Essays on Higher Education and Job Matching

Dissertation

Presented in Partial Fulfillment of the Requirements for the Degree Doctor of Philosophy

in the Graduate School of The Ohio State University

By

Apoorva S. Rama

Graduate Program in Economics

The Ohio State University

2019

Dissertation Committee

Audrey Light, Advisor

Kurt Lavetti

Bruce Weinberg

Abstract

This dissertation is comprised of three chapters on education economics, focusing on college graduates transitioning into the labor market. In the first chapter, “Multidimensional Skill Mismatch among College Graduates,” I use college transcript data from a sample of college graduates in the 1997 National Longitudinal Survey of Youth (NLSY97) and occupational descriptors from the Occupation Information Network (O*NET) database to develop a novel “skill mismatch” index. This index measures the distance between a vector of skills acquired in college and a vector of skills required in the post-college occupation. By assessing various skill groups (mathematics, language, etc.), the skill mismatch index treats both workers and occupations as multidimensional entities. I provide evidence that the skill mismatch index is a refinement over previously developed empirical mismatch measures that rely on degree or college major to define mismatch.

In the second chapter, “Measuring the STEM Wage Premium Among College Graduates,” I estimate the wage benefits associated with training in Science, Technology, Engineering, and Mathematics (STEM), and assess the sensitivity of the STEM wage premium to changes in the way STEM is measured. Measuring STEM can differ in two ways: the definition of STEM (i.e., determining what fields are STEM) and incorporating STEM training into the empirical analysis with a dichotomous or continuous measure of

STEM training. Using a sample of college graduates with college transcript data in the NLSY97, I compare a total of six measures STEM training: for three different definitions of STEM (based on lists published by three different U.S. government agencies), I construct a continuous measure of STEM training (based on the amount of STEM coursework completed in college) and a dichotomous measure (based on if the worker completed a STEM major). Although the results confirm the general finding that there is a STEM wage premium, they demonstrate that estimates of that premium are relatively insensitive to the definition of STEM training but highly sensitive to whether a dichotomous or continuous measure is used.

In the third chapter, “Education and Job Matching: A Two Cohort Comparison,” I compare the incidence and log-wage penalty of overeducation and undereducation among two generations of college graduates. Mismatch is defined based on degree, where a worker is classified as overeducated (undereducated) is he completes a degree that is greater (less) than what is required by his occupation. Data for the older cohort (born 1957-1964) is from the NLSY79 and the younger cohort (born 1980-1984) from the NLSY97. By directly comparing these two cohorts, this study provides unique insight into how the estimated overeducation/undereducation wage penalty has changed over the last few decades. When conditioning on a rich array of observables, the estimated overeducation wage penalty during the early career is roughly the same for men and women in both the older and younger cohorts. The estimated returns to undereducation shift from a small, not significant effect for the older cohort to a substantial, significant effect for the younger cohort.

Acknowledgments

Many thanks to my adviser, Audrey Light, for patiently guiding me through this process and helping me develop as a researcher. I am forever grateful to Audrey for her friendship and for the valuable professional and personal advice she imparted during my time at The Ohio State University. The tools she provided me and the guidance she gave me will last a lifetime. I would also like to thank Kurt Lavetti for introducing me to health economics and for giving me the opportunity to be his research assistant; because of Kurt, I discovered (and was able to pursue) my interest in health economics. I am grateful to Bruce Weinberg for always being open to chat and for providing interesting insights into my research. I also appreciate Daeho Kim, for providing advice and thoughtful discussions during the early stages of my dissertation, and both Hajime Miyazaki and Ana Ramirez, for providing support during my time as a graduate student.

I am grateful to my family, friends, and colleagues for helping me through this process. A special thanks to Carol Kane for giving me my dream job and for serving as a mentor at work. Most importantly, I want to thank my parents and my sister for being my role models and providing me with the love, support, and wisdom I needed to get through this process. I know many of the opportunities I pursued were only available because of the hard work of my parents and I thank them for their endurance in creating a great life for me. This would not have been possible without them.

Vita

June 2012…………………….....B.S. Economics and Mathematics, Binghamton University

June 2012…………..B.S. Management concentration in Finance, Binghamton University

June 2013…………………………………....M.A. Economics, The Ohio State University

June 2017……………………………………..Economist, American Medical Association

Fields of Study

Major Field: Economics

Table of Contents

List of Tables

Chapter 1. Multidimensional Skill Mismatch among College Graduates

1.1 Introduction

Beginning with the works of Burdett (1978), Johnson (1978), Jovanovic (1979 a, b), and Mortensen and Pissarides (1994), job matching theory plays an essential role in understanding earnings differences among workers. Match quality—defined as an idiosyncratic factor that uniquely determines worker productivity and, in turn, wages in a given job—is a widely-accepted theoretical concept, but no consensus exists on how to operationalize the concept empirically. Existing empirical studies have focused on empirical comparisons of educational attainment and educational job requirements; for example, Duncan and Hoffman (1981), Verdugo and Verdugo (1989), and Clark et al. (2017) examine differences in years of schooling or degree completed by the worker and years of schooling or degree required for his/her job, while Abel and Deitz (2015) and Robst (2007a) examine the relatedness of the worker's field of study (college major) and his/her occupation. Each study in this empirical literature focuses on identifying the wage penalty associated with the measure of mismatch, as predicted by theory.

In this chapter, I use data for a sample of college graduates from the 1997 National Longitudinal Survey of Youth (NLSY97) to develop a novel empirical measure of mismatch. The measure proposed in this chapter is the distance between a vector of skills acquired in college and a vector of skills required in the post-college occupation. In measuring worker skill, I depart from the convention of using degree or college major and instead use detailed information on college coursework. Specifically, I define the skill acquired in each subject (e.g. mathematics, language) based on the college credit hours earned by the worker in that subject. In measuring occupational skill requirements (for the employer side of the match), I depart from the convention of using occupation and instead use data from Occupational Information Network (O*NET) to identify the depth of knowledge required in each subject area. My "skill mismatch index" is a single measure of the distance between skills acquired and skills required in several skill areas. The main advantage of the skill mismatch index over existing measures is that it measures mismatch in multiple skill dimensions and on a continuum. In contrast, measures that use degree or college major to define skill implicitly assume skill is homogeneous among workers who, for example, have the same degree or college major.

I use the skill mismatch index to reexamine (and extend) the questions that are at the heart of this literature: What is the wage penalty associated with mismatch? Does the wage penalty associated with mismatch depend on whether the worker is over-skilled or under-skilled for his/her occupation? Does the wage penalty associated with mismatch vary across skills? Do the findings differ for men and women? To assess the value of this new measure, I also investigate how the skill mismatch index compares to existing mismatch measures that are based on comparing worker's schooling/degree or major with their occupation.

The analysis consists of the following steps. First, I compare my skill mismatch index (separately for men and women) with existing measures of mismatch. This exploration reveals considerable variation in the skill mismatch index among workers classified as matched or mismatched based on existing measures. Second, I estimate log-wage models (separately for men and women) in which the key regressor is the skill mismatch index. I also explore flexible forms of the skill mismatch index, by differentiating over-skilling and under-skilling (i.e., dropping the restriction that comparable positive and negative values of the skill mismatch index have wage effects that are equal in magnitude but opposite in sign) and examining the skill mismatch effect across specific skills (i.e., dropping the assumption that mismatch parameters are identical for all skill areas). I estimate these models separately for men and women due to well-known gender differences in education and occupation.[1] I estimate the models for the entire post-graduation period available in the data, as well as for two different cross-sections (one and five years after graduation) to determine whether the results change over time. Third, I introduce alternative log-wage model specifications in which existing mismatch measures replace the skill mismatch index and systematically compare the performance of each measure.

1.2 Literature Review

Until recently, the empirical literature on worker-job match or mismatch consisted of two types of studies. In the first type, mismatch is based on a one-dimensional comparison of

---

[1] Several studies touch on gender differences in education and/or occupation. For example, Daymont and Andrisani 1984 find that preferences for occupational roles and majors account for a substantial proportion of the gender earnings gap. Zafar 2013 examines determinants of college major choice but also notes gender differences in preferences in the workplace. Sicherman 1996 notes gender differences in reasons for changing jobs. Cortes and Pan 2018 examine the relationship between occupational characteristics and gender differences in both occupational choice and wages.

a worker's years of schooling or highest degree to the amount required by his/her occupations. These studies compare labor market outcomes for workers who are overeducated (meaning years of schooling or highest degree exceeds what the occupation requires) and undereducated (meaning years of schooling or highest degree is less than what the occupation requires). In the second type of study, mismatch is based on a one-dimensional comparison of college major (field of study) to occupation. Such studies typically examine wage effects associated with major mismatch, meaning the worker's college major is not closely related to his occupation.[2]

Focusing first on the overeducation/undereducation approach, findings tend to be consistent across studies. Duncan and Hoffman (1981) and Verdugo and Verdugo (1989) establish the common estimation strategy used in the literature. Duncan and Hoffman (1981) find that the estimated effect on log-wages of one year of overeducation (0.029) is significantly lower than the estimated effect of a required year of schooling (0.063); while overeducated workers earn more than their coworkers, they face a wage penalty because their wages are lower than workers with the same level of schooling in jobs requiring their level of schooling. Verdugo and Verdugo (1989) estimate a log-wage penalty of 13% for overeducated workers compared to non-overeducated workers with identical schooling levels. This finding is consistent with the theoretical expectation that lower match quality results in lower wages.

---

[2] Examples of overeducation/undereducation studies include Duncan and Hoffman 1981; Rumberger 1987; Hartog and Oosterbeek 1988; Verdugo and Verdugo 1989; Alba-Ramirez 1993; Groot 1993; Robst 1995; Groot 1996; Kiker et al. 1997; Dolton and Vignoles 2000; Groot and Maassen van den Brink 2000; Rubb 2003a; Rubb 2003b; Rubb 2003c; Groeneveld and Hartog 2004; Rubb 2006; Dolton and Silles 2008; Herrera-Idarraga et al. 2010; Clark et al. 2014. Examples of studies that incorporate major into mismatch include Frenette 2004; Robst 2007a; Robst 2007b; Robst 2008; Freeman and Hirsch 2008; Ortiz and Kucel 2008; Boudarbat and Chernoff 2010; Nordin et al. 2010; Roksa and Levey 2010; Abel and Deitz 2015.

Subsequent studies extend the analysis by examining the career dynamics or mobility of overeducated workers (Clark et al. 2017; Sicherman 1991), characterizing workers who are overeducated (Clark et al. 2017), using differing methods to define occupational education requirements (Alba-Ramirez 1993; Chevalier 2003; Rumberger 1987), comparing results across studies (Groot and Maassen van den Brink 2000; Hartog 2000), and linking empirical findings to theoretical expectations (Leuven and Oosterbeek 2011). Many of these studies also examine the wage effects of undereducation. Sicherman (1991), for example, finds that undereducated workers receive a log-wage premium (0.072) relative to workers who are not undereducated or overeducated, and argues that these workers compensate for their lack of education with unobserved experience or skills.

In the college major approach (Abel and Deitz 2015; Robst 2007a, 2007b, 2008), a mismatch is defined when the worker's field of study does not closely relate to his occupation. Robst (2007a, 2007b, 2008) identifies mismatch using data from the National Survey of College Graduates, where workers self-reported the extent to which their work relates to their highest degree field. He finds that mismatched workers earn less than their matched counterparts (where the estimated log-wage penalty of 0.119 reported in Robst (2007a) is a typical finding) and that this wage penalty is greater in majors that provide relatively more occupation-specific skills (e.g., business management, engineering, health professions) (Robst 2007a).

Abel and Dietz (2015) also study major mismatch, but instead of using workers' self-reported assessments they rely on a list published by the National Center for

Education Statistics (U.S. Department of Education) that links majors to their related occupations. They find that workers with a “correct” major-occupation match can expect to receive a log-wage benefit of 0.054. Additional studies have examined other features related to major mismatch: Boudarbat and Chernoff (2010) and Frenette (2004) study major as a determinant of mismatch, Ortiz and Kucel (2008) examine majors among overeducated workers, Roksa and Levey (2010) focus on occupational growth, and Freeman and Hirsch (2008) link majors and knowledge content of occupations in the market.

Two recent studies depart from the approaches described above, and closely relate to the current chapter. Hadavand et al. (2019) use college transcript data in the NLSY97 to identify the number of “relevant” courses completed by each respondent, where “relevant” means the course was in a subject area listed by O*NET as being related to the respondent’s occupation; “number of relevant courses” serves as their (continuous) measure of match quality. The authors insert this match quality measure into a standard, Mincer log-wage regression and find that each additional relevant course taken by the worker is associated with a wage increase of approximately seven percent. While Hadavand et al. (2019) introduce a valuable innovation by measuring the quantity of relevant skill acquired, the current chapter introduces an additional innovation by measuring the *discrepancy* between a coursework-based skill measure and occupational skill requirements.

In the second recent study referred to above, Guvenen et al. (2018) measure skill mismatch by examining the gap in worker ability (using cognitive and noncognitive test scores available in the 1979 cohort of the National Longitudinal Survey of Youth) and skills required for their occupation (characterized by O*NET occupation-specific skill descriptors). They focus on three aggregate skill areas: mathematics/quantitative, verbal, and social. The authors find that poorly matched workers earn lower wages, even many years after they have left the occupation; for example, wages are predicted to be 2.05% lower for workers whose mismatch is one standard deviation above the mean relative to those at the mean. The current chapter is similar to Guvenen et al. (2018) in that it measures the discrepancy between skills acquired and skills required, using multiple skill areas. However, in contrast to the three aggregate skills used by Guvenen et al. (2018), the current study uses 30 knowledge dimensions (ranging from mathematics to management to biology). Moreover, the method used to measure the skills levels differs: Guvenen et al. (2018) rely on aptitude tests, while the current study utilizes college coursework. Also, Guvenen et al. (2018) use factor loadings to weight each skill, while weights derived from O*NET data (described in section 1.3.2) are used in the current chapter.

Many of the studies discussed above restrict their samples to men (e.g., Guvenen et al. 2018; Sicherman 1991; Verdugo and Verdugo 1989), although several also examine women (e.g., Dolton and Silles, 2001; Dolton and Vignoles, 2000; Groot, 1996). When including both genders, a common approach is to separate men and women in the regression analysis. For example, in the overeducation/undereducation literature, Duncan and Hoffman (1981) separate their sample into four race-sex sub groups, noting that the estimated return to a year of overeducation is nearly twice as high for white women

(0.052) as for white men (0.029). In the major mismatch literature, Robst (2007a) points out that there are substantial differences between men and women in major and career choice as well as reasons for accepting a position outside the major; the estimated wage penalty associated with working in a field not related to their degree ("major mismatch") is smaller for women (-0.101) than for men (-0.119). Other studies pool men and women but control for gender in the regression analysis, such as Clark et al. (2017) in the overeducation/undereducation literature and Abel and Deitz (2015) in the major mismatch literature. Clark et al. (2017) argue that women may value non-financial factors (proximity to home, flexibility in working hours, etc.) that are typically associated with lower job requirements.

1.3 Data

My primary data source is the 1997 National Longitudinal Survey of Youth (NLSY97). The NLSY97 began in 1997 with 8,894 respondents (51.2% male) born between 1980 and 1984. Respondents were interviewed annually from 1997 to 2011 and biennially from 2013 onward. Data are currently available through the round conducted in 2015-2016.

The NLSY97 provides detailed information on the education and employment experiences of respondents. Two features of the data allow for the construction of the skill mismatch index. First, college transcript data are available for many respondents who attended post-secondary institutions. This information was collected by the Post-Secondary Transcript Study (PSTRAN) in 2012-2013; post-secondary transcripts were obtained from universities attended by respondents who signed a waiver. The coursework data in the college transcripts are critical to the construction of the skill mismatch index. Second, the data contain detailed job histories of respondents, including occupation codes for jobs held. These occupation codes can be linked to data from the Occupation Information Network (O*NET), a database sponsored by the U.S. Department of Labor which contains occupation-specific descriptors that I use to measure the skills required for an occupation.

1.3.1 Sample Selection Criteria

Table 1.1 contains an overview of the sample selection criteria that I impose. I begin the sample selection for this analysis by restricting the sample to 2,354 respondents in the NLSY97 who have earned a bachelor's degree. I also exclude respondents who do not have Armed Services Vocational Aptitude Battery (ASVAB) scores available. The ASVAB is an aptitude test administered in 1997 to the respondents in the NLSY97; in this chapter, it serves as a measure of pre-college aptitude (see section 1.3.3 for details). Next, I exclude respondents who do not have college transcripts collected by the PSTRAN study. The data do not indicate if all the respondent's transcripts were collected so I drop respondents who appear to have incomplete transcript data, which I define as fewer than 15 credit-adjusted courses.[3] The remaining sample contains 1,354 respondents.

---

[3] Credit-adjusted courses is total credit hours divided by the modal number of credit hours earned by the respondent at that university; this adjustment addresses the fact that universities may assign differing credit hours for the same course.

For the 1,354 respondents in the sample, I keep only post-college wage observations. My goal is to focus on terminal bachelor's degree recipients. To contend with graduate school enrollment, I terminate the observation period when an individual re-enrolls in school for a period of at least three months. I delete wage observations if the average hourly wage is not between $0.50 and $250 per hour. This is done because the average hourly wage is computed by NLSY97 staff (using a procedure based on usual wage, time unit of pay, and usual hours worked), which can lead to extremely low and extremely high values. Observations that lack a 2002 Census occupation code, which indicates the respondent's occupation/profession, are deleted because this information is needed to link the O*NET database. Lastly, I delete observations that have a valid 2002 Census occupation code but do not have a corresponding entry in the O*NET database, because the occupational descriptors from the O*NET database are needed to construct the skill mismatch index. The final sample consists of 11,270 wage observations for 1,313 respondents. There are 6,637 observations for 755 women and 4,633 observations for 558 are men. All observations are for wages earned within roughly 15 years of the respondent's college graduation date.

I also examine two cross-sections of the "panel data" sample described in the previous paragraph. To select the first cross-section, I select the wage observation that occurs within three months of the one-year mark after the respondent received his or her bachelor's degree; if multiple observations qualify, then the one with the highest hours per week is used. There are 558 men and 755 women in this cross-section. The second cross-section is constructed similarly, but tied to the five-year mark after college graduation. Not all respondents are observed several years after graduation, so there are only 375 men and 490 women in this five-year cross-section.[4]

1.3.2 Mismatch Variables

In this subsection, I explain how NLSY97 and O*NET data are used to construct the skill mismatch index. I also describe the construction of over/undereducation and major mismatch measures, which are two previously developed measures of mismatch that I use for comparison.

Skill mismatch index:

The purpose of the skill mismatch index is to measure the disparity between skills required by the occupation and skills acquired by the worker for multiple skill dimensions. To identify the skill requirements of each occupation I use O*NET, a database that contains occupational descriptors for almost 1,000 occupations in the 2017 release (version 22.1). For each occupation, O*NET provides “level scores” and “importance scores” for over 30 different knowledge dimensions (mathematics, education, etc.; see table A.1 for complete list). Level scores represent the depth (or amount) of knowledge required in a given dimension for the occupation, while

---

[4] I select five years after graduation because the majority of respondents *are* observed at this milestone. Compared to the sample of respondents who have data five years after graduation, the sample is only 6.3% larger if I select four years after graduation, and more than 12% smaller if I select six years after graduation.

importance scores reflect how important knowledge in a given dimension is to the occupation.[5] Both level and importance scores range from 0 to 100. [6]

I use a crosswalk to link the 2002 Census occupation codes associated with the 11,270 wage observations in the NLSY97 sample to the Standard Occupational System (O*NET-SOC) codes in the O*NET database. Thus, for each wage observation, there is an associated level score and importance score for 30 knowledge dimensions.[7] In cases where multiple O*NET-SOC codes map to a single 2002 Census code, I use the average level scores across the O*NET-SOC occupations. For comparability with the worker skill measure described below, I use the level score's percentile rank in the sample distribution (for each knowledge dimension) as my measure of occupation skill requirements.

To identify the skills acquired by workers, I use respondents' college coursework data from the NLSY97. The NLSY97 used the 2010 College Course Map (CCM), a taxonomy system for coding post-secondary education courses developed by the National Center for Education Statistics (NCES), to code the transcripts collected by PSTRAN;

---

[5] The O*NET website states "while the same skill can be important for a variety of occupations, the amount or level of the skill needed in those occupations can differ dramatically." For example, occupations "Lodging Managers" and "Materials Science" have an importance score of 82 for the knowledge dimension "Mathematics;" this suggests that mathematics has a similar degree of importance in both occupations. However, Materials Science has a level score of 83 in mathematics while Lodging Managers has a level score of 56; this suggests that the depth of knowledge required in mathematics is substantially higher for Materials Science. Thus, while Lodging Managers only require a moderate level of mathematics knowledge (enough training to manage financial activities), this knowledge is used heavily in the occupation.

[6] The level scores are constructed using responses from a survey fielded by O*NET. For all knowledge descriptors, respondents of the O*NET questionnaire are asked, for example, "what level of Economics and Accounting knowledge is needed to perform your current job?" Respondents are provided with scale anchors to guide them in scoring their occupations (see table A.2 for scale anchors). For example, in the knowledge descriptor Economics and Accounting, a level score ranging from 43-69 is associated with the ability to "develop financial investment programs for individual clients" and a score greater than 70 is associated with the ability to "keep a major corporation's financial records."

[7] The O*NET database contains information on 33 knowledge categories. However, I drop the "Clerical," "Customer and Personal Service," and "Personnel and Human Resources" categories as there are no corresponding coursework or subject areas, suggesting that college graduates would not be able to develop these skills in college through their coursework.

this allows subject areas and courses taken by respondents to be comparable across institutions.[8] Because the subject areas used in the CCM taxonomy differ from the 30 knowledge dimensions defined by O*NET, I map the former into the latter using a crosswalk I devised (see table A.3). For each knowledge dimension, I sum the total credit hours earned by the respondent and identify the percentile rank of that total in the sample distribution. This is my measure of worker skill in each knowledge dimension.

I define the skill mismatch index as the sum over knowledge areas of the absolute value of the difference between the credit hour percentile rank and the level score percentile rank, weighted by the (normalized) importance score percentile rank. Letting $PL_{\mathrm{j,k}}$, $PC_{i,k}$, and $PI_{\mathrm{j,k}}$ represent the percentile ranks of the level score, credit hours, and importance score for occupation *j*, individual *i*, and knowledge dimension *k*, the skill mismatch index is defined as follows:

$$M_{i,j} = \sum_{k=1}^{30} \frac{PI_{\mathrm{j,k}}}{\sum_{k=1}^{30} PI_{\mathrm{j,k}}} * \left| PC_{i,\mathrm{k}} - PL_{j,\mathrm{k}} \right| .$$

Using the weight ($\frac{PI_{\mathrm{j,k}}}{\sum_{k=1}^{30} PI_{\mathrm{j,k}}}$) allows for knowledge areas that are more important to the occupation to have a higher weight in the average and thus contribute more to the skill mismatch index.[9] The closer the skill mismatch index is to “0,” the better matched the worker's skills are to his occupation's skill requirements.

---

[8] For example, courses related to financial mathematics are coded as “27.0305,” where the “27” indicates that it is a mathematics course.

[9] For example, if mathematics is important to the occupation but fine arts is not, then the discrepancy in the worker’s fine arts skills and the occupation’s fine arts skill requirements will have minimal influence on the skill mismatch index.

Over- and Under-Skill Mismatch Index: Because the skill mismatch index defined above is based on the absolute value of the difference between the percentile ranks of level score and credit hours, it does not distinguish between cases where skills are greater than what is required and cases where skills are less than what is required. This distinction may capture key differences in the direction of the mismatch; over-skilling may reflect excess, underutilized skills that have no place in a job with low skill requirements while under-skilling may reflect deficiencies in skills in a job with high skill requirements. Not examining differences in over- and under-skill mismatch is akin to grouping overeducation and undereducation together into a single mismatch category. Thus, I drop the restriction in the skill mismatch index that over-skilling and under-skilling have equal (in magnitude) effects, and instead decompose the skill mismatch index into an over-skill mismatch index and an under-skill mismatch index:

$$M_{i,j}^{Over} = \sum_{k\in K_{1i,j}} \frac{PI_{\mathrm{j,k}}}{\sum_{k\in K_{1ij}} PI_{\mathrm{j,k}}} * Max[PC_{i,\mathrm{k}} - PL_{j,\mathrm{k}}, 0]$$

$$M_{i,j}^{Under} = \sum_{k\in K_{2ij}} \frac{PI_{\mathrm{j,k}}}{\sum_{k\in K_{2ij}} PI_{\mathrm{j,k}}} * Max[PL_{j,\mathrm{k}} - PC_{i,\mathrm{k}}, 0]$$

Elements of the over (under) skill mismatch index are non-zero for knowledge dimensions where the credit hour percentile rank is greater (less) than the level score percentile rank, suggesting that the worker has greater (less) skill than what is required for his occupation. The over and under skill mismatch indexes are constructed in the same manner as the skill mismatch index, but use only the relevant subset of knowledge areas; i.e., K_{1ij} is the set of knowledge areas the respondent is over-skilled and K_{2ij} is the set of knowledge areas the respondent is under-skilled. These refined indexes are convenient for identifying different wage effects of over-skill and under-skill.

Subject-Specific Skill Mismatch Index: I also decompose the skill mismatch index into four separate measures that reflect skill mismatch in specific knowledge areas (k). Although the skill mismatch index has the advantage of being based on a wide range of skills, it does not distinguish whether mismatches in certain skill areas have a greater impact on wages than do mismatches in other skill areas. To address this issue, I drop the assumption that the skill mismatch parameter is constant across skill areas by replacing it with subject-specific skill mismatch indexes. This decomposition is important because it allows us to understand if there are specific skill areas that drive the wage effects of the skill mismatch index. To do this, I begin by aggregating information on workers' knowledge acquired and occupation knowledge requirements into four categories: (1) management and communications, (2) science, engineering, technology and mathematics (STEM), (3) arts and humanities, and (4) social sciences. Table A.4 provides a complete list of subject areas that fall into each of these four categories. The subject-specific skill mismatch indexes are constructed in the same manner as the skill mismatch index but only use the relevant subset of the $K=30$ knowledge areas.

Overeducation and Undereducation: I define overeducation (undereducation) as a mismatch that occurs when the worker's highest degree is greater than (less than) what is required by his occupation. To identify the degree required for each occupation, I use a descriptor from O*NET that indicates the level of education necessary for the occupation. Table A.5 lists the education categories given by O*NET. Following a method similar to

Abel and Deitz (2015), I collapse them into three categories: less than a bachelor's degree, bachelor's degree, or greater than a bachelor's degree.[10] In my sample, 55% of men and 56% of women are overeducated while 11% of men and 16% of women are undereducated.

Major Mismatch: Following Abel and Deitz (2015), I define major mismatch using a crosswalk developed by the NCES that links Standard Occupational Classification (SOC) codes to Classification of Instructional Programs (CIP) codes.[11] A respondent has a major mismatch if his major is not associated with his occupation; i.e., the respondent's major is not linked to the respondent's occupation in the crosswalk.[12] In the sample for this study, 73% of men and 76% of women have a major mismatch; these rates are similar to Abel and Deitz, 2015, who report that 73% of their sample has a major mismatch.

1.3.3 Other Variables

The dependent variable used in the analysis is the log of the average hourly wage, CPI-U deflated to 2006 dollars. In addition to the mismatch variable described in 1.3.2, I use a rich set of baseline controls: The pre-college variables in the baseline controls include dummies that indicate if the respondent is black and Hispanic, as well as mother's highest

---

[10] Other methods for defining required education were tested, including using mean and mode level of education based on a national sample (CPS) as well as the corollary years of schooling. Ultimately, the Abel and Deitz (2015) criterion was implemented as it explained greater variation in log-wages than these other methods.

[11] The CIP taxonomy can be converted to the CCM taxonomy used in the NLSY97. The SOC taxonomy can be converted to the Census 2002 occupation codes used in the NLSY97. In the case where, say, multiple occupations in the SOC taxonomy (with differing lists of associated majors) map to a single occupation in the Census 2002, the associated major is only counted if at least 50% of the SOC occupations are associated with the major.

[12] The major-to-occupation associations are not mutually exclusive so it is possible for several occupations to be associated with the same major and several majors to be associated with the same occupation.

grade completed, mother's employment status when the respondent was 16 years old, an indicator that the respondent's primary language was English in 1997, and dummies that indicate family structure when the respondent was 16 years old (lived with both parents, lived only with mother, lived with mother and her partner, lived only with father, or other). I also include the 12 ASVAB sub-test scores. The ASVAB is a 12-part aptitude test that was administered to NLSY97 respondents in 1997-1998.[13] The respondent's final scores were scaled by the NLSY97 staff using item response theory and these scores capture multiple dimensions of the respondent's pre-college ability.

The in-college variables in the baseline controls include college grade point average (GPA), which I compute based on grades included in the transcript data), an indicator that is one if the respondent completed an Associate's degree, age at graduation (receipt of bachelor's degree), and years of labor market experience prior to college graduation. The post-college variables in the baseline controls include dummies that indicate if the respondent is married, cohabiting, residing in the South, residing in the West, residing in the Northeast, and residing in an urban area, as well as calendar year dummies, years of labor market experience after college graduation and its square, tenure and its square, and hours of work per week[14]; all the post-college variables are time-varying.

---

[13] The 12 sub-tests include general science, arithmetic reasoning, word knowledge, paragraph comprehension, mathematics knowledge, electronics information, auto information, shop information, mechanical comprehension, assembling objects, numerical operations, and coding speed.

[14] Hours of work per week is the number of hours worked per week at the time of the interview (or at the job's stop date if the respondent is no longer in that job at the time of the interview).

1.3.4 Descriptive Statistics of the Skill Mismatch Index

In this section, I describe how the skill mismatch index relates to the overeducation/undereducation and major mismatch variables. I supplement this discussion by providing examples of the mismatch variables from the data. Following this, I examine the dependent variable and baseline controls for the entire sample as well as for subsamples defined by low and high skill mismatch values.

In order to understand how the new measure constructed in this paper relates to existing measures, table 1.2 presents the distribution of the skill mismatch index for workers who are overeducated, correctly educated, and undereducated and workers with a major mismatch and major match. The mean skill mismatch for correctly educated workers (18.8 for men and 20.6 for women) is significantly lower than that of overeducated workers (27.0 for men and 26.0 for women) and similar-to that of undereducated workers (18.2 for men and 19.7 for women). The differences between overeducated and correctly educated workers is consistent with the fact that both overeducation and high skill mismatch indicate workers are not well matched with their occupation. However, the data also suggest that there may be differences in the level of mismatch among workers who are broadly categorized as overeducated. This is clearly seen in the spread of the skill mismatch index for overeducated workers: the standard deviation is 10.4 for men and 9.7 for women, which is slightly greater than the standard deviation for the gender-specific samples. Moreover, we see a minimum skill mismatch value of 6.5 (4.9) for an *overeducated* man (woman) and a maximum level of 60.0 (66.7) for a *correctly* educated man (woman). This suggests that some overeducated workers develop skills necessary for their occupation (reflected in a low skill mismatch) and that there are correctly educated workers who lack skills necessary for their occupation (reflected in a high skill mismatch).

The most surprising result of table 1.2 is how similar the skill mismatch index is for undereducated and correctly educated workers. The mean skill mismatch levels for these two groups of workers (18.2 vs. 18.8) are not significantly different from one another for men; for women, the mean values (19.7 vs. 20.6) are significantly different from one another, but the difference is small in magnitude. Because undereducation is a status of mismatch, we expect undereducated workers to have a higher mean skill mismatch than correctly educated workers. Since this is not the case, it is possible that for undereducated workers, the skill mismatch index reflects a previously unobserved element of the worker-occupation match. It could either be that, on average, these workers develop skills valued in their occupation despite not having a high enough degree to meet their occupations' qualifications or that, despite the higher degree requirements, these occupations are actually similar to occupation's requiring only bachelor's degree (as seen with the similar skill requirements).

Table 1.2 also provides the skill mismatch distribution of workers with a major match and major mismatch. Unsurprisingly, the mean skill mismatch is significantly higher for workers with a major mismatch (24.6 for men and 24.4 for women) compared to those with a major match (19.5 for men and 21.1 for women). However, within the major mismatch and major match categories, there is variation in the skill mismatch index that demonstrate differences among workers who fall into each category. There are workers with a major match but a high skill mismatch (e.g., the maximum skill mismatch for workers with a major match is 54.7 for men and 49.2 for women) and workers with a major mismatch but a low skill mismatch (e.g., the minimum skill mismatch for workers with a major mismatch is 5.6 for men and 4.9 for women). Thus, table 1.2 illustrates that the implicit assumption of these categorical measures (that mismatch is homogenous among workers with the same degree or major) does not hold in the current sample.

Lastly, table 1.2 allows for comparisons between men and women. The mean skill mismatch for overeducated and major mismatched men is similar to that of women in the respective category; however, the mean skill mismatch for undereducated, correctly educated, and major matched men is significantly lower than that of women in the respective category. It appears that gender differences in the skill mismatch index primarily manifest when the other empirical measures indicate the worker is well-matched to their occupation. Although there are significant differences between the genders, it is interesting to note that the magnitude of the difference is consistently small. Overall, the trends in table 1.2 not only show a link between skill mismatch and the other mismatch variables, but also that other mismatch variables suppress important variation in workers skill mismatch.

