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Hugh Cassidy
Kansas State University

Amanda Gaulke
Kansas State University

College-educated workers in jobs unrelated to their degree generally receive lower wages compared to well-matched workers. Our analysis of data from the National Survey of College Graduates shows that although the rate of this mismatch declined only slightly (19% to 17%), the wage penalty increased by 51% between 1993 and 2019. Changes in the composition of field of study over time, as well as declining returns to "excess" education above what is required for the occupation both help to explain the increasing penalty, especially for women. Mismatch has become more closely associated with lower-return occupations for men but not women.

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The Increasing Penalty to Occupation-Education Mismatch*

Hugh Cassidy† and Amanda Gaulke‡

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†Kansas State University. hughcassidy@ksu.edu.

‡Kansas State University and IZA. gaulke@ksu.edu.

Abstract

College-educated workers in jobs unrelated to their degree generally receive lower wages compared to well-matched workers. Our analysis of data from the National Survey of College Graduates shows that although the rate of this mismatch declined only slightly (19% to 17%), the wage penalty increased by 51% between 1993 and 2019. Changes in the composition of field of study over time, as well as declining returns to "excess" education above what is required for the occupation both help to explain the increasing penalty, especially for women. Mismatch has become more closely associated with lower-return occupations for men but not women.

1 Introduction

According to the National Survey of College Graduates (NSCG), around one in five U.S. college graduates report working in a job that is not related to their highest degree, a number that has declined only slightly between 1993 and 2019. Though there is an extensive literature that discusses education and occupation mismatch from a variety of perspectives, little work has explored how the wage penalty from being mismatched has changed over time. This is notable since the past few decades have seen a dramatic increase in the college wage premium; thus, the 11% mismatch penalty found in Robst (2007a) using the 1993 wave of the NSCG may be outdated.

We provide new evidence on the mismatch penalty over time by updating the analysis of Robst (2007a) to incorporate the 2003, 2010, and 2019 waves of the NSCG. We document an increase in the mismatch penalty between 1993 and 2003 of 35%, while the change between 1993 and 2010 was even larger, at 57%. The mismatch penalty declined slightly between 2010 and 2019, but it remained 51% higher in 2019 than in 1993. For those who are 'somewhat mismatched' we find the penalty increased by 179% between 1993 and 2019.[1]

We investigate several explanations for this increase in the mismatch penalty over time. Given that the mismatch penalty varies by field of study, the change in the composition of field of study over time can explain around a quarter of the increase in the mismatch penalty overall, though more for women (41%) than for men (16%). We find that controlling for occupation greatly reduces the mismatch penalty for men, whereas it has a much smaller impact for women. This indicates that mismatch for men is largely driven by men not working in occupations related to their degree. While we find evidence of larger mismatch penalties for those who have a degree in Science, Technology, Engineering, or Math (STEM), the difference between the mismatch penalty for STEM versus non-STEM degrees is largely constant over time. We find larger penalties for nearly all reasons for mismatch in later waves of the survey, including a shift from mismatch due to "pay or promotion" being associated with a premium to being associated with no benefit.

[1] These wage penalties are all in real terms as wages across cohorts have been adjusted using the CPI measurement of inflation.

We also explore the role of "vertical mismatch", i.e. years of schooling that are more/less than that required by the worker's occupation, since workers who are vertically mismatched may report working in a job not related to their degree. Many papers have used the Duncan and Hoffman (1981) model to show that each additional year of school above that needed by the job tends to earn a lower labor market return (for example, see Chiswick and Miller, 2008, for a discussion related to immigrants, while Leuven and Oosterbeek, 2011, and Quintini, 2011, provide overviews of this literature in general). Using U.S. Census data from 1990 to 2019, we find evidence that the returns to "excess" education have fallen over time, by 1.3 percentage points per year of schooling. The overall decrease has been dominated by women, who experienced a 3.0 percentage point reduction in the returns to excess years of schooling, while men have experienced an increase in return. We also find that, in the NSCG, workers with a BA who are in a job not related to their field of study have on average 1.7 additional years of excess education relative to workers in a closely related job. For higher levels of education, the difference is even larger. Combining these two observations, we conclude that the declining returns to excess years of schooling may explain a large portion of the increased mismatch penalty for women, though vertical mismatch does not explain the increase mismatch penalty among men.

These findings could help explain the enrollment puzzle surrounding why, despite increases to the college premium during the 1990s, college enrollment and completion did not keep pace (Goldin and Katz, 2007). The previous literature has explored the risk and uncertainty students face with regards to graduation (Athreya and Eberly, 2021 and Stange, 2012). Even though Webber (2016) finds benefits for the median student in most cases when accounting for uncertainty in completion, experimental evidence indicates that students' preferences over majors change when they are provided with information about the dispersion of returns instead of the median return (Ruder and Van Noy, 2017). Thus, the risk or dispersion within a major also matters. Despite the similar chance of mismatch across years, an increasing mismatch penalty would still reduce the value of the expected payoff to a college degree. This could partially offset the benefits from the increase in the college wage premium, especially for more marginal students who may fear a relatively high probability of job mismatch, and thus dampen enrollment responses. Thus, documenting the mismatch penalty over time matters for discussions surrounding whether college is worth it.

This is not to say that our findings only have implications for past education decisions. A recent survey of current and potential college students reveals that the Covid-19 pandemic and subsequent economic recession have raised concerns about mismatch (Klebs et al., 2021). Specifically, "getting a job that fits my degree once I graduate" is a concern for 79% of current college students and 77% of high school seniors. Thus, universities may need to focus more attention and efforts on addressing this concern through policies to help students be better matched after graduation. This may be especially important given concerns about an impending economic downturn, given that van den Berge (2018) finds that lower field-specific employment rates at graduation lead to larger levels of mismatch for graduates of those fields. Since Nunley et al. (2016) finds that internships matter for getting a business job more than just having the major, universities may want to see if internship programs help reduce mismatch.

Our results also have important implications for economic models of enrollment and persistence decisions. Ignoring the uncertainty created by the possibility of mismatch, and the wage penalty associated with it, would lead to models being incorrectly specified and potentially incorrect conclusions from counterfactual experiments. By overstating the expected payoff to degree completion through ignoring the chance of mismatch, models may overstate the impact of education policies on enrollment and completion decisions. Thus, more structural work should follow Kinsler and Pavan (2015) by including this skill uncertainty and the possibility of mismatch.

The rest of the paper is organized as follows. Section 2 provides background information surrounding mismatch. Section 3 discusses the NSCG data while Section 4 discusses the methodology for estimating the mismatch penalty and how the mismatch penalty has changed over time. Section 5 discusses whether different potential explanations seem to play a role in the increased penalty. Section 6 summarizes and concludes.

2 Background on Mismatch

There exists a sizable literature that studies the importance of educational and occupational match. Much of this literature has focused on vertical mismatch, or the match between the quantity of education and the education requirement of the job (see Hartog, 2000, Quintini, 2011, and Leuven and Oosterbeek, 2011, for some summaries). These papers quantify the frequency and labor market implications stemming from someone being over-educated or under-educated for their job. The returns to years of schooling are typically found to depend strongly on the years of schooling required for the job, with years of schooling more than the required level typically receiving lower labor market returns. An early example of this work is Duncan and Hoffman (1981), who study mismatch by race and gender. More recently, Chiswick and Miller (2008) use this framework to explore why the returns to schooling are typically lower for immigrants than non-immigrants.

A common criticism of defining match based on education quantity alone is that it is assuming workers are homogeneous within education levels. For example, Lise and Postel-Vinay (2020) convincingly argue that skills should not be measured as a scalar by documenting that the cost to mismatch varies greatly across cognitive, manual, and interpersonal skills. Given that different types of education (for example, different majors) teach different combinations of general and occupation specific skills, comparing quantity of education is likely to not accurately determine who is mismatched. For example, an engineering major and a Spanish major both have college degrees, but an engineering major could be mismatched in terms of the skills required for a job to translate documents and a Spanish major could be mismatched in terms of the skills required to do engineering jobs, even though the workers both have the required quantity of education. Additionally, Leuven and Oosterbeek (2011) note that some "clever" workers may appear under-educated but are in fact well matched and some "underachievers" who appear over-educated may in fact be well matched.

More recent work has thus focused on whether type of education and level of education match. Robst (2008) expands the definition of mismatch to consider both the quantity of education and type of education. He studies relatedness of jobs among the over-educated and finds larger penalties for more mismatched workers using the NSCG 1993 cohort. Over-education and under-education stem from determining whether the worker's educational level is within one standard deviation of the mean education level of actual workers in that occupation. Kim, Ahn, and Kim (2016) use data from Korean college graduates to study both vertical mismatch (based on workers' reports of whether their education level is the same as what is normally required) and horizontal mismatch (self-assessment of whether the type of education matches) using a quantile regression approach. They find that the size of the horizontal mismatch penalty is greatest at the lower- and middle-income segments. Budría and Moro-Egido (2008) use data from Spain and find no penalty for mismatch based on workers being "over-qualified" or "incorrectly qualified" but there is a penalty for workers who are "strongly mismatched" (have the skills to do a more demanding job and did not have formal education or training that has given the necessary skills for the current job). Sloane and Mavromaras (2020) conclude that the consensus in the literature is that being over-educated is especially problematic for those who are also over-skilled (not utilizing the skills from their college degree).

Another approach to measure mismatch is to use an external unbiased reviewer to determine tasks required for each occupation. For example, Yakusheva (2010) uses O*NET data to construct an alternative measure of mismatch. That measure is then applied using a sample of individuals from the High School and Beyond (1980/92) dataset who complete a certificate/license/trade award, an associate degree or a bachelor's degree. She finds that among the sample of bachelor's degree completers the relevance premium is 41.5%. While Hartog (2000) and Leuven and Oosterbeek (2011) note that this strategy is ideal in theory, there are practical issues in using outside experts. Mainly, the high cost of determining required skills often results in a lack of complete updates routinely being done. This can cause problems with longitudinal analyses because it is well documented that skill requirements change within occupation over time.

