monetary bonuses are binding. In this regard, the effectiveness of policies that aim to extend the school calendar are likely to vary depending on the scheme of incentives that are in place. Therefore, identifying the mechanisms that could lead to stronger complementarities between accountability programs and possible extensions of the school calendar could substantially contribute to make each extra day of class worth it.

4

Recovering Ex Ante Returns and Preferences for Occupations using Subjective Expectations Data

4.1 Introduction

Subjective expectations data are increasingly being used in economic research. While early work focused on the accuracy of individual's forecasts over objective events (e.g., Manski, 1993; Dominitz and Manski, 1996, 1997), subjective expectations are now being used in the estimation of structural dynamic models (e.g., Delavande, 2008; van der Klaauw and Wolpin, 2008, 2012).[1] Collecting data on subjective expectations makes it possible to estimate forward-looking models without making strong assumptions about how individuals form their beliefs about potential outcomes along different choice paths.

Relatively new to the literature is (i) the elicitation of the probabilities of taking particular courses of actions in the future and (ii) expectations about potential future outcomes corresponding to these counterfactual choices, which covers beliefs off the individual's actual choice path. In this chapter, we use data on future choice

[1] See Manski (2004) for a survey of the literature. See Pantano and Zheng (2010) on using subjective expectations data to recover unobserved heterogeneity.

probabilities as well as subjective expectations about outcomes both on and off the individual's choice path to recover the expected benefits as well as subjective costs associated with different treatments and assess the relative roles played by these two components with respect to the selection into alternative treatments. While the proposed approach can be applied to a broad class of potential outcome models, in this chapter we consider the examine the tradeoff of between *ex ante* monetary returns and non-pecuniary preferences over the choice of different occupations and college majors. As recently emphasized in the literature on schooling decisions (Cunha et al., 2005; Cunha and Heckman, 2007), agents' decisions are based on *ex ante* monetary returns, as opposed to *ex post* ones. In contrast to this literature, we use data that directly elicits agents' *ex ante* returns and non-pecuniary preferences that enter into their choices over occupations. As a result, we are agnostic about how agents form their information sets and do not require exclusion restrictions to identify the separate influence of monetary returns vs. non-pecuniary factors.[2]

Overall, there are large differences in the earnings of college graduates both across majors and occupations. For instance, data from the American Community Survey (2009-2010) reveal that those who majored in engineering earn as much as 77% more than those who majored in the humanities. To the extent that a sizable fraction of college graduates work in an occupation which does not match their major, those earnings differentials across majors mask the existence of substantial within-major dispersion (Kinsler and Pavan, 2012). However, the typical strategy for computing earnings differentials across college majors and/or occupations for use in empirical

[2] In a recent work, D'Haultfoeuille and Maurel (2013) investigate the relative importance of *ex ante* monetary returns versus non-pecuniary factors in the decision to attend college. Their approach, which can be used in the absence of subjective expectation data, requires imposing stronger restrictions on the non-pecuniary factors. See also Eisenhauer et al. (2012), who use exclusion restrictions between monetary returns and non-pecuniary factors to separately identify these two components.

choice models[3] is to use the earnings of those who chose particular college majors and occupations. Per se, earnings differentials based on such data tell us very little about what the individual would *expect* to earn had the individual pursued a counterfactual occupation or graduated with a different major. Moreover, such data, by itself, is not likely to be informative about how much individuals would need to be compensated for pursuing a different career path.

In this chapter, we use elicited beliefs from male undergraduates at Duke University to quantify the importance of sorting across occupations on *ex ante* monetary returns versus preferences. This unique dataset contains student expectations regarding the probability of working in different occupations as well as their expected income in each of the occupations where the period of reference is ten years after they graduate.[4] These occupation probabilities and expected incomes were asked not only for the major the individual chose but also for counterfactual majors, making it possible to disentangle both the monetary returns from different majors in different occupations as well as how attractive working in particular occupations is with different majors. By doing so, we add to a growing set of papers using subjective expectations data to distinguish between the role played by monetary returns versus non-pecuniary preferences in college major and occupational choices (Betts, 1996; Zafar, 2011, 2013; Arcidiacono et al., 2012; Wiswall and Zafar, 2012; Stinebrickner and Stinebrickner, 2013; Osman, 2013).

The data allow us to identify both the *ex ante* treatment effects of particular occupations on earnings, for any given college major, as well as the *ex ante* treatment effects of particular majors on the probabilities of working in any given occupation.

[3] See Altonji et al. (2012) for a recent review.

[4] This dataset was previously used to examine the determinants of college major choice by Arcidiacono et al. (2012b). Their paper treated occupations as lotteries where the lotteries were affected by the choice of major. In this paper, we follow a more conventional route and treat occupations as choices, consistent with, e.g., Miller (1984), Siow (1984), Keane and Wolpin (1997) and van der Klaauw (2012).

Even though we do not observe the actual occupations chosen by the individuals, we show that subjective expectation data on occupational choice probabilities can be used to recover the *ex ante* treatment effects of a given occupation (relative to a reference occupation) for the subpopulation of individuals who will end up working in that occupation (*ex ante* treatment effect on the treated). Taking the initial major as given, the *ex ante* treatment effect on the treated for a given occupation j is simply computed by weighting the reported earnings differences between occupation j and the reference by the probability the individual reports that he will work in occupation j (over the average declared probability of working in occupation j). *Ex ante* treatment effect on the untreated are obtained similarly, by using the declared probability that the individual will not work in occupation j. Importantly, our data allows us to go beyond these average effects and investigate the heterogeneity across individuals by estimating the full distributions of the *ex ante* treatment effects of working in any given occupation j relative to education, given the initial college major choice. Data on counterfactual occupational choice probabilities also allows us to recover the distribution of the *ex ante* treatment effects within the treated and untreated subpopulations.

The results reveal substantial differences in terms of expected earnings across majors as well as occupations. Treating the education occupation as the baseline, the *ex ante* treatment on the treated ranges from 25% higher earnings (government) to 89% higher earnings (health) ten years after graduation. Consistent with sorting across occupations being partly driven by expected monetary returns, the *ex ante* returns are generally higher for the treated than for the untreated, suggesting positive selection into occupations. Consistent with the existence of occupation-specific human capital accumulated within each major, we also document the existence of a substantial degree of heterogeneity in the *ex ante* returns for each occupation, conditional on a particular college major. For example, public policy majors who anticipate entering a health career expect a 38% premium (relative to a career in education), while natural sciences majors expect a 117% premium for a health career.

We next link the subjective expectations data to a model of occupational choice where individuals are uncertain over their preferences for particular occupations in the future. This simple framework allows us to link the subjective data on expected earnings and choice probabilities with the non-pecuniary preferences. Specifically, under standard assumptions, unobserved preferences will have continuous support, implying that perceived occupation probabilities should be bounded away from zero and one. However, in our data, some individuals do report zero probabilities of pursuing a particular occupation, conditional on a particular major. To reconcile our conceptual framework with the elicited choice probabilities we obtain, we assume that the resolution of preference uncertainty is costly to agents. That is, we assume that individuals must bear a cost to acquire additional information about a given occupation, but that they will only do so if the expected benefits of doing so are sufficiently high. In estimation, we then follow Hotz and Miller (1993) and Berry (1994) and invert the perceived choice probabilities, taking into account the selection introduced by costly information acquisition, to recover preferences over occupation-major combinations.

Our empirical model of agents' valuations of occupations – which depend, in part, on the expected incomes elicited for each occupation – allow us to calculate compensating differentials for these occupations and how they vary with different majors. Overall, our results are consistent with the existence of fairly large compensating differentials across occupations that vary substantially across majors. For instance, while public policy majors would have to receive a premium of 137.8% to pursue a career in education rather than in government, the opposite is true for those with a major in the humanities, who would have to receive a premium of 73.7% to pursue a governmental career. Aside from the complementarities of preferences between different majors and occupations, the large compensating differentials associated with major-occupation pairs are consistent with search frictions that result from different job offer arrival rates for each occupation across college majors. Regardless of the mechanism, our results provide clear evidence that majors have a substantial influence on occupations well beyond their impact on earnings.