To supplement the discussion in table 1.2, in table 1.3 I provide empirical examples, drawn from my data, for three worker-occupation pairs. For each worker-occupation pair, I provide information related to the worker-occupation mismatch, worker’s college major and coursework, and the occupation’s title and skills. From table 1.2, we saw that the categorical measures of empirical mismatch suppress differences in skill mismatch among workers within each category. Consider person 1 in table 1.3: this worker may not seem well-matched to his job because he has a major mismatch; i.e., he majored in Social Sciences, but his occupation is Network and Computer Systems Administrator. However, the value of his skill mismatch index is 23.2, which is less than the sample mean (24.6, per table 1.2) among men with a major mismatch, suggesting a relatively low skill mismatch. This is in part because he earned credit hours outside of his major that are required for his occupation (e.g., he completed 9 credit hours in "computer and electronics," a skill area for which his occupation requires a great depth of knowledge). This example illustrates the general point that the skill mismatch index incorporates training acquired by the respondent that may not be evident when only considering a categorical measure such as major or degree. More broadly, if overeducation is used to measure mismatch, then the three workers in table 1.3 are indistinguishable. If major mismatch is used to measure mismatch, then person 2 and 3 are indistinguishable. If the skill mismatch index is used to measure mismatch, the three workers can be differentiated from each other and thus be ranked to determine who is more mismatched. Even differences across respondents who have the same occupation and/or the same major can also be captured with the skill mismatch index. This can be seen when comparing person 2 to person 3; both have the same occupation and major, but different credit hours are earned in the knowledge dimension that require the greatest skill – this contributes to the differing skill mismatch index and may assist in explaining any wage differences between the two respondents. Overall, table 1.3 shows that the skill mismatch index allows for a more refined comparison of mismatch between workers and accounts for skills or components not incorporated in the other measures that impact how we assess the match of the worker-occupation pair.

In table 1.4, I present the mean and standard deviation of the dependent log-wage variable and variables from the baseline control. The purpose of this table is to examine observable differences between observations with a “high skill mismatch” (above the median) and a “low skill mismatch” (below the median). The average ASVAB scores for arithmetic reasoning for both genders and mathematics knowledge for women is significantly higher for low skill mismatch compared to high skill mismatch workers, whereas word knowledge for both genders and paragraph comprehension for women are significantly lower for low skill mismatch compared to high skill mismatch workers. There are also differences in the family structure variables; for example, a significantly higher percentage of high skill mismatch men and women cited living with both parents at age 16. The mean hours worked per week and job tenure is lower for high skill mismatch workers for both genders. The latter is consistent with the finding in job matching theory that mismatched workers are more likely to separate from their job sooner (and thus have lower tenure). Most importantly, there is a substantial difference in the dependent variable between the two groups which seems to be present for both genders. This comparison of the high and low skill mismatch groups demonstrates that there are important differences in observables across skill mismatch, although it is interesting to note that these differences seem to be consistent across gender.

1.4 Estimation Strategy

The method of parameterizing the log-wage model to incorporate an empirical mismatch variable is as follows:

$$\log(W_{ijt}) = \beta_0 + \beta_1 M_{ij} + \beta_2 X_{it} + \varepsilon_{it} + \varphi_{ij} \qquad (1.1)$$

where the dependent variable is the log of the average hourly wage for individual *i* in occupation *j* at time *t*, X_{it} is the vector of baseline controls (discussed in 1.3.3), ε_{it} is an idiosyncratic stochastic shock, and M_{ij} is the empirical mismatch variable. It is likely that M_{ij} does not capture the entirety of the unobservable match quality; thus φ_{ij} is the unobserved component of match quality that is not captured by the observed M_{ij}. The main parameter of interest is β_1, which is expected to be negative because greater mismatches reflects lower productivity and, thus, lead to lower wages. Thus, β_1 represents the estimated mismatch wage penalty.

A general concern noted in the literature is that the assignment of the education acquired by workers, education requirements for occupations, and the worker-occupation match quality resulting from the search process is non-random (see Leuven and Oosterbeek 2011 for further discussion). For example, Arcidiacono et al. (2012) find evidence of pre-college ability sorting across majors. Arcidiacono (2004) additionally notes that preferences for certain occupations play a role in major choice, Fouarge et al. (2014) find that economic preferences are related to occupation choice, and Kinsler and Pavan (2015) note that workers with higher ability levels may be more likely than others to work in related (well-matched) occupations.[15] In the current study, it is likely that

---

[15] Although none of these studies are part of the empirical mismatch literature, many of the conclusions and findings in these papers relate to concerns that underlie the empirical mismatch literature.

unobserved variables, particularly ability and preferences, are correlated with both wages and the mismatch variables. Most studies in the empirical mismatch literature do not address this endogeneity issue, but there are exceptions: some studies take an instrumental variable approach (Korpi and Tahlin 2009 use family background variables as instruments and Dolton and Silles 2008 use exogenous changes in the overeducation distribution as their instrument) or a fixed effects approach (Lindley and McIntosh 2010 use individual fixed effects for their panel data). In the current chapter, I estimate equation 1.1 using ordinary least squares and a “selection on observables” strategy in which I condition on a rich array of individual characteristics (listed in section 1.3.3), including measures of pre-college ability (12 ASVAB sub-test scores), a measure of academic performance (college grade point average), and family background characteristics. Other studies have similarly implemented this approach by conditioning on pre-college test scores (or ability) and background/demographic variables that impact the demand for and/or choice of schooling to estimate the wage effects across majors or other education choices (see Hamermesh and Donald 2008 and Grogger and Eide 1995).

There are two parts to the empirical analysis. First, I focus on comparing existing measures of mismatch to the newly developed skill mismatch index. I begin this part of the analysis by considering three alternative measures for M_{ij} in equation 1.1. First, I insert dummies that indicate if the respondent is overeducated and undereducated in the occupation for M_{ij}. Second, I insert a dummy that indicates if the respondent has a major mismatch for M_{ij}. Third, I insert the skill mismatch index as M_{ij}. After investigating numerous functional forms, I found that a linear specification (meaning the relationship between M_{ij} and log-wage is constrained to be linear) is sufficiently flexible for specifications that include the skill mismatch index.[16] All regressions are estimated separately for men and women due to gender differences in education and occupation choice established in the literature (see section 1.2).

I compare model quality across specifications using the Akaike Information Criterion (AIC). Information criteria measure predictive accuracy, are typically defined based on deviance, and are useful in evaluating different models on a common scale (Gelman et al., 2014). I use the AIC in this analysis, not only because it is applicable to the analytical framework used in the current study, but also because AIC does not require any of the models to be true to determine which is comparatively better. The main disadvantages of AIC, which are largely inapplicable to this paper, are that it does not perform well with large complex models or if the true model is of finite order (Gelman et al., 2014). It also tends to overfit models.

I complete this first part of the analysis by comparing the estimated mismatch wage penalties of the different mismatch measures. The purpose of this is to assess if the proposed skill mismatch index is a refinement of the other measures, in that it offers additional or different insight into the mismatch wage penalty. To supplement this comparison, I also estimate an alternative to equation 1.1, where I modify equation 1.1 to

---

[16] The linear specification best reflects the patterns in the data related to the skill mismatch index. The results of the Ramsey regression specification error test suggest that a linear functional form is the correct specification. I also examined other functional forms. When implementing the quadratic specification, the coefficient estimate was not significant. When including a cubic term, the coefficients have conflicting signs and/or are not significant. Further, there are no patterns in the data to suggest these polynomial functions are suited for the data. When implementing the quartile specification, the parameter estimates are significant with similar marginal effects for quartile 1-3; this suggests that the effect is similar at various points of the distribution although there is a higher penalty in quartile 4. The logarithmic function also has a significant estimate although there is nothing in the data patterns to suggest that this is a better choice.

include the overeducation/undereducation dummies, the major mismatch dummy, and the skill mismatch index in the same regression; I do this to examine how estimates of the empirical mismatch variables change when conditioned on each other.

In the second part of the analysis, I focus exclusively on the skill mismatch index. I begin by examining flexible forms of the skill mismatch index. First, I drop the restriction that over-skilling and under-skilling have equal effects. Instead, I estimate an alternative to equation 1.1, where I replace M_{ij} with the over and under skill mismatch index. Second, I drop the assumption that the skill mismatch parameter is the same for all skill areas; I estimate an alternative to equation 1.1, where I replace M_{ij} with four subject-specific skill mismatch indexes. Lastly, I estimate equation 1.1 for two alternative cross-sections of the data, defined one year and five years after college graduation (see section 1.3.1). This is done to examine if the estimates remain consistent at different points in time.

1.5 Results

In section 1.5.1, I present estimated log-wage parameters associated with the various mismatch variables (overeducation/undereducation, major mismatch, and skill mismatch index), and compare both the model fit and coefficient estimates. This analysis is based on gender-specific samples of all post-college wage observations. In section 1.5.2, I focus on the skill mismatch index, and present estimates for the over and under skill mismatch index and subject-specific skill mismatch index as well as for two cross-sections of the data defined one year and five years after graduation.

1.5.1 Wage Effects of Overeducation/Undereducation, Major Mismatch, and Skill Mismatch Index

In the first part of the analysis, I identify the wage penalty associated with mismatch, investigate how the skill mismatch index compares to existing mismatch measures that are based on comparing worker's schooling/degree or major with their occupation, and consider how findings differ for men and women.

Table 1.5 presents the estimated mismatch wage penalty for the different mismatch measures. Column 1 shows that the estimated log-wage penalty associated with overeducation and undereducation are, respectively, 0.234 and 0.219 among men.[17,18] The similarity in the overeducation and undereducation coefficient estimates suggests that the wage penalty for mismatch (when defined based on degree) is not sensitive to the direction of the mismatch. In column 2, I use major mismatch instead of overeducation/undereducation to operationalize worker-job mismatch in equation 1.1; the estimated log-wage penalty is 0.211 for men.[19] The results of column 1 and 2 collectively reveal that existing categorical mismatch measures (overeducation/undereducation and major mismatch) imply roughly the same predicted wage loss of 0.22 log-points.

---

[17] The overeducation estimates in the current study differ from the literature at large; for example, the overeducation parameter estimate for men is -0.130 in Verdugo and Verdugo 1989. Earlier studies do not limit their samples to only college graduates and use older data as well as differing methods of defining acquired/required education.

[18] Most empirical studies find a wage benefit associated with undereducation (including Verdugo and Verdugo 1989 and Sicherman 1991); however, the wage penalty found in table 1.5 is consistent with the theoretical expectation that mismatches are associated with (lower productivity and thus) lower wages.

[19] The major mismatch estimate in the current study differs from that of other studies; for example, Robst 2007a uses workers' self-reports of the relatedness of their job to their highest degree field and identifies a major mismatch penalty of 0.1194. Earlier studies use differing methods of defining major mismatch and do not limit the sample to bachelor's degree holders.

For column 3, I estimate equation 1.1 using the skill mismatch index to reflect worker-job mismatch; the estimated coefficient for the skill mismatch index is -0.011 for men. Thus, an index of 10 is associated with a predicted wage penalty of 0.11 log-points. Because the standard deviation of the skill mismatch index is 10.00 (table 1.2), an incremental change of 10 in the index and the subsequent estimated wage penalty of 0.11 is substantial. Further, because the estimated wage penalty for overeducation/undereducation and major mismatch is roughly 0.22 log-points, a skill mismatch index of 20 (equivalent to two standard deviations) is needed to estimate the same wage loss. An index of 20 is also similar to the median skill mismatch index (table 1.2); this suggests that the overeducation/undereducation and major mismatch variables measure the middle of the skill mismatch distribution, but because these measures are categorical, they suppress interesting variation in the wage penalties of workers with differing degrees of mismatch. For example, workers at the median of the skill mismatch index distribution (index of 21.8) shifting to the 10th percentile (index of 9.5) experience about a 0.135 *lower* log-wage penalty while those shifting to the 90th percentile (index of 42.5) experience an *additional* 0.228 log-wage penalty; while the categorical measures capture the wage penalty at the median, they will not capture these other differences.

Table 1.5 reveals that many of these patterns are present for women, although there are several important differences between men and women. First, the magnitude of the estimated mismatch wage penalty is substantially higher (roughly 1.5 to 2.5 times) for men compared to women regardless of the measure of mismatch. Other studies (including Clark et al. 2017 and Robst 2007b) have noted that non-pecuniary factors, such as flexible working hours, proximity to home, or child care, play a stronger role in the choice of jobs for women that may lead to gender differences in matching and, thus, differences in the subsequent wage penalty. Second, for women, the coefficient estimates in column 1 for overeducation (-0.124) differs from that of undereducation (-0.092); thus, the direction of the mismatch, when defined based on degree, matters more for women than it does for men. Third, although men and women have similar skill mismatch index distributions (e.g., the standard deviation is 9.06 for women and 10.00 for men), women have a substantially lower estimated wage penalty associated with the skill mismatch index (0.007 vs. 0.011 for men). Thus, there is a 0.12 predicted log point difference in wages for women who are one standard deviation below the mean compared to women who are one standard deviation above the mean; this is lower than the comparable 0.22 finding for men discussed in the previous paragraph. The similarities in the dispersion of skill mismatches but differences in the wage penalty across gender suggests that estimates based off the overeducation/undereducation and major mismatch measures mask a greater spread of wage penalties for men compared to women. Fourth, because the coefficient estimate is -0.124 for overeducated women (column 1), the skill mismatch index must equal 18 (per column 3) to produce the same predicted wage loss. This is similar to men because 18 is slightly less than the median of the skill mismatch distribution and roughly two times the standard deviation (9.06). However, because the coefficient estimate for women is -0.092 (columns 1-2) for both undereducation and major mismatch, the index value must be 13 to produce the same predicted wage loss;

this is 1.5 times the standard deviation and, although still substantial, is relatively small compared to men.

Column 4 of table 1.5 contains estimates for an alternative specification to equation 1.1, where I include all three mismatch variables in a single log-wage regression to compare how the coefficient estimates change when conditioned on other mismatch variables. For men, the coefficient estimates for overeducation and undereducation in column 4 (when conditioned on major mismatch and skill mismatch index) are 46% and 7% less in absolute value than the respective coefficient estimates in column 1 (when not conditioned on major mismatch and skill mismatch index). The estimated coefficient for major mismatch in column 4 is 34% less in absolute value than that of column 2. The estimated coefficient for the skill mismatch index in column 4 is 18% less in absolute value than the estimate in column 3. Thus, while there are substantial changes in the coefficient estimates of overeducation and major mismatch when conditioning on the skill mismatch index, this is not the case for the skill mismatch index when conditioning on these two mismatch variables. That is, even when conditioned on other mismatch variables, the skill mismatch index points to substantial wage penalties; this suggests the skill mismatch index captures a mismatch effect that is not identified by the other measures. However, it should be noted that there is not a substantial change in the coefficient estimate for undereducation, which changes by even less than the skill mismatch index when moving from column 1 to column 4; thus, the undereducation indicator has enough independence from the skill mismatch index that the latter variable fails to encompass elements of undereducation as effectively as it encompasses elements of overeducation and major mismatch.

Similar results are seen for women in table 1.5. When conditioned on the skill mismatch index, the coefficient estimates for overeducation and major mismatch (column 4) substantially decrease in absolute value relative to what is seen in columns 1-2. Further, the coefficient estimate for the skill mismatch index is roughly -0.006, regardless of whether the regression conditions on the other mismatch variables (columns 3-4). Thus, the skill mismatch index effectively captures the mismatch penalty previously encompassed by the overeducation, undereducation and major mismatch variables. This finding is stronger for women than for men.

The evidence presented thus far suggests that the skill mismatch measure is a refinement over the overeducation/undereducation and major mismatch measures because it in part captures the mismatch effects encompassed by these existing measures, while also allowing the wage penalty to vary with the degree of mismatch—an advantage that is not permitted by existing (categorical) measures.

To enhance the argument that the skill mismatch index dominates other measures, I assess the relative quality of the three models using the AIC. This metric estimates the relative amount of information lost in the model; a lower AIC reflects a relatively higher quality model. Table 1.5 presents the AIC value for each specification, but because it is the relative value and not the absolute value of AIC that is important, I discuss the "AIC difference" (i.e., the difference between the model's AIC value and the model with the minimum AIC) (Burnham and Anderson, 1998). For men, the AIC value of the skill mismatch model (column 3) is 584 less than overeducation/undereducation model (column 1) and 547 less than the major mismatch model (column 2). For women, the AIC value of the skill mismatch model is 601 less than the overeducation/undereducation model and 547 lower than major mismatch model. Burnham and Anderson (1998) explain that because an information criterion is not a null hypothesis test, there are no tests of "significance;" however, they argue that an AIC difference greater than 10 is strong evidence against the model with the larger AIC statistic. Because this threshold is met when comparing the model with the skill mismatch index to the other models, the evidence strongly suggests that using the skill mismatch index results in a better model than does the use of alternative mismatch measures.[20]

1.5.2 Wage Effects of Skill Mismatch Index, Over and Under Skill Mismatch, and Subject-Specific Skill Mismatch

In the second part of the analysis, I further investigate the estimated skill mismatch wage penalty by asking whether the estimated wage penalty depends on whether the worker is over-skilled or under-skilled for his/her occupation, varies across skill areas, and differs across time.

I begin by decomposing the skill mismatch index into an over and under skill mismatch index as described in section 1.3.2; table 1.6 presents the estimated log-wage penalty associated with over and under skill mismatch. For men, the coefficient estimate

---

[20] I also investigated other metrics of model comparison, including R^2, partial R^2, BIC, and AICC, and consistently found that the "skill mismatch index" model fits the data better than models including overeducation/undereducation dummies or the major mismatch dummy.

for over skill mismatch is -0.008, which implies that a 10-unit increase in the index is predicted to lower the wage by 0.08 log points. This incremental change is substantial, because the standard deviation for the over skill mismatch distribution is 11.4 (and the mean is 36.9) for men. Thus, the predicted difference in log-wage between a man one standard deviation above the mean and a man one standard deviation below the mean is 0.18.[21] The estimated log-wage penalty of under skill mismatch is -0.004, which is exactly half the magnitude of the estimated over-skill effect. The standard deviation for the under skill mismatch distribution is 9.0 (and the mean is 24.7) for men, so the predicted difference in log-wage between a man one standard deviation above the mean and a man one standard deviation below the mean is 0.07.[22] Although significant, this suggests a small change compared to the over skill mismatch index.

What is surprising about the results of column 1 in table 1.6, is that the magnitude of the log-wage penalty for over-skill and under-skill mismatch are not symmetric; this indicates that the direction of the mismatch *does* make a difference, despite the near-equality of the estimated coefficients for the overeducation and undereducation variables in column 1 of table 1.5. Men in this sample tend to have over skill mismatches that are greater in magnitude than the under skill mismatches and, although the standard deviation is only slightly higher for the over skill mismatch, there are other indications that the spread for the over skill mismatch is generally larger (than under skill mismatch) when comparing the percentiles (see footnote 21 and 22). The results suggest that the variation

---

[21] Taking another example, a shift from the 10th percentile of the over skill mismatch distribution (16.6) to the 90th percentile (54.3) results in a predicted 0.30 log-point change in wages for men.

[22] The same shift considered in footnote 21, from the 10th percentile of the under skill mismatch distribution (11.7) to the 90th percentile (40.6) results in a predicted 0.12 log-point change in wages for men.

in the over skill mismatch is the driving force behind the wage penalty found for the skill mismatch index.

Similar to men, over skill mismatch among women is associated with a significant wage penalty (-0.006, per column 1 of table 1.6) that is greater in magnitude than that of under skill mismatch (-0.002). However, the magnitude of the estimated over skill mismatch wage penalty is substantially lower for women than for men (-0.006 vs. -0.008). For women, the standard deviation of the over skill mismatch is 11.64 (and the mean 35.6), which is similar to the standard deviation for men. The predicted difference in log-wage between a woman one standard deviation above the mean and a woman one standard deviation below the mean is 0.14—which is substantial, but notably lower than the comparable log-wage difference of 0.18 seen for men.[23] Thus, despite men and women having similar over skill mismatch distributions, there is a gender difference in the estimated wage penalty. This may relate to gender differences in both curriculum and occupation choice, as the coursework women complete or the skill areas for the occupations they select may be less heavily penalized by mismatches than those for men. Further, the estimates are conditioned on family-oriented variables (marriage, children, etc.) that have enough independent variation from the skill mismatch index to explain variation in wages for women.

Table 1.6 also reveals that under skill mismatch estimates also differ by gender. For women, the estimated coefficient for the under skill mismatch variable is a

---

[23] Shifting from the 10th percentile of the over skill mismatch distribution (15.6) to the 90th percentile (54.4) results in a predicted 0.23 log-point change in wages for women.

statistically significant -0.002 (column 1), which is smaller in absolute value than the estimate of -0.004 for men.[24] This suggests that deficiency in skills is far less important for women than for men, either because the occupations women choose compensate for these types of mismatches (via on-the-job training, etc.) or because the curricula women pursue compensate for the lack of skills in areas that do not match well.

Turning to column 2 in table 1.6, I now examine the subject-specific skill mismatch indexes. Beginning with men, we see that only the management/communications skill mismatch and STEM skill mismatch indexes have a statistically significant, negative association with wages; both arts/humanities and social sciences have statistically insignificant coefficients, and the latter even has a positive association with wages. The coefficient estimate for management/communication skill mismatch is -0.001; because the standard deviation of the distribution is 23.9 (mean of 35.6), the estimated difference in log-wage between a man who is one standard deviation above the mean and a man who is one standard deviation below the mean is only 0.05. For the STEM skill mismatch index, the coefficient estimate is -0.003, the standard deviation of the distribution is 15.5 (mean 33.6), and the estimated change in log-wage associated with a two standard deviation increment is 0.09.[25] Clearly, this is a fairly substantial change in log-wage compared to the three other subject areas that I consider.

---

[24] In Guvenen et. al (2018), the authors examine a comparable "positive" (similar-to over-skill) and "negative" (similar-to under-skill) mismatch; they also find that the effect is not symmetric, although their estimated pattern is the opposite of what is seen in table 1.6 (the estimated coefficient of the negative mismatch variable is 4.2 times as large in absolute value as the estimated coefficient of the positive mismatch variable).

[25] Shifting from the 25th percentile of the STEM skill mismatch distribution (23.4) to the 75th percentile (39.6) results in a predicted 0.049 log-point change in wages for men.

These results reveal that the management/communication and STEM skill areas drive the results of the skill mismatch index for men noted in section 1.5.1.

For women, both the management/communications and STEM skill mismatch indexes are associated with statistically significant wage penalties, as was also seen for men. The estimated management/communications skill mismatch index coefficient is -0.001 for women, which is identical to the estimate for men; the standard deviation (27.2) and mean (38.5) for women are also similar to those for men, so there is no evidence of gender differences in the effects of management/communication skill mismatch. The STEM skill mismatch index has a statistically significant coefficient of -0.001 for women, which is substantially smaller than the -0.003 men. The standard deviation (14.4) and mean (34.2) for women are similar to those for men, so the estimated change in log-wage associated with a two standard deviation change in the index for women (0.03) is much smaller than the comparable estimate (0.09) for men. In contrast to what was seen among men, for women the estimated effects of the STEM skill mismatch index are statistically significant but smaller in magnitude than the estimated effects of the management/communication skill mismatch index. In addition, the estimated coefficient for the social science skill mismatch index is a statistically significant -0.002 for women, whereas the estimate for men is an insignificant 0.001. Because the standard deviation for the social science skill mismatch index is 13.2 (and the mean 33.7), the estimated log-wage difference between women who are one standard deviation above the mean and women who are one standard deviation below the mean is 0.05. In effect, both the social science and STEM skill mismatch indexes capture for women what the STEM skill mismatch index alone captures for men.

As my final exploration, I estimate wage effects of the skill mismatch index using two cross-sections of the data, defined one and five years after college graduation, to learn whether the wage penalties change over time; table 1.7 presents the coefficient estimates for the simplest specification corresponding to column 3 in table 1.5. For men, we see that the estimated wage penalty is 0.010 in the first year after graduation and 0.013 after five years. Compared to the 0.011 estimated wage penalty for the full (longitudinal) sample (table 1.5), the estimate for the first year after graduation is slightly lower and the estimate five years after graduation is higher.[26] This points to a slight upward trend in the estimated wage penalties over time, although none of the three point estimates are statistically distinguishable from one another. The "insignificant" upward trend is surprising because we expect a downward trend in the estimated wage penalty over time as mismatched workers seek to exit their poor matches for improved ones. This notion is supported by research on early career job mobility and wage growth that provides evidence of workers engaging in a high volume of early career job changes that are driven by wage growth.[27] However, Robst 2007b notes that amenities such as flexible hours, constraints on the job search, and demand side factors (such as the availability of an occupation that matches well) may prevent workers from changing jobs for better

---

[26] Recall that the estimates presented in table 1.5 use observations anywhere from 0 to 15 years after college graduation; on average, both men and women are each observed 4.7 years after graduation.

[27] For example, Topel and Ward (1992) note that job "attachments" early career are fragile and wages grow rapidly during this early career phase (with job changes accounting for a third of early-career wage growth). Likewise, Bartel and Borjas (1981) show that there are significant wage gains among young men that are movers (when examining wage growth before, during, and after job changes).

matches and wages. Thus, as workers are “left behind” by not shifting to better matches, it plausible that the estimated mismatch wage penalty will grow over time as suggested by my findings.

For women, the coefficient estimate associated with one year after graduation (-0.008) and five years after graduation (-0.007) are even more similar to each other than what was seen for men, and also more similar to the estimate for the entire sample (-0.007) found in table 1.5. In short, the weak, upward trend in the estimated wage penalty seen for men does not exist for women. This, too, is surprising because we expect a downward trend in the wage penalty as women with mismatches seek out and improve their match; however, it is possible that non-pecuniary factors (marriage, children, etc.) prevent this from occurring.

1.6 Conclusion

In this chapter, I develop a novel empirical measure of mismatch using data from a sample of college graduates in the NLSY97 and occupational descriptors from O*NET. The skill mismatch index identifies the disparity between the skills acquired by workers in college and the skills required in their post-college occupation for multiple skill areas; this multi-dimensional metric is measured on a continuum. This new measure serves as an alternative to existing categorical measures of mismatch that rely on degree or major to determine mismatch (i.e., overeducation/undereducation and major mismatch).

I use the skill mismatch index to reexamine the wage penalty associated with mismatch. First, I establish that there is a significant and substantial negative association between skill mismatch and log-wages. For men, the wage penalty associated with the overeducation/undereducation and major mismatch measures is roughly the same as that of the median skill mismatch index. However, the results suggest that workers at the median of the skill mismatch index distribution shifting to the 10th percentile have a lower wage penalty of 0.135 log-points while those shifting to the 90th percentile have a greater wage penalty of 0.228 log-points; these differences are not captured by the existing categorical measures of mismatch. For women, the estimated wage penalty is consistently smaller (roughly half) than that of men, regardless of the mismatch measure used. Nonetheless, the qualitative patterns in the findings are the same as men. Second, a comparison of model quality using the AIC statistic reveals that the model using the skill mismatch index provides a better model fit compared to the models using overeducation/undereducation and major mismatch.

I extend the main question by examining if mismatch depends on whether the worker is over-skilled or under-skilled for his/her occupation. I find that there is a wage penalty associated with both over and under-skilling, however the penalty is not symmetric as the estimated wage penalty for the over skill mismatch index is twice (three times) as large as that of the under skill mismatch index for men (women). The results suggest that the variation in the over skill mismatch index are driving the estimated skill mismatch wage penalty. I also extend the main question by examining if mismatches depend on subject-specific skill areas. I find that, for men, the STEM skill mismatch index has the greatest, significant negative effect on wages followed by the management and communication skill mismatch index. For women, management and communication skill mismatch index has the greatest negative impact on wages, followed by social sciences and STEM; the results suggest that, in effect, both the social science and STEM skill mismatch index capture what the STEM skill mismatch index alone captures for men. These results point towards the importance of skills matching in specific skill areas that differ across gender. The final exploration examines the estimated wage penalty for the skill mismatch index using only two cross-sections of the data. Although there is a slight (insignificant) upward trend in the estimated skill mismatch wage penalty for men from one to five years after graduation, the results generally suggest the estimated skill mismatch wage penalty is consistent over time.

Overall, this paper introduces a unique approach to the empirical mismatch literature that has proven to be a refinement of existing measures. Nonetheless, this chapter represents the beginning of a new approach to this topic and additional research can be done to advance the literature. This includes directly examining gender differences in the estimated wage penalty associated with the skill mismatch index, analyzing the demand for occupations and majors in the labor market not considered in this chapter, assessing the factors that determine curriculum and occupation choice, and implementing alternative estimation strategies.

Table 1.1: Sample Selection Criteria

Criterion	**Number of Respondents**	**Number of Wage Observations**
Respondents interviewed in 1997	8,984	
Respondents who complete a bachelor's degree	2,354	52,695
Remaining respondents with ASVAB scores available	2,026	46,080
Remaining respondents with at least one transcript	1,471	35,214
Remaining respondents with at least 15 credit-adjusted courses	1,354	32,469
Remaining respondents with at least one wage after completion of bachelor's Degree	1,348	16,085
Remaining respondents with at least one wage prior to re-enrollment	1,325	12,062
Remaining respondents with at least one valid wage, occupation, and O*NET Descriptors	1,313	11,270

Table 1.2: Distribution of Skill Mismatch Index by Other Mismatch Measures

Gender	Mismatch Measure	Skill Mismatch Index				
		mean	sd	min	median	max
Men	Overeducated	26.97*	10.38	6.52	25.56	73.88
	Correctly Educated	18.76	7.92	4.94	18.10	60.02
	Undereducated	18.21	4.82	8.28	17.44	36.10
	Major Mismatch	24.62	10.46	5.56	22.98	73.88
	Major Match	19.46+	7.84	4.94	18.54	54.74
	All	23.17	10.00	4.94	21.82	73.88
Women	Overeducated	26.01*	9.70	4.86	24.56	75.78
	Correctly Educated	20.59	7.14	5.56	20.18	66.72
	Undereducated	19.66*	6.52	8.36	18.38	43.80
	Major Mismatch	24.41	9.60	4.86	22.72	75.78
	Major Match	21.07+	6.88	5.56	20.38	49.16
	All	23.49	9.06	4.86	21.96	75.78

Note: * Indicates a statistically different mean from correctly educated workers at the 1% level; + indicates a statistically different mean from major match workers at the 1% level.