Another strand of literature measures required education level by directly surveying workers about requirements. Leuven and Oosterbeek (2011) note that this is often the best method in practice. Mavromaras and Sloane (2015) note there are important differences in how many people are considered mismatched when one uses self-reports versus external reviewer rules about requirements versus using "realized matches" (i.e., actual education levels of workers in an occupation) to determine what is within a certain range (often a standard deviation of the mean) as research has found the correlation to be rather low across the three measurements. They further note that using realized matches to back out the requirement or survey questions about requirements to get the job are inferior to using external reviewers to determine rules or a survey question about requirements to do a job. The wording of the survey matters because Green et al. (1999) finds a quarter of workers do not report the same requirements for getting a job as for doing a job. Hartog (2000) notes that worker responses may be biased if one asks for requirements for a job as they want to make themselves appear more important. While asking about relatedness directly may alleviate some of the concern about boasting about one's credentials, a downside to using the self-reports of relatedness is that it does not provide information on what causes the job to be only somewhat related or unrelated. Thus, one does not know if a job is unrelated due to a mismatch of education levels (vertical mismatch) or due to the type of education (horizontal mismatch).

The paper most related to ours is Robst (2007a), who also uses the NSCG data but focuses on only one wave. He uses the 1993 wave of the NSCG to measure the wage effects of working in a job either not related or only partially related to field of degree. He finds that workers who report being in an unrelated job suffer a wage penalty of around 11%. Variance in the size of the wage penalty across degree field is also explored, and he finds that the wage effects of mismatch tend to be greater in fields that teach occupation-specific skills. Robst (2007b), also using the 1993 NSCG, explores how the reason for a job mismatch impacts the wage effects and the differences by gender in the reasons for mismatch. He finds that, though men and women report similar levels of mismatch, women are much more likely than men to report family-related reasons as the most important reason for mismatch, but men and women both report that pay or promotion opportunities and change in career or professional interests as the two most important reasons for mismatch. The wage effects of mismatch are most negative for those mismatched for family-related reasons, and lowest for those mismatched for pay or promotion opportunities. This paper contributes to the literature by building off Robst (2007a) to document changes in the mismatch penalty over time.

Another paper which uses self-reports of mismatch to estimate labor market penalties is Kinsler and Pavan (2015), which use the U.S.-based Baccalaureate and Beyond Longitudinal Study - 1993 cohort to estimate a structural model of college major choice and career outcomes. They find significantly lower wages for science majors working in unrelated jobs (around 30%). Their model relies on uncertainty about an individual's skill level, which results in uncertainty about whether they will end up working in a job related to their field of degree, and thus uncertainty about the returns to their degree. The paper does not estimate how the penalty changes across cohorts or how it changes for the specific cohort studied over time.

Turning to international results, Lemieux (2014) also uses self-reports of education and job mismatch to find that Canadian workers are more productive and earn higher wages when their job is a good match with the field of degree. He finds that the returns to schooling can vary significantly depending on the occupation, field of study, and the match between the two. Yuen (2010), also using Canadian data and self-reports of relatedness, similarly finds significant wage premiums for those working in jobs either somewhat related or closely related to their field of degree. Nordin, Persson, and Rooth (2010) focus specifically on the relatedness of field of study and occupation to find that, in Sweden, the mismatch wage penalty for men is roughly twice as large as that observed in the U.S., while for women, it is approximately the same as the U.S. penalty. Note, however, that Nordin, Persson, and Rooth (2010) compare their mismatch penalties with those from Robst (2007a), which as our paper shows, were outdated by 2003 (the timeframe of the Swedish data).

3 Data

Our data comes from the NSCG for years 1993, 2003, 2010, and 2019 from the National Science Foundation.[2] The NSCG samples are derived from individuals who report having a college degree in either the decennial Census (1993 and 2003 NSCG) or American Community Survey (ACS) (2010 NSCG onward). Samples sizes range from approximately 77,000 to 148,000 per year. Later NSCG years include both a new sample of college graduates drawn from the ACS, as well as a sample of individuals surveyed in the previous NSCG sample.

The NSCG includes extensive information about educational attainment, including information from multiple degrees as well as field of degree. The primary question of interest included in each of the waves, and which Robst (2007a) makes extensive use of, is the following: "To what extent was your work on your principal job related to your highest degree?" The possible responses are: 1) "Closely related"; 2) "Somewhat related"; and 3) "Not related". The exact wording of the question and responses has remained very similar across waves. Given that this question focuses on the highest degree, we define field of study based on the highest degree attained. We use the minor coding for field of degree, which includes 31 unique categories that are almost entirely consistent across years.

Our estimation sample is restricted to individuals aged 22-64 who are currently employed and report a positive annual salary. We drop a small number of individuals with missing degree, field of degree, or region information, and a small number of observations with apparent errors in their year of graduation. The 1993 sample includes a very limited number of individuals who report a degree of "Other", which is a category not included in later samples, and thus we drop these observations. Our estimation sample ultimately includes

[2]Results using the 2013, 2015, and 2017 NSCG waves are excluded from the main paper for brevity. Overall, our results do not meaningfully change when considering these additional three waves.

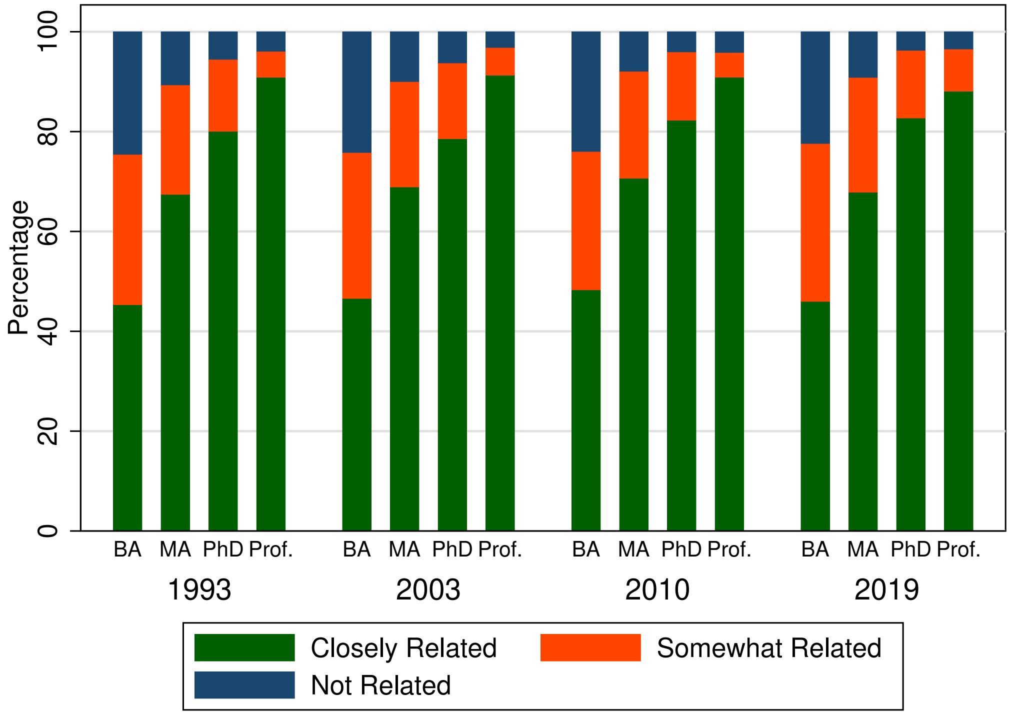

Figure 1. Closely Related, Somewhat Related and Not Related, by Degree and Year

293,184 observations across the four sample years, with a low of 50,765 in 2010 and a high of 108,452 in 1993. All analyses use the NSCG-provided weights to create a nationally representative sample.

We use the annualized salary information as our primary outcome of interest. To make the estimated penalties more comparable across waves, we use real salary values instead of nominal salary values. From 2003 onward, other than different top-coding values, the salary question is unchanged. However, in 1993, the salary question was collected differently than in later years. In the Data Appendix, we describe in detail how the salary variable was adjusted to make the surveys as comparable as possible. In brief, we adjusted the top-codes to have the same (inflation-adjusted) level as in 1993 and multiplied each top coded salary by 1.5, and we annualized the salaries in 2003 onward by first calculating the worker's hourly wage and converting that value to an annual salary. Also, salary information for part-time workers is not available in the 1993 survey; thus, as our goal is to make the survey years as similar as possible, our analysis focuses only on full-time workers.[3]

Figure 1 shows the fraction of workers who are in jobs that are closely related, somewhat related, and not related, separately by degree and year. Across years, both the rate of mismatch, both in terms of those

[3]Only the 1993 and 2003 samples directly ask about part-time vs full-time status, while in 2003 onward the number of hours typically worked is included. We define full-time using the direct question in 1993 and 2003, and in 2010 onward define full-time as typically working 35 or more hours per week.