The rest of the paper proceeds as follows. In section 4.2, we discuss the survey data used in the paper. Section 4.3 shows how to obtain *ex ante* treatment effects given the survey data with Section 4.4 giving the estimated treatment effects. We then link the subjective occupational choice probabilities and expected incomes with a model of occupational choice in Section 4.5. Estimates of the model and the corresponding implications in terms of compensating differentials and search frictions are presented in Section 4.6. Finally, we offer some concluding comments in Section 4.7.

4.2 Data

We use data collected on a sample of male undergraduate students at Duke University between February and April 2009. Gender was the only restriction on sample recruitment; students from any major, class, or race were eligible to participate in the survey. Sample members were recruited by posting flyers about our study around the Duke campus. Surveys were administered on computers in a designated room in Duke's Student Union.[5] All 173 students who completed the survey were paid $20.[6]

This is the same data as the one used in Arcidiacono et al. (2012b). That paper provided many descriptive statistics on how majors, occupations, and earnings were related and we refer the reader to that paper for an overview of the data. We

[5] The questionnaire which was used in the survey is discussed further in Kang (2009).

[6] We drop from our analysis five individuals who reported that they would choose an occupation with certainty for each major, resulting in a final sample of 168 students.

report in Table 4.1 a descriptive overview of our sample, compared with the overall male undergraduate population at Duke. One can see from Table 4.1 that our sample corresponds fairly closely to the Duke male undergraduate student body, even though it includes slightly more Asians and fewer Latinos and Blacks. It also appears that a higher percentage of our sample receives some financial aid than is the case in the Duke student body, although the 22.0% figure for the student body is based on aid provided by Duke, whereas the higher percentage of students receiving financial aid (40.5%) is likely due to the fact that our survey asked about receipt of financial aid, regardless of source. Finally, our sample is slightly tilted towards upper-classmen.

Table 4.1: Sample Descriptive Statistics

	Sample	Duke Male Student Body
Current/Intended Major:		
Natural Sciences	18.5%	14.8%
Humanities	9.5%	9.4%
Engineering	19.1%	20.7%
Social Science	18.5%	18.8%
Economics	20.2%	18.0%
Public Policy	14.3%	18.0%
Class/Year at Duke:		
Freshman	20.2%	
Sophomore	20.2%	
Junior	27.4%	
Senior	32.1%	
Characteristics of Students:		
White	66.7%	66.0%
Asian	19.6%	16.6%
Latino	4.8%	8.3%
Black	4.2%	5.9%
Other	4.8%	3.0%
U.S. Citizen	95.2%	94.1%
Receives Financial Aid	40.5%	22.0%
Sample Size	168	

Distinctive to this analysis is our focus on occupations as choices, as the previous paper treated occupations as lotteries. Evidence that individuals are viewing occupations as choices can be found in Table 4.2. Table 4.2 presents students' expected earnings associated with each of the possible college majors under two scenarios. In the first, "Reported Probabilities," expected earnings are based on the weighted average of elicited earnings for the different occupations where the weights are the subjective probabilities of entering each occupation that were elicited from students. In the second, "Random Assignment," we use the elicited earnings for an occupation selected at random.[7] For the random assignment case, we use the population probabilities of choosing each occupation for those in the same major. For all majors, students expected earnings are higher using their reported probabilities of sorting into occupations relative to if they were randomly assigned. This pattern points to the existence of sizable gains to sorting, consistent with the individuals pursuing their comparative advantage when choosing an occupation.[8]

Table 4.2: Expected Earnings for Occupations (Annual Earnings, in dollars)

Major	Reported Probabilities	Random Assignment	Difference
Natural Sciences	169,385	144,710	24,675
Humanities	115,786	106,325	9,461
Engineering	125,578	115,413	10,165
Social Sciences	125,578	111,214	14,364
Economics	160,488	133,363	27,125
Public Policy	180,350	154,823	25,527

Table 4.3 presents the average subjective probabilities of working in each occu-

[7] In our sample, only 1.57% of the expected earnings are missing. For these cases, expected earnings, for each major and occupation, are set equal to the predicted earnings computed from a linear regression of log-earnings on major and occupation indicators, interaction between major and occupation, average log-earnings across all occupations and majors and an indicator for whether the subjective probability of working in this occupation is equal to zero.

[8] Since we are using subjective data, one might be concerned that these gains to sorting are partly driven by ex post rationalization. However, in the paper we focus on the question of sorting across occupations, which have not been effectively chosen by the students at the time of the survey. Furthermore, in our sample, 90% of the declared probabilities of working in a given occupation are smaller than 40%, so that ex post rationalization is unlikely to affect our results.

pation, conditional on each major. Not surprisingly, the subjective probabilities of entering each occupation vary substantially across majors. Nonetheless, it is worth noting that none of the majors are concentrated into only one (or two) occupations. Even for majors which appear to be more tied to a specific occupation, such as business career for economics majors, subjective probabilities exhibit a fairly large dispersion across individuals (see Figure 1). Overall, the likelihood of going into the various occupations are different across individuals, even after conditioning on a college major.

Finally, Table 4.4 reports the prevalence of zero probability reported by students, for each major-occupation combination.[9] While some combinations display a large share of zero subjective probabilities – e.g., economics or public policy and science or engineering and law – there is always a substantial fraction of students who do report a non-zero probability of choosing a particular occupation, conditional on a particular college major.

Table 4.3: Elicited Probability of Choosing Different Occupations, Conditional on Major

	Probability of Occupation in:					
Major:	Science	Health	Business	Government	Education	Law
Natural Sciences	0.345	0.323	0.124	0.072	0.071	0.066
Humanities	0.076	0.121	0.230	0.149	0.231	0.194
Engineering	0.399	0.200	0.191	0.076	0.069	0.065
Social Sciences	0.095	0.145	0.246	0.192	0.131	0.191
Economics	0.058	0.078	0.512	0.159	0.064	0.129
Public Policy	0.055	0.116	0.229	0.320	0.077	0.203

Since it is important for the rest of our analysis that these expectations reflect actual underlying beliefs, we attempt to assess how "reasonable" they are by com-

[9] The survey design was such that the default values of the subjective probabilities were set equal to zero for all occupation-major combinations. As a result, it might be that some of the zero probabilities observed in the data reflect missing probabilities rather than "true" zeros. However, in the former case, it seems likely that the latent (unobserved) probabilities are close to zero, so that aggregating these two types of zero probabilities should not be a concern.

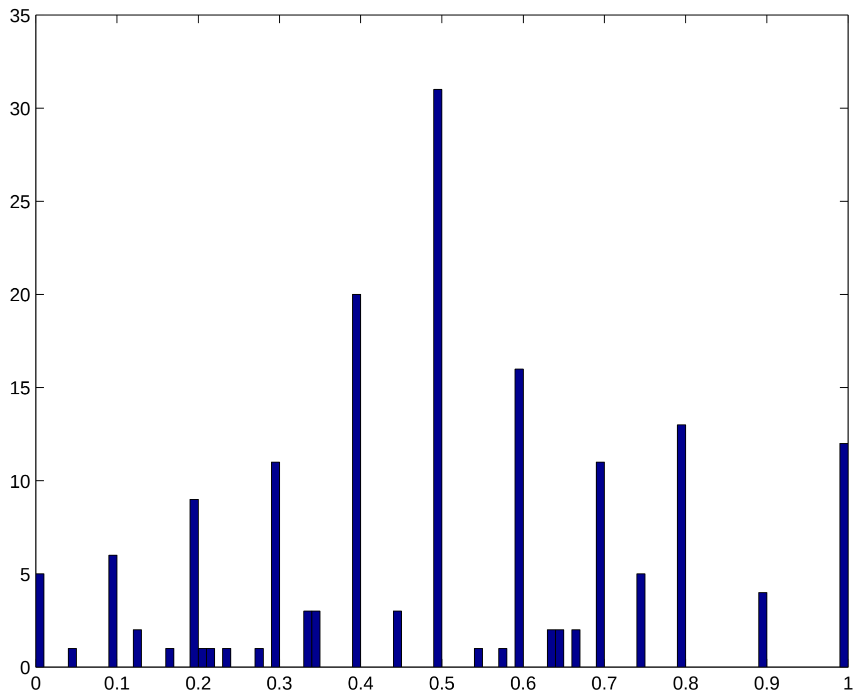

Figure 4.1: Distribution of Subjective Probabilities (Economics Major, Business Occupation)