Table 1.3: Mismatch Examples of Select Worker-Occupation Pairs in the NLSY97

					Highest Level Score			
Person	Skill Mismatch Index	Over-educated	Major Mismatch	**Occupation**	Category	Level Score	Credit Hours Earned	**Major**
1	16.9	X	√	Network and Computer Systems Administrator	Computers and Electronics	96	9	Social Sciences
2	14.8	X	X	Financial Manager	Economics and Accounting	77	13	Business, Management, Marketing, and Related Support Services
3	17.2	X	X	Financial Manager	Economics and Accounting	77	18	Business, Management, Marketing, and Related Support Services

Table 1.4: Means and Standard Deviations of Variables Used in Wage Regressions

	Men			Women		
	High skill mismatch (>p50)	Low skill mismatch (= or < p50)	All	High skill mismatch (>p50)	Low skill mismatch (= or < p50)	All
Dependent variable						
Log of average hourly wage	2.69*	2.87	2.78+	2.60*	2.76	2.68
	(0.61)	(0.61)	(0.62)	(0.55)	(0.57)	(0.56)
Independent variables						
ASVAB scores:						
General science	0.31	0.34	0.33+	-0.01*	0.06	0.03
	(0.72)	(0.75)	(0.74)	(0.71)	(0.69)	(0.70)
Arithmetic reasoning	0.38*	0.45	0.42+	0.12*	0.16	0.14
	(0.76)	(0.74)	(0.75)	(0.76)	(0.71)	(0.73)
Work knowledge	0.16*	0.08	0.12+	-0.02*	-0.12	-0.07
	(0.73)	(0.74)	(0.74)	(0.75)	(0.74)	(0.74)
Paragraph comp.	0.37	0.37	0.37	0.40*	0.33	0.36
	(0.73)	(0.72)	(0.72)	(0.65)	(0.69)	(0.67)
Numerical operations	20.34	20.08	20.21+	19.69	19.77	19.73
	(6.04)	(6.08)	(6.06)	(5.30)	(5.26)	(5.28)
Coding speed	7.66	7.60	7.63+	8.26	8.14	8.20
	(3.19)	(3.16)	(3.17)	(2.94)	(2.87)	(2.90)

Continued

Table 1.4 continued

Auto information	-0.82*	-0.75	-0.78+	-1.07	-1.07	-1.07
	(0.53)	(0.57)	(0.56)	(0.43)	(0.44)	(0.44)
Shop information	-0.55	-0.52	-0.53+	-0.92*	-0.99	-0.96
	(0.60)	(0.64)	(0.62)	(0.50)	(0.51)	(0.51)
Mathematics knowledge	0.69	0.71	0.70+	0.59*	0.65	0.62
	(0.86)	(0.80)	(0.83)	(0.83)	(0.78)	(0.80)
Mechanical comp.	0.04	0.05	0.05+	-0.30*	-0.35	-0.32
	(0.62)	(0.71)	(0.67)	(0.57)	(0.57)	(0.57)
Electronics comp.	-0.19*	-0.14	-0.17+	-0.63*	-0.69	-0.66
	(0.77)	(0.80)	(0.78)	(0.60)	(0.62)	(0.61)
Assembling objects	0.14	0.14	0.14+	0.11	0.11	0.11
	(0.92)	(0.85)	(0.89)	(0.81)	(0.81)	(0.81)
Mother's highest grade completed	14.55	14.57	14.56+	14.09*	13.84	13.96
	(2.35)	(2.59)	(2.47)	(2.59)	(2.99)	(2.80)
1 if mother employed (when resp is age 16)	0.74	0.73	0.74	0.71*	0.75	0.73
1 if English is primary language (1997)	0.98*	0.95	0.96+	0.98*	0.97	0.97
Family Structure (age 16)						
1 if live with both parents	0.80*	0.76	0.78+	0.73*	0.68	0.71
1 if live with mother only	0.09*	0.15	0.12+	0.16	0.17	0.16
1 if live with mother and Partner	0.05	0.04	0.04+	0.07	0.06	0.07

Continued

Table 1.4 continued

1 if live with father only	0.05*	0.02	0.03	0.03*	0.05	0.04
1 if Hispanic	0.10*	0.13	0.12+	0.08*	0.13	0.10
1 if black	0.13	0.12	0.12+	0.14*	0.17	0.15
1 if Associate degree	0.13	0.15	0.14	0.12*	0.17	0.15
Age at receipt of Bachelor’s degree	23.00*	23.34	23.17+	22.72*	22.98	22.85
	(1.73)	(2.04)	(1.90)	(1.73)	(1.95)	(1.85)
College grade point average	2.62	2.61	2.62+	2.84	2.81	2.82
	(0.69)	(0.64)	(0.67)	(0.59)	(0.58)	(0.59)
1 if cohabiting	0.12	0.11	0.11+	0.14	0.14	0.14
1 if married	0.24	0.22	0.23+	0.30*	0.25	0.27
1 if children	0.18	0.17	0.17+	0.27*	0.21	0.24
Hours worked per week	36.49*	37.89	37.19+	32.05*	33.67	32.86
	(15.72)	(14.82)	(15.29)	(14.8)	(15.03)	(14.94)
Tenure	2.36*	2.55	2.45+	2.11*	2.27	2.19
	(2.40)	(2.78)	(2.6)	(2.26)	(2.33)	(2.30)
Pre-degree experience	3.96	4.10	4.03	3.86*	4.10	3.98
	(2.44)	(2.65)	(2.55)	(2.27)	(2.49)	(2.38)
Experience	3.63	3.57	3.60+	3.23*	3.48	3.36
	(3.08)	(2.86)	(2.97)	(2.78)	(2.81)	(2.80)

Continued

Table 1.4 continued

1 if urban	0.87*	0.90	0.88	0.89	0.89	0.89
1 if reside in northeast	0.17	0.17	0.17+	0.16	0.15	0.16
1 if reside in south	0.31*	0.36	0.34	0.34	0.35	0.35
1 if reside in west	0.19*	0.25	0.22	0.23	0.23	0.23
Number of obs.	2316	2317	4633	3313	3324	6637

Note: * Indicates a statistically different mean in the variable between high and low skill mismatch at the 5% level; + indicates a statistically different mean in the variable between men and women at the 5% level. Additional regressors include dummy variables for calendar year, tenure squared, and experience squared. Excluded variables are “1 if reside in north central” and “1 if other family structure.”

Table 1.5: Coefficient Estimates for Mismatch Variables for Alternative Specifications, by Gender

Mismatch Measure	Men				Women			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
1 if overeducated	-0.234**			-0.126**	-0.124**			-0.086**
	(0.035)			(0.036)	(0.027)			(0.029)
1 if undereducated	-0.219**			-0.204**	-0.092*			-0.085
	(0.054)			(0.055)	(0.043)			(0.044)
1 if major mismatch		-0.211**		-0.139**		-0.092**		-0.056*
		(0.033)		(0.033)		(0.026)		(0.027)
Skill mismatch index			-0.011**	-0.009**			-0.007**	-0.006**
			(0.001)	(0.001)			(0.001)	(0.001)
N	4633	4633	4633	4633	6637	6637	6637	6637
AIC Criterion	7792	7755	7208	7055	10107	10053	9506	9412

Note: ** significant at 1% level, * significant at 5% level. Each column corresponds to a separate specification. Standard errors (clustered at the individual level) are in parenthesis. The dependent variable is the log of the CPI-U-deflated, average hourly wage. Coefficient estimates of other regressors can be found in table A.6.

Table 1.6: Coefficient Estimates for Over/Under Skill Mismatch and Subject Specific Skill Mismatch Index, by Gender

Mismatch Measure	Men		Women	
	(1)	(2)	(1)	(2)
Over skill mismatch	-0.008**		-0.006**	
	(0.001)		-0.001	
Under skill mismatch	-0.004*		-0.002	
	(0.001)		-0.001	
Management/communication skill mismatch		-0.001*		-0.001*
		(0.001)		(0.001)
STEM skill mismatch		-0.003**		-0.001*
		(0.001)		-0.001
Arts/humanities skill mismatch		-0.001		0.000
		(0.001)		(0.000)
Social science skill mismatch		0.001		-0.002*
		(0.001)		(0.001)
N	4633	4633	6637	6637
R^2	0.199	0.182	0.179	0.170

Note: ** significant at 1% level, * significant at 5% level. Each column corresponds to a separate specification. Standard errors (clustered at the individual level) are in parenthesis. The dependent variable is the log of the CPI-U-deflated, average hourly wage. Coefficient estimates of other regressors can be found in table A.7.

Table 1.7: Coefficient Estimates for Skill Mismatch Index, by Gender, for Cross-Sectional Samples

Mismatch Measure:	Men		Women	
	One Year After Graduation	Five Years After Graduation	One Year After Graduation	Five Years After Graduation
Skill mismatch index	-0.010** (0.003)	-0.013** (0.003)	-0.008** (0.002)	-0.007* (0.003)
N	558	375	755	490
R^2	0.296	0.282	0.206	0.262

Note: ** significant at 1% level, * significant at 5% level. Each column corresponds to a separate specification based on a one-year and five-year cross-section. Standard errors are in parenthesis. Coefficient estimates of other regressors can be found in table A.8.

Chapter 2. Measuring the STEM Wage Premium Among College Graduates

2.1 Introduction

Wage benefits associated with training in Science, Technology, Engineering and Mathematics (STEM) have garnered significant interest in the U.S., with agencies including the National Center for Education Statistics (NCES), U.S. Department of Commerce, and National Science Foundation (NSF) publishing regular studies on the topic.[28] While researchers consistently find that STEM training has a positive association with wages, the magnitude of the estimated wage premium varies. For example, Noonan (2017) finds that STEM majors receive a 12% wage premium while Kinsler and Pavan (2015) find that science degrees are associated with a 24% wage premium. The lack of consensus on the magnitude of the estimated STEM wage premium can reflect differences across studies in sample composition and estimation techniques, but it can also be influenced by how STEM is measured.

In this chapter, I assess the sensitivity of the estimated STEM wage premium to two distinct dimensions of STEM measurement. First, I consider the definition of STEM; i.e., which fields are considered to be STEM. While certain fields, such as mathematics and engineering, are common to any STEM definition, there is no consensus as to

---

[28] See https://nces.ed.gov/programs/raceindicators/indicator_reg.asp for NCES data on STEM degrees, https://nsf.gov/nsb/sei/edTool/index.html for the NSF STEM Education Resource Center, and https://www.commerce.gov/news/fact-sheets/2017/03/stem-jobs-2017-update for STEM updates from the U.S. Department of Commerce.

whether other fields (e.g., health care, agriculture and natural resources, economics) should be considered STEM (Noonan 2017, U.S. Department of Education 2017). Second, I consider the distinction between dichotomous and continuous measures of STEM. It has long been the norm to consider STEM as a “yes/no” distinction (Beede et al. 2011, Graham and Smith 2005, Jiang 2017, Noonan 2017). However, Light and Rama (2019) (hereafter referred to as LR) depart from this convention in proposing a non-dichotomous “STEM-intensity of coursework” measure. They measure the percentage of total college credit hours contributed by STEM courses which ranges, obviously, from 0 to 100. I systematically introduce changes in both dimensions of STEM measurement (while using a uniform sample and estimation method) to assess their effects on the estimated STEM wage premium.

Throughout my analysis, I use a uniform sample of college graduates drawn from the 1997 National Longitudinal Survey of Youth (NLSY97). I use three published lists of STEM fields developed by the NCES, NSF, and U.S. Immigration and Customs Enforcement (ICE) to construct three alternative, dichotomous STEM measures and three analogous continuous measures based on the LR “STEM-intensity of coursework” approach. Although some researchers construct their own list of STEM fields, I focus on three lists published by government agencies because they are used in related studies and effectively capture differences across virtually all definitions. I separately analyze men and women due to well-known gender differences in major choice (Robst 2007a) that relate to the decision to acquire STEM training.

I use ordinary least squares to identify the STEM wage premium, controlling for a rich array of covariates and inserting one of the three dichotomous or continuous measures of STEM training into the log-wage model. Because evidence suggests that the three continuous STEM measures have a nonlinear relationship with log-wages, I also estimate the STEM wage premium using a quartile specification, where dummies that indicate the worker's quartile in the gender specific (and definition specific) STEM-intensity of coursework distribution are used. I compare the coefficient estimates across measures to draw conclusions about the sensitivity of the STEM wage premium based on the STEM measure used.

I begin the analysis by comparing the three dichotomous STEM measures. The log-wage premium associated with STEM ranges from 0.09 to 0.18 for men and 0.03 to 0.13 for women; this spread suggests that the estimated STEM wage premium under the dichotomous measure is sensitive to the definition of STEM (i.e., the list of fields included in STEM), particularly when STEM=1 expands beyond "hard" sciences (engineering, mathematics, etc.) to include "soft" sciences (economics, psychology, etc.).

However, this sensitivity is not apparent under the continuous STEM measure. For men, using the linear specification the estimated log-wage premium associated with a 10 percentage point increase in STEM-intensity of coursework is 0.05 for all three definitions. Using the quartile specification, the estimated log-wage premium associated with moving from quartile 1 to quartile 4 of the STEM-intensity of coursework distribution is 0.22 for all three definitions. For women, there is some sensitivity to the definition used for the continuous measure: depending on the definition, the linear specification produces an estimated log-wage premium ranging from 0.025 to 0.038 for a 10 percentage point increase, while the estimated log-wage premium for the quartile specification ranges from 0.078 to 0.111 when moving from quartile 1 to quartile 4. Nonetheless, this sensitivity is less than what is found when using the dichotomous measure.

As the preceding discussion suggests, the results are highly sensitive to whether the dichotomous or non-dichotomous measure of STEM is used. For men, the estimated log-wage benefit of shifting from low (quartile 1) to high (quartile 4) levels of STEM training under the non-dichotomous measure exceeds the estimated log-wage benefit of shifting from STEM=0 to STEM=1 under the dichotomous measure by 0.046 to 0.133 log-points. This reflects the fact that a quartile 1-to-4 comparison in the STEM-intensity of coursework distribution isolates a more pronounced gap in marketable skill than does the comparison between relatively heterogeneous pools of STEM majors and non-STEM majors. Similar to LR, I conclude that compared to the non-dichotomous measure, the dichotomous measure underestimates the wage benefit associated with completing high levels of STEM training. Further, because sensitivity to the STEM definition depends on the dichotomous/non-dichotomous distinction while sensitivity of the dichotomous/non-dichotomous distinction depends less on the definition used, I conclude that differences in the STEM wage premium are primarily driven by the method used to isolate workers with the highest echelon of STEM training rather than by the fields/courses included as STEM (NCES vs. ICE vs. NSF).

I find that the magnitude of the estimated STEM wage premium is consistently smaller for women than for men across definitions and the dichotomous/non-dichotomous distinction. This is mainly due to women pursuing less STEM-intensive curriculums (e.g., women in quartile 4 of the gender-specific STEM-intensity of coursework distribution are less STEM-intensive then are men in quartile 4) and completing STEM majors that garner relatively lower premiums (e.g., men are more likely than women to major in engineering, which tends to have higher wages than other hard sciences). Under the dichotomous measure, the gender gap in the estimated STEM premium grows as the definition of STEM broadens. This is not the case under the non-dichotomous measure, because as the definition of STEM broadens to include soft sciences, women are *not* more likely to complete relatively more STEM coursework than men, even though they are more likely to be classified as a STEM major.

2.2 Literature Review

I begin by discussing existing estimates of the STEM wage premium that are based on a dichotomous measure of STEM but utilize different definitions of STEM. Each study uses either the NCES, ICE, or NSF list of STEM fields referred to in the introduction, or a slight variation on one of these lists. I describe each list in detail in section 2.3.2. For now, it is important to note that the NCES list of STEM fields is the narrowest and the NSF list is the broadest.

I focus first on studies that rely on the NCES definition of STEM, where only traditional “hard” sciences (engineering, mathematics, biology, etc.) are classified as

STEM. Kinsler and Pavan (2015) use a “science degree” classification, which includes all majors from the NCES list of STEM fields except mathematics. Using data from the Baccalaureate and Beyond Longitudinal Study (collected in 1993 with follow-ups in 1994, 1997, 2003), they initially estimate a wage premium of 23.9% (compared to workers with a non-science and non-business degree) using ordinary least squares; they examine only men and control for demographic characteristics as well as SAT scores (pre-college ability) and major-specific grade point average (GPA). The authors also develop a structural model of major choice and labor market outcomes, with which they estimate a wage premium of 20.2% for science degrees. Beede et al. (2011) and Noonan (2017) classify STEM majors in a manner similar to the NCES definition.[29] Beede et al. (2011) use data from the 2009 American Community Survey to estimate a log-wage model via ordinary least squares, and control for observables such as age, education attainment, and region of residence; they estimate the STEM wage premium to be 12% for men and 9% for women. Noonan (2017) draw their sample from the 2015 American Community Survey; using ordinary least squares (controlling for demographic and geographic characteristics), they estimate the STEM wage premium to be 13% for women and 11% for men. Cataldi et al. (2014) use the NCES list to identify STEM majors among a pooled sample of men and women from the 2012 Baccalaureate and Beyond data. They find a 31% unconditional difference in the median earnings of full-

---

[29] The majors classified as STEM in Beede et al. (2011) and Noonan (2017) match the NCES list, except the two studies broadly allows for “life sciences” majors which is more inclusive than the analogous “biology and biomedical sciences” under the NCES definition. Beede et al. (2011), Noonan (2017), and Kinsler and Pavan (2015) do not explicitly indicate that they use the “NCES definition” of STEM fields. However, the fields they list as STEM closely match the fields on the NCES list.

time workers who completed a STEM major ($65,000) compared to non-STEM majors ($49,500).

Funk et al. (2018) classify STEM majors similar to the ICE list; in addition to the traditional “hard” sciences, this list also classifies other fields, such as health, as STEM (see section 2.3.2). For a pooled sample of men and women from the 2014-2016 American Community Survey, they find a 33% unconditional difference in the median STEM degree earnings ($81,011) compared to the median non-STEM degree earnings ($60,828). Baird et al. (2017) use a pooled sample of men and women from the 2015 American Community Survey. Using U.S. Census Bureau major codes, they develop a list of STEM fields which is similar to the NSF list (i.e., a list, broader than the NCES or ICE lists, that includes fields such economics, social sciences, psychology). They find an unconditional difference of 19.6% in the hourly wages for workers with a STEM bachelor’s degrees ($37.67 per hour) compared to workers with a non-STEM bachelor’s degree ($31.50). As this summary of the literature demonstrates, existing studies utilize different definitions of STEM, data sources, samples, and estimation strategies—and produce a wide range of estimates of the STEM wage premium.[30]

Other studies examine the wage premium of specific college majors, although they do not explicitly group majors into STEM. Such studies often find that traditional STEM majors have the greatest wage premium, although other majors that may be

---

[30] Several other studies examine labor market outcomes related to STEM training although they do not explicitly identify the percent difference in earnings for STEM majors compared to non-STEM majors. For example, Jiang (2017) uses the ICE definition for STEM and finds that men (women) in STEM majors and occupations earn $9,925 ($13,651) more than those in non-STEM majors and occupations. Graham and Smith (2005) study the gender differential in Science and Engineering (S&E); the list of S&E fields they use is similar-to the NCES list. From the summary statistics it can be discerned that men (women) with both an S&E degree and job unconditionally earn 3.1% (20.0%) more than those without both an S&E degree and job.

considered STEM in some lists but not others are also found to have substantial wage benefits. Altonji et al. (2014) construct a sample of workers from multiple data sources (collected during 1976 to 2011) and, using ordinary least squares, find that chemical and electrical engineering majors have the highest estimated returns for men.[31] However, fields such as finance and economics also have high estimated returns — even higher than some traditional STEM fields such as mathematics, physics, and earth and other physical sciences. Similar results are found for women with differing magnitudes. Altonji et al. (2012) use data from the 2009 American Community Survey to estimate the returns to majors. Using ordinary least squares (controlling for ethnicity, race, degree, and potential experience) they note that the estimated log-wage gap between general education and electrical engineering (0.561) is close to the gap between college and high school graduates (0.577) for men. Finance (0.518), economics (0.517), accounting (0.431), and nursing (0.408) also have high estimated returns, not unlike traditional STEM fields such as mathematics (0.426). They find similar results for women, but with differing magnitudes. Hamermesh and Donald (2008) use a sample of University of Texas at Austin graduates. After adjusting for non-response bias and conditioning on background and ability variables, they find that engineering and "hard" business (finance, actuarial sciences, business engineering) are the highest-paying majors for a pooled sample of men and women; these majors pay roughly three times that of the lowest-paying major (education). As a group, these studies show that there is variation in the

---

[31] The authors standardize the major fixed effects (mean = 0 and standard deviation = 1); the earnings premium for chemical engineering is 1.90, electrical engineering is 1.48, finance is 1.42, economics is 1.40, mathematics is 0.71, physics is 0.69, and earth and other physical sciences is 0.07.

estimated wage returns even among majors that are traditionally considered STEM (e.g., engineering vs. mathematics) and high estimated returns to some majors that are not STEM (e.g., finance) or that are defined as STEM only in certain lists (e.g., economics, actuarial sciences, nursing). This suggests that the fields included in the STEM definition play an important role in explaining the differing estimated STEM wage premium.

Instead of using a dichotomous measure of STEM, Light and Rama (2019) (LR) define STEM on a continuum to identify the estimated wage benefits of STEM training. Using the NLSY97, LR construct a "STEM-intensity of coursework" measure, calculated as the percentage of total credit hours earned through STEM coursework (based on a match between NLSY97 postsecondary transcript course codes and the ICE list of STEM fields). This measure enables the authors to differentiate a non-STEM major who completes a high proportion of STEM courses from another respondent in the same major who does not take many STEM courses. In fact, the authors demonstrate that there is high variation in the STEM-intensity of coursework among both STEM and non-STEM majors. The authors also construct an analogous "STEM-intensity of occupation" measure, where they sum occupation-specific scores (from the Occupation Information Network database) that indicate the knowledge required for seven STEM skill areas (mathematics, engineering, etc.) as a percentage of the maximum possible score in these seven skill areas. Due to nonlinear patterns in the data, LR replace the continuous STEM-intensity variables with quartile indicators that better fit the data. After controlling for a rich array of covariates (including family background and pre-college ability test scores), they estimate a substantial wage premium for workers in the 4th quartile of both STEM-intensity distributions (0.443 for men and 0.321 for women) using ordinary least squares. Additionally, LR provide evidence that a dichotomous measure of STEM (also based on the ICE list) understates the value of completing a high-intensity STEM curriculum for men and women.

Despite the lack of consensus on the wage premium associated with STEM training, no prior study has asked how much of the variation is attributable to the definition of STEM. Further, aside from LR, no prior study has considered the value of departing from the standard “yes/no” (dichotomous) measure of STEM. This study tackles both issues in a unified framework that eliminates variation in data source, sample, model specification and estimation method.

2.3 Data

I use data from the National Longitudinal Survey of Youth 1997 (NLSY97). The NLSY97 is a U.S. based survey that collects data on a sample of 8,894 young men and women born between 1980 and 1984. Respondents were between the ages of 12 and 18 during the first-round interview in 1997. Respondents were interviewed annually until 2011 (rounds 1-15) and biennially from 2013 (round 16) onward. Data are currently available through 2015-2016 (round 17).

An important feature of the NLSY97 is that college transcript data are available for many respondents who attended post-secondary institutions. This information was collected by the Post-Secondary Transcript Study (PSTRAN) in 2012-2013; post-secondary transcripts were obtained from universities attended by respondents who signed a waiver. Data on college courses and majors contained in the transcripts were coded using the 2010 College Course Map (CCM), a system for coding post-secondary education courses developed by the NCES. The coursework and major data in the college transcripts are critical to the construction of both the dichotomous and continuous measures of STEM.

2.3.1 Sample Selection

The sample selection process follows chapter 1 (see table 1.1 for complete details), and produces a sample consisting of respondents with a bachelor's degree, Armed Services Vocational Aptitude Battery (ASVAB) test scores (discussed in section 2.3.3), college transcript data, and a valid post-college wage. The final sample has 1,313 respondents with 11,270 wage observations; 755 women have 6,637 wage observations and 558 men have 4,463 wage observations.

2.3.2 Measuring STEM

I consider two dimensions of STEM measurement: (1) fields that are classified as STEM and (2) dichotomous or continuous measure of STEM.

Dimension 1: Determining fields that are STEM

I utilize three published lists developed by government agencies that define what fields are considered to be STEM. Each published list follows or can be converted (using a crosswalk) to the CCM taxonomy used to code the NLSY97 transcript data. I confine my attention to these three lists because they are used in the related literature and capture relevant differences across definitions of STEM.

The first list was produced by the National Center for Education Statistics (NCES), a federal entity located within the U.S. Department of Education's Institute of Education Sciences that collects and analyzes data related to education. The NCES regularly releases studies on post-secondary education, including analyses related to STEM degrees. The following 2-digit CCM codes (majors) are classified as STEM based on the NCES list: biological and biomedical sciences, computer and information sciences, engineering and engineering technologies, mathematics and statistics, and physical sciences and science technologies.[32] A large number of college majors that fall into these aggregate fields are classified as STEM, including chemistry, astronomy, physics, geology, etc., but this classification excludes a number of highly technical majors (economics, geographic information systems, environmental sciences, etc.) in the social sciences and health fields.

The second list was developed by U.S. Immigration and Customs Enforcement (ICE), a federal agency under the jurisdiction of the Department of Homeland Security (DHS) that enforces immigration laws. International students who attend college on an F-1 visa can qualify for the "24-Month STEM Opt Extension" which provides the option of engaging in practical training in the U.S. after they complete their degree. To be eligible, students must have completed a degree from the ICE "STEM Designated Degree Program" list. Although DHS drew upon the NCES list of STEM fields, it modified the

---

[32] The NCES list of STEM degrees can be found at https://nces.ed.gov/programs/raceindicators/indicator_reg.asp

list to provide a "straightforward and objective measure" for designated school officials to identify STEM fields (U.S. Department of Homeland Security, 2016). In contrast to the NCES list, ICE lists a very large number of 6-digit CCM codes that are classified as STEM; each code includes a 2-digit subject area followed by a 4-digit course title.[33] The list includes many of the same STEM fields in the NCES list as well as several additional 6-digit CCM codes including econometrics, environmental sciences, and geographic information sciences.

The third list comes from the National Science Foundation (NSF), an independent federal agency that promotes science and research. The "STEM Education Data" webpage is a resource developed by the NSF that compiles their related research for individuals to learn more about STEM training at all education levels. Their list of STEM fields includes the ones on the NCES list as well as additional 2-digit CCM codes (majors) such as psychology and social sciences.[34]

Dimension 2: Dichotomous and continuous measures of STEM training

For the dichotomous measure of STEM, workers are categorized as having completed either a STEM major or a non-STEM major. Because there are three published lists that determine what fields are STEM, I construct three dichotomous STEM measures. For the NCES and NSF lists, this entails a simple match between the 2-digit

---

[33] ICE's STEM Designated Degree Program List can be found at https://www.ice.gov/sites/default/files/documents/Document/2016/stem-list.pdf. Additional details on the 24-Month Opt STEM extension can be found at https://www.ice.gov/sevis/practical-training

[34] The NSF list of STEM degrees can be found at https://www.btaa.org/docs/default-source/diversity/nsf-approved-fields-of-study.pdf?sfvrsn=1bc446f3_2

CCM codes on the list and the 2-digit CCM code of the respondent's major provided by the NLSY97. The ICE list provides 6-digit CCM codes that they classify as STEM (a 2-digit subject area followed by a 4-digit course title). Following LR, if the majority of the 4-digit course codes listed under the corresponding 2-digit subject area in the CCM taxonomy are also on the ICE list, then that 2-digit subject area is classified as a STEM major. The STEM major lists build on each other: all STEM majors included in the NCES list are also included in the ICE and NSF lists, and all STEM majors included in the ICE list are also on the NSF list.

The continuous measure of STEM is the STEM-intensity of coursework measure developed by LR. Unlike the dichotomous measure, where the respondent has either completed a STEM major or not, this continuous measure assigns a numerical value that reflects the intensity of STEM in the respondent's training. This is calculated as the credit hours earned in STEM courses as a percentage of all credit hours earned by the respondent.[35] Again, because there are three published lists that determine what fields are STEM, I construct three measures of STEM-intensity of coursework. For the NCES and NSF definitions, all courses taken by the respondent for which the subject area (the first 2-digits of the 6-digit CCM code) can be found on the NCES and NSF lists are classified as STEM. For example, NCES considers the major/subject area "biological and biomedical sciences" to be STEM so all courses taken by the respondent in this subject

---

[35] For the STEM-intensity of coursework measure, I only include credit hours earned for courses that began prior to when the respondent completed his bachelor's degree. I do not include remedial and failed courses. The number of credit hours earned for each course is normalized by dividing by the mode number of credit hours earned by the respondent at the university. I do this to ensure that the credit hours of each respondent are on the same scale since the number of credit hours earned per course can vary depending on university attended by the respondent.

area contribute to the STEM-intensity of coursework measure. The ICE list provides 6-digit CCM codes, which not only indicate the subject area but also the course title. Thus, only courses completed by the respondent that have a 6-digit CCM code found on the ICE list are classified as STEM courses.

In contrast to the dichotomous measure—where an increasing number of respondents are classified as STEM=1 when I switch from the NCES list to the ICE list to the NSF list—the continuous measure is not necessarily smallest when I use the NCES list because of the distinction between 2-digit and 6-digit codes. For example, the NCES list (which consists of 2-digit CCM codes) implicitly includes all courses under the 2-digit “Computer and Information Sciences” as STEM courses, whereas the ICE list of 6-digit codes only includes a subset of courses in that field; thus, courses like “Computer Logic and/or Digital Logic” are considered STEM under the NCES definition, but are excluded from the ICE list of STEM courses. Nonetheless, the ICE list is relatively broader than the NCES list. Both the NCES and ICE lists are narrower than the NSF list.

2.3.3 Other Variables

The dependent variable in my regression analysis is the natural log of the average hourly wage, CPI-U deflated to 2006 dollars. In addition to the STEM measures described in 2.3.2, I use a range of covariates in the regression analysis. Pre-college variables include race and ethnicity indicators, mother’s highest grade completed, and dummies that indicate whether the respondent’s mother was employed when the respondent was 16 years old, whether English was the respondent’s primary language in 1997, and the respondent’s family structure at age 16 (i.e., if the respondent lived with both parents, lived with the mother and her partner, lived only with the mother, lived only with the father, or another family structure). I also include scores for the 12-Armed Services Vocational Aptitude Battery (ASVAB) sub-tests. The ASVAB is an aptitude test completed by respondents in 1997-1998, at ages 12-18. The 12 ASVAB sub-tests are general science, arithmetic reasoning, word knowledge, paragraph comprehension, mathematics knowledge, electronics information, auto information, shop information, auto and shop information, mechanical comprehension, numerical operations, and assembling objects. I use scores based on item response theory that were constructed by NLSY97 staff and allow for direct comparisons across respondents.

The baseline controls also include the following in-college variables: a dummy that indicates whether the respondent completed an associate degree, age at graduation, college grade point average (GPA) (which I compute based on transcript data), and years of labor market experience from age 16 to college graduation. The post-college variables include dummies that indicate if the respondent is married, is cohabiting, resides in an urban area, and has children. The following are also post-college variables in the baseline controls: region of residence dummies (i.e., resides in the south, west, or northeast), year dummies, years of labor market experience after college graduation and its square, years of tenure and its square, and average hours of work per week. All post-college controls are time-varying and are measured at the time the wage is earned. Table 2.1 provides the means and standard deviations for variables used in the regression analysis.

2.3.4 Descriptive Comparison of Alternative STEM Measures

In table 2.2, I show the distribution of the six alternative STEM training measures by gender. When using the (narrowest) NCES definition, 21% of men completed a STEM major.[36] In comparison, 26% are STEM majors under the ICE definition and 42% are STEM majors under the (broadest) NSF definition. The 21 percentage point spread across STEM major definitions demonstrates that the dichotomous STEM measure is sensitive to the list used to define STEM.

There is a 28-percentage point spread across definitions for women: 8% are STEM majors under the NCES definition, 15% under the ICE definition, and 36% under the NSF definition. Although the percentage of women who are STEM majors is less than men for all three definitions, the gender gap narrows as the definition of STEM broadens; i.e., men are 2.6 times more likely than women to be STEM under the NCES definition, 1.7 times more likely under the ICE definition, and 1.2 times more likely under the NSF definition. This indicates that women are far less likely than men to be drawn into the STEM = 1 designation when only hard sciences are included as STEM majors; only when soft sciences are included as STEM majors does the proportion of women in STEM=1 begin to rival that of men.

Table 2.2 also shows that the continuous STEM-intensity of coursework measure is sensitive to which fields are considered STEM. For men, as the definition of STEM broadens from the NCES list to the ICE list to the NSF list, the mean STEM-intensity of

---

[36] Following LR, I use the administrative transcript data from the NLSY97 to identify the respondent's major. However, unlike LR, when a respondent's major was not clearly identified through the transcript data, I use the self-reported major indicated by the respondent as part of an interview.

coursework (along with other points in the distribution) increases. For men, all three definitions yield similar standard deviations and interquartile ranges: the difference between p25 and p75 is 29.0 under the NCES definition, 30.1 under the ICE definition, and 35.6 under the NSF definition. Thus, the *distribution* of the continuous measure does not appear to be sensitive to the definition used.

There are two important differences for women. First, women have a lower mean STEM-intensity of coursework compared to men across all three definitions. However, unlike the dichotomous measure, as the definition broadens the gender difference under the non-dichotomous measure scarcely shrinks (from 7.6 percentage points under the NCES list to 4.7 under the ICE list to 2.7 under the NSF list). This is because, compared to men, women are *not* more likely to complete relatively more STEM coursework as the definition of STEM expands to include more courses, even though they are more likely to be classified as a STEM major as the definition broadens to include the softer sciences. Second, although the standard deviation is roughly similar across definitions, unlike what is seen for men the interquartile range differs across definitions, from 10.8 percentage points under the NCES definition to 18.4 under the ICE definition to 30.5 under the NSF definition. Thus, as the definition broadens, the STEM-intensity of coursework increases for both genders, but the distribution remains the same for men while the distribution becomes less skewed for women.

In table 2.3, I report the mean log-wage for various subsamples to examine how the unconditional wage benefit of STEM training differs across definitions. In the top panel, I segment the sample based on the dichotomous measure. Focusing first on men, the top row of table 2.3 shows that the mean log-wage for STEM majors common to all three lists (math, engineering, etc.) is 2.92. As the definition of STEM broadens to the ICE list, STEM majors (e.g., health, agriculture) with a substantially lower mean log-wage (2.80) are introduced into the pool of STEM majors.[37] When the definition of STEM further broadens to the NSF list, the softer science majors (e.g., social sciences, psychology) introduced into the pool of STEM majors have an even lower mean log-wage (2.74).[38] Thus, broadening the STEM major definition from the NCES to the ICE to the NSF list results in respondents with comparatively lower wages becoming STEM=1. In the bottom row, the mean log-wage of majors that are STEM=0 under all three definitions (history, foreign language, etc.) is 2.75; surprisingly, this is not significantly different than the mean (2.74) for STEM majors introduced under the NSF list. In other words, majors who are STEM=1 only on the NSF list do not have an unconditional wage benefit.

Table 2.3 reveals similar patterns for women, although there are two differences compared to men. First, for women, the mean log-wage of non-STEM majors for all three definitions is significantly *higher* than STEM majors introduced under the NSF list (2.68 compared to 2.61), suggesting an unconditional wage penalty for women in the softer sciences classified as STEM=1. Second, women have a lower mean log-wage compared

---

[37] Because the ICE list is broader than the NCES list, respondents who are STEM = 1 in the ICE list can be either STEM = 1 on the NCES list (e.g., engineering and math majors) or STEM = 0 on the NCES list (e.g., health and agriculture majors). In table 2.3, I compare respondents who are STEM on both the ICE and NCES lists (e.g., engineering and math majors) to the respondents who are STEM on the ICE list but not the NCES list (e.g., health and agriculture majors).