Table 1. Descriptive Statistics

	1993		2003		2010		2019	
	Related	Not Rel.	Related	Not Rel.	Related	Not Rel.	Related	Not Rel.
Somewhat Related	0.31	0.00	0.31	0.00	0.29	0.00	0.33	0.00
	(0.46)	(0.00)	(0.46)	(0.00)	(0.45)	(0.00)	(0.47)	(0.00)
Female	0.39	0.40	0.43	0.41	0.46	0.46	0.50	0.48
	(0.49)	(0.49)	(0.50)	(0.49)	(0.50)	(0.50)	(0.50)	(0.50)
Age	42.04	42.16	43.38	44.16	42.79	43.21	42.07	42.06
	(9.21)	(8.79)	(9.67)	(9.81)	(10.80)	(11.32)	(11.27)	(11.07)
Salary	85,344	68,296	87,765	68,836	82,807	61,338	83,277	61,854
	(50,100)	(43,682)	(58,880)	(52,316)	(54,471)	(49,519)	(53,685)	(44,276)
Degree								
BA	0.58	0.83	0.58	0.83	0.58	0.85	0.58	0.83
	(0.49)	(0.38)	(0.49)	(0.38)	(0.49)	(0.35)	(0.49)	(0.38)
MA	0.28	0.15	0.30	0.15	0.31	0.12	0.31	0.15
	(0.45)	(0.35)	(0.46)	(0.35)	(0.46)	(0.33)	(0.46)	(0.36)
PhD	0.05	0.01	0.04	0.01	0.04	0.01	0.05	0.01
	(0.21)	(0.11)	(0.20)	(0.11)	(0.20)	(0.09)	(0.21)	(0.09)
Professional	0.09	0.02	0.08	0.01	0.07	0.01	0.06	0.01
	(0.28)	(0.12)	(0.27)	(0.11)	(0.26)	(0.12)	(0.24)	(0.10)
Race/Ethnicity								
White	0.88	0.86	0.84	0.84	0.82	0.81	0.77	0.77
	(0.32)	(0.34)	(0.37)	(0.37)	(0.38)	(0.39)	(0.42)	(0.42)
Black	0.06	0.06	0.07	0.07	0.07	0.09	0.08	0.11
	(0.24)	(0.25)	(0.25)	(0.26)	(0.26)	(0.28)	(0.27)	(0.31)
Asian	0.05	0.06	0.07	0.07	0.09	0.07	0.11	0.08
	(0.22)	(0.24)	(0.26)	(0.25)	(0.28)	(0.26)	(0.31)	(0.27)
Other	0.01	0.01	0.02	0.02	0.02	0.03	0.04	0.05
	(0.10)	(0.11)	(0.15)	(0.15)	(0.15)	(0.17)	(0.19)	(0.22)
Hispanic	0.03	0.03	0.05	0.06	0.07	0.07	0.09	0.11
	(0.17)	(0.18)	(0.23)	(0.23)	(0.26)	(0.26)	(0.29)	(0.32)
Married	0.73	0.68	0.74	0.67	0.69	0.61	0.65	0.56
	(0.44)	(0.47)	(0.44)	(0.47)	(0.46)	(0.49)	(0.48)	(0.50)
Immigrant	0.09	0.10	0.13	0.14	0.15	0.14	0.18	0.15
	(0.28)	(0.30)	(0.34)	(0.35)	(0.36)	(0.35)	(0.38)	(0.35)
Observations	91,008	17,444	60,843	9,927	44,512	6,253	55,908	7,289

Notes: Age measured in years, and salary is measured in 2017 dollars. All other variables are percent. Sources: 1993, 2003, 2010, and 2019 National Survey of College Graduates.

who are in a somewhat related job as well as an unrelated job, has remained quite stable, both overall as well as within degree. Around 25% of individuals with a BA report working in an unrelated job, while only 10% of those with an MA and 4-5% of those with a PhD or professional degree are in unrelated jobs. Thus, mismatch appears to be a largely BA-related phenomenon.

Table 1 shows key descriptive statistics of our sample separately by those who report working in a somewhat or closely related job versus not related. Not surprisingly, in each year, salary is lower for workers in a job not related to their degree, compared to those in a closely related or somewhat related job. Age, race and ethnicity, marital status and immigrant status are similar in all four years between the related

Table 2. Probability of Being Mismatched, by Year and Group

	1993	2003	2010	2019
Overall	0.187	0.183	0.176	0.169
Gender:				
Men	0.184	0.191	0.176	0.175
Women	0.192	0.173	0.177	0.163
Degree:				
BA	0.246	0.242	0.240	0.224
MA	0.107	0.100	0.079	0.092
PhD	0.055	0.062	0.040	0.037
Professional	0.039	0.032	0.042	0.035
Race:				
White	0.184	0.183	0.175	0.168
Black	0.202	0.197	0.207	0.215
Asian	0.217	0.174	0.148	0.123
Other	0.201	0.195	0.224	0.214
Ethnicity:				
Non-Hispanic	0.187	0.183	0.177	0.165
Hispanic	0.194	0.191	0.172	0.205
Marital Status:				
Single	0.218	0.219	0.212	0.204
Married	0.175	0.169	0.160	0.149
Immigrant Status:				
Native-born	0.185	0.182	0.179	0.174
Immigrant	0.210	0.189	0.161	0.145

Notes: Table shows the percentage of each group in each year that is in a job not related to their degree. Sources: 1993, 2003, 2010, and 2019 National Survey of College Graduates.

and not related groups, though Asian and immigrant workers have become relatively more represented in the related group than the not related group, suggesting their rates of mismatch have decreased over time. Major demographic trends are present, with large increases in the fraction of female, Hispanic, Asian, and immigrant workers over time in both related and unrelated jobs, as well as a reduction in the fraction of married workers.

Table 2 shows the fraction of each group, e.g. BA holders or immigrants, who report being in a job not related to their degree, including the fraction of men and women in each year who are mismatched. Racial differences in mismatch were small in 1993, though over time, the mismatch rate for whites dropped slightly while for blacks it rose slightly, and for Asians it fell substantially: from 21.7% in 1993 to 12.3% in 2019. While the mismatch rate among immigrants is similar to the overall rate in 1993, the rate shrunk by 5.5 percentage points between 1993 and 2019. These results, as well as the results for Asians, are both consistent with an increase in the prevalence of highly educated, largely H-1B immigrants, from Asian countries (notably India) who, due to nature of the H-1B visa program, are almost guaranteed to be properly matched in their job to their degree. Finally, married workers are less likely to be mismatched than single workers in each year, though the gap between the groups rose from 4.3 percentage points in 1993 to 5.5 percentage points in 2019.

The NSCG also asks for the most important reason why an individual does not work in a job related

Table 3. Most Important Reason for Working in Job Unrelated to Field of Degree

Panel A: Men

	1993	2003	2010	2019
Pay, promotions	0.35	0.35	0.37	0.36
	(0.48)	(0.48)	(0.48)	(0.48)
Work conditions	0.08	0.09	0.10	0.10
	(0.27)	(0.29)	(0.30)	(0.30)
Job location	0.04	0.07	0.06	0.08
	(0.20)	(0.25)	(0.24)	(0.27)
Change career	0.19	0.20	0.18	0.18
	(0.39)	(0.40)	(0.38)	(0.38)
Family	0.06	0.06	0.06	0.07
	(0.24)	(0.24)	(0.24)	(0.25)
No job available	0.16	0.14	0.18	0.16
	(0.37)	(0.35)	(0.39)	(0.37)
Other	0.12	0.08	0.06	0.07
	(0.33)	(0.27)	(0.23)	(0.25)
Observations	10,618	6,023	3,585	3,924

Panel B: Women

	1993	2003	2010	2019
Pay, promotions	0.24	0.26	0.28	0.28
	(0.42)	(0.44)	(0.45)	(0.45)
Work conditions	0.08	0.12	0.11	0.11
	(0.27)	(0.32)	(0.31)	(0.31)
Job location	0.04	0.06	0.06	0.08
	(0.19)	(0.23)	(0.24)	(0.27)
Change career	0.21	0.19	0.19	0.19
	(0.41)	(0.40)	(0.39)	(0.39)
Family	0.12	0.15	0.09	0.12
	(0.33)	(0.36)	(0.28)	(0.32)
No job available	0.18	0.15	0.22	0.16
	(0.39)	(0.36)	(0.41)	(0.37)
Other	0.13	0.07	0.05	0.07
	(0.34)	(0.25)	(0.22)	(0.25)
Observations	6,826	3,904	2,668	3,365

Notes: Sample includes workers who report working in a job not related to their field of degree. All values are percent. Sources: 1993, 2003, 2010, and 2019 National Survey of College Graduates.

to their degree. Table 3 shows the means of these values separately for men and women (Panels A and B, respectively) and separately by year. For both men and women, “Pay or promotion” is the most cited reason for being mismatched, followed by “Change career” and then “No job available”. Women are much more likely than men to report being mismatched for family reasons. During the period examined, the reasons for mismatch have remained mostly constant, though job location has become more relevant for both men and women. Overall, no major trends over time in the most important reason for being mismatched is evident, which is important for our analysis since the cause of mismatch affects the size of the mismatch penalty (Robst 2007b).

4 Results: Increasing Mismatch Penalty Over Time

We measure the wage penalty from a mismatch between degree and job using the following Mincerian regression:

$$lnS_{it} = \beta_0^t + \beta_X^t X_{it} + \beta_{SomewhatRelated}^t SomewhatRelated_{it} + \beta_{NotRelated}^t NotRelated_{it} + \epsilon_{it} \quad (1)$$

where lnS_{it} is the dependent variable and is the log of annual salary of worker i in year t, X_{it} is a vector of individual characteristics including age (as a third-order polynomial), region, highest degree (BA, MA, PhD, or Professional degree), field of highest degree, field of BA (if highest degree is above BA), race (white, black, Asian, or other), and dummy variables for Hispanic, immigrant, and married. $SomewhatRelated_{it}$ and $NotRelated_{it}$ are dummy variables that equal one if the worker's job is somewhat related/not related to their degree, and zero otherwise, where the omitted group is working in a job that is closely related to their degree. The coefficients of interest are $\beta_{SomewhatRelated}^t$ and $\beta_{NotRelated}^t$, and they capture the partial mismatch penalty ($\beta_{SomewhatRelated}^t$) and the full mismatch penalty ($\beta_{NotRelated}^t$) in year t, both relative to workers in a job that closely matches their degree.

