ESSAYS IN LABOR ECONOMICS

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ESSAYS IN LABOR ECONOMICS

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This dissertation examines the effects of social and economic context on civic participation, human capital accumulation, and labor market outcomes.

In the first chapter,"Labor Market Conditions and Civic Participation", I examine the impact of prime-age individuals' employment opportunities on U.S. voter turnout from 1984-2016. I leverage a Bartik-style (shift-share) strategy that uses variation in industry and occupation composition across demographic groups and states to predict changes in employment opportunities. The effects of statewide versus own-group economic conditions are asymmetric: while improved statewide economic conditions decrease turnout, improved own-group conditions increase turnout. The magnitude indicates that a one standard deviation increase in labor demand for an individual's own group increases turnout by about 0.8 percentage points. This impact does not differ significantly by gender, but is stronger for non-Hispanic whites than non-Hispanic blacks. Rising own-group labor demand increases interest in voting and may increase individuals' sense that voting is a civic duty. Finally, I also find suggestive evidence that increased own-group labor demand is associated with greater participation in community projects.

In the second chapter, "The Brother Earnings Penalty", we examine the impact of sibling gender on adolescent experiences and adult labor market outcomes for a recent cohort of U.S. women. We document an earnings penalty from the presence of a younger brother (relative to a younger sister), finding that a next-youngest brother reduces adult earnings by about 7 percent. Using rich data on parent-child interactions, parents' expectations, disruptive behaviors, and adult outcomes, we provide a first step at examining the mechanisms behind this result. We find that brothers reduce parents' expectations and school monitoring of

female children while also increasing females' propensity to engage in more traditionally feminine tasks. These factors help explain a portion of the labor market penalty from brothers. This work is coauthored with Eleonora Patacchini.

In the third chapter, "Girls, Boys, and High Achievers", we study the effect of exposure to female and male "high-achievers" in high school on the long-run educational outcomes of their peers. Using data from a recent cohort of students in the United States, we identify a causal effect by exploiting quasi-random variation in the exposure of students to peers with highly-educated parents across cohorts within a school. We find that greater exposure to "high-achieving" boys, as proxied by their parents' education, decreases the likelihood that girls go on to complete a bachelor's degree, substituting the latter with junior college degrees. It also affects negatively their math and science grades and, in the long term, decreases labor force participation and increases fertility. We explore possible mechanisms and find that greater exposure leads to lower self-confidence and aspirations and to more risky behavior (including having a child before age 18). The girls most strongly affected are those in the bottom half of the ability distribution (as measured by the Peabody Picture Vocabulary Test), those with at least one college-educated parent, and those attending a school in the upper half of the socio-economic distribution. The effects are quantitatively important: an increase of one standard deviation in the percent of "high-achieving" boys decreases the probability of obtaining a bachelor's degree from 2.2-4.5 percentage points, depending on the group. Greater exposure to "high-achieving" girls, on the other hand, increases bachelor's degree attainment for girls in the lower half of the ability distribution, those without a college-educated parent, and those attending a school in the upper half of the socio-economic distribution. The effect of "high-achievers" on male outcomes is markedly different: boys are unaffected by "high-achievers" of either gender. This work is coauthored with Eleonora Patacchini and Raquel Fernández.

BIOGRAPHICAL SKETCH

Angela Cools received her B.A. in Economics from Pomona College in 2010. She worked as an undergraduate associate and financial analyst at Disney Consumer Products from 2010-2012 and as a research assistant at the Federal Reserve Bank of Boston from 2012-2014. From 2014-2019, she attended Cornell University to pursue a Ph.D. in Economics.

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CHAPTER 1

LABOR MARKET CONDITIONS AND CIVIC PARTICIPATION

1.1 Introduction

The impact of economic conditions on voting behavior has received growing attention in the context of the 2016 U.S. Presidential Election and the Brexit vote in Europe.[1] How do macroeconomic conditions affect turnout rates? And whose turnout is affected – those experiencing the greatest labor market upheaval or those more insulated from the shocks? Recent literature examines the first of these questions and finds that spikes in unemployment increase overall turnout (Charles and Stephens, 2013; Burden, Canon, Mayer, and Moynihan, 2014; Autor, Dorn, Hanson, and Majlesi, 2016; Cebula, 2017). However, given that unemployed people vote at lower rates on average [see, e.g., Rosenstone (1982); Emmenegger, Marx, and Schraff (2015)], it is unclear whether the increase is driven by those who become unemployed themselves or those less directly affected who might witness negative conditions on the news. Increases driven solely by the latter group are cause for concern since those who keep their jobs may not have the same preferences and needs as the unemployed. Furthermore, since voter turnout is an indicator of social capital (Putnam, 2001), increases occurring exclusively among the latter group may indicate that those experiencing the most negative economic circumstances forfeit the benefits of strong community engagement and connections. In this paper, I examine the impact of economic circumstances on voter turnout focusing on different responses to conditions facing an individual and her demographic group versus the state as a whole.

The primary challenge in estimating the relationship between employment and turnout is that individual characteristics such as motivation and intelligence and state policies such

[1] See, for example, https://www.nytimes.com/2016/12/13/business/economy/jobs-economy-voters.html; https://www.theguardian.com/commentisfree/2016/jun/17/britain-working-class-revolt-eu-referendum.

as transfer programs affect both employment and turnout rates. To address this concern, I proxy for employment with a Bartik index, which is a measure of local labor demand that exploits variation in industry and occupation concentration across demographic groups and geographic areas. The identifying assumption for this strategy is that, after controls and fixed effects, labor demand shocks to particular industries and occupations at the national level are unrelated to voter turnout except through their impacts on local economic conditions. I provide various tests in support of this identifying assumption in Section 1.5. I use data from the Current Population Survey (CPS) Voter Supplement from 1984-2016 and focus on individuals in their prime working years (i.e. ages 25-54) since this group is most likely to be employed and has been shown to have the strongest relationship between employment opportunities and social outcomes.[2]

I find evidence of different responses to state-level versus own economic conditions. Improvements in state labor market conditions depress turnout but improvements in own labor market conditions increase turnout. Specifically, a one standard deviation increase in the labor demand index for one's own demographic group increases voter turnout by 0.8 percentage points. This result is robust to the inclusion of linear time trends and various controls for changing unionization rates. The effect does not differ substantially by gender but is stronger for non-Hispanic whites than non-Hispanic blacks. I then find suggestive evidence that good own-group employment opportunities increase interest in voting and may increase the sense that voting is a civic duty. Finally, I also test for similar effects on other forms of social engagement and find that greater own labor demand increases participation in community projects.

This paper makes two main contributions. First, it resolves a puzzle in the voting literature. Studies at the individual level have found a strong, positive correlation between employment and voting rates [see, e.g., Rosenstone (1982); Emmenegger et al. (2015)]. Showing that

[2]For example, see Hetschko, Knabe, and Schöb (2013); Breuer (2015); Charles, Hurst, and Schwartz (2018).

this effect may in fact be causal, Corman, Dave, and Reichman (2017) find that welfare reform increased voting in presidential elections by 2-3 percentage points among the target population largely through its impacts on employment.[3] In contrast to this individual level evidence, however, the literature examining aggregate voting finds that economic expansions at the county, commuting zone, and state level *decrease* voting rates (Charles and Stephens, 2013; Burden et al., 2014; Autor et al., 2016; Cebula, 2017). Previous literature has not determined whether the differences in studies at the individual versus aggregate level are due to legitimately different effects or to bias (e.g., omitted variable bias) in the individual correlations between employment and voting.

In this paper, I show that there are in fact different responses to state-level versus own economic conditions. Falling employment opportunities *increase* turnout generally but *decrease* turnout for the individuals whose groups are most negatively affected by the shock. With respect to mechanisms, an individual's own employment may increase her own interest in the political process and sense that voting is a civic duty. Meanwhile, statewide expansions (reflected in indicators such as the state unemployment rate) may increase her confidence in the performance of politicians and decrease her voting rate.[4,5]

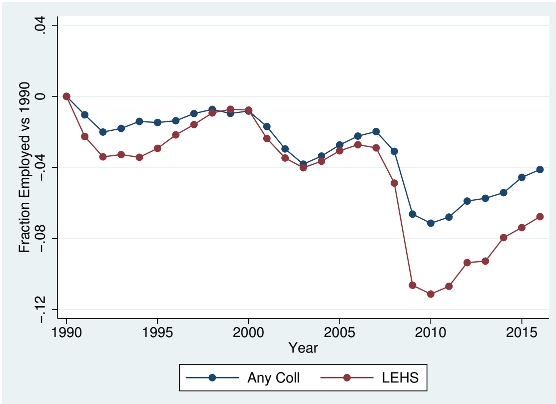
Second, this paper also contributes to the growing literature that seeks to understand the social impacts of ongoing declines in employment and wages for less-educated Americans [see Figure 1.1 and Krueger (2017); Abraham and Kearney (2018)]. For example, declines in employment have been tied to increased opioid use, especially for less-educated individuals (Charles et al., 2018). Furthermore, negative shocks to male labor markets have been tied

[3] Given that employment increased by 6-10 percentage points, Corman et al. (2017) note that the imputed IV for the effect of employment on voting is .19 to .36.

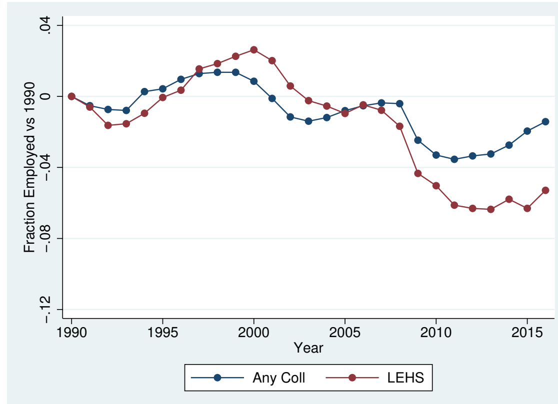
[4] The theory that individuals use aggregate, rather than individual, conditions to judge economic performance of politicians was originally put forward by Kinder and Kiewiet (1979), who show that individuals report subjective economic conditions related to aggregate statistics rather than individual circumstances between 1956-1976. Lim and Sander (2013) and Smets and Van Ham (2013) provide evidence of these associations for a more recent period. However, causal tests of this theory have not been implemented.

[5] Recent papers examining other social outcomes have highlighted differential responses of own-group versus aggregate economic shocks [see, e.g., Stevens, Miller, Page, and Filipski (2015), for the impact of economic shocks on mortality].

Figure 1.1: Employment-Population Ratio versus 1990 (Ages 25-54)



(a) Males



(b) Females

Source: Author calculations from the Current Population Survey. LEHS refers individuals having a high school degree or less education. Any Coll refers to individuals having at least some college education.

to lower fertility, lower rates of marriage, and worse child health, among other outcomes [see, e.g., Blau, Kahn, and Waldfogel (2000); Schaller (2016); Page, Schaller, and Simon (2017); Shenhav (2017); Autor, Dorn, and Hanson (2018); Lindo, Schaller, and Hansen (2018)]. This paper highlights negative consequences of falling employment and wages on civic engagement. In contrast to many previous studies, which find different consequences from male versus female employment, this paper finds no significant gender differences in the impact of employment on turnout.[6]

This paper is organized as follows. Section 1.2 discusses the theoretical mechanisms through which employment status may affect civic participation. Section 1.3 discusses the data used for the analysis, and Section 1.4 gives descriptive results about the relationship between employment and voting behavior. In Section 1.5, I describe my estimation strategy and, in Section 1.6, detail the main results. In Section 1.7, I examine mechanisms, and in Section 1.8 explore extensions. Section 1.9 concludes.

[6]For example, Page et al. (2017) finds that improvements to male labor markets improve child health, while improvements to female labor markets worsen child health. Similarly, Lindo et al. (2018) find that improvements to male labor markets decrease child maltreatment, while improvements to female labor markets increase child maltreatment.

1.2 Theoretical Overview

In recent years, a growing literature in economics, political science, and sociology has explored various models of civic participation.[7] These models of voting examine factors that change either the cost or benefit of turnout. In this section, I highlight the key mechanisms through which an individual's own employment opportunities and aggregate economic conditions may affect either the cost or benefit of voting and thus influence political participation. Good own-employment opportunities may increase civic participation through improved resources, increased mobilization, and psychological factors; may have ambiguous effects through political knowledge and support for government redistribution; and may decrease participation by increasing the opportunity cost of time. Good aggregate conditions may decrease turnout by increasing satisfaction with incumbent performance.

I first discuss how own-employment opportunities may increase civic participation. First, the "resource theory" of voter turnout suggests that voting is positively affected by access to resources (money, health, etc.), which may be obtained through employment (Brady, Verba, and Schlozman, 1995; Verba, Schlozman, and Brady, 1995). For example, income may enable an individual to buy a car and thus more easily get to the polls. If voting is a "normal good", greater income/resources should increase an individual's consumption of voting. Studies examining this resource theory have found mixed results. Income transfers from non-governmental agencies have been shown to have no immediate effect on voter turnout (though appear to increase turnout of children later in life) (Bagues and Esteve-Volart, 2016; Akee, Copeland, Costello, Holbein, and Simeonova, 2018). However, positive wealth shocks

[7] Smets and Van Ham (2013) divides the various models of voting into six categories: the resource model, the mobilization model, the socialization model, the rational choice model, the psychological model, and the political-institutional model. The resource model suggests that individuals' resources (education, money, etc.) can lower the cost of voting. The mobilization model claims that individuals are influenced to vote by those around them. The socialization model emphasizes the importance of family and social context in voting decisions. The rational choice model assumes individuals maximize utility given the costs and benefits of voting. The psychological model examines how factors such as information or efficacy may influence individuals' voting behavior. The political-institution model suggests that turnout is a function of the system of governance and institutional factors.

from changes in housing prices are associated with greater voter turnout (McCartney, 2017). In addition to these direct effects of income, greater resources may increase marriage, fertility, and duration at residence (and housing purchases). Marriage and housing purchases in particular may be complements to voting (Glaeser, Laibson, and Sacerdote, 2002; Smets and Van Ham, 2013; Bellettini, Ceroni, Cantoni, and Monfardini, 2018).

The second mechanism through which good own employment opportunities may increase turnout is through mobilization or recruitment. The "mobilization" theory of voter turnout suggests that individuals' turnout is influenced by social pressure from coworkers, friends, and political campaigns (Gerber and Green, 2000; Arceneaux and Nickerson, 2009). This theory suggests that employment increases turnout if individuals are recruited/mobilized to vote at their place of employment. Coworkers may encourage individuals to vote through social pressure, and workplace organizations such as unions may also encourage individuals to vote (Feigenbaum, Hertel-Fernandez, and Williamson, 2018). Freeman (1997) notes that the workplace can be an important site of recruitment for community involvement such as volunteering, and the same may apply to voting.

Finally, psychological theories suggest alternative ways in which own employment opportunities may increase political participation. In the political science literature, a number of studies in recent years have highlighted the importance of individual "efficacy" and feelings of "civic duty" to voter turnout. Efficacy has been defined as the "feeling that individual political action does have, or can have, an impact upon the political process, that is, that it is worthwhile to perform one's civic duties" [Campbell, Gurin, and Miller (1954), p. 187]. Efficacy includes both beliefs about one's own power to influence political outcomes (internal efficacy), and beliefs that politicians care about/respond to the concerns of the individual (external efficacy) (Balch, 1974). Employment has been tied to greater feelings of efficacy, because employment increases self-esteem and makes individuals more likely to feel well-represented by politicians (McKee-Ryan, Song, Wanberg, and Kinicki, 2005). Higher levels of efficacy are in turn tied to higher political participation (Bandura, 1997; Morrell, 2003; Beaumont, 2011; Marx and Nguyen, 2016).[8] Comparing political participation of Dutch twins, Emmenegger et al. (2015) directly find that employment increases political participation by increasing individual efficacy.[9] Finally, feelings of civic duty have been shown to strongly affect voting behavior (Blais and Achen, 2017; Goodman, 2018). Employment may increase feelings of civic duty, through workplace organizations or other factors (Kohler, 1994).[10]

In contrast to the three mechanisms described above, there are three ways in which own employment may have ambiguous or negative impacts on voter turnout: political knowledge, changing support for redistribution, and the opportunity cost of time. Having better information about candidates and issues can cause individuals to vote more often because uncertainty or ambiguity aversion imposes higher costs on voting in the absence of political information [see Strömberg (2004); Oberholzer-Gee and Waldfogel (2009), among others]. However, it is not clear whether employment should increase or decrease political knowledge. Notably, coworkers and workplace organizations may provide individuals with information about candidates, issues, and voting procedures (Feigenbaum et al., 2018). At the same time, employment may reduce time available for consumption of political news (Charles and Stephens, 2013). Second, individual economic success has been shown to reduce support for redistributive policies (Margalit, 2013; Karadja, Mollerstrom, and Seim, 2017). Thus, individuals who already support weak levels of redistribution (conservatives) may find their positions strengthened by employment, and individuals who already support high redistribution may find their positions weakened.[11] Since strong political preferences

[8]Other studies have noted that those who feel relatively powerless are less likely to vote. For example, Sugie (2015) finds that partner incarceration reduces political participation (voting, registration, and beliefs about the value of voting in elections).

[9]In addition to efficacy, general life satisfaction may affect civic engagement. Fang, Galambos, Johnson, and Krahn (2018) shows a strong association between an individual's happiness and her future level of civic engagement.

[10]For example, Bowler and Donovan (2013) shows a link between income and duty to vote in the United Kingdom.

[11]Brunner, Ross, and Washington (2011) find that positive economic shocks reduce support for redistribution in California.

are tied to higher political participation (Smets and Van Ham, 2013), the mechanism of political preferences may lead to greater participation among conservatives, but weaker participation among liberals. Finally, employment and/or higher wages may decrease turnout by increasing the opportunity cost of time and thus making the process of voting and/or gathering information necessary to vote more "costly" (Glaeser et al., 2002).

In addition to the effects of own (or own-group) employment on turnout, state-level economic conditions may also affect turnout. A substantial literature indicates that voters blame incumbents for bad overall economic conditions (Lewis-Beck and Stegmaier, 2000). To the extent that individuals are mobilized by a desire to remove the incumbent from office, improvements to overall economic conditions may lower turnout. Interestingly, aggregate economic conditions have been shown to influence opinions of government performance more strongly that own employment experiences (Lim and Sander, 2013; Smets and Van Ham, 2013).

In summary, the literature suggests mechanisms through which own-employment opportunities may increase (resources, mobilization, and efficacy) or decrease (opportunity cost of time and evaluation of incumbents) voter turnout, as well as two way in which employment opportunities may have ambiguous effects. In contrast, the literature generally predicts that aggregate conditions will be negatively related to turnout. As a result, whether own employment opportunities have the same impact as statewide economic conditions is an empirical question.

1.3 Data

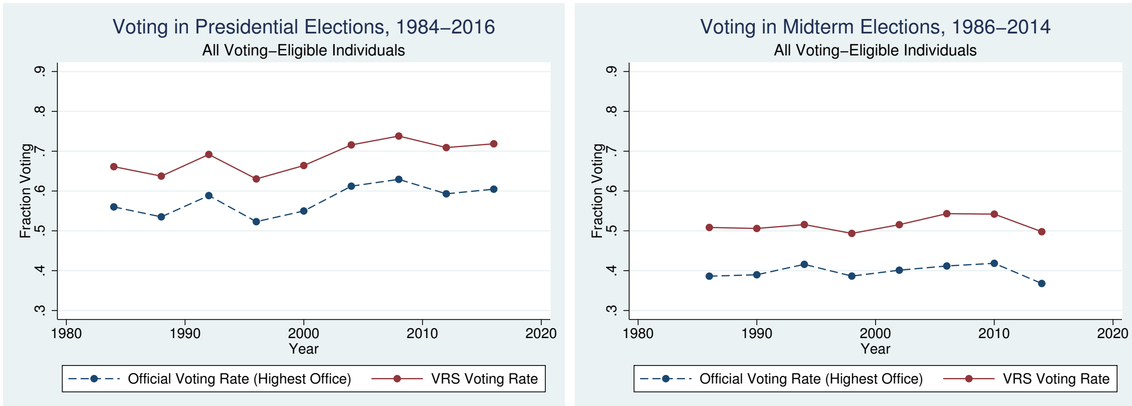
1.3.1 CPS Voting and Registration Supplement

Since this analysis relies on demographic characteristics (education, race, and gender) to identify own-group labor market conditions, it requires data on voting tied to information on demographic characteristics. This data is provided by the Current Population Survey Voting and Registration Supplement (CPS VRS), conducted every two years since 1964. In November of each election year, individuals responding to the basic CPS are asked a set of supplemental questions about their own voting behavior and that of others in their household in the VRS. In particular, if they reported being eligible to vote, they are asked whether or not they registered and voted in the most recent election. In recent years, individuals have also been asked additional questions such as the reason for not voting or not registering to vote.[12] The VRS has been referred to as the "gold standard" of survey data on voting behavior in the United States (Hur and Achen, 2013), and used in a variety of studies on turnout (Washington, 2006; Alvarez, Bailey, and Katz, 2008; Holbein and Hillygus, 2016; Corman et al., 2017). In contrast to official election statistics, which generally provide only information on aggregate turnout rates at the county or state level, the VRS allows voter turnout information for all individuals in the survey to be tied to their demographic characteristic such as age, race, educational attainment, and marital status.[13] As noted above, this demographic information is essential for the identification strategy in this paper, which uses an individual's demographic characteristics combined with her location to predict that individual's group's economic situation. In addition, relative to other surveys of political

[12] Since 1996, individuals who were registered but did not vote have been asked about their reasons for not voting. Since 2004, individuals who were not registered have been asked about their reasons for not registering.

[13] Since the questions in the VRS are asked of respondents from the basic monthly CPS, the individual voting responses can be tied to detailed demographic information for individuals (including information on age, race, level of education, employment, martial status, and duration at current residence).

Figure 1.2: VRS Versus Official Ballot Counts: Estimate of Voting Rates



(a) Presidential Elections

(b) Midterm Elections

Source: Author calculations from current Population Survey Voting and Registration Supplement downloaded from Flood et al. (2015) and United States Election Project http://www.electproject.org/home/voter-turnout/voter-turnout-data.

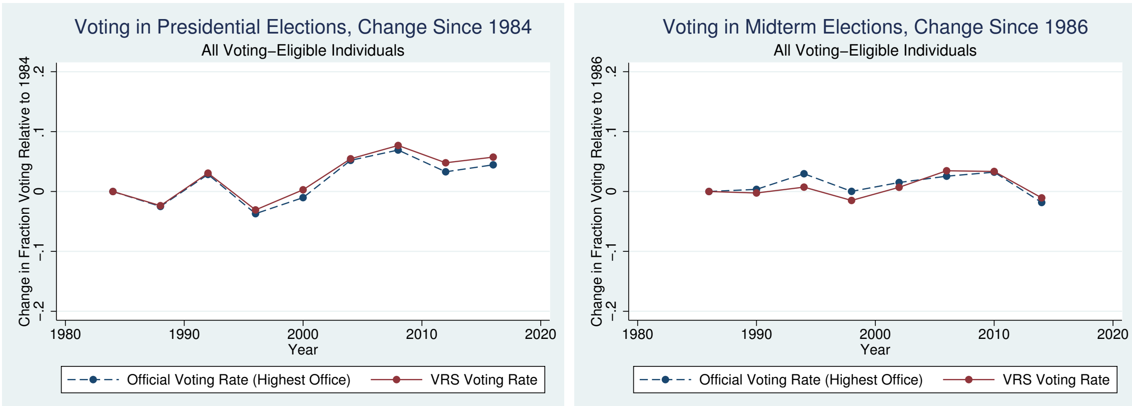
participation with demographic information (such as the American National Election Studies (ANES) or General Social Survey (GSS)), the VRS provides both a much larger sample size and higher response rates (DeBell, Krosnick, Gera, Yeager, and McDonald, 2018).

In general, the VRS reveals somewhat higher rates of turnout than official election statistics (DeBell et al., 2018). Figure 1.2 shows the rates of voter turnout reported in the VRS and official statistics in presidential (panel (a)) and midterm (panel (b)) elections from 1984-2016. The VRS turnout rate is calculated as the fraction of respondents reporting that they did vote divided by the total number of those answering the voting question.[14] The official rates are calculated as the number of individuals casting a vote for the highest office in a given election divided by an estimate of the voting-eligible population.[15] As shown in the figure, rates calculated from the VRS are about 10 percentage points higher than those calculated from official state vote tallies in every year between 1984 and 2016. This discrepancy results

[14]Those who are not eligible to vote or report that they do not know if they voted are not counted as respondents in this analysis. In official calculations, the Census Bureau has often engaged in the practice of counting those not responding to the voting question as not voting. However, as noted by Hur and Achen (2013), this procedure is not standard in survey research and produces inaccuracies in voting trends. However, my results are robust to using this specification for turnout rates.

[15]Official election statistics are obtained from the United States Election Project, at http://www.electproject.org/home/voter-turnout/voter-turnout-data.

Figure 1.3: VRS Versus Official Ballot Counts: Estimate of Voting Rates Versus Base Year



(a) Presidential Elections

(b) Midterm Elections

Source: Author calculations from current Population Survey Voting and Registration Supplement downloaded from Flood et al. (2015) and United States Election Project http://www.electproject.org/home/voter-turnout/voter-turnout-data.

from an underestimate of voter turnout from official state tallies and a likely overestimate from the VRS. Official state vote tallies underestimate voting rates because they exclude individuals whose ballots are rejected (or not received, in the case of absentee ballots) and individuals who did not vote for the highest office (president and/or governor/senator).[16] The VRS responses may be an overestimate due to non-response bias or over-reporting due to social pressure ("social desirability bias") (DeBell et al., 2018). As noted by Berent, Krosnick, and Lupia (2016), however, among those individuals whose survey responses about voting can validated by official records, self-reporting about voter turnout is highly accurate, suggesting a limited role for social desirability bias.[17]

Although the VRS and official turnout statistics report slightly different raw rates of voter turnout, this analysis requires only that they report similar *changes* in voting rates over time. That is because the analysis used in this paper focuses on voting behavior of groups

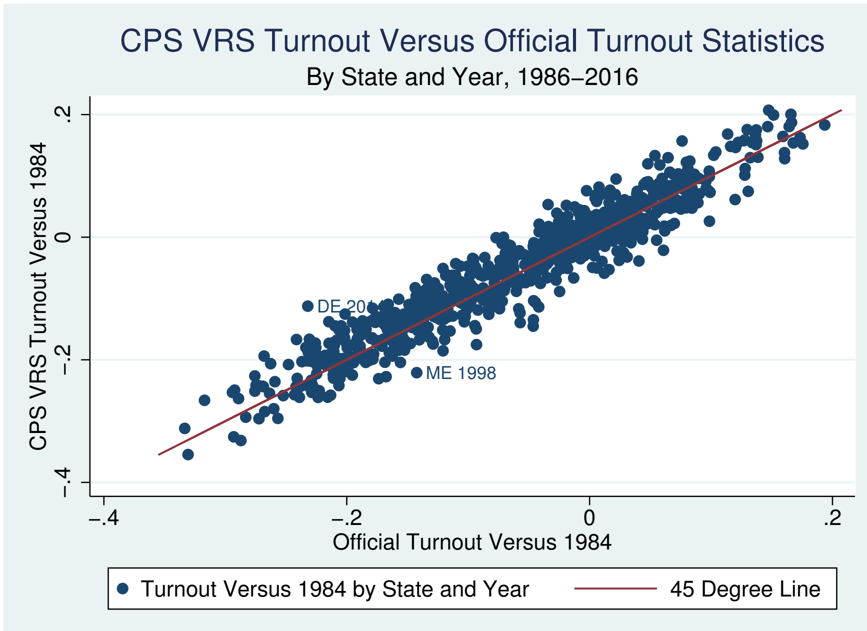
[16]See $https://cps.ipums.org/cps/voter_sample_notes.shtml$; $http://www.electproject.org/home/voter-turnout/faq/numerator$

[17]One way to address issues related to "social desirability bias" is to use only reports of voter turnout given by "proxy," that is, to only use information on turnout that is reported by another individual in the household. In theory, information reported by proxy may be less susceptible to "social desirability bias" and reduce the overestimate of voting rates in the VRS (DeBell et al., 2018). The results in this paper are robust to including only individuals with voting information reported by proxy.

relative to a base year (i.e. using fixed effects for each demographic group in each location in all specifications). Thus, Figure 1.3 shows the rates of voter turnout relative to a base year (1984 for presidential elections and 1986 for midterm elections) in both the VRS and official statistics. The two track relatively well with the exception of 1994.[18] This provides evidence that the VRS portrays an accurate assessment of changes in voter turnout. Figure 1.4 further examines the relationship between the change in voting rates reported by the CPS and official statistics at the state-year level. In Figure 1.4, the x-axis reports the change in the official voting rate for each state and year relative to 1984 according to official statistics. The y-axis reports the change in the voting rate relative to 1984 in the CPS VRS. The two statistics track well at the state-year level, with almost all points close to the 45-degree line. This provides evidence that the CPS voting data accurately represents changes in voting rates over time, and thus is suitable for this analysis. Furthermore, I confirm that measurement error in the CPS VRS relative to official statistics is unrelated to economic conditions. In Table A.1, I perform a variety of OLS regressions in which the dependent variable is the VRS minus the official voting rate at the state level. As shown in the table, this dependent variable is unrelated to overall or group-specific economic conditions. This again supports the claim that the CPS VRS accurately tracks changes in voter turnout, and that any measurement error in tracking trends is unrelated to state economic conditions. To further ensure that my results are not driven by over-reporting of voter turnout, I also perform results with adjustments for over-reporting of voting behavior as recommended by Hur and Achen (2013) (see Section 1.8).

[18] The results of this paper are qualitatively similar if 1994 data is excluded. Results excluding 1994 are available upon request.

Figure 1.4: VRS Versus Official Ballot Counts: Estimate of Voting Rates Versus Base Year by State and Year



Source: Author calculations from current Population Survey Voting and Registration Supplement downloaded from Flood et al. (2015) and United States Election Project http://www.electproject.org/home/voter-turnout/voter-turnout-data.

1.3.2 Sample Selection and Definition of Variables

Using data from the VRS linked to basic CPS information on demographic characteristics, I am able to classify individuals in each state into 8 different demographic groups, based on 2 gender (male or female), 2 race (black or non-Hispanic white), and 2 education (high school education or less, or any college) groupings.[19] I exclude those who are under age 25 or over age 54 and those who are any race other than black or non-Hispanic white. I impose the age restriction to target the population most likely to be at work. I restrict my analysis to non-Hispanic whites and blacks, because other racial groups are too small to allow for comparisons across states over the entire period from 1984-2016.[20] I exclude those who are not eligible to vote such as non-citizens.[21] I also exclude state-demographic groups that

[19] I obtain similar results if I also split demographic groups by age. These results are available upon request.

[20] The Hispanic and Asian populations also disproportionately reflect non-citizen populations, who are not able to vote. For my sample period, about half of the Hispanic adults are non-citizens and are therefore ineligible to vote. Results including Hispanics and Asians are available upon request.

[21] The results are robust to the exclusion of all foreign born citizens.

are particularly small (such as black women with any college in Idaho), because their labor market conditions cannot be estimated precisely. Specifically, I exclude state-demographic groups with under 500 observations in the 1980 Census (corresponding to roughly 10,000 people in the demographic group-state population in 1980). This is below the 5th percentile of my sample.

To obtain information on labor market conditions facing members of different demographic groups in each state, I use information on the employment-population ratio and the log of average wage by group. Data on overall employment-population ratios and wages by demographic group within each state are provided by the Current Population Survey March supplements, downloaded from IPUMS (Flood et al., 2015). I create a weighted average of the March supplements, putting 1/3 weight on the March statistics in the year of the election (8 months before the election) and 2/3 weight on the March statistics the year following the election (4 months after the election) to get an approximate measure of economic conditions around election day.[22] Since the CPS samples are relatively small, the employment-population ratios and wages are reported with error.[24]

To construct an index of labor demand (described below), I rely on two data sources. The 1980 Census 5 percent sample provides detailed information on the industry and occupation composition of demographic groups within states (Ruggles, Genadek, Goeken, Grover, and Sobek, 2015), which allows me to construct baseline shares for the labor demand indexes. The monthly CPS provides information on employment by industry and occupation for each year since 1983, allowing me to calculate predicted demand indexes based on national

[22]Following Autor, Katz, and Kearney (2008), I multiply top-coded earnings by 1.5. I calculate hourly wage as total earnings last year divided by weeks worked last year times hours work per week last year.[23] I set a maximum wage equal to maximum income in the prior year (1.5 times the top-coded value) divided by 1400 hours. All earnings are deflated by the CPI.

[24]I do not use the basic monthly CPS to estimate employment rates, because the individuals in the VRS are also those in the basic monthly supplements during January, February, August, September, October, November, and December. It is, however, still likely that some individuals in the November CPS are part of the oversample used to construct the March ASEC data. Any endogeneity from the same individuals being included in the measures of the employment-population ratio and voting behavior provides further justification for the use of the labor demand index as outlined below.

Table 1.1: Summary Statistics

	All		Males		Females	
	mean	sd	mean	sd	mean	sd
Age	39.38	8.47	39.45	8.47	39.33	8.47
Non-Hispanic Black	0.09	0.29	0.08	0.27	0.11	0.31
Any College	0.57	0.49	0.57	0.50	0.58	0.49
Married	0.69	0.46	0.69	0.46	0.70	0.46
Any Child in HH	0.59	0.49	0.54	0.50	0.63	0.48
Number of Children in HH	1.16	1.23	1.08	1.23	1.23	1.23
Employed	0.81	0.39	0.89	0.32	0.75	0.44
Employed 35+ Hours	0.65	0.48	0.80	0.40	0.52	0.50
Weekly Hours	30.34	19.79	36.67	18.26	25.05	19.46
Voting Rate (0-100)	61.44	48.67	59.80	49.03	62.95	48.29
Observations	703841		336900		366941	

Source: Author calculations from current Population Survey Voting and Registration Supplement downloaded from Flood et al. (2015).