[38] The difference in the mean log-wage for STEM majors common to all three lists (2.92) and STEM majors introduced in the ICE list (2.80) are significantly different at the 1% level. The difference in the mean log-wage for the STEM majors introduced in the ICE list (2.80) and the STEM majors introduced in the NSF list (2.74) are significantly different at the 1% level.

to men for all four sample segments. However, the gender difference varies substantially across segments, with gaps of 0.11 among STEM=1 under the NCES definition (which is confined to hard sciences) and 0.13 for the new STEM=1 under the NSF definition (which includes softer sciences), but only 0.01 for the new STEM=1 majors under the ICE definition (which includes health, agriculture, etc.). This points to gender differences in wages of traditional hard sciences (NCES definition) and soft sciences (only in the NSF definition), but not for "in-between" majors such as health and agriculture that are first introduced in the ICE definition.

The bottom panel of table 2.3 compares the mean log-wage across definitions for each quartile of the STEM-intensity of coursework distribution. For men, the mean log-wage is lowest in quartile 1 for all three definition and within 0.02 log-points of each other definition. Compared to quartile 1, the mean log-wage for quartile 2 in all three definitions is 0.04 to 0.07 log-points higher; it is within 0.03 log-points of each other definition. Thus far, patterns in the data are similar across definitions. However, compared to quartile 2, the mean log-wage for quartile 3 is substantially higher under the NCES (by 0.07 log-points) and ICE (by 0.10 log-points) lists, but remains the same under the NSF list. This suggests there is no unconditional wage benefit for quartile 3 under the NSF list while there is a substantial benefit under the other two lists.[39] The differences in quartile 3 are corrected in quartile 4, as all three definitions have a mean log-wage of roughly 2.90; this is a substantial increase from quartile 3 (by 0.11 log-points in the

---

[39] As the definition of STEM broadens from only including courses in the hard sciences on the NCES list to include additional technical fields found on the ICE list, there is an increase in the mean log-wage for quartile 3; however, as courses from the soft sciences are included as STEM (as is the case under the NSF definition), there are now respondents shifting into quartile 3 that bring down the mean log-wage.

NCES list, 0.07 log-points in the ICE list, and 0.15 log-points in the NSF list). Thus, the lack of an increase in the mean log-wage from quartile 2 to 3 that occurs only under the NSF list is compensated by an increase from quartile 3 to 4 that is greater than the other lists. Overall, there is a substantial wage increase in the shift from quartile 2 to 3 only under the NCES and ICE lists, and another wage increase in the shift from quartile 3 to 4 under all three lists.

The patterns for women differ than those for men. For the NCES and NSF lists the mean log-wage in quartile 1 is roughly the same as quartile 2, while the mean log-wage is *lower* in quartile 3 (by 0.06 log-points). The mean log-wage in quartile 4 is 2.78, which is substantially higher than that of quartile 3 (by 0.17 log-points). In contrast, for the ICE list, the mean log-wage in quartile 1 is the lowest and increases in increments of 0.02 (to quartile 2), 0.04 (to quartile 3), and 0.10 (to quartile 4) log-points. Although there are differences in the patterns for women across definitions, the mean log-wage for quartile 4 is 2.78 and substantially higher than that of quartile 1 for all three lists.

2.4 Estimation Strategy

To estimate the STEM wage premium, I use the following specification:

$$\mathrm{logW}_{\mathrm{it}} = \beta_1 ST_i + \beta_2 X_{it} + \varepsilon_{it} \qquad (2.1)$$

where the dependent variable is the log of the average hourly wage for individual *i* at time *t*, X_{it} represents the baseline controls (discussed in section 2.3.3), and ST_i is the measure of STEM training. The main parameter of interest is β_1, which represents the

STEM wage premium. Specification 2.1 is estimated using ordinary least squares, separately for men and women.

I begin the analysis by considering the six alternative (three dichotomous and three continuous) measures for ST_i in specification 2.1. Following the same process as LR, I experimented with various functional forms and found (a) nonlinear patterns in the relationship between the continuous measures and log-wage; and (b) that quartile indicators best reflect patterns in the data for all three definitions of STEM.[40] Thus, I also examine an alternative to specification 2.1, where I replace ST_i with dummies that indicate the worker's quartile in the gender- and definition-specific STEM-intensity of coursework distribution.

2.5 Results

Table 2.4 presents estimates of the STEM wage premium using alternative measures of STEM training. Within gender, each panel refers to a separate regression that was used to produce the parameter estimate (or set of three parameter estimates) corresponding to a unique combination of STEM list (NCES/ICE/NSF) and STEM measure (dichotomous/non-dichotomous). After examining how the results change when varying the definition of STEM using the three published lists, I compare the results of the dichotomous measure to that of the non-dichotomous measure.

---

[40] Results of the Ramsey regression specification error test suggest that a linear functional form is not the correct specification. However, introducing squared or cubic terms (or even interactions) does not fit the data as well as the quartile indicators. LR expand upon the failure of these alternatives and provide evidence that the quartile indicators implemented both in LR and the current study are robust.

I begin by examining the estimates for men. The first panel (top row) of Table 2.4 presents the estimates of specification 2.1 using the dichotomous measure of STEM training. The estimated wage benefit of STEM=1 is 0.177 under the NCES definition, 0.154 under the ICE definition, and 0.087 under the NSF definition; the first two estimates are not statistically distinguishable, but the latter is significantly smaller than the others at a 1% significance level. Because the NCES and ICE estimates are roughly twice the magnitude of the NSF estimate, it is clear that inferences about the wage benefit of STEM training are highly sensitive to the definition used *when STEM is measured dichotomously*. As noted in section 2.3.4, the NSF list includes social sciences and psychology majors that pay, on average, less than “hard science” STEM majors and roughly the same as the remaining non-STEM majors. Because these “soft science” majors make up a large proportion of STEM majors under the NSF definition, we see a substantial decrease in the estimated STEM wage premium when they are included as STEM.

In the second panel (second row) of table 2.4, I present estimates of specification 2.1 using the continuous measure of STEM training. The estimated log-wage premium associated with a 10-percentage point increase in STEM-intensity of coursework is 0.045 under the NCES definition, 0.046 under the ICE definition, and 0.043 under the NSF definition; each estimate is statistically distinguishable from zero, but statistically *indistinguishable* from each other. In contrast to what was found when using the dichotomous measure, the estimated STEM wage premium is entirely insensitive to the definition of STEM when STEM is measured non-dichotomously. This is because, as seen in table 2.2, the spread of the STEM-intensity of coursework distribution remains roughly the same when the STEM definition broadens.

A more nuanced comparison of the first and second panels in table 2.4 reveals that the estimated wage benefit of shifting from low to high levels of STEM training is *understated* in the dichotomous measure compared to the continuous measure. For example, the 21% of STEM majors identified by the NCES definition can be likened to the 79th percentile of the STEM-intensity of the coursework distribution—and 0.211 is the estimated wage premium of a shift from the average STEM-intensity of coursework of men below the 79th percentile (for whom 13.2% of coursework is STEM) to the average STEM-intensity of coursework of men above the 79th percentile (for whom 60.1% of coursework is STEM). This is higher than 0.177 (the estimated STEM coefficient under the NCES definition), which represents the estimated wage premium of a shift from the average of men with STEM = 0 to the average of men with STEM = 1.

Because there are nonlinear patterns in the relationship between the continuous STEM measures and log-wage, I present an alternative to specification 2.1 in the third panel, where STEM-intensity of coursework is replaced with dummies that indicate the worker’s quartile in the gender- and definition-specific STEM-intensity of coursework distribution. For all three definitions, the estimated log-wage premium for high levels of STEM training (a shift from quartile 1 to quartile 4), is 0.22. This reaffirms the finding from the second panel that the non-dichotomous measure is not sensitive to the definition used.[41]

Comparing the results of the first and third panels in table 2.4 reaffirms the finding that the estimated wage benefit of shifting from low to high levels of STEM training is *understated* by the dichotomous measure compared to the non-dichotomous measure. This is because shifting from quartile 1 to quartile 4 of the STEM-intensity of coursework distribution (using the non-dichotomous measure) results in an estimated STEM wage premium that is higher than shifting from STEM=0 to STEM=1 (using the dichotomous measure) by 0.046 log-points under the NCES definition, 0.061 log-points under the ICE definition, and 0.133 log-points under the NSF definition.[42] This comparison suggests that the estimates are highly sensitive to the dichotomous/non-dichotomous distinction. This sensitivity reflects the fact that male workers in quartile 4 of the non-dichotomous distribution are more “select” (i.e., more STEM-intensive) than are men for whom STEM=1 under the dichotomous definition. Further, as additional fields are classified as STEM, the pool of men in STEM=1 (dichotomous measure) continues to broaden while the narrow pool of men in quartile 4 (non-dichotomous

---

[41] However, it is important to note that some of the underlying patterns differ across definitions. Under both the NCES and ICE definitions, the 0.22 log-wage premium is the result of a significant, incremental increase from quartile 2 to quartile 3 and another significant, but smaller incremental increase from quartile 3 to quartile 4. In contrast, under the NSF definition, the incremental change from quartile 2 to quartile 3 is not significant and the lion’s share of the estimated payoff is associated with the move from quartile 3 to quartile 4. As the definition of STEM broadens from the NCES and ICE lists to the NSF list with the inclusion of softer sciences, men in the fourth quartile see increases in their STEM-intensity of coursework that are similar to, if not greater than men elsewhere in the distribution (thus keeping them in the highest echelon of STEM training even when the quartile cut offs and STEM-intensity of coursework values are derived from the NSF list) while men in the third quartile do not consistently have similar increases in their STEM-intensity of coursework (thus displacing them to lower quartiles when the quartile cut offs and the STEM-intensity of coursework values are derived from the NSF list).

[42] The differences are calculated using the results row 1 and row 3. For example, under the NCES definition, the wage premium of STEM Major = 1 (dichotomous measure) is 0.177 and the wage premium of STEM-intensity of coursework quartile 4 (non-dichotomous measure) is 0.223, resulting in a difference of 0.046 log-points

measure) remains static; the latter occurs because as the definition of STEM expands, the STEM-intensity of coursework increases for men throughout the distribution, leaving the identity of men in quartile 4 largely unchanged. We saw that sensitivity to the definition used depends heavily on the dichotomous/non-dichotomous distinction, but here we see that the sensitivity of the dichotomous/non-dichotomous distinction does not depend on the definition used. In other words, sensitivities in the results for men are primarily driven by the method used to isolate workers with the highest echelon of STEM training (i.e., the dichotomous/non-dichotomous distinction) rather than the fields/courses included as STEM (i.e., the hard sciences in the NCES definition vs. the introduction of the soft sciences in the NSF definition).

Turning to estimates for women in table 2.4, we see three main gender differences. First, the STEM wage premium is consistently lower for women than for men, regardless of the STEM list used or whether STEM is measured dichotomously or non-dichotomously. Under the dichotomous measure, the estimated STEM wage premium for women is 15% to 72% less than men, in part because women pursue different STEM majors from men, that increasingly have lower premiums as the definition of STEM broadens.[43] Under the continuous measure (second panel), the estimated STEM wage premium for women is 17% to 41% less than men, in part because women have less STEM-intensive curriculums; in particular, women in quartile 4 tend to

---

[43] For example, among STEM=1 under the NCES definition, 67% of men are computer science or engineering majors compared to only 33% of women and, among majors that are STEM=1 only under the NSF definition, 55% of men are social science majors compared to 33% of women. Previous studies have demonstrated that estimated returns differ by college major (see section 2.2), and these differences seem to grow when shifting away from the hard sciences towards the soft sciences; e.g., Altonji et al. 2014, after standardizing the major fixed effects, find the wage premium of engineering, chemistry, biology, mathematics, physics etc. ranges from 0.69 to 1.9 while the wage premium of economics, psychology, agricultural sciences, etc. has a much larger range from -1.0 to 1.4.

be “less STEM” than their male counterparts (e.g., table 2.2 shows that the 75th percentile STEM-intensity of coursework under the NCES definition is twice has high for men as for women). As a result, we also see that under the non-dichotomous measure (third panel of table 2.4), the estimated wage premium associated with moving from quartile 1 to quartile 4 is 48% to 64% smaller for women than for men.[44]

Although it is noteworthy to compare the estimated wage premium of quartile 4 over quartile 1 across genders in the third panel, this comparison must be qualified because the quartiles are based on gender-specific STEM-intensity of coursework distributions. Thus, in the fourth panel of table 2.4 I present estimates for the STEM wage premium using dummies that indicate the worker’s quartile in the definition specific, but *not* gender specific, STEM-intensity of coursework distribution. Even in the fourth panel, we see that the quartile 4 estimates for women remain 32% to 47% lower than the estimates for men.[45] Thus, the results show a gender difference in the estimated STEM wage premium that remains consistent across definitions under the non-dichotomous measure but grows as the definition broadens under the dichotomous measure.

---

[44] It should also be noted that the underlying pattern from the shift to quartile 1 to quartile 4 of the NCES and ICE definitions differ by gender. For men, this premium is the result of a significant incremental shift from quartile 2 to 3 and quartile 3 to 4 whereas for women, it is solely the result of a significant incremental shift from quartile 3 to quartile 4. This is because women pursue less STEM-intensive curriculums then men; table 2.2 shows that the threshold for quartile 4 is substantially lower for women compared to men. Thus, quartile 3 and quartile 4 both capture STEM-intensive workers for men but only quartile 4 will do the same for women.

[45] Many of the trends noted in the third panel are present in fourth panel. However, one notable difference is that the quartile 4 estimates for men are not the same across definition in the fourth panel as they were in the third panel. For example, the NCES quartile 4 estimate (0.179) is lower than the other two definitions (0.216 under the ICE list and 0.209 under the NSF list). Because women pursue less training in the hard sciences, men previously in quartile 3 in the gender-specific quartile cutoffs get shifted into quartile 4 when using the entire sample of men and women in determining the quartile cutoffs. This makes the pool of quartile 4 broader and dilutes it with workers who have comparative lower STEM-intensities, thus leading to a lower estimate.

Second, there is more sensitivity in the estimates for women than for men. Under the dichotomous measure, both genders are sensitive to the definition used, with the NSF definition leading to substantially smaller estimates than the NCES and ICE definitions. However, under the non-dichotomous measure, men have the same estimates across definitions while women's estimates exhibit some sensitivity: in the second panel, for example, the ICE definition results in an estimate of 0.038, which is somewhat higher than the estimates (0.030 and 0.025) under the NCES and ICE definitions; a similar pattern is seen in the third panel. As the STEM definition expands from the NCES list to the ICE list, women do not see an increase in the estimated wage premium under the STEM=1 designation but they do see an increase in the estimated wage premium associated with STEM-intensity of coursework. This suggests that the additional STEM coursework included under the ICE definition (for the non-dichotomous measure) is associated with higher wages that are not brought to bear under the dichotomous definition.

Third, under the NCES and ICE definitions the estimated wage premium is greater for women under the dichotomous measure (0.129 and 0.130, respectively) than under the non-dichotomous measure (0.089 and 0.111, respectively). Because women pursue relatively less STEM-intensive curriculums, quartile 4 in the non-dichotomous measure becomes *less* exclusive compared to STEM=1 under the dichotomous measure; e.g., under the NCES definition 25% of women are in quartile 4 but only 8% of women are STEM majors. However, examining the results of the second panel shows that, compared to the continuous measure, the dichotomous measure *understates* the STEM wage premium for women. For example, the 8% of STEM majors identified by the NCES definition can be likened to the 92nd percentile of the STEM-intensity of the coursework distribution—and 0.149 is the estimated wage premium of a shift from the average STEM-intensity of coursework of women below the 92nd percentile (for whom 12.3% of coursework is STEM) to the average STEM-intensity of coursework of women above the 92nd percentile (for whom 62.1% of coursework is STEM). This is higher than 0.129 (the estimated STEM coefficient under the NCES definition), which represents the estimated wage premium of a shift from the average of women with STEM = 0 to the average of women with STEM = 1.

Overall, comparing the results of men and women demonstrates that inferences about gender differences are affected by which workers are classified as STEM. This, in turn, depends on whether we focus on workers with the highest competition rates of STEM coursework or simply workers with a STEM major (STEM =1), and also on whether the STEM definition includes the hard sciences only, as in the NCES definition, or is extended to include soft sciences as under the NSF definition.

2.6 Conclusion

In this chapter, I use transcript data from the NLSY97 to construct alternative measures of STEM training and I examine the extent to which the estimated STEM wage premium is sensitive to the measure used. I vary the measure of STEM training by, first, alternating between three government lists that identify STEM fields and, second, using either a dichotomous measure (STEM major equal to 0 or 1) or a continuous measure (the STEM-intensity of coursework). The major findings for men are as follows. First, the dichotomous STEM measure is sensitive to the definition used: broadening the definition of STEM beyond the traditional “hard” sciences (engineering, mathematics, etc.) to include other technical and science-based fields (health, etc.) has minimal impact on the estimated STEM wage premium, but further expanding the definition to include the “softer” sciences (social sciences, psychology, etc.) results in substantially lower estimates. In contrast, the STEM-intensity of coursework measure is less sensitive to the definition used: estimates in the linear specification are not significantly different across definitions and estimates in the preferred quartile specification maintain similar patterns across definitions; for example, the estimated STEM wage premium associated with the highest (quartile 4) STEM-intensity is not significantly different across definitions. Second, when comparing the dichotomous and non-dichotomous STEM measures, the results for all three definitions indicate the dichotomous measure underestimates the wage benefits of completing a curriculum with high STEM-intensities for men– a finding also found by LR. Because the sensitivity of the definition used depends on the dichotomous/non-dichotomous distinction while the sensitivity of the dichotomous/non-dichotomous distinction depends less on the definition used, I conclude that the sensitivities of the estimated STEM wage premium are driven by the dichotomous/non-dichotomous distinction rather than the definition used. Collectively, the results point towards the robustness of the STEM-intensity of coursework measure but not the STEM major measure.

The simplicity of this analysis has allowed us to learn about the importance of the STEM training measures used in estimating the STEM wage premium. Given the revelations related to both the sensitivity of the dichotomous/non-dichotomous distinction, the literature would benefit from additional research using the non-dichotomous measure. LR has already conducted a study examining both STEM occupation and coursework intensities. Additional studies could directly examine gender differences in the estimated STEM wage premium, analyze the demand for STEM training in the labor market, assess the factors that determine choice of STEM training, or estimate the STEM wage premium by further distinguishing types of STEM training (e.g., separating the components of STEM training by each subject area or course type).

.

Table 2.1: Means and Standard Deviations of Variables Used in Wage Regressions

	Men	Women
Dependent variable		
Log of average hourly wage	2.78*	2.68
	(0.62)	(0.56)
Independent variables		
ASVAB scores:		
General science	0.33*	0.03
	(0.74)	(0.70)
Arithmetic reasoning	0.42*	0.14
	(0.75)	(0.73)
Work knowledge	0.12*	-0.07
	(0.74)	(0.74)
Paragraph comprehension	0.37	0.36
	(0.72)	(0.67)
Numerical operations	20.21*	19.73
	(6.06)	(5.28)
Coding speed	7.63*	8.20
	(3.17)	(2.90)
Auto information	-0.78*	-1.07
	(0.56)	(0.44)
Shop information	-0.53*	-0.96
	(0.62)	(0.51)
Mathematics knowledge	0.70*	0.62
	(0.83)	(0.80)
Mechanical comprehension	0.05*	-0.32
	(0.67)	(0.57)
Electronics comprehension	-0.17*	-0.66
	(0.78)	(0.61)
Assembling objects	0.14*	0.11
	(0.89)	(0.81)
Mother's highest grade completed	14.56*	13.96
1 if mother employed (when resp was age 16)	0.74	0.73
1 if English is primary language (1997)	0.96*	0.97

Continued

Table 2.1 continued

Family Structure (age 16)		
1 if live with both parents	0.78*	0.71
1 if live with mother only	0.12*	0.16
1 if live with mother and partner	0.04*	0.07
1 if live with father only	0.03	0.04
1 if Hispanic	0.12*	0.10
1 if black	0.12*	0.15
1 if Associate degree	0.14	0.15
Age at receipt of Bachelor's degree	23.17* (1.90)	22.85 (1.85)
College grade point average	2.62* (0.67)	2.82 (0.59)
1 if cohabiting	0.11*	0.14
1 if married	0.23*	0.27
1 if children	0.17*	0.24
Hours worked per week	37.19* (15.42)	32.86 (14.95)
Tenure	2.45* (2.60)	2.19 (2.30)
Pre-degree experience	4.03 (2.55)	3.98 (2.39)
Experience	3.60* (2.97)	3.36 (2.80)
1 if urban	0.88*	0.89
1 if reside in northeast	0.17*	0.16
1 if reside in south	0.34*	0.35
1 if reside in west	0.22*	0.23
Number of observations	4,633	6,637

Note: *Significantly different for men and women at the 5% level. Additional regressors include dummy variables for calendar year, tenure squared, and experience squared.

Table 2.2: Distribution of STEM Measures by Gender

Gender	List	Measure	N	Mean	Sd	Min	P25	P50	P75	Max
Men	NCES list	STEM major	558	0.21						
		STEM-intensity of coursework	558	25.04	22.57	0.00	8.63	14.96	37.57	84.03
	ICE list	STEM major	558	0.26						
		STEM-intensity of coursework	558	29.60	20.75	0.00	13.04	22.64	43.08	83.74
	NSF list	STEM major	558	0.42						
		STEM-intensity of coursework	558	46.34	21.25	6.90	28.95	43.23	64.55	100.00
Women	NCES list	STEM major	755	0.08						
		STEM-intensity of coursework	755	17.43	16.92	0.00	7.27	11.71	18.06	87.79
	ICE list	STEM major	755	0.15						
		STEM-intensity of coursework	755	24.89	18.15	0.00	12.44	19.35	30.85	87.79
	NSF list	STEM major	755	0.36						
		STEM-intensity of coursework	755	43.62	19.72	2.41	27.91	41.38	58.39	100.00

Note: “STEM major” is an indicator that equals one if the respondent completed a STEM major and zero otherwise. “STEM-intensity of coursework” is the percentage of total credit hours the respondent earned in STEM courses. Qualifying fields and/or courses for STEM major and STEM-intensity of coursework are determined based the corresponding published list (NCES, ICE, NSF). All variables are time-invariant, and the statistics presented in this table include only one observation per respondent. p25, p50, and p75 refer to 25th, 50th, and 75th percentile.

Table 2.3: Mean and Standard Deviation of Log-wage by STEM Definition by Gender

		Men				Women			
				Log-wage				Log-wage	
Measure	List	Resp.	Obs.	Mean	Sd	Resp.	Obs.	Mean	Sd
Dichotomous	STEM=1 in NCES, STEM=1 in ICE, STEM=1 in NSF	118	877	2.92	0.60	57	443	2.81	0.62
	STEM=0 in NCES, STEM=1 in ICE, STEM=1 in NSF	26	228	2.80	0.50	60	547	2.79	0.51
	STEM=0 in NCES, STEM=0 in ICE, STEM=1 in NSF	90	814	2.74	0.62	154	1356	2.61	0.56
	STEM=0 in NCES, STEM=0 in ICE, STEM=0 in NSF	324	2714	2.75	0.62	484	4291	2.68	0.56
Non-Dichotomous	Quartile 1 in NCES list	134	1168	2.69	0.62	182	1671	2.67	0.57
	Quartile 1 in ICE list	136	1172	2.67	0.64	167	1678	2.62	0.54
	Quartile 1 in NSF list	132	1160	2.69	0.64	167	1660	2.67	0.58
	Quartile 2 in NCES list	129	1153	2.73	0.62	173	1650	2.67	0.58
	Quartile 2 in ICE list	123	1148	2.73	0.57	175	1643	2.64	0.58
	Quartile 2 in NSF list	126	1159	2.76	0.59	185	1661	2.66	0.54
	Quartile 3 in NCES list	134	1154	2.80	0.59	192	1663	2.61	0.54
	Quartile 3 in ICE list	140	1164	2.83	0.63	201	1660	2.68	0.55
	Quartile 3 in NSF list	137	1157	2.76	0.61	195	1661	2.61	0.55
	Quartile 4 in NCES list	161	1158	2.91	0.61	208	1653	2.78	0.55
	Quartile 4 in ICE list	159	1149	2.90	0.60	212	1656	2.78	0.56
	Quartile 4 in NSF list	163	1157	2.91	0.60	208	1655	2.78	0.57

Table 2.4: Estimated STEM Wage Premium

	Men			**Women**		
		STEM list			STEM list	
STEM measure	NCES	ICE	NSF	NCES	ICE	NSF
1 if STEM major	0.177**	0.154**	0.087*	0.129*	0.130**	0.024
	(0.045)	(0.041)	(0.035)	(0.057)	(0.037)	(0.027)
STEM-intensity of coursework	0.045**	0.046**	0.043**	0.030**	0.038**	0.025**
	(0.008)	(0.009)	(0.009)	(0.008)	(0.008)	(0.006)
STEM-intensity of coursework quartile (with gender specific cut-offs)						
1 if quartile 2	0.013	0.045	0.070	-0.009	-0.016	-0.046
	(0.046)	(0.045)	(0.050)	(0.036)	(0.036)	(0.036)
1 if quartile 3	0.138**	0.176**	0.073	-0.083*	0.044	-0.046
	(0.047)	(0.050)	(0.046)	(0.034)	(0.034)	(0.034)
1 if quartile 4	0.223**	0.215**	0.220**	0.089*	0.111**	0.078*
	(0.048)	(0.050)	(0.050)	(0.035)	(0.033)	(0.035)
STEM-intensity of coursework quartile (no gender specific cut-offs)						
1 if quartile 2	-0.006	0.055	0.067	-0.024	0.008	-0.028
	(0.049)	(0.048)	(0.051)	(0.034)	(0.035)	(0.034)
1 if quartile 3	0.118*	0.177**	0.074	-0.037	0.024	-0.045
	(0.047)	(0.051)	(0.049)	(0.034)	(0.032)	(0.034)
1 if quartile 4	0.179**	0.216**	0.209**	0.107**	0.147**	0.110**
	(0.046)	(0.048)	(0.049)	(0.039)	(0.036)	(0.037)
N	4633	4633	4633	6637	6637	6637

Note: ** and * indicates significance at the 1% level and 5% level, respectively. A separate regression was used to produce each parameter estimate or set of three parameter estimates corresponding to a unique STEM measure/STEM list combination for each gender; in all, twelve regressions were used for each gender. STEM wage premium estimates from specification 2.1 is in the top panel (top row) using the dichotomous measure and second panel (second row) using the continuous measure. STEM wage premium estimates from the alternative to specification 2.1 that uses quartile indicators for the STEM-intensity of coursework measure are in the bottom two panels (non-dichotomous measure); the third panel uses the distribution of each gender to determine the quartiles while the fourth panel uses the entire sample (both men and women) to determine the quartiles. Standard errors (clustered at the individual level) are in parenthesis. Coefficient estimates for other regressors can be found in table B.1 for the NCES list, table B.2 for the ICE list, and table B.3 for the NSF list.

Chapter 3: Education and Job Matching: A Two Cohort Comparison

3.1 Introduction

Beginning with the works of Freeman (1976) and Duncan and Hoffman (1981), researchers have been interested in matches between workers' attained schooling and the schooling required for their jobs. A worker is overeducated (undereducated) if he has completed more (less) schooling than what is required for his job. Overeducation suggests that workers' skills are underutilized, thus making completed educational investments less profitable for the individual and less productive for society (Duncan and Hoffman 1981). In fact, most researchers (including Rumberger 1987; Verdugo and Verdugo 1989; Clark et al. 2017) find a significant wage penalty associated with overeducation. As our society and policies increasingly emphasize the pursuit of higher education, we must grapple with the fact that segments of our educated population may continue to underutilize skills and face a resulting wage penalty.

Despite interest in (and exhaustive research on) this topic, we know little about changes over time in the consequences of overeducation. This is because differences across studies prevent direct comparisons. Meta-analyses and literature reviews (e.g., Groot and Maassen van den Brink 2000; Hartog 2000; and Leuven and Oosterbeek 2011) find varying wage penalties and incidences of overeducation, even with data from the same year; this is due to different data sources, samples, estimation strategies, and methods of defining a job’s education requirements.[46] In this chapter, I systematically compare the wage effects of overeducation and undereducation for two cohorts of college graduates born approximately two decades apart to develop a better understanding of how the consequences of these mismatches have changed over time.

For the analysis, I use data on college graduates in the 1997 National Longitudinal Survey of Youth (NLSY97) and the 1979 National Longitudinal Survey of Youth (NLSY79).[47] The respondents from the NLSY97 were born between 1980 and 1984, and respondents from the NLSY79 were born between 1957 and 1964. I define each respondent’s level of education as his or her highest degree completed and, combining the approaches of Clark et al. (2017) and Abel and Deitz (2015), the level of education required for an occupation as the most common (modal) degree held by workers in that occupation, based on data from the Current Population Survey. I examine respondents during their early career, defined as the first 15 years after college graduation. Because several studies (including McGoldrick and Robst 1996 and García-Mainar 2014) have documented gender differences in overeducation (driven by differences in occupation choice, degree specialization, etc.) and gender roles have evolved over the past few decades, I conduct the analysis separately for men and women.

I begin the analysis by examining the incidence of overeducation and undereducation. The rates of overeducation for men are 42% for the older cohort and

---

[46] For example, Dolton and Vignoles (2000) report 29% of male workers are overeducated from their 1986 sample and Alba-Ramirez (1993) report 15% of male workers are overeducated from their 1985 sample.

[47] The two studies are part of the same National Longitudinal Survey and were designed for cross-cohort comparisons. The NLSY website provides instructions on how to make the data sets comparable. Further, methodology from the NLSY79 was used to inform the NLSY97. The survey questionnaires also suggest a similar approach to data collection for both data sets.

51% for the younger cohort; for women, 44% of the older cohort are overeducated compared to 50% of the younger cohort. Because these cross-cohort findings are based on a uniform methodology, this is the best evidence to date that overeducation has increased substantially over time (by 6-9 percentage points for my timeframe), while the unconditional gender gap in overeducation has remained small, yet reversed sign (i.e., women are two percentage points more likely than men to be overeducated in the 1979 cohort vs. one percentage point *less* likely in the 1997 cohort). In contrast, undereducation is much less common than overeducation and *declining* across cohorts for men and women: the rates for men (women) are 9% (7%) for the older cohort and 4% (5%) for the younger cohort. The unconditional gender gap in undereducation is just as small (1-2 percentage points) as is the gap in overeducation, but the sign reversal is in the opposite direction.

Turning to the main analysis (examining wage penalties associated with overeducation and undereducation for both cohorts), I estimate an overeducation wage penalty of roughly 0.27 during the early career for both men and women, in both the NLSY97 and the NLSY79. This finding of gender and cohort stability is based on a regression that controls for a rich array of observables that does *not* include occupational indicators. However, when occupation dummies are incorporated into the regression model (that is, when education mismatch penalties are identified "within" occupation), gender and cohort differences in the estimated wage penalty of overeducation emerge. Specifically, the estimated wage penalty increases across cohorts by three percentage points (from -0.207 to -0.244) for men and by nine percentage points (from -0.181 to -

0.275) for women, and the gender gap reverses across cohorts (i.e., the penalty is three percentage points larger for men in the 1979 cohort, and three percentage points larger for women in the 1997 cohort). Clearly, the within-occupation wage penalty associated with overeducation has grown across cohorts, especially for men.

For undereducation, the primary finding is that the estimated wage effect—for men *and* women, with *o*r without occupational controls—grew from a small, statistically insignificant, positive effect for the earlier cohort to a large, negative effect for the later cohort. Specifically, the estimated, within-occupation wage penalty to undereducation is 0.050 (0.020) for men (women) in the 1979 cohort (both of which are statistically indistinguishable from zero) and -0.282 (-0.190) for men (women) in the 1997 cohort. As I demonstrate in section 3.5.1, the lack of an undereducation wage penalty for the earlier cohort is attributable to the fact that many undereducated workers in the NLSY79 hold occupations that closely resemble those of correctly educated workers; i.e., undereducated workers are “barely” classified as undereducated.

3.2 Literature Review

In this section, I focus on studies that examine overeducation and undereducation. Overeducation and undereducation refers to situations where a worker has, respectively, more and less education than his occupation requires. In discussing this literature I focus on three issues: (1) how both acquired and required education have been measured and, therefore, how overeducation and undereducation have been defined, (2) how wage effects of overeducation and undereducation have been identified, and (3) what has been done to compare findings across studies. Unless otherwise specified, most studies do not focus solely on college graduates.

Studies use differing methods to define overeducation and undereducation. Some studies use data where respondents self-report whether their education and qualifications match the job's requirements. For example, Chevalier (2003) and Chevalier and Lindley (2009) use data from a U.K.-based survey where respondents assessed the extent to which their qualifications matched their job. However, most studies first devise a method to define the education acquired by the worker and required for the job, before constructing a measure of overeducation and undereducation. For example, Rumberger (1987) defines education acquired by the worker based on years of schooling completed and defines the job's required education based on a professional, objective analysis of the occupation's requirements (found in the Dictionary of Occupation Titles); he then defines overeducation (undereducation) as cases where the years of schooling exceeds (is less than) the required amount. Abel and Deitz (2015) also define the job's required education based on an external analysis of the occupation's requirements (using the Occupation Information Network), but define the education acquired by the worker as his highest degree; overeducation is represented by a dummy variable equal to one if the worker's degree is greater than the required degree. In contrast, Clark et al. (2017) define acquired education by grouping workers into categories based on years of schooling completed (i.e., 12-13 years, 14-15 years, etc.) while education required for the job is based on the modal education completed by workers from a national sample in that occupation; overeducation (undereducation) is then defined by if the schooling attained is higher (lower) than the schooling requirements. Duncan and Hoffman (1981) and Sicherman (1991) use data from the Panel Study of Income Dynamics, where respondents indicated both their level of education and the level of education required to obtain their job; overeducation (undereducation) is defined as having a level of education greater (less) than what is required. Other studies use different combinations of these methods of defining acquired education, required education, and overeducation/undereducation; see Leuven and Oosterbeek 2011, McGuinness 2006, or Sloane 2003 for a literature review.