Table 4 shows the $\beta_{SomewhatRelated}^t$ and $\beta_{NotRelated}^t$ coefficients from equation (1) separately by year for the full sample (Panel A), men (Panel B), and women (Panel C). In the full sample of men and women, in 1993 the penalty for workers in a job not related to their degree was 15.0%;[4] this penalty rose to 20.2% in 2003, and it increased again to 23.5% in 2010. Between 2010 and 2019, the penalty declined slightly to 22.7%. Thus, between 1993 and 2019, the penalty to working in a not related job rose by 51.3% (15.0% to 22.7%). The penalty to being somewhat mismatched also rose over time, and unlike the penalty for mismatch, it continued to increase throughout the entire sample period. Specifically, it increased from 2.9% in 1993, to 6.1% in 2003, to 7.2% in 2010, and finally to 8.1% in 2019. In both the somewhat related and not related cases, the increase in the mismatch penalty from 1993 to 2019 is statistically significant at the 5% level.

Panels B and C reveal increasing trends in the penalty for both men and women. In 1993, women had a lower not related penalty than men (13.8% vs 16.3%). Between 1993 and 2003, the penalty rose similarly for men and women (30.7% for men and 37.0% for women). However, from 2003 to 2010, the not related penalty rose much more for men (25.3%) than for women (6.3%). One potential cause for this difference is that 2010 was just after the end of the Great Recession, when unemployment was still very high. Men experienced a greater change in employment during the Great Recession (Sahin et al., 2010). Thus, it is possible that the factors that led to greater job loss for men during the Great Recession might have also led

[4]Our 1993 wage penalty results are 4 percentage points higher than Robst's (2007a) due to omitted controls for full-time and part-time experience and job training. We dropped these controls to maintain consistency across samples, since they are largely unavailable after the 1993 sample.

Table 4. OLS Regressions, Log of Annual Salary and Job Somewhat Related or Unrelated To Degree Field

Panel A: All

	1993	2003	2010	2019
Somewhat Related	-0.029***	-0.061***	-0.072***	-0.081***
	(0.005)	(0.007)	(0.015)	(0.012)
Not Related	-0.150***	-0.202***	-0.235***	-0.227***
	(0.006)	(0.009)	(0.019)	(0.016)
N	105,618	68,977	49,242	63,197

Panel B: Men

	1993	2003	2010	2019
Somewhat Related	-0.037***	-0.062***	-0.101***	-0.077***
	(0.006)	(0.009)	(0.020)	(0.016)
Not Related	-0.163***	-0.213***	-0.267***	-0.229***
	(0.008)	(0.012)	(0.026)	(0.021)
N	67,153	42,569	29,870	36,828

Panel C: Women

	1993	2003	2010	2019
Somewhat Related	-0.026***	-0.068***	-0.047*	-0.085***
	(0.007)	(0.010)	(0.022)	(0.017)
Not Related	-0.138***	-0.189***	-0.201***	-0.218***
	(0.009)	(0.014)	(0.027)	(0.023)
N	38,465	26,408	19,372	26,369

Notes: Robust standard errors shown in parentheses. Dependent variable is log of annual salary. Omitted category is working in a job closely related to field of highest degree. All specifications include controls for region, age as a third-order polynomial, highest degree (BA, MA, PhD, or professional degree), race (white, black, Asian, or other), dummy variables for Hispanic, immigrant, and married, and field of highest degree (measured using minor code). Panel A includes gender dummy variable. Sources: 1993, 2003, 2010, and 2019 National Survey of College Graduates. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

to a greater mismatch penalty for men. Overall, from 1993 to 2019, the not related penalty rose by 40.5% for men and 58.0% for women, suggesting that job mismatch has become more costly for all workers over time, but especially for women.[5] The penalty for being in a somewhat related job also rose for both men and women between 1993 to 2019, by 108.1% for men (3.7% to 7.7%) and 226.9% (2.6% to 8.5%) for women.

One potential explanation for the increase over time is that survey respondents interpreted the question differently across waves. However, given the wording of the question and responses were very consistent across survey waves, it seems unlikely that respondents were systematically changing their interpretation of the question. We of course have no way to contact survey respondents and verify this though.

Having established a large increase in the mismatch penalty from 1993 to 2019, the remainder of the paper investigates multiple potential explanations for this finding.

[5]For men and women, the difference in the not related penalty between 1993 and 2019 is significant at the 10% and 5% levels, respectively.

5 Explaining the Increasing Mismatch Penalty

5.1 Field of Study Composition

The mismatch penalty varies significantly by field of study (Robst, 2007a), with fields such as computer science and engineering exhibiting large mismatch penalties, while other fields like liberal arts have little to no penalty. As a result, even if the mismatch penalty within field of study has remained constant over time, changes in the composition of fields of study could help to explain the overall increase in the mismatch penalty documented above.

We decompose the changes in the mismatch penalty based on changes in the composition of field of study over time, versus changes in the mismatch penalty within field of study over time. We perform this decomposition by fixing the field-specific mismatch penalty to either the 1993 or 2019 levels and comparing how the penalty would have evolved based only on changes in composition of field of study with how the mismatch penalty actually evolved. To calculate the field of study-specific mismatch penalty in a given year, we estimate a modified version of equation (1):

$$\begin{aligned} lnS_{it} =& \beta_0^t + \beta_X^t X_{it} + \sum_j \beta_{SomewhatRelated}^{t,j} SomewhatRelated_{it} * 1[degfield_{it} = j] \\ &+ \sum_j \beta_{NotRelated}^{t,j} NotRelated_{it} * 1[degfield_{it} = j] + \epsilon_{it} \end{aligned} \tag{2}$$

where j refers to field of highest degree, and $1[degfield_{it} = j]$ equals 1 if individual i in period t has field of degree equal to j, and zero otherwise. All other controls are identical to equation (1). The $\beta_{NotRelated}^{t,j}$ coefficients provide our field j mismatch penalty in year t.

Using these estimates, we fix the penalties to their value in a given year but allow the composition by field of study to evolve over time. Results are shown in Table 5, with Panel A showing the results from the full sample, while Panels B and C show the results for men and women.[6] Between 1993 and 2019, the mismatch penalty rose by 7.7 percentage points (-0.130 -(-0.207)). Fixing the mismatch penalty for each field of study to the level from 1993, the penalty would have risen by only 1.9 percentage points (-0.130 -(-0.149)), which represents 24.7% (1.9/7.7) of the total increase.

In Panels B and C, we re-estimate equation (2) for men and women separately, i.e. we allow the field of study mismatch penalty to vary by gender, and we also allow the field of study composition to differ by gender. The overall increase in the mismatch penalty is very similar between gender: 7.7 percentage points

[6] Note that the mean value for the "Penalty", as shown in Table 5 do not exactly match the coefficients in Table 4, since in Table 4 we are showing a coefficient value from equation (1), while in Table 5 we are averaging across coefficients from equation (2). However, the differences in the penalty estimates are very similar in each year, and so our analysis is largely unaffected.

Table 5. Mean Not Related Penalty Using Penalty within Field of Study by Year

Panel A: All

	1993	2003	2010	2019
Penalty	-0.130	-0.179	-0.217	-0.207
	(0.122)	(0.120)	(0.152)	(0.149)
Penalty: 1993	-0.130	-0.141	-0.152	-0.149
	(0.122)	(0.127)	(0.128)	(0.129)
Penalty: 2019	-0.187	-0.200	-0.210	-0.207
	(0.139)	(0.145)	(0.148)	(0.149)
Observations	17,444	9,927	6,253	7,289

Panel B: Men

	1993	2003	2010	2019
Penalty	-0.133	-0.184	-0.238	-0.210
	(0.138)	(0.145)	(0.194)	(0.147)
Penalty: 1993	-0.133	-0.139	-0.150	-0.145
	(0.138)	(0.138)	(0.134)	(0.135)
Penalty: 2019	-0.200	-0.205	-0.213	-0.210
	(0.153)	(0.149)	(0.149)	(0.147)
Observations	10,618	6,023	3,585	3,924

Panel C: Women

	1993	2003	2010	2019
Penalty	-0.125	-0.174	-0.193	-0.201
	(0.104)	(0.108)	(0.124)	(0.175)
Penalty: 1993	-0.125	-0.143	-0.157	-0.156
	(0.104)	(0.120)	(0.123)	(0.128)
Penalty: 2019	-0.160	-0.184	-0.194	-0.201
	(0.133)	(0.154)	(0.154)	(0.175)
Observations	6,826	3,904	2,668	3,365

Notes: This table shows the mean mismatch penalty by year. Results are based off of the $\beta^{t,j}_{NotRelated}$ coefficient from equation (2). Row "Penalty" is the mean value of the coefficient from the given year; row "Penalty: 1993" is the mean value of the coefficient from the 1993 regression; and row "Penalty: 2013" is the mean value of the coefficient from the 2013 regression. Sources: 1993, 2003, 2010, and 2019 National Survey of College Graduates.

for men and 7.6 percentage points for women. However, changes in the composition of field of study appear to be more relevant for women than men. For men, fixing the mismatch penalty to the 1993 level, the penalty would have risen by only 1.2 percentage points, so only 15.6% of the total increase. For women, by fixing the mismatch penalty to the 1993 level, we find that the penalty would have risen by 3.1 percentage points, or 40.8% of the total amount. We conclude that, though the primary cause of the increasing mismatch penalty over time is due to within-field of study changes, changes in the composition of field of study over time has contributed meaningfully to the increasing mismatch penalty, especially for women.