Table 4.4: Incidence of Elicited Zero Probabilities of Choosing Occupations, Conditional on Major

	Occupation:					
Major:	Science	Health	Business	Government	Education	Law
Natural Sciences	3.57%	7.14%	27.98%	35.50%	39.29%	43.45%
Humanities	48.81%	34.52%	13.69%	18.45%	17.26%	16.67%
Engineering	7.14%	22.02%	20.83%	45.24%	47.02%	50.60%
Social Sciences	45.24%	30.95%	10.12%	13.10%	25.00%	17.86%
Economics	53.57%	49.40%	2.38%	17.26%	45.24%	28.57%
Public Policy	55.36%	36.31%	13.10%	3.57%	38.69%	11.90%

paring them with data form the American Community Survey (ACS).[10] From this set of comparisons, we can see where Duke students believe they rank relative to the

[10] We note in this survey, we also elicited students' expectations over first year salaries. Conditional on chosen majors, these expectations matched well with data on actual salaries of undergraduates one-year out collected by Duke's career office.

population of college graduates who actually chose particular major-occupation combinations. We utilize data from the 2009-2011 ACS which contains data on wages, college major and current occupation. We limit the ACS sample to males between the ages of 29 and 35[11] with a reported major field for their college degree. Majors in the ACS were categorized similarly to the Duke data. Several majors in the ACS are not offered at Duke; to the extent they clearly fell into a major category, they were included.[12] To construct occupations, matches between the occupations categories in the ACS and the occupation groupings in the Duke data were constructed.[13]

To compare the ACS to the Duke expected earnings, we estimated the following regression:

$$\ln(w_{ij}) = \alpha_j + \beta age_i + \nu_{ij}, \tag{4.1}$$

where w_{ij} is the wage of person i with major j and α_j is a vector of dummy variables for each major j. This regression was estimated separately for each occupation. The regression results were then used to compute the average log wage at age 32 for each occupation conditional on major. The variance of the distribution of log wages was calculated from the regression residuals, enabling the comparison of the ACS income and Duke expected income distributions.

4.3 Ex Ante Treatment Effects

In this section we outline the different types of *ex ante* treatment effects we are interested in, and discuss how each of these effects can be estimated using our subjective expectations data. We further discuss the estimation of the distributions of

[11] The Duke respondents, on average, would be of age 32 ten years after graduation.

[12] Most of the excluded majors were health services majors or vocational majors such as construction services.

[13] Science, computing, and engineering classifications were coded as science and technology careers; medicine was coded as health careers; business and finance was coded as business career; education was coded as education careers; legal was coded as law careers. Workers classified as nonprofit works or local, state or federal employees were coded as government/nonprofit.

Table 4.5: Percentile of the ACS for the Median Duke Student Conditional on Chosen Major

	Occupation:					
Major:	Science	Health	Business	Government	Education	Law
Natural Sciences	87.61%	91.33%	93.33%	90.06%	87.07%	79.26%
Humanities	80.06%	90.31%	82.44%	83.58%	78.79%	68.25%
Engineering	58.08%	91.82%	68.29%	69.01%	76.03%	55.76%
Social Sciences	91.24%	94.82%	98.37%	86.68%	78.79%	56.29%
Economics	72.45%	93.68%	73.33%	70.23%	52.05%	79.46%
Public Policy	73.86%	85.52%	87.01%	70.58%	73.94%	78.40%

the *ex ante* treatment effects within the overall population, as well as "treated" and "untreated" subpopulations.

We define the *ex ante* treatment effects for particular occupations relative to careers in Education, which is chosen as our baseline occupation[14] and which we label as occupation $k = 1$. We calculate the *ex ante* treatment on the *treated* for any given occupation $k \in \{2, 3, 4, 5, 6\}$, denoted by $TT(k)$, by weighting the differences in the reported earnings between occupation k and the baseline occupation by the probability the individual reports that he will work in occupation k 10 years after graduation (over the average declared probability of working in occupation k). Namely:

$$TT(k) = \frac{\sum_i \sum_j' I(d_i = j')p_{ij'k}\left[w_{ij'k} - w_{ij'1}\right]}{\sum_i \sum_{j'} I(d_i = j')p_{ij'k}}, \tag{4.2}$$

where $p_{ij'k}$ is the probability declared by individual i of choosing occupation k given major j', $I(d_i = j')$ is an indicator for whether i chose major j', and $w_{ij'k}$ the earnings expected by individual i in occupation k given major j'. Thus, $TT(k)$ characterizes the average *ex ante* treatment on the treated effect taken over the population distribution of chosen majors.

Similarly, we compute the *ex ante* treatment on the *untreated* for occupation k

[14] We choose to use Education as the baseline occupation because it appears to be is less tied to particular majors compared to other occupations. See Table 4.3.

as:

$$TUT(k) = \frac{\sum_i \sum_j' I(d_i = j')(1 - p_{ij'k}) \left[w_{ij'k} - w_{ij'1}\right]}{\sum_i \sum_{j'} I(d_i = j')(1 - p_{ij'k})} \tag{4.3}$$

Note that the treated or untreated status for the effects defined in (4.2) and (4.3), respectively, are not based on actual occupational choices, which we do not observe, but on whether we use students' elicited probabilities of choosing or not choosing the various occupations.

Finally, the average *ex ante* treatment effect is given by:

$$ATE(k) = \frac{\sum_i \sum_{j'} I(d_i = j') \left[w_{ij'k} - w_{ij'1}\right]}{N}, \tag{4.4}$$

where N is the sample size.

We also can calculate the occupation *ex ante* treatment effects *conditional* on each of the particular majors chosen. More precisely, the *ex ante* treatment on the treated, treatment on the untreated and average *ex ante* treatment effect for each chosen major j is given by:

$$TT(k|j) = \frac{\sum_i I(d_i = j) p_{ijk} \left[w_{ijk} - w_{ij1}\right]}{\sum_i I(d_i = j) p_{ijk}}, \tag{4.5}$$

$$TUT(k|j) = \frac{\sum_i I(d_i = j)(1 - p_{ijk}) \left[w_{ijk} - w_{ij1}\right]}{\sum_i I(d_i = j)(1 - p_{ijk})}, \tag{4.6}$$

$$ATE(k|j) = \frac{\sum_i I(d_i = j) \left[w_{ijk} - w_{ij1}\right]}{\sum_i I(d_i = j)}. \tag{4.7}$$

Moreover, given that we also elicit the subjective expectations for *counterfactual majors*, we can compute the *ex ante* treatment effects for those who did *not* choose major j by replacing $I(d_i = j)$ with $I(d_i \neq j)$.

Our data allows us to generate no only average effects, but also estimate the *distributions* of the *ex ante* treatment effects of working in any given occupation

k, relative to education as the baseline occupation, conditional on students' initial college major choices. We can estimate distributions for three different subgroups of interest, namely (i) the overall population, (ii) the treated subpopulation, and (iii) the untreated subpopulation. We briefly sketch the steps involved in their estimation.

First, the density of the distribution of the *ex ante* treatment effects on the overall population can be simply estimated with a kernel density estimator, using the fact that we have direct measures of the *ex ante* treatment effects for occupation k, $k = 2, ..., 6$, for each student in our sample. We denote the resulting density by $f_{TE,k}(.)$ and its estimator by $\widehat{f_{TE,k}(.)}$.

Second, it follows from Bayes' rule that we can estimate the density of the distribution of the *ex ante* treatment effects on the treated subpopulation, denoted by $f_{TE,k}^{Treated}(.)$ for any scalar u as follows:

$$\widehat{f_{TE,k}^{Treated}}(u) \quad = \quad \frac{\widehat{f_{TE,k}(u)} \times E(\sum_{j'} I(\widehat{d_i = j')p}_{ij'k}|TE = u)}{1/N \times \sum_i \sum_{j'} I(d_i = j')p_{ij'k}}. \tag{4.8}$$

The conditional expectation term above can be simply estimated using a Nadaraya-Watson nonparametric regression estimator.