Studies that assess the wage penalty associated with overeducation or undereducation differ in their specifications, estimation strategies, and controls. The two primary specifications were established by Duncan and Hoffman (1981) and Verdugo and Verdugo (1989). In Duncan and Hoffman (1981), years of required schooling and schooling in excess or deficit of what is required are separately included in a Mincer-type earnings regression. The authors find that the estimated return to a “required year” of schooling is 0.063 (i.e., workers in jobs that require more schooling have higher wages), while the return to an “overeducated year” is 0.029 (i.e., workers with excess schooling earn more than others in their job, but, because 0.029 is less than 0.063, they earn less than those in jobs requiring more schooling) and the return to an “undereducated year” is -0.042; all three estimates are statistically significant at conventional significance levels. This suggests, for example, that college-educated workers in a high school job earn less than college-educated workers in a college job but earn more than high school workers in a high school job.

Several studies, including Allen and van der Velden (2001), also use this approach. In contrast, Verdugo and Verdugo (1989) insert dummies into the wage regression that indicate if the worker is overeducated and undereducated; the amount by which the worker is overeducated or undereducated is not reflected in this approach. They find a statistically significant estimate of -0.130 for overeducated workers and 0.096 for undereducated workers. Abel and Deitz (2015) and Chevalier (2003) implement a similar approach.

Virtually all studies of overeducation/undereducation (including both Duncan and Hoffman 1981 and Verdugo and Verdugo 1989) utilize a linear regression framework, but relatively few address endogeneity concerns (due to effects of unobserved ability, preferences, motivation, and other factors on both overeducation/undereducation and wages). An instrumental variable approach is employed by both Korpi and Tahlin (2009) (using family background variables as instruments) and Dolton and Silles (2008) (using exogenous changes caused by labor market rigidities in the overeducation distribution as instruments). Lindley and McIntosh (2010) use individual fixed effects to absorb the effects of any time-invariant, person-specific unobservables. Some studies control for a rich array of covariates to account for self-selection on ability; for example, Clark et al. (2017) incorporate pre-college ability test scores (AFQT) into their analysis.

In addition to differing in how mismatch is controlled for, existing studies also differ in the set of conditioning factors. Most condition on worker characteristics (age, race, ethnicity, etc.). Studies that focus solely on college graduates often incorporate measures related to the respondent’s field of study; for example, Chevalier (2003) and

Allen and van der Velden (2001) include field of study dummies and Abel and Deitz (2015) incorporates college major match (i.e., a dummy that indicates if the respondent is in an occupation that relates to his major) in their analyses. Controlling for job characteristics is less common, but Duncan and Hoffman (1981) and Sicherman (1991) include indicators for occupation categories and Chevalier (2003) includes indicators for firm size.

Most studies on overeducation and undereducation focus on workers and occupations at all degree levels. Only a few studies examine the wage effects of overeducation and undereducation specifically for college graduates. Abel and Deitz (2015) study college-educated men and women; using OLS and controlling for several covariates (including metro unemployment rate and agglomeration) they estimate a log-wage benefit of 0.244 for a college degree match (meaning the college educated worker is in a “college education” occupation). Chevalier (2003) studies graduates from a U.K. university and, after controlling for a rich array of education and job characteristics, finds that “genuinely” overeducated workers (i.e., overeducated workers who are dissatisfied with their match) have an estimated log-wage penalty of 0.264. Allen and van der Velden (2001) study higher education graduates from Europe and Japan and find that an additional year of overeducation is associated with an 8% decrease in wages.

Additional research has focused on reviewing and comparing studies on overeducation and undereducation. Hartog (2000) compares a large number of empirical studies from several countries, conducted with data spanning two decades; the author assesses differences in methods used to define required education as well as the incidence and wage penalty of overeducation and undereducation. Groot and Maassen van den Brink (2000) conduct a meta-analysis of overeducation studies, concluding that there is no indication of mismatches in education having increased significantly in the past 20 years. Despite the thorough work conducted in both studies, differences in data sources, estimation strategies, and definitions of overeducation make it difficult to compare studies and understand how both the incidence of overeducation and its effect on labor market outcomes have changed over time. The literature can benefit from a direct comparison across time that uses a consistent data source and uniform estimation and sampling strategies. Additionally, most studies rely on cross-sectional data, and are unable to track the evolution of overeducation over an individual's career. Thus, the literature can also benefit from a study that learns about the time-varying effects of overeducation for a single cohort.

3.3 Data

My primary data sources are the 1997 National Longitudinal Survey of Youth (NLSY97) and the 1979 National Longitudinal Survey of Youth (NLSY79). The NLSY97 is a survey of young men and women that began in 1997 with a sample of 8,894 respondents born between 1980 and 1984. Respondents were interviewed annually from 1997 to 2011 and biennially from 2013 onward; data are currently available through the 2015-2016 round. The NLSY79 is a survey of young men and women that began in 1979 with a sample of 12,686 respondents born between 1957 and 1964. Respondents were interviewed annually through 1994 and biennially through 2014. Data were available through the 2014-2015 round when I conducted this analysis. Both the NLSY97 and NLSY79 provide detailed information on schooling attainment and previously held jobs including average hourly wages (computed by NLSY staff) and occupation codes. Additionally, respondents of both the NLSY97 and NLSY79 completed the Armed Services Vocational Aptitude Battery (ASVAB), a multi-part test that measure respondents' aptitudes in various academic and vocational topics. The ASVAB was administered to NLSY97 respondents in 1997-1998 when they were ages 12-18, and to NLSY79 respondents in 1980 when they were ages 15-23. Armed Forces Qualifications Test (AFQT) scores, which are a widely-used measure of pre-college ability, can be constructed with scores on four ASVAB subtests (writing knowledge, mathematical knowledge, arithmetic reasoning, and paragraph comprehension).

3.3.1 Sample Selection

I begin the sample selection for my analysis by only considering the 2,354 respondents in the NLSY97 and the 2,685 respondents in the NLSY79 who have completed a bachelor's degree and have their graduation date available in the data. Among the respondents with a bachelor's degree, I exclude an additional 328 from the NLSY97 and 83 from the NLSY79 who are missing the ASVAB/AFQT score.

For the remaining 2,026 respondents in the NLSY97 and 2,602 respondents in the NLSY79, I keep only post-bachelor's degree wage observations. I terminate the observation period when the respondent re-enrolls in graduate school, is fifteen years past the graduation date, or is last interviewed (whichever comes first). The 15-year cutoff is imposed to increase the comparability of the samples because most respondents in the NLSY79 are observed far longer than NLSY97 respondents.[48] I delete wage observations if the average hourly wage is not between $0.50 and $250 or if the occupation code is not available.

The final sample for the NLSY97 consists of 1,930 respondents (with 15,525 wage observations): 794 are male (with 6,384 wage observations) and 1,136 are female (with 9,141 wage observations). The final sample for the NLSY79 has 2,266 respondents (with 24,671 wage observations): 1,039 are male (with 11,941 wage observations) and 1,227 are female (with 12,730 wage observations). The final NLSY79 sample is about 17% larger than the NLSY97 sample because more respondents were initially surveyed in the NLSY79 then the NLSY97 (12,686 vs 8,894) and because NLSY79 respondents have had far longer to complete college and earn post-college wages. To contend with the fact that "late" observations (6-15 years after college graduation) are much more common in the NLSY79 sample than in the NLSY97 sample, a portion of my analysis is conducted with fixed, uniform experience levels (1 year and 5 years).

3.3.2 Defining Overeducation and Undereducation

The education required for an occupation is determined using the Current Population Survey (CPS), a monthly survey of U.S. households that includes the education level and occupations of respondents.[49] Similar to Clark et al. (2017), I define education required

---

[48] The earliest bachelor's degree in the NLSY97 sample was completed in 2000 and the current round was conducted in 2015-2016.

[49] Unlike earlier chapters in this dissertation, the O*NET database is not used to determine level of education. This is because O*NET was first constructed and released in 1998 with subsequent updates occurring every 6-12 months.

for an occupation by using the modal education acquired by workers in the same occupation in the CPS data.[50,51] However, I modify this approach based on the work of Abel and Deitz (2015) by collapsing the schooling/degree levels into the following three degree categories: (1) less than a bachelor’s degree, (2) a bachelor’s degree, or (3) greater than a bachelor’s degree. The education category that the plurality of CPS respondents belonged to for a given occupation is defined as the required level of education. This approach is based on realized matches because it relies on the education levels of other members of the population who have already matched to the same occupation to determine the occupation’s degree requirements. Hartog (2000) points out that this method does not define required education based on the technological requirements of a job because it measures allocation determined by hiring standards and labor market conditions. Although not ideal, this may have interesting implications in the current study, because labor market conditions and hiring standards, which may have differed when respondents in the NLSY79 and the NLSY97 graduated, are now reflected in the mismatch measures.

While it is useful for the NLSY97 cohort, it is less relevant for the early jobs of the NLSY79 cohort. The precursor to O*NET is the US department of labor funded Dictionary of Occupation Titles (DOT), which had four releases between 1938 and 1991. However, these two databases are fundamentally different sources of occupation descriptors as the DOT is a task-based database while O*NET is a skill-based database. As a result, the way the education-oriented information is collected and presented is different between the databases and thus, not comparable (e.g., DOT identifies “General Education Development” on a scale from 1 to 6, which is a more objective estimate of occupation training requirements made by experts in the field; researchers have developed different methods to convert this scale into years of schooling, none of which are universally accepted. In contrast, O*NET surveys individuals within an occupation on what they think is required for their occupation). Using the CPS data allows for a consistent method of defining the required level of education for an occupation during this vast period of 1980-present.

[50] Occupation is defined using the 1980 and 2000 *three-digit* Census Occupation Classification (the most refined definition of occupation available in the Census taxonomy).

[51] The March CPS data from the year in which the respondent started their occupation is used. Because the occupation taxonomy is often updated, if the data is unavailable or the occupation coding taxonomy differed between the NLSY and the CPS, then the closest year to when the respondent started their occupation that also has CPS data with a matching occupation taxonomy is used.

Based on the required level of education defined above, a respondent in the NLSY97 and NLSY79 is overeducated if his job requires a degree less than a bachelor's degree. Similarly, a respondent is undereducated if his job requires a degree greater than a bachelor's degree and correctly educated if his job requires a bachelor's degree.

3.3.3 Other Variables

The dependent variable is log average hourly wage, CPI-U deflated to 2006 dollars. In addition to the overeducation and undereducation measures described in the previous section, I also use a set of baseline controls in the regression analysis. The variables included in the baseline controls are the same for the NLSY97 and NLSY79 samples and, in most cases, are constructed in a similar manner. The pre-college variables in the baseline controls include dummies that indicate race and ethnicity, mother's highest degree completed, and AFQT score. The in-college variables in the baseline controls include an indicator that the worker completed an Associate's degree, and age at graduation. The baseline controls also include several post-college variables, including dummies that indicate if the respondent is married, is cohabiting, is residing in an urban area, and has any children. Further, region of residence dummies (resides in the south, west, or northeast), year of wage observation dummies, years of labor market experience after college graduation, its square, and its cube, tenure, its square, and its cube, and hours of work per week are included.[52,53]

In addition to the baseline controls, in select specifications I also control for occupation and/or college major dummies.[54] A related body of literature provides compelling evidence that major-specific and occupation-specific factors are related to both wages and overeducation/undereducation. Studies have shown that estimated wage returns vary by major (e.g., Altonjii et al. 2012 and Altonji et al. 2014) and occupation (e.g., Sullivan 2010). Other studies have shown that overeducation relates to majors (e.g., Frenette 2004 finds that overqualification varies considerably by major field at the bachelor degree level) as well as occupations (e.g., Duncan and Hoffman 1981 note substantial differences in overeducation incidence by occupation group and Verdugo and Verdugo 1989 find the estimated returns to overeducation/undereducation vary substantially across occupation groups). If certain occupation groups command a wage premium over others, then it stands to reason that the returns to non-college jobs are closer to college jobs in certain occupation groups but not others. For example, professional and management occupations both contain non-college jobs, yet non-college jobs in these occupation groups may require skilling that make college graduates attractive enough for employers to pay more to hire compared to employers for non-

---

[52] Both the NLSY79 and NLSY97 have a plethora of family background variables, cognitive and non-cognitive test scores, etc. that the other data set does not have. Upon investigating these additional covariates, I found that they did not provide any additional insights. For these reasons, I choose not to include these variables in the current study.

[53] Hours of work per week is the number of hours worked per week at the time of the interview (or at the job's stop date if the respondent is no longer in that job at the time of the interview).

[54] The occupation groupings were determined based off of the Census 2000 one-digit occupation codes. Because some of the earlier NLSY79 occupations are coded using the Census 1980 codes, I map them into these groups as well. The major groupings are based on a taxonomy I created that can be seen in table C.1.

college jobs in other occupation groups. Thus, considering within occupation and within major variation is important in identifying the overeducation wage penalty.

3.3.4 Sample Summary Statistics

Table 3.1 presents means and standard deviations of the dependent and independent variables, by cohort and gender. I begin by examining the rates of overeducation and undereducation. For men, 42% are overeducated in the NLSY79 compared to 51% in the NLSY97; because a consistent approach was used to construct both samples, this change would suggest overeducation has increased substantially (by 9 percentage points) over this time frame. Similarly, for women, 44% are overeducated in the NLSY79 compared to 50% in the NLSY97. Although the (unconditional) gender gap in overeducation is small, the direction of the gap differs as women are two percentage points *more* likely than men to be overeducated in the NLSY79 but one percentage point *less* likely than men to be overeducated in the NLSY97.

In contrast, the incidence of undereducation is substantially lower compared to that of overeducation. For men, 9% in the NLSY79 are undereducated compared to 4% in the NLSY97; for women, 7% in the NLSY79 are undereducated compared to 5% in the NLSY97. Unlike overeducation, the rates of undereducation are declining across cohorts for both genders. Although the (unconditional) gender gap in undereducation is small and similar to that of overeducation, the direction of the gap is reversed; i.e., in the NLSY79, women are more likely to be overeducated but less likely to be undereducated then men and in the NLSY97, women are less likely to be overeducated but more likely to be undereducated then men.

Within gender, the means for most demographic and regional variables are similar for the two cohorts, as is the mean of the dependent variable; the latter is consistent with evidence that real wages among college graduates have not grown over the time period spanned by the two cohorts' early careers.[55] However, there are differences across cohorts in various family/background variables. For example, women in the NLSY79 compared to the NLSY97 are more likely to be married (29% compared to 26%) and to have children (26% compared to 23%), and have lower average AFQT scores (65.6 compared to 67.5). Studies have shown differences across generations that may account for the NLSY79 and NLSY97 differences; for example, Bialik and Fry (2019) find that the percentage of women between ages 25 and 37 who are married is substantially lower among millennials compared to baby boomers.

The data also show a higher percentage of respondents in the NLSY97 completed an Associate degree; this suggests differences between the two cohorts in the path taken to complete bachelor's degree. Compared to the NLSY79, the NLSY97 cohort has a lower mean age at graduation (25 compared to 23 for women), tenure (3 years compared to 2 years for women), and experience (11 years compared to 7 years for women); this is because the observation window is longer for the NLSY79 cohort compared to the NLSY97.[56]

---

[55] Research suggests there has not been a substantial increase in real wages due to purchasing power despite nominal wages increasing. See the Congressional Research Services report: https://fas.org/sgp/crs/misc/R45090.pdf

[56] These differences are an unavoidable result of my decision to examine observations in the early career (up to fifteen years after graduation). Because more NLSY79 respondents than NLSY97 respondents are observed over the entirety

In table 3.2, I present the percentage of respondents who are overeducated, correctly educated, and undereducated for each occupation and college major. I begin by examining the occupation groups in the top panel of table 3.2. While the clerical, services, and farmers, laborers, etc. occupation groups are almost entirely made up of overeducated workers, the management, professional, and sales occupation groups have a mix of overeducated and correctly educated workers. For men in the NLSY79, roughly two-thirds of management occupations are correctly educated and a third are overeducated; in the NLSY97 closer to three-quarters of management occupations are correctly educated and a quarter are overeducated. Similar patterns are present for women. For men in professional occupations, 64.8 percent are correctly educated in the NLSY79 compared to 69.6 percent in the NLSY97; however, 21.2 percent are undereducated in the NLSY79 compared to 9.7 percent in the NLSY97 and only 14.4 percent are overeducated in the NLSY79 compared to 20.7 percent in the NLSY97. These cohort differences in professional occupations are not present for women. Nonetheless, the top panel suggests in most cases, there variation in the mismatch status within occupation group and this differs substantially across cohorts.

In the bottom panel of table 3.2 I examine the overeducation, correctly educated, and undereducation distribution for each college major. For management/communications majors, there is roughly a 50/50 split in the percentage that are overeducated and correctly educated; this is consistent across cohorts and gender.

---

of this time frame, there are imbalances in my sample. Thus, I also examine two cross-sections of the data to ensure the results are robust over time.

However, for STEM majors, 34.5 percent of men in the NLSY79 are overeducated compared to 44.6 percent in the NLSY97 and 10.3 percent of men in the NLSY79 are undereducated compared to 2.6 percent in the NLSY97. This suggests that there are differences in the mismatch distribution within each major group. Although these cohort differences are not present for women in STEM, there are both gender and cohort differences in the mismatch distribution for arts/humanities, social sciences, and other majors.

3.4 Estimation Strategy

My goals are to identify the wage penalties associated with overeducation and undereducation, and assess differences across cohorts and genders. To estimate the wage penalty, I use the following specification established by Verdugo and Verdugo (1989):

$$\log(w_{ijt}) = \beta_1 IO_{ijt} + \beta_2 IU_{ijt} + \beta_3 X_{it} + \varepsilon_{it} \qquad (3.1)$$

where the dependent variable is the log of the average hourly wage for individual *i* in occupation *j* at time *t*. The two independent variables of interest are IO_{ijt} and IU_{ijt}, indicators that equal one if respondent *i* is, respectively, overeducated and undereducated in occupation *j* and zero otherwise; thus, β_1 and β_2 represent the estimated mismatch wage penalty. The vector X_{it} represents a rich array of covariates described in section 3.3.3 (including the baseline controls, occupation fixed effects, and major fixed effects). I estimate equation 3.1 using ordinary least squares, separately for the two cohorts and for each gender; as indicated in section 3.3.3, I first use only baseline controls, then add occupation fixed effects and/or major fixed effects to determine if the wage penalty differs when identified using within occupation and/or within major variation. I also estimate equation 3.1 for two cross-sections of the data to examine if the wage penalty remains consistent throughout the observation window.

3.5 Results

In section 3.5.1, I discuss the estimated wage penalty associated with overeducation and undereducation during the early career, by cohort and gender. In section 3.5.2, I examine the estimated wage penalty associated with overeducation and undereducation for two cross-sections of the data, defined one year and five years after graduation. I compare the estimates between the two cross-sections for each cohort and then compare any differences across cohorts to understand changes in the associated wage penalty during workers' careers.

3.5.1. Overeducation and Undereducation Wage Penalty During the Early Career

I present estimates of the wage penalty associated with overeducation and undereducation during the early career in both cohorts for men in table 3.3 and women in table 3.4. Each column presents a separate regression that was used to produce the two parameter estimates (overeducation and undereducation) corresponding to different set of controls (e.g., baseline controls only, inclusion of occupation group dummies, inclusion of major group dummies, or inclusion of both occupation/major group dummies).

I begin my analysis by examining the overeducation coefficient estimates in column 1; i.e., the results are based on a specification that includes baseline controls only in addition to the over/undereducation indicators. For men, the estimated coefficient for overeducation is -0.271 in the NLSY79 and -0.279 in the NLSY97. Since the estimates only differ by a small and imprecise amount (0.008 log-points), it appears that the estimated wage penalty for overeducation is stable over the time frame examined. This finding is also present for women; i.e., the estimated coefficient for overeducation is -0.278 in the NLSY79 and -0.283 in the NLSY97, a difference of only 0.005 log-points. Further, because the estimated wage penalty is only slightly higher for women than men (a gap of 0.007 log-points in the NLSY79 and 0.004 log-points in the NLSY97), there is gender stability in the results over this time frame. Overall, the results suggest an overeducation wage penalty of roughly 0.27 during the early career for both genders in both cohorts when estimates are based on a regression that controls for a rich array of observables.

Turning to column 2, I introduce occupation group dummies into the regression models. For both cohorts and genders, the estimated wage penalty is lower in column 2 compared to column 1 as some of the variation in wages due to the occupation dummies was previously attributed to the overeducation/undereducation distinction in column 1; the wage penalty changes more for the older cohort than the younger cohort. Unlike column 1, there are substantial cohort and gender differences when the estimated overeducation/undereducation wage penalties are identified “within” occupation. For men (table 3.3), the estimated wage penalty increases by 0.037 log-points (from -0.207 in the NLSY79 to -0.244 in the NLSY97); for women (table 3.4), the estimated wage penalty increases even more by 0.094 log-points (from -0.181 in the NLSY79 to -0.275 in the NLSY97). Thus, the within-occupation wage penalty associated with overeducation (column 2) has grown substantially over this time frame even though the wage penalty identified using total variation (column 1) is stable. Also different from column 1 is a substantial gender gap in the wage penalty (roughly 0.03 log-points for both cohorts); the direction of the gap differs across cohorts, as the estimated overeducation wage penalty for women is higher than for men in the NLSY79 but lower in the NLSY97.

In column 3, I incorporate major group dummies when estimating equation 3.1 to identify the overeducation/undereducation penalties "within" major. Although the estimated wage penalties for men (table 3.3) in both cohorts is lower in column 3 compared to column 1, it is not as low as column 2. For women (table 3.4), the estimates in column 3 remain within 0.006 log-points of the column 1 estimates for both cohorts (although the penalty slightly increases for the NLSY97 and decreases for the NLSY79). The difference in the penalties across cohorts in column 3 are small and imprecise (0.000 for men and 0.004 for women); this is similar to column 1 and suggests that there is stability in the estimated wage penalty over this time frame whether or not estimates are identified using within major variation. However, there are substantial gender differences in column 3 (that are not present in column 1) as the estimated wage penalty is roughly 0.02 log-points higher for women compared to men in both cohorts. Nonetheless, this gap is slightly smaller than that of column 2 (i.e., wage penalty identified "within" occupation).

In column 4, I include both occupation and major group dummies in estimating equation 3.1. From these results, it is clear that there are substantial cohort and gender differences in the overeducation/undereducation wage penalties when identified using within occupation and within major variation. The estimated log-wage penalty increases by 0.031 log-points for men (from -0.199 for the NLSY79 to -0.230 for the NLSY97) and by an even greater 0.075 log-points for women (from -0.203 for the NLSY79 to -0.278 for the NLSY97). Given the results of column 2 and 3, these differences appear to be driven by the inclusion of occupation group dummies (and would suggest some of the variation between major groups overlaps with that of occupation groups). However, unlike columns 1-3 where the gender gap (if any) is roughly the same for both cohorts, there appears to be a more substantial cohort difference in column 4; the estimated wage penalty is only 0.004 log-points higher for women compared to men in the NLSY79 while it is 0.048 in the NLSY97.

I now shift my analysis to examining the undereducation coefficient estimates. Returning to column 1, I estimate equation 3.1 conditioning on only the baseline controls in addition to the over/undereducation dummies. For men (table 3.3), the estimated coefficient for undereducation is (not significant) -0.010 in the NLSY79 and -0.297 in the NLSY97. Unlike overeducation, there is a substantial cohort difference (of 0.287 log-points) in the undereducation wage penalty. This suggests that the undereducation wage penalty has considerably increased over the time frame examined. Further, the direction of the mismatch matters a great deal for the younger cohort unlike the older cohort. The lack of an undereducation wage penalty for the older cohort is attributable to the fact that many undereducated workers in the NLSY79 hold occupations that closely resemble those of correctly educated workers; i.e., undereducated workers in the older cohort are “barely” classified as undereducated. Recall that required education is defined based on whether the plurality of respondents in the CPS had less than, equal to, or greater than a bachelor’s degree for a given occupation. For the occupations of undereducated workers, the average percentage of CPS respondents with a degree greater than a bachelor’s degree is 76% for the NLSY97 and 59% for the NLSY79. This suggests the undereducated occupations for the older cohort were less firm in their undereducation status than that of the younger cohort. Similar qualitative patterns are apparent for women (table 3.4) since the coefficient estimates for undereducation are not significant (albeit positive) in the NLSY79 (0.026) but becomes a substantial negative effect in the NLSY97 (-0.196). However, the gender gap in the wage penalties varies across cohorts; it shifts from 0.036 log-points in the NLSY79 to an even greater 0.101 log-points in the NLSY97. Nonetheless, for both cohorts, the wage penalty is greater for men.

The qualitative patterns for the estimated returns to undereducation discussed in column 1 are apparent when introducing occupation fixed effects (column 2), major fixed effects (column 3) or both (column 4) to the regression. For all three columns, the undereducation wage penalty for men (table 3.3) remains small and imprecise (albeit positive) for the NLSY79 and changes by 0.333 (column 2), 0.252 (column 3), or 0.296 (column 4) log-points to a substantial wage penalty for the NLSY97. Likewise, for women (table 3.4), the undereducation wage penalty is small and imprecise (and still positive) for the NLSY79 and changes by 0.210 (column 2/3) or 0.199 (column 4) log-points to a substantial wage penalty for the NLSY97. The gender gap in the (not significant) undereducation estimates vary across columns for the NLSY79; the inclusion of the occupation group dummies results in undereducated men having a greater wage benefit (by 0.031 log-points) while the inclusion of major group dummies is the reverse (women have a greater wage benefit of 0.018 log-points). However, for the NLSY97, men consistently have a larger wage penalty associated with undereducation that considerably narrows from 0.092 log-points (column 2) when including the occupation group dummies to 0.059 log-points (column 3) when including the major group dummies and 0.053 (column 4) when including both sets of fixed effects.

3.5.2. Overeducation and Undereducation Wage Penalty One Year and Five Years After Graduation

Because respondents in the NLSY79 are observed for a longer period after college graduation than are respondents in the NLSY97, in the previous section I restricted both panels to wage observations in the first 15 years after graduation to enhance comparability. However, not all respondents in either cohort are observed for the entirety of this 15-year window, due to survey attrition in both cohorts and the fact that many NLSY97 respondents are too young to reach the 15-year milestone prior to the last (2015-16) interview. To ensure that my findings are not driven by differences between the two samples in the proportion of observations associated with “low” versus “high” experience levels, in this section I examine two cross-sections defined at experience levels of one and five years. To clarify, all respondents, regardless of cohort, are one year (five years) beyond the college graduation date in the first (second) cross-section. Table 3.5 presents the estimated overeducation and undereducation effect for cross-sections of the data.

For men, the estimated wage penalty associated with overeducation for one year after graduation is -0.222 for the NLSY79 and -0.235 for the NLSY97; this is, respectively, 0.023 log-points greater and 0.005 log-points smaller than the estimated wage penalty for the entire sample (found in table 3.3). In contrast, the estimated wage penalty for five years after graduation is the same as the entire sample for the NLSY79 and smaller by 0.009 log-points for the NLSY97. This suggests that the overeducation wage penalty decreases over time (e.g., the wage penalty is greater one year compared to five years after graduation). This is consistent overeducation being indictive of a bad match (quality), resulting in such workers changing jobs sooner to improve their matches (Sicherman, 1991); this would inevitably reduce the wage penalty over time.[57] However, because the gap between the two cohorts is smaller for each cross-section (e.g., 0.013 log-points one year and 0.022 log-points five year after graduation) compared to the entire sample (e.g., 0.031 log-points in table 3.3), it appears that the unbalanced sample has some influence in the magnitude of the estimates found in table 3.3 but not enough to alter the qualitative patterns noted in the previous section.

Similar patterns are present for women: the wage penalty slightly decreases from one year to five years after graduation for both the NLSY79 (from -0.204 to -0.195) and

---

[57] It should be noted that the decrease in the overeducation wage penalty over time is at odds with the slight (not significant) increase in the skill mismatch index wage penalty over time discussed in chapter 1 (where I explained the wage penalty may increase due to non-pecuniary factors impacting occupation selection). However, because overeducated workers can have high skill mismatches and correctly educated workers can have low skill mismatches, it is plausible that factors impacting occupation selection that drive up the skill mismatch index wage penalty can occur simultaneously with the decrease in the overeducation wage penalty.

the NLSY97 (from -0.284 to -0.246); the estimate for the entire sample falls in this range for both cohorts (found in table 3.3). Because in each cohort there are greater changes in the wage penalty for one gender (men in the NLSY79 and women in the NLSY97) but relative stability for the other gender, the gender gap narrows from one year after graduation (0.013 for the NLSY79 and 0.049 for the NLSY97) to five years after graduation (0.004 for the NLSY79 and 0.025 for the NLSY97); the gender gap of the entire sample (0.004 for the NLSY79 and 0.048 for the NLSY97) falls in this range.[58]

Turning to the undereducation wage penalty, for both genders in the NLSY79, the estimates are not significant for both cross-sections and the entire sample. Nonetheless, because the magnitude of the estimates vary (e.g., for men in both cross-sections the wage benefit is greater than that of the entire sample and for women there is a wage benefit one year after graduation but a penalty five years after graduation), the gender gap fluctuates (0.011 one year after graduation and 0.130 five years after graduation) even though the returns to undereducation are consistently greater for men.

For the NLSY97, the coefficient estimate for undereducation changes from (significant) -0.275 one year after graduation to a (not significant) -0.110 five years after graduation, suggesting the undereducation wage penalty decreases to a not significant or less substantial amount in later years.[59] However, for women, the estimated wage penalty

---

[58] In the case of the NLSY79, women have a lower wage penalty than men in both cross-sections but the reverse is true for the entire sample. This occurs because other cross-sections show that the wage penalty is lower for men than women and women tend to have low fluctuations in their rates rather than the consistent and sharp downward trend men have.

[59] Although the estimate for the entire sample is less than that of both cross-sections, it should be noted that other earlier cross-sections (e.g., two years and three years after graduation) have substantially greater wage penalties. In general, it appears that the undereducation wage penalty is significant and substantial the first few years after graduation, but becomes smaller and, in some cases, imprecise several years after graduation.

for undereducation is relatively consistent at -0.179 one year after graduation and -0.189 five years after graduation.[60] Due to the substantial changes in the undereducation estimates for men but relative stability for women, men have a greater penalty one year after graduation (by 0.096 log-points) - as found in the entire sample - but women have a greater penalty five years after graduation (by 0.079 log-points). This suggests the gender gap in the undereducation estimates for the entire sample is sensitive to the cross-sections included.

The results of table 3.4 generally show that the overeducation wage penalty decreases from one to five years after graduation in both cohorts regardless of gender; the undereducation wage penalty appears to be consistent (i.e., not significant in the NLSY79 and roughly the same for women in the NLSY97) except for men in the NLSY79 (where the penalty decreases substantially from one to five years after graduation). The results also point to the fact that the differences in the volume of observations available for both samples impact the results, but most of the qualitative patterns discussed for the entire sample in section 3.5.1 are present for the cross-sections; this points to the robustness of the results presented in table 3.3.

3.6 Conclusion

Since in the 1970s, a plethora of research examining the incidence of and returns to overeducation and undereducation have been developed. Nonetheless, little is known

---

[60] Although the estimate for the entire sample (-0.167) is greater than that of both cross-sections, results from other cross-sections of the data show that the undereducation wage penalty for women in the NLSY97 fluctuates usually within 0.02 log-points of the estimate for the entire sample without any definitive trend.

about the changes over time in the consequences of overeducation and undereducation as studies in this vast literature utilize different data sources, sampling techniques, and methods of defining overeducation/undereducation. This study contributes to the literature by using a consistent estimation strategy to compare the wage penalty associated with overeducation for two cohorts of college graduates born decades apart.

The results show that the incidence of overeducation increased for men and women by, respectively, nine and six percentage points while the incidence of undereducation decreased by, respectively, five and two percentage points. When conditioning on a rich array of covariates, the estimated log-wage penalty associated with overeducation is roughly -0.27 for both men and women in both cohorts. However, past research suggests that identifying education mismatch penalties within occupation and major is the best strategy given the strong ties between wages, occupation/major, and overeducation/undereducation. When conditioned on occupation dummies, the coefficient estimates differ across cohorts such that the log-wage penalty for overeducation increases when shifting from the NLSY79 to the NLSY97 for both genders.

For undereducation, regardless of conditioning on occupation and major fixed effects, the coefficient estimates for undereducation are not significant for the NLSY79 but suggest a substantial wage penalty for the NLSY97. Patterns in the data suggest that this is the result of both an increased wage penalty and a greater likelihood of the NLSY79 over the NLSY97 for undereducated jobs to more closely resemble correctly educated jobs. In the last part of the analysis, the estimated wage penalty of overeducation slightly decreases from one year to five years after graduation (when conditioning on occupation and major fixed effects) regardless of the worker's cohort. This suggests that the overeducation wage penalty decreases over the worker's career regardless of when the worker first entered the labor market. The returns to undereducation remained consistent when examining one year and five years after graduation for both cohorts and genders, except for men in the NLSY97 (where the overeducation wage penalty decreased from one to five years after graduation). Overall, this study provides compelling evidence on what has changed in overeducation/undereducation over generations.