5.2 Field of Study Characteristics

While the previous section documents an important role for changes in the composition of field of study over time in explaining the overall increase in the mismatch penalty, most of the increase appears to be driven by within-field changes. In this section, we explore whether the mismatch penalty within field of study has evolved over time differently by characteristics of the field. Skill-biased technical change (SBTC) - which describes the increasing returns to skill over the past several decades - may have led to changes in the mismatch penalty across different fields of study differently impacted by SBTC (see Acemoglu and Autor, 2011, for a discussion of SBTC). Also, the increase in the college wage premium associated with SBTC could have certainly impacted who selects into college and each field of study across time. For example, Lindley and McIntosh (2015) use data from the UK to first document large variation in wage distributions over time, including large variation existing within the same field of study. They find that the changing distribution of test scores, and thus the underlying ability of those attending college, is especially important. Thus, it could be important to test for differences within field of study over time.[7]

Specifically, we focus on whether STEM-related fields of study have experienced different trends in their mismatch penalty over time.[8] We again turn to the field of study-year level as our unit of analysis to determine whether field of study characteristics can explain the rise in mismatch penalty. Using equation (2), we extract the field-specific mismatch penalty for each year by obtaining the $\beta^{t,j}_{NotRelated}$ coefficients for each field of study j in year t. These coefficients are the dependent variables in our secondary regressions, where an observation is a field of study in a given year.[9] To illustrate our estimation, the following is the specification that includes controls for survey year and STEM, which corresponds to column (3) in Table 6:

$$\beta^{t,j}_{NotRelated} = \alpha_0 + \alpha_t Year_t + \alpha_{STEM} STEM_j + \epsilon_{tj} \tag{3}$$

where $Year_t$ are year fixed effects, with 1993 as the omitted year, $STEM_j$ is a dummy variable that equals one if field of study j is a STEM field, and zero otherwise. The coefficients α_t capture the extent to which the mismatch penalty has changed by degree over time, while α_{STEM} captures the degree to which STEM and non-STEM fields differ in their mismatch penalty.

Column (1) includes controls for only survey year. Consistent with our main results in Table 4, we observe

[7]We also tested for changes across cohorts to address the concern of grade inflation or lowering of college standards over time. However, we do not find a clear pattern of more recent cohorts facing larger penalties.

[8]We define the following as STEM fields: Computer and information sciences, Mathematical sciences, Agricultural and food sciences, Biological sciences, Environmental life sciences, Chemistry, except biochemistry, Earth science, geology, and oceanography, Physics and astronomy, Other physical sciences, Aerospace and related engineering, Chemical engineering, Civil and architectural engineering, Electrical and related engineering, Industrial engineering, Mechanical engineering, Other engineering, Health-related fields, and Technology and Technical Fields.

[9]Each field of degree-year observation is weighted by the number of workers in that field in each year, where we re-weight the samples such that each year contributes the same total weight to the regression.

an increase in the mismatch penalty over time. This increase captures both changes in composition across fields over time as well as changes within each field of study in the mismatch penalty. Adding field of study controls in column (2) shrinks the mismatch penalty in each year, relative to 1993; for instance, without field of study controls (column 1) the mismatch penalty is 7.9 percentage points greater in 2019 than 1993 but with field of study controls (column 2) it is only 5.5 percentage points, a 30.4% reduction. Adding field of study effects controls for changing composition over time, and thus these results are consistent with our earlier results regarding the relative importance of composition effects.

Column (3) adds a control for whether the field of study is STEM-related, where we now omit field of study effects. The mismatch penalty is substantially larger (9.2 percentage points) within STEM fields compared to non-STEM fields. Column (4) adds interaction terms between year and STEM degrees. The omitted year is 1993, so the additional STEM coefficients test whether there has been a change in the mismatch penalty within STEM fields over time. We find little evidence that the mismatch penalty has changed over time in STEM-related fields differently than non-STEM fields. Also, comparing columns (3) and (4), we observe an inconsistent change in the year coefficients, suggesting that accounting for changing penalties in STEM fields does not help to explain the overall increasing penalty over time. Finally, column (5) adds the full field of study controls (which subsumes the STEM variable, though we are still able to identify changes in the penalty within STEM fields over time). Comparing columns (2) and (5), we again see little evidence that changes in the mismatch penalty in STEM fields is useful in explaining the increasing mismatch penalty over time.

In summary, though STEM-related fields have much higher mismatch penalties on average than non-STEM fields, there is little evidence that this difference has changed over time, or that accounting for changes in mismatch penalty in STEM fields over time can meaningfully help to explain the overall increasing mismatch penalty after 1993.

5.3 Occupation-Specific Demand Shifts

Previous research has found that demand shocks have hit some occupations much more than others because of SBTC. For example, Autor and Dorn (2013) documents that wage and employment growth has not been the same across the entire distribution of occupations. So, we next test how the estimated mismatch penalty changes when we add occupational controls.[10] Results are shown in Table 7, where for each year we also show the results from Table 4 without occupation controls (columns 1, 3, 5, and 7) for ease of comparison. Now, the increase in the mismatch penalty over time is somewhat mitigated. In the full sample, with

[10]To maximize consistency in occupation codes across years, we use the occupation codes from Altonji and Zhong (2021). See subsection A.2 for additional details.

Table 6. OLS Regressions, Mismatch Penalty by Field of Study-Year

	(1)	(2)	(3)	(4)	(5)
2003	-0.051	-0.041**	-0.050	-0.047	-0.039*
	(0.056)	(0.015)	(0.056)	(0.076)	(0.018)
2010	-0.072	-0.054**	-0.070	-0.078	-0.064**
	(0.065)	(0.019)	(0.067)	(0.091)	(0.023)
2019	-0.079	-0.055***	-0.074	-0.064	-0.050**
	(0.060)	(0.014)	(0.060)	(0.082)	(0.016)
STEM			-0.092*	-0.087	
			(0.037)	(0.071)	
x2003				-0.012	-0.008
				(0.096)	(0.031)
x2010				0.025	0.036
				(0.109)	(0.038)
x2019				-0.029	-0.013
				(0.108)	(0.034)
Field of Study	No	Yes	No	No	Yes
N	122	122	122	122	122

Notes: Robust standard errors shown in parentheses. Dependent variable the field of study-year mismatch penalty, which are the $\beta^{t,j}_{NotRelated}$ coefficients from equation (2). Omitted year is 2019. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

occupation controls the not related penalty rose from 13.1% in 1993 (column 2) to 17.7% in 2019 (column 8), a more modest increase of 35.1% compared to the 51.3% increase found when excluding occupation controls. For men, the not related penalty increased relatively little in the presence of occupation controls (14.0% to 16.3%, so a 16.4% increase), though for women, a large 57.1% increase (from 11.9% to 18.7%) between 1993 and 2019 is present.[11] The increase in the somewhat mismatched penalty is also smaller with the inclusion of occupation controls as it now only increases from a 5.1 percent penalty in 1993 to a 7.3 percent penalty in 2019 (43.1% increase). For men, the somewhat mismatched penalty increases by 18.2% between 1993 and 2019, which is much smaller than the 108.1% increase without occupation controls; for women, the somewhat mismatched penalty more than doubled without occupational controls, but with occupational controls it increased by only 62.7% between 1993 and 2019.

The type of mismatch used for identifying the penalty differ between Table 4 and Table 7. In Table 4 mismatch can occur through someone being in an occupation that does not match the degree field or through being in an occupation that matches but having a job within that occupation that does not. In Table 7, since occupational controls are added, the penalty can only come from job variation within the same occupation. So, finding a large growth in the mismatch penalty for men in Table 4, but not in Table 7, suggests that the penalty for men has largely been driven by men working in occupations that do not match their degree. For women the mismatch is not driven by occupational choice, but rather by the types of jobs within occupation.

Put differently, in 1993, being mismatched in your job provided a smaller amount of occupation-specific

[11] The 1993 and 2019 coefficients on not related penalty for women in Table 7 differ at the 10% level of significance, while for men, the coefficients do not differ statistically at conventional levels.

Table 7. OLS Regressions, Log of Annual Salary and Job Somewhat Related or Unrelated To Degree Field, With Occupation Controls

Panel A: All

	1993		2003		2010		2019	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Somewhat Related	-0.029***	-0.051***	-0.061***	-0.063***	-0.072***	-0.071***	-0.081***	-0.073***
	(0.005)	(0.005)	(0.007)	(0.007)	(0.015)	(0.014)	(0.012)	(0.011)
Not Related	-0.150***	-0.131***	-0.202***	-0.170***	-0.235***	-0.164***	-0.227***	-0.177***
	(0.006)	(0.006)	(0.009)	(0.010)	(0.019)	(0.019)	(0.016)	(0.016)
Occupation	No	Yes	No	Yes	No	Yes	No	Yes
N	105,618	103,183	68,977	68,238	49,242	48,956	63,197	62,884

Panel B: Men

	1993		2003		2010		2019	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Somewhat Related	-0.037***	-0.055***	-0.062***	-0.061***	-0.101***	-0.094***	-0.077***	-0.065***
	(0.006)	(0.006)	(0.009)	(0.009)	(0.020)	(0.019)	(0.016)	(0.015)
Not Related	-0.163***	-0.140***	-0.213***	-0.174***	-0.267***	-0.185***	-0.229***	-0.163***
	(0.008)	(0.008)	(0.012)	(0.013)	(0.026)	(0.025)	(0.021)	(0.021)
Occupation	No	Yes	No	Yes	No	Yes	No	Yes
N	67,153	65,706	42,569	42,193	29,870	29,735	36,828	36,692

Panel C: Women

	1993		2003		2010		2019	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Somewhat Related	-0.026***	-0.051***	-0.068***	-0.071***	-0.047*	-0.048*	-0.085***	-0.083***
	(0.007)	(0.007)	(0.010)	(0.011)	(0.022)	(0.021)	(0.017)	(0.017)
Not Related	-0.138***	-0.119***	-0.189***	-0.164***	-0.201***	-0.146***	-0.218***	-0.187***
	(0.009)	(0.010)	(0.014)	(0.015)	(0.027)	(0.028)	(0.023)	(0.022)
Occupation	No	Yes	No	Yes	No	Yes	No	Yes
N	38,465	37,477	26,408	26,045	19,372	19,221	26,369	26,192

Notes: Robust standard errors shown in parentheses. Dependent variable is log of annual salary. Omitted category is working in a job closely related to field of highest degree. All specifications include controls for region, age as a third-order polynomial, highest degree (BA, MA, PhD, or professional degree), race (white, black, Asian, or other), dummy variables for Hispanic, immigrant, and married, field of highest degree (measured using minor code), and field of first BA. Occupation is measured using the codes from Altonji and Zhong (2021). Panel A includes gender dummy variable. Sources: 1993, 2003, 2010, and 2019 National Survey of College Graduates. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

information, controlling for other demographic characteristics (compare columns 1 and 2 in Table 7), than in 2019, especially for men. This result suggests that occupations that have enjoyed the largest increase in returns over time may be occupations where mismatch is relatively uncommon, and thus being mismatched is a more negative indication of occupational returns in 2019 than in 1993, particularly for men.