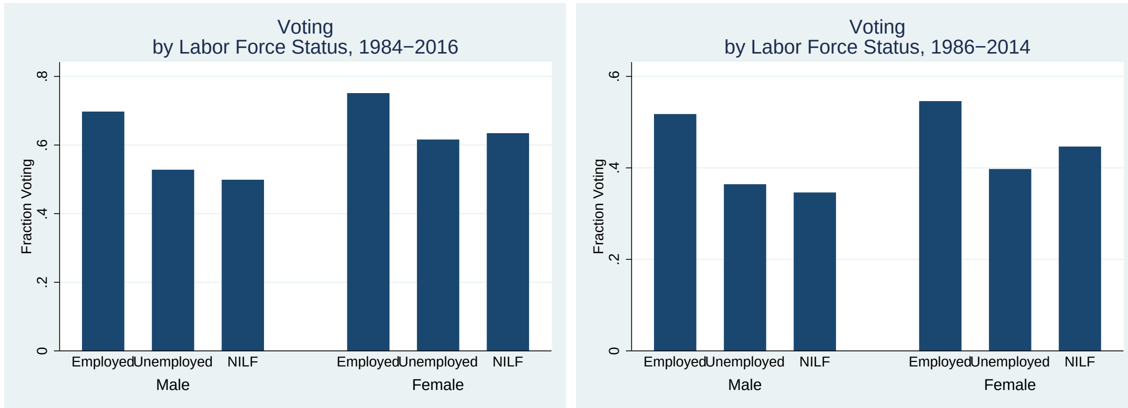
employment growth by industry and occupation. I use the October - November national industry growth measures to estimate shocks to each industry and occupation around the time of the election.[25]

My final sample includes 703,841 observations, or roughly 41,000 observations per year, spread across 301 demographic-state groups over 17 elections (1984-2016).[26] Summary statistics for the final sample are shown in Table 1.1. The average age for both men and women in my sample is about 39 years. About 9 percent of the sample is non-Hispanic black, and the remainder is non-Hispanic white, and 57 percent of the sample has any college education. About 89 percent of men and 75 percent of women work outside the home. An average of 61.4 percent of the sample reported voting in the November elections.

[25]The results are robust to using a larger span of months to estimate labor market conditions. A major occupational re-coding in the 1980 census was not implemented until the 1983 CPS, making the data not comparable prior to 1983.

[26]There are 8 demographic groups in 50 states, which would give 400 demographic-state groups per year. Of these, 99 are omitted because the sample sizes are too small to give accurate estimates of the initial industry-occupation composition and the group employment-population ratio.

Figure 1.5: Association between Voting and Labor Force Status, Prime Age Individuals



(a) Presidential Elections (b) Midterm Elections

Source: Author calculations from current Population Survey Voting and Registration Supplement downloaded from Flood et al. (2015).

1.4 Descriptive Evidence and Identification Strategy

In this section, I present basic stylized facts on the association between employment and voting from 1990-2016. I first compare the voting rates of individuals by employment status. Figure 1.5 displays voting rates for prime-age men and women in presidential (panel a) and midterm (panel b) elections from 1984-2016. As shown in both panels (a) and (b), voter turnout increases with employment for both men and women. The magnitude of the increase is larger for men. The figure also shows that women vote at higher rates than men overall, which is consistent with standard findings on voter turnout (Smets and Van Ham, 2013).

To more thoroughly assess the association between employment opportunities and voting, I next run simple linear probability models. I pool all observations for individuals 25-54 years old, and run a regression in which the dependent variable is equal to 1 if the individual voted (and 0 otherwise) and the key independent variable is equal to 1 if the individual is employed (and 0 otherwise). Unfortunately, the November supplement of the CPS does not have information on wages, so I cannot include this in the individual-level regressions. Specifically, I use the following model:

$$y_{i,g,s,t} = \alpha + \beta_1 employed_{i,g,s,t} + \sum_{k=1}^{K} \phi^k x^k_{i,g,s,t} + \theta_{g,s} + \lambda_{g,t} + \eta_{s,t} + \epsilon_{i,g,s,t} \tag{1.1}$$

where $y_{i,g,s,t}$ is equal to 1 if individual i in demographic group g in state s at time t voted, and 0 otherwise. $employed_{i,g,s,t}$ is equal to 1 if the individual reports being employed in the November CPS, and 0 otherwise, and $x^k_{i,g,s,t}$ represents k individual characteristics including age dummies, whether voting behavior is reported by self or proxy, an indicator for whether the individual has at least a high school education, and an indicator for whether an individual has at least a bachelor's degree. There are three sets of fixed effects. State x demographic group fixed effects, $\theta_{g,s}$, control for baseline differences in voting for a particular demographic group in a particular state (e.g., that white women with any college in Massachusetts were more likely to vote over the entire time period than another state-demographic group). Demographic group x year fixed effects, $\lambda_{g,t}$ control for election-specific factors that may drive higher turnout by certain demographic groups in particular years (for example, higher African American turnout in 2008 due to the candidacy of Barack Obama). State x year fixed effects, $\eta_{s,t}$ control for a variety of state-year specific factors that may influence the level of participation in elections, such as concurrent elections for senate or governor or the competitiveness of the presidential race in a given state. Finally, $\epsilon_{i,g,s,t}$ is an individual error term.

The results of estimating equation (1.1) are shown in Table 1.2. Standard errors are clustered at the state level and regressions are weighted using CPS basic monthly weights. In all columns, I include 30 separate age dummies and controls for the individual's education level (high school graduate and college graduate) since turnout has been shown to vary with age and education level [e.g., Glaeser et al. (2002); Wattenberg (2015)]. I also control for whether voting behavior is reported by an individual herself or by a proxy.[27] In column (1), I include state x demographic group fixed effects, $\theta_{g,s}$, and demographic group x year

[27]If information on who reported voting behavior is not available, I include a separate dummy for "self reporting missing."

fixed effects, $\lambda_{g,t}$. To control for state economic conditions, I use two variables: the state unemployment rate in the election year from the Bureau of Labor Statistics [column (1)], or the state prime-age employment-population ratio calculated from the CPS [column (2)]. Both columns also include controls for a variety of state-level demographics, including the percent ages 25-54, percent ages 55-64, percent over 64, percent non-Hispanic white, percent non-Hispanic black, percent Hispanic, percent with high school degrees, percent with some college, and percent with college degrees. I also add controls for state-specific factors shown to influence turnout, including the presence of concurrent governor or senate elections, the state log average wage, and the state unemployment rate (Burden et al., 2014). The coefficient on "employed" in columns (1)-(2) is 6.7, reflecting the fact that a person who is employed is about 7 percentage points more likely to vote than a person who is not employed. At the same time, good overall labor market conditions reduce turnout. As shown in column (1), high unemployment is associated with higher turnout. The magnitude of the effect of unemployment rates is consistent with the estimates of Cebula (2017) and others, suggesting that a 10 percentage point increase in the state unemployment rate increases an individual's turnout by about 9 percentage points. In column (2), I replace the state unemployment rate with the state prime-age employment-population ratio and find directionally similar effects: a 10 percentage point increase in the state prime-age employment-population ratio decreases turnout by 2 percentage points. In column (3), I replace the state-year unemployment rate/employment-population ratio, with a full set of state x year fixed effects, $\eta_{s,t}$, to more flexibly control for state-specific factors that may affect turnout. The association between individual employment and turnout is unchanged. Overall, the results in Table 1.2 show that, for prime-age individuals as a whole, employment is associated with voting rates that are roughly 7 percentage points higher, regardless of which fixed effects are included. At the same time, good state labor market conditions depress turnout.

Table 1.2: Voting and Individual Employment

	(1)	(2)	(3)
Employed	6.718***	6.688***	6.703***
	(0.302)	(0.299)	(0.307)
Concurrent Governor Election	1.740***	1.757***	
	(0.560)	(0.556)	
Concurrent Senate Election	1.534***	1.571***	
	(0.242)	(0.240)	
State Unemployment Rate	91.22***		
	(13.18)		
State Prime Age E-P Ratio		-19.81**	
		(9.581)	
Age FE	Yes	Yes	Yes
Group x Year FE	Yes	Yes	Yes
Group x State FE	Yes	Yes	Yes
State x Year FE	No	No	Yes
Observations	703841	703841	703841
R^2	0.175	0.174	0.180
Adjusted R^2	0.174	0.174	0.179

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (1.1). The dependent variable equals 1 if the individual voted and 0 otherwise. Employment is at the individual level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for whether voting behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree. Columns (1) and (2) also include state demographic controls (percent ages 25-54, ages 55-64, and 65 and over; percent white, black, and Hispanic; and percent with high school degrees, some college, and four-year college degrees) and log average state wage. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

Although Table 1.2 controls for an individual's observed characteristics, it likely does not present an unbiased estimate of the effect of employment opportunities on voting. In particular, there are likely to also be a number of unobserved characteristics that may make individuals more likely to work and more likely to vote, such as differences in intelligence, motivation, extroversion, or other factors. These may bias the impact of individual employment on voting up. At the same time, individuals who are particularly committed to their communities may choose to be stay-at-home parents rather than working, which may bias the estimate down. Therefore, I proxy for an individual's employment opportunities with the labor market conditions that the individual's demographic group faces. This strategy leverages the fact that members of similar demographic groups (defined by education, race, and gender) often work in particular industries/occupations and face somewhat similar labor demand conditions (DeBoer and Seeborg, 1984). To proxy for labor market conditions, I rely on two measures: group employment-population ratio and log average group wages. I run the following models:

$$y_{i,g,s,t} = \alpha + \beta_1 EPOP_{g,s,t} + \sum_{k=1}^{K} \phi^k x^k_{i,g,s,t} + \theta_{g,s} + \eta_{s,t} + \lambda_{g,t} + \epsilon_{i,g,s,t} \tag{1.2}$$

$$y_{i,g,s,t} = \alpha + \beta_1 LNWAGE_{g,s,t} + \sum_{k=1}^{K} \phi^k x^k_{i,g,s,t} + \theta_{g,s} + \eta_{s,t} + \lambda_{g,t} + \epsilon_{i,g,s,t} \tag{1.3}$$

where $EPOP_{g,s,t}$ is the employment-population ratio for the demographic group g in state s at year t, and $LNWAGE_{g,s,t}$ is the log average wage for the demographic group g in state s at year t, and all other variables are as defined above.

Table 1.3, panel (a) shows the results of specification (1.2). I introduce controls in the same order as in Table 1.2. Columns (1)-(2) provide strong evidence for the different effect of own labor market conditions versus statewide labor market conditions. Low state unemployment/high state employment-population ratios depress turnout, but high group-specific employment-population ratios increase turnout. Columns (1)-(2) indicate that a 10 percent increase in the group employment-population ratio increases turnout by 0.4 percentage points. In column (3), I include the full set of state x year fixed effects $\eta_{s,t}$. With this more flexible control for state-specific factors affecting turnout, the results suggest that a 10 percent increase in the group employment-population ratio increases turnout by 0.5 percentage points. This result is significant at the 1 percent level.

Table 1.3, panel (b) shows the results of model (1.3). Regardless of the specification, there is no strong evidence of a relationship between log average wage and voting behavior. This suggests that employment, rather than wages, is the primary channel through which labor demand affects voter turnout.

Table 1.3: Voting and Employment Opportunities

(a) Employment-Population Ratio

	(1)	(2)	(3)
Group E-P Ratio	4.195	3.957*	4.990***
	(2.883)	(1.990)	(1.829)
Concurrent Governor Election	1.738***	1.734***	
	(0.541)	(0.538)	
Concurrent Senate Election	1.564***	1.604***	
	(0.247)	(0.244)	
State Unemployment Rate	93.387***		
	(15.016)		
State Prime Age E-P Ratio		-22.006**	
		(9.328)	
Age FE	Yes	Yes	Yes
Group x State FE	Yes	Yes	Yes
Group x Year FE	Yes	Yes	Yes
State x Year FE	No	No	Yes
Observations	703841	703841	703841
R^2	0.172	0.172	0.178
Adjusted R^2	0.171	0.171	0.176

(b) Wage

	(1)	(2)	(3)
Log Group Wage	-0.191	-0.529	-0.891
	(1.671)	(1.721)	(1.498)
Concurrent Governor Election	1.753***	1.745***	
	(0.545)	(0.541)	
Concurrent Senate Election	1.576***	1.607***	
	(0.248)	(0.245)	
State Unemployment Rate	89.682***		
	(14.647)		
State Prime Age E-P Ratio		-18.227*	
		(9.524)	
Group x State FE	Yes	Yes	Yes
Group x Year FE	Yes	Yes	Yes
State x Year FE	No	No	Yes
Observations	703835	703835	703835
R^2	0.172	0.172	0.178
Adjusted R^2	0.171	0.171	0.176

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (1.2) in panel (a) and equation (1.3) in panel (b). The dependent variable equals 1 if the individual voted and 0 otherwise. The employment-population ratio and log average wage are defined at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for whether voting behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree. Columns (1) and (2) also include state demographic controls (percent ages 25-54, ages 55-64, and 65 and over; percent white, black, and Hispanic; and percent with high school degrees, some college, and four-year college degrees) and log average state wage. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

Although using group employment-population ratios and wages is less endogenous than using individual measures of employment, there are still a number of concerns with these estimates. As noted above, the employment-population ratio and wages are subject to measurement error, since they rely on relatively small samples. This may cause attenuation bias, which may reduce the magnitude of the estimates. The results may also suffer from various forms of endogeneity. For example, upward bias may occur if a state's introduction of job training programs leads to higher employment and higher voter turnout. At the same time, downward bias may occur if factors such as government transfers decrease employment and increase voting rates.[28] There is even potential for reverse causality. Since I use a combination of pre-election and post-election surveys to measure economic conditions, a certain group may benefit economically if they turn out in high numbers and elect politicians amenable to their industries or occupations. To examine the causal effect of employment, a driver of employment is needed that is exogenous to both individual characteristics and state government policies. Thus, in the next section, I turn to a Bartik strategy to obtain an exogenous measure of labor market conditions.

1.5 Estimation Strategy

The results in Section 1.4 provide suggestive evidence of a positive relationship between employment and voting. To obtain an exogenous driver of the group's employment opportunities, I develop an index of labor demand along the lines used by Bartik (1991), Blanchard and Katz (1992), Katz and Murphy (1992), Blau et al. (2000), Aizer (2010), and others. The literature has noted that members of particular demographic groups are often clustered in certain industries and occupations within a state (DeBoer and Seeborg, 1984), and members of different groups are not perfectly substitutable across tasks. Thus, the index predicts

[28] Various studies have highlighted that conditional cash transfers from the government increase turnout (De La O, 2013), although non-governmental income does not have the same effect (Akee et al., 2018).

labor demand for a particular group in a given state and year based on the group's initial industry-occupation composition within the state and national growth in employment by industry and occupation. The specific index of labor demand is:

$$LD_{g,s,t} = \sum_j \gamma_{sjg,1980} g_{jte,-s} \tag{1.4}$$

where $\gamma_{sjg,1980}$ represents the share of employment for group g in state s in industry-occupation j in 1980, and $g_{jte,-s}$ represents the national growth rate of workers of education type e (LEHS, any college) in industry-occupation j (excluding state s). I use 18 industry groupings.[29] I cross these with 11 occupational groupings.[30] The labor demand index is standardized to have a mean of 0 and standard deviation of 1 in each year. I also construct a statewide labor demand index, which is equal to the weighted average of the labor demand indexes for all prime age individuals in the state, where the weights are the count of employed persons in the 1980 Census 5 percent files in each group and state.

To gain insight into the intuition behind this index, one can think of two demographic groups in the sample: white men with less than or equal to a high school education in Indiana, and white men with less than or equal to a high school education in Wyoming.

[29]The industry groupings are: (1) Agriculture, Forestry, and Fishing; (2) Mining (ex Oil and Gas); (3) Construction; (4) Low Tech Manufacturing (lumber furniture, stone, clay, glass, food, textiles, apparel, and leather); (5) Basic Manufacturing [primary metals, fabricated metals, machinery, electrical equipment, automobile, other transport equipment (excluding aircraft), tobacco, paper, printing, rubber, and miscellaneous manufacturing]; (6) High Tech Manufacturing (aircraft, instruments, chemicals, petroleum); (7) Transportation; (8) Communications; (9) Utilities; (10) Wholesale Trade; (11) Retail Trade; (12) Finance, Insurance, and Real Estate; (13) Business Services; (14) Personal Services; (15) Entertainment; (16) Professional Services; (17) Public Administration; and (18) Oil Extraction. The grouping of industries is from Katz and Murphy (1992). I make one adjustment to the Katz and Murphy (1992) industry categories: I break out oil and gas extraction from other mining. This is due to the divergent paths taken by oil and gas industries and other forms of mining from 1980-2016.

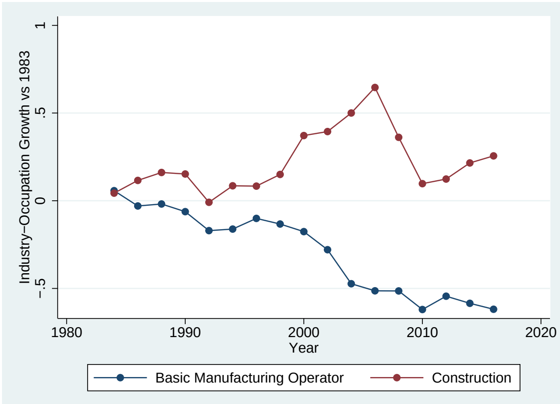
[30]The occupational groupings are: (1) executive, administrative and managerial occs; (2) management related occs; (3) professional specialty occs; (4) technicians and related support occs; (5) financial sales and related occs; (6) retail sales occs; (7) administrative support occs; (8) fire fighting, police, and correctional institutions occs; (9) machine operators, assemblers and inspectors; (10) transportation/construction/mechanics/mining/agricultural occs; and (11) low-skill service occupations. These occupational groupings are based on those in Autor and Dorn (2013), but collapses occupation groups (10), (11), and (12) due to small sample sizes.

The Indiana group is heavily concentrated in the industry of basic manufacturing and the occupation of operator, an industry-occupation which declined in total employment over the period 1984-2016 and especially from 2000-2010. In contrast, the Wyoming group is highly concentrated in the construction occupation-industry, which did not experience major declines over the period 1984-2016. In Figure 1.6, I show the national trends in employment growth for basic-manufacturing operators and construction workers over the period. As indicated before, construction work continued with strong demand into the 2000s, while the number of basic manufacturing operators declined. Therefore, the labor demand index should predict that employment should decrease for white men with less than or equal to a high school education in Indiana, and should stay constant or rise for white men with less than or equal to a high school education in Wyoming.

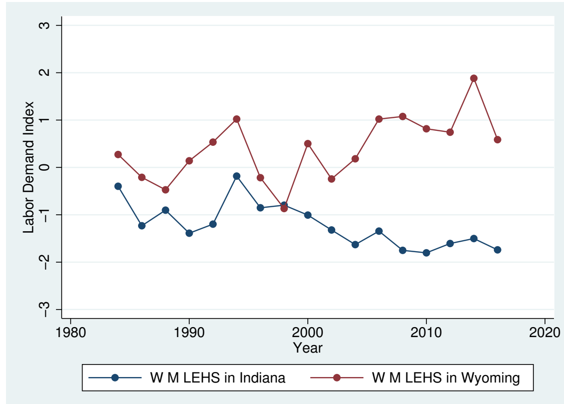
Figure 1.6, panel (b) shows the value of the labor demand index for both groups of white men with LEHS over the period. To get the full measure of the labor demand index, I calculate a weighted sum of growth rates for all the industries and occupations that each demographic-state group was in 1980. Since white men with LEHS in Indiana were heavily concentrated in basic manufacturing-operators, their labor demand index is heavily influenced by this industry-occupation national trend. Similarly, the labor demand index for white men with LEHS in Wyoming is heavily influenced by national trends in construction. In Figure 1.6, panel (c), I show the actual change in the employment-population ratios for white men with LEHS in Indiana and Wyoming. As predicted by the labor demand index, the Indiana and Wyoming groups are somewhat different from 1984-1998, but the Indiana group falls far below the Wyoming group beginning in 2000, consistent with predictions of the Bartik index.

To demonstrate the predictive power (relevance) of the Bartik index more generally, I present a set of regressions that show the association between the group's labor demand index and various employment outcomes. Results are shown in Table 1.4 with all two-way fixed

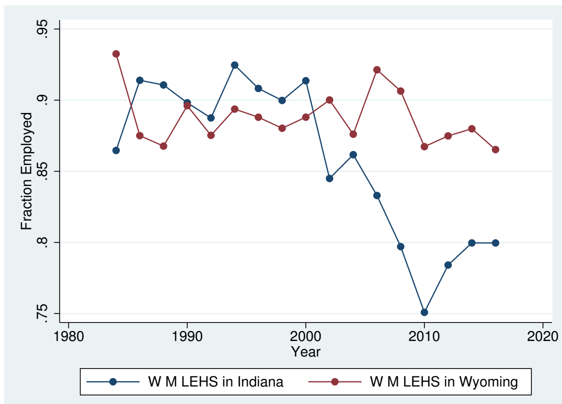
Figure 1.6: Labor Demand Index and Employment-Population Ratio: White Men with LEHS



(a) National Industry-Occupation Growth



(b) Labor Demand Index



(c) Employment-Population Ratio

Source: Author calculations as described in text. Data from Current Population Survey Voting and Registration Supplement and Current Population Survey Basic Monthly Samples downloaded from Flood et al. (2015).

effects (group x state, state x year, and group x year). As shown, a one standard deviation increase in the labor demand index is associated with a 1.2 percentage point increase in the likelihood that an individual is employed, a 1.7 percentage point increase in the likelihood that the individual is employed full time (at least 35 hours per week), a 0.8 percentage point increase the group employment-population ratio, and a 1.2 percent increase in the average group wage. Table A.4 shows the relationship between the state labor demand index and the state unemployment rate and prime-age employment-population ratio.

To further support the identification strategy, I examine the exogeneity of the labor demand index. The identifying assumption for my estimation strategy is that labor demand

Table 1.4: Predictive Power of Labor Demand Index

	(1) Is Employed	(2) Is Employed Full Time	(3) Group E-P	(4) Log Group Wage
Labor Demand Index	1.175***	1.730***	0.830**	1.224**
	(0.380)	(0.328)	(0.331)	(0.478)
Group x State FE	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes
Group x Year FE	Yes	Yes	Yes	Yes
Observations	784961	542298	5117	5116
R^2	0.092	0.242	0.925	0.970
Adjusted R^2	0.091	0.240	0.901	0.961

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (1.5) with the dependent variables listed in the column headings. The labor demand index is at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for whether voting behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree (or, in columns (3)-(4) the group average of these values). Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

index is exogenous to other determinants of civic participation after controlling for group-state, state-year, and group-year fixed effects (that is, the labor demand index only affects voting through its effects on employment). More formally, the assumption can be stated as: $E[(\sum_j \gamma_{sjg,1980} g_{jte,-s})\epsilon_{i,g,s,t}|\theta_{g,s},\eta_{s,t},\lambda_{g,t}] = 0$. Since the labor demand index relies on growth in national industry-occupation cells, this assumption requires that, in a particular year, demographic-state groups clustered in fast-growing industries do not also receive other exogenous shocks to their political participation.[31]

There are no complete tests of this exogeneity assumption. However, to test the assumption,

[31] A weaker assumption would be that places with high and low values of the Bartik index are otherwise similar in characteristics. As noted by Goldsmith-Pinkham, Sorkin, and Swift (2018), under this assumption the Bartik index can be thought of as a GMM estimator with the industry-occupation shares interacted with each year as the instrument and the growth rates as weights. I test the validity of the assumption that places with high Bartik and low Bartik values are similar in Table A.2. To do so, I regress the value of the Bartik index on characteristics of the group in 1984 and state-year and group-year fixed effects. As shown, groups with high Bartik values are not statistically different from other groups after controlling for group x year, state x year, and age fixed effects. This provides evidence for my identification strategy under weaker assumptions. It also confirms the importance of testing the robustness of the results for various controls for unionization. I also test for balance in the industry-occupation groups driving the greatest variation in the Bartik index, using Rotenberg weights as in Goldsmith-Pinkham et al. (2018). To deal with the potential for unobserved differences, I always include group-state fixed effects to control for initial differences.

I use a common falsification test.[32] The test involves regressing this period's voting rate on the next period's Bartik index. If the coefficient on next-period's labor demand index is insignificant, the regression suggests that the voting behavior of demographic-state groups receiving large shocks to the labor demand index were trending similarly to other groups before the shocks. This provides support to the assumption that shocks are exogenous to trends in voter turnout. I perform this test in Table A.3. As shown, the coefficients on next period's labor demand index are positive but not significant. I also examine the coefficients on the labor demand index two periods (i.e. four years) in the future, that is, on the labor demand index during the next election of the same type. As shown, the labor index four years in the future is also unrelated to voting behavior in the current period. Therefore, this provides support for the identifying assumption that the labor demand index only affects voting through its effects on employment, since future shocks to employment are not associated with any changes in voting behavior in the current period.

1.6 Main Results

In this Section, I present the main results of the paper using the labor demand index to proxy for employment.

1.6.1 Baseline Results

I begin by examining the impact of the employment demand index on voting. In Table 1.5, I run linear probability models, in which the key independent variable is the labor demand index, and the dependent variable is equal to 1 if the individual voted in the November (presidential or midterm) election and 0 if she was eligible to vote but did not vote.

[32] See, e.g., Autor and Dorn (2013); Shenhav (2017).

$$y_{i,g,s,t} = \alpha + \beta LD_{g,s,t} + \sum_{k=1}^{K} \phi^k x^k_{i,g,s,t} + \theta_{g,s} + \eta_{s,t} + \lambda_{g,t} + \epsilon_{i,g,s,t} \tag{1.5}$$

I present the results in Table 1.5 with standard errors clustered at the state level. All coefficients are multiplied by 100; thus, each coefficient reflects the effect of a one unit change in the independent variable on the percentage point change in voting probability. Regressions are weighted using CPS basic monthly weights. The main variable is the group labor demand index. In all columns, I include group x state fixed effects (to account for original differences in voting across groups), group x year fixed effects, and controls for individual characteristics (age, educational attainment, and reporting of voting behavior by self or proxy). In column (1), I proxy for state economic conditions with a state labor demand index, which is a weighted average of the labor demand indexes for all individuals ages 25-54 in a given state.[33,34] As shown in column (1), individuals are *more* like to vote if their own group faces strong labor demand, but *less* likely to vote if the state as a whole faces strong labor demand. In column (2), I employ the full set of state x year fixed effects. The results gain significance, and indicate that a one standard deviation increase in the labor demand index increases voter turnout by 0.8 percentage points.

[33] Weights are equal to the number of people in this group in the 1980 Census PUMS files. The statewide index includes Hispanics, Asians, and other racial/ethnic groups.

[34] Column (1) also includes controls for a variety of state-level demographics, including the percent ages 25-54, percent ages 55-64, percent over 64, percent non-Hispanic white, percent non-Hispanic black, percent Hispanic, percent with high school degrees, percent with some college, and percent with college degrees.

Table 1.5: Main Results: Voting and the Labor Demand Index

	(1)	(2)
Labor Demand Index	1.057*	0.837**
	(0.563)	(0.373)
State Labor Demand Index	-3.225***	
	(0.811)	
Concurrent Governor Election	1.726***	
	(0.551)	
Concurrent Senate Election	1.548***	
	(0.233)	
Age FE	Yes	Yes
Group x State FE	Yes	Yes
Group x Year FE	Yes	Yes
State x Year FE	No	Yes
Observations	703841	703841
R^2	0.172	0.178
Adjusted R^2	0.171	0.176

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (1.5). The dependent variable equals 1 if the individual voted and 0 otherwise. The labor demand index is at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for whether voting behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree. Columns (1) and (2) also include state demographic controls (percent ages 25-54, ages 55-64, and 65 and over; percent white, black, and Hispanic; and percent with high school degrees, some college, and four-year college degrees) and log average state wage. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

I next show the validity of the results in Table 1.5 to a variety of additional controls. In Table 1.6, I re-run the results from Table 1.5, column (2), with a variety of additional controls. I include these controls one at a time. I first consider the possibility that groups with high values of the Bartik index are trending differently over time in terms of their voting behavior than groups with low values of the Bartik index. To account for this, in column (1), I include group x state linear time trends. The magnitude of the coefficient is reduced slightly, but maintains significance in the overall regression. I next consider whether different state-demographic groups have differential voting rates in midterm versus presidential elections. In column (2), I add group x state x election type controls, to account for potential differences in midterm voting versus presidential voting rates at the state-group level. The results remain significant. Finally, I examine the robustness of my results to unionization controls. Unions have historically been a source of political mobilization (Feigenbaum et al., 2018), and private-sector unions have experienced declines in power and size in recent years, and have notably declined in many of the states that have also experienced overall declines in employment. Thus, it is possible that the decline in unions, rather than the decline in employment, has contributed to decreased political participation. To ensure that declines in unionization are not driving my results, in column (3), I add a control for the group unionization rate in each year, to account for differential trends in unionization. In column (4), I interact the group unionization rate with dummy variables for the year, to allow a different impact of unionization by year. The results are qualitatively similar and maintain significance in all specifications. Since the results are not substantially different when including these additional controls, I revert to the original specification for the future regressions.

The results in Tables 1.5 and Table 1.6 indicate that a one standard deviation increase in labor demand increases turnout by 0.8 percentage points. To assess the economic significance of this measure, we can consider that the labor demand index increases the probability that an individual is employed full time by 1.7 percentage points and the probability that an individual

Table 1.6: Main Results with Additional Controls: Voting and the Labor Demand Index

	(1) Add Group x State Linear TT	(2) Add Election Type x State x Group FE	(3) Add Unionization Rate Control	(4) Add Unionization Rate x Year Control
Labor Demand Index	0.740*	0.752*	0.825**	0.827**
	(0.423)	(0.393)	(0.374)	(0.375)
Unionization Rate			1.646***	
			(0.155)	
Age FE	Yes	Yes	Yes	Yes
Group x State FE	Yes	Yes	Yes	Yes
Group x Year FE	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes
Group x State Linear TT	Yes	No	No	No
Group x State x Election Type FE	No	Yes	No	No
Unionization Rate x Year Control	No	No	No	Yes
Observations	703841	703841	703841	703841
R^2	0.178	0.178	0.178	0.178
Adjusted R^2	0.177	0.176	0.176	0.176

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (1.5) with additional controls as listed in the column headings. The dependent variable equals 1 if the individual voted and 0 otherwise. The labor demand index is at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for whether voting behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

is employed by 1.2 percentage points. If it is assumed that full time/any employment is the primary pathway through which labor demand impacts voting, this suggests an imputed instrumental variable estimate of the effect of full time employment on voting of about 0.4-0.5 (or of any employment on voting from 0.6-0.7). That is, being employed full time appears to increase the likelihood of voting by 40-50 percentage points. This is somewhat larger than the imputed IV obtained by Corman et al. (2017) for those affected by welfare reform in the 1990s, which ranged from 19-36 percentage points. This may reflect differences in the effect of employment induced by the labor demand index versus welfare reform. Also note that the estimate of 45-70 is an upper bound; there may be other pathways through which labor demand increases voting (such as wages) that also have a positive causal impact on voting. However, even the lower end of this estimate is economically very significant.

1.6.2 Heterogeneity

I next examine heterogeneity in these main results. To examine heterogeneity along the dimensions of gender, race, educational attainment, and age, I interact various dummies with the labor demand index. Table 1.7 shows the results of the main regressions with the addition of these interaction terms. The p-values of tests for equality between the two main coefficients are shown at the base of the table. As shown in the Table, there is not evidence of a strong difference in the effect of the labor demand index on voting by gender, age, or education level. However, the evidence does indicate that whites may be more strongly affected by changes to labor demand than blacks.

I next examine heterogeneity by state ideology and type of election. In Table 1.7, columns (5) and (6), I interact the main variable with measures of election type and state ideology.[35] In column (5), I cannot reject equality of the coefficients for midterm and presidential elections. In column (6), I cannot reject equality by partisanship of states, but the evidence indicates that individuals living in Democratic states may be more likely than others to be affected by employment. This may consistent with a literature suggesting that external characteristics (such as own employment) are more mobilizing when turnout would otherwise be low. If elections are less competitive in non-swing states, mobilization may be more greatly affected by employment (Fraga, Hersh, et al., 2011).

[35] I use information from Gallup on political partisanship by state in 2016 to separate out Democratic, Republican, and Swing States. I create a "democratic index" that equals the percent of people identifying as Democrats minus the percent identifying as Republicans. If the "democratic index" is greater than 5 percent, the state is classified as Democratic. If the index is between -1 and 5 percent, the state is classified as a Swing State. If the index is below -1 percent, the state is classified as Republican. The Republican states are: Alabama, Alaska, Arizona, Idaho, Indiana, Kansas, Mississippi, Montana, Nebraska, North Dakota, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Utah, and Wyoming. The swing states are: Arkansas, Colorado, Florida, Georgia, Iowa, Kentucky, Louisiana, Maine, Missouri, Nevada, New Hampshire, North Carolina, Ohio, Virginia, and Wisconsin. The democratic states are: California, Connecticut, Delaware, Hawaii, Illinois, Maryland, Massachusetts, Michigan, Minnesota, New Jersey, New Mexico, New York, Oregon, Pennsylvania, Rhode Island, Vermont, Washington, and West Virginia.

Table 1.7: Heterogeneous Responses to Labor Demand

	(1)	(2)	(3)	(4)	(5)	(6)
Labor Demand Index * Male	0.591*					
	(0.316)					
Labor Demand Index * Female	1.283**					
	(0.585)					
Labor Demand Index * White		1.271***				
		(0.379)				
Labor Demand Index * Black		-1.021				
		(0.772)				
Labor Demand Index * LEHS			0.425			
			(0.503)			
Labor Demand Index * Any College			1.264**			
			(0.474)			
Labor Demand Index * Age 25-39				0.603		
				(0.480)		
Labor Demand Index * Age 40-54				1.035**		
				(0.400)		
Labor Demand Index * Presidential Election					1.079***	
					(0.412)	
Labor Demand Index * Midterm Election					0.363	
					(0.508)	
Labor Demand Index * Democratic State						1.396**
						(0.590)
Labor Demand Index * Swing State						0.389
						(0.666)
Labor Demand Index * Republican State						0.265
						(0.691)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes
Group x State FE	Yes	Yes	Yes	Yes	Yes	Yes
Group x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	703841	703841	703841	703841	703841	703841
R^2	0.178	0.178	0.178	0.180	0.178	0.178
Adjusted R^2	0.176	0.176	0.176	0.178	0.176	0.176
Equality P Value	0.150	0.007	0.206	0.381	0.220	
Equality P Value Dem vs SS						0.265
Equality P Value Rep vs SS						0.898

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (1.5). The dependent variable equals 1 if the individual voted and 0 otherwise. The labor demand index is at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for whether voting behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

1.7 Examining the Mechanisms

I next explore the mechanisms through which greater employment opportunities increase turnout. I first examine the role of marriage, fertility, and duration of residence. I then explore more direct mechanisms, first using data from the CPS and then turning to more detailed questions from the American National Election Studies.