Table 3.1: Means and Standard Deviations of Variables Used in Wage Regressions

	NLSY79		NLSY97	
	Men	Women	Men	Women
Dependent variable				
Log of average hourly wage	2.78 (0.64)	2.65 (0.61)	2.81 (0.66)	2.68 (0.58)
Independent variables				
1 if Overeducated	0.42	0.44	0.51	0.50
1 if Undereducated	0.09	0.07	0.04	0.05
Ability test score	72.43 (23.32)	65.55 (23.91)	71.85 (22.59)	67.49 (22.79)
1 if Hispanic	0.09	0.11	0.11	0.11
1 if black	0.20	0.22	0.14	0.17
1 if Associate degree	0.05	0.10	0.12	0.13
Age at receipt of Bachelor's degree	24.35 (4.63)	25.44 (6.37)	23.15 (1.9)	22.9 (1.93)
1 if reside in northeast[a]	0.19	0.20	0.19	0.16
1 if reside in south[a]	0.35	0.39	0.33	0.36
1 if reside in west[a]	0.18	0.18	0.22	0.23
1 if married[a]	0.22	0.29	0.22	0.26
1 if children[a]	0.21	0.26	0.17	0.23
Tenure[a]	3.08 (3.59)	2.93 (2.14)	2.4 (2.51)	2.18 (2.33)
Experience[a]	11.40 (5.42)	11.05 (5.40)	7.17 (5.92)	6.58 (5.53)
Hours worked per week[a]	41.39 (12.17)	35.91 (12.36)	36.87 (15.43)	32.85 (15.17)

Note: Standard deviation is in parenthesis. Ability test score is the ASVAB Math/Verbal percentile in the NLSY97 and the AFQT score in the NLSY79. Additional regressors include dummy variables for calendar year, tenure squared and cubed, experience square and cubed.

aVariable is time-varying for a given individual.

Table 3.2: Percentage of Overeducated, Undereducated and Correctly Educated Workers in each Occupation and Major Group, by Gender and Cohort

	Men						Women					
	NLSY79			NLSY97			NLSY79			NLSY97		
	Over	Correct	Under	Over	Correct	Under	Over	Correct	Under	Over	Correct	Under
Occupations												
Management	34.1	63.0	2.9	21.3	76.4	2.4	25.7	65.2	9.1	13.6	82.4	4.0
Professional	14.1	64.8	21.2	20.7	69.6	9.7	18.1	70.1	11.8	21.3	69.0	9.7
Services	92.2	7.9	0.0	98.1	1.9	0.0	96.3	3.7	0.0	98.5	1.5	0.0
Sales	52.4	47.6	0.0	61.8	38.2	0.0	64.0	36.1	0.0	73.5	26.5	0.0
Clerical	92.5	7.6	0.0	99.9	0.2	0.0	95.0	5.0	0.0	99.1	0.9	0.0
Farmers, laborers, etc.	97.8	2.2	0.0	98.0	2.1	0.0	97.2	2.8	0.0	99.4	0.6	0.0
Majors												
Mgmt/comm.	45.8	51.9	2.3	50.5	48.1	1.4	45.5	50.7	3.8	50.6	48.2	1.2
STEM	34.5	55.2	10.3	44.6	52.9	2.6	41.9	50.2	8.0	44.1	51.2	4.7
Arts/humanities	54.5	33.0	12.5	59.8	33.4	6.8	48.9	42.8	8.3	60.0	34.9	5.2
Social sciences	45.3	44.8	10.0	54.6	38.6	6.8	41.6	49.6	8.8	45.5	47.4	7.1
Other	46.1	38.8	15.1	52.6	43.8	3.7	47.8	45.0	7.1	54.8	41.7	3.6

Table 3.3: Coefficient Estimates for Overeducation and Undereducation for Men During the Early Career
(dependent variable is log-wage)

	NLSY79				NLSY97			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
1 if Overeducated	-0.271** (0.024)	-0.207** (0.031)	-0.257** (0.025)	-0.199** (0.031)	-0.279** (0.028)	-0.244** (0.042)	-0.257** (0.026)	-0.230** (0.039)
1 if Undereducated	-0.010 (0.050)	0.051 (0.052)	0.022 (0.049)	0.076 (0.050)	-0.297** (0.043)	-0.282** (0.045)	-0.230** (0.042)	-0.220** (0.044)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation Group Indicators		Yes		Yes		Yes		Yes
Major Group Indicators			Yes	Yes			Yes	Yes
N	11919	11919	11919	11919	6384	6384	6384	6384
R^2	0.270	0.287	0.280	0.295	0.172	0.179	0.196	0.200

Note: ** significant at 1% level, * significant at 5% level. Standard errors (clustered at the individual level) are in parenthesis. Coefficient estimates of other regressors can be found in table C.2.

Table 3.4: Coefficient Estimates for Overeducation and Undereducation for Women During the Early Career (dependent variable is log-wage)

	NLSY79				NLSY97			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
1 if Overeducated	-0.278**	-0.181**	-0.281**	-0.203**	-0.283**	-0.275**	-0.277**	-0.278**
	(0.019)	(0.025)	(0.019)	(0.024)	(0.019)	(0.027)	(0.019)	(0.027)
1 if Undereducated	0.026	0.020	0.040	0.032	-0.196**	-0.190**	-0.171**	-0.167**
	(0.050)	(0.051)	(0.047)	(0.048)	(0.041)	(0.041)	(0.040)	(0.040)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation Group Indicators		Yes		Yes		Yes		Yes
Major Group Indicators			Yes	Yes			Yes	Yes
N	12696	12696	12696	12696	9141	9141	9141	9141
R^2	0.228	0.248	0.253	0.271	0.168	0.170	0.186	0.188

Note: ** significant at 1% level, * significant at 5% level. Standard errors (clustered at the individual level) are in parenthesis. Coefficient estimates of other regressors can be found in table C.3.

Table 3.5: Coefficient Estimates for Overeducation and Undereducation, One and Five Years after Graduation

	Men				Women			
	NLSY79		NLSY97		NLSY79		NLSY97	
	One year after graduation	Five years after graduation	One year after graduation	Five years after graduation	One year after graduation	Five years after graduation	One year after graduation	Five years after graduation
1 if Overeducated	-0.222**	-0.199**	-0.235**	-0.221**	-0.204**	-0.195**	-0.284**	-0.246**
	(0.061)	(0.064)	(0.078)	(0.079)	(0.051)	(0.059)	(0.033)	(0.051)
1 if Undereducated	0.093	0.103	-0.275*	-0.110	0.082	-0.027	-0.179*	-0.189*
	(0.102)	(0.098)	(0.139)	(0.095)	(0.076)	(0.073)	(0.074)	(0.081)
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Occupation Group Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Major Group Indicators	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1035	825	794	541	1223	853	1136	602
R^2	0.324	0.366	0.258	0.247	0.336	0.307	0.238	0.187

Note: ** significant at 1% level, * significant at 5% level. Standard errors (clustered at the individual level) are in parenthesis. Coefficient estimates of other regressors can be found in table C.4 for men and table C.5 for women

References

Abel, J. R., and Deitz, R. (2015). Agglomeration and job matching among college graduates. *Regional Science and Urban Economics, 51*, 14-24.

Alba-Ramirez, A. (1993). Mismatch in the Spanish labor market: overeducation? *Journal of Human Resources*, 259-278.

Allen, J., and Van der Velden, R. (2001). Educational mismatches versus skill mismatches: effects on wages, job satisfaction, and on-the-job search. *Oxford economic papers, 53*(3), 434-452.

Altonji, J. G., Kahn, L. B., and Speer, J. D. (2014). Trends in earnings differentials across college majors and the changing task composition of jobs. *The American Economic Review*, *104*(5), 387-393

Altonji, J. G., Blom, E., and Meghir, C. (2012). Heterogeneity in human capital investments: High school curriculum, college major, and careers. *Annual. Review of Economics, 4*(1), 185-223.

Arcidiacono, P., Hotz, V. J., and Kang, S. (2012). Modeling college major choices using elicited measures of expectations and counterfactuals. *Journal of Econometrics*, *166*(1), 3-16.

Arcidiacono, P. (2004). Ability sorting and the returns to college major, *Journal of Econometrics*, 121 (1-2), 343–375.

Baird, M. D., Bozick, R., and Harris, M. (2017). Postsecondary Education and STEM Employment in the United States.

Bialik, K. and Fry, R. (2019). Millennial life: How young adulthood today compares with prior generations. *Pew Research Center, Social and Demographic Trends.*

Bartel, A. P., & Borjas, G. J. (1981). Wage growth and job turnover: An empirical analysis. In *Studies in labor markets* (pp. 65-90). University of Chicago Press.

Beede, D. N., Julian, T. A., Langdon, D., McKittrick, G., Khan, B., and Doms, M. E. (2011). Women in STEM: A gender gap to innovation. *U.S. Department of Commerce Economics and Statistics Administration Issue Brief*, (04-11).

Boudarbat, B., and Chernoff, V. (2010). The determinants of education-job match among Canadian university graduates. *CIRANO-Scientific Publication*, (2010s-14).

Burdett, K. (1978). A theory of employee job search and quit rates. *The American Economic Review*, *68*(1), 212-220.

Burnham, K. P., and Anderson, D. R. (1998). Practical use of the information-theoretic approach. In *Model Selection and Inference* (pp. 75-117). Springer, New York, NY.

Cataldi, E. F., Siegel, P., Shepherd, B., and Cooney, J. (2014). Baccalaureate and Beyond: A First Look at the Employment Experiences and Lives of College Graduates, 4 Years On (B&B: 08/12). NCES 2014-141. *National Center for Education Statistics*.

Chevalier, A. (2003). Measuring over-education. *Economica*, *70*(279), 509-531.

Chevalier, A., and Lindley, J. (2009). Overeducation and the skills of UK graduates. *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, *172*(2), 307-337.

Clark, B., Joubert, C., and Maurel, A. (2017). The career prospects of overeducated Americans. *IZA Journal of Labor Economics*, *6*(1), 3.

Cortes, P., & Pan, J. (2018). Occupation and gender. *The Oxford Handbook of Women and the Economy*, 425

Daymont, T. N., & Andrisani, P. J. (1984). Job preferences, college major, and the gender gap in earnings. *Journal of Human Resources*, 408-428.

Dolton, P. J., and Silles, M. A. (2001). Over education in the graduate labour market: some evidence from alumni data. *Centre for the Economics of Education, London School of Economics and Political Science*, (No. 9).

Dolton, P. J., and Silles, M. A. (2008). The effects of over-education on earnings in the graduate labour market. *Economics of Education Review*, *27*(2), 125-139.

Dolton, P. J., and Vignoles, A. (2000). The incidence and effects of overeducation in the UK graduate labour market. *Economics of Education Review*, *19*(2), 179-198.

Duncan, G. J., and Hoffman, S. D. (1981). The incidence and wage effects of overeducation. *Economics of Education Review*, *1*(1), 75-86.

Fouarge, D., Kriechel, B., and Dohmen, T. (2014). Occupational sorting of school graduates: The role of economic preferences. *Journal of Economic Behavior & Organization*, *106*, 335-351.

Freeman, R. B. (1976). The overeducated american (pp. 117-23). *New York: Academic press.*

Freeman, J. A., and Hirsch, B. T. (2008). College majors and the knowledge content of jobs. *Economics of Education Review*, *27*(5), 517-535.

Frenette, M. (2004). The overqualified Canadian graduate: the role of the academic program in the incidence, persistence, and economic returns to overqualification. *Economics of Education Review*, *23*(1), 29-45.

Funk, C., and Parker, K. (2018). Women and men in STEM often at odds over workplace equity. *Pew Research Center*, 1-158.

García-Mainar, I., García-Martín, G., and Montuenga, V. (2015). Over-education and gender occupational differences in Spain. *Social Indicators Research*, *124*(3), 807-833.

Gelman, A., Hwang, J., and Vehtari, A. (2014). Understanding predictive information criteria for Bayesian models. *Statistics and computing*, *24*(6), 997-1016.

Graham, J. W., and Smith, S. A. (2005). Gender differences in employment and earnings in science and engineering in the US. *Economics of education review*, *24*(3), 341-354.

Groeneveld, S., and Hartog, J. (2004). Overeducation, wages and promotions within the firm. *Labour Economics*, *11*(6), 701-714.

Grogger, J., and Eide, E. (1995). Changes in college skills and the rise in the college wage premium. *Journal of Human Resources*, 280-310.

Groot, W. (1993). Overeducation and the Returns to Enterprise-related Schooling. *Economics of Education Review*, *12*(4), 299-309.

Groot, W. (1996). The incidence of, and returns to overeducation in the UK. *Applied Economics*, *28*(10), 1345-1350.

Groot, W., and Maassen van den Brin, H. (2000). Overeducation in the labor market: a meta-analysis. *Economics of education review*, *19*(2), 149-158.

Guvenen, F., Kuruscu, B., Tanaka, S., and Wiczer, D. (2018). Multidimensional Skill Mismatch. *Stony Brook University Department of Economics Working Papers* 18-02.

Hadavand, A., Thomas, S. M., Almasi, P., and Gharehgozli, O. (2019). Relevance of Education to Occupation: A New Empirical Approach Based on College Courses.

Hamermesh, D. S., and Donald, S. G. (2008). The effect of college curriculum on earnings: An affinity identifier for non-ignorable non-response bias. *Journal of Econometrics*, *144*(2), 479-491.

Hartog, J. (2000). Over-education and earnings: where are we, where should we go? *Economics of education review*, *19*(2), 131-147.

Hartog, J., and Oosterbeek, H. (1988). Education, allocation and earnings in the Netherlands: Overschooling? *Economics of Education Review*, *7*(2), 185-194.

Herrera-Idárraga, P., López-Bazo, E., and Motellón, E. (2012). Informality and overeducation in the labor market of a developing country. *XREAP2012-20*.

Jiang, X. (2017). Planting the Seeds for Success: Why Women in STEM Do Not Stick in the Field. *Purdue University working paper*.

Johnson, W. R. (1978). A theory of job shopping. *The Quarterly Journal of Economics*, 261-278.

Jovanovic, B. (1979a). Job matching and the theory of turnover. *The Journal of Political Economy*, 972-990.

Jovanovic, B. (1979b). Firm-specific capital and turnover. *Journal of political economy*, *87*(6), 1246-1260.

Kiker, B. F., Santos, M. C., and De Oliveira, M. M. (1997). Overeducation and undereducation: Evidence for Portugal. *Economics of Education Review*, *16*(2), 111-125.

Kinsler, J., and Pavan, R. (2015). The specificity of general human capital: Evidence from college major choice. *Journal of Labor Economics*, *33*(4), 933-972.

Korpi, T., and Tåhlin, M. (2009). Educational mismatch, wages, and wage growth: Overeducation in Sweden, 1974–2000. *Labour Economics*, *16*(2), 183-193.

Leuven, E., and Oosterbeek, H. (2011). Overeducation and mismatch in the labor market. *Handbook of the Economics of Education*, *4*, 283-326.

Light, A., and Rama, A. (2019). Moving beyond the STEM/non-STEM dichotomy: wage benefits to increasing the STEM-intensities of college coursework and occupational requirements. *Education Economics*, 1-25.

Lindley, J., and McIntosh, S. (2010). Is the over-education wage penalty permanent?.

McGoldrick, K., and Robst, J. (1996). Gender differences in overeducation: A test of the theory of differential overqualification. *The American Economic Review*, *86*(2), 280-284.

McGuinness, S. (2006). Overeducation in the labour market. *Journal of economic surveys*, *20*(3), 387-418.

Mortensen, D. T., and Pissarides, C. A. (1994). Job creation and job destruction in the theory of unemployment. *The review of economic studies*, *61*(3), 397-415.

Noonan, R. (2017). STEM Jobs: 2017 Update. *U.S. Department of Commerce Economics and Statistics Administration Issue Brief*, (02-17).

Nordin, M., Persson, I., and Rooth, D. O. (2010). Education–occupation mismatch: Is there an income penalty?. *Economics of education review*, *29*(6), 1047-1059.

Ortiz, L., and Kucel, A. (2008). Do fields of study matter for over-education? The cases of Spain and Germany. *International Journal of Comparative Sociology*,*49*(4-5), 305-327.

Robst, J. (2007a). Education and job match: The relatedness of college major and work. *Economics of Education Review*, *26*(4), 397-407.

Robst, J. (2007b). Education, college major, and job match: Gender differences in reasons for mismatch. *Education Economics*, *15*(2), 159-175.

Robst, J. (2008). Overeducation and college major: expanding the definition of mismatch between schooling and jobs. *The Manchester School*, *76*(4), 349-368.

Roksa, J., and Levey, T. (2010). What can you do with that degree? College major and occupational status of college graduates over time. *Social forces*,*89*(2), 389-415.

Rubb, S. (2003a). Overeducation in the labor market: A comment and re-analysis of a meta-analysis. *Economics of Education review*, *22*(6), 621-629.

Rubb, S. (2003b). Overeducation: a short or long run phenomenon for individuals?. *Economics of education review*, *22*(4), 389-394.

Rubb, S. (2003c). Post-college schooling, overeducation, and hourly earnings in the United States. *Education Economics*, *11*(1), 53-72.

Rubb, S. (2006). Educational mismatches and earnings: Extensions of occupational mobility theory and evidence of human capital depreciation. *Education Economics*, *14*(2), 135-154.

Rumberger, R. W. (1987). The impact of surplus schooling on productivity and earnings. *Journal of Human Resources, 22(1)*, 24–50.

Sicherman, N. (1991). "Overeducation" in the Labor Market. *Journal of labor Economics*, 101-122.

Sicherman, N. (1996). Gender differences in departures from a large firm*, Industrial and Labor Relations Review, 49(3),* 484–505

Sloane, P. J. (2003). Much ado about nothing? What does the overeducation literature really tell us. *Overeducation in Europe*, 11-45.

Sullivan, P. (2010). Empirical evidence on occupation and industry specific human capital. *Labour economics*, *17*(3), 567-580.

Topel, R. H., & Ward, M. P. (1992). Job mobility and the careers of young men. *The Quarterly Journal of Economics*, *107*(2), 439-479.

U.S. Department of Homeland Security (2016). Eligible CIP Codes for the STEM OPT Extension. Retrieved from: https://studyinthestates.dhs.gov/eligible-cip-codes-for-the-stem-opt-extension

Verdugo, R. R., and Verdugo, N. T. (1989). The impact of surplus schooling on earnings: Some additional findings. *Journal of Human Resources*, 629-643.

Zafar, B. (2013). College major choice and the gender gap. *Journal of Human Resources*, 48(3), 545-595.

Appendix A. Appendix to Chapter 1

Table A.1: List of Knowledge Categories in O*NET

Administration and Management: Knowledge of business and management principles involved in strategic planning, human resources modeling, leadership technique, etc.

Biology: Knowledge of plant and animal organisms, their tissues, cells, functions, interdependencies, and interactions with each other and the environment.

Building and Construction: Knowledge of materials, methods, and the tools involved in the construction or repair of buildings and structures such as highways and roads.

Chemistry: Knowledge of the chemical composition, structure, and properties of substances and of the chemical processes and transformations that they undergo.

Clerical: Knowledge of administrative and clerical procedures and systems such as word processing, managing files and records, stenography and transcription, etc.

Communications and Media: Knowledge of media production, communication, and dissemination techniques and methods.

Computers and Electronics: Knowledge of circuit boards, processors, chips, electronic equipment, computer hardware and software, including applications and programming.

Customer and Personal Service: Knowledge of principles and processes for providing customer and personal services

Design: Knowledge of design techniques, tools, and principles involved in production of precision technical plans, blueprints, drawings, and models.

Economics and Accounting: Knowledge of economic and accounting principles and practices, the financial markets, banking and the analysis and reporting of financial data.

Education and Training: Knowledge of principles and methods for coursework and training design, teaching and instruction for individuals and groups

Continued

Table A.1 continued

Engineering and Technology: Knowledge of the practical application of engineering science and technology

English Language: Knowledge of the structure and content of the English language including the meaning and spelling of words, rules of composition, and grammar.

Fine Arts: Knowledge of the theory and techniques required to compose, produce, and perform works of music, dance, visual arts, drama, and sculpture.

Food Production: Knowledge of techniques and equipment for planting, growing, and harvesting food products (both plant and animal) for consumption

Foreign Language: Knowledge of the structure and content of a foreign (non-English) language

Geography: Knowledge of principles and methods for describing the features of land, sea, and air masses

History and Archeology: Knowledge of historical events and their causes, indicators, and effects on civilizations and cultures.

Law and Government: Knowledge of laws, legal codes, court procedures, precedents, government regulations, executive orders, agency rules, and political process.

Mathematics: Knowledge of arithmetic, algebra, geometry, calculus, statistics, and their applications.

Mechanical: Knowledge of machines and tools, including their designs, uses, repair, and maintenance.

Medicine and Dentistry: Knowledge of the information and techniques needed to diagnose and treat human injuries, diseases, and deformities.

Personnel and Human Resources: Knowledge of principles and procedures for personnel recruitment, selection, training, compensation and benefits, labor relations, etc.

Philosophy and Theology: Knowledge of different philosophical systems and religions. This includes their basic principles, values, ethics, ways of thinking, customs, etc.

Physics: Knowledge and prediction of physical principles, laws, their interrelationships, and applications

Production and Processing: Knowledge of raw materials, production processes, quality control, costs, etc. for maximizing the effective manufacture and distribution

Continued

Table A.1 continued

Psychology: Knowledge of human behavior and performance

Public Safety and Security: Knowledge of relevant equipment, policies, procedures, and strategies to promote effective local, state, or national security operations

Sales and Marketing: Knowledge of principles and methods for showing, promoting, and selling products or services.

Sociology and Anthropology: Knowledge of group behavior and dynamics, societal trends and influences, human migrations, ethnicity, cultures and their history and origins.

Telecommunications: Knowledge of transmission, broadcasting, switching, control, and operation of telecommunications systems.

Therapy and Counseling: Knowledge of methods, and procedures for diagnosis, treatment, and rehabilitation of physical and mental dysfunctions, and for career counseling

Transportation: Knowledge of principles and methods for moving people or goods by air, rail, sea, or road, including the relative costs and benefits

Information from O*NET; For full descriptions see https://www.onetonline.org/find/descriptor/browse/Knowledge/

Table A.2: List of O*NET Knowledge Categories and Scale Anchors for the High-Level Scores

Knowledge Descriptor	High Scale Anchor (Level Score of ~70-100)
Administration and Management	Manage a $10 million company
Biology	Isolate and identify a new virus
Building and Construction	Build a high-rise office tower
Chemistry	Develop a safe commercial cleaner
Clerical	Organize a storage system for company forms
Communications and Media	Write a novel
Computers and Electronics	Create a program to scan computer disk for viruses
Customer and Personal Service	Respond to a citizen's request for assistance after a major disaster
Design	Develop detailed plans for a high-rise office building
Economics and Accounting	Keep a major corporation's financial records
Education and Training	Design a training program for new employees
Engineering and Technology	Plan for the impact of weather in designing a bridge
English Language	Teach a college English class
Fine Arts	Design an artistic display for a major trade show
Food Production	Run a 100,000 acre farm
Foreign Language	Write an English language review of a book written in a foreign language
Geography	Develop a map of the world showing mountains, deserts, and rivers
History and Archeology	Determine the age of bones for placing them in fossil history
Law and Government	Serve as a judge in a federal court
Mathematics	Derive a complex mathematical equation
Mechanical	Overhaul an airplane jet engine
Medicine and Dentistry	Perform open heart surgery
Personnel and Human Resources	Design a new personnel selection and promotion system for the army
Philosophy and Theology	Compare the teachings of major philosophers
Physics	Design a cleaner burning gasoline engine
Production and Processing	Manage an international shipping company distribution center
Psychology	Treat a person with severe mental illness
Public Safety and Security	Command a military operation
Sales and Marketing	Develop a marketing plan for a nationwide telephone system
Sociology and Anthropology	Create a new theory about the development of civilizations
Telecommunications	Develop a new, world-wide telecommunications network
Therapy and Counseling	Counsel an abused child
Transportation	Control air traffic at a busy airport

Table A.3: Relating O*NET Knowledge Groups to NLSY97 Subject Areas

O*NET Groups that directly relate to NLSY97 Subject Areas

ONET Group	Transcript Categories
Building and Construction	Construction Trades
Communications and Media	Communication, Journalism, and Related Programs
Computers and Electronics	Computer and Information Sciences and Support Services
Education and Training	Education
English Language	English Language and Literature/Letters
Foreign Language	Foreign Languages, Literatures, and Linguistics
Mathematics	Mathematics and Statistics
Mechanical	Mechanic and Repair Technologies/Technicians
Public Safety and Security	Homeland Security, Law Enforcement, Firefighting and Related Protective Services
Telecommunications	Communications Technologies/Technicians and Support Services
Transportation	Transportation and Materials Moving
Psychology	Psychology

O*NET Groups that relate to multiple NLSY97 Subject Areas

ONET Group	Transcript Categories
Biology	Biological and Biomedical Sciences
	Natural Resources and Conservation Biology
Law/Government	Legal Professions and Studies
	Public Administration and Social Service Professions (Partial)
	Citizenship Activities
Medicine/Dentistry	Health-Related Knowledge and Skills
	Health Professions and Related Programs
	Residency Programs (e.g., Medical)
	Biological and Biomedical Sciences (Partial)
Philosophy/Theology	Philosophy and Religious Studies
	Theology and Religious Vocations
Food Production	Agriculture, Agriculture Operations, and Related Services

Continued

Table A.3 continued

Engineering and Technology	Engineering
	Engineering Technologies and Engineering-Related Fields
	Military Technologies and Applied Sciences
Design	Architecture and Related Services
	Visual and Performing Arts (Partial)
	Family and Consumer Sciences (Partial)
Customer and Personal Service	Interpersonal and Social Skills
	Personal and Culinary Services (Partial)
History and Archeology	History
	Social Sciences (Partial)
Production and Processing	Precision Production
	Business, Management, Marketing, and Related Support Services (Partial)

Multiple O*NET Groups match one or more NLSY97 Subject Areas

ONET Group	**Transcript Categories**
Chemistry	Physical Sciences (Partial)
Physics	Physical Sciences (Partial)
Administration and Management	Business, Management, Marketing, and Related Support Services (Partial)
Economics and Accounting	Business, Management, Marketing, and Related Support Services (Partial)
	Social Sciences (Sociology and Economics)
Sales and Marketing	Business, Management, Marketing, and Related Support Services (Partial)
Sociology and Anthropology	Social Sciences (Partial)
Geography	Social Sciences (Partial)
Fine Arts	Visual and Performing Arts (Partial)
Therapy and Counseling	Family and Consumer Sciences (Partial)
	Education (Partial)
	Public Administration and Social Service Professions (Partial)
	Health Professions and Related Programs (Partial)

Continued

Table A.3 continued

O*NET Group doesn't match with NLSY97 Subject Areas or vice versa	
ONET Group	**Transcript Categories**
Clerical	N/A
Personnel and Human Resources	N/A
Other	Parks, Recreation and Leisure Studies
	Basic Skills and Developmental/Remedial Education
	Citizenship Activities
	Leisure and Recreational Activities
	High School/Secondary Diplomas and Certificates
	Other
	Multi/Interdisciplinary Studies (This mapped into several areas)

Data: O*NET and College Course Mapping 2010. Note: “Partial” indicates that only a subset of courses is included in the O*NET group

Table A.4: Five General Knowledge Dimensions used to construct Subject-Specific Skill Mismatch Index

Knowledge Category	O*NET Knowledge Descriptors	NLSY97 Subject Areas
MANAGEMENT and COMMUNICATION	Administration and Management, Sales and Marketing, Communications and Media, Customer and Personal Service, Personnel and Human Resources, Telecommunications	Business, Marketing, Communication and related programs, Communications technologies, Communication, Journalism, and Related Programs, Military Science, Leadership, and Operational Art
STEM	Biology, Chemistry, Computers and Electronics, Engineering and Technology, Mathematics, Medicine and Dentistry, Physics	Agriculture and natural resources, Natural Resources and Conservation, Architecture and Related Services, Biological and biomedical sciences, Computer and information sciences, Engineering, Engineering technologies, Health professions and related programs, Mathematics and statistics, Military technologies and applied sciences, Physical sciences, Science technologies.
ARTS and HUMANITIES	English Language, Fine Arts, Design, Foreign Language, Philosophy and Theology	English language and literature/letters, Family and consumer sciences/human sciences, Foreign languages, literatures, and linguistics, Liberal arts and sciences, general studies, and humanities, Library science, Philosophy and religious studies, Theology and religious vocations, Visual and performing arts

Continued

Table A.4 continued

SOCIAL SCIENCES	Geography, History and Archeology, Law and Government, Sociology and Anthropology, Therapy and Counseling, Education and Training, Economics and Accounting, Psychology	Area, ethnic, cultural, gender, and group studies, Education, Legal professions and studies, Public administration and social services, Social sciences, History, Psychology
OTHER	Public Safety and Security, Transportation, Building and Construction, Clerical, Mechanical, Production and Processing, Food Production	Homeland security, law enforcement, and firefighting, Multi/interdisciplinary studies, Construction, Parks, recreation, leisure, and fitness studies, Transportation and materials moving, Precision production, Mechanic and Repair Technologies/Technicians, Other, Personal and Culinary Services

Note: The following “courses” are not grouped above: Basic Skills and Developmental/Remedial Education, Citizenship Activities, Health-Related Knowledge and Skills, High School/Secondary Diplomas and Certificates, Interpersonal and Social Skills, Leisure and Recreational Activities, Personal Awareness and Self-Improvement, Residency Programs

Table A.5: Level of Education Categories in O*NET

Less than a High School Diploma

High School Diploma (or GED or High School Equivalence Certificate)

Post-Secondary Certificate - awarded for training completed after high school (for example, in Personnel Services, Engineering-related Technologies, Vocational Home Economics, Construction Trades, Mechanics and Repairers, Precision Production Trades)

Some College Courses

Associate degree (or other 2-year degree)

Bachelor's Degree

Post-Baccalaureate Certificate - awarded for completion of an organized program of study; designed for people who have completed a Baccalaureate degree but do not meet the requirements of academic degrees carrying the title of Master.

Master's Degree

Post-Master's Certificate - awarded for completion of an organized program of study; designed for people who have completed a Master's degree but do not meet the requirements of academic degrees at the doctoral level.