The differences in importance of occupation for men and women may be driven by the fact there is occupational sorting by gender (for an overview of this literature see Cortes and Pan, 2018). Given this sorting, one could imagine that the occupations men tend to select into are the types of occupation most benefiting from SBTC (mainly STEM-related occupations) and therefore have experienced the largest increase in demand and wages over this time. Thus, if a man is mismatched due to being an in occupation that does not match his field, he is likely giving up a much higher wage in more recent times.

We further explore the relationship between occupational returns and mismatch by estimating the following:

$$lnS_{it} = \beta_0^t + \beta_X^t X_{it} + \sum_k \beta_k^t * 1[occupation_{it} = k] + \epsilon_{it} \tag{4}$$

where, as before X_{it} is a vector containing the same demographic controls (age, etc.), but we introduce occupation controls, and β_k^t are the occupation k returns, relative to the omitted occupation, in year t. Note that we do not include controls for mismatch in this specification.

We use the β_k^t coefficients from Equation 4 and compare them to the average level of mismatch in an occupation in each year. We do this for the full sample, as well as separately by gender, where we also calculate the mean mismatch level within occupation separately by gender. Table 8 shows the correlations between occupational returns and the average level of mismatch in that occupation.[12] In 1993, the correlation between the average level of mismatch and the occupational returns was -0.401 overall, which indicates that occupations where mismatch is more common experienced lower occupational returns. The relationship was slightly stronger for men (-0.447) than for women (-0.379). Over time, however, we see a large divergence in the correlation between the mean mismatch level and occupational returns between men and women: by 2019, for men, the correlation had strengthened to -0.575, while for women, it had remained nearly constant at -0.375.

Thus, for women, being mismatched in 2019 sends approximately the same (negative) signal regarding occupational returns - net of demographic characteristics - as it did in 1993. For men, however, being mismatched in 2019 is more strongly (negatively) related to occupational returns than it was in 1993. Hence,

[12]Each observation is an occupation in a given year, and we weight each observation by the total survey weight of workers in that occupation in that year.

Table 8. Correlations between Occupation Returns and Average Level of Mismatch in Occupation

	1993	2003	2010	2019
All	-0.401	-0.463	-0.524	-0.475
Men	-0.447	-0.531	-0.575	-0.575
Women	-0.379	-0.406	-0.440	-0.375

Notes: Table shows the correlation coefficient between the occupational returns, measured from the β_k^t coefficients in Equation 4, and the average level of mismatch by occupation. Sources: 1993, 2003, 2010, and 2019 National Survey of College Graduates.

adding controls for occupation flattens the increase in the mismatch penalty over time for men significantly more than it does for women.

5.4 Vertical Mismatch

The key survey question asked in the NSCG that we exploit states: "To what extent was your work on your principal job related to your highest degree?" One interpretation of this question is how a worker's field of study relates to their job ("horizontal match"). Indeed, follow-up questions that are asked of workers who report being in a job unrelated to their degree explicitly ask about working outside the field of your highest degree, which strengthens interpreting this question from the perspective of field of degree mismatch. Nonetheless, workers might interpret this question from the perspective of over/required/under education ("vertical match"), where workers who are in jobs that require less/more education than the worker has are "overeducated"/"undereducated", and may reasonably respond to this survey question as working in an unrelated job. Thus, changes in the mismatch penalty over time may in part be attributable to changes in the penalties for vertical mismatch.

The first thing we do to test whether mismatched workers tend to work in occupations mostly filled by workers of different levels of education is to compare occupational congruence across groups. We explore this idea by dividing our NSCG sample into four total groups: two education levels (BA and MA) by relatedness (somewhat or closely related, and not related). For brevity, we omit PhD and professional degree levels, since mismatch is relatively rare in those groups and thus few mismatched workers are in those levels of education. We then compute the degree of similarity in the occupational distribution between each pair of groups (k and l) using Welch's (1999) index of congruence:

$$G_{kl} = \frac{\sum_c (q_{kc} - \bar{q}_c)(q_{lc} - \bar{q}_c)/\bar{q}_c}{\sqrt{(\sum_c (q_{kc} - \bar{q}_c)^2/\bar{q}_c)(\sum_c (q_{lc} - \bar{q}_c)^2/\bar{q}_c)}} \tag{5}$$

where q_{kc} and q_{lc} gives the fraction of groups k and l in occupation c, respectively, while $\bar{q}_c$ gives the fraction

Table 9. Occupational Congruence

		Some College	BA		MA	
			Not Rel.	Related	Not Rel.	Related
Some College		1.000				
BA	Not Related	-0.111	1.000			
	Related	-0.941	0.005	1.000		
MA	Not Related	-0.405	0.865	0.242	1.000	
	Related	-0.841	-0.169	0.665	0.197	1.000

Notes: Table shows the occupational congruence between the groups in the row and column, calculated using the approach in Welch (1999). Sources: 1993, 2003, 2010, and 2019 National Survey of College Graduates, the 1990 and 2000 decennial Census, and the 2003, 2010, and 2019 American Community Survey. BA and MA data are derived from the NSCG samples, while Some College data is derived from the Census and American Community Survey.

of the total population in occupation c. If two groups k and l have identical occupational distributions, their congruence would equal positive one, while if their occupational distributions were completely different, the value would equal negative one. Thus, values closer to positive one indicate more similar occupational distributions between two groups.

To compare college graduates from the NSCG to workers at lower levels of education, we utilize data from the 1990 and 2000 decennial Census, as well as the 2003, 2010, and 2019 1-year ACS samples, chosen to match our primary NSCG years. Specifically, we extract a sample of "Some college" workers, which becomes our fifth group. Additional details regarding the construction of this pooled sample and occupational crosswalks are discussed in subsection A.2.

Table 9 shows the results from equation (2) for each G_{kl} combination, where we pool all years and both men and women together.[13] First, we find the occupational congruence between matched (i.e., "Related") BA holders and matched MA holders is .665, one of the highest levels of occupational congruence among the groups of workers. For mismatched (i.e., "Not Related") BA holders and mismatched MA holders the occupational congruence is even more similar with a value of .865. Relative to workers with 'Some College' level of education, the closest group is mismatched BA holders (-0.111), which is a much greater congruence than that between some college and matched BA holders (-0.941). These results are consistent with mismatched workers "downgrading" occupationally.

To further examine the role of vertical mismatch, we compare required education levels for the occupation with actual education level of the worker. If we find that mismatched workers are more likely to have education levels exceeding the requirement, this suggests the high level of congruence between mismatched MA and mismatched BA workers is due to these workers being "downgraded" and that mismatched workers may be competing for jobs with workers who have lower levels of education. The perspective of vertical mismatch may help to explain the increasing job mismatch penalty under two conditions: first, if workers

[13] Though not shown here, we also calculated the congruence values both separately by survey year, as well as separately by gender. Overall, the results do not change qualitatively from those presented here, and thus for brevity we show only the full sample results.

who report working in a job not related to their degree are more likely to be overeducated; and second, if the returns to "excess" education, i.e. education above the required level in an occupation, have declined over time. To measure the extent of overeducation by job relatedness in the NSCG, we require using a representative sample of the US working population. Thus, we again make use of the 1990 and 2000 decennial Census samples, as well as the 2003, 2010, and 2019 1-year ACS samples.

As discussed in the background section, there are multiple methods that have been employed to determine vertical mismatch, each with its own set of pros and cons. The NSCG does not explicitly ask what the required level of school is and thus we cannot use a self-reported measure to compare with their actual education level. Following Robst (2008) we determine the required schooling by using Census data to ensure our sample is not restricted to only those with a college degree. He importantly notes that while each measure can produce a different amount of mismatch, the estimated wage penalties are often similar. Specifically, we measure the modal educational requirement by occupation, utilizing the "realized matched" method (Chiswick and Miller, 2008). Wen and Maani (2023) argue that the modal method is preferred to the mean method because it is more sensitive to changes in skill requirements due to technological change, a concern during this time frame. This approach uses the actual level of education of workers in an occupation to determine how much education is needed, rather than professional assessments of the necessary education levels. While there are concerns about the two-sided matching process in the labor market causing problems, the upside is that the required level of education naturally changes over time and could thus more accurately reflect the true requirements. While professional assessments address some of the concerns associated with realized matches, finding professional assessments that have been updated over time to accurately reflect requirements is a challenge. Thus, for each occupation in a given year, we calculate the modal education level in that occupation, which we consider the "required" level of education.[14] Workers with more than this level of education are "overeducated", while those with less are "undereducated".[15]

We then take this required education level per occupation to the NSCG, where for each worker, we now have their actual level of education as well as the level of education required to perform their job. We show the average required level of education by degree, as well as by level of job match relatedness in Table 10.[16] We find that well-matched workers are, on average, in jobs that required higher levels of education than workers with the same level of education but who are mismatched; in other words, mismatched workers are more

[14]We match the 1993 NSCG sample to the 1990 Census sample, and the remaining NSCG samples are matched to their respective ACS years.