1.7.1 Family Structure and Duration of Residence

As noted in the literature review section, economic conditions have been shown to influence family structure and migration decisions. In particular, higher employment demand for males has been tied to higher marriage rates and higher fertility (Schaller, 2013; Bertrand, Kamenica, and Pan, 2015; Schaller, 2016; Shenhav, 2017), which may be complements to voter participation (Bellettini et al., 2018). In Table 1.8, I therefore perform the regressions from the last column of Table 1.5 with controls for family structure and duration of residence in the current house/apartment. As shown in the Table, consistent with the literature, marriage, more children, and longer residence at a location is associated with higher rates of voting (Glaeser et al., 2002; Bellettini et al., 2018). Thus, these may be a mechanism through which labor demand increases voting. However, the coefficients are large and significant even with controls for marriage, indicating that they may not be able to fully explain the impact of labor demand on voting behavior.

1.7.2 Direct Mechanisms

In this section, I provide an examination of the possible mechanisms (outside of marriage and duration at residence) through which employment may directly affect voter turnout. I

Table 1.8: Mechanisms: Voting and the Labor Demand Index with Controls for Family Structure and Duration of Residence

	(1)	(2)	(3)	(4)
Labor Demand Index	0.837**	0.799**	0.825**	0.793**
	(0.373)	(0.368)	(0.362)	(0.359)
Married		8.323***		7.425***
		(0.330)		(0.279)
Own Child in Household		-0.217		-0.317
		(0.318)		(0.298)
Number of own children in household		0.685***		0.457***
		(0.119)		(0.105)
At Residence at Least 1 Year			11.336***	10.636***
			(0.341)	(0.337)
At Residence at Least 5 Years			8.549***	8.207***
			(0.345)	(0.337)
Age FE	Yes	Yes	Yes	Yes
Group x State FE	Yes	Yes	Yes	Yes
Group x Year FE	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes
Observations	703841	703841	703841	703841
R^2	0.178	0.184	0.194	0.198
Adjusted R^2	0.176	0.183	0.192	0.197

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (1.5) with additional controls. The dependent variable equals 1 if the individual voted and 0 otherwise. The labor demand index is at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for whether voting behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree.Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

begin by using data from the Current Population Survey. In every year since 2004, individuals have been asked follow up questions if they reported that they did not vote in a given year. In particular, if they are registered to vote, they are asked why they did not vote, with the possible responses being: illness or disability (own or family's); out of town or away from home; forgot to vote (or send in absentee ballot); not interested, felt like my vote wouldn't matter; too busy, conflicting work or school schedule; transportation problems; didn't like candidates or campaign issues; registration problems; bad weather conditions; inconvenient polling place or hours; or other. If an individual reports that he did not register, he is asked why not, with answers being: did not meet registration deadlines; did not know where or how to register; permanent illness or disability; difficulty with English; not interested in the election; my vote would not make a difference; not eligible to vote; or other reason. From this information, I can examine how employment demand impacts individuals' reasons for not voting.

From the information on why individuals did not vote or register, I create dummy variables that can be used to examine reasons for not voting. The variable "Too Busy" is set equal to 1 if the individual says she reports one of the following as the reason for not voting: "too busy", "conflicting work or school schedule", or "inconvenient polling place or hours", and 0 otherwise. The variable "Away" is equal to 1 if the individual did not vote because he/she was away from home and 0 otherwise. The variable "Illness" is equal to 1 if the individual did not vote because of "illness or disability (own or family's)" or if the individual did not register because of "permanent illness or disability", and 0 otherwise. The variable "Forgot" is equal to 1 if the individual did not vote because she forgot. The variable "Registration Problems" is equal to 1 if the individual did not register because she"did not meet registration deadlines", "did not know where or how to register", "did not meet residency requirements/did not live here long enough", the individual was not registered in the current location, did not receive absentee ballot, or had other problems with eligibility. The variable "No Interest" is set equal to 1 if the individual did not vote because she was "Not interested, felt like my vote wouldn't make a difference", or "Didn't like candidates or campaign issues", or did not register because "Not interested in the election or not involved in politics" or "My vote would not make a difference". The variable "transportation problems" equals 1 if the individual did not vote because of problems with transportation. The variable "other" is set equal to 1 if the individual did not vote for any other reason. In Table 1.9, I examine how employment demand affects these four measured reasons for voting in all elections. As shown in the tables, labor demand decreases the response of "no interest" and "illness" reasons for not voting, and increases the response of “other”. However, the decline in “no interest” is twice as large as the increase in “other”, and thus can explain the bulk of the increase in voting with higher labor demand.

Overall, the results on individuals’ reasons for voting improves understanding of the mechanisms at work. Among the mechanisms through which employment may increase turnout, both psychological factors and recruitment may fall under the category of “interest”, and are thus supported by the results of the CPS question on why individuals did not vote. In addition, the ambiguous mechanisms of changing political opinions and political knowledge may also fall under the criteria of “interest”, and may thus be supported by the data. The results on “illness” are slightly less clear. Economic downturns have been shown to increase take up of disability programs (Autor and Dorn, 2013; O’Brien, 2013), and thus people may be more likely to report this disability as the reason for not voting. However, it is unclear whether this disability actually physically prevents them from voting or is also combined with other effects of nonemployment. However, the results from the CPS help to rule out the mechanisms of information on logistics of voting. In particular, there is little evidence that employment improves the logistical ease of voting (by helping people remember to vote or register, or enabling them to get to polling places more easily). Finally, with respect to the primary mechanism through which employment may decrease turnout (opportunity cost of time), there is little evidence of this pathway.

To distinguish between the remaining mechanisms (psychological factors, recruitment, information, and political opinions), I turn to another dataset with more detailed questions on individual’s opinions and behaviors: the American National Election Studies.

Table 1.9: Mechanisms: Reported Reasons for Not Voting, All Elections, 2004-2016

	(1) Not Vote	(2) No Interest	(3) Too Busy	(4) Reg Prob	(5) Illness	(6) Away	(7) Forgot	(8) Trans Prob	(9) Other
Labor Demand Index	-1.858*	-3.481***	-0.261	0.265	-0.814**	0.413	0.339	0.254	1.427*
	(1.024)	(0.716)	(0.461)	(0.453)	(0.387)	(0.301)	(0.234)	(0.178)	(0.771)
Age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group x State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Group x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	247655	247655	247655	247655	247655	247655	247655	247655	247655
R^2	0.188	0.079	0.040	0.021	0.018	0.012	0.017	0.009	0.038
Adjusted R^2	0.185	0.076	0.037	0.018	0.015	0.009	0.015	0.007	0.035
Dep Var Mean	38.555	13.321	5.221	4.256	1.827	1.491	0.867	0.290	8.332

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (1.5) with the dependent variables listed in the column headings. The dependent variable equals 1 if the individual voted and 0 otherwise. The labor demand index is at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for whether voting behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

1.7.3 Additional Evidence on Mechanisms

The American National Election Studies (ANES) are a set of surveys conducted from a representative sample of the United States population every 2-4 years (around Presidential and Midterm elections). In most years, the ANES survey a new cross section of individuals. Using the information provided by the ANES for the years 1990-2016, I examine the mechanisms of individual efficacy, duty, information, recruitment, preferences, and evaluation of the performance of politicians. To examine psychological factors, I use the following questions: "Public Officials Care about People Like Me", coded 1 if the individual agrees or strongly agrees with the statement and 0 otherwise; and "Elections Make the Government Pay Attention", coded 1 if individuals agree or strongly agree with the statement and 0 otherwise. To obtain a measure of duty, I use the question "For you personally, is voting mainly a duty or mainly a choice," coding a value of 1 if the individual says it is a duty. To obtain a measure of political knowledge, I use the interviewer's rating of the respondent's level of political knowledge, with the dependent variable coded as 1 if the political knowledge is "high" or "very high", and 0 otherwise. To examine the mechanism of recruitment, I use a dummy equal to 1 if the individual has talked to anyone about getting out to vote. To obtain a measure of political preferences, I use a question about whether individuals believe the federal budget should increase aid to the poor, decrease aid to the poor, or keep aid to the poor at the same level. I create a dependent variable equal to "1" if the individual beliefs aid should be increased and 0 otherwise. To obtain a measure of the evaluation of the economy, I use the individual's report of whether the economy was better or worse in the last year. The indicator is equal to 1 if she reports that the economy is worse and 0 if she says it is unchanged or better.

I estimate equation (5) using the data from the ANES, with the key dependent variables being the various measures of psychological factors, information, recruitment, preferences, and evaluation of the performance of politicians. Due to the limited sample size, I cannot include the full set of fixed effects. I include state x group fixed effects, education group x year fixed effects, and controls for the state unemployment rate, and concurrent governor and senate elections. Rather than including age dummies, I include a linear and quadratic term for age.

Table 1.10 displays the results.[36] Strong group labor demand is associated with higher reported belief that voting is a civic duty. Similarly, there is a suggestive positive effect on the belief that public officials care about individuals and that government pays attention to elections. However, labor demand does not strongly affect these measures of recruitment or information. Interestingly, columns (5)-(6) indicate that individuals' beliefs about the performance of the economy and preferences for redistribution respond to overall economic conditions, but not to group-specific circumstances. Overall, the results from the ANES indicate support for the psychological mechanisms through which employment opportunities increase voting.

[36]Impacts of the labor demand index on employment and voting in this sample are shown in Table A.5.

Table 1.10: Mechanisms: American National Election Studies

	Psychological		Mobilization	Information	Evaluation/Preferences	
	(1) Gov Pays Attention to Elections	(2) Duty to Vote	(3) Talk About Getting Out To Vote	(4) High Political Knowledge	(5) Aid to Poor Should be Increased	(6) Economy Worse In Past Year
Labor Demand Index	1.295	29.559*	0.145	-0.272	4.494	-1.303
	(2.176)	(15.409)	(6.582)	(4.890)	(4.195)	(2.211)
State Unemployment Rate	78.951	-223.346	214.030	-92.167	342.990*	274.802**
	(78.520)	(303.908)	(815.408)	(261.305)	(173.130)	(110.145)
Group x State FE	Yes	Yes	Yes	Yes	Yes	Yes
Education x Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5303	1865	1411	3252	3722	5432
R^2	0.111	0.303	0.213	0.260	0.178	0.385
Dep Var Mean	87.604	64.552	33.229	51.641	53.288	53.288
Years Available	1996-2016	2008-2016	1998-2000	1998-2016	2000-2016	2000-2016

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (1.5) with the dependent variables listed in the column headings. The dependent variable equals 1 if the individual voted and 0 otherwise. The labor demand index is at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include a quadratic control for age. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

1.8 Robustness and Extensions

In this section, I provide robustness checks and extensions to my results. I examine adjustments for over-reporting of voting behavior. I the explore the impact of cross-gender labor markets in addition to own labor markets, and perform a placebo test examining the effect of employment conditions on those outside prime working years. Finally, I examine the impact of employment on other forms of civic participation.

1.8.1 Adjustments to CPS VRS Voting Rates

As noted in Section 1.3, the CPS VRS reports voting rates that are higher than official statistics. Although the CPS VRS accurately predicts changes in voting rates, and thus is suitable for this analysis, in this section I show the robustness of my results to adjustments. Hur and Achen (2013) suggest adjusting CPS VRS weights. Hur and Achen (2013) suggest over-weighting individuals who respond that they did not vote and under-weighting individuals who respond that they did vote at the state level, so that weighted averages for each state and year accurately match state-year voting rates. They provide detailed adjustments from 1986-2016. In Table 1.11, I display my main results from Table 1.5 with the weights adjusted so that state CPS voting rates match official election statistics by state and year. The results are robust to these adjustments.

1.8.2 Cross Gender Labor Markets and Voter Turnout

In the analysis in Section 1.6, I focused on the labor market demand for the individual's demographic group only. However, a substantial literature in economics indicates that individuals may be influenced by their spouse's (or potential spouse's) employment as well as

Table 1.11: Voting and the Labor Demand Index, Adjusted Weights

	Original Weights 1986-2016		Adjusted Weights 1986-2016	
	(1)	(2)	(3)	(4)
Labor Demand Index	1.036	0.785*	1.099	0.842*
	(0.682)	(0.429)	(0.675)	(0.483)
Concurrent Governor Election	1.722***		1.332**	
	(0.562)		(0.568)	
Concurrent Senate Election	1.563***		1.843***	
	(0.277)		(0.285)	
State Labor Demand Index	-4.228***		-4.553***	
	(0.845)		(0.827)	
Age FE	Yes	Yes	Yes	Yes
Group x State FE	Yes	Yes	Yes	Yes
Group x Year FE	Yes	Yes	Yes	Yes
State x Year FE	No	Yes	No	Yes
Observations	654535	654535	654535	654535
R^2	0.173	0.180	0.180	0.187
Adjusted R^2	0.172	0.178	0.180	0.186

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (1.5). The dependent variable equals 1 if the individual voted and 0 otherwise. The labor demand index is at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for whether voting behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree. Columns (1) and (3) also include state demographic controls (percent ages 25-54, ages 55-64, and 65 and over; percent white, black, and Hispanic; and percent with high school degrees, some college, and four-year college degrees) and log average state wage. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

their own [e.g., (Bertrand et al., 2015; Schaller, 2016)], since many resources are shared at the household level.

In order to estimate the impact of (potential) spouse's employment, I rely on the well-documented pattern that individuals are most likely to choose spouses with similar racial and educational backgrounds. Thus, white men with less than or equal to high school education are the most likely spouses of white women with less then equal to high school education, etc. In Table 1.12, I regress an individual's likelihood of voting on the labor demand for the most likely group of (potential) spouses: the same-education, same-race, cross-gender group.[37] The results indicate that cross-gender labor market effects do not have a significant impact on turnout. This provides further evidence against a traditional resource theory of voter turnout, since the employment opportunities of a (potential) spouse should provide access to additional monetary resources.

[37] This is consistent with the approach of many studies, see, for example, Charles and Luoh (2010); Bertrand et al. (2015); Schaller (2016); Shenhav (2017).

Table 1.12: Voting and Cross-Gender Labor Markets

	(1)	(2)
Cross Gender Labor Demand Index	0.184	-0.211
	(0.523)	(0.422)
State Labor Demand Index	-2.406**	
	(0.933)	
Concurrent Governor Election	1.734***	
	(0.552)	
Concurrent Senate Election	1.554***	
	(0.235)	
Age FE	Yes	Yes
Group x State FE	Yes	Yes
Group x Year FE	Yes	Yes
State x Year FE	No	Yes
Observations	703841	703841
R^2	0.172	0.178
Adjusted R^2	0.171	0.176

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (1.5). The dependent variable equals 1 if the individual voted and 0 otherwise. The main independent variable is the labor demand index for the group that is of the the opposite gender, the same race, the same education, the same state, and the same year. All columns also include controls for whether voting behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree. Columns (1) and (3) also include state demographic controls (percent ages 25-54, ages 55-64, and 65 and over; percent white, black, and Hispanic; and percent with high school degrees, some college, and four-year college degrees) and log average state wage. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

1.8.3 Results for those Outside Prime Working Ages

Individuals outside of the prime working years of 25-54 are much less likely to be employed than others (Hetschko et al., 2013). As a result, testing the impact of prime-age labor demand on voting rates for older individuals provides a good placebo test, to ensure that no other race-gender factors associated with labor demand are affecting turnout of particular demographic group in a particular state and year. In Table 1.13, I replicate the regressions from Table 1.5 for those ages 18-24 and 65-79. As shown in the Table, the impact of prime-age labor demand does not significantly affect the voting outcomes for younger individuals or older individuals. This provides additional support to the assumption that the labor demand index only affects voter turnout through its impact on employment opportunities.

1.8.4 Other Civic Participation

As noted in Section 1.1 above, voting is an important civic activity and has also been used as a barometer of overall civic engagement [see, e.g., Putnam (2001)]. Therefore, in this section, I examine whether, in fact, voting and other forms of civic participation respond to employment in the same way.

To examine other forms of community participation, I turn to the CPS Volunteer Supplement, issued every September between 2002 and 2015. The Volunteer Supplement asks individuals a variety of questions about their activities over the past year, including whether they volunteered with any organization, the types of organizations with which they volunteered, the total number of organizations with which they volunteered (up to 7), and the total hours they volunteered. The Volunteer supplement also asks a variety of other questions about participation in community activities over the previous year: whether the individual attended a public meeting (asked from 2006-2015), whether the individual worked with

Table 1.13: Labor Market Conditions and Voting for those Outside Prime Working Ages

	Ages 18-24		Ages 65-79	
	(1)	(2)	(3)	(4)
Prime Age Labor Demand Index	0.506	0.787	-0.417	0.053
	(0.713)	(0.643)	(0.401)	(0.367)
Concurrent Governor Election	1.036*		1.384**	
	(0.552)		(0.575)	
Concurrent Senate Election	0.850**		0.723***	
	(0.378)		(0.266)	
State Labor Demand Index	-2.330*		-0.219	
	(1.160)		(0.880)	
Age FE	Yes	Yes	Yes	Yes
Group x State FE	Yes	Yes	Yes	Yes
Group x Year FE	Yes	Yes	Yes	Yes
State x Year FE	No	Yes	No	Yes
Observations	105313	105313	156368	156368
R^2	0.167	0.177	0.113	0.119
Adjusted R^2	0.164	0.169	0.110	0.113

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (5). The dependent variable equals 1 if the individual voted and 0 otherwise. The labor demand index is at the demographic group-state-year level and is for those ages 25-54 within the same group, state, and year. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for whether voting behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree. Columns (1) and (3) also include state demographic controls (percent ages 25-54, ages 55-64, and 65 and over; percent white, black, and Hispanic; and percent with high school degrees, some college, and four-year college degrees) and log average state wage. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

others in the neighborhood on a project (asked from 2006-2015).[38] I create four variables reflecting volunteer engagement. The first, total number of volunteer organizations, is a count variable reflecting the number of organizations with which an individual volunteers.[39] Next, the variable "number of civic or political organizations" is equal to the count of civic organizations, political parties, or a political advocacy organizations for which an individual volunteers. Third, "Community Project" is equal to 1 if the individual has worked with others in the neighborhood on a project and 0 otherwise. Finally, "Public Meeting" is equal to 1 if the individual has attended a public meeting and 0 otherwise. Summary statistics on these variables are available in Table A.6.

In Table 1.14, I show the relationship between the employment demand index and measures of community involvement.[40] Overall, there is suggestive evidence of a relationship between employment opportunities and other measures of community engagement. All coefficients are positive. The coefficient on "Number of Civic or Political Organizations" is on the margin of statistical significance, and the coefficient on "Community Project" is significant at the 5 percent level. These results indicate that good employment opportunities may also increase other civic and political activities in addition to voter turnout.

[38]There is also a question about whether the individual donated at least $25 to charity (asked from 2008-2015). However, number of years provided in the donation sample is too small to enable the labor demand index to strongly predict changes to employment over the short period, and therefore I do not use this question.

[39]Although individuals are not allowed to report more than 7 organizations, less than 0.001 percent of the sample reports the maximum (7 organizations).

[40]The labor demand index is calculated as described in Section 1.5 but with 1990 as the base year. Since there are fewer years of data, a slightly greater sample size restriction is needed to ensure a strong relationship between the labor demand index and employment opportunities. The sample is restricted to those demographic-state groups with at least 500 individuals who are employed in the 1990 Census and at least 50 observations per year in the September CPS.

Table 1.14: Other Forms of Civic Participation

	(1) No. of Volunteer Orgs	(2) No. of Civic or Political Orgs	(3) Community Project	(4) Public meeting
Labor Demand Index	0.830	0.462	1.366**	0.453
	(1.192)	(0.315)	(0.594)	(0.624)
Age FE	Yes	Yes	Yes	Yes
Group x State FE	Yes	Yes	Yes	Yes
Group x Year FE	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes
Observations	497078	497078	337169	337149
R^2	0.107	0.011	0.031	0.045
Dep Var Mean	42.69	1.890	8.054	9.001

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (1.5) with the dependent variables listed in the column headings. The labor demand index is at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for whether volunteer behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree. Data in columns (1)-(2) is from 2002-2015. Data in columns (3)-(4) is from 2006-2015. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

1.9 Conclusion

In this paper, I provide evidence of asymmetric effects of aggregate versus own economic conditions. While aggregate economic improvements decrease turnout, own improvements increase turnout. Specifically, a one standard deviation increase in the labor demand index is associated with a 0.8 percentage point increase in voter turnout, which implies an impact of own employment on voting of up to 40-70 percentage points. By finding different responses to own-group versus aggregate conditions, this paper helps to resolve a puzzle in a literature that finds positive correlations between individual voting and employment but negative correlations between aggregate voting and employment.

Over the past 30 years, employment and wages have fallen in both relative and absolute terms for those with a high school degree or lower levels of education. This paper provides evidence that the falling relative economic status also reduces political participation. If decreased political participation leads to less favorable policies, the impacts of declining economic conditions may be exacerbated. Furthermore, since other civic involvement tends to correlate with voting, falling political participation may indicate decreased community involvement overall. This may have consequences for the next generation, as high levels of civic/community involvement are associated with greater child health and intergenerational mobility (Putnam, 2001; Chetty, Hendren, Kline, and Saez, 2014).

CHAPTER 2

THE BROTHER EARNINGS PENALTY

Angela Cools and Eleonora Patacchini

2.1 Introduction

Growing up with a brother relative to a sister can strongly influence environment. The presence of a male (versus female) sibling can affect parents' treatment of the remaining children, the division of household tasks and development of interests, and even indirectly affect family size. Traditional economic theories suggest that a male sibling may pull parental investment of time, money, or expectations away from females, because boys may be seen as the "higher return" investment (Becker, 1991), or because their more disruptive behavior may change parental expectations for all siblings (Powell and Steelman, 1990). At the same time, psychological evidence indicates that mixed sex environments create stronger gender differentiation (McGuire, McGuire, and Winton, 1979; Cota and Dion, 1986; Turner, Hogg, Oakes, Reicher, and Wetherell, 1987; Abrams, Thomas, and Hogg, 1990; Schneeweis and Zweimüller, 2012; Booth, Cardona, and Nolen, 2013) and, as a result, brothers may cause girls to develop more traditionally feminine behaviors and attitudes and become closer to their mothers relative to their fathers (Grotevant, 1978; Brody and Steelman, 1985; McHale, Crouter, and Tucker, 1999). Finally, brothers may influence parents' fertility choices (if parents have preferences for a child of a specific sex) (Angrist and Evans, 1998; Dahl and Moretti, 2008; Blau, Kahn, Brummund, Cook, and Larson-Koester, 2017).

In this paper, we examine the impact of sibling gender on home environment and adult earnings for a recent cohort of women in the United States. We investigate mechanisms including parental investment, gendered attitudes and behaviors, and participation in risky behaviors.

The existing literature on the impacts of sibling gender focuses largely on cohorts born before 1970, finding no consistent impact of sibling gender on women's educational or labor market outcomes (Butcher and Case, 1994; Kaestner, 1997; Hauser and Kuo, 1998; Conley, 2000; Anelli and Peri, 2015; Rao and Chatterjee, 2017).[1] However, recent work using large administrative datasets in Europe suggests that more recent cohorts of women may experience an earnings penalty from brothers (Gielen, Holmes, and Myers, 2016; Brenøe, 2018; Peter, Lundborg, Mikkelsen, and Webbink, 2018).[2] In fact, these papers seem to indicate a growing earnings penalty from brothers for women over time. Peter et al. (2018) finds that a next-youngest brother reduces female earnings by 0.5 percent for females born from 1938-1977 in Sweden; Gielen et al. (2016) finds that a brother who is close in age reduces female earnings by about 2 percent for women born from 1959-1979 in the Netherlands; and Brenøe (2018) finds that a next-youngest brother reduces female earnings by up to 2 percent for women born from 1962-1975 in Denmark. Little is known, however, about recent cohorts in the United States. Furthermore, the pathways through which sibling gender affects home environment and labor market outcomes are largely unexplored in the literature. This paper makes use of a unique longitudinal dataset, the National Longitudinal Study of Adolescent to Adult Health (Add Health), which provides a number of advantages. First, Add Health examines a recent cohort of women (born in the late 1970s and early 1980s) relative to the more frequently used surveys such as the National Longitudinal Survey of Youth 1979 (NLSY79).[3] Second, it asks detailed questions about childhood environment, behaviors, expectations, and

[1] A related literature looks at the impact of sibling gender on parent-child occupational transmission, generally finding that brothers block father-daughter transmission of occupational preferences. Oguzoglu and Ozbeklik (2016) finds that brothers decrease the likelihood that females with STEM fathers choose STEM majors, and Mishkin (2017) finds that brothers decrease the probability that daughters of self-employed fathers become self-employed themselves. A larger set of papers also looks at the impact of sibling characteristics on long-run outcomes, regardless of sibling gender. See, for example, Yi, Heckman, Zhang, and Conti (2015); Black et al. (2017); Heissel (2017), among others.

[2] Recent results from East Asia also suggest an education penalty from brothers. For example, Ono (2004); Chen, Chen, and Liu (2017); Lei, Shen, Smith, and Zhou (2017) find that brothers decrease educational attainment in Japan, Taiwan, and China, respectively.

[3] The NLSY79 focuses on cohorts born in the early to mid-1960s.

activities. Third, it also contains follow-up interviews that capture information on work, demographic, and attitudinal variables later in life. This longitudinal structure allows us to examine a wide variety of mechanisms, from parents' activities during adolescence (such as interactions with teachers) to attitudes and behaviors during adulthood. Specifically, we examine parents' expectations and school monitoring using responses from the parent survey. We use information on activities (e.g. parents' presence during dinner) to examine parents' time investment, use information on allowances and financial assistance during young adulthood to explore parents' financial support, and use information on medical visits to explore health investment. We use information on activities with mothers versus fathers and reported attitudes on work and desired family size to examine gender differentiation. Finally, we use a battery of questions on risky behavior in adolescence to explore whether sibling gender affects the propensity to engage in risky behavior. Following recent papers in the literature on sibling gender composition (Vogl, 2013; Brenøe, 2018; Peter et al., 2018) we restrict our sample to those with at least one younger sibling and examine the effect of sibling gender composition using the gender of the next-youngest sibling, defined as the next sibling born after the sample respondent (regardless of the birth order of the sample respondent herself). We choose to focus on the gender of the next-youngest sibling because it provides the cleanest identification strategy. The gender of an older sibling may in fact indicate parental gender preferences (if parents make fertility choices based on the gender composition of earlier children). Conditional on deciding to have another child, the gender of that next child should be random. This should hold regardless of whether the additional child is the first, second, or third born (and beyond) in the family.[4] We control for whether a child is the first born child, second born child, or third (or later) born child in all regressions.

We find that the presence of a next-youngest brother lowers earnings for women in their

[4]We provide balance tests supporting the exogeneity of the next-youngest sibling's gender by birth order in Section 3. In Section 6, we also show that our main earnings results are similar when including only first born children in our sample. Results for Tables 3-11 with first born children only are available in the Web Appendix. The evidence remains qualitatively unchanged.

late 20s and early 30s by approximately 7 percent. Our investigation of the mechanisms behind this effect reveals that the presence of a next-youngest brother lowers parental investment in the form of expectations and schoolwork monitoring for daughters and increases daughters' propensity to engage in traditionally feminine tasks.

Besides contributing to a growing literature on the impact of sibling gender (discussed above), our research contributes to the literature on the earnings gap. As noted by Blau and Kahn (2017), despite gains in relative earnings over the past half-century, women continue to earn less than men on both an annual and weekly basis. A variety of mechanisms have been put forward to explain the earnings gap [see Blau and Kahn (2017) for an overview]. These include factors such as differences in occupation and industry (Blau and Kahn, 2017), differences in time spent on home production (including housework and childcare) (Hersch and Stratton, 1997, 2002), differences in the propensity to work very long hours (Gicheva, 2013; Goldin, 2014; Cortés and Pan, 2016), and discrimination, among others. A recent literature has also examined the role of gender norms, noting that women with more traditional gender norms may strive to earn less than their husbands (Bertrand et al., 2015), and that exposure to more working females in adolescence may lead women to have higher labor force participation when they have children (Olivetti, Patacchini, and Zenou, 2018). By noting pathways through which brothers reduce women's earnings, we show that exogenous changes to family environments can lead women to receive greater parental expectations and reduce the extent of their family responsibilities relative to other household members, potentially reducing earnings gaps between men and women later in life.

Our analysis proceeds as follows. We first outline the potential theoretical implications of sibling gender on women's labor market outcomes. We then discuss our primary data source and our identification strategy. Next, we detail our results and examine mechanisms. Finally, we provide extensions to our results and conclude.

2.2 Theoretical Overview

In this Section, we describe in greater detail the theoretical basis for the impact of sibling gender on long-run outcomes. We first examine the role of sibling gender on household size (parents' fertility choices) and structure, and then explore parental investment, gender differentiation, and risky behavior.

First, one natural way that sibling gender may influence long-run outcomes is through effects on fertility choices. Parents may have preferences over the gender of their offspring. Since parents may want at least one child of each gender (Angrist and Evans, 1998) or want at least one son (Dahl and Moretti, 2008), the birth of a male sibling to a woman's parents may lead the parents to stop having children, reducing her overall family size (Angrist and Evans, 1998). Smaller family size may increase the resources allocated to each child (Cáceres-Delpiano, 2006), and research has indicated that smaller families may improve achievement measures such as educational attainment or test scores (Jaeger, 2008; Booth and Kee, 2009; Åslund and Grönqvist, 2010; Silles, 2010).[5] Fewer children may also increase the labor force participation of mothers, which may then influence daughters' participation (Angrist and Evans, 1998; Fernández, 2013; Boustan and Collins, 2014; Olivetti et al., 2018). In addition to decreasing overall family size, brothers may affect the structure of households. Morgan, Lye, and Condran (1988), Dahl and Moretti (2008), and Blau et al. (2017) document that parents are more likely to be married if there is a male child; thus, females with brothers may be more likely to live in a two-parent household. Two-parent households have also been tied to a variety of positive outcomes, including improved performance in school (Pong,

[5]The effect of brothers through reductions in family size has been labeled the "indirect effect" of brothers, and has been shown to be large and positive in Taiwan (Chen et al., 2017). In contrast, Black, Devereux, and Salvanes (2005) finds little evidence of a causal impact of sibship size on educational attainment or earnings for individuals in Norway when instrumenting for sibship size with twins, and Angrist, Lavy, and Schlosser (2010) finds no impact of sibship size on long-run outcomes in Israel. Black, Devereux, and Salvanes (2010) finds that expected increases in family size do not affect IQ scores for young men in Norway, but unexpected increases in family size decrease IQ scores. However, very few studies to our knowledge indicate that smaller families lead to worse outcomes.

Dronkers, and Hampden-Thompson, 2003; Steele, Sigle-Rushton, and Kravdal, 2009). Overall, the impacts of a brother on household size and structure should generally be positive for women, because reductions in family size, increases in mothers' labor force participation, and presence in a two-parent household all generally have an insignificant or positive impact on hours and/or earnings (see above).

In addition to influencing fertility choices and family structure, brothers may affect females more directly by influencing parents' and siblings' attitudes and behaviors, which may also affect women's long-run outcomes. Below, we provide theoretical motivation for three key mechanisms through which brothers may directly impact long-run earnings: parental investment, gendered attitudes and behaviors, and disruptive behaviors.

We first consider the impact of sibling gender composition on broadly defined parental investment (including expectations, school monitoring, overall time with children, financial investment, and health care investment). These measures of investment have all been shown to differ across families in developed countries and have been shown to impact long-run earnings.[6] According to the framework developed by Becker (1991), parents choose to maximize the sum of lifetime earnings of their children subject to a resource constraint (time, energy, financial resources, etc.). Thus, parents will invest most in the child with the highest marginal return (or lowest marginal cost). If males have a higher return on investment in the labor market relative to females, parents may invest more resources (money and time) in boys.[7] Prior to the 1970s, most women worked for short periods of life and had few career opportunities open to them (Blau and Kahn, 2017). As a result, the benefit of investing in

[6]For the influence of parental expectations, see Yamamoto and Holloway (2010), among others. For monitoring, see Wang and Sheikh-Khalil (2014), among others. For parental time and activities, see Datcher-Loury (1988); Pleck (1997); Zick, Bryant, and Österbacka (2001); Milkie, Nomaguchi, and Denny (2015). For financial investment, see Yeung, Linver, and Brooks-Gunn (2002); Menning (2002). Finally, for health care investment, see Case, Lubotsky, and Paxson (2002) and Datar, Kilburn, and Loughran (2010).

[7]Evidence that parents may invest more in cognitively higher-ability children in the United States is given in Grätz and Torche (2016). For evidence on parental investment in boys relative to girls in India, see Barcellos, Carvalho, and Lleras-Muney (2014). A related literature examines whether parents compensate for or reinforce differences in health endowments between children, with the majority of studies finding that parents reinforce endowment differences [see Almond and Mazumder (2013) for a review of the literature].

the future careers of these women was often lower than the costs, regardless of whether the woman had brothers or sisters. However, in recent years, the earnings potential of women has grown dramatically, so that parents may be more willing to invest in a girl (but still less than in a boy in the presence of constraints).[8]

In addition to affecting parental investment, male siblings may affect gender role views and feminine identity, leading females to specialize in more traditionally feminine tasks and attitudes. A large literature in psychology suggests that siblings strive to "differentiate" themselves from each other, or establish their uniqueness from their siblings [see, e.g., Ansbacher and Ansbacher (1956); Plomin and Daniels (1987); Feinberg and Hetherington (2000)].[9] This form of sibling differentiation appears to become especially important during adolescence and involves both younger and older siblings trying to differentiate themselves from each other (McHale, Updegraff, Helms-Erikson, and Crouter, 2001). One form of differentiation in mixed-sex environments is gendered behavior and attitudes. The psychology literature has documented that the presence of more members of the opposite gender in a household or group increases the "salience" of one's own gender (i.e. the importance of gender to overall identity) (McGuire et al., 1979; Cota and Dion, 1986; Turner et al., 1987; Abrams et al., 1990), which can then lead an individual to engage in behavior more consistent with traditional gender roles and attitudes (Turner, 1982; Cota and Dion, 1986; Favara, 2012; Schneeweis and Zweimüller, 2012; Booth et al., 2013). This gender differentiation may be reinforced if parents of mixed-gender siblings may choose to specialize, with the father spending more time with the son and the mother spending more time with the daughter (Brenøe, 2018). In support of this theory, evidence from sociology indicates that the presence of opposite-gender siblings may lead to the adoption of more traditional sex-typed activities and interests [see Grotevant (1978); Brody and Steelman (1985); McHale et al. (1999)]. The

[8] In support of this view, studies have shown declining son preference in the United States. Parents before 1980 had more additional children after a firstborn daughter relative to a firstborn son (Dahl and Moretti, 2008), but this son preference has been generally eliminated in recent years (Blau et al., 2017).