First Professional Degree - awarded for completion of a program that o requires at least 2 years of college work before entrance into the program, includes a total of at least 6 academic years of work to complete, and provides all remaining academic requirements to begin practice in a profession

Doctoral Degree

Post-Doctoral Training

*Data: O*NET

Table A.6: Estimates Not Reported in Table 1.5

Model	Men				Women			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
ASVAB scores:								
General science	-0.029	-0.008	-0.011	-0.023	-0.045	-0.050	-0.043	-0.050
	(0.039)	(0.040)	(0.038)	(0.038)	(0.034)	(0.036)	(0.034)	(0.036)
Arithmetic reasoning	0.061	0.054	0.041	0.060	0.030	0.034	0.027	0.039
	(0.039)	(0.041)	(0.039)	(0.039)	(0.032)	(0.034)	(0.031)	(0.033)
Work knowledge	-0.088*	-0.078*	-0.053	-0.073*	-0.035	-0.034	-0.033	-0.030
	(0.036)	(0.037)	(0.035)	(0.035)	(0.033)	(0.035)	(0.033)	(0.035)
Paragraph comp.	-0.045	-0.050	-0.049	-0.049	-0.002	0.005	0.003	0.006
	(0.036)	(0.036)	(0.034)	(0.035)	(0.026)	(0.028)	(0.026)	(0.027)
Numerical operations	-0.001	-0.002	-0.001	-0.001	0.003	0.003	0.003	0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Coding speed	0.009	0.004	0.005	0.008	0.005	0.005	0.004	0.006
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Auto information	0.044	-0.010	-0.025	0.013	-0.047	-0.048	-0.055	-0.054
	(0.042)	(0.044)	(0.042)	(0.041)	(0.033)	(0.034)	(0.032)	(0.033)
Shop information	0.052	0.054	0.045	0.058	-0.081**	-0.082**	-0.069*	-0.068*
	(0.043)	(0.045)	(0.043)	(0.041)	(0.030)	(0.031)	(0.031)	(0.031)
Mathematics knowledge	0.055	0.096*	0.070	0.059	0.090**	0.084**	0.086**	0.084**
	(0.041)	(0.041)	(0.040)	(0.039)	(0.027)	(0.028)	(0.027)	(0.027)

Continued

Table A.6 continued

Mechanical comp.	-0.030	-0.060	-0.052	-0.031	0.037	0.025	0.033	0.018
	(0.043)	(0.044)	(0.040)	(0.040)	(0.031)	(0.034)	(0.031)	(0.033)
Electronics info	-0.012	0.009	0.005	-0.018	-0.028	-0.026	-0.027	-0.028
	(0.037)	(0.038)	(0.036)	(0.035)	(0.029)	(0.030)	(0.029)	(0.029)
Assembling objects	-0.006	-0.009	-0.001	-0.001	-0.015	-0.019	-0.016	-0.022
	(0.026)	(0.028)	(0.027)	(0.026)	(0.020)	(0.021)	(0.020)	(0.021)
Mother's highest grade completed	0.018*	0.016*	0.018*	0.017*	0.002	0.000	0.002	-0.000
	(0.008)	(0.008)	(0.008)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)
1 if mother employed when resp age 16	-0.014	-0.012	-0.013	0.002	-0.001	-0.002	-0.010	-0.012
	(0.040)	(0.043)	(0.040)	(0.039)	(0.029)	(0.030)	(0.029)	(0.030)
1 if English primary language	0.080	0.135	0.153	0.117	-0.037	-0.081	-0.026	-0.081
	(0.079)	(0.092)	(0.087)	(0.077)	(0.061)	(0.063)	(0.063)	(0.069)
1 if live with mom/dad	0.083	0.098	0.131	0.158	-0.122	-0.148*	-0.109	-0.133
	(0.093)	(0.091)	(0.088)	(0.093)	(0.069)	(0.074)	(0.071)	(0.077)
mom only	0.069	0.102	0.116	0.125	-0.153*	-0.171*	-0.140	-0.153
	(0.100)	(0.100)	(0.097)	(0.099)	(0.071)	(0.075)	(0.072)	(0.079)
mom/partner	0.141	0.133	0.166	0.206*	-0.229**	-0.249**	-0.210**	-0.226*
	(0.104)	(0.104)	(0.098)	(0.104)	(0.080)	(0.087)	(0.080)	(0.088)
dad only	0.032	0.055	0.119	0.147	-0.159	-0.182	-0.169	-0.189
	(0.138)	(0.136)	(0.134)	(0.135)	(0.097)	(0.100)	(0.096)	(0.100)
1 if Hispanic	0.036	0.049	0.038	0.038	-0.025	-0.042	-0.034	-0.046
	(0.060)	(0.061)	(0.057)	(0.056)	(0.048)	(0.049)	(0.047)	(0.050)

Continued

Table A.6 continued

1 if black	0.068	0.047	0.067	0.057	-0.077	-0.096*	-0.083*	-0.097*
	(0.057)	(0.059)	(0.056)	(0.054)	(0.040)	(0.043)	(0.041)	(0.043)
1 if Associate degree	0.013	0.030	0.027	0.006	-0.029	-0.031	-0.044	-0.031
	(0.050)	(0.054)	(0.051)	(0.051)	(0.031)	(0.032)	(0.032)	(0.033)
Age at receipt of Bachelor’s degree	0.002	-0.005	-0.004	0.003	-0.003	-0.007	-0.002	-0.008
	(0.017)	(0.018)	(0.017)	(0.017)	(0.011)	(0.012)	(0.011)	(0.012)
College grade point average	0.019	0.016	0.025	0.023	0.013	0.011	0.023	0.008
	(0.026)	(0.027)	(0.025)	(0.026)	(0.024)	(0.025)	(0.024)	(0.024)
1 if cohabiting	0.071	0.089*	0.090*	0.072	0.091**	0.086**	0.090**	0.086**
	(0.038)	(0.039)	(0.037)	(0.038)	(0.027)	(0.028)	(0.026)	(0.028)
1 if married	0.044	0.041	0.042	0.037	0.096**	0.088**	0.094**	0.079**
	(0.035)	(0.037)	(0.035)	(0.033)	(0.027)	(0.028)	(0.026)	(0.028)
1 if children	-0.015	-0.001	-0.007	0.001	-0.052	-0.034	-0.054	-0.036
	(0.039)	(0.041)	(0.039)	(0.037)	(0.029)	(0.031)	(0.028)	(0.030)
Hours worked per week	-0.002	-0.002	-0.003	-0.002	0.001	0.002	0.002	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Job tenure (T)	0.062**	0.062**	0.061**	0.061**	0.051**	0.048**	0.049**	0.047**
	(0.010)	(0.011)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
$T^2/10$	-0.004**	-0.004**	-0.004**	-0.004**	-0.002*	-0.002*	-0.002*	-0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Pre-degree experience	0.011	0.010	0.012	0.011	0.013	0.011	0.013	0.012
	(0.010)	(0.010)	(0.010)	(0.010)	(0.007)	(0.008)	(0.007)	(0.008)

Continued

Table A.6 continued

Experience (X)	0.077**	0.071**	0.071**	0.080**	0.071**	0.077**	0.073**	0.076**
	(0.020)	(0.021)	(0.020)	(0.020)	(0.015)	(0.016)	(0.015)	(0.016)
$X^2/10$	-0.001	-0.001	-0.000	-0.001	-0.004**	-0.004**	-0.004**	-0.004**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
1 if urban	0.045	0.044	0.036	0.039	0.039	0.041	0.038	0.038
	(0.032)	(0.034)	(0.033)	(0.032)	(0.021)	(0.021)	(0.021)	(0.021)
1 if reside in northeast	0.058	0.041	0.033	0.044	0.071	0.073	0.072	0.074
	(0.049)	(0.052)	(0.049)	(0.048)	(0.041)	(0.042)	(0.041)	(0.042)
South	0.049	0.055	0.031	0.041	-0.039	-0.037	-0.035	-0.041
	(0.043)	(0.046)	(0.042)	(0.041)	(0.034)	(0.035)	(0.033)	(0.035)
West	0.141**	0.134*	0.104*	0.129**	0.111**	0.110**	0.108**	0.106**
	(0.050)	(0.053)	(0.051)	(0.048)	(0.039)	(0.041)	(0.039)	(0.040)
Constant	2.002**	2.108**	2.270**	1.935**	2.210**	2.369**	2.222**	2.604**
	(0.442)	(0.455)	(0.434)	(0.430)	(0.305)	(0.330)	(0.304)	(0.333)
N	4633	4633	4633	4633	6637	6637	6637	6637
R^2	0.194	0.192	0.211	0.200	0.170	0.169	0.177	0.175

Note: ** significant at 1% level, * significant at 5% level. Each column corresponds to a separate specification. Standard errors (clustered at the individual level) are in parenthesis. All specifications include dummy variables for calendar years 2002-16 (2006 excluded). Excluded variables are “1 if reside in north central” and “1 if other family structure.” The dependent variable is the log of the CPI-U-deflated, average hourly wage.

Table A.7: Estimates Not Reported in Table 1.6
Coefficient Estimates for Over/Under Skill Mismatch Index and Subject-Specific Skill Mismatch Index, by Gender

	Men		Women	
	(1)	(2)	(1)	(2)
ASVAB scores:				
General science	-0.024	-0.019	-0.048	-0.044
	(0.038)	(0.039)	(0.034)	(0.034)
Arithmetic reasoning	0.057	0.072	0.028	0.026
	(0.039)	(0.039)	(0.031)	(0.031)
Work knowledge	-0.080*	-0.087*	-0.027	-0.035
	(0.035)	(0.036)	(0.033)	(0.033)
Paragraph comp.	-0.051	-0.044	0.005	0.001
	(0.036)	(0.035)	(0.027)	(0.026)
Numerical operations	-0.000	-0.002	0.003	0.003
	(0.004)	(0.004)	(0.003)	(0.003)
Coding speed	0.008	0.008	0.005	0.004
	(0.007)	(0.007)	(0.006)	(0.006)
Auto information	0.013	0.027	-0.054	-0.050
	(0.040)	(0.042)	(0.032)	(0.032)
Shop information	0.053	0.058	-0.070*	-0.075*
	(0.042)	(0.043)	(0.030)	(0.031)
Mathematics knowledge	0.065	0.067	0.085**	0.092**
	(0.039)	(0.041)	(0.027)	(0.027)
Mechanical comprehension	-0.024	-0.043	0.034	0.038
	(0.041)	(0.042)	(0.031)	(0.031)
Electronics information	-0.007	-0.014	-0.027	-0.029
	(0.035)	(0.036)	(0.029)	(0.029)
Assembling objects	-0.002	-0.002	-0.016	-0.015
	(0.026)	(0.026)	(0.020)	(0.020)
Mother’s highest grade completed	0.018*	0.017*	0.001	0.002
	(0.008)	(0.008)	(0.004)	(0.005)

Continued

Table A.7 continued

1 if mother employed when worker was age 16	0.001	-0.009	-0.012	-0.006
	(0.040)	(0.040)	(0.029)	(0.029)
1 if English primary language	0.109	0.070	-0.027	-0.030
	(0.073)	(0.083)	(0.061)	(0.063)
1 if live with mother/father	0.144	0.126	-0.108	-0.120
	(0.095)	(0.097)	(0.072)	(0.067)
mother only	0.111	0.110	-0.142	-0.151*
	(0.100)	(0.103)	(0.074)	(0.069)
mother/partner	0.211	0.165	-0.211*	-0.235**
	(0.110)	(0.110)	(0.082)	(0.079)
father only	0.127	0.070	-0.171	-0.168
	(0.137)	(0.138)	(0.095)	(0.094)
1 if Hispanic	0.035	0.032	-0.043	-0.032
	(0.057)	(0.060)	(0.047)	(0.046)
1 if black	0.056	0.043	-0.082*	-0.079
	(0.054)	(0.055)	(0.040)	(0.041)
1 if Associate degree	-0.007	0.010	-0.048	-0.035
	(0.051)	(0.052)	(0.031)	(0.031)
Age at receipt of Bachelor's degree	-0.001	-0.002	-0.005	-0.003
	(0.017)	(0.018)	(0.011)	(0.011)
College grade point average	0.021	0.018	0.023	0.024
	(0.026)	(0.027)	(0.023)	(0.024)
1 if cohabiting	0.070	0.076	0.089**	0.087**
	(0.038)	(0.039)	(0.026)	(0.026)
1 if married	0.031	0.049	0.096**	0.099**
	(0.034)	(0.035)	(0.026)	(0.026)
1 if children	0.002	-0.005	-0.049	-0.051
	(0.038)	(0.039)	(0.028)	(0.028)
Hours worked per week	-0.002	-0.001	0.001	0.002
	(0.002)	(0.002)	(0.001)	(0.001)
Job tenure (T)	0.063**	0.064**	0.050**	0.049**
	(0.010)	(0.010)	(0.009)	(0.009)
T^2	-0.004**	-0.005**	-0.002*	-0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
Pre-degree experience	0.010	0.012	0.013	0.012
	(0.010)	(0.010)	(0.007)	(0.007)

Continued

Table A.7 continued

Experience (X)	0.076**	0.076**	0.067**	0.074**
	(0.020)	(0.020)	(0.015)	(0.015)
X^2	-0.001	-0.001	-0.004**	-0.004**
	(0.001)	(0.001)	(0.001)	(0.001)
1 if urban	0.038	0.049	0.033	0.039
	(0.031)	(0.032)	(0.021)	(0.022)
1 if reside in northeast	0.044	0.052	0.075	0.074
	(0.048)	(0.049)	(0.041)	(0.041)
South	0.028	0.061	-0.041	-0.036
	(0.042)	(0.042)	(0.033)	(0.033)
West	0.124*	0.149**	0.107**	0.111**
	(0.049)	(0.049)	(0.039)	(0.039)
Constant	1.574**	2.044**	1.982**	2.209**
	(0.446)	(0.443)	(0.303)	(0.306)
N	4633	4633	6637	6637
R^2	0.199	0.182	0.179	0.170

Note: ** significant at 1% level, * significant at 5% level. Each column corresponds to a separate specification. Standard errors (clustered at the individual level) are in parenthesis. All specifications include dummy variables for calendar years 2002-16 (2006 excluded). Excluded variables are “1 if reside in north central” and “1 if other family structure.” The dependent variable is the log of the CPI-U-deflated, average hourly wage.

Table A.8: Estimates Not Reported in Table 1.7

Coefficient Estimates for Skill Mismatch Index, by Gender, for Cross-Sectional Samples

Model	Men		Women	
	One Year After Graduation	Five Years After Graduation	One Year After Graduation	Five Years After Graduation
ASVAB scores:				
General science	-0.028	0.069	-0.035	-0.061
	(0.056)	(0.072)	(0.046)	(0.059)
Arithmetic reasoning	0.192*	0.057	0.057	0.083
	(0.076)	(0.064)	(0.035)	(0.047)
Work knowledge	-0.097*	-0.060	-0.063	-0.014
	(0.047)	(0.064)	(0.038)	(0.050)
Paragraph comp.	-0.154**	-0.129*	0.010	0.035
	(0.052)	(0.063)	(0.042)	(0.045)
Numerical operations	0.001	-0.006	0.002	0.005
	(0.006)	(0.007)	(0.004)	(0.005)
Coding speed	0.008	0.009	-0.004	-0.000
	(0.009)	(0.011)	(0.009)	(0.008)
Auto information	0.014	-0.021	-0.065	-0.018
	(0.068)	(0.073)	(0.053)	(0.053)
Shop information	0.057	0.097	0.026	-0.089
	(0.063)	(0.076)	(0.042)	(0.051)
Mathematics knowledge	-0.016	0.037	0.070	0.107*
	(0.070)	(0.077)	(0.041)	(0.047)
Mechanical comprehension	-0.099	-0.055	-0.047	-0.009
	(0.057)	(0.068)	(0.043)	(0.049)
Electronics information	0.063	-0.013	0.035	-0.041
	(0.047)	(0.061)	(0.041)	(0.048)
Assembling objects	0.024	0.009	0.001	-0.063
	(0.037)	(0.050)	(0.033)	(0.036)
Mother's highest grade completed	0.015	0.006	-0.006	0.001
	(0.012)	(0.013)	(0.007)	(0.007)

Continued

Table A.8 continued

1 if mother employed when resp. was 16	0.017	0.049	0.008	0.062
	(0.063)	(0.072)	(0.040)	(0.045)
1 if English primary language	0.083	0.106	0.032	0.157
	(0.113)	(0.098)	(0.072)	(0.108)
1 if live with mother/father	0.080	0.145	-0.110	-0.056
	(0.128)	(0.205)	(0.074)	(0.121)
mother only	-0.087	-0.020	-0.150*	-0.150
	(0.144)	(0.217)	(0.076)	(0.130)
mother/partner	0.080	0.055	-0.160	-0.202
	(0.155)	(0.240)	(0.083)	(0.135)
father only	-0.078	0.208	-0.074	-0.138
	(0.205)	(0.240)	(0.111)	(0.143)
1 if Hispanic	0.114	0.082	-0.031	0.002
	(0.088)	(0.092)	(0.071)	(0.066)
1 if black	0.029	0.119	0.039	-0.066
	(0.095)	(0.096)	(0.057)	(0.063)
1 if Associate degree	0.003	-0.065	-0.064	-0.110
	(0.078)	(0.081)	(0.049)	(0.056)
Age at receipt of Bachelor’s degree	-0.009	0.017	-0.021	-0.026
	(0.029)	(0.031)	(0.017)	(0.022)
College grade point average	0.011	0.051	-0.018	-0.010
	(0.043)	(0.046)	(0.032)	(0.044)
1 if cohabiting	0.085	-0.001	0.108*	0.202**
	(0.066)	(0.089)	(0.049)	(0.076)
1 if married	0.132*	0.016	0.062	0.067
	(0.061)	(0.079)	(0.041)	(0.047)
1 if children	-0.080	0.019	-0.096*	-0.036
	(0.071)	(0.075)	(0.045)	(0.048)
Hours worked per week	-0.010*	-0.012**	-0.003	-0.002
	(0.004)	(0.004)	(0.002)	(0.003)
Job tenure (T)	0.085**	0.056	0.073*	0.082**
	(0.033)	(0.031)	(0.029)	(0.025)
T^2	-0.008**	-0.003	-0.006	-0.005*
	(0.003)	(0.002)	(0.003)	(0.002)
Pre-degree experience	0.015	-0.008	0.016	0.013
	(0.016)	(0.018)	(0.011)	(0.012)

Continued

Table A.8 continued

Experience (X)	0.034	0.145	0.030	0.265*
	(0.105)	(0.108)	(0.093)	(0.116)
X^2	0.011	-0.006	0.013	-0.025
	(0.012)	(0.012)	(0.016)	(0.014)
1 if urban	-0.046	0.100	0.049	0.082*
	(0.071)	(0.074)	(0.041)	(0.040)
1 if reside in northeast	0.195*	0.114	0.242**	0.006
	(0.077)	(0.106)	(0.064)	(0.091)
south	0.192**	0.060	0.035	-0.015
	(0.065)	(0.085)	(0.056)	(0.051)
west	0.240**	0.135	0.172**	0.121*
	(0.081)	(0.099)	(0.051)	(0.057)
Constant	2.928**	1.899*	3.129**	2.187**
	(0.707)	(0.781)	(0.481)	(0.574)
N	558	375	755	490
R^2	0.296	0.282	0.206	0.262

Note: ** significant at 1% level, * significant at 5% level. Each column corresponds to a separate specification. Standard errors (clustered at the individual level) are in parenthesis. All specifications include dummy variables for calendar years 2002-16 (2006 excluded). Excluded variables are “1 if reside in north central” and “1 if other family structure.” The dependent variable is the log of the CPI-U-deflated, average hourly wage.

Appendix B. Appendix to Chapter 2

Appendix Table B.1: Estimates Not Reported in Table 2.4, NCES List
Estimated STEM Wage Premium

	Men				Women			
	STEM Measure							
Variables	1 if STEM Major	STEM-Intensity of Crswk	STEM-Intensity of Crswk Quartile (with gender specific cut-offs)	STEM-Intensity of Crswk Quartile (no gender specific cut-offs)	1 if STEM Major	STEM-Intensity of Crswk	STEM-Intensity of Crswk Quartile (with gender specific cut-offs)	STEM-Intensity of Crswk Quartile (no gender specific cut-offs)
ASVAB scores:								
General science	-0.033	-0.050	-0.048	-0.042	-0.050	-0.058	-0.050	-0.046
	(0.040)	(0.039)	(0.038)	(0.039)	(0.034)	(0.034)	(0.033)	(0.034)
Arithmetic reasoning	0.077*	0.051	0.048	0.049	0.023	0.020	0.022	0.016
	(0.039)	(0.040)	(0.039)	(0.040)	(0.031)	(0.031)	(0.031)	(0.031)
Work knowledge	-0.078*	-0.046	-0.061	-0.073	-0.037	-0.020	-0.029	-0.024
	(0.039)	(0.038)	(0.037)	(0.038)	(0.033)	(0.034)	(0.033)	(0.033)
Paragraph comp.	-0.038	-0.035	-0.027	-0.023	0.007	0.009	0.005	0.002
	(0.035)	(0.035)	(0.034)	(0.034)	(0.027)	(0.026)	(0.026)	(0.026)

Continued

Table B.1 continued

Numerical operations	-0.001	0.001	0.001	0.001	0.004	0.003	0.003	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Coding speed	0.009	0.007	0.006	0.006	0.004	0.004	0.006	0.005
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Auto information	0.037	0.031	0.032	0.041	-0.051	-0.057	-0.055	-0.048
	(0.043)	(0.041)	(0.042)	(0.042)	(0.033)	(0.033)	(0.032)	(0.033)
Shop information	0.060	0.041	0.050	0.048	-0.081**	-0.087**	-0.084**	-0.090**
	(0.043)	(0.041)	(0.042)	(0.042)	(0.030)	(0.031)	(0.030)	(0.030)
Mathematics knowledge	0.049	0.050	0.057	0.055	0.084**	0.078**	0.086**	0.082**
	(0.041)	(0.040)	(0.040)	(0.040)	(0.027)	(0.027)	(0.026)	(0.027)
Mechanical comprehension	-0.045	-0.030	-0.031	-0.031	0.037	0.039	0.034	0.032
	(0.043)	(0.041)	(0.041)	(0.042)	(0.031)	(0.031)	(0.030)	(0.031)
Electronics information	-0.026	-0.032	-0.029	-0.029	-0.026	-0.023	-0.024	-0.026
	(0.037)	(0.036)	(0.036)	(0.036)	(0.030)	(0.029)	(0.028)	(0.029)
Assembling objects	-0.011	-0.013	-0.017	-0.011	-0.016	-0.024	-0.022	-0.017
	(0.027)	(0.027)	(0.027)	(0.028)	(0.020)	(0.020)	(0.019)	(0.019)
Mother’s highest grade	0.015	0.015	0.015	0.016*	0.003	0.003	0.002	0.002
	(0.008)	(0.008)	(0.008)	(0.008)	(0.005)	(0.004)	(0.004)	(0.004)
1 if mother empl. when R age 16	-0.016	-0.016	-0.017	-0.015	0.004	-0.004	0.005	0.002
	(0.041)	(0.041)	(0.041)	(0.041)	(0.030)	(0.029)	(0.029)	(0.029)
1 if english primary lang	0.063	0.094	0.120	0.111	-0.029	-0.026	-0.059	-0.037
	(0.080)	(0.077)	(0.080)	(0.080)	(0.060)	(0.061)	(0.066)	(0.063)

Continued

Table B.1 continued

1 if live with mom/dad	0.145	0.133	0.134	0.138	-0.103	-0.113	-0.130*	-0.114
	(0.097)	(0.097)	(0.094)	(0.095)	(0.068)	(0.068)	(0.065)	(0.066)
mom only	0.139	0.131	0.136	0.142	-0.140*	-0.142*	-0.171**	-0.153*
	(0.104)	(0.103)	(0.100)	(0.101)	(0.069)	(0.070)	(0.066)	(0.068)
mom/partner	0.179	0.165	0.174	0.194	-0.219**	-0.219**	-0.235**	-0.224**
	(0.109)	(0.112)	(0.109)	(0.109)	(0.078)	(0.079)	(0.076)	(0.077)
dad only	0.081	0.082	0.075	0.097	-0.139	-0.146	-0.154	-0.144
	(0.138)	(0.135)	(0.134)	(0.136)	(0.096)	(0.097)	(0.091)	(0.094)
1 if Hispanic	0.048	0.040	0.032	0.039	-0.022	-0.027	-0.005	-0.021
	(0.058)	(0.057)	(0.058)	(0.058)	(0.047)	(0.047)	(0.044)	(0.046)
1 if black	0.053	0.044	0.049	0.048	-0.083*	-0.095*	-0.074	-0.086*
	(0.057)	(0.054)	(0.055)	(0.056)	(0.041)	(0.041)	(0.039)	(0.040)
1 if Associate's degree	0.005	0.024	0.022	0.021	-0.033	-0.026	-0.036	-0.033
	(0.051)	(0.049)	(0.050)	(0.050)	(0.032)	(0.032)	(0.031)	(0.032)
Age at receipt of Bachelor's deg.	0.003	0.002	0.000	0.002	-0.001	-0.000	-0.004	-0.003
	(0.018)	(0.017)	(0.018)	(0.018)	(0.011)	(0.011)	(0.011)	(0.011)
College grade point average	0.009	0.015	0.024	0.023	0.023	0.019	0.028	0.018
	(0.027)	(0.027)	(0.027)	(0.027)	(0.024)	(0.024)	(0.024)	(0.024)
1 if cohabiting	0.051	0.050	0.050	0.053	0.088**	0.093**	0.094**	0.096**
	(0.039)	(0.039)	(0.039)	(0.039)	(0.026)	(0.026)	(0.026)	(0.026)
1 if married	0.051	0.046	0.044	0.044	0.099**	0.098**	0.103**	0.102**
	(0.035)	(0.035)	(0.034)	(0.034)	(0.027)	(0.026)	(0.026)	(0.026)

Continued

Table B.1 continued

1 if children	-0.009	-0.008	-0.018	-0.014	-0.047	-0.042	-0.047	-0.043
	(0.039)	(0.038)	(0.039)	(0.039)	(0.028)	(0.028)	(0.028)	(0.028)
Hours worked per week	-0.001	-0.001	-0.001	-0.001	0.002*	0.002*	0.002*	0.002*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Job tenure (T)	0.060**	0.058**	0.060**	0.060**	0.050**	0.048**	0.049**	0.048**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
$T^2/10$	-0.044**	-0.043**	-0.044**	-0.044**	-0.020*	-0.018*	-0.018*	-0.018*
	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
Pre-degree experience	0.012	0.013	0.011	0.009	0.013	0.012	0.013	0.013
	(0.010)	(0.010)	(0.010)	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)
Experience (X)	0.087**	0.090**	0.086**	0.085**	0.076**	0.077**	0.072**	0.074**
	(0.021)	(0.020)	(0.020)	(0.020)	(0.015)	(0.015)	(0.015)	(0.015)
$X^2/10$	-0.014	-0.014	-0.014	-0.014	-0.042**	-0.042**	-0.040**	-0.041**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)	(0.011)
1 if urban	0.041	0.043	0.048	0.047	0.039	0.039	0.044*	0.041
	(0.032)	(0.032)	(0.031)	(0.031)	(0.021)	(0.022)	(0.022)	(0.022)
1 if reside in northeast	0.057	0.039	0.051	0.041	0.071	0.072	0.063	0.069
	(0.050)	(0.050)	(0.049)	(0.049)	(0.041)	(0.041)	(0.040)	(0.041)
south	0.055	0.044	0.050	0.050	-0.036	-0.044	-0.044	-0.042
	(0.043)	(0.043)	(0.042)	(0.042)	(0.034)	(0.034)	(0.033)	(0.033)
west	0.145**	0.130**	0.148**	0.141**	0.111**	0.105**	0.096*	0.103**
	(0.050)	(0.049)	(0.049)	(0.049)	(0.040)	(0.039)	(0.038)	(0.039)

Continued

Table B.1 continued

Constant	1.739**	1.665**	1.660**	1.607**	1.964**	1.932**	2.089**	2.059**
	(0.447)	(0.438)	(0.445)	(0.450)	(0.303)	(0.300)	(0.295)	(0.298)
N	4633	4633	4633	4633	6637	6637	6637	6637
R^2	0.183	0.191	0.190	0.187	0.167	0.171	0.174	0.171

Note: ** and * indicates significance at the 1% level and 5% level, respectively. Each column represents a separate regression using the NCES list but a different STEM dichotomous/non-dichotomous distinction. Estimates from specification 2.1 using the dichotomous measure are in the first column and using the non-dichotomous measure are in the second column. Estimates from the alternative to specification 2.1 that uses quartile indicators for the STEM-intensity of coursework distribution are in the third and fourth columns (non-dichotomous measure); the third column uses the distribution of each gender to determine the quartiles while the fourth column uses the entire sample (both men and women) to determine the quartiles. Standard errors (clustered at the individual level) are in parenthesis. Coefficient estimates for the STEM wage premiums can be found in table 2.4.

Appendix Table B.2: Estimates Not Reported in Table 2.4, ICE List
Estimated STEM Wage Premium

	Men				Women			
	STEM Measure							
Variables	1 if STEM Major	STEM-Intensity of Coursework	STEM-Intensity of Crswk Quartile (with gender specific cut-offs)	STEM-Intensity of Crswk Quartile (without gender specific cut-offs)	1 if STEM Major	STEM-Intensity of Coursework	STEM-Intensity of Crswk Quartile (with gender specific cut-offs)	STEM-Intensity of Crswk Quartile (without gender specific cut-offs)
ASVAB scores:								
General science	-0.022	-0.048	-0.046	-0.042	-0.053	-0.057	-0.048	-0.054
	(0.039)	(0.039)	(0.038)	(0.038)	(0.034)	(0.033)	(0.034)	(0.034)
Arithmetic reasoning	0.070	0.048	0.049	0.051	0.024	0.015	0.020	0.017
	(0.040)	(0.040)	(0.040)	(0.040)	(0.031)	(0.030)	(0.031)	(0.031)
Work knowledge	-0.084*	-0.042	-0.055	-0.059	-0.036	-0.021	-0.025	-0.027
	(0.038)	(0.038)	(0.037)	(0.036)	(0.033)	(0.033)	(0.033)	(0.033)
Paragraph comp.	-0.040	-0.037	-0.036	-0.038	0.002	0.011	0.002	0.009
	(0.035)	(0.035)	(0.035)	(0.035)	(0.027)	(0.026)	(0.027)	(0.026)

Continued

Table B.2 continued

Numerical operations	-0.001	0.001	0.001	0.001	0.004	0.003	0.003	0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Coding speed	0.008	0.006	0.005	0.005	0.004	0.004	0.004	0.005
	(0.007)	(0.007)	(0.007)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Auto info.	0.034	0.025	0.029	0.025	-0.051	-0.058	-0.058	-0.054
	(0.042)	(0.042)	(0.043)	(0.042)	(0.033)	(0.033)	(0.033)	(0.033)
Shop info.	0.060	0.047	0.051	0.052	-0.081**	-0.086**	-0.089**	-0.085**
	(0.043)	(0.041)	(0.042)	(0.042)	(0.031)	(0.031)	(0.031)	(0.031)
Mathematics knowledge	0.061	0.054	0.064	0.062	0.081**	0.076**	0.080**	0.076**
	(0.041)	(0.040)	(0.040)	(0.040)	(0.027)	(0.026)	(0.027)	(0.027)
Mechanical comp.	-0.049	-0.034	-0.032	-0.035	0.039	0.034	0.030	0.032
	(0.043)	(0.042)	(0.041)	(0.041)	(0.031)	(0.031)	(0.031)	(0.031)
Electronics info.	-0.025	-0.025	-0.020	-0.017	-0.026	-0.017	-0.018	-0.015
	(0.037)	(0.036)	(0.035)	(0.035)	(0.029)	(0.029)	(0.029)	(0.029)
Assembling objects	-0.010	-0.014	-0.016	-0.010	-0.015	-0.020	-0.016	-0.017
	(0.027)	(0.027)	(0.027)	(0.027)	(0.020)	(0.020)	(0.020)	(0.020)
Mother’s highest grade	0.016*	0.013	0.012	0.013	0.002	0.004	0.003	0.003
	(0.008)	(0.008)	(0.008)	(0.008)	(0.005)	(0.004)	(0.005)	(0.005)
1 if mom emp when R 16	-0.006	-0.011	-0.013	-0.009	0.000	-0.003	-0.002	-0.003
	(0.041)	(0.041)	(0.041)	(0.041)	(0.030)	(0.029)	(0.029)	(0.030)
1 if English prim lang.	0.066	0.080	0.097	0.097	-0.039	-0.040	-0.041	-0.042
	(0.080)	(0.077)	(0.076)	(0.075)	(0.059)	(0.061)	(0.063)	(0.062)

Continued

Table B.2 continued

1 if live with mom/dad	0.131	0.135	0.148	0.144	-0.091	-0.116	-0.126	-0.118
	(0.097)	(0.095)	(0.092)	(0.092)	(0.070)	(0.067)	(0.065)	(0.066)
mom only	0.130	0.139	0.165	0.152	-0.125	-0.142*	-0.154*	-0.148*
	(0.103)	(0.101)	(0.099)	(0.098)	(0.072)	(0.068)	(0.066)	(0.067)
mom/partner	0.169	0.187	0.209	0.210*	-0.198*	-0.217**	-0.233**	-0.225**
	(0.109)	(0.110)	(0.108)	(0.107)	(0.081)	(0.078)	(0.076)	(0.077)
dad only	0.075	0.080	0.089	0.084	-0.131	-0.153	-0.164	-0.158
	(0.138)	(0.135)	(0.135)	(0.135)	(0.096)	(0.095)	(0.096)	(0.093)
1 if Hispanic	0.040	0.049	0.048	0.049	-0.021	-0.021	-0.018	-0.023
	(0.058)	(0.058)	(0.058)	(0.058)	(0.047)	(0.046)	(0.045)	(0.045)
1 if black	0.053	0.042	0.050	0.052	-0.081*	-0.094*	-0.089*	-0.094*
	(0.056)	(0.053)	(0.055)	(0.054)	(0.041)	(0.041)	(0.040)	(0.041)
1 if Associate's degree	0.011	0.029	0.026	0.033	-0.045	-0.027	-0.035	-0.033
	(0.051)	(0.049)	(0.049)	(0.050)	(0.030)	(0.031)	(0.031)	(0.032)
Age bachelor's degree	0.004	0.003	-0.002	0.001	0.001	-0.001	-0.002	-0.002
	(0.018)	(0.017)	(0.018)	(0.018)	(0.011)	(0.011)	(0.011)	(0.011)
College grade point avg.	0.013	0.019	0.024	0.026	0.025	0.021	0.022	0.021
	(0.027)	(0.027)	(0.027)	(0.027)	(0.024)	(0.024)	(0.024)	(0.024)
1 if cohabiting	0.050	0.049	0.045	0.045	0.090**	0.089**	0.089**	0.091**
	(0.040)	(0.039)	(0.039)	(0.038)	(0.026)	(0.025)	(0.026)	(0.026)
1 if married	0.044	0.045	0.042	0.044	0.097**	0.096**	0.100**	0.098**
	(0.035)	(0.034)	(0.034)	(0.034)	(0.026)	(0.026)	(0.027)	(0.026)

Continued

Table B.2 continued

1 if children	-0.008	-0.009	-0.015	-0.016	-0.043	-0.044	-0.048	-0.045
	(0.039)	(0.038)	(0.040)	(0.039)	(0.028)	(0.028)	(0.028)	(0.028)
Hours worked per week	-0.001	-0.001	-0.001	-0.001	0.002*	0.002*	0.002*	0.002*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Job tenure (T)	0.063**	0.059**	0.060**	0.060**	0.050**	0.047**	0.047**	0.047**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
$T^2/10$	-0.047**	-0.044**	-0.045**	-0.045**	-0.021*	-0.019*	-0.018*	-0.019*
	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
Pre-degree experience	0.010	0.013	0.012	0.012	0.013	0.012	0.013	0.013
	(0.010)	(0.010)	(0.010)	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)
Experience (X)	0.085**	0.089**	0.084**	0.085**	0.076**	0.076**	0.076**	0.076**
	(0.020)	(0.020)	(0.020)	(0.020)	(0.015)	(0.015)	(0.015)	(0.015)
$X^2/10$	-0.012	-0.014	-0.014	-0.013	-0.041**	-0.041**	-0.042**	-0.041**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.011)	(0.011)	(0.011)	(0.011)
1 if urban	0.049	0.048	0.051	0.050	0.042*	0.038	0.039	0.039
	(0.032)	(0.032)	(0.031)	(0.031)	(0.021)	(0.021)	(0.022)	(0.021)
1 if reside in northeast	0.054	0.039	0.045	0.044	0.073	0.074	0.067	0.072
	(0.049)	(0.050)	(0.050)	(0.050)	(0.041)	(0.041)	(0.041)	(0.041)
south	0.056	0.045	0.047	0.045	-0.038	-0.042	-0.044	-0.039
	(0.043)	(0.043)	(0.042)	(0.042)	(0.034)	(0.034)	(0.033)	(0.033)
west	0.143**	0.127**	0.140**	0.134**	0.108**	0.108**	0.102**	0.108**
	(0.050)	(0.049)	(0.048)	(0.049)	(0.039)	(0.038)	(0.038)	(0.038)

Continued

Table B.2 continued

Constant	1.724**	1.638**	1.733**	1.655**	1.922**	1.940**	1.993**	2.005**
	(0.446)	(0.439)	(0.451)	(0.452)	(0.300)	(0.296)	(0.298)	(0.297)
N	4633	4633	4633	4633	6637	6637	6637	6637
R^2	0.182	0.189	0.189	0.189	0.170	0.176	0.171	0.172

Note: ** and * indicates significance at the 1% level and 5% level, respectively. Each column represents a separate regression using the ICE list but a different STEM dichotomous/non-dichotomous distinction. Estimates from specification 2.1 using the dichotomous measure are in the first column and using the non-dichotomous measure are in the second column. Estimates from the alternative to specification 2.1 that uses quartile indicators for the STEM-intensity of coursework distribution are in the third and fourth columns (non-dichotomous measure); the third column uses the distribution of each gender to determine the quartiles while the fourth column uses the entire sample (both men and women) to determine the quartiles. Standard errors (clustered at the individual level) are in parenthesis. Coefficient estimates for the STEM wage premiums can be found in table 2.4.