Finally, we note that the distribution of the *ex ante* treatment effects on the untreated can be estimated by replacing $p_{ij'k}$ with $(1 - p_{ij'k})$ in (4.8).

4.4 Results: Ex Ante Treatment Effects

4.4.1 Occupation Treatment Effects

Table 4.6 provides estimates of the three *ex ante* treatment effects of occupations on earnings 10 years after graduation which correspond to the formulas in (3.1)-(3.3) in Section 4.3. Relative to education as the baseline occupation, the average *ex ante* treatment effects range from \$22,542 for science (30.6% of the mean expected earnings in education) to as much as \$90,066 in law (122.3% of the mean expected earnings in education). Health, business and law careers all have very large earnings premia of over 94%, while those entering a science or government occupation expect a much smaller premium of 30.6% to 35.7% ten years after graduation. Consistent with sorting across occupations being partly based on comparative advantages, the *ex ante* treatment on the untreated effects show that, for each occupations, the untreated anticipate lower premia than the treated. The difference is particularly large for health occupations, which is almost two times smaller for those not anticipating a career in health compared to those who plan to enter a health related occupation. Interestingly, these sorting effects are much weaker for science careers, where the untreated anticipate to earn 70% as much than the treated, and are negligible for government careers.

But, as noted in Section 4.3, our data on elicited expectations provides substantially more than just average effects. Namely, we can plot the full distributions of the treatment on the treated and the treatment on the untreated. Figure 4.4.2, 4.3, and 4.4 plot the full distributions for government, health, and business occupations, respectively. Each of the figures shows a different pattern of selection. For government, the distributions for the treated and the untreated are essentially the same: there is little role for selection into government jobs, at least relative to education. For health, the treated distribution is to the right of the untreated distribution, suggesting substantial selection. For business careers, while there appears to be significant selection at the bottom end of the distribution, it is much less at the top end. This latter pattern suggests that there is a sizable number of students would do quite well in business if they were to choose such a career, but these same people expect they would do well in education, suggesting that factors other than expected earnings influence students' occupational choices. Overall, these results suggest that there is much more to the distributions of *ex ante* treatment effects than just their means.

4.4.2 Occupation Treatment Effects Conditional on Major

In Table 4.7, we present treatment effects $(TT(k), TUT(k), ATE(k))$ conditional on students' chosen majors. There is a substantial amount of heterogeneity in the expected earnings premium for a given occupation across majors. Notably, natural science majors expect on average a \$136,450 premium for a health career relative to education, which is more than five times larger than the \$24,670 premium expected by public policy majors who anticipate to enter this type of occupation. Examining some of the other average *ex ante* treatment effects, economics majors have the highest premium for business occupations, while engineering and natural science majors have the highest premia for science careers. Overall, these patterns are consistent with certain majors being closely tied to specific occupations. In particular, the major-occupation pairs that are typically thought of as being closely related to one another – such as economics and business, science and health, and engineering or natural science and science occupations – do have the highest premia. These patterns are consistent with the accumulation of occupation-specific human capital within each major; they are also consistent with a form of selectivity in choice of major, i.e., individuals who expect to be more productive in health are more likely to choose a science major.

Ex ante treatment effects on the untreated by student's major are still generally lower than the treatment on the treated effects. There are however, a couple of exceptions. For instance, science careers have higher effects on the untreated in social science majors, while government careers have a higher effect on the untreated in the humanities and social sciences. The difference between the *ex ante* treatment on the treated and the *ex ante* treatment on the untreated effects provides an interesting measure of the importance of selection on the expected differences in occupation-major premia. For a majority of occupation-major pairs, this difference is positive, consistent with selective sorting on expected earnings in different occupations, but the differences are quantitatively small. But, selection into legal careers by social sciences majors explains more than 40% of the major-occupation premium. And, while selection effects into government are, on average, virtually nonexistent across all majors, selection turns out to explain a large share of the earnings premia for science majors (around 50%).

Finally, Table 4.8 provides estimates of the three *ex ante* treatment effects by counterfactual non-chosen major. The treatment on the treated effects are again generally larger than the treatment on the untreated, with a few exceptions: engineering and economics majors with science careers, government occupations with economics and public policy majors, and law with humanities and public policy. It is worth noting that these *ex ante* treatment effects also exhibit a substantial degree of heterogeneity across majors. Notably, expected premia for business (relative to education) careers are higher for economics majors, while returns to science careers are higher for engineering and natural science majors. The fact that these types of complementarities between majors and occupations still hold when focusing on the majors which were *not* chosen by the individuals points to the accumulation of occupation-specific human capital within majors.[15]

Table 4.6: *Ex Ante* Treatment Effects of Occupations (Annual Earnings, in dollars)

Occupation	TT	TUT	ATE	ATE share of Education income
Science	30,040	20,903	22,542	30.6%
Health	117,770	59,241	69,556	94.4%
Business	101,720	83,740	88,562	120.2%
Government	26,740	26,214	26,282	35.7%
Law	116,590	85,159	90,066	122.3%

[15] See also Kinsler and Pavan (2012) on the importance of major-specific human capital. They find, using data from the Baccalaureate and Beyond Longitudinal Study, that individuals have higher wages when working in an occupation related to one's field of study compared to working in non-related occupations.

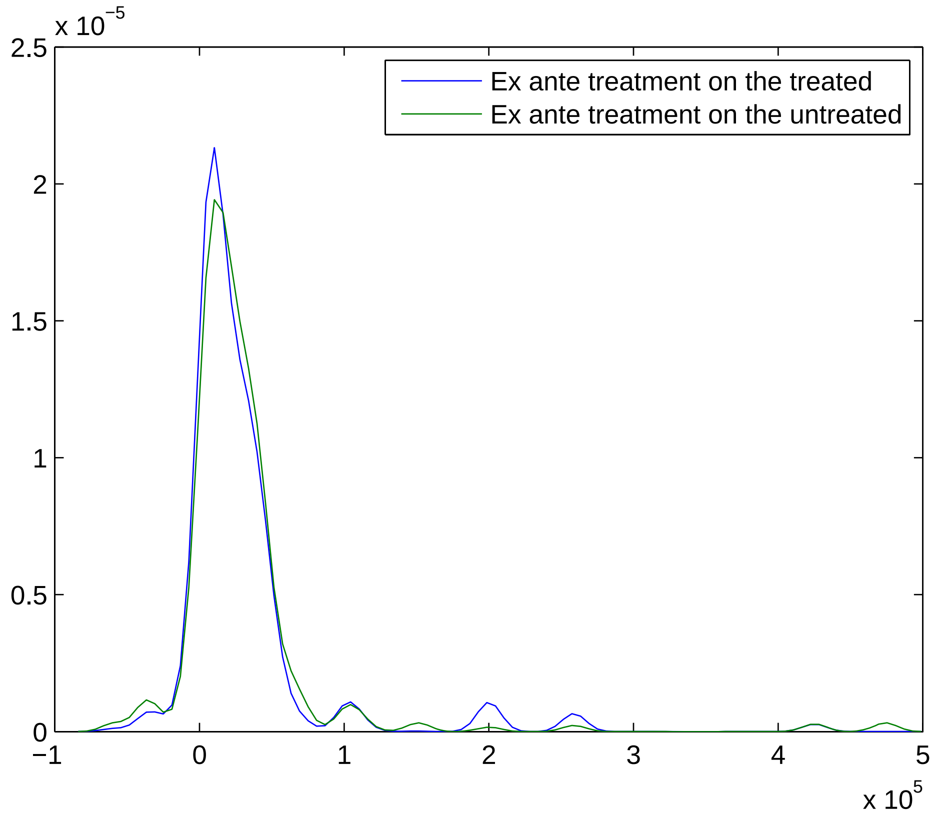

FIGURE 4.2: Distribution of *Ex Ante* Treatment Effects: Government (Annual Earnings, in dollars)

4.5 An Estimable Occupational Choice Model Using Subjective Expectations Data

In this section, we layout a model of occupational choice that can be estimated with elicited data on *ex ante* expected earnings and expected probabilities of choosing alternative occupations. The choice of an occupation is characterized in three stages. First, an individual enrolls in a given college major. Second, upon completing one's major, the individual decides whether or not to acquire more information about the value of a set of particular occupations. Finally, after receiving this information about selected occupations, the individual makes a one-time decision regarding his occupation.