[9] This theory is known as sibling deidentification.

identity framework developed by Akerlof and Kranton (2000) suggests that individuals suffer a utility loss from acting in ways that are in conflict with their perceived identities. Therefore, if a woman raised with brothers develops a stronger traditionally female identity than a woman raised with sisters, she may face a greater cost of acting in traditionally non-feminine ways later in life (e.g., working long hours or in male dominated careers) and may be less likely to do so.

Finally, siblings may influence participation in disruptive behaviors such as criminal activity and substance use. As discussed above, if parents choose to gender specialize, a girl with a brother may spend less time with her father. Since distance from one's father is associated with higher delinquency rates (Bronte-Tinkew, Moore, and Carrano, 2006; Fosco, Stormshak, Dishion, and Winter, 2012), females with brothers may be more likely to engage in substance abuse and/or delinquent behaviors in adolescence, which may in turn affect their health and cognitive development (Volkow, Baler, Compton, and Weiss, 2014), educational outcomes (Bray, Zarkin, Ringwalt, Qi, et al., 2000; Balsa, Giuliano, and French, 2011) and ultimately their labor market outcomes (Ringel, Ellickson, and Collins, 2006). In addition to time spent with father, lower parental expectations and (other) investment as outlined previously may also lead to increases in delinquent behavior.

Below, we use questions on parental expectations, parent-child activities, parental spending, and other factors to examine the importance of these mechanisms in explaining the earnings penalty from brothers.

2.3 Data and Identification

2.3.1 Data

The primary data set used in this paper is the National Longitudinal Survey of Adolescent to Adult Health (Add Health).[10] Add Health was designed to study the impact of family, neighborhood, and school environment on adolescents' behavior. In 1994-1995, it collected data from students in grades 7-12 from a nationally representative sample of about 130 private and public schools (Wave I). From the roughly 90,000 students sampled in 1994-1995, a subset of about 20,000 students was selected for a more detailed in-home interview (which included both a student and parent interview). This subsample was again interviewed in 1996 (Wave II), in 2001-2002 (Wave III), and in 2008 (Wave IV). The longitudinal aspect of this data allows us to examine characteristics of the household in adolescence, and employment outcomes and family structure in adulthood.

We use data from Wave I to obtain information about family characteristics of our sample. In Wave I, students are asked to give a detailed household roster listing up to 20 individuals and their relation to the student. We count as brothers those listed as full brothers or half brothers, and count as sisters those listed as full sisters or half sisters. We exclude step siblings and adopted siblings, since the gender of these siblings may be endogenous. As discussed below, our key independent variable is the gender of the next-youngest sibling. Other individual and family characteristics pulled from Wave I include student age (cohort), race, mother's age, parents' education, and parents' immigration status. We also pull

[10]This research uses data from Add Health, a program project directed by Kathleen Mullan Harris and designed by J. Richard Udry, Peter S. Bearman, and Kathleen Mullan Harris at the University of North Carolina at Chapel Hill, and funded by grant P01-HD31921 from the Eunice Kennedy Shriver National Institute of Child Health and Human Development, with cooperative funding from 23 other federal agencies and foundations. Special acknowledgment is due Ronald R. Rindfuss and Barbara Entwisle for assistance in the original design. Information on how to obtain the Add Health data files is available on the Add Health website (http://www.cpc.unc.edu/addhealth). No direct support was received from grant P01-HD31921 for this analysis.

information on whether the student lives in a two-parent household, the total number of siblings in the household, mother's working behavior, activities with parents, health care, allowances, disruptive behaviors, parents' expectations, and parents' monitoring of student school performance. We use data from later waves on parents' financial investment over time and on students' family responsibilities and intentions.

Our key dependent variable is log earnings in Wave IV. We include earnings for anyone who reports working at least 10 hours a week in a current or previous job. We exclude those who claim to have worked at least 10 hours per week, but report earnings under $2000. We also exclude earnings for those in the military or prison.[11] We winsorize earnings at the 95th percentile to limit the influence of outliers. We do so by setting earnings above the 95th percentile of log earnings equal to the value for the 95th percentile.

Our final sample includes those with any younger sibling in the household. We exclude those born outside the United States and those who are of Asian descent and have an immigrant parent.[12] Descriptions of our variables and summary statistics for our final sample are presented in Appendix Table B.1. As shown in Table B.1, as of Wave IV, most of the individuals in our sample have a high school degree, and about one-third have a college degree. Over 60 percent of our sample works full time, which is consistent with full time labor force participation rates of women in 2007 (Solis and Hall, 2009).[13] The average earnings of women in our sample is about $32,000 per year, and the median earnings is $28,000. This is consistent with the median weekly earnings for women in 2007 as reported by the Bureau of

[11] At a minimum wage of $5.15 per hour in 2007, $2000 corresponds to just under 4000 hours per year, or about 40 weeks of work at 10 hours per week. In addition, $2000 was the minimum earnings amount needed to count as two quarters of work by the Social Security Administration in 2007. See https://www.ssa.gov/oact/cola/QC.html

[12] We exclude these groups because there is evidence of sex-selective abortion outside the United States and among Asian American immigrant populations (Almond and Edlund, 2008; Abrevaya, 2009; Sen, 2017), which may interfere with our identification strategy (see Section 3.2).

[13] In particular, roughly 80 percent of those ages 25-34 were in the labor force in 2007. About three-quarters of working women work full time, and thus we estimate that roughly 60 percent of women ages 25-34 work full time. Our measure of any earnings is higher than that in the 80 percent labor force participation reported by Solis and Hall (2009), likely due to our very inclusive definition of employment that allows for any labor force attachment over the previous year (not just attachment in the week of the survey).

Labor Statistics (Solis and Hall, 2009).[14]

2.3.2 Identification

The goal of this paper is to identify the impact of sibling gender on long-run outcomes. A natural approach to answer this question would be to regress long-run earnings on the presence of a brother or total number of brothers in the household. However, as noted by Vogl (2013), Brenøe (2018), Peter et al. (2018), and others, this approach poses identification problems. In particular, the decision to have an additional child is not exogenous, and may depend on the gender composition of previous children. For example, parents with a strong preference for sons may continue having children until the birth of a son. In this family, the presence of a brother would not be exogenous, but would be correlated with family characteristics.

Following a growing literature [see, e.g., Vogl (2013); Brenøe (2018); Peter et al. (2018)], we focus our analysis on the gender of the next-youngest sibling. The idea is that, conditional on the decision to have another child, the gender of that child is random.[15] Focusing on just the next-youngest sibling allows us to obtain a causal estimate of the impact of having an additional male (relative to female) sibling. Since a younger brother may decrease family size (and thus positively impact earnings), our estimated effect may in fact be a lower bound for the estimate of a next-youngest brother conditional on family size.

To examine our identification strategy, in Table 2.1 we present summary statistics of individual and family characteristics for those with a next-youngest brother versus next-

[14] Specifically, Solis and Hall (2009) reports median weekly earnings of $597 for those ages 25-34. If we multiply this by 52 weeks, we get earnings of roughly $31,000 per year, slightly higher than the estimate in our sample.

[15] This is true absent widespread sex-selective abortion. There is little evidence of sex-selective abortion among the population born in the United States, with the exception of descendants of Asian immigrants (who are excluded from the sample) (Almond and Edlund, 2008; Abrevaya, 2009).

youngest sister for the entire sample [panel (a)] and distinguishing by birth order [panels (b)-(d)]. Evidence showing that individual and parent characteristics that are determined prior to birth are the same across groups supports the assumption that sibling gender is distributed randomly. At the bottom of each subtable, we also examine the relationship between the next-youngest sibling's gender and family size, which we do not expect to be balanced across the gender of the next-youngest sibling, as noted in Section 2. As expected, family size is larger for those with younger sisters, due to parental preferences for sons and/or preferences for at least one child of each gender. Perhaps interestingly, this effect is only significant for first born children. Among the other characteristics, there are few differences between those with a next-youngest brother and next-youngest sister that are statistically significant. Among first born girls, those with a younger sister are slightly older than those with a younger brother. Among second born children, those with a younger sister are slightly less likely to have parents that have completed high school than those with a younger brother. Among the over 30 outcomes studied, only 3 are significant at the 10 percent level and none is significant at the 5 percent level. This is lower than what would be expected by chance, and therefore supports our assumption that the gender of the next-youngest sibling is distributed randomly.

In the sections below, our main independent variable is a dummy that equals 1 if the next-youngest sibling is male (and 0 if she is female).

Table 2.1: Balance Tests

(a) All Females

	(1) Next-Youngest Brother	(2) Next-Youngest Sister	(3) *T-Stat*	(4) *P-Value*
White	0.67	0.67	*0.28*	*0.78*
Black	0.18	0.19	*-0.88*	*0.38*
Latino	0.12	0.11	*0.32*	*0.75*
Asian	0.01	0.01	*1.31*	*0.19*
Other Race	0.03	0.03	*-0.34*	*0.73*
Immigrant Parent	0.12	0.12	*0.24*	*0.81*
Age	15.06	15.23	*-1.93*	*0.06*
Mother's Age	38.64	38.41	*1.06*	*0.29*
Mother Completed High School	0.8	0.78	*0.99*	*0.32*
Mother Completed College	0.21	0.23	*-0.81*	*0.42*
Father Completed High School	0.81	0.81	*0.32*	*0.75*
Father Completed College	0.22	0.2	*1.04*	*0.3*
Total Siblings in HH in Wave I	1.86	1.97	*-2.04*	*0.04*
Birth Order	1.43	1.47	*-0.72*	*0.47*
Observations	1814	1786		

(b) First Born Females

	Next-Youngest Brother	Next-Youngest Sister	*T-Stat*	*P-Value*
White	0.7	0.69	*0.19*	*0.85*
Black	0.18	0.18	*-0.11*	*0.91*
Latino	0.09	0.1	*-0.31*	*0.76*
Asian	0.01	0.01	*1.03*	*0.3*
Other Race	0.02	0.03	*-0.59*	*0.56*
Immigrant Parent	0.11	0.11	*-0.29*	*0.78*
Age	14.98	15.16	*-1.91*	*0.06*
Mother's Age	37.96	37.69	*1.11*	*0.27*
Mother Completed High School	0.83	0.82	*0.48*	*0.63*
Mother Completed College	0.23	0.24	*-0.32*	*0.75*
Father Completed High School	0.82	0.85	*-1.12*	*0.26*
Father Completed College	0.21	0.2	*0.39*	*0.69*
Total Siblings in HH in Wave I	1.59	1.74	*-2.55*	*0.01*
Birth Order	1	1		
Observations	1240	1192		

(c) Second Born Females

	Next-Youngest Brother	Next-Youngest Sister	*T-Stat*	*P-Value*
White	0.68	0.67	*0.21*	*0.84*
Black	0.16	0.19	*-0.74*	*0.46*
Latino	0.14	0.13	*0.32*	*0.75*
Asian	0.01	0.01	*0.79*	*0.43*
Other Race	0.02	0.02	*-0.3*	*0.76*
Immigrant Parent	0.1	0.11	*-0.27*	*0.79*
Age	15.34	15.43	*-0.58*	*0.56*
Mother's Age	39.92	39.32	*1.33*	*0.19*
Mother Completed High School	0.81	0.74	*1.7*	*0.09*
Mother Completed College	0.19	0.23	*-1.38*	*0.17*
Father Completed High School	0.83	0.75	*1.73*	*0.09*
Father Completed College	0.27	0.22	*1.46*	*0.15*
Total Siblings in HH in Wave I	2.22	2.33	*-1.18*	*0.24*
Birth Order	2	2		
Observations	398	423		

(d) Third Born (and Beyond) Females

	Next-Youngest Brother	Next-Youngest Sister	*T-Stat*	*P-Value*
White	0.49	0.48	*0.11*	*0.92*
Black	0.22	0.31	*-1.44*	*0.15*
Latino	0.23	0.18	*0.77*	*0.45*
Asian	0.01	0	*1.36*	*0.18*
Other Race	0.05	0.03	*0.78*	*0.44*
Immigrant Parent	0.29	0.18	*1.25*	*0.22*
Age	15.08	15.18	*-0.43*	*0.67*
Mother's Age	41.24	41.7	*-0.73*	*0.47*
Mother Completed High School	0.57	0.63	*-0.78*	*0.43*
Mother Completed College	0.14	0.13	*0.18*	*0.86*
Father Completed High School	0.72	0.63	*1.01*	*0.32*
Father Completed College	0.17	0.16	*0.18*	*0.86*
Total Siblings in HH in Wave I	2.96	2.82	*0.61*	*0.54*
Birth Order	3.56	3.64	*-0.43*	*0.67*
Observations	176	171		

Note: Means are reported in Columns (1) and (2). The T-Statistics and P-Values are from tests of differences in means between Columns (1) and (2). Wave I sampling weights are used in all calculations.

2.4 Results

Table 2.2 presents ordinary least squares (OLS) regressions for females in which the dependent variable is the log of earnings in adulthood, and the key independent variable is an indicator for whether the next-youngest sibling is male. Column (1) includes only controls for student age, student race, birth order, mother's age, and parents' immigration status, column (2) adds controls for mother's education, and column (3) adds controls for father's education. This table shows that the presence of a next-youngest brother decreases female earnings by about 7 percent relative to a sister. We perform similar regressions for males, and display the results in Web Appendix Table A.1.1. For males, the presence of a next-youngest brother shows a positive association with earnings, although the effects do not retain statistical significance once we add controls for parental education. Given the strength of our results for females, we focus on investigation of the mechanisms underlying the penalty for women.

Because our dependent variable is log earnings, women with no earnings are excluded from the regressions in Table 2.2. To test whether selection into employment is an issue, we develop a measure "any earnings". Our measure of "any earnings" is equal to 1 if the individual had any personal income (over $2000) in the prior year. We run a linear probability model in which the dependent variable is 1 if the individual has earnings in the prior year (and is 0 otherwise). The results are shown in Table 2.3, column (1). There is no significant impact of the next-youngest sibling's gender on employment (that is, on selection into our earnings sample), providing confidence that our results are not driven by differential selection into paid employment. We next examine the intensive margin of employment. We create a measure of the log hours worked for individuals who report working at least 10 hours per week in the current or most recent job. Table 2.3, column (2), shows the relationship between a next-youngest brother and log hours worked. The results indicate that a next-youngest

Table 2.2: Next-Youngest Brother and Earnings in Adulthood, Females

	(1) Log Earnings	(2) Log Earnings	(3) Log Earnings
Next Youngest is Brother	-0.0678*	-0.0725**	-0.0771**
	(0.0357)	(0.0353)	(0.0350)
Mother's Age	0.187***	0.124**	0.111**
	(0.0547)	(0.0537)	(0.0508)
Mother's Age Squared	-0.00205***	-0.00137**	-0.00124*
	(0.000688)	(0.000674)	(0.000638)
First Child	0.0839	0.00408	-0.0148
	(0.0611)	(0.0627)	(0.0621)
Second Child	-0.00542	-0.0562	-0.0765
	(0.0678)	(0.0673)	(0.0655)
Mother HS Graduate		0.264***	0.220***
		(0.0555)	(0.0579)
Mother College Graduate		0.187***	0.129**
		(0.0535)	(0.0537)
Father HS Graduate			0.209***
			(0.0732)
Father College Graduate			0.125
			(0.0756)
Cohort controls	Yes	Yes	Yes
Race Controls	Yes	Yes	Yes
Observations	2315	2315	2315
R^2	0.087	0.115	0.127

Note: OLS Parameter estimates and standard errors (in parentheses) are reported. Standard errors are clustered at the school level. Unless otherwise specified, all controls are for Wave I, and Wave I sampling weights are used in all calculations. Race controls include indicators for Black, Latino, Asian, and Other Race (with the omitted category being White). Cohort controls include dummies for the student's age. All regressions also include an indicator for whether the mother is an immigrant and whether the father is an immigrant. If parents' education, age, immigration status, or respondent's birth order is missing, we set the value equal to zero and include a dummy for the presence of a missing value. * p<0.1, ** p<0.05, *** p<0.01

Table 2.3: Next-Youngest Brother and Educational and Other Labor Market Outcomes, Females

	(1) Any Earnings	(2) Log Weekly Hours Worked	(3) College	(4) Occupational Prestige Score	(5) Occupational Earnings Score
Next Youngest is Brother	-0.0120	-0.0384***	-0.0189	-0.338	-0.323
	(0.0152)	(0.0139)	(0.0181)	(0.596)	(1.237)
Mother's Age	-0.0119	-0.00808	0.0877***	1.472*	1.370
	(0.0198)	(0.0164)	(0.0239)	(0.829)	(1.760)
Mother's Age Squared	0.000237	0.0000791	-0.000890***	-0.0156	-0.0128
	(0.000237)	(0.000196)	(0.000300)	(0.0101)	(0.0212)
First Child	0.0977**	0.0191	0.154***	2.933**	5.668**
	(0.0411)	(0.0275)	(0.0470)	(1.138)	(2.572)
Second Child	0.0741	0.0119	0.108***	1.106	2.868
	(0.0464)	(0.0291)	(0.0407)	(1.440)	(2.884)
Mother HS Graduate	0.0534**	0.0169	0.131***	3.295***	4.361**
	(0.0269)	(0.0213)	(0.0248)	(1.007)	(2.000)
Mother College Graduate	0.00490	-0.00525	0.147***	1.559**	4.993***
	(0.0206)	(0.0306)	(0.0338)	(0.781)	(1.786)
Father HS Graduate	0.0579*	0.0498*	0.121***	2.617**	6.168***
	(0.0343)	(0.0263)	(0.0294)	(1.128)	(1.993)
Father College Graduate	-0.0135	0.0303	0.270***	4.300***	8.747***
	(0.0260)	(0.0372)	(0.0304)	(0.926)	(2.152)
Cohort controls	Yes	Yes	Yes	Yes	Yes
Race Controls	Yes	Yes	Yes	Yes	Yes
Observations	2678	2105	2836	2777	2777
R^2	0.044	0.031	0.263	0.123	0.097
Dep Var Mean	0.872	3.677	0.332	45.49	44.98

Note: See notes to Table 2.2 The occupational prestige score refers to the Nakao-Treas Prestige Score [see Nakao and Treas (1994)], obtained from Ruggles et al. (2016). The occupational earnings score is obtained from Ruggles et al. (2016).

brother decreases weekly hours by about 4 percent.[16]

Differences in income may be a result of differences in educational attainment and occupational choice, or differences within educational groups or occupational categories. In Table 2.3, columns (3)-(5) we examine the impact of a next-youngest brother on educational and occupational outcomes. In column (3), the dependent variable is equal to 1 if the individual has graduated from college. We find no significant difference in college graduation rates by the gender of the next-youngest sibling. Even though the majority of our sample consists of first born females, we do find evidence that those of higher birth order are more likely to be employed and are more likely to complete college, consistent with Black et al.

[16] We cannot construct an accurate measure of hourly earnings, since hours are calculated from the question "How many hours a week (do/did) you usually work at this job?", answered for the current or most recent job. Earnings are based on total personal earnings in the prior year, and thus the two may not be consistent if the individual changed jobs or changed hours over the past year.

(2005) and others.

In Column (4), the dependent variable is the Nakao-Treas Prestige Score for the individual's reported occupation [obtained from Ruggles et al. (2016)].[17] We find no significant effect of sibling gender on occupational prestige. In Column (5), the dependent variable is the 1990 occupational earnings score from Ruggles et al. (2016).[18] We again find no significant impact of the next-youngest sibling's gender on the self-reported occupation's median earnings.

We view decisions about hours, education, and occupation as part of the larger set of work-related outcomes. These factors jointly determine earnings and are likely influenced by the same mechanisms. As a result, in our examination of mechanisms below, we use regressions without hours, education, or occupation controls. This allows us to examine the impact of these mechanisms on earnings in totality rather than focusing on whether these differences come through education, occupation, hours, or earnings within group.

2.5 Mechanisms

We now consider the mechanisms through which the presence of a brother may affect women's earnings.

[17] In 1989, the General Social Survey asked respondents to rank 110 occupations by social standing on a scale of 1 (low) to 9 (high). The Nakao-Treas Prestige Score converts these average responses into a ranking for each occupation of the 1980 Census from 0 to 100, and women in our sample have occupations ranking from 17 to 86. See Nakao and Treas (1994) for details on the methodology.

[18] The occupational earnings score takes the median earnings in a given occupation in the 1990 Census, standardizes it, and then converts it to a percentile, and women in our sample have occupations ranking from 7 to 100. See https://usa.ipums.org/usa-action/variables for more details.

2.5.1 Fertility Choices and Family Structure

As noted above, the presence of an additional brother may affect women by changing family size and structure. In Table 2.4, we examine whether the gender of the next-youngest sibling is related to the total number of siblings in the household [column (1)], the probability of being in a two-parent household [column (2)], and the probability that the mother works outside the home [column (3)]. Consistent with the literature, we find that sibling gender diversity is associated with smaller families: having a next-youngest brother reduces the number of siblings by 0.1. However, as discussed in Section 2, the literature suggests that, if anything, smaller family size should be beneficial for long-run outcomes. The estimates on mother working and two-parent household are not statistically significant.

2.5.2 Parental Investment

We separate our analysis of parental investment into four categories: school monitoring and expectations, time investment, financial investment, and health care investment. We separate these forms of investment because they reflect very different pathways through which parents may invest in children.

First, we consider whether sibling gender affects **school-specific monitoring or expectations** of a child. To measure this form of investment, we use three questions asked of parents in Wave I. The three questions are: "Have you talked with any of NAME's teachers about (his/her) school work this school year, either informally or in a regularly scheduled parent-teacher conference?" (answers are 0=no or 1=yes), labeled "Talked with Child's Teachers"; "In the past week, have you and NAME talked about (his/her) school work or grades?" (answers are 0=no or 1=yes), labeled "Talked with Child about School or Grades"; and "How disappointed would you be if NAME did not graduate from college?"

Table 2.4: Family Structure in Adolescence, Females

	(1) Number of Siblings in HH	(2) Two-parent Household	(3) Mother Worked
Next Youngest is Brother	-0.108**	-0.00144	0.0232
	(0.0471)	(0.00954)	(0.0207)
Mother's Age	-0.00260	-0.0137	0.0385
	(0.0649)	(0.0125)	(0.0282)
Mother's Age Squared	-0.000351	0.000173	-0.000517
	(0.000817)	(0.000149)	(0.000354)
First Child	-1.136***	0.0201	0.0525
	(0.161)	(0.0202)	(0.0351)
Second Child	-0.511***	0.00139	0.0927**
	(0.159)	(0.0187)	(0.0374)
Mother HS Graduate	-0.210***	0.0162	0.187***
	(0.0785)	(0.0133)	(0.0310)
Mother College Graduate	-0.172***	0.00141	0.0814***
	(0.0451)	(0.0131)	(0.0254)
Father HS Graduate	0.0455	0.0171	0.0203
	(0.0913)	(0.0149)	(0.0370)
Father College Graduate	0.0877	-0.00445	-0.0138
	(0.0626)	(0.0100)	(0.0327)
Cohort controls	Yes	Yes	Yes
Race Controls	Yes	Yes	Yes
Observations	3600	3600	3600
R^2	0.208	0.781	0.139
Dep Var Mean	1.914	0.707	0.725

Note: See notes to Table 2.2

(answers range from 1=Not Disappointed to 3=Very Disappointed, and transformed by us into a dummy of 0=Not Disappointed or Somewhat Disappointed and 1=Very Disappointed), labeled "Disappointed if Child not Grad Coll". In Table 2.5, we examine whether these measures of school monitoring/expectations vary by the gender of the next-youngest sibling. As shown in the table, the presence of a next-youngest brother reduces parents' monitoring and expectations for female students' academic achievement across all three categories. Interestingly, there also appears to be a positive association between high parental expectations and being the first born child.

Table 2.5: Mechanisms: Parents' School Monitoring and Parents' School Expectations, Females

	(1) Talked with Child's Teachers	(2) Talked with Child about School or Grades	(3) Disappointed if Child not Grad College
Next Youngest is Brother	-0.0452*	-0.0347*	-0.0350*
	(0.0229)	(0.0194)	(0.0181)
Mother's Age	0.0362	0.00778	-0.00725
	(0.0280)	(0.0273)	(0.0309)
Mother's Age Squared	-0.000477	-0.000123	0.000102
	(0.000343)	(0.000340)	(0.000381)
First Child	-0.0164	-0.0214	0.0841*
	(0.0445)	(0.0373)	(0.0499)
Second Child	0.0108	-0.0192	0.0161
	(0.0488)	(0.0342)	(0.0480)
Mother HS Graduate	0.0309	0.0890***	0.00824
	(0.0322)	(0.0340)	(0.0343)
Mother College Graduate	0.00525	0.0130	0.147***
	(0.0265)	(0.0232)	(0.0309)
Father HS Graduate	0.0441	0.00372	-0.0431
	(0.0335)	(0.0368)	(0.0353)
Father College Graduate	0.0574*	0.0288	0.113***
	(0.0330)	(0.0252)	(0.0296)
Cohort controls	Yes	Yes	Yes
Race Controls	Yes	Yes	Yes
Observations	3212	3600	3206
R^2	0.022	0.035	0.060
Dep Var Mean	0.349	0.771	0.410

Note: See notes to Table 2.2

We next consider **parents' general time investment**, which we measure by the frequency of parent-child activities. The Wave I survey asks students whether they engaged in a variety of activities with their parents over the past four weeks. We create variables for each of these activities that equal 2 if the student has engaged in the activity with both the mother and the father over the past four weeks, 1 if she has engaged in the activity with one parent, and 0 otherwise. The questions are: "Have you gone to a movie, play, museum, concert, or sports event?", labeled "Event"; "Have you played a sport?", labeled "Sports"; "Have you gone shopping?", labeled "Shopping"; and "Have you talked about someone you're dating, or a party you went to?" or "Have you had a talk about a personal problem you were having?", labeled "Conversation".[19] Finally, we also use the frequency of parents and children eating

[19] These last two questions are combined into an indicator for "Conversation", which takes a value of 2 if the student has done either activity with both parents, 1 if she has done either activity with one parent, and 0 otherwise.

meals together to measure parental time investment. We use the Wave I question: "On how many of the past 7 days was at least one of your parents in the room with you while you ate your evening meal?" (answers range from 0=None to 7=All), labeled "Meal Days". To test for differences in parental time investment by sibling gender, we present regressions for each of the parent-child activities in Table 2.6. The results in columns (1)-(5) indicate that a next-youngest brother (relative to a sister) generally has a statistically insignificant impact on time investment by parents as measured by parent-child activities. Thus, this evidence does not point towards differences in parental time investment by sibling gender.

We next consider parents' **financial investment**. We use two variables in Add Health to measure parents' financial investment. First, we use the Wave I question, "How much is your allowance each week? If you don't receive your allowance weekly, how much would it be each week?" (answers range from $0 to $95). We create a dummy variable for whether or not the student received an allowance based on the answer to this question, which is labeled "Received Allowance during Adolescence". Next, we use two questions from Wave III to estimate financial help in adulthood. The first question is: "Has HE/SHE [MOTHER/FATHER] given you any money or paid for anything significant for you during the past 12 months? Don't include regular birthday or holiday gifts" (answers are 0=no or 1=yes). The second question is: "Please give an estimate of this financial help in the past 12 months. Include money given directly to you and the cost of significant items bought for you by [MOTHER/FATHER]" (answers range from 1=less than $200 to 4=$1000 or more). We create a dummy variable equal to 1 if either parent has given financial help of $1000 or more, and 0 otherwise, which is labeled "Parent Helped Financially during Adulthood". In Table 2.7, we examine whether parents' financial investment varies by the gender of the next-youngest sibling. The results in Table 2.7 indicate that parents' financial investment as measured by our two variables does not vary significantly by the gender of the next-youngest sibling. Thus, Table 2.7 provides suggestive evidence that financial investment differences may not strongly contribute to the earnings penalty from brothers.

Table 2.6: Mechanisms: Parents' Time Investment, Females

	(1) Event	(2) Sports	(3) Shopping	(4) Conversation	(5) Meal Days
Next Youngest is Brother	0.0632**	0.0219	0.0262	0.0429	0.0119
	(0.0285)	(0.0258)	(0.0222)	(0.0339)	(0.104)
Mother's Age	0.105**	0.0364	0.0180	-0.0703	-0.0175
	(0.0428)	(0.0302)	(0.0321)	(0.0458)	(0.142)
Mother's Age Squared	-0.00129**	-0.000564	-0.000281	0.000795	0.000339
	(0.000527)	(0.000376)	(0.000396)	(0.000568)	(0.00176)
First Child	0.100*	-0.00454	-0.00238	0.00743	0.339**
	(0.0551)	(0.0434)	(0.0423)	(0.0488)	(0.169)
Second Child	0.0619	-0.0203	0.0226	-0.0305	0.0733
	(0.0523)	(0.0485)	(0.0510)	(0.0522)	(0.192)
Mother HS Graduate	0.121***	0.0531*	0.0663**	0.0801**	-0.0462
	(0.0404)	(0.0308)	(0.0303)	(0.0386)	(0.163)
Mother College Graduate	0.0295	0.0740*	-0.00319	0.0623	-0.229
	(0.0407)	(0.0391)	(0.0347)	(0.0427)	(0.164)
Father HS Graduate	0.0535	0.0939*	-0.0911**	-0.00417	0.163
	(0.0538)	(0.0476)	(0.0452)	(0.0596)	(0.191)
Father College Graduate	0.161***	0.0539	0.0199	0.0413	0.262
	(0.0457)	(0.0466)	(0.0355)	(0.0438)	(0.211)
Cohort controls	Yes	Yes	Yes	Yes	Yes
Race Controls	Yes	Yes	Yes	Yes	Yes
Observations	3382	3382	3382	3382	3580
R^2	0.083	0.137	0.071	0.120	0.123
Dep Var Mean	0.450	0.327	0.959	0.947	4.703

Note: See notes to Table 2.2

Finally, we examine our last measure of parents' investment: **health care investment**. To measure this, we use three questions from Wave I about medical care over the prior year: "In the past year, have you had a routine physical examination?" (answers are 0=no or 1=yes). We create a dummy variable that equals 1 if an individual had a physical in the

Table 2.7: Mechanisms: Parents' Financial Investment, Females

	(1) Received Allowance during Adolescence	(2) Parent Helped Financially during Adulthood
Next Youngest is Brother	-0.0260	-0.00591
	(0.0251)	(0.0190)
Mother's Age	-0.00247	0.0498**
	(0.0357)	(0.0238)
Mother's Age Squared	0.0000350	-0.000516*
	(0.000439)	(0.000298)
First Child	0.164***	0.110***
	(0.0418)	(0.0353)
Second Child	0.0903**	0.0792**
	(0.0436)	(0.0364)
Mother HS Graduate	0.0118	0.0350
	(0.0348)	(0.0214)
Mother College Graduate	0.0768**	0.0978***
	(0.0296)	(0.0287)
Father HS Graduate	0.0386	0.0551**
	(0.0420)	(0.0261)
Father College Graduate	0.0450	0.123***
	(0.0342)	(0.0268)
Cohort controls	Yes	Yes
Race Controls	Yes	Yes
Observations	3554	3600
R^2	0.076	0.114
Dep Var Mean	0.459	0.241

Note: See notes to Table 2.2

last year, labeled "Routine Physical." Next, we use the question: "When did you last have a dental examination by a dentist or hygienist?" (answers range from 1=less than a year ago to 4=never), and create a dummy equal to 1 if the individual had a dental examination less than a year ago, labeled "routine dental." Finally we use the question, "Has there been any time over the past year when you thought you should get medical care, but you did not?" (answers are 0=no 1=yes). We create a dummy equal to 1 if the individual always received medical care when needed, labeled "Medical Care Received".[20] In Table 2.8, we examine whether health care investment varies by sibling gender. As shown in columns (1)-(3), we find no significant evidence of a difference in these basic items for health care. We again

[20]Although Add Health has extensive information on specific medical conditions, we do not use this information because parents' investment could affect the likelihood of being diagnosed as well as the likelihood of developing a condition.

interpret this as suggestive evidence that differences in health care investment do not drive our results.[21]

Table 2.8: Mechanisms: Parents' Health Care Investment, Females

	(1) Routine Physical	(2) Routine Dental	(3) Medical Care Received
Next Youngest is Brother	0.0221	-0.0117	-0.00572
	(0.0183)	(0.0195)	(0.0175)
Mother's Age	0.0255	0.0855***	-0.0401*
	(0.0350)	(0.0253)	(0.0236)
Mother's Age Squared	-0.000309	-0.00100***	0.000494*
	(0.000428)	(0.000316)	(0.000287)
First Child	0.0891*	0.105***	0.00392
	(0.0466)	(0.0397)	(0.0287)
Second Child	0.0611	0.0713*	-0.0111
	(0.0461)	(0.0393)	(0.0287)
Mother HS Graduate	-0.0239	0.0573**	0.0418
	(0.0276)	(0.0283)	(0.0277)
Mother College Graduate	0.0387	0.00482	-0.0128
	(0.0378)	(0.0267)	(0.0326)
Father HS Graduate	0.0842**	0.0512	0.00342
	(0.0384)	(0.0364)	(0.0289)
Father College Graduate	0.0154	0.103***	0.0144
	(0.0324)	(0.0268)	(0.0220)
Cohort controls	Yes	Yes	Yes
Race Controls	Yes	Yes	Yes
Observations	3579	3589	3599
R^2	0.016	0.070	0.043
Dep Var Mean	0.662	0.683	0.793

Note: See notes to Table 2.2

Overall, our results on parental investment provide suggestive evidence that parents display less interest in students' academic performance and lower expectations for academic

[21]Since medical care may be more constrained in lower income families, we also test whether health care investment varies by the gender of the next-youngest child for the subsample of low-income families only (those reporting family income at or below $37,000 per year in 1994, the median of the sample). Even in this subsample, we find no evidence that health care investment varies significantly by the gender of the next-youngest sibling. Results are available in the Web Appendix.

performance in the presence of a younger brother. We do not find evidence of other parental investment mechanisms we test at work. However, our measures (particularly those of financial and health care investment) are limited, and it is possible that there are differences in forms of parental investment we cannot observe.