[15]We replicated our analysis but using the Bureau of Labor Statistics-provided assessment of educational requirement by occupation. Specifically, the returns to overeducation for women are unchanged from 1993 to 2019, while we find a larger increase in the returns to overeducation for men. However, since this approach does not allow the required level of education to evolve over time, we view these results as inadequate for addressing how the returns to overeducation have changed over time.

[16]For simplicity, we pool all NSCG years together, though our results are qualitatively unchanged when we show the results separately by year. The results for men and women separately are very similar, so for brevity, we show only the full sample.

Table 10. Required Education Level, by Degree and Job Relatedness

	BA	MA	PhD	Prof
Closely Related	15.66	16.40	17.12	17.70
	(1.73)	(1.66)	(1.54)	(1.01)
Somewhat Related	14.79	15.50	16.00	15.50
	(1.98)	(1.83)	(1.91)	(2.23)
Not Related	13.92	14.54	15.06	14.72
	(2.07)	(2.14)	(2.14)	(2.21)
Observations	160,348	92,508	22,475	17,853

Notes: Table shows the required level of education, based off of calculations from the U.S. Census from 1990 to 2019, of a worker's occupation level. Sources: 1993, 2003, 2010, and 2019 National Survey of College Graduates.

likely to be overeducated. For BA holders, the difference in the required level of education between workers in a closely related versus not-related job is 1.7 years (15.66-13.92). A slightly higher gap is observed among MA holders, while the gap is even higher for Professional and PhD-level degrees. The gap in educational requirements between closely related and somewhat related workers is around half of the gap observed between closely related and not related workers. Undereducation in this sample is rare, since the NSCG contains only college graduates. Thus, these differences in the required years of schooling between matched and mismatched workers almost entirely represent differences in terms of years of "excess" schooling.

Having established that overeducation is more common among workers who report working in a job not related to their degree, we now assess whether the returns to excess education have changed over time using our Census samples. Following Chiswick and Miller (2008), for each Census year, we estimate the following annual log earnings regressions for individual i in year t:

$$lnY_{it} = \alpha_0 + \alpha_X^t X_{it} + \alpha_{Over}^t Over_{it} + \alpha_{Required}^t Required_{it} + \alpha_{Under}^t Under_{it} + \epsilon_{it} \tag{6}$$

where lnY_{it} is log of annual earned income, $Over_{it}$ is years of education above the required level for the occupation (equals zero for those who are not overeducated), $Under_{it}$ is years of education below the required level for the occupation (equals zero for those who are not undereducated), and $Required_{it}$ is the required years of education for the occupation.[17] X_{it} is a vector of controls that includes age (as a third-order polynomial), state of residence, race, Hispanic indicator, immigrant indicator, and marital status.

The coefficient on years of overeducation (α_{Over}^t) is expected to be positive but smaller than the coefficient on required years of education ($\alpha_{Required}^t$), which would be consistent with additional years of schooling above those required to perform your job being rewarded in the labor market at a lower rate than years up to the required level. We are specifically interested in how α_{Over}^t has changed over time: if this coefficient has

[17] For the Census regressions, we calculate the required education level using the IPUMS-provided *occ1990* occupation code.

shrunk, then this would imply that "extra" schooling is being rewarded at a lower rate than in the past, and because mismatched workers are around one and a half years more overeducated than matched workers, the declining returns to overeducation would help to explain the increasing mismatch penalty over time.

We estimate Equation 6 on a sample of full-time workers ages 22 to 64 with positive earned income and positive weeks worked, separately by survey year. To match the NSCG sample, our regressions include only workers with a BA or higher. Results are shown in Table 11, with the full sample shown in Panel A, and the results for men and women shown in Panels B and C.

Considering the full sample, in 1990, for each year of schooling exceeding the amount required for the occupation, annual earned income increased by 3.6%, which is significantly lower than the 10.8% for each year of schooling up to that required by their occupation. The difference in these coefficients is evidence of a discounting of "excess" education in the labor market, though note that in 1990, workers still enjoyed higher earnings for each year of schooling above their occupation's required level. Over time, however, the returns to an "extra" year of schooling fell; from 3.6% in 1990 to 2.3% in 2019, a difference of 1.3 percentage points, or a 36.1% decrease.

Comparing men and women separately, an interesting pattern emerges. For men, the returns to excess schooling in 1990 was only 2.2% per year, and increased between 1990 and 2019 to 4.0% per year. This increase would tend to *lower* the mismatch penalty over time. For women, however, we observe a marked reduction in the returns to excess education, from 5.0% in 1990 to only 2.0% in 2019, a 3.0 percentage point (60.0%) drop. Thus, the overall decline in the returns to excess education among college educated workers appears to be driven entirely by women.

The above results suggest that excess education has become less valuable over time, at least among college graduates. Since workers who reported working in unrelated jobs are more likely to be overeducated, we would expect that their earnings - relative to well-matched workers - should decline. To provide a back-of-the-envelope calculation, consider that among BA holders in the NSCG, the required level of education for those in a job unrelated to their degree is 1.7 years lower than those in a closely related job. Given the overall decline in the returns to excess education from 1993 to 2019 of 1.3 percentage points, this translates to earnings that are: $1.3*1.7 = 2.2$ percentage points lower. Note that, as shown in Table 4, the mismatch penalty rose 7.7 percentage points, and so the drop in the returns to excess education may explain around 29% of the decline.

Considering men and women separately, however, the increase in the returns to excess education for men implies that the mismatch penalty should have shrunk over time, all else being equal. Clearly, at least for men, other factors are at work. For women, however, the declining returns to excess education among college graduates can explain a large portion of the increasing mismatch penalty: given the 3.0 percentage point

Table 11. OLS Regressions, Log of Annual Earned Income, Over/Required/Under Education, U.S. Census

Panel A: All

	1990	2000	2003	2010	2019
Over Education	0.036***	0.038***	0.040***	0.029***	0.023***
	(0.001)	(0.001)	(0.003)	(0.002)	(0.002)
Required Education	0.108***	0.115***	0.138***	0.123***	0.117***
	(0.001)	(0.001)	(0.003)	(0.002)	(0.002)
Under Education	-0.284***	-0.273***	-0.234***	-0.273***	-0.294***
	(0.002)	(0.002)	(0.006)	(0.002)	(0.003)
N	1,148,077	1,486,075	152,254	386,934	472,727

Panel B: Men

	1990	2000	2003	2010	2019
Over Education	0.022***	0.032***	0.041***	0.050***	0.040***
	(0.001)	(0.001)	(0.004)	(0.002)	(0.002)
Required Education	0.097***	0.115***	0.143***	0.148***	0.143***
	(0.001)	(0.001)	(0.004)	(0.002)	(0.002)
Under Education	-0.303***	-0.302***	-0.258***	-0.298***	-0.334***
	(0.003)	(0.003)	(0.009)	(0.004)	(0.004)
N	696,824	835,028	84,454	204,611	240,458

Panel C: Women

	1990	2000	2003	2010	2019
Over Education	0.050***	0.039***	0.039***	0.019***	0.020***
	(0.001)	(0.001)	(0.004)	(0.003)	(0.002)
Required Education	0.124***	0.117***	0.135***	0.114***	0.113***
	(0.001)	(0.001)	(0.004)	(0.002)	(0.002)
Under Education	-0.222***	-0.221***	-0.180***	-0.211***	-0.237***
	(0.003)	(0.003)	(0.008)	(0.003)	(0.003)
N	451,253	651,047	67,800	182,323	232,269

Notes: Robust standard errors shown in parentheses. Dependent variable is log of annual salary. Omitted category is working in a job closely related to field of highest degree. All specifications include controls for region, age as a third-order polynomial, highest degree (BA, MA, PhD, or professional degree), race (white, black, Asian, or other), dummy variables for Hispanic, immigrant, and married, and field of highest degree (measured using minor code). Panel A includes gender dummy variable. Sources: 1990 and 2000 U.S. Decennial Census, and 2003, 2010, and 2019 1-year American Community Survey.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

drop in the returns to excess education from 1993 to 2019 for women, and 1.7 additional years of excess schooling for mismatched workers, we would predict a change of: 3.0*1.7 = 5.1 percentage points, which is well over half of the overall increase in the mismatch penalty for women of 8.0 percentage points.