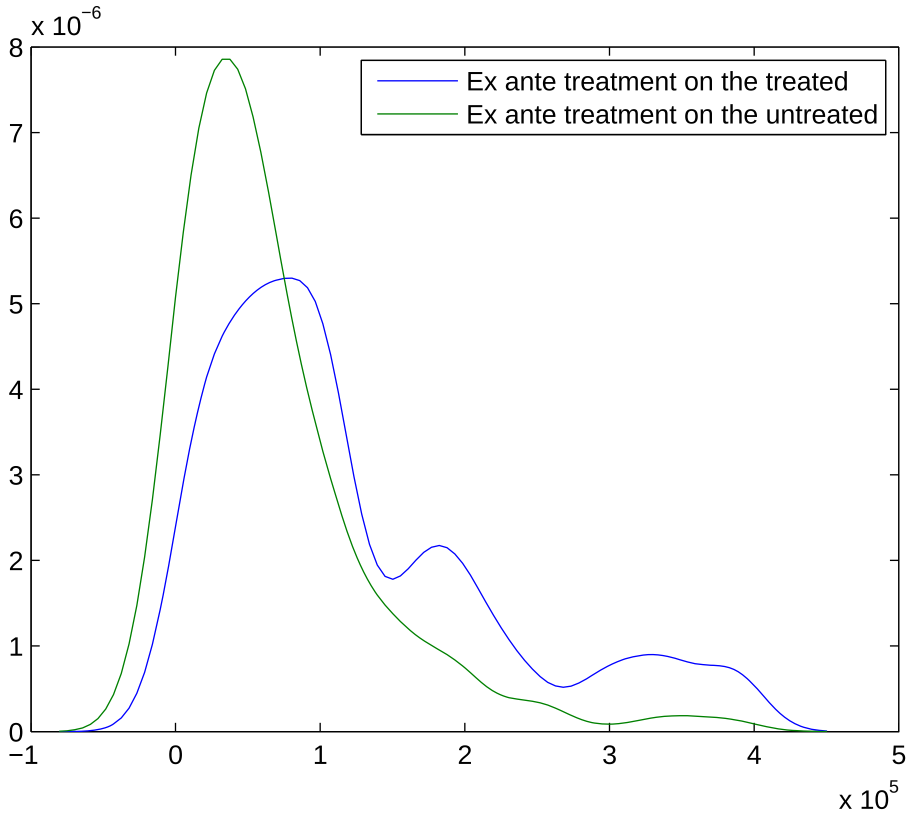

FIGURE 4.3: Distribution of *Ex Ante* Treatment Effects: Health (Annual Earnings, in dollars)

4.5.1 Choice of Occupation

We begin by examining the last decision, namely the choice of occupation conditional on major and paying the information cost for a subset of the occupations.[16] Let v_{ijk} denote the expected present value of lifetime utility for individual i from choosing occupation k conditional on major j, *before* the realization of the information shock. Individuals form their subjective expectations regarding the probabilities of entering different careers based on these *ex ante* value functions. The new information consists of a vector of shocks ϵ_{ijk} that vary at the individual-major-occupation level. For any given major j, we assume that the ϵ_{ijk}'s are independent draws from a Type 1 extreme

[16] In practice, the information cost can be thought of as a cost of application (per occupation).

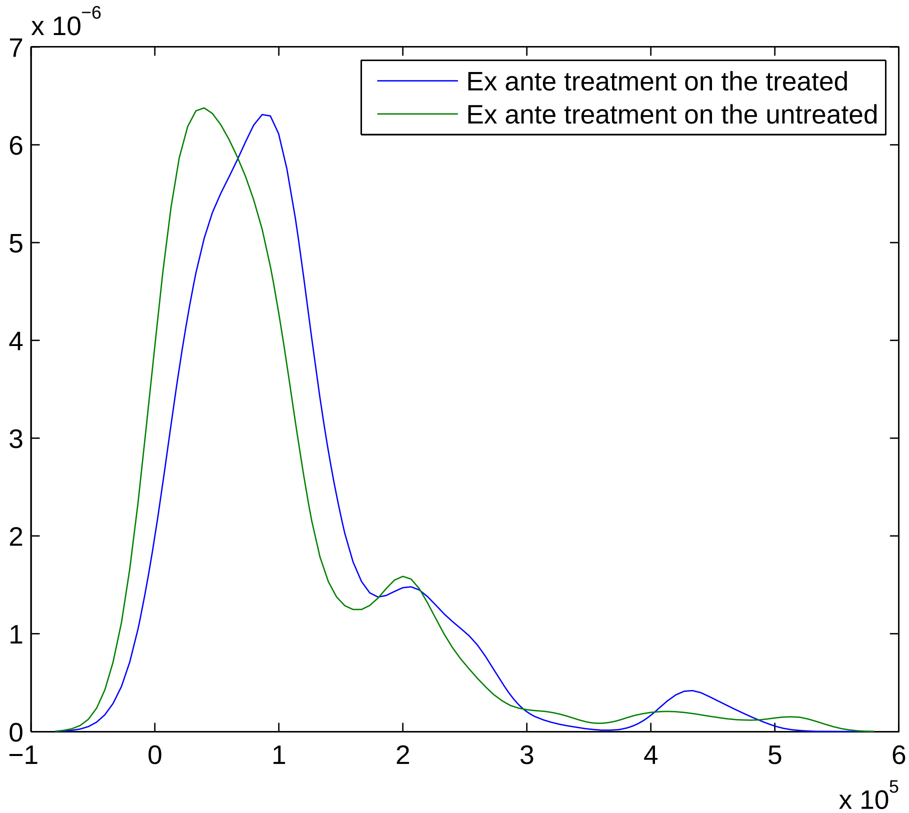

FIGURE 4.4: Distribution of *Ex Ante* Treatment Effects: Business (Annual Earnings, in dollars)

value distribution. After making an initial major choice and graduating from college, these shocks are realized and the individual then proceeds to choose an occupation. An individual who chose major j then chooses his occupation k^* according to:

$$k^* = \arg\max_{k \in K^*_{ij}} (v_{ijk} + \epsilon_{ijk}) \tag{4.9}$$

where K^*_{ij} is the set of occupations where the individual has paid for the new information conditional on an initial major j. We will discuss the decision to acquire more information about particular occupations in Subsection 4.5.3.

Table 4.7: Heterogeneous *Ex Ante* Treatment Effects of Occupations by Chosen Major (Annual Earnings, in dollars)

		Chosen Major:					
Occupation:		Economics	Engineering	Humanities	Natural Sciences	Public Policy	Social Sciences
Science	TT	18,590	39,330	17,320	28,840	25,840	14,630
	TUT	17,680	27,420	6,460	36,040	17,030	19,600
	ATE	17,750	31,980	7,040	33,710	17,280	18,970
Health	TT	89,740	84,090	53,970	82,780	38,620	69,140
	TUT	60,440	57,480	59,170	106,830	23,740	55,750
	ATE	62,940	62,450	58,380	136,450	24,670	57,770
Business	TT	120,430	71,990	66,120	112,070	94,240	92,630
	TUT	120,450	70,810	56,640	107,140	67,580	75,490
	ATE	120,440	71,070	57,880	107,580	74,580	79,290
Government	TT	26,740	11,310	16,250	66,660	31,200	16,750
	TUT	25,770	11,820	23,880	33,670	25,440	36,310
	ATE	25,880	11,790	22,810	35,320	27,290	33,650
Law	TT	91,590	57,730	94,930	116,580	174,810	114,270
	TUT	93,630	67,550	62,090	88,930	138,780	63,000
	ATE	93,380	66,720	70,690	90,160	148,330	75,320

Table 4.8: Heterogeneous Ex Ante Treatment Effects by Counterfactual Major (Annual Earnings, in dollars)