2.5.3 Gender Specialization

As highlighted in Section 2 above, siblings may increase specialization in traditionally sex-typed activities and interests. This may come from siblings' own desire to differentiate themselves, or from gendered parenting (i.e. girls with brothers spend more time with mothers relative to fathers, leading them to develop more traditionally feminine interests). While Add Health does not ask detailed questions about preferences for many gender-specific tasks (such as housework) in adolescence, one can gain insight into gender specialization by looking at the time spent with the same-gender versus opposite-gender parent. To examine time with same versus opposite-gender parent, we use the questions about activities with parents from Table 2.6 (parental time investment) described above. We sum up the total activities done with mother and the total activities done with father [from those used in columns (1)-(4) of Table 2.6]. We create a variable equal to total number of activities with mother, and one for the total number of activities with father (each ranging from 0 to 4).

We further explore gender specialization by using questions on attitudes toward family/household tasks during Wave IV. Specifically, we use the following questions: "(In the past 12 months/Since you started your current job/In the last year of your most recent job), how often on your primary job (have you had/did you have) to cut back your hours or turn down overtime because of your family responsibilities?" [answers range from 1=frequently or sometimes to 4=never]. We create a dummy variable equal to 1 if the answer to this question is "frequently or sometimes" (and 0 otherwise), and the variable is labeled "Family

Interruptions to Work." To further examine traditional gender attitudes, we also use a question on total number of children intended: "Including any children you may already have, how many children, in total, do you intend to have?" (answers range from 0 to 9). We use the answer to this question as the variable "Number of Children Intended".

Table 2.9 examines whether gender specialization (as measured by these variables) is associated with the gender of the next-youngest sibling. As shown in column (1), total activities with mother is positively and significantly affected by a next-youngest brother; total activities with father [column (2)] is not significantly impacted. The results in column (4) also indicate that those with a next-youngest brothers display greater orientation toward traditionally feminine activities and tasks as indicated by preferences for children.

Table 2.9: Mechanisms: Gender Task Specialization, Females

	(1) Total Activities with Mother	(2) Total Activities with Father	(3) Family Interruptions to Work	(4) Number of Children Intended
Next Youngest is Brother	0.105***	0.0523	0.0295	0.163**
	(0.0396)	(0.0319)	(0.0192)	(0.0780)
Mother's Age	0.0606	0.0257	-0.00970	0.0493
	(0.0464)	(0.0438)	(0.0241)	(0.0907)
Mother's Age Squared	-0.000810	-0.000478	0.0000388	-0.000609
	(0.000577)	(0.000547)	(0.000301)	(0.00113)
First Child	0.0298	0.0514	-0.0348	0.0142
	(0.0880)	(0.0676)	(0.0391)	(0.125)
Second Child	-0.0167	0.0429	-0.0322	-0.0452
	(0.0838)	(0.0853)	(0.0378)	(0.130)
Mother HS Graduate	0.196***	0.104**	-0.0677**	-0.102
	(0.0478)	(0.0477)	(0.0304)	(0.0963)
Mother College Graduate	0.124**	0.0504	-0.0280	0.139
	(0.0591)	(0.0590)	(0.0257)	(0.115)
Father HS Graduate	-0.0550	0.132*	-0.0320	0.00232
	(0.0597)	(0.0711)	(0.0314)	(0.101)
Father College Graduate	0.0964*	0.153**	-0.0337	-0.00830
	(0.0543)	(0.0613)	(0.0314)	(0.0984)
Cohort controls	Yes	Yes	Yes	Yes
Race Controls	Yes	Yes	Yes	Yes
Observations	3507	3473	2986	3032
R^2	0.052	0.276	0.038	0.013
Dep Var Mean	1.897	0.802	0.213	2.403

Note: See notes to Table 2.2

2.5.4 Disruptive Behavior

The final mechanism we consider is that of disruptive behavior (i.e. delinquency and substance use). Add Health asks numerous questions about adolescents' risky behavior. For each behavior listed in the questions below, we create a dummy variable equal to 1 if the individual has ever engaged in the behavior (and 0 otherwise). We use the following questions: "Since school started this year/During the 1994-1995 school year, how often have you had trouble getting along with your teachers?" (answers range from 0=never to 4=everyday), labeled "Trouble with Teachers"; "Do you ever drink beer, wine, or liquor when you are not with your parents or other adults in your family?" (answers are 1=yes and 0=no), labeled "Alcohol"; "Have you ever tried cigarette smoking, even just 1 or 2 puffs?" (answers are 1=yes and 0=no); " During your life, how many times have you used marijuana?" (answers range from 0 to 950), labeled "Marijuana". The Add Health survey also contains questions pertaining to criminal activity, ranging from petty crimes to serious offenses. We use the following questions about property damage or graffiti: "In the past 12 months, how often did you deliberately damage property that didn't belong to you?" (answers range from 0=never to 3=5 or more times); and "In the past 12 months, how often did you paint graffiti or signs on someone else's property or in a public place?" (answers range from 0=never to 3=5 or more times). We create a dummy variable equal to 1 if the individual has engaged in either behavior, labeled "Damage or Graffiti." We also use two questions about theft: "In the past 12 months, how often did you steal something worth more than $50?" (answers range from 0=never to 3=5 or more times); and "How often did you steal something worth less than $50?" (answers range from 0=never to 3=5 or more times). We create a dummy variable equal to 1 if the individual reports stealing any item (regardless of price), labeled "Steal".

In Table 2.10, we test whether these behaviors are related to the gender of the next-youngest sibling. We find suggestive evidence that the presence of younger brothers may be associated with more disruptive behavior. The coefficients on marijuana use, alcohol use, and trouble with teachers are significant and positive. This suggests there is a positive impact of having a younger brother on disruptive behavior (as measured by these questions).

Table 2.10: Mechanisms: Disruptive Behaviors, Females

	(1) Trouble with Teachers	(2) Alcohol	(3) Cigarettes	(4) Marijuana	(5) Damage or Graffiti	(6) Steal
Next Youngest is Brother	0.0392*	0.0395*	0.00520	0.0399**	0.00808	0.0119
	(0.0207)	(0.0236)	(0.0193)	(0.0198)	(0.0183)	(0.0175)
Mother's Age	-0.0688***	0.00149	-0.0461	-0.0206	-0.0137	0.00512
	(0.0262)	(0.0331)	(0.0299)	(0.0226)	(0.0228)	(0.0214)
Mother's Age Squared	0.000786**	0.00000967	0.000487	0.000232	0.000143	-0.0000650
	(0.000336)	(0.000423)	(0.000377)	(0.000283)	(0.000270)	(0.000264)
First Child	-0.0522	0.000493	-0.0469	-0.0162	0.0246	0.00184
	(0.0440)	(0.0368)	(0.0394)	(0.0340)	(0.0270)	(0.0357)
Second Child	0.00679	0.0515	-0.00217	0.0343	0.0691**	-0.00613
	(0.0458)	(0.0450)	(0.0398)	(0.0413)	(0.0269)	(0.0354)
Mother HS Graduate	0.0103	-0.00507	0.0177	0.00107	-0.0239	-0.00194
	(0.0319)	(0.0337)	(0.0314)	(0.0268)	(0.0263)	(0.0251)
Mother College Graduate	0.00221	-0.0373	-0.0160	-0.000355	-0.0187	-0.0207
	(0.0277)	(0.0284)	(0.0278)	(0.0243)	(0.0181)	(0.0216)
Father HS Graduate	-0.0541	0.00109	-0.0900**	0.0211	0.00466	-0.0353
	(0.0445)	(0.0349)	(0.0370)	(0.0297)	(0.0274)	(0.0345)
Father College Graduate	0.00648	-0.0370	-0.0611*	-0.0505*	0.0129	0.0196
	(0.0340)	(0.0373)	(0.0326)	(0.0285)	(0.0236)	(0.0255)
Cohort controls	Yes	Yes	Yes	Yes	Yes	Yes
Race Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3531	3580	3589	3569	3587	3585
R^2	0.032	0.101	0.064	0.089	0.026	0.026
Dep Var Mean	0.552	0.397	0.334	0.249	0.147	0.172

Note: See notes to Table 2.2

2.5.5 Testing the Mechanisms

We now test the relative strength of the mechanisms in explaining the earnings penalty from brothers.

To reduce the dimensionality of the data for each of the mechanisms described above, we use principal component analysis (PCA) to create indexes of expectations, school monitoring, time investment, financial investment, health care investment, gender specialization, and disruptive behavior.[22] Details on the composition of each of the indexes can be found in the Web Appendix.

We test the strength of the various mechanisms by adding them one at a time into our main earnings regression. Table 2.11 displays the results. We begin in column (1) by repeating our main regression [column (3) of Table 2.2] for the subsample of individuals with non-missing information on the variables used to construct our indexes of mechanisms. In columns (2)-(8), we then add our indexes to the regression one at a time. Table 2.11 shows that parental expectations and gender specialization appear to explain the largest portion of the earnings penalty. When adding the different indexes jointly in a horse race [column (9)], the expectations and gender specialization mechanisms appear most potent.[23] Taken as a whole, our evidence seems to indicate that brothers may primarily reduce women's earnings by lowering investment in the form of expectations and encouraging greater gender specialization.

[22] PCA is an orthogonal transformation that converts a set of correlated variables into a fewer number of orthogonal variables (each called a principal component). The first principal components, our indexes, account for the most variance in the data. Because we do not have multiple measures of parental expectations, we use just the variable displayed in column (3) of Table 2.5 as our measure of parental expectations.

[23] To investigate whether these results are driven by multicollineary, we examine the correlation between our indexes. Of the seven indexes, the largest positive correlation between any two given indexes is 0.22 (correlation between time and financial investments), and the greatest negative correlation is -0.24 (correlation between time investment and disruptive behavior). This suggests that our results are not due to multicollineary. Results are available in the Web Appendix.

Table 2.11: Testing the Mechanisms: Earnings in Adulthood, Females

	(1) Log Earnings	(2) Log Earnings	(3) Log Earnings	(4) Log Earnings	(5) Log Earnings	(6) Log Earnings	(7) Log Earnings	(8) Log Earnings	(9) Log Earnings
Next Youngest is Brother	-0.0901**	-0.0824**	-0.0817**	-0.0932**	-0.0901**	-0.0871**	-0.0785*	-0.0897**	-0.0645
	(0.0398)	(0.0394)	(0.0396)	(0.0401)	(0.0398)	(0.0400)	(0.0404)	(0.0400)	(0.0406)
Mother's Age	0.0995*	0.0964	0.103*	0.0976*	0.0995*	0.0943	0.104*	0.0969*	0.0997*
	(0.0584)	(0.0582)	(0.0579)	(0.0584)	(0.0582)	(0.0582)	(0.0569)	(0.0579)	(0.0560)
Mother's Age Squared	-0.00109	-0.00105	-0.00114	-0.00107	-0.00109	-0.00104	-0.00116	-0.00106	-0.00110
	(0.000733)	(0.000732)	(0.000730)	(0.000735)	(0.000732)	(0.000731)	(0.000718)	(0.000728)	(0.000708)
First Child	0.00633	0.00892	-0.0170	0.00448	0.00635	-0.00988	0.000493	0.00660	-0.0325
	(0.0697)	(0.0680)	(0.0678)	(0.0698)	(0.0696)	(0.0678)	(0.0682)	(0.0677)	(0.0640)
Second Child	-0.0651	-0.0684	-0.0796	-0.0653	-0.0651	-0.0781	-0.0673	-0.0590	-0.0917
	(0.0779)	(0.0762)	(0.0758)	(0.0771)	(0.0778)	(0.0757)	(0.0762)	(0.0757)	(0.0708)
Mother HS Graduate	0.218***	0.208***	0.222***	0.207***	0.218***	0.214***	0.196***	0.215***	0.183***
	(0.0699)	(0.0696)	(0.0687)	(0.0711)	(0.0700)	(0.0700)	(0.0696)	(0.0687)	(0.0692)
Mother College Graduate	0.104*	0.101*	0.0812	0.105*	0.104*	0.0953	0.111*	0.102*	0.0815
	(0.0598)	(0.0603)	(0.0602)	(0.0594)	(0.0600)	(0.0605)	(0.0595)	(0.0601)	(0.0609)
Father HS Graduate	0.221***	0.225***	0.227***	0.220***	0.221***	0.217***	0.208**	0.221***	0.211**
	(0.0775)	(0.0776)	(0.0786)	(0.0773)	(0.0777)	(0.0773)	(0.0796)	(0.0772)	(0.0810)
Father College Graduate	0.133	0.130	0.109	0.121	0.133	0.114	0.122	0.128	0.0743
	(0.0908)	(0.0902)	(0.0910)	(0.0913)	(0.0894)	(0.0899)	(0.0878)	(0.0900)	(0.0856)
School Monitoring Index		0.0489*							0.0360
		(0.0257)							(0.0255)
Parental Expectations			0.151***						0.137***
			(0.0398)						(0.0415)
Time Investment Index				0.0336					0.0185
				(0.0262)					(0.0271)
Health Investment Index					-0.000129				-0.00314
					(0.0234)				(0.0231)
Financial Investment Index						0.0463**			0.0387*
						(0.0222)			(0.0226)
Gender Specialization Index							-0.0876***		-0.0858***
							(0.0256)		(0.0252)
Disruptive Behavior Index								-0.0350	-0.0122
								(0.0249)	(0.0242)
Cohort controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Race Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1785	1785	1785	1785	1785	1785	1785	1785	1785
R^2	0.116	0.120	0.125	0.117	0.116	0.119	0.128	0.118	0.142

Note: See notes to Table 2.2

2.6 Additional Evidence

In this Section, we extend our evidence as follows. We first examine results when focusing on the sample of first born children only. We then consider as our main explanatory variable the total number of brothers in the household, rather than just the next-youngest brother. Finally, we discuss the results obtained when using an even more recent cohort of women in the National Longitudinal Study of Youth 1979 Child and Young Adult (NLSY-CYA) data.

As noted above, to achieve identification, we focus our analysis on the gender of the next-youngest sibling. There are two potential downsides to using this independent variable. First, women from larger families are more likely to be in our sample (because they are more likely to have a younger sibling). Second, our estimate is only for a next-youngest brother, and an older brother or second-youngest brother may affect women differently than a next-youngest brother. We perform two sets of regressions to address these concerns. First, we address the concern that individuals from larger families are more likely to be in our sample. To show that our results hold when we pull equally from all families with at least 2 children, we perform regressions restricting our sample to first born children with at least one younger sibling. In Table 2.12, we repeat Table 2.2 for the sample of female oldest children. As shown in columns (1)-(3), a next-youngest brother continues to exert a negative impact on women's earnings. The magnitude of the estimate is larger, but the standard errors are also larger (likely due to a decrease in sample size).[24]

Second, we examine the associations between having any additional brother in the household and earnings. The identification strategy used in this paper only allows us to obtain a causal effect of having a younger brother versus a younger sister. However, the

[24] In Web Appendix Table A.1.4, we test for the presence of heterogeneity of results by birth order. We interact the dummies for first born, second born, and third born and higher with the dummy for whether the next-youngest sibling is a brother. We then test whether these interactions are statistically different from each other with F-tests for the equality of coefficients. As shown at the base of the table, we do not find evidence of significant differences of the effect of a next-youngest brother by birth order.

Table 2.12: Earnings in Adulthood, Firstborn Females

	(1) Log Earnings	(2) Log Earnings	(3) Log Earnings
Next Youngest is Brother	-0.0857*	-0.0861*	-0.0865*
	(0.0449)	(0.0444)	(0.0445)
Mother's Age	0.190**	0.120	0.109
	(0.0782)	(0.0814)	(0.0786)
Mother's Age Squared	-0.00209**	-0.00135	-0.00122
	(0.000991)	(0.00103)	(0.000997)
Mother HS Graduate		0.294***	0.267***
		(0.0714)	(0.0751)
Mother College Graduate		0.192***	0.141**
		(0.0618)	(0.0620)
Father HS Graduate			0.132
			(0.0840)
Father College Graduate			0.127
			(0.0773)
Cohort controls	Yes	Yes	Yes
Race Controls	Yes	Yes	Yes
Observations	1613	1613	1613
R^2	0.095	0.126	0.133

Note: See notes to Table 2.2

literature indicates that both older and younger siblings influence each other's behaviors and attitudes (Whiteman and Christiansen, 2008; Whiteman, Becerra, and Killoren, 2009; Whiteman, Jensen, and McHale, 2017).[25] We repeat our main regressions using as the independent variable the total number of brothers in the household. This allows our sample to include those without any younger siblings. The results are shown in Table 2.13.[26] They indicate that an additional brother is associated with an earnings decline of roughly 8 percent.

[25]The literature indicates that, while older siblings may be more influential in childhood and early adolescence when they have greater responsibilities and ability than their younger siblings, sibling relationships become more egalitarian and younger siblings become more likely to influence older siblings in adolescence (Buhrmester and Furman, 1990; Buhrmester, 1992).

[26]In these regressions, we do not control for total number of siblings since fertility choices may be endogenous (see Section 2). However, the magnitude and significance of our results is unchanged if we control for the total number of siblings.

Although this regression cannot fully account for the endogeneity of fertility choices, it provides suggestive evidence that any additional brother, not just a next-youngest brother, may lower earnings.

Table 2.13: Total Number of Brothers and Earnings in Adulthood, Females

	(1) Log Earnings	(2) Log Earnings	(3) Log Earnings
Total Number of Brothers	-0.0906***	-0.0816***	-0.0833***
	(0.0242)	(0.0241)	(0.0242)
Mother's Age	0.150***	0.0967**	0.0867**
	(0.0401)	(0.0417)	(0.0416)
Mother's Age Squared	-0.00160***	-0.00104**	-0.000934*
	(0.000487)	(0.000503)	(0.000502)
First Child	0.0878*	0.0340	0.0215
	(0.0483)	(0.0464)	(0.0482)
Second Child	0.0547	0.0200	0.00762
	(0.0550)	(0.0518)	(0.0528)
Mother HS Graduate		0.227***	0.187***
		(0.0546)	(0.0577)
Mother College Graduate		0.185***	0.122**
		(0.0457)	(0.0491)
Father HS Graduate			0.174***
			(0.0661)
Father College Graduate			0.138**
			(0.0690)
Cohort controls	Yes	Yes	Yes
Race Controls	Yes	Yes	Yes
Observations	3248	3248	3248
R^2	0.072	0.096	0.107

Note: See notes to Table 2.2

Finally, we also examine the effect of the next-youngest brother in the NLSY-CYA survey, which includes women mostly born in the 1980s and 1990s. Although the sample size is small, we find a qualitatively similar estimate of the effect of the next-youngest brother on earnings which is higher in magnitude than our results in Table 2.2.[27]

2.7 Conclusion

This paper provides the first estimates of the impact of sibling gender composition on women's earnings for recent U.S. cohorts and provides a first step at examining the mechanisms through which this effect may occur. In particular, we find that the presence of a next-youngest brother lowers earnings by approximately 7 percent for a woman in her late 20s or early 30s. The magnitude of our results for both men and women is somewhat larger than the ones obtained in studies using data from northern Europe, which have indicated that brothers decrease women's earnings by about 2 percent. However, the United States as a whole displays greater earnings dispersion than European countries, and we examine more recent cohorts, where a greater fraction of women may be working.

The unique contribution of this paper is to provide a first investigation of the mechanisms that may underlie the effect of sibling gender on earnings. We find evidence that females experience lower parental expectations and adopt more traditional attitudes and behaviors toward gender in the presence of brothers. Our results help advance the literature on the continuing earnings gap between men and women, suggesting that both parents' attitudes toward their children and gendered behavior/family responsibilities are important in shaping women's earnings. Future research could explore in more detail the interplay between interactions with parents and direct interactions with siblings in adolescence (akin to peer effects) in shaping choices later on in life.

[27] We also examine the earlier cohort represented in the NLSY79. Consistent with earlier work, we find no impact of sibling gender composition on earnings for women in this survey. However, we do find that women with a next-youngest brother (or any additional brother) are significantly less likely to be in the labor force (by about 3-5 percentage points) in most survey years between 1986 and 2012. We suspect that the mechanisms may be operating along the extensive margin for these earlier cohorts (born before 1970). Results using both the NLSY79 and NLSY-CYA data are available upon request.

CHAPTER 3

GIRLS, BOYS, AND HIGH ACHIEVERS

Angela Cools, Raquel Fernández, and Eleonora Patacchini

3.1 Introduction

The gender composition of a class, or of a group of competitors, or of a team has been shown to affect both individual and group outcomes. These findings point to potentially important consequences for issues ranging from effective teaching, to the optimal structuring of teams, or to how best design evaluations in a variety of environments. In this paper we attempt to move beyond the question of gender per se and instead focus on investigating a particular characteristic: "high-achievers" of a given gender, which is the term we use to refer to students with very highly-educated parents. Does greater exposure to "high-achievers" of the same or different gender matter? To whom does it matter? And why?

We investigate these questions in the context of high-school education using the National Longitudinal Survey of Adolescent to Adult Health (Add Health) which was designed to be a nationally representative sample of students in grades 7-12 in the US. We make use of a predetermined student characteristic – whether at least one of their parents has some post-college education – to proxy for a bundle of student characteristics. Aggregating this number across students at the grade level by gender allows us to use plausibly exogenous variation across grades within the same school in the proportions of "high achievers" of each gender. Our main focus is on the longer-run education effects of this variation. We find a very strong asymmetric gender effect: the proportion of "high-achieving" boys has a statistically and economically significant negative effect on the probability that girls will end up with a bachelor's degree some 14 years later. There is no similar asymmetric gender effect in the proportion of “high-achieving” girls: a greater proportion of these does not affect outcomes for either gender and, furthermore, boys are not affected by the proportion of male “high-achievers.” These results are robust to a wide variety of controls and alternative specifications, including the proportion of females in the class, the rank of the student, and the proportion of students of different races/ethnicities.

We investigate potential heterogeneity and non-linear effects. Performing various cuts of the data, we find that the negative effect of “high-achieving” boys on girls is concentrated in the lower half of the ability distribution (as measured by a student’s Peabody Picture Vocabulary Test score), among those with a (at least) college-educated parent, and in the upper half of the socio-economic distribution of schools (as measured by the fraction of students that are performing at or above grade level). We show that girls exposed to a higher proportion of boys with highly-educated parents tend to have a lower math and science grades in high school and to substitute away from a four-year college degree into a two-year college. Furthermore, they have lower labor force participation and higher fertility by the ages of 26-32.

We are especially interested in understanding the mechanisms that drive the asymmetric gender results. Using questions in Add Health administered in the baseline year, we show that a larger proportion of “high-achieving” boys is associated with lower self-confidence/ambition in girls, an increase in their risky behavior, and a higher chance of becoming a mother before age 18. The data does not permit us to identify the exact mechanism by which this occurs. Although we call these students (and their parents) “high achievers” a complex mix of their characteristics, and responses these, could be responsible for the outcomes. For example, it could be that teachers pay less attention to girls when faced with boys who perform well in school. Alternatively, it could be that girls are more likely to feel discouraged in the face of competition from boys. Nor can we rule out that their parents interact differently with schools, although this must occur in such a way that is detrimental to girls. Thus, although we cannot isolate different potential mechanisms, we do provide suggestive evidence regarding the pathways at work as they affect girls' propensity to engage in risky behavior and reduce their aspirations.

Our analysis relies on variation in the proportions of boys and girls that are "high-achievers" across grades within the same school. As the baseline grade-level data is a "snapshot" of a school (grades) in 1994, we also conduct our analysis with an alternative definition of the main variable – the number of high achievers – as the proportion may vary in a systematic fashion due to dropouts. We show the results are robust to this alternative specification. In addition to a school-specific time trend, the paper conducts a variety of checks to make sure that the variation obtained is "as good as random." As in Lavy and Schlosser (2011), we conduct Monte Carlo simulations in which we randomly generate the post-college status of each student's parents and compare the simulated vs empirical standard deviation in each school. We also show that variation in the key peer explanatory variables is not related, within a school, to a number of important individual characteristics. Lastly, as suggested by Athey and Imbens (2017), we examine the extent to which the results could have been obtained by chance by reassigning to each individual the proportion of "high-achieving" peers from another grade within the same school, keeping all other variables as in the data and then comparing the results of these placebo tests with the estimated treatment coefficients.

Our paper is related to a growing literature on asymmetric gender effects in a variety of contexts. Asymmetric gender effects have been identified, for example, in a series of experiments by Niederle, Segal, and Vesterlund (2013). They show that in addition to female subjects being less likely to enter competitive situations (tournaments) than male subjects, the gender composition of the other competitors matters. Women are markedly more likely to participate when the competition consists solely of other women. Bordalo, Coffman, Gennaioli, and Shleifer (2018) show that, controlling for ability, a female subject's belief about the probability she answered a question correctly is more affected by the gender stereotype about the category in which a question is asked (e.g., cars and sports vs cooking and art) than a male subject's. Furthermore, when subjects play a cooperative game with a partner in which each needs to decide how willing they are to answer for the group, female subjects' beliefs become even more stereotyped if their partner is known to be male, leading them to decrease the probability with which they are answer and reducing the overall performance of their group. Our results suggest that the decline in self-confidence may be accentuated when faced with a signal of individual performance, a conjecture that would be of interest to confirm in the lab.

In the school setting there is also evidence that gender composition matters. The literature in this area, like ours, has mainly relied on quasi-random variation across cohorts or grades within a school to study topics ranging from the effect of immigrant peers in 5th grade on high-school outcomes in Israel (Gould, Lavy, and Paserman, 2009), to gender roles and their intergenerational perpetuation (Olivetti et al., 2018; Rodríguez-Planas, Sanz-de Galdeano, and Terskaya, 2018) to the long-run educational and labor-market consequences of disruptive peers in elementary school in Florida (Carrell, Hoekstra, and Kuka, 2018).[1] Hoxby (2000b) exploits idiosyncratic variation in gender and race composition of adjacent cohorts in Texas public schools and finds that a greater share of female peers improves reading and mathematics test scores for both genders. Using Israeli data and relying on variations in the proportion of female students across adjacent cohorts within the same school, Lavy and Schlosser (2011) find that a greater proportion of girls is associated with positive high-school outcomes for both sexes. On the other hand, Black, Devereux, and Salvanes (2013), using Norwegian administrative data, find that there are asymmetric gender effects: a larger proportion of females among ninth-grade peers reduces males' long-run educational attainment whereas it decreases women's rates of becoming a teenage mother and increases the likelihood that as adults they work full time and their earnings. As the studies are based in different countries and use different specifications, the difference in results could stem from

[1] See the excellent review of the literature in Handbook of Education chapter by Sacerdote (2011).

a variety of sources including short vs longer-run effects.

The papers closest to ours are Mouganie and Wang (2017) and Feld and Zölitz (2018) as they too are concerned with both the gender of a peer and distinguishing its effect by gender as well.[2] Mouganie and Wang (2017) study high-school students in China and find that high-performing male peers (defined by their performance on a national exam in mathematics prior to their entry in high school which occurs in 10th grade) reduce women's likelihood of choosing a science track (relative to an arts track) for the remainder of high school whereas high-performing female peers have the opposite effect. Feld and Zölitz (2018) exploit the random assignment of first-year students within compulsory courses to teaching sections in a Dutch business school. They show that having male peers with higher pre-assignment GPA is associated with men taking more mathematical courses. Women, on the other hand, choose to take fewer mathematical courses and are less likely to choose a mathematically intensive major. Our analysis adds to these finding by showing that greater exposure to "high-achieving" males not only influences women's fields of study but also their overall educational attainment. It has the advantage not only of being nationally representative but also of showing that these effects are already present in high school and have long-term consequences. Furthermore, and perhaps most importantly, the nature of our data set allows us to explore some of the pathways by which these effects occur (self-confidence and risk) and identify the characteristics of those girls who are most likely to be negatively affected.

[2]How high or low performing students affect various outcomes is the focus of a large literature. It does not, however, distinguish necessarily between male and female peers nor on their differential gender effects. The results of this literature are mixed. See, e.g., Imberman, Kugler, and Sacerdote (2012) that finds that high school students in the top of the student distribution benefit most from the arrival of high-performing peers (Katrina evacuees) and are the hurt by the arrival low-achieving peers whereas Angrist and Lang (2004) find no significant impact of low-performing students from the Metropolitan Council for Educational Opportunity desegregation program in Boston. Carrell, Fullerton, and West (2009) find that higher-ability peers at the US Air Force Academy provide greater positive peer effects for lower-ability students than for middle-ability students. Bifulco, Fletcher, Oh, and Ross (2014) study the effects of the greater exposure to school peers with a college-educated mother, also using Add Health data. Interestingly, they do not find any significant long-run effect on education but this may well be a result of not distinguishing between male and female peers with a college educated mother. Lastly, Fischer (2017) examines the impact of relatively high-achieving peers in an introductory chemistry classes at a large public university. She finds that being in a chemistry class with more high-ability peers decreases the likelihood that women complete a STEM degree whereas men are not affected. In this case as well, there is no differentiation in the gender of these peers.

Our paper proceeds as follows. In Section 3.2, we present the data and sample selection. In Sections 3.3 and 3.4, we detail the construction of the main variables and our identification strategy. Section 3.5 is devoted to the main regression analysis. We explore possible pathways for the effects in Section 3.6. Sections 3.7 explores heterogeneity in the results and examines additional long-term consequences. In Section 3.8 we perform several robustness checks and conclude in Section 3.9.

3.2 Data and Sample Selection

This analysis uses data from the National Longitudinal Survey of Adolescent to Adult Health (Add Health).[3] Add Health is a school-based longitudinal survey designed to be nationally representative of students in grades 7-12. It examines students at a representative set of 132 schools in the United States, beginning in the 1994-1995 school year.[4] Add Health contains both in-school and in-home survey components. First, between September 1994 and April 1995, an in-school survey was issued to students in each of the sample schools. Every student in attendance on the school's survey day was asked to complete an in-school questionnaire which included basic questions about the student's demographics (sex, age, race, nativity status) and information about the characteristics including educational attainment of a mother figure (biological mother, stepmother, foster mother, or adoptive mother) and

[3]This research uses data from Add Health, a program project directed by Kathleen Mullan Harris and designed by J. Richard Udry, Peter S. Bearman, and Kathleen Mullan Harris at the University of North Carolina at Chapel Hill, and funded by grant P01-HD31921 from the Eunice Kennedy Shriver National Institute of Child Health and Human Development, with cooperative funding from 23 other federal agencies and foundations. Special acknowledgment is due Ronald R. Rindfuss and Barbara Entwisle for assistance in the original design. Information on how to obtain the Add Health data files is available on the Add Health website (http://www.cpc.unc.edu/addhealth). No direct support was received from grant P01-HD31921 for this analysis.

[4]To select schools, Add Health used a stratified sampling design. High schools were chosen from strata based on the following characteristics: region, urbanicity, school type, ethnicity, and size. If a school refused to participate in the survey, another school from the same stratum was selected. Participating high schools then assisted in the identification of feeder schools, typically a middle school, whose students tend to attend the sample high schools.

father figure (biological father, stepfather, foster father, or adoptive father) living in the student's household (henceforth called "residential" parents). A total of about 90,000 students completed this in-school questionnaire.

Following the conclusion of the in-school surveys, Add Health randomly selected a subsample of 20,000 students from the roster of the sample schools for more detailed interviews in the in-home sample. Approximately 17 male and 17 female students from each grade level in each school were chosen for the core in-home sample.[5] The core was then supplemented with oversamples for particular populations of interest, defined by ethnicity, presence of siblings in the sample, adoption status, and disability. Interviews with the core and supplemental students took place between April and December 1995 in the students' homes (Wave I).[6] They included questions on parents' background for both residential parents (the mother and father figures living in the same household as the student) and biological parents.

Wave I also include questions on students' academic performance, attitudes, criminal behavior, and other sensitive topics. At the beginning of the Wave I survey, students were also asked to complete an abbreviated version of the Peabody Picture Vocabulary Test (PVT), a test widely used to measure verbal ability.[7] Interviewers also issued a survey to the student's residential parent, asking questions about attitudes and family income, among other topics.[8] Those individuals selected for the Wave I in-home sample were re-interviewed in 1996 (Wave II), 2001-2002 (Wave III), and 2008 (Wave IV). In these follow-up interviews,

[5]At 2 large schools and 14 small schools, in-home interviews were administered to all students to achieve a saturated sample.

[6]The majority (92 percent) of Wave I interviews for individuals in our sample took place between April 1995 and August 1995. For these students, Wave I responses and information on peer characteristics are from the same school year. For those interviewed between September and December, Wave I responses are from the following school year. We include, therefore, an indicator for this in any regression with a Wave I outcomes.

[7]The Add Health PVT is a condensed version of the Peabody Picture Vocabulary Test–Revised (PPVT), a standard assessment of verbal ability used in the United States. Scores are standardized by age to a mean of 100 and standard deviation of 15 for each age group, and neither the student nor the interviewer is made aware of the results of the test. For more information, see http://www.cpc.unc.edu/projects/addhealth/design/wave1.

[8]The interviewers attempted to interview the student's resident mother. If unavailable, they interviewed another adult in the household. Overall, 93 percent of parent interviews took place with a female parent.

individuals were asked questions about their living situation, health behaviors, daily activities, and, importantly, level of educational attainment to date. Our analysis uses both Wave I (in-school and in-home survey) and Wave IV information. All the information on school peers is obtained from the in-school survey. In addition to information on educational attainment, our data also provides rich information on behaviors and perceptions in adolescence, which enable us to inspect several mechanisms for our results. Throughout the analysis we use Wave I in-home information on residential rather than biological parents for consistency with the in-school sample (which only collects information on residential parents).