5.5 Reason for Mismatch

As shown in Table 3, there are several reasons why a worker may be in a job outside of their degree field, and the reason why they are mismatched may affect the size of their mismatch penalty. Examining how the mismatch penalty by most important reason has evolved over time may provide some insights into

Table 12. OLS Regressions, Log of Annual Salary and Job Unrelated To Degree Field, by Reason Most Important

Panel A: Men

	1993	2003	2010	2019
Pay, promotion	0.033**	0.021	-0.015	-0.009
	(0.011)	(0.018)	(0.033)	(0.034)
Work conditions	-0.235***	-0.284***	-0.382***	-0.320***
	(0.022)	(0.033)	(0.061)	(0.048)
Job location	-0.326***	-0.375***	-0.292***	-0.384***
	(0.028)	(0.042)	(0.072)	(0.060)
Change career	-0.122***	-0.182***	-0.209***	-0.109**
	(0.015)	(0.023)	(0.061)	(0.039)
Family	-0.212***	-0.280***	-0.338***	-0.370***
	(0.027)	(0.042)	(0.079)	(0.075)
No job available	-0.327***	-0.414***	-0.465***	-0.382***
	(0.015)	(0.022)	(0.053)	(0.037)
Other	-0.301***	-0.358***	-0.444***	-0.377***
	(0.020)	(0.032)	(0.082)	(0.053)
N	67,153	42,569	29,870	36,828

Panel B: Women

	1993	2003	2010	2019
Pay, promotion	0.082***	0.077***	0.069	0.015
	(0.014)	(0.021)	(0.047)	(0.035)
Work conditions	-0.211***	-0.220***	-0.239***	-0.319***
	(0.023)	(0.040)	(0.058)	(0.065)
Job location	-0.266***	-0.258***	-0.264***	-0.188**
	(0.029)	(0.039)	(0.058)	(0.063)
Change career	-0.082***	-0.152***	-0.171**	-0.064
	(0.017)	(0.028)	(0.054)	(0.043)
Family	-0.279***	-0.343***	-0.399***	-0.356***
	(0.022)	(0.032)	(0.071)	(0.052)
No job available	-0.233***	-0.286***	-0.383***	-0.374***
	(0.016)	(0.029)	(0.041)	(0.042)
Other	-0.208***	-0.276***	-0.182*	-0.355***
	(0.021)	(0.057)	(0.074)	(0.061)
N	38,465	26,408	19,372	26,369

Notes: Robust standard errors shown in parentheses. Dependent variable is log of annual salary. Omitted category is working in a job closely or somewhat related to field of highest degree. All specifications include controls for region, age as a third-order polynomial, highest degree (BA, MA, PhD, or professional degree), race (white, black, Asian, or other), dummy variables for Hispanic, immigrant, and married, and field of highest degree (measured using minor code). Sources: 1993, 2003, 2010, and 2019 National Survey of College Graduates. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

understanding the underlying reasons for the increasing mismatch penalty.

We re-estimate equation (1), but where we allow the mismatch penalty to differ depending on the most important reason cited for being in a job not related to the field of highest degree. Results are shown in Table 12. Note that only those who are "Not Related" to their degree are asked this survey question; those who report being in a "Somewhat Related" job are not included. Thus, the omitted group is now workers in either a closely related or somewhat related job. For brevity, we show only the results for men and women separately, omitting the full sample results.

In 1993, workers who are mismatched due to "Pay or promotion" (the most common reason for mismatch) have a premium, for both men and women, compared to closely or somewhat related workers. For the other six reported reasons, all exhibit (often substantial) wage penalties. For example, women in 1993 who are mismatched due to family reasons have a 27.9% wage penalty, while for men, that value is 21.2%.

Comparing the mismatch penalties within reason but over time, we notice an increase (i.e. more negative) in the mismatch penalty for nearly all reported reasons. For men, all but one reason for mismatch ("Change career") had a higher mismatch penalty in 2019 than in 1993; for women, all but two reasons for mismatch ("Job location" and "Change career") had a higher mismatch penalty in 2019 than 1993.[18]

In summary, the increasing mismatch penalty appears to be largely general across the most important reason for mismatch, and our results do not point to any particular reason (or reasons) for mismatch that may be driving the overall expansion of the mismatch penalty over time.

6 Conclusion

Using data from the NSCG, we document a large increase between 1993 and 2010 in the wage penalty for college educated workers who report working in a job not related to their degree. Between 2010 and 2019, the penalty declined slightly, but remained much higher than the 1993 level. The increase in the mismatch penalty has been larger for women than men, though men experienced an especially large mismatch penalty in 2010, i.e. just after the Great Recession. We also document large increases in the penalty to being somewhat mismatched over time, again for both men and women.

We test several potential explanations for the observed increase in the mismatch penalty over time. For women, we find that almost half of the increase can be attributed to a change in the composition of field of study over time, which plays a much smaller role for men. For men, a key factor seems to be occupation-specific demand shocks, as the increase in the mismatch penalty is greatly reduced once occupation controls are included in the analysis. Further, for men, the correlation between occupational returns and average level of mismatch in an occupation has increased over time, indicating that, conditional on the control variables, being mismatched has become more negatively associated with occupational returns over time. For women, however, the relationship between occupational returns and mismatch has changed little.

Finally, we provide evidence from the U.S. Census between 1990 and 2019 that the returns to excess education - defined as years of schooling above what is required to perform a job - have declined over time for women, though not for men. Since mismatched workers in the NSCG tend to work in occupations

[18]Using the full sample between 1993 and 2019, the mismatch penalty increased for 5 of the 7 reasons, and two of those five ("No job available" and "Other") are statistically different between 1993 and 2019 at conventional levels. For the two that decreased ("Job location" and "Change career"), the changes differences over time were small and statistically insignificant (p-values of 0.82 and 0.71, respectively).

with lower educational requirements, they therefore have greater levels of “excess” schooling. As a result, decreases in the returns to this excess schooling over time for women can help to explain most of their increasing mismatch penalty.

The causes of the mismatch penalty are complex, and thus there are likely many contributing factors that have affected the evolution of the mismatch penalty over time. This paper aims to both document the increase mismatch penalty and shed light on some potential contributing factors. We do not, however, consider our results to be the end of the story, and we anticipate future work that can both validate our findings with independent data, if possible, as well as provide more insights into why the mismatch penalty has expanded over time.

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A Data Appendix

A.1 Annual Salary

In this section, we elaborate on the steps taken to make the NSCG surveys as comparable as possible, especially with regards to salary. Fortunately, nearly all the variables included in our analysis have remained unchanged from 1993 onward. One exception is that only the 1993 and 2003 samples directly ask about part-time vs full-time status, while in 2003 onward the number of hours typically worked is included. We define full-time using the direct question in 1993 and 2003, and in 2010 onward define full-time as typically working 35 or more hours per week.

The major exception, however, is annual salary, which changed significantly in several important ways between the 1993 and 2003 surveys, though has remained largely unchanged since. In all surveys, individuals are asked for their "basic annual salary" before deductions in their job of the reference week. In 2003 and in all subsequent surveys, for workers who are not salaried, they are asked to "estimate your earned income, excluding business expenses." In 1993, however, non-salaried workers are asked for their earned income, and then are instructed to choose the unit of time over which they earn that income (i.e. per hour, week, month, year, academic year, or other). Another notable difference between 1993 and later surveys, which is relevant for how we treat their annualized salaries, is that though weeks worked and typical hours per week worked are asked in 2003 onward, they are not asked in 1993.

The public use data for the 1993 NSCG does not contain the salary as reported; instead, an annualized salary is included in the data files. The annualized salary is calculated by multiplying the reported per-unit earned income (say, per hour) by what a full-time worker would work, i.e. 40 hours per week times 52 weeks per year for a per-hour worker, 52 for a per-week worker, and 12 for a per-month worker. For part-time workers, no annualized salary information is presented, and hence we drop part-time workers from all surveys.

Clearly the 1993 and subsequent surveys contain earnings data that are, in their provided form, not directly comparable. We take several steps to make the salary data as comparable as possible between all survey years. Our approach mostly follows that used by Hunt (2010). First, in 1993, we make no adjustments to the reported salary data, other than adjusting for inflation using the Consumer Price Index.[19] In 2003 and all subsequent surveys, we first impose the same inflation-adjusted top coding that exists in 1993 ($150,000 in 1993 dollars) to the reported annualized salary. Then, we calculate the worker's implied hourly wage, given their annualized salary, weeks worked, and typical hours worked per week. We drop observations where the hourly wage is lower than the federal minimum wage in that year. Finally, to calculate our annualized salary used in cross-year comparisons, we multiply the hourly wage by 40 hours/week, and multiply again by 52 weeks.

A.2 Occupational Coding and Matching Some College Sample

To get a sample of workers with some college level of education, we must rely on a dataset beyond the NSCG, as the NSCG includes only college graduates. We use the 1990 and 2000 decennial Census, as well as the 2003, 2010, and 2019 1-year ACS. We choose these samples to correspond to our main NSCG analysis years of 1993, 2003, 2010, and 2019.

The first issue we encounter is that the occupational coding schemes differ between the Census/ACS and the NSCG. We use the occupation codes from Altonji and Zhong (2021) - with some modifications - where a new set of uniform occupation codes - with 67 unique values - are defined. They then develop a series of crosswalks to match from the Census/ACS *occ1990* occupation codes to these newly defined uniform occupation codes, as well as crosswalks to match from the NSCG-provided occupation codes to the new uniform codes.[20] The end result after utilizing these crosswalks is that, in both our Census/ACS samples as well as our NSCG samples, common occupation codes are available. Our congruence calculations are carried out using these occupation codes, and we use these codes when we perform any occupation-specific analyses since they are consistently defined across NSCG survey years.

A second issue is that, because they derive from different surveys, we need to ensure that the observation weighting scales of the Census/ACS sample of some college matches the NSCG sample of BA or higher. We

[19] We use CPI data provided by the Minneapolis Federal Reserve: https://www.minneapolisfed.org/about-us/monetary-policy/inflation-calculator/consumer-price-index-1800-.

[20] Our thanks to Joseph Altonji and Ling Zhong for providing us with the occupational crosswalks.

do this by first calculating the fraction of BA to some college in the Census/ACS in the years mentioned above, which gives us a value of 0.66, i.e. there are 66% as many BA holders as there are those with only some college level of education. We then calculate an adjustment factor that needs to be used to change the Census/ACS observation weights so that in our final sample that includes the Census/ACS and NSCG, we achieve the same 0.66 ratio between BA and some college. We calculate an adjustment factor of 0.93, meaning that we modify the Census/ACS sample weights by multiplying each by 0.93. This achieves the same BA/some college ratio (0.66) in our combined sample as we observed the Census/ACS datasets.