		Chosen Major:					
Occupation:		Economics	Engineering	Humanities	Natural Sciences	Public Policy	Social Sciences
Science	TT	6,660	42,960	19,250	35,860	18,560	12,070
	TUT	10,344	47,353	10,378	32,540	17,999	13,708
	ATE	10,140	45,580	11,090	33,700	18,030	13,560
Health	TT	63,330	108,620	88,130	87,000	73,350	74,080
	TUT	49,949	77,177	51,290	78,234	57,628	51,953
	ATE	51,000	83,570	55,650	80,930	59,620	55,170
Business	TT	130,560	88,140	67,360	62,780	100,270	94,280
	TUT	98,567	80,886	58,376	56,942	84,208	63,424
	ATE	114,390	82,220	60,540	57,710	87,800	71,200
Government	TT	20,160	28,550	24,370	24,900	28,760	34,370
	TUT	24,453	25,163	19,383	18,862	35,450	20,232
	ATE	23,720	25,440	20,140	19,340	33,310	23,120
Law	TT	88,460	111,330	78,380	80,020	78,150	78,460
	TUT	78,666	98,807	78,593	68,761	87,613	82,883
	ATE	79,960	99,600	78,550	69,580	85,790	82,080

4.5.2 Linking Subjective Probabilities to Occupation-Major Preferences

An individual's self-reports of the probabilities of choosing particular occupations can then be used to recover their expected utilities (up to a reference alternative). To see this, first consider the case where it is optimal for the individual to pay the informational cost for all occupations conditional on major j. With the Type 1 extreme value assumption on the ϵ_{ijk}'s, we can recover the difference in conditional value functions by inverting the choice probabilities following Hotz and Miller (1993) and Berry (1994):

$$\ln(p_{ijk}) - \ln(p_{ij1}) = v_{ijk} - v_{ij1} \tag{4.10}$$

We assume that the conditional value functions, for any given major j and occupation k, can be written as follows:

$$v_{ijk} = \alpha_{ik} + \delta_{jk} + \gamma_w \ln w_{ijk} + \eta_{ijk}$$

where α_{ik} is the preference i has for occupation k, δ_{jk} captures the average complementarity of preferences between major j and occupation k, w_{ijk} is the expected earnings measure for i under choices $\{j, k\}$, and η_{ijk} is an orthogonal preference term for occupation k given major j.[17] Similarly to Arcidiacono (2004, 2005) in the context of college major choice, the value function is assumed to depend on future labor market outcomes through the logarithm of the expected earnings.[18]

Taking the difference with respect to the baseline occupation, it follows that the

[17] In practice, monetary or psychic costs of schooling associated with the occupations which typically require an advanced degree, such as law, would also be captured by these preference terms. It follows that our empirical strategy will presumably lead to underestimate the true preferences for those specific occupations.

[18] While forward-looking individuals should consider the present value of lifetime earnings associated with each occupation, in practice we only observe the expected earnings ten years after graduation. However, we show in Appendix B that, under some plausible assumptions on the discount factor, worklife duration and earnings growth, the earnings ten years out are, up to a constant, a reasonably good approximation of the present value of lifetime earnings.

following equality holds:

$$\ln(p_{ijk}) - \ln(p_{ij1}) = (\alpha_{ik} - \alpha_{i1}) + (\delta_{jk} - \delta_{j1}) + \gamma_w(\ln w_{ijk} - \ln w_{ij1}) + \zeta_{ijk} \quad (4.11)$$

where $\zeta_{ijk} \equiv \eta_{ijk} - \eta_{ij1}$.

4.5.3 Information Costs

We now consider the information acquisition stage. Note that this stage arises because the subjective probability of some choices (conditional on a particular major) are zero. With the information having continuous support, a subjective probability of zero would not be possible if the information was costless. However, if the individual can choose whether or not to acquire the information, zero probabilities can result.

The decision to acquire information hinges on expectations of the maximal utility associated with different choice sets. Given the Type-1 Extreme Value assumptions regarding the distribution of the ϵ's, McFadden (1978) showed that the expected maximum utility for any choice set K, $V_{ij}^{(K)}$, can be written as:

$$V_{ij}^{(K)} = \ln\left[\sum_{k\in K} \exp(v_{ijk})\right] + \gamma$$

where γ is Euler's constant.

Without loss of generality, denote v_{ij1} as the payoff associated with the career that gives the highest utility prior to the new information, denote v_{ij2} as the utility associated with the next highest, etc. We denote the utility cost of obtaining information on a particular occupation-major pair as c. Individuals only obtain information if the expected gain is high enough to overcome the cost. Conditional on paying the information cost for the first $(k-1)$ occupations, information on career k (the kth highest payoff) is obtained when:[19]

[19] Note that, at the individual level, it is always optimal to consider the occupations in this order.

$$
\begin{aligned}
c &\leqslant \ln\left(\sum_{k'=1}^{k}(\exp(v_{ijk'}))\right) - \ln\left(\sum_{k'=1}^{k-1}(\exp(v_{ijk'}))\right) \\
&\leqslant \ln\left(\frac{\sum_{k'=1}^{k}(\exp(v_{ijk'}))}{\sum_{k'=1}^{k-1}(\exp(v_{ijk'}))}\right) = -\ln(1-p_{ijk}) \qquad (4.12)
\end{aligned}
$$

We can then get an upper bound estimate of c from the lowest positive self-reported probability of choosing an occupation conditional on a major.

4.5.4 Selection

Reports of zero probabilities can not be ignored in estimation because of the selection problem: those who report zero probabilities have particularly low values for those occupation-major pairs. Now suppose that k is not in the set K^*_{ij}. In this case the inequality in (4.12) is flipped:

$$
c > -\ln(1-p_{ijk}) \qquad (4.13)
$$

Note that the p_{ijk} term in (4.13) is *conditional* on k being in the choice set. Since k was not in the choice set (the information cost was not paid), we have no measure of p_{ijk}. However, we can substitute in for (4.13) with the relevant v_{ijk}'s where the choice set is now $K^*_{ij} \cup \{k\}$:

$$
\begin{aligned}
c &> -\ln\left(1 - \frac{\exp(v_{ijk})}{\exp(v_{ijk}) + \sum_{k'\in K^*_{ij}}\exp(v_{ijk'})}\right) \qquad (4.14) \\
&> -\ln\left(1 - \frac{\exp(v_{ijk}-v_{ij1})}{\exp(v_{ijk}-v_{ij1}) + \sum_{k'\in K^*_{ij}}\exp(v_{ijk'}-v_{ij1})}\right) \qquad (4.15)
\end{aligned}
$$

We then need to solve this equation for $v_{ijk} - v_{ij1}$ as the other differenced conditional value functions are known from (4.10). Solving,

$$\begin{aligned}
\exp(-c) &< \left(\frac{\sum_{k' \in K^*_{ij}} \exp(v_{ijk'} - v_{ij1})}{\exp(v_{ijk} - v_{ij1}) + \sum_{k' \in K^*_{ij}} \exp(v_{ijk'} - v_{ij1})} \right) \\
\exp(v_{ijk} - v_{ij1}) &< \frac{(1 - \exp(-c)) \sum_{k' \in K^*_{ij}} \exp(v_{ijk'} - v_{ij1})}{\exp(-c)} \\
v_{ijk} - v_{ij1} &< \ln \left(\frac{(1 - \exp(-c)) \sum_{k' \in K^*_{ij}} \exp(v_{ijk'} - v_{ij1})}{\exp(-c)} \right) \equiv c^*_{ijk}
\end{aligned}$$

Up to now we have not needed to make a distributional assumption on the ζ_{ijk}'s. With zero probabilities, this is no longer the case. We assume that ζ_{ijk} is distributed i.i.d. $N(0, \sigma)$, implying that the log likelihood contribution in the zero probability case is:

$$\ln(p_{ijk} = 0) = \ln \Phi \left(\frac{c^*_{ijk} + (\alpha_{i1} - \alpha_{ik}) + (\delta_{j1} - \delta_{jk}) + \gamma_w (Y_{ij1} - Y_{ijk})}{\sigma} \right) \tag{4.16}$$

where Φ is the standard normal cdf.

4.5.5 Heterogeneous Information Sets

It may be that students have better information about the labor market for some majors than others. In particular, it may be the case that individuals have better information about the labor market in their own major than in counterfactual majors. The model we have developed can be relaxed to allow for counterfactual majors to have higher variances associated with the information shocks.