After dropping students who were not followed through Wave IV and those that cannot be matched with peer characteristics we also eliminated particular grades/schools (e.g., an all-male school, 7th and 8th graders from a school that doubles in size between 8th and 9th grade) the final sample consists of 10,853 students (5899 females and 4954 males) and 118 schools.[9] Summary statistics for the Wave IV-weighted sample are reported by sex in Table 1. On average, the students are almost 16 years old in July of 1995. About two-thirds of our sample is Non-Hispanic White, 15-17 percent is Black, 10 percent is Hispanic/Latino, and the remainder is Asian or other races. By the time of the Wave IV survey (2008), the vast majority of students in our sample (94 percent of females and 91 percent of males) have achieved a high school diploma or GED, and about one third (35 percent of females and 28 percent of males) have completed a bachelor's degree. Data from the 2008 American Community Survey (ACS) that is re-weighted to match the age distribution of the Add Health sample is also presented in Table 3.1.[10] As shown, the Add Health population is broadly similar to the U.S. population as calculated from the ACS.

[9]See the Appendix for the exact details. The robustness section examines whether attrition is a function of the main variables introduced in the next section and concludes that this is not a problem.

[10]We exclude those from the ACS sample who immigrated to the United States after 1994 as immigrants since 1994 would not have been in the Add Health sample.

Table 3.1: Summary Statistics

	Add Health				2008 ACS			
	Females		Males		Females		Males	
	mean	sd	mean	sd	mean	sd	mean	sd
Age in Years (July 1995)	15.77	1.75	15.88	1.80				
Age in Years (July 2008)	28.77	1.75	28.88	1.80	28.77	1.75	28.88	1.80
White	0.65	0.48	0.67	0.47	0.67	0.47	0.67	0.47
Black	0.17	0.38	0.15	0.36	0.14	0.35	0.13	0.34
Latino	0.10	0.30	0.11	0.31	0.13	0.34	0.15	0.35
Asian	0.03	0.17	0.04	0.19	0.03	0.17	0.03	0.17
Other Race	0.05	0.21	0.04	0.20	0.03	0.16	0.03	0.16
Foreign Born	0.05	0.22	0.05	0.22	0.07	0.25	0.07	0.26
HS Graduate	0.94	0.24	0.91	0.29	0.91	0.28	0.88	0.33
Bachelor's Degree	0.35	0.48	0.28	0.45	0.33	0.47	0.26	0.44
Observations	5899		4954		153,269		148,470	

Note: This table reports summary statistics for the Add Health data sample used in the paper and the 2008 American Community Survey. The ACS sample excludes those who immigrated to the United States after 1994. ACS age in years is the average age in years of the sample with responses pooled over all survey months (ranging from January 2008 - December 2008) as birth dates are not reported. The ACS female and male samples are restricted to those aged 25-34 and re-weighted to match the age distribution of the Add Health female and male final samples, respectively. Wave IV weights are used in Add Health data.

3.3 The Main Variables

The objective of this paper is to study the impact of "high-achieving" girls and boys on the long-run educational outcomes of their peers. In order to do so, we require a variable that is plausibly exogenous to a student's experience. This prevents one from using grades as an outcomes as, for example, a high-ability student may encourage her peers to study harder and thus raise their grades. In this case of reverse causality, an association between peers' achievement and an individual's long-run outcomes may be due to the individual's own ability. We therefore turn to a measure that is determined before individuals meet and interact with their fellow students at school: residential parents' education.

3.3.1 Defining "High Achievers"

Parents' educational attainment is usually determined before students enter school and the literature finds a strong, positive relationship between an individual's academic achievement and the educational attainment of his or her parents.[11] We next document this link for individuals in our sample in two ways: via a student's GPA and via a student's ability as measured by their PVT score. It is important to reiterate here that parental education is not only a proxy for a student's achievement but is also correlated with a bundle of other characteristics. Thus, we cannot disentangle which features associated with parental education are playing a critical role. Nonetheless, we think it is useful to document the correlation between parental education and student achievement.

To measure achievement using a student's GPA, we average a student's self-reported grades in the four subjects that are asked (English, History, Mathematics, and Science) obtained during the Wave I interview.[12] Table 3.2 shows the results of an OLS regression in which the dependent variable is GPA and the main independent variables are indicators of residential parental education.[13] A residential parent's education is coded as less than high school, high school graduate (including a GED), some college if there is any education past high school that does not result in college graduation, college graduate, and post college if "any professional training beyond a four-year college or university." Students who do not know or refuse to answer are included via a dummy variable for the presence of a missing

[11] See, e.g., Davis-Kean (2005); Reardon (2011); and Van Tetering, de Groot, and Jolles (2018).

[12] For each subject, the student was asked to report the following grade categories: A, B, C, D or lower, subject was not graded that way, or did not take this subject. We set A=4.0, B=3.0, C=2.0, D or lower=1.0 and averaged across the four subjects. If the student reported that the subject was not graded in this manner or did not take this subject (approximately 10 percent of the students), we do not include that subject in the calculation of the student's GPA.

[13] Information on parental education is from the student's responses in the Wave I survey. Students were asked, for each residential parent, to select how far the parent went in their education: never went to school; eighth grade or less; more than eighth grade, but did not graduate from high school; went to a business, trade, or vocational school instead of high school; high school graduate; completed a GED; went to a business, trade, or vocational school after high school; went to college, but did not graduate; graduated from a college or university; professional training beyond a four-year college or university; doesn't know what level; doesn't know if he/she went to school.

value. If there is no residential mother (equivalently father), all education dummies are given a value of zero and we include a dummy variable for "mother (equivalently father) not in household."

The first four columns of Table 3.2 are for girls; the last four are equivalent specifications for boys. Columns (1)-(3) (respectively, (5)-(7) for males) include increasing number of individual and family controls – first only residential parental education categories (including a category for missing), then age, race, and family income, and lastly the student's PVT score as a control for individual ability.[14,15] All specifications include grade and school fixed-effects to control for different grading philosophies across schools and also across grades. Lastly, specification (4) (respectively, (8) for males) includes a school-specific linear time trend to account for trends in grading practices or student composition at the school level.

As shown, having highly-educated residential parents is associated with a higher GPA for both girls and boys. For girls, a mother with a post-college education – defined as any education past college graduation – is associated with a GPA that is about 0.3-0.5 points higher relative to the baseline group of a mother without a high school degree. A father with a post-college education is associated with a GPA that is 0.3-0.4 points higher relative to the baseline group of a father without a high school degree. The results are similar for boys: a mother with a post-college education is associated with a GPA that is 0.2-0.4 points higher than the baseline group whereas a father with a post-college education is associated with a GPA that is about 0.4-0.5 points higher than the baseline group. These are sizable

[14]Using information on the student's month and year of birth, we create a variable for age in months that reflects each student's age in July 1995. Race controls include dummies for Black, Latino, Asian, and other races. Students are classified as Latino if they report any Hispanic/Latino ethnicity. They are classified as other races if they report more than one race (White, Black, Asian) or being of another race outside of these three categories.

[15]The Add Health parent survey asks the following question to collect family income information: "About how much total income, before taxes did your family receive in 1994? Include your own income, the income of everyone else in your household, and income from welfare benefits, dividends, and all other sources." The number is given in thousands and thus multiplied by 1000 before inclusion in the regression. If family income is missing, we impute it at the average of that reported by parents of other Wave I in-home survey students at their school and include a dummy for missing family income.

effects: ceteris paribus, they increase a girl's GPA from a mean of 2.9 to 3.2-3.4 and a boy's GPA from a mean of 2.7 to 2.9-3.2.

Although we have included several individual controls in the regressions of student achievement, note that if what one is interested is how other students react to high achievers, it is not clear that these controls are relevant. That is, other students will not care necessarily if a student is a high-achiever after controlling for, say, family income. Nonetheless, it is of interest to note that the correlation between parental education and achievement exists both with and without controls.

We next consider whether a parent with a post-college education predicts achievement more strongly than any other levels of parental education. For both girls and boys, a post-college mother is quantitatively more important than simply college. A post-college rather than only-college father is also highly predictive for boys but not for girls. For the latter, a college versus a post-college father is associated with roughly the same increase in GPA. To show this rigorously, we perform F-tests for equality between the coefficients on parents with post-college education and parents with only a college education. We focus on two sets of coefficients: mother with college versus mother with post-college, and father with college versus father with post-college. The equality p values are shown at the bottom of the table. The values are under 0.05 for mother post-college versus mother college, and are under 0.01 for father post college versus father college for boys. Consequently, we define a student as a "high-achiever" if at least one of their parents has a post-college education.[16]

[16]Not only is this variable a stronger predictor of high achievement than college graduate but its interpretation is also clearer since the latter does not explicitly distinguish between a two and four-year college whereas a parent is only classifed as post-college if they have gone beyond a four-year college.

Table 3.2: Parental Education and Child GPA: In-Home Sample

	Dependent Variable: Grade Point Average							
	Females				Males			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mother HS Grad	0.136***	0.097**	0.074	0.069	0.138**	0.116**	0.091*	0.074
	(0.044)	(0.045)	(0.045)	(0.046)	(0.055)	(0.055)	(0.054)	(0.054)
Mother Some College	0.221***	0.170***	0.118**	0.108**	0.219***	0.181***	0.139**	0.133**
	(0.048)	(0.048)	(0.046)	(0.044)	(0.059)	(0.058)	(0.057)	(0.057)
Mother College Grad	0.319***	0.245***	0.187***	0.187***	0.247***	0.194***	0.149**	0.144**
	(0.052)	(0.053)	(0.050)	(0.049)	(0.066)	(0.066)	(0.065)	(0.065)
Mother Post College	0.488***	0.402***	0.291***	0.294***	0.381***	0.327***	0.265***	0.246***
	(0.065)	(0.066)	(0.059)	(0.060)	(0.077)	(0.077)	(0.072)	(0.071)
Mother Not in HH	0.053	0.039	0.049	0.059	-0.155*	-0.147*	-0.154*	-0.158*
	(0.060)	(0.061)	(0.062)	(0.064)	(0.082)	(0.078)	(0.083)	(0.086)
Father HS Grad	0.106**	0.073	0.055	0.074*	0.065	0.025	-0.001	0.009
	(0.047)	(0.047)	(0.046)	(0.043)	(0.064)	(0.057)	(0.060)	(0.059)
Father Some College	0.212***	0.166***	0.126**	0.152**	0.228***	0.175***	0.134**	0.130**
	(0.062)	(0.061)	(0.058)	(0.059)	(0.066)	(0.059)	(0.061)	(0.059)
Father College Grad	0.359***	0.302***	0.258***	0.266***	0.256***	0.209***	0.162**	0.158**
	(0.056)	(0.057)	(0.054)	(0.051)	(0.073)	(0.066)	(0.069)	(0.066)
Father Post College	0.370***	0.306***	0.260***	0.275***	0.486***	0.418***	0.382***	0.409***
	(0.066)	(0.064)	(0.064)	(0.063)	(0.077)	(0.075)	(0.080)	(0.078)
Father Not in HH	0.016	0.028	0.004	0.011	-0.042	-0.028	-0.057	-0.067
	(0.043)	(0.043)	(0.044)	(0.042)	(0.067)	(0.061)	(0.066)	(0.064)
Age in Months		-0.017***	-0.013***	-0.013***		-0.017***	-0.013***	-0.012***
		(0.002)	(0.002)	(0.002)		(0.002)	(0.003)	(0.003)
Foreign Born		0.233***	0.325***	0.308***		0.141*	0.202**	0.219**
		(0.071)	(0.065)	(0.068)		(0.080)	(0.091)	(0.088)
Log Family Income		0.038**	0.029*	0.026		0.039**	0.028	0.023
		(0.015)	(0.016)	(0.016)		(0.019)	(0.019)	(0.020)
PVT Score			1.204***	1.195***			0.945***	0.999***
			(0.132)	(0.137)			(0.135)	(0.133)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Race FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
School Linear TT	No	No	No	Yes	No	No	No	Yes
Observations	5788	5788	5548	5548	4851	4848	4611	4611
R^2	0.193	0.224	0.257	0.293	0.183	0.225	0.246	0.290
Equality P Value Moth Coll vs Moth Postcoll	0.001	0.001	0.017	0.013	0.013	0.016	0.021	0.049
Equality P Value Fath Coll vs Fath Postcoll	0.825	0.926	0.968	0.857	0.000	0.000	0.000	0.000

Note: This table reports parameter estimates and standard errors (in parentheses) from a regression of the student's grade point average on individual characteristics for the in-home sample. Grade point average is calculated based on self-reported student grades in math, science, english, and history from the Wave I in-home survey where A=4, B=3, C=2, and D or lower=1. All columns include dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Race fixed effects include dummies for Black, Latino, Asian, and other races. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Coefficient on PVT score multiplied by 100. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

Table 3.3: Parental Education and Child GPA: In-School Sample

	Dependent Variable: Grade Point Average					
	Females			Males		
	(1)	(2)	(3)	(4)	(5)	(6)
Mother HS Grad	0.125***	0.117***	0.115***	0.100***	0.094***	0.092***
	(0.016)	(0.015)	(0.015)	(0.019)	(0.019)	(0.019)
Mother Some College	0.232***	0.227***	0.225***	0.204***	0.201***	0.200***
	(0.019)	(0.018)	(0.018)	(0.023)	(0.023)	(0.022)
Mother College Grad	0.306***	0.284***	0.283***	0.270***	0.256***	0.253***
	(0.018)	(0.019)	(0.018)	(0.024)	(0.024)	(0.024)
Mother Post College	0.339***	0.318***	0.317***	0.324***	0.313***	0.309***
	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)
Mother Not in HH	-0.008	-0.016	-0.017	0.000	-0.002	-0.005
	(0.020)	(0.018)	(0.018)	(0.021)	(0.020)	(0.020)
Father HS Grad	0.066***	0.051***	0.049***	0.073***	0.066***	0.066***
	(0.018)	(0.018)	(0.018)	(0.020)	(0.020)	(0.020)
Father Some College	0.227***	0.208***	0.207***	0.205***	0.192***	0.195***
	(0.022)	(0.021)	(0.021)	(0.023)	(0.022)	(0.022)
Father College Grad	0.272***	0.240***	0.238***	0.299***	0.275***	0.279***
	(0.021)	(0.021)	(0.021)	(0.024)	(0.024)	(0.024)
Father Post College	0.381***	0.347***	0.344***	0.380***	0.354***	0.357***
	(0.028)	(0.027)	(0.027)	(0.029)	(0.028)	(0.028)
Father Not in HH	-0.079***	-0.063***	-0.063***	-0.058***	-0.041**	-0.039**
	(0.017)	(0.016)	(0.016)	(0.018)	(0.017)	(0.017)
Foreign Born		0.136***	0.135***		0.119***	0.119***
		(0.023)	(0.022)		(0.023)	(0.023)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Race FE	No	Yes	Yes	No	Yes	Yes
School Linear TT	No	No	Yes	No	No	Yes
Observations	39181	39181	39181	38021	38021	38021
R^2	0.171	0.190	0.201	0.160	0.175	0.182
Equality P Value Moth Coll vs Moth Postcoll	0.049	0.033	0.038	0.001	0.001	0.001
Equality P Value Fath Coll vs Fath Postcoll	0.000	0.000	0.000	0.000	0.000	0.000

Note: This table reports parameter estimates and standard errors (in parentheses) from a regression of the student's grade point average on individual characteristics for the in-school sample. Grade point average is calculated based on self-reported student grades in math, science, english, and history from the in-school survey where A=4, B=3, C=2, and D or lower=1. Race fixed effects include dummies for Black, Latino, Asian, and other races. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

Table 3.4: Parental Education and Child PVT: In-Home Sample

	Dependent Variable: PVT Score					
	Females			Males		
	(1)	(2)	(3)	(4)	(5)	(6)
Mother HS Grad	3.157***	1.925***	1.839**	4.558***	3.031***	2.970***
	(0.752)	(0.725)	(0.745)	(0.952)	(0.985)	(1.022)
Mother Some College	5.320***	3.855***	3.873***	7.326***	5.319***	5.427***
	(0.916)	(0.885)	(0.918)	(1.012)	(0.992)	(1.014)
Mother College Grad	6.227***	4.518***	4.713***	7.100***	4.968***	5.147***
	(0.833)	(0.830)	(0.832)	(1.125)	(1.094)	(1.130)
Mother Post College	9.426***	7.327***	7.607***	8.828***	6.495***	6.798***
	(1.122)	(1.100)	(1.134)	(1.433)	(1.380)	(1.424)
Mother Not in HH	0.269	-0.092	-0.307	2.015	0.982	1.498
	(1.497)	(1.470)	(1.516)	(1.456)	(1.414)	(1.510)
Father HS Grad	1.529*	0.802	0.663	2.459**	1.558	1.159
	(0.841)	(0.747)	(0.765)	(1.090)	(1.065)	(1.080)
Father Some College	4.014***	2.922***	2.578**	5.724***	4.311***	3.580***
	(1.106)	(1.034)	(1.062)	(1.314)	(1.246)	(1.285)
Father College Grad	4.545***	3.041***	2.935***	5.241***	3.808***	3.256**
	(0.965)	(0.924)	(0.924)	(1.410)	(1.275)	(1.288)
Father Post College	5.816***	4.143***	3.929***	6.951***	5.530***	4.990***
	(1.208)	(1.183)	(1.216)	(1.592)	(1.402)	(1.362)
Father Not in HH	1.128	1.366	1.250	1.757	1.974*	1.529
	(0.889)	(0.824)	(0.838)	(1.159)	(1.097)	(1.162)
Age in Months		-0.344***	-0.346***		-0.397***	-0.406***
		(0.042)	(0.043)		(0.036)	(0.037)
Foreign Born		-5.715***	-5.848***		-7.019***	-7.184***
		(1.769)	(1.770)		(1.353)	(1.346)
Log Family Income		0.641*	0.683**		0.813***	0.825***
		(0.329)	(0.336)		(0.270)	(0.301)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
Race FE	No	Yes	Yes	No	Yes	Yes
School Linear TT	No	No	Yes	No	No	Yes
Observations	5650	5650	5650	4703	4703	4703
R^2	0.293	0.351	0.378	0.265	0.354	0.392
Equality P Value Moth Coll vs Moth Postcoll	0.000	0.001	0.001	0.113	0.148	0.129
Equality P Value Fath Coll vs Fath Postcoll	0.255	0.325	0.385	0.088	0.061	0.060

Note: This table reports parameter estimates and standard errors (in parentheses) from a regression of the student's Peabody Picture Vocabulary Test (PVT) score on individual characteristics for the in-home sample. All columns include dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Race fixed effects include dummies for Black, Latino, Asian, and other races. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

We can repeat this exercise using a larger sample – the in-school sample – but in that case losing the information on household income and the student's PVT score. In Table 3.3 we examine the predictive power of parental education on student achievement for this sample. As can be seen from this table, the results are similar to those obtained for the in-home sample: either parent post-college is the quantitatively most important education level for a student's GPA.

As a final check, in Table 3.4, we return to our main sample and examine the relationship between a proxy for a student's ability – their PVT score – and parental education. The results are similar to the ones obtained for GPA. In this case, a girl with a post-college mother is associated with a PVT score that is 7-9 points higher (more than half a standard deviation of PVT scores as these are normalized to have a standard deviation of 15); a boy with a post-college father has a 5-7 point higher PVT score.

3.3.2 Construction of the Main Variables

As discussed in the previous section, we call a student a "high achiever" if at least one parent has a post-college education. Next, we construct a composite measure for the extent to which a student faces or interacts with this type of student. A natural measure to use for this is the *fraction* of students in the same grade who are "high achievers," i.e., who have residential post-college parents. We use the in-school survey which records the student's response to the highest level of education attained by their residential father and residential mother and create a dummy variable PC_i for student i that takes the value one if either the residential mother or the residential father of student i has a post-college education, i.e., obtained any education beyond a four-year college degree, and takes the value of 0 otherwise. If a student either does not have a residential father/mother or the information is missing, we impute that parent's level of education using the other parent's education. For example, if the residential father's education is missing, but the residential mother has a high-school education, we impute a value for father post-college by taking the average value of father post-college among students of the same gender and and in the same school who also have a residential mother with a high-school education.[17]

We measure the *fraction* of male and female high achievers separately. Specifically, we define the variables:

$$MF_{igs} = \frac{1}{n}\sum_{j(i)=1}^{n} PC_{jgs}, \qquad FF_{igs} = \frac{1}{q}\sum_{k(i)=1}^{q} PC_{kgs} \tag{3.1}$$

where $j(i) = 1, ..., n$ indexes student i's male peers and $k(i) = 1, ..., q$ indexes student i's female peers in grade g and school s. Thus, MF_{igs} (respectively, FF_{igs}) is the fraction of male peers (respectively, female peers) in the same school and grade as i with at least one post-college parent. Both MF_{igs} and FF_{igs} are the sample moments of the *leave-one-out* distribution of students with a post-college parent belonging to a specific gender, grade, and school. That is, for each student i, these variables capture the proportion of students of each gender with a post-college parent computed from the school-grade distribution of students by gender after eliminating student i from the distribution.

3.4 Empirical Model and Identification Strategy

This section presents the benchmark regression model and conducts several tests. In particular, it reports the variation that exists once in the main variables net of fixed effects and school time trends. It performs Monte Carlo simulations and balance tests.

[17]See the Appendix for further details.

3.4.1 The Regression Model

In Wave IV we observe individuals at ages 26-32, by which point individuals are likely to have completed a substantial portion of their education.[18] Hence, the main long-run outcome that we are interested in studying and that is feasible given the nature of the data is completion of a bachelor's degree. We create a variable for "bachelor's degree" which equals 1 if the individual reports any of the following degrees: bachelor's degree, certificate from a 1- or 2-year post-baccalaureate academic program, master's degree, PhD degree or equivalent (EDD, DrPH, etc.), or professional doctorate (MD, JD, LLB, DDS, etc.). The variable "bachelor's degree" is set equal to 0 if the individual does not report any of these degrees.

We estimate the following regression model:

$$y_{igs,t+1} = \alpha_g + \beta_s + \delta_s g + \phi_1 MF_{igs} + \phi_2 FF_{igs} + \theta X_{i,t} + \gamma Z_{igs,t} + \varepsilon_{i,t+1} \tag{3.2}$$

where i denotes a student, g denotes grade or cohort, s denotes school, and t denotes time. $y_{igs,t+1}$ is a dummy variable taking value 1 if, as of Wave IV ($t+1$), the student has obtained a bachelor's degree. α_g is a grade fixed effect, β_s is a school fixed effect, and $\delta_s g$ is a school-specific linear time trend (which is equivalent to a linear trend in grade level). The linear time trend is implemented by creating dummy variables by school that are set equal to the student's grade if they attend the given school, and 0 otherwise. We also include a vector of controls for individual characteristics, X_i, and a vector of other peer characteristics $Z_{igs,t}$, as measured in Wave I. Finally, $\varepsilon_{i,t+1}$ are i.i.d., mean 0 innovations.

Our empirical strategy exploits idiosyncratic variation in exposure to high achievers across different cohorts of high-school students within a given school, a common approach in the literature.[19] The grade fixed-effects control for initial differences across cohorts whereas

[18] To the extent that younger individuals may still go on to finish college, this is controlled for by the grade fixed effect.

[19] See, e.g., Hoxby (2000b, 2000a); Angrist and Lang (2004); Gould et al. (2009); Lavy and Schlosser (2011);

the school fixed-effects control for unobserved differences in average student characteristics across schools as well as for aspects of school quality that are constant across cohorts within a school. The main assumption we make is the usual one: we assume that while parents may make decisions based on overall characteristics of a school, they do not do so based on the specific characteristics of their child's cohort within the school. Thus, the variation due to differences in cohorts across schools can be treated as quasi-random. To deal with the possibility that the average characteristics of a school may be changing over time/grade, and thus that there may also be changes in selection over time, we include a school linear time trend in all specifications. Thus, the quasi-random variation is obtained from the deviation from this trend, rather than simply from the deviation around the average cohort in the school.

3.4.2 Evidence in Support of the Identification Strategy

Our ability to exploit this identification strategy relies on there being sufficient residual variation in the main variables. Table 3.5, panel (a), reports variation in the fraction of male (female) "high achievers" (always using the leave-one-out distribution as described earlier) by sex. As shown in the first row, the average of MF is 0.145 for females and 0.143 for males (that is, about 14.3-14.5 percent of male peers have a post-college parent), and the standard deviation is about 0.10. After removing grade fixed effects and the school time trend, the residual variation is about 0.02, accounting for just under one-fifth of the overall raw variation for both genders. The average of FF is slightly lower, about 0.12, and the standard deviation is approximately 0.10.[20] The residual variation in FF of 0.02 accounts for about one-fifth of the overall variation as well.

Lavy, Paserman, and Schlosser (2011); Olivetti et al. (2018); and Merlino, Steinhardt, and Wren-Lewis (2019).

[20]This could be due to girls with more-educated parents having a greater tendency to attend all-girls schools.

Table 3.5: Variation in Main Variables

(a) Variation in MF and FF

	Females		Males	
	MF	FF	MF	FF
Raw Variation				
Mean	0.145	0.122	0.143	0.119
SD	0.102	0.102	0.097	0.098
Min, Max	0.000, 0.707	0.000, 0.909	0.000, 0.713	0, 0.870
Net of Fixed Effects and School Trends				
Mean	0.000	0.000	0.000	0.000
SD	0.020	0.019	0.021	0.019
Min, Max	-0.109, 0.095	-0.133,0.165	-0.171, 0.108	-0.072,0.127
Count	5899	5899	4954	4954

(b) Variation in MN and FN

	Females		Males	
	MN	FN	MN	FN
Raw Variation				
Mean	16.398	14.178	16.139	13.785
SD	15.543	15.090	14.810	14.230
Min, Max	0.00, 86.805	0.00, 80.457	0.00, 86.805	0.00, 80.457
Net of Fixed Effects and School Trends				
Mean	0 .000	0 .000	0 .000	0 .000
SD	2.670	2.095	2.540	2.099
Min, Max	-13.091, 16.843	-16.032,10.179	-18.211, 14.169	-15.715, 14.704
Count	5899	5899	4954	4954

Note: This table reports the raw and residual (net of fixed effects and time trends) variation in MF, FF, MN, and FN. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent) in the grade and school. MN (respectively, FN) is the number of male (respectively, female) "high achievers" (those with at least one post-college parent) in the grade and school. Wave IV weights used.

Next, to test whether the variation we observe in our main variables is "as good as random" we perform Monte Carlo simulations.[21] Specifically, for each student i in our sample we randomly generate the post-college status of i's parents using a binomial distribution function with p equal to the fraction of students of the same gender as i who have post-college

[21]See, e.g., Lavy and Schlosser (2011) and Rodríguez-Planas et al. (2018).

parents in that student's school. We then compute the within-school standard deviation, by gender, using the residuals from regressions of MF and FF on school fixed effects, grade fixed effects, and school-specific time trends. We repeat this process 1,000 times to obtain an empirical 90 percent confidence interval for the standard deviation in MF and FF by gender, for each school.[22]

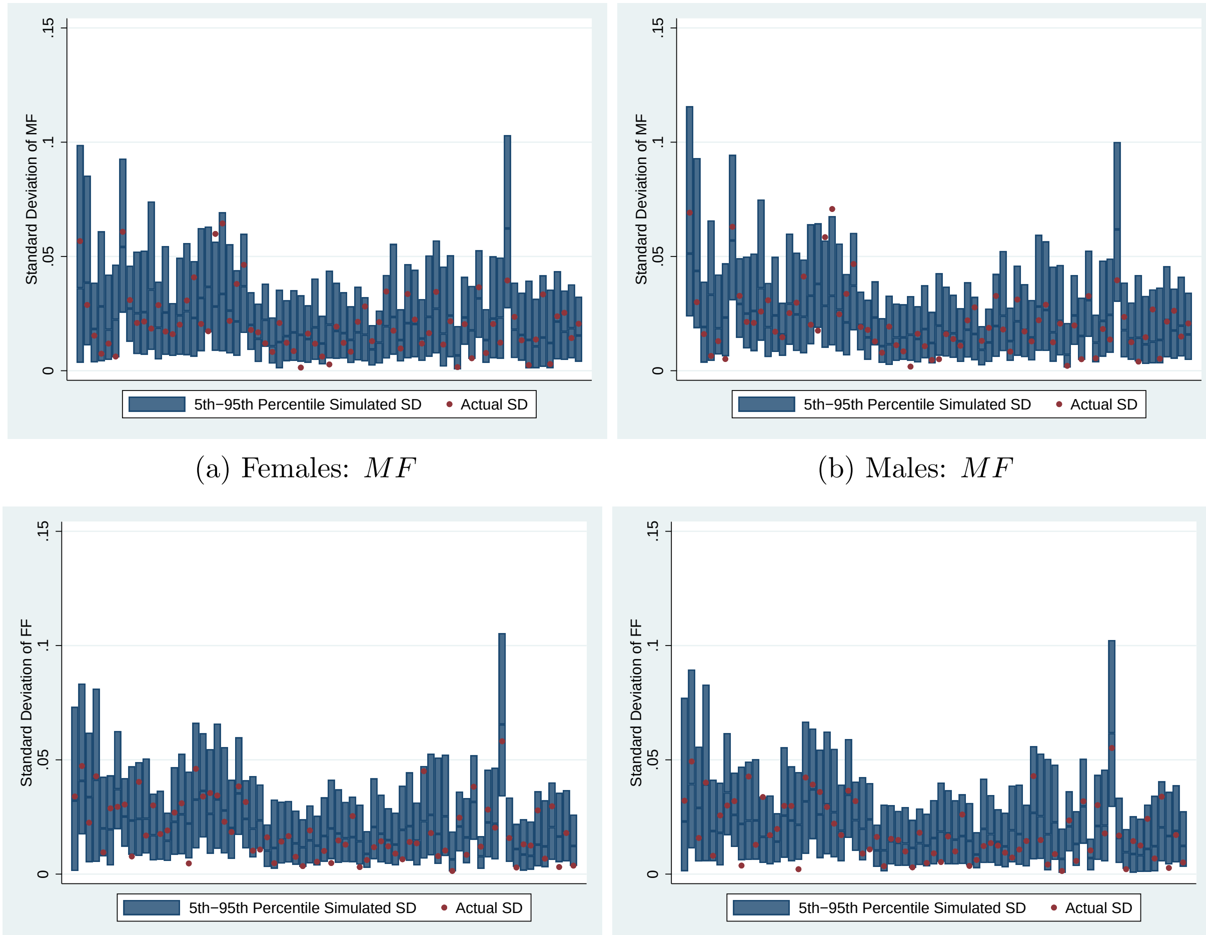
Figure 3.1 shows the simulated standard deviation of MF and FF for females (panels a and c) and males (panels b and d). The upper and lower edges of the bars represent the 5th and 95th percentiles of the simulated standard deviation. The dot represents the empirical standard deviation. As shown in the figure, about 90 percent of our schools have a standard deviation of MF and FF within the 90 percent confidence interval obtained from our simulations for both males and females.[23]

Following Lavy and Schlosser (2011), we next investigate the validity of the identification strategy by examining whether the variation in the main variables is related to the variation in a number of predetermined student characteristics. We consider family income, family social structure (as captured by whether the mother/father lives with the student), the student's ability (as measured by the Picture Vocabulary Test), race (whether or not the student is Black), and age in months. We run separate regressions with each of these as alternate dependent variables and the main variables as the independent variables, always including grade and school fixed effects and time trends. Note that there is a mechanical negative correlation between girls' FF (boys' MF) and own parent's post-college status as a result of the leave-one-out strategy. For example, a girl with a post-college parent will have a

[22] We perform this exercise only for schools with at least 3 grades (71 schools); for those with fewer grades, the variation in our main variables is absorbed by the school fixed effect and its time trend. Specifically, MF necessarily takes the same value for all girls in the same grade and school; likewise for boys and FF. As a result, variation in MF for females and FF for males will be completely absorbed by the school fixed effect and the time trend in schools with fewer than three grades. MF for males and FF for females take on separate values for students in the same grade and school due to the leave-one-out nature of the variable construction.

[23] Specifically, 96 percent (females) and 90 percent (males) of schools have a standard deviation of MF falling within the estimated 90 percent confidence interval, and 90 percent (females) and 92 percent (males) have a standard deviation of FF falling within the estimated 90 percent confidence interval.

Figure 3.1: Monte Carlo Estimates of MF and FF



(a) Females: MF

(b) Males: MF

(c) Females: FF

(d) Males: FF

Note: These figures display simulated and actual standard deviations for schools in the sample with at least three grades, with each bar representing a different school. Upper and lower edges of the bar represent the 5th and 95th percentiles respectively of the simulated within-school standard deviation of MF (or FF). The dot represents the empirical standard deviation.

lower FF than her peers without a post-college parent since the former's parents are not included in the grade average of FF. To eliminate this source of bias, we control for own parent's post-college status in the regression with girls and FF (respectively, boys and MF). As shown in Table 3.6, only one of the estimated correlations is significantly different from zero at the five percent level, which is slightly less than what would be expected by chance. This evidence mitigates concerns regarding systematic differences due to sorting across grades

Table 3.6: Balance Tests for MF and FF

	Log Family Income	PVT Score	Mother Not in HH	Father Not in HH	Black	Age in Months
Panel A, Females						
MF	0.411	0.856	-0.147	-0.329	0.001	4.670
	(0.592)	(6.627)	(0.111)	(0.250)	(0.200)	(3.691)
FF	0.245	-4.232	0.134	-0.195	0.162	2.050
	(0.443)	(8.250)	(0.113)	(0.301)	(0.132)	(3.921)
Own Parent Post College	0.312***	5.163***	-0.009	-0.018	0.004	-1.103***
	(0.036)	(0.725)	(0.007)	(0.019)	(0.019)	(0.288)
Panel B, Males						
	Log Family Income	PVT Score	Mother Not in HH	Father Not in HH	Black	Age in Months
MF	-0.037	3.858	0.107	-0.239	0.328	-2.782
	(0.666)	(7.933)	(0.144)	(0.356)	(0.208)	(3.748)
Own Parent Post College	0.277***	4.183***	-0.014	-0.009	0.030*	-1.157***
	(0.042)	(0.615)	(0.010)	(0.019)	(0.015)	(0.294)
FF	0.370	3.731	-0.0879	-0.557**	0.155	-2.980
	(0.506)	(9.972)	(0.207)	(0.226)	(0.134)	(5.399)

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of MF and FF on individual characteristics. The estimates displayed in each row are for separate regressions in which the dependent variable is the variable name in the column and the independent variable is displayed in the row. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent) in the grade and school. The regressions of FF in Panel A and MF in Panel B include a control for whether the individual has at least one post-college parent. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. All regressions are unweighted. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

within the same school along observable students' characteristics.[24,25] Taken together, the results in Tables 3.5 and 3.6, along with Figure 3.1 lend support to the identification strategy.