Appendix Table B.3: Estimates Not Reported in Table 2.4, NSF List

Estimated STEM Wage Premium

	Men				Women			
	STEM Measure							
Variables	1 if STEM Major	STEM-Intensity of Crswk	STEM-Intensity of Crswk Quartile (with gender specific cut-offs)	STEM-Intensity of Crswk Quartile (no gender specific cut-offs)	1 if STEM Major	STEM-Intensity of Crswk	STEM-Intensity of Crswk Quartile (with gender specific cut-offs)	STEM-Intensity of Crswk Quartile (no gender specific cut-offs)
ASVAB scores:								
General science	-0.019	-0.031	-0.054	-0.028	-0.048	-0.050	-0.054	-0.056
	(0.040)	(0.039)	(0.034)	(0.039)	(0.035)	(0.034)	(0.034)	(0.034)
Arithmetic reasoning	0.070	0.050	0.024	0.055	0.029	0.024	0.024	0.020
	(0.040)	(0.040)	(0.031)	(0.040)	(0.032)	(0.031)	(0.031)	(0.031)
Work knowledge	-0.099**	-0.063	-0.034	-0.065	-0.037	-0.028	-0.034	-0.034
	(0.037)	(0.036)	(0.033)	(0.037)	(0.033)	(0.033)	(0.033)	(0.033)
Paragraph comp.	-0.040	-0.033	0.005	-0.037	-0.001	-0.000	0.005	0.011
	(0.036)	(0.036)	(0.027)	(0.036)	(0.027)	(0.027)	(0.027)	(0.027)

Continued

Table B.3 continued

Numerical operations	-0.001	0.000	0.003	-0.000	0.003	0.003	0.003	0.003
	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Coding speed	0.008	0.007	0.004	0.006	0.004	0.004	0.004	0.004
	(0.007)	(0.007)	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Auto info.	0.041	0.035	-0.048	0.028	-0.048	-0.053	-0.048	-0.045
	(0.042)	(0.042)	(0.033)	(0.043)	(0.033)	(0.033)	(0.033)	(0.033)
Shop info.	0.060	0.037	-0.084**	0.045	-0.083**	-0.080*	-0.084**	-0.083**
	(0.042)	(0.042)	(0.030)	(0.043)	(0.031)	(0.031)	(0.030)	(0.030)
Math know.	0.058	0.051	0.080**	0.056	0.087**	0.081**	0.080**	0.080**
	(0.041)	(0.040)	(0.027)	(0.041)	(0.027)	(0.027)	(0.027)	(0.026)
Mechanical comp.	-0.044	-0.038	0.038	-0.042	0.038	0.035	0.038	0.043
	(0.044)	(0.043)	(0.031)	(0.042)	(0.031)	(0.031)	(0.031)	(0.031)
Electronics info.	-0.019	-0.016	-0.017	-0.020	-0.026	-0.022	-0.017	-0.019
	(0.037)	(0.036)	(0.029)	(0.036)	(0.030)	(0.029)	(0.029)	(0.029)
Assembling objects	-0.003	-0.007	-0.017	-0.007	-0.013	-0.014	-0.017	-0.020
	(0.026)	(0.026)	(0.020)	(0.026)	(0.020)	(0.020)	(0.020)	(0.020)
Mom’s highest grade	0.017*	0.016*	0.003	0.017*	0.002	0.002	0.003	0.003
	(0.008)	(0.008)	(0.005)	(0.008)	(0.005)	(0.005)	(0.005)	(0.005)
1 if mom emp When R 16	-0.002	0.000	0.000	0.002	0.001	-0.001	0.000	-0.001
	(0.041)	(0.039)	(0.029)	(0.039)	(0.030)	(0.030)	(0.029)	(0.029)
1 if English prim language	0.073	0.079	-0.040	0.088	-0.030	-0.023	-0.040	-0.039
	(0.078)	(0.074)	(0.061)	(0.078)	(0.059)	(0.063)	(0.061)	(0.060)

Continued

Table B.3 continued

1 if live with mom/dad	0.121	0.109	-0.103	0.111	-0.123	-0.121	-0.103	-0.098
	(0.094)	(0.100)	(0.063)	(0.098)	(0.067)	(0.066)	(0.063)	(0.063)
mom only	0.099	0.091	-0.143*	0.103	-0.156*	-0.150*	-0.143*	-0.137*
	(0.102)	(0.107)	(0.065)	(0.104)	(0.069)	(0.068)	(0.065)	(0.064)
mom/partner	0.171	0.143	-0.222**	0.153	-0.235**	-0.234**	-0.222**	-0.219**
	(0.107)	(0.114)	(0.077)	(0.112)	(0.079)	(0.078)	(0.077)	(0.077)
dad only	0.043	0.032	-0.126	0.023	-0.162	-0.154	-0.126	-0.123
	(0.140)	(0.148)	(0.091)	(0.144)	(0.096)	(0.096)	(0.091)	(0.091)
1 if Hispanic	0.035	0.031	-0.026	0.044	-0.025	-0.028	-0.026	-0.025
	(0.060)	(0.058)	(0.048)	(0.059)	(0.047)	(0.048)	(0.048)	(0.048)
1 if black	0.047	0.042	-0.080	0.050	-0.081*	-0.093*	-0.080	-0.078
	(0.056)	(0.055)	(0.041)	(0.056)	(0.041)	(0.042)	(0.041)	(0.041)
1 if Associate’s degree	0.016	0.026	-0.037	0.032	-0.036	-0.039	-0.037	-0.034
	(0.052)	(0.049)	(0.030)	(0.050)	(0.031)	(0.030)	(0.030)	(0.030)
Age at receipt of Bachelor’s deg.	0.005	0.004	-0.001	0.003	-0.001	0.000	-0.001	-0.002
	(0.018)	(0.017)	(0.011)	(0.017)	(0.012)	(0.011)	(0.011)	(0.011)
College grade point average	0.013	0.015	0.022	0.019	0.023	0.024	0.022	0.019
	(0.027)	(0.027)	(0.023)	(0.027)	(0.024)	(0.024)	(0.023)	(0.023)
1 if cohabiting	0.055	0.049	0.094**	0.054	0.092**	0.090**	0.094**	0.094**
	(0.039)	(0.039)	(0.027)	(0.039)	(0.027)	(0.026)	(0.027)	(0.026)
1 if married	0.048	0.041	0.098**	0.047	0.100**	0.095**	0.098**	0.096**
	(0.035)	(0.034)	(0.026)	(0.034)	(0.027)	(0.026)	(0.026)	(0.026)

Continued

Table B.3 continued

1 if children	-0.003	0.002	-0.047	0.003	-0.050	-0.046	-0.047	-0.045
	(0.039)	(0.038)	(0.028)	(0.038)	(0.029)	(0.028)	(0.028)	(0.028)
Hours worked per week	-0.001	-0.001	0.002*	-0.001	0.002*	0.002*	0.002*	0.002*
	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Job tenure (T)	0.063**	0.062**	0.049**	0.063**	0.051**	0.048**	0.049**	0.049**
	(0.010)	(0.010)	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)	(0.009)
$T^2/10$	-0.047**	-0.047**	-0.020*	-0.048**	-0.021*	-0.020*	-0.020*	-0.021*
	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
Pre-degree experience	0.010	0.012	0.013	0.012	0.012	0.012	0.013	0.013
	(0.010)	(0.010)	(0.007)	(0.010)	(0.007)	(0.007)	(0.007)	(0.007)
Experience (X)	0.083**	0.088**	0.075**	0.086**	0.074**	0.075**	0.075**	0.074**
	(0.021)	(0.020)	(0.015)	(0.020)	(0.015)	(0.015)	(0.015)	(0.015)
$X^2/10$	-0.013	-0.014	-0.041**	-0.013	-0.041**	-0.041**	-0.041**	-0.041**
	(0.013)	(0.013)	(0.011)	(0.013)	(0.011)	(0.011)	(0.011)	(0.011)
1 if urban	0.051	0.058	0.044*	0.052	0.041	0.040	0.044*	0.046*
	(0.032)	(0.031)	(0.021)	(0.032)	(0.022)	(0.021)	(0.021)	(0.021)
1 if reside in northeast	0.055	0.044	0.071	0.040	0.069	0.066	0.071	0.074
	(0.050)	(0.050)	(0.041)	(0.049)	(0.041)	(0.041)	(0.041)	(0.040)
south	0.057	0.050	-0.037	0.046	-0.035	-0.041	-0.037	-0.035
	(0.043)	(0.043)	(0.034)	(0.044)	(0.034)	(0.034)	(0.034)	(0.034)
west	0.147**	0.129**	0.108**	0.130**	0.108**	0.103**	0.108**	0.112**
	(0.050)	(0.049)	(0.039)	(0.049)	(0.040)	(0.039)	(0.039)	(0.039)

Continued

Table B.3 continued

Constant	1.678** (0.453)	1.527** (0.442)	2.013** (0.302)	1.617** (0.444)	1.988** (0.305)	1.885** (0.302)	2.013** (0.302)	2.031** (0.301)
N	4633	4633	6637	4633	6637	6637	6637	6637
R^2	0.177	0.188	0.171	0.186	0.164	0.171	0.171	0.174

Note: ** and * indicates significance at the 1% level and 5% level, respectively. Each column represents a separate regression using the NSF list but a different STEM dichotomous/non-dichotomous distinction. Estimates from specification 2.1 using the dichotomous measure are in the first column and using the non-dichotomous measure are in the second column. Estimates from the alternative to specification 2.1 that uses quartile indicators for the STEM-intensity of coursework distribution are in the third and fourth columns (non-dichotomous measure); the third column uses the distribution of each gender to determine the quartiles while the fourth column uses the entire sample (both men and women) to determine the quartiles. Standard errors (clustered at the individual level) are in parenthesis. Coefficient estimates for the STEM wage premiums can be found in table 2.4.

Appendix C. Appendix to Chapter 3

Table C.1: NLSY79 and NLSY97 Major Groupings

Major Groupings	**List of Majors Constructed by NLSY79**	**List of Majors Constructed by NLSY97**	**CCM Taxonomy used by NLSY97**
MANAGEMENT and COMMUNICATION	Business and management, Communications	Business management, Communications	Business, Marketing, Communication and related programs, Communications technologies, Communication, Journalism, and Related Programs, Military Science, Leadership, and Operational Art
STEM	Agriculture and natural resources, Architecture and environmental design, Computer and information sciences, Engineering, Biological Sciences, Mathematics, Military Sciences, Health professions, Physical sciences	Agriculture and natural resources, Architecture/Environmental design, Computer/Information science, Engineering, Biological sciences, Nursing, Other health professions, Pre-dental, Pre-med, Pre-vet, Mathematics, Physical sciences	Agriculture and natural resources, Natural Resources and Conservation, Architecture and Related Services, Biological and biomedical sciences, Computer and information sciences, Engineering, Engineering technologies, Health professions and related programs, Mathematics and statistics, Military technologies and applied sciences, Physical sciences, Science technologies.

Continued

Table C.1 continued

ARTS and HUMANITIES	Foreign languages, Home economics, Letters, Theology, Fine and applied arts	Foreign languages, Home economics, English, Philosophy, Theology/religious studies, Fine and applied arts	English language and literature/letters, Family and consumer sciences/human sciences, Foreign languages, literatures, and linguistics, Liberal arts and sciences, general studies, and humanities, Library science, Philosophy and religious studies, Theology and religious vocations, Visual and performing arts
SOCIAL SCIENCES	Area studies, Education, Law, Library Science, Public Affairs and services, Psychology, Social sciences, History	Area studies, Ethnic studies, Education, Pre-law, Psychology, Anthropology, Archaeology, Criminology, Economics, Political science and government, Sociology, History	Area, ethnic, cultural, gender, and group studies, Education, Legal professions and studies, Public administration and social services, Social sciences, History, Psychology

Continued

Table C.1 continued

OTHER	Interdisciplinary studies	Interdisciplinary studies, Other	Homeland security, law enforcement, and firefighting, Multi/interdisciplinary studies, Construction, Parks, recreation, leisure, and fitness studies, Transportation and materials moving, Precision production, Mechanic and Repair Technologies/Technicians, Other, Personal and Culinary Services

Note: The NLSY97 originally asked respondent's majors using a list they constructed and later switched to using the CCM taxonomy. The NLSY79 asked for respondent's majors using a list they constructed. Because of the various lists of majors used, I map the majors from each of the three lists into five broad categories that are used in this assessment.

Appendix Table C.2: Estimates Not Reported in Table 3.3, Men

	NLSY79				NLSY97			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
ASVAB	0.005**	0.005**	0.005**	0.005**	-0.001	-0.000	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
1 if Hispanic	-0.036	-0.042	-0.046	-0.053	-0.034	-0.021	-0.054	-0.043
	(0.055)	(0.052)	(0.056)	(0.052)	(0.054)	(0.054)	(0.053)	(0.054)
1 if black	-0.058	-0.052	-0.055	-0.049	-0.002	-0.001	0.001	0.002
	(0.046)	(0.045)	(0.045)	(0.044)	(0.046)	(0.046)	(0.045)	(0.045)
1 if Associate degree	0.048	0.031	0.043	0.029	0.021	0.024	0.049	0.050
	(0.080)	(0.079)	(0.080)	(0.079)	(0.047)	(0.047)	(0.042)	(0.042)
Age at receipt of Bachelor's degree	-0.015**	-0.013**	-0.015**	-0.013**	0.007	0.008	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.013)	(0.013)	(0.013)	(0.013)
1 if reside in northeast	0.131**	0.131**	0.147**	0.143**	0.055	0.053	0.064	0.062
	(0.042)	(0.040)	(0.042)	(0.040)	(0.047)	(0.047)	(0.047)	(0.047)
south	0.042	0.041	0.046	0.041	0.053	0.053	0.056	0.056
	(0.037)	(0.036)	(0.036)	(0.035)	(0.041)	(0.041)	(0.040)	(0.039)
west	0.126**	0.130**	0.139**	0.137**	0.143**	0.141**	0.137**	0.136**
	(0.045)	(0.044)	(0.045)	(0.044)	(0.047)	(0.046)	(0.046)	(0.046)
1 if married	0.054	0.044	0.042	0.033	0.032	0.028	0.014	0.012
	(0.032)	(0.031)	(0.032)	(0.031)	(0.031)	(0.031)	(0.030)	(0.030)

Continued

Table C.2 continued

1 if children	0.129**	0.129**	0.127**	0.126**	0.021	0.019	0.011	0.010
	(0.032)	(0.031)	(0.031)	(0.030)	(0.035)	(0.034)	(0.034)	(0.033)
Job tenure (T)	0.114**	0.114**	0.114**	0.113**	0.077**	0.076**	0.064**	0.063**
	(0.022)	(0.021)	(0.021)	(0.021)	(0.018)	(0.017)	(0.018)	(0.018)
$T^2/10$	-0.085**	-0.081**	-0.084**	-0.080**	-0.057	-0.053	-0.039	-0.037
	(0.032)	(0.031)	(0.031)	(0.031)	(0.034)	(0.034)	(0.034)	(0.034)
T3/100	0.007*	0.007*	0.007*	0.007*	0.006	0.003	-0.002	-0.004
	(0.003)	(0.003)	(0.003)	(0.003)	(0.015)	(0.015)	(0.015)	(0.015)
Experience (X)	0.046**	0.044**	0.048**	0.047**	0.118**	0.113**	0.120**	0.116**
	(0.013)	(0.013)	(0.013)	(0.013)	(0.025)	(0.025)	(0.026)	(0.026)
$X^2/10$	-0.026**	-0.026**	-0.028**	-0.027**	-0.121**	-0.118**	-0.124**	-0.122**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.044)	(0.043)	(0.044)	(0.043)
X3/100	0.004**	0.004**	0.004**	0.004**	0.075**	0.074**	0.075**	0.074**
	(0.002)	(0.002)	(0.001)	(0.001)	(0.025)	(0.025)	(0.025)	(0.025)
Hours worked per week	0.004**	0.003**	0.003**	0.002*	-0.002	-0.002*	-0.003*	-0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
1 if Management Occupation		0.347**		0.324**		0.206**		0.167**
		(0.052)		(0.053)		(0.054)		(0.053)
Professional		0.206**		0.188**		0.141**		0.114*
		(0.059)		(0.061)		(0.050)		(0.049)
Sales		0.152**		0.125*		0.126*		0.099
		(0.054)		(0.055)		(0.051)		(0.051)

Continued

Table C.2 continued

Clerical		0.056		0.041		0.095		0.074
		(0.048)		(0.049)		(0.053)		(0.051)
Farming, construction, etc.		0.059		0.040		0.179**		0.149**
		(0.052)		(0.053)		(0.049)		(0.047)
1 if Management			0.209**	0.150*			0.274**	0.253**
			(0.068)	(0.067)			(0.056)	(0.055)
STEM			0.231**	0.223**			0.278**	0.271**
			(0.067)	(0.066)			(0.056)	(0.057)
Arts and Humanities			0.074	0.068			0.109	0.101
			(0.068)	(0.066)			(0.056)	(0.055)
Other			0.102	0.085			0.253**	0.245**
			(0.081)	(0.077)			(0.064)	(0.063)
Constant	2.785**	2.627**	2.670**	2.542**	2.445**	2.301**	2.347**	2.244**
	(0.157)	(0.157)	(0.164)	(0.164)	(0.302)	(0.302)	(0.297)	(0.298)
N	11919	11919	11919	11919	6384	6384	6384	6384
R^2	0.270	0.287	0.280	0.295	0.172	0.179	0.196	0.200

Note: ** significant at 1% level, * significant at 5% level. Each column corresponds to a separate specification. Standard errors (clustered at the individual level) are in parenthesis. Coefficient estimates of other regressors can be found in table 3.3. All regressions include year dummies. Excluded variables are “1 if social sciences major,” “1 if reside in north central,” and “1 if services occupation.”

Appendix Table C.3: Estimates Not Reported in Table 3.4

	NLSY79				NLSY97			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
ASVAB	0.003**	0.003**	0.002**	0.002**	0.001	0.001	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
1 if Hispanic	0.112**	0.115**	0.092*	0.094*	-0.028	-0.025	-0.023	-0.021
	(0.042)	(0.042)	(0.040)	(0.039)	(0.034)	(0.034)	(0.033)	(0.033)
1 if black	0.036	0.033	0.038	0.036	-0.032	-0.033	-0.033	-0.035
	(0.036)	(0.036)	(0.034)	(0.033)	(0.027)	(0.027)	(0.027)	(0.027)
1 if Associate degree	0.099*	0.085	0.064	0.051	-0.016	-0.016	-0.023	-0.023
	(0.045)	(0.044)	(0.044)	(0.043)	(0.026)	(0.026)	(0.026)	(0.026)
Age at receipt of Bachelor's degree	-0.004	-0.003	-0.005*	-0.004*	-0.013	-0.012	-0.012	-0.011
	(0.002)	(0.002)	(0.002)	(0.002)	(0.009)	(0.009)	(0.009)	(0.009)
1 if reside in northeast	0.169**	0.163**	0.168**	0.161**	0.137**	0.136**	0.132**	0.131**
	(0.039)	(0.038)	(0.038)	(0.036)	(0.035)	(0.035)	(0.034)	(0.034)
south	0.060	0.062	0.051	0.052	-0.003	-0.003	-0.014	-0.015
	(0.034)	(0.033)	(0.031)	(0.030)	(0.028)	(0.028)	(0.026)	(0.027)
west	0.190**	0.194**	0.186**	0.187**	0.131**	0.127**	0.125**	0.122**
	(0.044)	(0.043)	(0.041)	(0.040)	(0.032)	(0.032)	(0.031)	(0.031)
1 if married	0.041	0.038	0.044	0.042	0.048*	0.049*	0.045*	0.045*
	(0.024)	(0.023)	(0.023)	(0.022)	(0.022)	(0.023)	(0.021)	(0.022)

Continued

Table C.3 continued

1 if children	-0.006	-0.008	-0.016	-0.016	-0.026	-0.025	-0.021	-0.021
	(0.026)	(0.026)	(0.025)	(0.024)	(0.023)	(0.023)	(0.023)	(0.023)
Job tenure (T)	0.190**	0.186**	0.191**	0.189**	0.049**	0.048**	0.045**	0.044**
	(0.021)	(0.021)	(0.021)	(0.020)	(0.012)	(0.012)	(0.012)	(0.012)
T^2/10	-0.241**	-0.234**	-0.248**	-0.243**	-0.026	-0.022	-0.022	-0.020
	(0.043)	(0.042)	(0.044)	(0.042)	(0.024)	(0.023)	(0.023)	(0.023)
T3/100	0.073**	0.071**	0.076**	0.074**	0.004	0.003	0.004	0.003
	(0.015)	(0.014)	(0.015)	(0.015)	(0.012)	(0.012)	(0.012)	(0.012)
Experience (X)	0.025	0.022	0.028*	0.026	0.148**	0.149**	0.148**	0.148**
	(0.014)	(0.014)	(0.014)	(0.014)	(0.018)	(0.018)	(0.018)	(0.018)
X^2/10	-0.002	-0.001	-0.002	-0.002	-0.231**	-0.234**	-0.231**	-0.232**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.036)	(0.036)	(0.036)	(0.036)
X3/100	0.001	0.001	0.001	0.001	0.118**	0.119**	0.118**	0.119**
	(0.002)	(0.001)	(0.001)	(0.001)	(0.021)	(0.021)	(0.021)	(0.021)
Hours worked per week	0.003**	0.002*	0.003**	0.002*	-0.002*	-0.002**	-0.002**	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
1 if Management Occupation		0.455**		0.431**		0.065		0.034
		(0.060)		(0.060)		(0.038)		(0.039)
Professional		0.282**		0.243**		0.001		-0.014
		(0.059)		(0.059)		(0.036)		(0.036)
Sales		0.186**		0.183**		-0.022		-0.033
		(0.069)		(0.070)		(0.042)		(0.042)

Continued

Table C.3 continued

Clerical		0.173**		0.175**		0.030		0.021
		(0.053)		(0.053)		(0.029)		(0.030)
Farming, construction, etc.		0.273**		0.256**		-0.073		-0.071
		(0.072)		(0.072)		(0.064)		(0.063)
1 if Management			0.119**	0.078			0.162**	0.152**
			(0.042)	(0.042)			(0.038)	(0.038)
STEM			0.257**	0.252**			0.200**	0.203**
			(0.044)	(0.043)			(0.037)	(0.037)
Arts and Humanities			-0.024	-0.023			0.029	0.031
			(0.040)	(0.039)			(0.033)	(0.033)
Other			-0.031	-0.041			0.145**	0.142**
			(0.056)	(0.056)			(0.038)	(0.038)
Constant	1.970**	1.775**	1.925**	1.752**	2.724**	2.703**	2.644**	2.641**
	(0.121)	(0.127)	(0.124)	(0.129)	(0.210)	(0.212)	(0.211)	(0.213)
N	12696	12696	12696	12696	9141	9141	9141	9141
R^2	0.228	0.248	0.253	0.271	0.168	0.170	0.186	0.188

Note: ** significant at 1% level, * significant at 5% level. Each column corresponds to a separate specification. Standard errors (clustered at the individual level) are in parenthesis. Coefficient estimates of other regressors can be found in table 3.3. All regressions include year dummies. Excluded variables are "1 if social sciences major," "1 if reside in north central," and "1 if services occupation."

Appendix Table C.4: Estimates Not Reported in Table 3.5, Women

	NLSY79		NLSY97	
	(1)	(2)	(1)	(2)
	One year after graduation	Five years after graduation	One year after graduation	Five years after graduation
ASVAB	0.003**	0.004**	-0.002	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
1 if Hispanic	0.020	0.002	0.027	-0.054
	(0.070)	(0.074)	(0.071)	(0.069)
1 if black	-0.039	-0.120	0.022	-0.011
	(0.069)	(0.077)	(0.085)	(0.084)
1 if Associate’s degree	-0.026	0.040	0.103	0.085
	(0.092)	(0.124)	(0.061)	(0.057)
Age at receipt of Bachelor’s degree	-0.010**	-0.011*	-0.007	0.003
	(0.004)	(0.005)	(0.025)	(0.021)
1 if reside in northeast	0.155*	0.162*	0.161*	-0.003
	(0.069)	(0.065)	(0.075)	(0.067)
south	0.095	0.077	0.076	0.107
	(0.058)	(0.059)	(0.069)	(0.058)
west	0.201**	0.092	0.140	0.199*
	(0.067)	(0.065)	(0.077)	(0.081)
1 if married	0.155**	0.045	0.127	0.036
	(0.059)	(0.065)	(0.067)	(0.066)
1 if children	0.087	0.088	-0.096	-0.056
	(0.058)	(0.062)	(0.068)	(0.063)
Job tenure (T)	0.043	0.239**	0.097	-0.004
	(0.061)	(0.075)	(0.052)	(0.055)
$T^2/10$	0.110	-0.627**	-0.143	0.132
	(0.146)	(0.165)	(0.095)	(0.115)
T3/100	-0.081	0.245**	0.061	-0.097
	(0.085)	(0.063)	(0.044)	(0.059)
Experience (X)	0.054	0.067	0.096	0.279**
	(0.035)	(0.037)	(0.055)	(0.069)
$X^2/10$	-0.039	-0.040	-0.077	-0.440**
	(0.023)	(0.023)	(0.116)	(0.141)

Continued

Table C.4 continued

X3/100	0.008	0.007	0.046	0.236**
	(0.004)	(0.004)	(0.063)	(0.082)
Hours worked per week	0.001	0.000	-0.002	-0.006*
	(0.002)	(0.003)	(0.003)	(0.003)
1 if Occupation is Management	0.320**	0.368**	0.226*	0.289*
	(0.123)	(0.096)	(0.107)	(0.113)
Professional	0.166	0.122	0.334**	0.179
	(0.120)	(0.099)	(0.111)	(0.106)
Sales	0.365**	0.259*	0.112	0.193
	(0.139)	(0.117)	(0.116)	(0.100)
Clerical	0.198	0.030	0.229*	0.148
	(0.119)	(0.097)	(0.103)	(0.090)
Farming, construction, etc.	0.114	0.143	0.254*	0.217*
	(0.126)	(0.110)	(0.099)	(0.095)
1 if Management	0.137	0.096	0.255**	0.260**
	(0.096)	(0.096)	(0.086)	(0.083)
STEM	0.166	0.208*	0.121	0.266**
	(0.094)	(0.095)	(0.080)	(0.074)
Arts & Humanities	0.062	0.027	-0.002	0.045
	(0.094)	(0.098)	(0.096)	(0.073)
Other	0.103	0.089	0.188	0.296**
	(0.123)	(0.116)	(0.101)	(0.089)
Constant	2.372**	2.545**	2.381**	2.178**
	(0.283)	(0.319)	(0.571)	(0.464)
N	1035	825	794	541
R^2	0.324	0.366	0.258	0.247

Note: ** significant at 1% level, * significant at 5% level. Each column corresponds to a separate specification. Standard errors (clustered at the individual level) are in parenthesis. Coefficient estimates of other regressors can be found in table 3.4. All regressions include year dummies. Excluded variables are “1 if social sciences major,” “1 if reside in north central,” and “1 if services occupation.”

Appendix Table C.5: Estimates Not Reported in Table 3.5, Women

	NLSY79		NLSY97	
	(1)	(2)	(1)	(2)
	One year after graduation	Five years after graduation	One year after graduation	Five years after graduation
ASVAB	0.002	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
1 if Hispanic	-0.044	0.113	-0.005	-0.027
	(0.072)	(0.068)	(0.046)	(0.059)
1 if black	0.067	0.012	-0.022	0.034
	(0.051)	(0.056)	(0.052)	(0.060)
1 if Associate's degree	0.118*	0.017	-0.041	-0.031
	(0.059)	(0.061)	(0.047)	(0.052)
Age at receipt of Bachelor's degree	-0.004	-0.005	0.015	-0.012
	(0.003)	(0.003)	(0.014)	(0.016)
1 if reside in northeast	0.203**	0.173**	0.192**	0.077
	(0.055)	(0.062)	(0.061)	(0.062)
south	0.108*	0.008	0.070	-0.006
	(0.046)	(0.053)	(0.051)	(0.053)
west	0.221**	0.162*	0.176**	0.154**
	(0.063)	(0.072)	(0.052)	(0.051)
1 if married	0.098*	-0.001	-0.001	0.111*
	(0.043)	(0.042)	(0.040)	(0.056)
1 if children	0.035	0.014	-0.021	-0.091
	(0.043)	(0.049)	(0.042)	(0.051)
Job tenure (T)	0.238**	0.154*	0.031	0.025
	(0.052)	(0.068)	(0.026)	(0.035)
$T^2/10$	-0.315**	-0.107	0.009	-0.029
	(0.090)	(0.222)	(0.044)	(0.076)
T3/100	0.095**	-0.030	-0.015	0.025
	(0.030)	(0.162)	(0.018)	(0.041)
Experience (X)	0.030	0.033	0.170**	0.117**
	(0.030)	(0.037)	(0.044)	(0.040)

Continued

Table C.5 continued

$X^2/10$	-0.001	0.003	-0.197	-0.136
	(0.018)	(0.023)	(0.105)	(0.087)
X3/100	0.001	-0.002	0.085	0.059
	(0.003)	(0.004)	(0.065)	(0.049)
Hours worked per week	-0.001	0.003	0.000	-0.006**
	(0.002)	(0.002)	(0.002)	(0.002)
1 if Occupation is	0.302**	0.381**	0.211**	0.099
Management	(0.108)	(0.128)	(0.082)	(0.088)
Professional	0.147	0.253*	0.233**	0.020
	(0.103)	(0.123)	(0.071)	(0.077)
Sales	0.156	0.121	-0.119	0.067
	(0.128)	(0.140)	(0.087)	(0.097)
Clerical	0.089	0.113	-0.115	-0.010
	(0.101)	(0.113)	(0.064)	(0.065)
Farming,	0.110	0.217	-0.101	-0.303
construction, etc.	(0.134)	(0.130)	(0.108)	(0.208)
1 if Management	0.000	0.047	0.127	0.136*
	(0.061)	(0.075)	(0.066)	(0.064)
STEM	0.232**	0.194*	0.227**	0.163**
	(0.064)	(0.077)	(0.068)	(0.060)
Arts & Humanities	-0.101	-0.066	-0.009	0.063
	(0.060)	(0.070)	(0.059)	(0.057)
Other	-0.020	-0.128	0.129	0.125
	(0.085)	(0.094)	(0.067)	(0.070)
Constant	1.857**	1.850**	1.667**	2.499**
	(0.253)	(0.264)	(0.353)	(0.396)
N	1223	853	1136	602
R^2	0.336	0.307	0.238	0.187

Note: ** significant at 1% level, * significant at 5% level. Each column corresponds to a separate specification. Standard errors (clustered at the individual level) are in parenthesis. Coefficient estimates of other regressors can be found in table 3.4. All regressions include year dummies. Excluded variables are “1 if social sciences major,” “1 if reside in north central,” and “1 if services occupation.”