Absent additional assumptions, discrete choice models are only identified relative to the variance scale parameter. Implicit in (4.11) is a normalization of the variance scale parameter to one. With the structure we have placed on (4.11), we can allow for the variance parameter to be different for counterfactual majors. We then specify

(4.11) (without loss of generality) as:

$$\ln(p_{ijk}) - \ln(p_{ij1}) = \frac{(\alpha_{ik} - \alpha_{i1}) + (\delta_{jk} - \delta_{j1}) + \gamma_w(Y_{ijk} - Y_{ij1}) + \zeta_{ijk}}{1 + \phi I(d_i = j)} \tag{4.17}$$

If ϕ is greater than zero, then students are less certain about outcomes in counterfactual majors than they are in their own majors.

4.5.6 Compensating Differentials

Our specification of the payoffs for major-occupation bundles allows us to recover individual-level preferences for occupation k relative to occupation 1, $\alpha_{ik} - \alpha_{i1}$, as well as estimates of the average preferences for occupation k relative to occupation 1 conditional on major j, $\delta_{jk} - \delta_{j1}$. We can translate this into monetary units using the expected earnings coefficient γ, thus translating those parameters into (expected) compensating differentials for the different occupations (given each college major).

Of key interest here is the average compensating differential for occupation k relative to occupation 1, conditional on major j, which is given by:

$$CD(k|j) = \frac{\delta_{j1} - \delta_{jk}}{\gamma_w} \tag{4.18}$$

Furthermore, using the estimates of the parameters $\alpha_{ik} - \alpha_{i1}$, we can also see how compensating differentials for each occupation vary across individuals. In particular, similar to the *ex ante* treatment effects parameters that we have estimated (namely ATE, TT and TUT), we can compute, for each occupation k, the average compensating differential, the average compensating differential conditional on choosing occupation k, as well as the average compensating differential conditional on not choosing occupation k. For example, the additional compensating differential for occupation k relative to occupation 1 for those who chose major j is:

$$CD(k|d_j = 1) = \frac{\sum_i I(d_{ij'} = 1)\alpha_{ik}}{\gamma_w \sum_i I(d_{ij'} = 1)} - \frac{\sum_i I(d_{ij} = 1)\alpha_{ik}}{\gamma_w \sum_i I(d_{ij} = 1)} \tag{4.19}$$

4.6 Results: Compensating Differentials

Estimates of the earning parameter, γ, for different specification of the conditional valuation functions are given in Table 4.9. For our earnings measure, we use the log of expected earnings ten years after graduation. Hence, when discussing compensating differentials, they will be percentage increases in earnings ten years out. For each of the specifications, log expected earnings are statistically significant.

The final column allows the variance on the new information to be different for counterfactual majors. The coefficient estimate for ϕ was small and insignificant and we can not reject that it is zero. Note that this specification is adding the flexibility in the variance after controlling for individual occupation dummies. In estimates not reported here, the variance for counterfactual majors was higher and statistically significant if we allowed for different variances in Models 1 and 2. Given these results, we focus on Model 3 as our preferred specification.

To assess the extent to which expected earnings affects occupational choice, we can calculate the percentage change in the probability of choosing an occupation given a percentage change in earnings. At the intensive margin, the elasticity formula for our specification is (see Train, 2003):

$$\eta_{ijk} = (1 - Pr_{ijk})\gamma_w$$

For those on the intensive margin, the subjective probabilities of entering a given career conditional on a given major range from 0.003 to 0.962, yielding elasticities from zero to 0.64 for our preferred specification (Model 3). Taking the major from the data as given, we can estimate the population elasticity of occupation k using:

$$\hat{\eta_k} = \frac{\sum_i \sum_j I(j|i)(1 - Pr_{ijk})\gamma_w}{N}$$

These occupation-specific elasticities range from 0.49 (for business) to 0.60 (for education), resulting in a mean elasticity across all occupations equal to 0.55.

Table 4.9: Structural Model Estimates

	Model 1	Model 2	Model 3	Model 4
Log expected earnings 10 years out	1.252	0.617	0.664	0.668
	(0.027)	(0.020)	(0.017)	(0.017)
Occupation dummies	yes	no	no	no
Occupation-major dummies	no	yes	yes	yes
Individual occupation dummies	no	no	yes	yes
Better information in own major	no	no	no	yes
Log likelihood (000's)	-18.47	-14.03	-6.466	-6.466

4.6.1 Compensating Differentials

We next report how compensating differentials for particular occupations vary among those who chose particular majors using equation (4.19). All of the heterogeneity in compensating differential is relative to the education occupation. Note that the average compensating differential in the population is not present here because it is captured by the δ_{jk}'s.

Table 4.10 gives the results with the units reported as percentage changes in expected earnings ten years out to make the average individual of a particular major indifferent between the two occupations, all else equal. Economics majors and public policy majors have strong preferences to avoid the education occupation relative to the average Duke student and strongly prefer business and government occupations relative to other majors. On the other hand, natural science majors, social science majors, and humanities majors prefer education over business.

The estimates of the individual preferences for occupations also allow us to examine their correlation patterns. Table 4.11 gives the variance of the occupation-specific preferences while the off-diagonal elements give the correlation coefficients. Preferences for business and law tend to be negatively associated with preferences for education, resulting in particularly high correlation coefficients between business

Table 4.10: Heterogeneity in Compensating Differentials by Chosen Major Relative to Education

	Science	Health	Business	Government	Law
Natural Science	0.1%	4.6%	105.1%	48.7%	138.5%
Humanities	100.0%	57.1%	110.3%	73.7%	57.0%
Engineering	15.1%	64.1%	-21.6%	-10.9%	-15.3%
Social Science	26.7%	13.6%	61.2%	56.6%	-26.9%
Economics	-83.2%	-117.7%	-130.5%	-62.2%	-56.6%
Public Policy	-31.1%	5.0%	-111.7%	-137.8%	-123.6%

and law with each other as well as with health and, to a lesser extent, government.

Table 4.11: Variances and Correlation Coefficients for Occupation-Specific Preferences

	Science	Health	Business	Government	Law
Science	1.837	0.215	0.149	-0.134	0.178
Health	0.215	2.55	0.607	-0.037	0.569
Business	0.149	0.607	2.235	0.373	0.681
Government	-0.134	-0.037	0.373	2.543	0.329
Law	0.178	0.569	0.681	0.329	3.258

4.6.2 Major-Specific Compensating Differentials

We next examine how compensating differentials are affected by major, translating our estimates of the δ_{jk}'s into percentage increases in earnings. Table 4.12 reports average compensating differentials for particular occupation-major combinations, again relative to the education occupation. Although the signs are all intuitive, the magnitudes are such that there is likely more to the story than just compensating differentials. For example, an economics major makes working in business so attractive that on average individuals would need to make over three times as much in education (or making less than a third of what they would make in business) to be indifferent between the two occupations. Similarly, a science major makes working in a science occupation so attractive that on average individuals would need to make over two and a half times more in education to be indifferent between the two occupations.[20]

The final column of Model 3 reports what the compensating differentials would need to be if we did not account for differences in earnings. In this case, a coefficient on earnings is therefore not estimated and we use the coefficient from Model 3 with earnings to perform the calculations. Comparing the last two columns of Table 4.12 then allows use to see the role earnings play in mitigating compensating differentials. As expected, the compensating differentials in the last column are all higher (in absolute value) than those when earnings are accounted for, as expected earnings in education are substantially lower than in other occupations. Not accounting for those earnings differences would make it appear as though education was even more unattractive than it actually was.

4.6.3 Search Frictions

What can explain the very large estimates of the compensating differentials? One explanation is that the average differences in compensating differential across majors is partly driven by search frictions. That is, being an economics major does not make business occupations more attractive beyond the salary gains but the arrival rate of offers in the business occupations is higher if the individual is an economics major.

To illustrate how search frictions will affect our estimates of compensating differentials, consider a simple case where there are two occupations, $k \in \{1, 2\}$. Suppose for major j individuals are given one offer in occupation 1 but two offers in occupation 2. The difference between the two offers in occupation 2 comes solely through the non-pecuniary shocks, not through income. If the non-pecuniary shocks are treated as just another extreme value shock, then the probability of choosing occupation 2

[20] It is interesting to note that these findings are in line with the literature on major choice, which tends to find that preferences play a key role in this decision (see, e.g., Arcidiacono, 2004, Beffy et al., 2012, and Wiswall and Zafar, 2012) .