3.5 Results

This section presents the benchmark results. In response to the potential concern that the main results could be driven by dropouts, it then explores an alternative measure of exposure that relies on numbers rather than proportions. Lastly, to assess the likelihood that the results could have occurred by chance, it conducts a simulation exercise.

[24] Altonji, Elder, and Taber (2005) suggest that the degree of selection on observables can provide a good indicator of the degree of selection on unobservables. In light of this argument, the evidence of no correlation from Table 3.6 support the notion that our model specification identifies an exogenous source of variation.

[25] We can also examine whether the residual variations in MF and FF are related. To test this, we regress the fraction of females with a post college parent on the fraction of males with a post college parent, as well as grade and school fixed effects, and linear time trends. The correlation between the two variables is 0.12 and statistically insignificant.

3.5.1 The Benchmark Results

Table 3.7 reports the estimation results of the model in equation (3.2), with the completion of a bachelor's (four-year college) degree as the dependent variable. The results are shown with increasing controls and all specifications include grade fixed-effects, school fixed-effects, and a school time trend.[26] Standard errors are clustered at the school level. The first six columns are for girls; the last six are for boys.

In column (1), we include only grade and school fixed effects and linear time trends. In column (2), we add controls for individual characteristics and parental background. We include age in months, an indicator for foreign born, race, the student's PVT score, parental education (coded as discussed previously), an indicator for mother or father not in the household, and the log of household income. Looking across columns, note that, conditional on student grade and school, age in months is negatively associated with attainment of a bachelor's degree. This may in part reflect students who have been held back as these tend to complete college at lower rates (or students who have skipped grades could be completing at higher rates). An individual's foreign-born status has a positive association with college completion.

As expected, both higher parental education and higher parental income increase the likelihood of completing college, as does a higher PVT score. In column (3) (respectively, column (9) for males) we include the PVT percentile rank of the student in addition to their PVT score, where the rank is calculated relative to the other students in the in-home sample from the same grade and school as only these students have PVT reported. The PVT rank has been shown in previous literature to affect educational attainment and career aspirations

[26] All specifications include a dummy for whether the Wave I interview was completed between April and August 1995 (the 1994-1995 school year) or between September and December 1995 (the 1995-1996 school year). Since the in-school survey was completed during the 1994-1995 school year, this dummy accounts for whether students answer the in-home survey with respect to contemporaneous peers or the peer composition in the prior school year.

Table 3.7: High Achievers and Bachelor's Degree Attainment

	Dependent Variable: Bachelor's degree											
	Females						Males					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
MF	-1.053***	-1.046***	-1.019***	-0.991***	-1.083***	-0.945***	0.024	0.285	0.288	0.221	0.124	0.110
	(0.293)	(0.276)	(0.269)	(0.296)	(0.317)	(0.286)	(0.428)	(0.387)	(0.389)	(0.365)	(0.365)	(0.380)
FF	-0.258	0.176	0.199	0.184	0.291	0.318	0.553	0.306	0.307	0.323	0.201	0.213
	(0.384)	(0.357)	(0.348)	(0.364)	(0.338)	(0.331)	(0.550)	(0.522)	(0.526)	(0.502)	(0.493)	(0.498)
Foreign Born		0.089**	0.081**	0.089**	0.087**	0.061		0.102**	0.099**	0.101**	0.104**	0.068
		(0.041)	(0.040)	(0.040)	(0.040)	(0.040)		(0.045)	(0.045)	(0.045)	(0.046)	(0.042)
PVT Score		0.530***	0.233*	0.533***	0.535***			0.455***	0.211	0.450***	0.449***	
		(0.065)	(0.133)	(0.065)	(0.064)			(0.071)	(0.168)	(0.071)	(0.070)	
Age in Months		-0.006***	-0.005***	-0.006***	-0.006***	-0.008***		-0.005***	-0.005***	-0.005***	-0.005***	-0.007***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother HS Grad		0.060***	0.060***	0.060***	0.059***	0.064***		0.007	0.007	0.009	0.009	0.025
		(0.022)	(0.022)	(0.022)	(0.022)	(0.021)		(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Mother Some College		0.115***	0.114***	0.115***	0.116***	0.138***		0.086***	0.086***	0.088***	0.088***	0.107***
		(0.026)	(0.026)	(0.026)	(0.026)	(0.027)		(0.025)	(0.025)	(0.025)	(0.025)	(0.024)
Mother College Grad		0.213***	0.213***	0.213***	0.214***	0.239***		0.098***	0.098***	0.100***	0.099***	0.127***
		(0.036)	(0.036)	(0.036)	(0.036)	(0.036)		(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
Mother Post College		0.286***	0.284***	0.287***	0.287***	0.319***		0.202***	0.202***	0.205***	0.202***	0.241***
		(0.043)	(0.043)	(0.042)	(0.043)	(0.043)		(0.040)	(0.040)	(0.040)	(0.040)	(0.043)
Mother Not in HH		0.067*	0.066*	0.066*	0.067*	0.063		-0.043	-0.045	-0.043	-0.042	-0.029
		(0.039)	(0.039)	(0.038)	(0.038)	(0.038)		(0.041)	(0.041)	(0.041)	(0.040)	(0.037)
Father HS Grad		0.053**	0.054**	0.053**	0.052**	0.061**		0.010	0.011	0.010	0.011	0.017
		(0.023)	(0.023)	(0.023)	(0.023)	(0.024)		(0.027)	(0.027)	(0.028)	(0.027)	(0.025)
Father Some College		0.120***	0.119***	0.120***	0.121***	0.137***		0.107***	0.106***	0.106***	0.105***	0.116***
		(0.026)	(0.025)	(0.026)	(0.026)	(0.025)		(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Father College Grad		0.239***	0.240***	0.240***	0.240***	0.254***		0.201***	0.202***	0.201***	0.200***	0.218***
		(0.035)	(0.035)	(0.035)	(0.035)	(0.035)		(0.045)	(0.045)	(0.045)	(0.044)	(0.041)
Father Post College		0.301***	0.298***	0.300***	0.298***	0.323***		0.319***	0.319***	0.318***	0.315***	0.317***
		(0.052)	(0.052)	(0.052)	(0.052)	(0.052)		(0.044)	(0.044)	(0.044)	(0.044)	(0.047)
Father Not in HH		0.034	0.034	0.034	0.033	0.036		0.042	0.043	0.041	0.042	0.049*
		(0.023)	(0.023)	(0.023)	(0.023)	(0.024)		(0.030)	(0.030)	(0.030)	(0.030)	(0.028)
Log Family Income		0.046***	0.046***	0.046***	0.046***	0.042***		0.024**	0.024**	0.023**	0.023**	0.028**
		(0.012)	(0.012)	(0.012)	(0.012)	(0.012)		(0.011)	(0.011)	(0.011)	(0.011)	(0.012)
PVT Rank			0.135**						0.109			
			(0.058)						(0.067)			
Fraction Female				0.394*	0.438*	0.325				-0.540**	-0.576**	-0.594**
				(0.226)	(0.226)	(0.217)				(0.270)	(0.267)	(0.265)
Fraction Foreign Born					-0.942	-0.816					1.294**	1.478***
					(0.642)	(0.627)					(0.522)	(0.500)
Fraction Black					-0.703	-0.606					-0.341	-0.344
					(0.522)	(0.509)					(0.603)	(0.592)
Fraction Latino					-0.292	-0.154					-0.120	-0.199
					(0.431)	(0.414)					(0.410)	(0.411)
Fraction Asian					0.638	0.546					-1.534*	-1.628**
					(0.686)	(0.645)					(0.793)	(0.786)
Fraction Other Races					-0.431	-0.274					-0.113	-0.126
					(0.310)	(0.298)					(0.250)	(0.246)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Race FE	No	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
School Linear TT	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5899	5650	5649	5650	5650	5898	4954	4703	4703	4703	4703	4948
R^2	0.187	0.332	0.332	0.332	0.334	0.318	0.199	0.326	0.326	0.326	0.328	0.312
Adjusted R^2	0.152	0.299	0.299	0.300	0.300	0.285	0.158	0.286	0.286	0.287	0.287	0.273

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of bachelor's degree attainment on individual and peer characteristics. The dependent variable is equal to 1 if the individual has completed a bachelor's (four-year college) degree and 0 otherwise. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent). All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Race fixed effects include dummies for Black, Latino, Asian, and other races. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Coefficient on PVT score multiplied by 100. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

[see Elsner and Isphording (2017, 2018)].[27] As can be seen, the PVT rank is positively and significantly associated with college graduation for women (but not for men which may be a function of the sample size over which rank is calculated). The coefficients on the main variables are unaffected by its inclusion. In column (4) (respectively, column (10) for men), we include the fraction of peers who are female as the literature has tended to find that students perform better when the fraction of boys is smaller (see, e.g.,Lavy and Schlosser (2011)) and that long-run educational attainment increases with the fraction of same-gender peers (Black et al., 2013). The results here are in agreement with (Black et al., 2013): a higher fraction of girls increases long-run education outcomes for girls and decreases them for boys. In column (5) (respectively, column (11) for men), we include other peer variables: the fractions foreign born, Black, Asian and Latino.[28] These fractions are calculated in the same "leave-one-out" fashion described earlier for the main variables.

Finally, in column (6) (respectively, column (12) for males), we remove the individual's PVT score but keep all other control variables. The PVT score is generally viewed as a measure of innate ability and the convention in the literature is to include it in the set of baseline individual controls.[29] However, to the extent that PVT measures content or skills learned in the classroom, it could be an endogenous outcome and should thus be excluded. We thus show the results without the PVT score to allay possible concerns about its endogeneity. Note that once it is omitted, the coefficient on foreign born is insignificant, as would be

[27] We follow Elsner and Isphording (2017, 2018) and first calculate the absolute rank of each student relative to others in her grade and school (with the worst-performing student having a value of 1) before converting into a percentile to render the measure comparable across samples of different sizes. To convert absolute rank to a percentile rank, the following formula is used: Percentile Rank=(Absolute Rank-1)/(Number of Students in the Same Grade and School -1). Thus, the percentile rank ranges from 0 (the worst student) to 1 (the best student).

[28] Students report their race and ethnicity in the in-school survey. Individuals are classified as Hispanic if they report their ethnicity as Hispanic/Latino. Individuals are given a race/ethnicity of other if they report multiple races, do not report a race, or report an "other" race (not Black, White, or Asian). There are a number of students who do not report their foreign born status. To construct the fraction of peers who are foreign born, we impute the missing values by using the average fraction foreign born among students of the same race/ethnicity in the same grade and school. Bifulco, Fletcher, and Ross (2011) find that a higher fraction of Black and Hispanic peers is associated with worse reported student-teacher relationships and an increase in disruptive behaviors.

[29] See, e.g., Bifulco et al. (2011); Elsner and Isphording (2017); Olivetti et al. (2018).

expected if the test requires proficiency in English.

As can be seen clearly in this table, across specifications there is a great deal of stability in the magnitude of the estimated coefficients on all variables. Across all specifications, a greater fraction of "high-achieving" male peers decreases the likelihood of a girl completing a bachelor's degree. The effect is sizable: using the most complete specification (column 5), an increase in MF by a standard deviation (2.0 percentage points) is associated with a 2.2 percentage point decrease in the probability that these girls go on to complete a four-year college degree by Wave IV.[30] The effect of female high-achievers, on the other hand, while positive is statistically insignificant. The results are radically different for males. The effect of the fraction of "high-achieving" males on males' college completion, while positive for the most part, is not statistically different from zero. The effect of "high-achieving" female peers again is positive and insignificant.

As shown in columns (6) and (12), excluding the PVT score as an individual control leaves the results unchanged.[31] Since the results do not differ substantially with or without the PVT score, we follow the convention in the literature and focus on specification (5). Furthermore, as shown previously in Table 3.6, there is no evidence that the PVT score covaries with MF or FF.

3.5.2 Alternative Measure of Exposure to "High Achievers"

The results above show that "high-achieving" boys have a negative influence on girls' long-run education outcomes. This is a strong and intriguing result since, if anything, one might expect their effect to be positive. A potential concern is that the variation in the

[30] Whenever the effect of a standard deviation change in a variable is considered, it is *the standard deviation net of fixed effects and of time trend*.

[31] The sample size increases as there were around 250 observations, by gender, dropped because of missing PVT scores.

fraction of high achievers could be driven by variation in the number of dropouts across grades in a school. Dropouts tend to have parents with relatively lower levels of education, and therefore a higher-than-average number of dropouts in a grade would mechanically generate a disproportionately high fraction of "high achievers."[32] A high value of MF (or FF) could thus reflect a high number of "bad boys" (or "bad girls") in previous years rather than a high number of "good boys" (or "good girls"). In that case, the correct interpretation of the result would be the *opposite* of the conclusion we offered: it would be that having been previously in a grade with a disproportionately large number of future male dropouts is bad for girls. We deal with this important concern in a variety of ways.

First, to the extent that the dropout rate results in linear trend in the main variables, this issue is taken care of in the school time trend. To the extent that it does not, we can eliminate the ambiguity introduced by relying on proportions and use the *number* of "high achievers" rather than the *fraction* of these. The number of "high achievers" is not affected by variation in the number of dropouts. Thus, we define, for each student i in grade g and school s, the *number* of "high achievers" of a given sex (i.e, the number of students of a given gender with a post-college parent):

$$MN_{igs} = \sum_{j(i)=1}^{n} PC_{jgs}, \qquad FN_{igs} = \sum_{k(i)=1}^{q} PC_{kgs} \tag{3.3}$$

where $j(i) = 1, ..., j$ indexes student i's male peers and $k(i) = 1, ..., q$ indexes i's female peers. Therefore, MN_{igs} (equivalently, FN_{igs}) is the number of male peers (equivalently, female peers) in the same school and grade as i who are high achievers. As before, both MN_{igs} and FN_{igs} are the sample moments of the leave-one-out distribution of the students with a post-college parent belonging to a specific grade and school. Table table:variation, panel(b) reports variation in the number of male (female) "high achievers" (always using the leave-one-out distribution as described earlier).

[32]For example, among ninth graders in our sample interviewed before September 1995, all those with a post-college parent are in school the following year. In contrast, 96.7% of those without a post-college parent are in school the following year.

To facilitate the interpretation of the coefficients on the main variables, we perform an inverse hyperbolic sine transformation on MN and FN. This allows the coefficients ϕ_1 and ϕ_2 to be interpreted as measuring the effect of a percentage change in MN or FN, respectively, and, unlike logs, be defined at zero.[33] We use:

$$y_{igs,t+1} = \alpha_g + \beta_s + \delta_s g + \phi_1 IHS(MN_{igs}) + \phi_2 IHS(FN_{igs}) + \theta X_{i,t} + \gamma Z_{igs,t} + \varepsilon_{i,t+1} \quad (3.4)$$

where all variables are as defined previously. The identification tests (the Monte Carlo simulation and the check on the balance in other peer variables) for MN and FN are in the Appendix and provide evidence that the variation in these variables is random after controlling for fixed effects and time trends.

Table 3.8 reports the estimation results of regression model (3.4) using MN and FN as the main explanatory variables and the completion of a bachelor's degree as the dependent variable. As noted, we use the inverse hyperbolic sine of MN and FN – our main explanatory variables – and consequently of all peer variables such as the number of Asians, Blacks, and Hispanics.[34] The table follows the same format as Table 3.7: the first six columns report the results for females; the last six columns do the same for males. As before, all columns include grade fixed effects, school fixed effects, and school linear time trends. Standard errors are clustered at the school level.

Analogously to the results in Table 3.7, we find that "high-achieving" males reduce females' college completion. Across the columns, the effect of MN on the proportion of girls who graduate from college stays remarkably constant. Different from Table 3.7, we now find a statistically significant positive effect of the number of "high-achieving" girls on the outcomes of other girls. Interestingly, the coefficient on the fraction of girls in the grade becomes

[33] The inverse hyperbolic sine of x is defined as: $log(x + (1 + x^2)^{0.5})$ [see, e.g., Burbidge, Magee, and Robb (1988)].

[34] We use number rather than fraction for the same reason that we turned to numbers to measure "high-achieving" peers, i.e., the concern that the results may be driven by selection into dropping out/being absent. The only exception is for percent female. We kept this as a percent as including the number of girls would transform our main variable into a percentage again.

Table 3.8: High Achievers and Bachelor's Degree Attainment

	Dependent Variable: Bachelor's degree											
	Females						Males					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
IHS MN	-0.100***	-0.103***	-0.100***	-0.094***	-0.095***	-0.082***	-0.005	0.027	0.027	0.005	0.004	0.001
	(0.023)	(0.028)	(0.027)	(0.031)	(0.030)	(0.026)	(0.052)	(0.045)	(0.046)	(0.046)	(0.046)	(0.047)
IHS FN	0.036	0.058**	0.058**	0.053*	0.050*	0.051*	0.016	0.001	0.001	0.017	0.035	0.036
	(0.033)	(0.027)	(0.026)	(0.028)	(0.026)	(0.027)	(0.050)	(0.046)	(0.046)	(0.043)	(0.047)	(0.051)
Foreign Born		0.089**	0.081**	0.089**	0.087**	0.062		0.103**	0.100**	0.101**	0.100**	0.064
		(0.040)	(0.040)	(0.040)	(0.040)	(0.040)		(0.045)	(0.045)	(0.045)	(0.045)	(0.041)
PVT Score		0.531***	0.235*	0.532***	0.532***			0.455***	0.211	0.450***	0.450***	
		(0.065)	(0.135)	(0.065)	(0.064)			(0.071)	(0.169)	(0.071)	(0.071)	
Age in Months		-0.006***	-0.005***	-0.006***	-0.006***	-0.008***		-0.005***	-0.005***	-0.005***	-0.005***	-0.007***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Mother HS Grad		0.060***	0.059***	0.060***	0.058**	0.062***		0.008	0.008	0.010	0.007	0.024
		(0.022)	(0.022)	(0.022)	(0.022)	(0.021)		(0.021)	(0.021)	(0.021)	(0.021)	(0.021)
Mother Some College		0.115***	0.114***	0.115***	0.115***	0.138***		0.086***	0.086***	0.089***	0.085***	0.104***
		(0.026)	(0.026)	(0.026)	(0.026)	(0.027)		(0.025)	(0.025)	(0.024)	(0.025)	(0.024)
Mother College Grad		0.213***	0.212***	0.213***	0.212***	0.238***		0.099***	0.098***	0.101***	0.098***	0.127***
		(0.036)	(0.036)	(0.036)	(0.036)	(0.036)		(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
Mother Post College		0.287***	0.285***	0.287***	0.286***	0.318***		0.203***	0.203***	0.205***	0.204***	0.244***
		(0.043)	(0.043)	(0.043)	(0.043)	(0.043)		(0.040)	(0.040)	(0.040)	(0.040)	(0.043)
Mother Not in HH		0.067*	0.066*	0.067*	0.065*	0.062		-0.043	-0.045	-0.043	-0.044	-0.030
		(0.039)	(0.038)	(0.038)	(0.038)	(0.038)		(0.041)	(0.041)	(0.041)	(0.040)	(0.037)
Father HS Grad		0.052**	0.052**	0.052**	0.051**	0.060**		0.010	0.011	0.010	0.012	0.019
		(0.023)	(0.023)	(0.023)	(0.023)	(0.024)		(0.027)	(0.027)	(0.027)	(0.027)	(0.025)
Father Some College		0.119***	0.118***	0.119***	0.117***	0.135***		0.106***	0.106***	0.106***	0.106***	0.117***
		(0.026)	(0.026)	(0.026)	(0.026)	(0.025)		(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Father College Grad		0.240***	0.240***	0.240***	0.238***	0.251***		0.202***	0.203***	0.201***	0.202***	0.220***
		(0.035)	(0.035)	(0.035)	(0.035)	(0.035)		(0.045)	(0.045)	(0.045)	(0.044)	(0.041)
Father Post College		0.300***	0.296***	0.299***	0.296***	0.321***		0.319***	0.319***	0.318***	0.316***	0.319***
		(0.051)	(0.051)	(0.052)	(0.052)	(0.052)		(0.044)	(0.044)	(0.044)	(0.044)	(0.047)
Father Not in HH		0.034	0.034	0.034	0.031	0.035		0.041	0.042	0.041	0.042	0.049*
		(0.023)	(0.023)	(0.023)	(0.023)	(0.024)		(0.031)	(0.031)	(0.031)	(0.030)	(0.028)
Log Family Income		0.045***	0.046***	0.045***	0.045***	0.041***		0.024**	0.024**	0.023**	0.023**	0.028**
		(0.012)	(0.012)	(0.012)	(0.012)	(0.011)		(0.011)	(0.011)	(0.011)	(0.011)	(0.012)
PVT Rank			0.134**						0.109			
			(0.059)						(0.067)			
Fraction Female				0.222	0.383	0.261				-0.575**	-0.722**	-0.747**
				(0.248)	(0.256)	(0.236)				(0.280)	(0.290)	(0.292)
IHS Number Foreign Born					-0.050**	-0.046**					0.048**	0.049**
					(0.022)	(0.021)					(0.021)	(0.021)
IHS Number Black					0.004	0.013					-0.032	-0.028
					(0.027)	(0.026)					(0.043)	(0.043)
IHS Number Latino					0.022	0.023					-0.009	-0.012
					(0.022)	(0.019)					(0.023)	(0.022)
IHS Number Asian					0.021	0.014					-0.060*	-0.054*
					(0.032)	(0.030)					(0.031)	(0.029)
IHS Number Other Race					-0.010	0.008					-0.031	-0.038
					(0.047)	(0.046)					(0.044)	(0.039)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Race FE	No	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes
School Linear TT	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5899	5650	5649	5650	5650	5898	4954	4703	4703	4703	4703	4948
R^2	0.186	0.332	0.332	0.332	0.333	0.318	0.199	0.325	0.326	0.326	0.328	0.312
Adjusted R^2	0.152	0.300	0.299	0.300	0.300	0.286	0.158	0.285	0.286	0.286	0.287	0.273

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of bachelor's degree attainment on individual and peer characteristics. The dependent variable is equal to 1 if the individual has completed a bachelor's (four-year college) degree and 0 otherwise. MN (respectively, FN) is the number of male (respectively, female) "high achievers" (those with at least one post-college parent). IHS refers to the inverse hyperbolic sine transformation. All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Race fixed effects include dummies for Black, Latino, Asian, and other races. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Coefficient on PVT score multiplied by 100. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

insignificant. As before, the effect of own-parent education, household income, and PVT score are positive and significant, as expected, as is (for girls) the PVT rank. For boys, the impacts of both MN and FN are positive but statistically insignificant.

The magnitudes implied by the estimated coefficients on MN for girls are again substantial. On average, a one standard-deviation increase in the number of "high-achieving" boys (2.7) from a mean of 16.4 is associated with about a 1.4 percentage point decrease in women's college graduation. Since roughly 35 percent of women in our sample complete a bachelor's degree, a one standard-deviation increase in MN is associated with about a 4 percent decrease in the proportion of women who attain bachelor's degrees.

Lastly, in addition to demonstrating that the results are robust to the use of numbers, rather than fractions, of "high achievers" one can also test directly whether the variation in MF and FF is driven by dropouts. One way to do this is to examine the correlation between the fraction of boys with post-college parents, net of fixed effects and school time trend, and the number of boys in the grade (again net of fixed effects and time trend). If higher levels of MF were driven by higher dropout-rates of boys with less-educated parents, we would expect a negative and significant relationship between these two variables. Instead, the correlation is 0.13. The same exercise but for fraction of girls with post-college parents and the number of girls yields a correlation of 0.05. This exercise provides further evidence that the variation in the main variables is not driven by variation in dropouts.

Since the results are robust to using the main variables expressed both as fractions and as numbers and given that there is not a negative correlation between the proportion of "high achievers" of a given gender and the total number of individuals of the same gender, we henceforth restrict the remainder of the analysis to MF and FF.

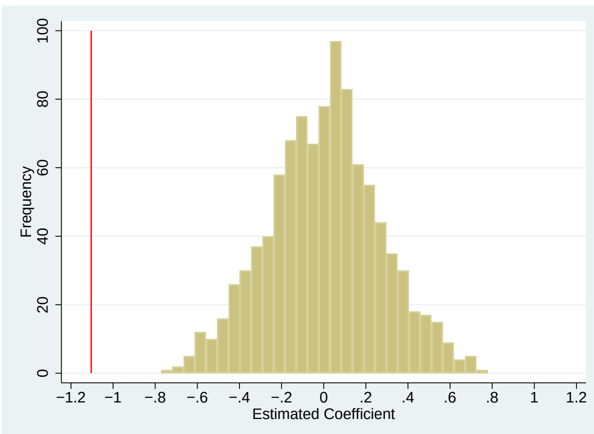
3.5.3 Placebo Tests

We next use a simulation exercise, as described in Athey and Imbens (2017), to study the likelihood that the results obtained could have occurred by chance. For both the fraction and number specifications of the main variables, we calculate the likelihood of obtaining the observed treatment effects by chance by generating randomness in the exposure of individuals to "high-achieving" students. We do this by re-assigning to each individual in the sample the MF of a random grade within the same school, keeping all other variables (both own and those of one's peers) at their true levels. We repeat this procedure 1,000 times, and run the fullest specification of Table 3.7 using MF and FF as the key explanatory variables.

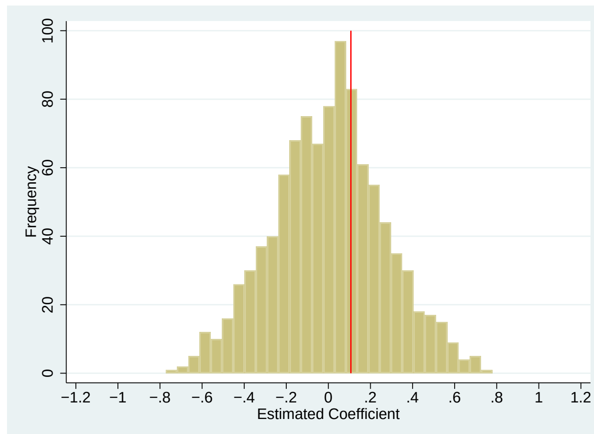
The distributions of the estimated coefficients on MF and FF, by gender, are shown in Figure 3.2. The vertical line in each graph indicates the estimated treatment effect we obtained in Table 3.7. The share of estimates that is larger in absolute value than the dashed line (actual treatment) represents the randomization-based p-value. As can be seen in the figure, the estimated coefficient of MF for girls is larger in absolute value than any of the randomization-based estimates, providing evidence that this is unlikely to have occurred by chance. The share of estimates of FF that is larger in absolute value than the line (actual treatment) is 0.27. For boys, the share of estimates that is larger in absolute value than the estimated coefficient for MF is 0.66 and of FF is 0.46.[35]

[35] Repeating this exercise for MN and FN, the results by gender are shown in Figure 3.3. The results for MN for females are similar to those obtained for MF: the estimated treatment coefficient is larger in absolute value than any of the placebo estimates. For FN, the estimated treatment coefficient is larger in absolute value than about 96% of the placebo estimates. As expected, the randomization-based estimates of the treatment effects for both MN and FN for boys are likely to be obtained by chance.

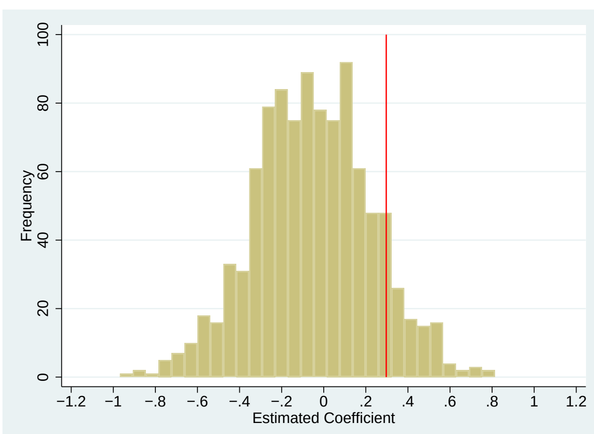
Figure 3.2: Randomization-Based Inference for MF and FF



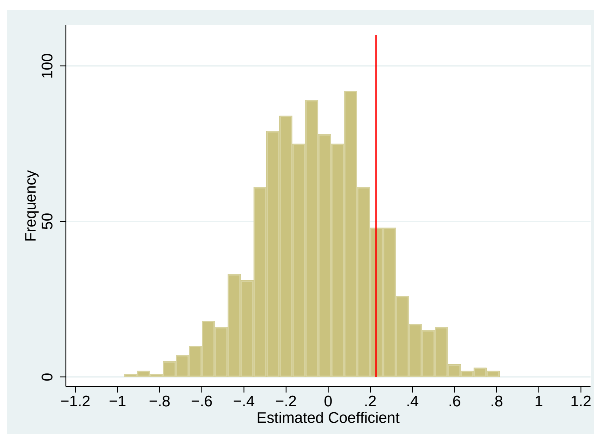
(a) Females: MF



(b) Males: MF



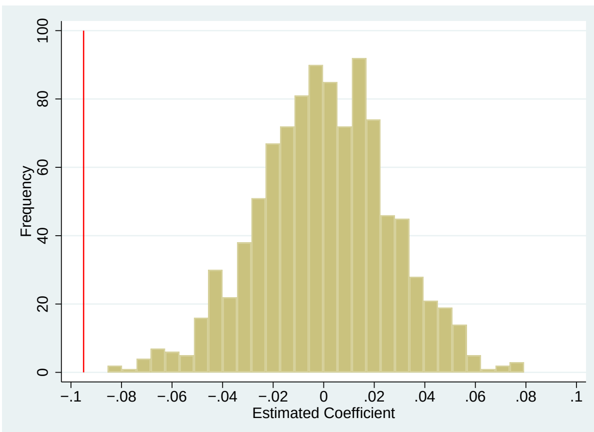
(c) Females: FF



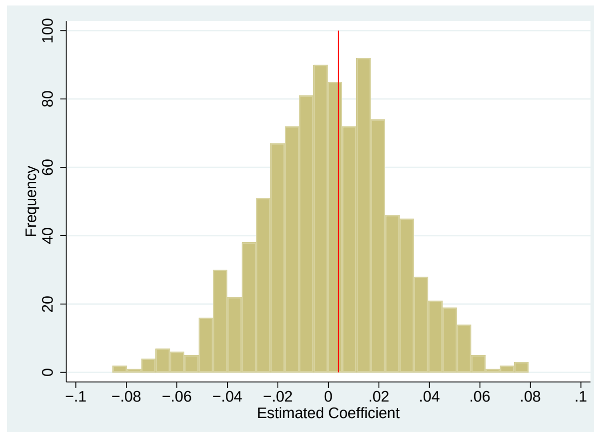
(d) Males: FF

Note: These figures show distribution of coefficients obtained from the final OLS specification in Table 7 while replacing MF (respectively, FF) with the value of MF (respectively, FF) from a random grade in the same school. Red line represents actual estimate obtained in specifications (5) and (11) in Table 7.

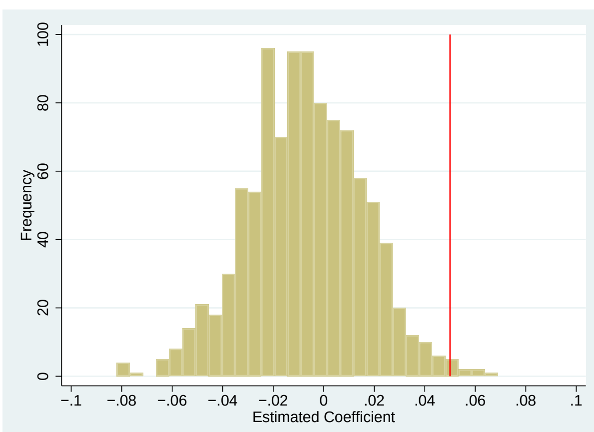
Figure 3.3: Randomization-Based Inference for MN and FN



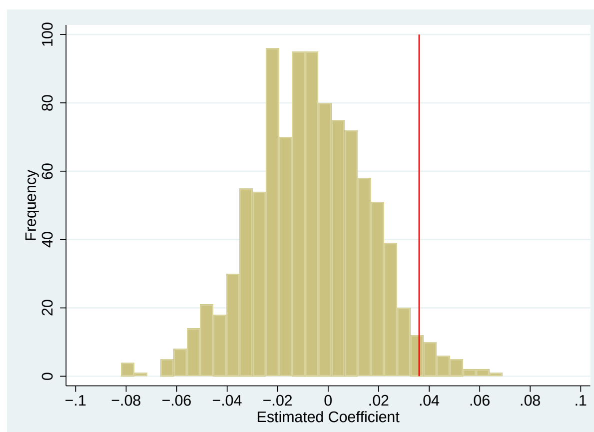
(a) Females: MN



(b) Males: MN



(c) Females: FN



(d) Males: FN

Note: These figures show distribution of coefficients obtained from the final OLS specification in Table 7 while replacing MN (respectively, FN) with the value of MN (respectively, FN) from a random grade in the same school. Red line represents actual estimate obtained in specifications (5) and (11) in Table 8.

3.6 Exploring Possible Mechanisms

We next explore mechanisms that may be responsible for our most striking result: the negative effect of “high-achieving” boys on their female peers’ probability of graduating from college. Although we cannot determine the exact mechanism (e.g., do teachers pay more attention to “high-achieving” boys to the detriment of girls? Are girls more discouraged when they face greater competition from “high-achieving” boys? etc.), we are able to explore various pathways that lead to girls achieving a worse long-run education outcome.

3.6.1 Two vs Four-Year College

Having established that girls exposed to greater levels of “high-achieving” boys are less likely to graduate with a bachelor’s degree, we next ask what level of education they attain. A natural possibility is that they obtain a different type of post-secondary degree such as a vocational or associate’s degree. The latter is a two-year post-high-school degree, usually obtained from a community college. These degrees represent a level of education greater than a high-school degree but less than a bachelor’s degree.

In Table 3.9, column (1) (respectively, column 3 for men), we use the most complete specification of Table 3.7 but with the dependent variable equal to 1 if the individual has a vocational or associate’s degree as her highest level of education. This implies that the individual reported that they had a “certificate or degree from a 1-, 2-, or 3-year vocational/technical program (after high school)” or an associate’s degree.[36] As shown, girls exposed to “high achievers” are more likely to compete a vocational or associate’s degree, substituting this for a bachelor’s degree basically one for one as the absolute magnitude is similar and the sign is opposite to that obtained for the completion of a bachelor’s degree.