Table 4.12: Average Compensating Differentials by Major-Occupation Pairs Relative to Education

Major	Occupation	Model 2	Model 3	Model 3 w/o Earnings
Natural Science	Science	-258%	-279%	-325%
	Health	-197%	-215%	-296%
	Business	-35%	-66%	-129%
	Government	11%	-33%	-61%
	Law	78%	56%	-2%
Humanities	Science	131%	117%	93%
	Health	121%	138%	83%
	Business	-29%	-11%	-66%
	Government	88%	48%	24%
	Law	182%	143%	87%
Engineering	Science	-298%	-306%	-358%
	Health	-130%	-136%	-209%
	Business	-152%	-151%	-221%
	Government	-15%	-52%	-77%
	Law	67%	44%	-11%
Social Science	Science	-51%	-30%	-59%
	Health	-43%	-6%	-62%
	Business	-166%	-118%	-184%
	Government	-95%	-105%	-136%
	Law	-5%	-17%	-81%
Economics	Science	-95%	-102%	-132%
	Health	-70%	-48%	-107%
	Business	-372%	-348%	-444%
	Government	-162%	-205%	-234%
	Law	-44%	-76%	-139%
Public Policy	Science	-75%	-65%	-98%
	Health	-140%	-90%	-148%
	Business	-247%	-197%	-271%
	Government	-284%	-280%	-321%
	Law	-111%	-129%	-198%

will be:

$$Pr(k=2|j) = \frac{2 * \exp(v_{j2})}{\exp(v_{j1}) + 2 * \exp(v_{j2})} = \frac{\exp(v_{j2} + \ln(2))}{\exp(v_{j1}) + \exp(v_{j2} + \ln(2))} \tag{4.20}$$

Hence, if offer rates for various occupations differ by major, then this will manifest itself as a compensating differential.[21]

We cannot separate compensating differentials from offer rates, but we can say how big differences in offer rates would have to be to explain the average compensating differentials we find for particular major-occupation combinations. Denote λ_{jk} as the arrival rate of offers for occupation k conditional on major j. We assume that the offers unobserved component is Type 1 extreme value: there is no correlation of offers within occupation categories. Allowing for correlation in this component within an occupation category would result in increases in magnitudes of the differences in arrival rates in order to account for the estimated differences in compensating differentials. Hence, one can think of our approach as identifying the *minimum* amount of differences in occupation-major arrival rates that account for the observed compensating differentials. Our estimates of $(\delta_{jk} - \delta_{j1})$ can be transformed into differences in arrival rates using:

$$\delta_{jk} - \delta_{j1} = \ln(\lambda_{jk}) - \ln(\lambda_{j1}) \tag{4.21}$$

Since we can only identify five of the six arrival rates for each major, we normalize λ_{j1} to one. Solving for λ_{jk} then gives the number of offers in occupation k per offer in education.

Results are presented in Table 4.13. In order for job offer rates to account for the estimated compensating differentials, natural science majors would have to receive at least 6.4 offers in science occupations and at least 1.5 offers in business for every one offer in education. In contrast, humanities majors would expect significantly fewer offers in the sciences, 0.5 offer for every offer in education, with roughly equal offers in business as in education. Majoring in economics would need to result in at least 10 offers in business for every offer in education to account for the compensat-

[21] Note that variance in earnings from which offers were drawn would also generate a similar result, but would require heterogeneity in the variance due to the major. Variance in offered wages would have to unreasonably different across majors to explain our results.

ing differential associated with the economics-business combination. These results, combined with those in Table 4.12, show that some combination of large differences in arrival rates occur due to one's major or one's major makes jobs in particular occupations much more enjoyable. In either event, majors have a substantial effect on the labor market outcomes beyond their impact on earnings.

Table 4.13: Number of Offers per Offer in Education Necessary to Account for Average Major-Occupation Compensating Differentials

	Occupation				
Major:	Science	Health	Business	Government	Law
Natural Science	6.38	4.17	1.55	1.24	0.69
Humanities	0.46	0.40	1.08	0.73	0.39
Engineering	7.63	2.47	2.73	1.41	0.75
Social Science	1.22	1.04	2.19	2.01	1.12
Economics	1.97	1.38	10.08	3.90	1.66
Public Policy	1.54	1.82	3.70	6.42	2.36

Note: Calculations from estimates of Model 3

4.7 Conclusion

This chapter shows how subjective expectation data on counterfactual outcomes can be used to recover the *ex ante* treatment effects as well as the non-pecuniary benefits associated with different treatments. We consider the particular context of sorting across occupations, using elicited beliefs from a sample of male undergraduates at Duke University on the probability of working in different occupations as well as the expected income in each of those occupations (10 years after graduation). Importantly, these beliefs were asked not only for the college major the individual chose, but also for counterfactual majors, thus making it possible to examine the heterogeneity across majors of the *ex ante* returns to different occupations and the subjective probabilities of working in any given occupation. This individual variation across counterfactual majors is key to tell apart the role of *ex ante* returns and preferences in the context of sorting across occupations. While sorting across occupations is found to be partly driven by the *ex ante* monetary returns, large differences in expected income across occupations remain after controlling for selection on monetary returns, which in turn points to the existence of substantial compensating differentials for particular occupations.

Appendix A

Assessing the Effect of School Days and Absences on Test Score Performance

A.1 Appendix

Table A.1: Baseline Regression: Alternative Sample

	Math Test Score	Reading Test Score
Days Absent	-0.0054***	-0.0029***
	(0.0002)	(0.0002)
Days of Class	0.0018***	0.0004
	(0.0005)	(0.0005)
Student FE	Yes	Yes
School FE	Yes	Yes
Teacher FE	Yes	Yes
Lagged Student Score	No	No
N	705,781	705,781

Source: NCERDC, 2006-2010, grades 3-5. Dependent variable is standardized by grade and year. All specifications include dummy variables for grade, year, and free/reduced-price lunch status. Bootstrapped standard errors are reported in parenthesis. Significance levels: $***$ denotes 1%; $**$ denotes 5%; $*$ denotes 10%.

Appendix B

Recovering Ex Ante Returns and Preferences for Occupations using Subjective Expectations Data

B.1 Appendix

We provide below some sufficient conditions under which the earnings ten years after graduation can be used to approximate the present value of lifetime earnings, for any major and occupation.

Specifically, we let w_{10} (respectively w_t) denote the earnings ten years out (resp. t years out), β the annual discount factor and T the worklife duration. We define the approximation error as $\Delta \equiv \left| w_{10} - \frac{\sum_{t=1}^{T} \beta^t w_t}{\sum_{t=1}^{T} \beta^t} \right|$. Individual, major and occupation subscripts are omitted to save on notations. Assuming that earnings grow at a constant rate ρ ($w_{t+1} = w_t \exp(\rho)$), it follows that the approximation error can be written as:

$$
\begin{aligned}
\Delta &= \left(\frac{w_{10}}{\sum_{t=1}^{T}\beta^t}\right)\left(\sum_{t=1}^{T}\beta^t\left(\exp((t-10)\rho)-1\right)\right) \\
&= \left|w_{10}\left(\exp(-10\rho)\frac{\sum_{t=1}^{T}\beta^t\exp(\rho t)}{\sum_{t=1}^{T}\beta^t}-1\right)\right|
\end{aligned}
$$

Setting $\beta = 0.9$, $\rho = 3\%$ and $T = 40$ years yields $\frac{\Delta}{w_{10}} \simeq 0.015$. It follows that, under those assumptions, the earnings ten years after graduation are reasonably close to the present value of lifetime earnings, weighted by the sum $\sum_{t=1}^{T}\beta^t$. The latter term does not vary across occupations and therefore drops out when taking the difference with respect to the baseline occupation (see Equation 4.11 in the main text).

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Teresa spent the subsequent three years as an Economic Policy Analyst at the Federal Reserve Bank of Boston before returning to graduate study at Duke University in 2008. She earned her M.A. in Economics in August 2009 and a Ph.D. in Economics as well as a Certificate in College Teaching from Duke University in 2014. After graduation, she will work as an Assistant Professor of Economics at Goucher College.