[36]The variable is coded as 0 if the individual reports a degree from a vocational program lasting less than 1 year.

Table 3.9: High Achievers and Educational Attainment

	Females		Males	
	(1)	(2)	(3)	(4)
	Vocational or Associate's Degree	High School Graduate	Vocational or Associate's Degree	High School Graduate
MF	1.120***	0.060	0.055	0.284
	(0.300)	(0.110)	(0.281)	(0.173)
FF	-0.243	0.038	0.073	-0.114
	(0.320)	(0.132)	(0.240)	(0.197)
PVT Score	-0.042	0.220***	-0.076	0.206***
	(0.061)	(0.044)	(0.075)	(0.048)
Fraction Female	-0.243	-0.124	0.172	0.120
	(0.287)	(0.110)	(0.261)	(0.185)
School, Grade FE	Yes	Yes	Yes	Yes
School Linear TT	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
Peer Characteristics Controls	Yes	Yes	Yes	Yes
Observations	5650	5650	4703	4703
R^2	0.097	0.199	0.115	0.242
Adjusted R^2	0.052	0.159	0.062	0.196

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of educational attainment on individual and peer characteristics with the dependent variable listed in the column heading. The variable Vocational or Associate's Degree takes a value of 1 if the individual has a vocational/technical degree from a program lasting 1-3 years or an associate's degree and 0 otherwise. The variable High School Graduate takes a value of 1 if the individual has completed high school and 0 otherwise. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent). All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Individual controls include race dummies (Black, Latino, Asian, and other races), age in months, mother and father's education (dummies for each parent for high school, some college but no degree, college degree, and post college), and log family income. Peer characteristics controls include fraction foreign born, Black, Latino, Asian, and other races. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Coefficient on PVT score multiplied by 100. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

This indicates that "high-achieving" males influence the decision about the type of degree to pursue rather than the decision to pursue any education after high school.

Finally, we can consider the impact of "high achievers" on the probability of high-school completion. In column (2) (respectively, column (4) for males), the dependent variable is set equal to 1 if the individual reports their highest level of education as "high-school graduate" or greater. As shown, there is no impact of either the proportion of "high achievers" on high-school completion for either girls or boys.

3.6.2 GPA

Why should greater exposure to "high-achieving" boys lead girls to switch from a bachelor's to a junior college degree? One possibility is that greater exposure to these boys may reduce girls' grades. This may happen mechanically if teachers conform to a fixed grade distribution and, for example, only give out grades of "A" to some fixed percent of the class. In that case, however, we would expect a symmetric effect on boys from greater exposure to "high-achieving" girls. Alternatively, the presence of these boys may direct teachers' attention away from female students or simply discourage the latter's efforts, resulting in lower grades.

In column (1) of Table 3.10 (respectively, column (5) for boys), we repeat the main specification with the student's average GPA in Wave I as the dependent variable. As shown, "high-achieving" males are associated with a negative but statistically insignificant impact on GPA for girls whereas "high-achieving" females have a positive and statistically insignificant effect. For boys, the signs of these two variables are positive, but they are both insignificant at conventional levels.

Turning to subject-specific grades, we find that "high-achieving" boys have a significant negative effect on girls' math and science grades, with a one standard-deviation increase in MF decreasing female math grades by 0.05 points, or 5 percent of a standard deviation. These boys also negatively impact females' science grades, with a similar magnitude associated with a one-standard deviation increase in their proportion as for math grades.[37] There is no statistically significant effect of either peer variable on boys' grades in math or science.

The results above are suggestive of findings in Bordalo et al. (2018). Their set of experiments show that, for a given level of difficulty, the greater the average performance gender gap in a domain, the less confident girls are that their answers are correct. Boys,

[37]Students may not be taking all subjects, but we do not find a significant effect of MF on girls' probability of taking any of the four subjects.

Table 3.10: High Achievers and Grades

	Females					Males				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	GPA	Math	Science	English	History	GPA	Math	Science	English	History
MF	-1.043	-2.289***	-2.010**	-0.699	0.038	0.543	0.674	-0.347	0.686	0.522
	(0.643)	(0.850)	(0.856)	(0.850)	(0.925)	(0.531)	(0.820)	(0.919)	(0.799)	(0.979)
FF	0.222	-0.141	-0.546	0.901	0.352	0.692	-0.839	0.744	1.529	1.282
	(0.521)	(0.658)	(0.858)	(0.698)	(0.891)	(0.728)	(0.978)	(0.904)	(0.944)	(1.055)
PVT Score	1.197***	0.710***	1.355***	1.304***	1.559***	1.006***	0.467***	1.173***	0.912***	1.498***
	(0.136)	(0.169)	(0.173)	(0.149)	(0.179)	(0.132)	(0.169)	(0.190)	(0.160)	(0.183)
Fraction Female	0.175	0.512	0.633	-0.363	0.109	0.872	0.617	0.468	0.948	2.034**
	(0.450)	(0.684)	(0.607)	(0.648)	(0.655)	(0.598)	(0.723)	(0.864)	(0.906)	(0.999)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Linear TT	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Peer Characteristics Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5548	5224	4961	5508	4946	4611	4388	4135	4552	4126
R^2	0.295	0.181	0.248	0.252	0.255	0.292	0.239	0.253	0.234	0.257
Adjusted R^2	0.259	0.137	0.206	0.214	0.213	0.248	0.189	0.201	0.186	0.206

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of grades on individual and peer characteristics. Grades are based on student reports of their grade in each subject over the previous year, with A=4, B=3, C=2, and D or lower=1. Average GPA reflects the average across these four subjects. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent). All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Individual controls include race dummies (Black, Latino, Asian, and other races), age in months, mother and father's education (dummies for each parent for high school, some college but no degree, college degree, and post college), and log family income. Peer characteristics controls include fraction foreign born, Black, Latino, Asian, and other races. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Coefficient on PVT score multiplied by 100. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

on the other hand, are not affected. In our setting, a similar phenomenon may be taking place. Faced with a greater proportion of "high-performing" boys, girls may become less self-confident about their own ability in traditionally male-dominated fields such as math and science. More generally, these high-school girls may become more discouraged or think themselves less competent which could then affect their actual performance.

Our results also complement those in Feld and Zölitz (2018) and Mouganie and Wang (2017) that find that high-performing male peers reduce females' completion of mathematics and/or science courses. Feld and Zölitz (2018) study outcomes of random assignment of students to sections in the first year of study in a Dutch business school, finding that women in sections with men with a higher average GPA (as measured prior to the start of the course) tend to choose a mathematical type major less frequently. Being in a section in which women have a high average GPA does not affect either men or women's choice of major, on the other hand. Mouganie and Wang (2017) study high-school students in China and find that high-performing male peers in tenth grade (defined as those scoring in the top 20 percent in the national high-school-entrance mathematics exam) reduce girls' likelihood of choosing a science track relative to an arts track for the remainder of high school.

3.6.3 Self Confidence and Aspirations

We next turn to psychological mechanisms such as self-confidence and aspirations/ambition more directly related to college attendance. For this, we can use three questions in Add Health. First, from the question "On a scale of 1 to 5, where 1 is low and 5 is high, how much do you want to go to college?", we create the variable "Want College", which equals 1 if the student reports that they want to go to college as a 5 and equals 0 otherwise.[38] Second, from the question "On a scale of 1 to 5, where 1 is low and 5 is high, how likely is it that you will go to college?" we create the variable "College likely" which equals 1 if the student says the likelihood that they go to college is a 5 and equals 0 otherwise.[39] Finally, from the question "Compared with other people your age, how intelligent are you?" (answers are: moderately below average, slightly below average, about average, slightly above average, moderately above average, extremely above average), we create the variable "Very intelligent" which equals 1 if the student reports that their intelligence level is "moderately above average" or "extremely above average" relative to others their own age and equals 0 otherwise.[40]

[38] In our sample, 75 percent of girls and 68 percent of males rate the amount that they want to go to college as a 5.

[39] In our sample, 61 percent of girls and 50 percent of boys answer 5. We also tried classifying the dependent variable as 1 if the individual answers either 4 or 5 for this question, with similar results.

[40] The results are similar with a dependent variable equal to 1 only if the individual answers that she is "extremely above average". Overall, 33 percent of females and 35 percent of males in our sample rate their intelligence as "moderately above average" or "extremely above average".

Since the above indicators of confidence and motivation are highly correlated, we use factor analysis to reduce the dimensionality of the dependent variables.[41] We perform factor analysis separately for males and females on the set of variables described above and use the first factor (the only one with an eigenvalue greater than 1) as an index of confidence and motivation. Table 3.11 shows the variance explained by the first two factors, their eigenvalues, and the factor loadings. As shown, the measures of desire to attend college and likeliness of attending college load most strongly. By construction, the index has a mean of 0 and a standard deviation of 1.

Column (1) in Table 3.12 reports the results of using the main specification with the confidence index as the dependent variable. As shown in the table, a one standard deviation increase in the proportion of "high-achieving" boys decreases girls' confidence and motivation by about 3 percent of a standard deviation; it has no effect on boys.[42] The effect of "high-achieving" girls is positive but statistically insignificant for both boys and girls irrespective of the specification of the main variable.

[41] Factor analysis is an orthogonal transformation that converts a set of correlated variables into a fewer number of orthogonal variables. Each of the confidence and motivation outcomes is then viewed as a function of the "latent" variables reflected in the factors.

[42] The results for each of the variables in the index separately are shown in Appendix Table 2.

Table 3.11: Confidence and Motivation Factor Loadings

(a) Females

	Eigenvalue	Proportion of Variance
Factor 1	1.65	0.55
Factor 2	0.88	0.29
Factor 3	0.47	0.16

Rotated Factor Loadings	Factor 1
Very Intelligent	0.51
College Likely	0.85
Want College	0.82

(b) Males

	Eigenvalue	Proportion of Variance
Factor 1	1.58	0.53
Factor 2	0.87	0.29
Factor 3	0.54	0.18

Rotated Factor Loadings	Factor 1
Very Intelligent	0.54
College Likely	0.82
Want College	0.79

Note: This table reports eigenvalues and factor loadings based on factor analysis of the following variables: "Want College", which equals 1 if the student reports that they want to go to college as a 5 on a scale of 1-5 and equals 0 otherwise; "College likely", which equals 1 if the student says the likelihood that they will go to college is a 5 on a scale of 1-5 and equals 0 otherwise; and "Very intelligent", which equals 1 if the student reports that their intelligence level is "moderately above average" or "extremely above average" relative to others their own age and equals 0 otherwise. Factor analysis performed separately for males and females. Wave IV weights used.

Table 3.12: High Achievers and Confidence and Risky Behaviors

	Females (1) Confidence Index	(2) Risky Index 1	(3) Risky Index 2	(4) Birth Before 18	Males (5) Confidence Index	(6) Risky Index 1	(7) Risky Index 2	(8) Birth Before 18
MF	-1.337**	1.397*	0.662	0.467***	0.111	0.175	-0.871	0.111
	(0.673)	(0.720)	(0.759)	(0.164)	(0.894)	(0.724)	(0.779)	(0.103)
FF	0.353	-1.346**	1.306**	0.354*	1.378*	-1.597**	-1.974**	-0.179**
	(0.712)	(0.674)	(0.607)	(0.183)	(0.780)	(0.698)	(0.914)	(0.081)
PVT Score	1.265***	-0.302*	-0.364*	-0.122***	0.955***	0.094	-0.266	-0.063**
	(0.166)	(0.169)	(0.187)	(0.045)	(0.167)	(0.155)	(0.174)	(0.028)
Fraction Female	-0.220	-0.247	-0.693	0.000	-0.518	0.573	-0.235	0.106
	(0.540)	(0.500)	(0.549)	(0.122)	(0.660)	(0.767)	(0.684)	(0.123)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Linear TT	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Peer Characteristics Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5631	5513	5513	5548	4685	4505	4505	4610
R^2	0.237	0.235	0.174	0.130	0.245	0.291	0.191	0.154
Adjusted R^2	0.199	0.196	0.132	0.086	0.199	0.247	0.140	0.102

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of confidence and risky behaviors on individual and peer characteristics. The Confidence Index is the first factor from a factor analysis of three variables measuring self-perceptions of intelligence, desire to go to college, and the likelihood of going to college. The Risky Index 1 (respectively, 2) is the first (respectively, second) factor from a factor analysis of 8 variables measuring risky behavior. First Birth Before 18 takes a value of 1 if the individual has had a child by the time she turns age 18 and 0 otherwise. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent). All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Individual controls include race dummies (Black, Latino, Asian, and other races), age in months, mother and father's education (dummies for each parent for high school, some college but no degree, college degree, and post college), and log family income. Peer characteristics controls include fraction foreign born, Black, Latino, Asian, and other races. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Coefficient on PVT score multiplied by 100. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

Table 3.13: Risky Behavior Factor Loadings

(a) Females

	Eigenvalue	Proportion of Variance
Factor1	2.97	0.37
Factor2	1.06	0.13
Factor3	0.97	0.12

Rotated Factor Loadings	Factor 1	Factor 2
Any Cigarette	0.61	0.20
Any Alcohol	0.75	0.04
Drunk	0.86	0.03
Binge	0.84	0.00
Any Marijuana	0.58	0.26
Fight	0.07	0.73
Arrested before 18	0.01	0.71
Unprotected Sex	0.35	0.18

(b) Males

	Eigenvalue	Proportion of Variance
Factor1	2.97	0.37
Factor2	1.12	0.14
Factor3	0.94	0.12

Rotated Factor Loadings	Factor 1	Factor 2
Any Cigarette	0.59	0.17
Any Alcohol	0.75	0.06
Drunk	0.88	0.06
Binge	0.87	0.02
Any Marijuana	0.55	0.23
Fight	0.07	0.73
Arrested before 18	0.04	0.74
Unprotected Sex	0.27	0.28

Note: This table reports eigenvalues and factor loadings based on factor analysis of the following variables: "any alcohol", which equals 1 if the individual has ever had more than a "couple of sips" of alcohol and equals 0 otherwise; "any cigarettes", which equals 1 if the individual has ever smoked cigarettes and equals 0 otherwise; "any marijuana", which equals 1 if the individual has smoked any marijuana in the past 30 days and equals 0 otherwise; "binge drinking", which equals 1 if the individual has had 5 or more drinks "in a row" in the past year and equals 0 otherwise; "drunk", which equals 1 if the individual reports being drunk in the past year and equals 0 otherwise; "fight", which equals 1 if the individual reports getting in a "serious physical fight" in the past year and 0 otherwise; "unprotected sex,", whick equals 1 if the individual did not use any form of birth control the most recent time she had sex and 0 otherwise; and arrest before 18, which equals 1 if the individual was arrested before age 18 and 0 otherwise. Factor analysis performed separately for males and females. Wave IV weights used.

3.6.4 Risky Behavior

Another channel that can affect the ability and desire to attend and graduate from college is the extent to which students engage in risky behavior. As shown by Elsner and Isphording (2018), students with lower-ranked PVT scores relative to their peers are more likely to engage in risky behavior.[43] We can use behavioral questions asked in Wave I to examine whether "high achievers" influence the extent to which individuals engage in risky behavior such as drug and alcohol use, unprotected sex, and smoking among others.

Since many of the indicators of risky behavior are highly correlated, we use factor analysis to reduce the dimensionality of the dependent variables. Using behavioral questions from Wave I, we consider the following outcomes: "any alcohol", which equals 1 if the individual has ever had more than a "couple of sips" of alcohol and equals 0 otherwise; "any cigarettes," which equals 1 if the individual has ever smoked cigarettes and equals 0 otherwise; "any marijuana," which equals 1 if the individual has smoked any marijuana in the past 30 days and equals 0 otherwise; "binge drinking," which equals 1 if the individual has had 5 or more drinks "in a row" in the past year and equals 0 otherwise; "drunk," which equals 1 if the individual reports being drunk in the past year and equals 0 otherwise; "fight," which equals 1 if the individual reports getting in a "serious physical fight" in the past year and 0 otherwise; "unprotected sex," which equals 1 if the individual did not use any form of birth control the most recent time she had sex and 0 otherwise; and arrest before 18 which equals 1 if the individual was arrested before age 18 and 0 otherwise.[44,45]

We perform factor analysis separately for males and females on the set of risky behaviors

[43] The authors did not distinguish effects by gender either of the individual or their peers. As can be seen, both the gender of the individual and her peers seems to matter – only "high-achieving" male peers affect girls' risky behavior.

[44] Since there is no question on arrests in Wave I, we use the Wave IV question: "How many times were you arrested before your 18th birthday?", and create a dummy equal to 1 if the individual was arrested before age 18.

[45] The means of these variables are reported in the base of Appendix Table 3.

described above and use the first two factors as indices of risky behavior (both have eigenvalues greater than 1).[46] Table 3.13 shows the variance explained by the first two factors, their eigenvalues, and the factor loadings. As shown, the various measures of alcohol use load most strongly for both girls and boys for the first factor (the first index of risky behavior). Physical fights and arrests load most strongly for the second factor.

Lastly, we explore an additional measure of behavior that may make college less likely: having a child before age 18. Wave IV asks individuals about the date (month and year) of each birth. We create a dummy variable for whether the individual first had a child before 18 years of age and set it equal to one if yes and to zero otherwise. In the sample, 7 percent of girls and 2 percent of boys reported having a child before age 18.

Table 3.12 reports the results using the full specification as usual. As shown in columns (2) and (3), exposure to a greater proportion of "high-achieving" boys increases girls' risky behavior as measured by index 1 (with no significance for the index 2). Furthermore, as shown in column (4), greater exposure also increases the probability of having a child before age 18. A one standard-deviation increase in MF is associated with an increase of almost 1 percentage point in this probability. FF, on the other hand, decreases the first index of risky behavior (associated with drinking) but increases the second index (associated with fighting and arrests) and the probability of having a child before age 18. This effect could be driven by "high-achieving" boys lowering girls' ability rank, as Elsner and Isphording (2018) find that lower PVT rank leads to greater risky behavior. To see whether this is the case, Table 3.12 repeats the analysis including a control for PVT rank calculated in the manner described in Section 5. The results given in Appendix Table 4 are very similar to those without rank control.

For boys, there is no significant impact of "high-achieving" boys as shown in columns (5)-(8). The effect of greater exposure to "high-achieving" girls is positive and significant: it

[46]The loadings are very similar for girls and boys.

reduces risky behavior as measured by both indices and also the likelihood of having a child before age 18.

Altogether, we take this as evidence that a possible pathway by which "high-achieving" boys affect girls is via increasing the latter's propensity to engage in risky behavior including having a child before the age of 18. Exactly why this happens, as discussed previously, we cannot determine as it may be a reaction to either teacher or student behavior.[47]

3.7 Heterogeneous Effects and Further Outcomes

The previous section showed that decreased confidence/aspirations and increased risky behavior were possible pathways leading girls to have worse long-run educational outcomes when exposed to a greater fraction of "high-achieving" boys in high school. We next examine heterogeneity in the main results, both with respect to individual/family characteristics and those of a school, as this will allow us to increase our understanding of the mechanisms at play. We also consider other long-run outcomes.

3.7.1 Heterogeneity

Individual ability and family background are strongly associated with the probability of graduating from college. To examine how these attributes matter we can split the sample according to (i) individual PVT score and (ii) parents' education levels. For PVT scores, we divide the sample into at-or-below the median and above the median PVT.[48] For parental

[47] If we include the measures of risky behavior or early motherhood as additional controls in our main regression with bachelor's degree as the dependent variable, the point estimate of MF is reduced in magnitude but remains significant. These regressions do not have a straightforward interpretation, however, these controls are themselves endogenous variables and hence we omit them.

[48] The median PVT score for females is 100 and we use this cutoff for both males and females to construct the samples.

education, we divide the sample into students who have at least one residential parent with any kind of college degree or higher levels of education than this, and those students whose parents have lower levels of education than a college degree.[49]

Table 3.14 displays the results. As can be seen in columns (1) and (2), higher levels of MF reduce the likelihood that girls will graduate with a bachelor's degree if their PVT is below the median. Specifically, a one standard deviation increase in MF (0.016 for girls with below-median PVT, always net of fixed effects and time trend) decreases bachelor's degree attainment by 2.3 percentage points for this group. This is a very large effect: 21 percent of girls in this group on average graduate with a bachelor's degree so this is over a 10 percent decrease. The effect on girls with an above-median PVT score is negative but not statistically significant.[50] A higher proportion of "high-achieving" girls, on the other hand increases college completion for below-median PVT females. From the magnitude of the coefficients, it is clear that an equal proportion of male and female "high-achievers" would have essentially a zero net effect on these girls. For girls with an above-median PVT score, there is no statistically significant effect from female "high-achievers."[51] This result is consistent with research suggesting that lower-ability females may be particularly positively influenced by higher-performing friends.[52] As can be seen in columns (7) and (8), there is no statistically significant effect of "high achievers" of either gender on boys.

Turning to parental education, we use the information on the highest level of education obtained by the residential mother and residential father as explained previously.[53] Contrasting column (3) with column (4), it is clear that the negative impact of "high-achieving" boys

[49]That is, if the student answers that either residential parent "graduated from a college or university" or has "professional training beyond a four-year college or university", this is coded as college degree for the purpose of this question.

[50]A Wald test, however, is unable to reject equality of the coefficients on MF across columns (1) and (2) (p value=0.30).

[51]In this case a Wald test can reject equality of the coefficient across the two columns at the 1 percent level (p value=0.003).

[52]See Hahn, Islam, Patacchini, and Zenou (2017).

[53]We exclude students for whom both parents' educational information is missing. If there is only one parent in the household, we use that parent's educational attainment.

Table 3.14: High Achievers and Bachelor's Degree Attainment: Heterogeneity

	Dependent Variable: Bachelor's Degree											
	Females						Males					
	Bel Med PVT	Ab Med PVT	Neither Parent Coll	Parent Coll	Bel Med Test	Ab Med Test	Bel Med PVT	Ab Med PVT	Neither Parent Coll	Parent Coll	Bel Med Test	Ab Med Test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
MF	-1.410***	-0.739	-0.038	-2.138***	-0.144	-1.351**	-0.192	0.188	-0.217	0.554	0.943	0.317
	(0.476)	(0.486)	(0.431)	(0.546)	(0.820)	(0.619)	(0.668)	(0.359)	(0.503)	(0.525)	(0.832)	(0.419)
FF	1.493***	-0.353	0.697*	0.325	0.357	1.156**	0.776	0.097	0.344	-0.015	-0.282	0.313
	(0.469)	(0.490)	(0.390)	(0.679)	(0.453)	(0.494)	(0.707)	(0.609)	(0.615)	(0.755)	(0.531)	(0.924)
PVT Score	0.465***	0.587***	0.505***	0.601***	0.508***	0.620***	-0.011	0.795***	0.374***	0.596***	0.412***	0.562***
	(0.119)	(0.201)	(0.083)	(0.142)	(0.091)	(0.102)	(0.104)	(0.161)	(0.085)	(0.164)	(0.103)	(0.121)
Fraction Female	0.331	0.583	0.348	0.442	0.210	0.420	-0.491	-0.635*	-0.525	-0.585	-1.530***	-0.437*
	(0.264)	(0.373)	(0.222)	(0.579)	(0.535)	(0.324)	(0.428)	(0.346)	(0.322)	(0.554)	(0.458)	(0.249)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Linear TT	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Peer Characteristics Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2913	2737	3443	1939	2245	2207	2093	2610	2744	1731	1842	1872
R^2	0.330	0.357	0.254	0.405	0.303	0.339	0.351	0.368	0.279	0.443	0.336	0.336
Adjusted R^2	0.263	0.290	0.192	0.313	0.266	0.297	0.258	0.296	0.203	0.345	0.292	0.286

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of bachelor's degree attainment on individual and peer characteristics. The dependent variable is equal to 1 if the individual has completed a bachelor's (four-year college) degree and 0 otherwise. Columns (1)-(2) and (7)-(8) split the sample into below-median and above-median PVT score. Columns (3)-(4) and (9)-(10) split the sample by whether at least one parent has a college degree. Columns (5)-(6) and (11)-(12) split the sample by the fraction of students at the individual's school testing at or above grade level. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent). All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Individual controls include race dummies (Black, Latino, Asian, and other races), age in months, mother and father's education (dummies for each parent for high school, some college but no degree, college degree, and post college), and log family income. Peer characteristics controls include fraction foreign born, Black, Latino, Asian, and other races. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Coefficient on PVT score multiplied by 100. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

is concentrated among girls with a college-educated parent; there is no effect on the other group of girls. The magnitude of the estimate for girls with a college-educated parent is twice the one we obtained previously for the entire sample. A one standard-deviation increase in MF (2.1 percentage points for this sample) leads to a 4.5 percentage point decrease in college completion. This is roughly a 7 percent decrease on a mean of 61 percent. This suggests that the negative impact of boys is precisely on those girls who, from a family-background perspective, would be most likely to attend and graduate from college. The impact of FF is restricted to girls whose parents do not have college degree. For this group, a one standard deviation increase in FF is associated with a 1.3 percentage point increase in the probability of obtaining a college degree. Lastly, note that the equivalent sample split for boys in columns (9) and (10) once again shows no significant effect of either male or female "high achievers."

Finally, we split the sample by an indicator of the socio-economic characteristics of the school rather than by individual characteristics. We rely on a question from the Add Health school administrator survey given to an administrator (e.g., the principal) in each sample school. Add Health asks the following question: "According to standardized achievement tests, approximately what percentage of all students at this school are testing: below grade level, at grade level, above grade level?" The fraction reported as testing at or above grade level is positively correlated with the average family income and average parental education level of students in the school.[54] The median fraction testing at or above grade level is 80 percent. We split schools according to whether they are strictly above this median or not.

Consistent with the results obtained when we split the sample by parental education, as can be seen comparing columns (5) and (6), the negative impact of "high-achieving" boys on girls appears in higher socio-economic/better performing schools. The effect of "high-achieving" girls is positive in this group as well. A one standard deviation increase in MF (2.0) is associated with a 2.7 percentage point decrease in bachelor's degree attainment – a 7 percent decrease on its mean of 36 percent. A one standard deviation increase in FF (1.8) increases bachelor's degree attainment by 2.1 percentage points (or 6 percent of the mean). For boys, we again find no impact of "high achievers" in either group of schools.

[54]The correlation between the fraction in the school testing at or above grade level and the median family income of the Wave I students in the school is 0.46.

3.7.2 Further Outcomes

Below we explore further the effect of "high achievers" on other long-term outcomes such as choice of major, labor force participation, fertility, and marriage.

In Wave III (when respondents are approximately 19-26), Add Health asks individuals who have some sort of college degree (including those with an associate or junior college degree (AA)) to list up to two major or minor fields of study.[55] We use this question to examine the impact of "high-achieving" peers on major/minor choice, creating a dummy variable equal to 1 if the individual reported a STEM field, where the classification of the latter is based on the National Science Foundation (NSF) Classification of STEM fields.[56] We restrict the sample to those individuals who were in at least grade 10 in Wave I (1995) and therefore would have been able to graduate with a BA by the summer of 2001 (Wave III). We focus on STEM fields because recent work has indicated that "high-achieving" males may affect women's likelihood of choosing or completing mathematics-intensive and/or STEM majors.[57]

Table 3.15, column (1) (respectively column (5) for males) examines the effect on choice of major using STEM major/minor as a dependent variable. We include the same controls as in our main specification and in addition control for whether the individual has completed a bachelor's degree (relative to a junior college degree) by Wave III. Although the coefficient on MF is negative, it is not statistically significant. There is, on the other hand, evidence that FF is associated with a higher proportion of STEM majors. The effect is large: a one standard deviation increase in FF (.019) is associated with an increase of 1.9 percentage points, a 13.2 percent increase over the mean of 14.5 percent.

Turning next to labor force participation, marriage, and fertility, we start by constructing

[55]Unfortunately, field of study was not asked in Wave IV.

[56]See $https://www.lsamp.org/help/help_stem_cip_2015.cfm$.

[57]E.g., Mouganie and Wang (2017) and Feld and Zölitz (2018).

Table 3.15: Other Outcomes

	Females				Males			
	(1) STEM Major	(2) LFP	(3) Ever Married	(4) Total No. Children	(5) STEM Major	(6) LFP	(7) Ever Married	(8) Total No. Children
MF	-0.202	-0.623**	-0.239	1.670**	0.115	-0.085	-0.237	-0.248
	(0.561)	(0.305)	(0.457)	(0.775)	(0.733)	(0.212)	(0.430)	(0.681)
FF	1.021*	0.465*	-0.022	-1.428	0.564	-0.078	-0.470	-1.908**
	(0.522)	(0.241)	(0.467)	(0.971)	(1.316)	(0.235)	(0.528)	(0.877)
PVT Score	0.153	0.102	-0.107	-0.962***	0.301	0.027	-0.013	-0.466***
	(0.160)	(0.070)	(0.075)	(0.205)	(0.338)	(0.057)	(0.095)	(0.168)
Fraction Female	0.547	-0.077	0.189	0.616	-3.223***	0.383**	-0.034	0.569
	(0.822)	(0.260)	(0.308)	(0.630)	(1.176)	(0.155)	(0.282)	(0.599)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Linear TT	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Peer Characteristics Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	909	4090	5649	5640	584	3275	4698	4678
R^2	0.298	0.100	0.202	0.222	0.455	0.161	0.205	0.208
Adjusted R^2	0.148	0.061	0.162	0.183	0.267	0.114	0.156	0.159

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of STEM field choice, labor force participation, marriage, and fertility on individual and peer characteristics. STEM major choice is equal to 1 if the individual lists a STEM field as one of her two major/minor fields of study and is equal to 0 otherwise. LFP is equal to 1 if the individual is currently working at least 10 hours per week, is on sick leave or temporarily disabled, is on maternity/paternity leave, or is unemployed and looking for work and is equal to zero otherwise. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent). All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Individual controls include race dummies (Black, Latino, Asian, and other races), age in months, mother's and father's education (dummies for high school, some college but no degree, college degree, post college for each parent), and log family income. Peer characteristics controls include fraction foreign born, Black, Latino, Asian, and other races. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Coefficient on PVT score multiplied by 100. Columns (1) and (5) also include an indicator for whether the individual has completed a bachelor's degree by Wave III. Sample for STEM field of study restricted to students in grades 10-12 in Wave I who have completed a postsecondary degree by Wave III. Sample for LFP restricted to students in grades 9-12 in Wave I. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

indicators or measures of each. For labor force participation, we create a dummy equal to one if an individual states that they are currently employed, are on sick leave or temporarily disabled, are on maternity/paternity leave, or are unemployed and looking for work; the dummy is set equal to zero otherwise.[58] For the purposes of this question, we exclude those

[58]Employment information is based on the question: "are you currently working for pay at least 10 hours per week?", where yes=1 and no=0. Individuals who report still working at their first full-time job are not asked this question so we code them as 1.

in the military or prison in Wave IV and restrict the sample to those individuals who were in 9th-12th grades in Wave I. This ensures that they are 28-32 years old by Wave IV, and thus likely to have completed all schooling.[59] Marriage and fertility are based on questions to all sample respondents in Wave IV about whether they have ever been married (1=yes, 0=no) and the total number of (non-deceased) biological children they have.

The results are shown in columns (2)-(4) (respectively, columns (6)-(8) for males) of Table 3.15. As shown, a one standard deviation increase in MF (0.023) is associated with a 1.4 percentage point decline in labor force participation of women.[60] In contrast, a one standard deviation increase in FF (0.021) is associated with higher labor force participation of a slightly smaller magnitude. There is no impact of "high achievers" on a woman's probability of having ever being married, but a one standard deviation increase in MF (0.020) increases a woman's total number of biological children by 0.03, a 3 percent increase over its mean of 1.07. There is no effect of FF on males' labor force participation or marriage, but a one standard deviation increase in FF decreases a man's total number of biological children by 0.04, a 5 percent decrease on a mean of 0.72.

3.8 Robustness Checks

In this section we investigate the sensitivity of the results to alternative definitions of the main variable, the exclusion of outliers, alternative specifications, and attrition.

A natural alternative formulation of the explanatory variable would be the fraction of "high achievers" of a given gender among *all* students in the grade. It would be interesting and potentially informative if this formulation gave different answers, but as shown the answers

[59]In total, 16 females and 75 males are dropped due to being in the military or prison in Wave IV.

[60]We also explore employment as an outcome, and find a significant negative impact of MF on employment. This appears to be driven primarily, however, by differences in the work behavior of those who say they are students in Wave IV.

are on the whole similar.[61] Table 3.16 repeats the most complete specification from Table 3.7 with alternative explanatory variables. In column (1) (respectively, column (3) for males) MF and FF replaced with MFA and FFA, where the A indicates that the high-achievers are measured as a fraction of the entire grade (All). As is clear, the negative impact of "high-achieving" males on females' bachelor degree completion is a robust result. Furthermore, there is no symmetric negative effect of "high-achieving" girls on boys' long-run education. Quantitatively, the coefficient on MFA indicates that a one standard deviation increase (0.011) in this fraction decreases females' bachelor's degree attainment by 1.7 percentage points.

An alternative definition would be to require both parents, rather than solely one, to have a post-college education. We denote these main variables MFB and FFB, for which each student receives a value of 1 if both parents have post-college education and 0 otherwise. This more restrictive version of high-achievers necessarily results in lower means (e.g., for females the mean of MFB is 0.040 versus 0.145 for MF; the mean of FFB is 0.035 versus 0.122 for FF). As indicated by the coefficients in column (2), a one standard deviation increase in MFB (1.1 percentage points) decreases females' college completion by 1.7 percentage points. There is no significant impact of FFB for girls nor of any of the main peer variables for boys. There is no statistically significant effect of high-achievers on boys' outcomes.

We next consider our results without school-specific linear time-trends. While these allow us to control for time-varying attributes of a school in a linear fashion, they also decreases the amount of variation in the main variables. Columns (1)-(2) (respectively, columns (5)-(6) for boys) of Table 3.17 display our main results without time trends. As is clear from the table, the results remain strong and significant. A one standard deviation increase in MF for females (0.027) decreases the likelihood of graduating with a bachelor's degree by 2.3 percentage points, a similar magnitude to that obtained for our main specification (Table

[61] For example, if we found that the results only hold when using MF and FF then we would be inclined to think that the pathway would be related to the formation of a perception about males.

Table 3.16: High Achievers and Bachelor's Degree Attainment: Alternative Measures

	Dependent Variable: Bachelor's degree			
	Females		Males	
	(1)	(2)	(3)	(4)
MFA	-1.579**		0.208	
	(0.644)		(0.651)	
FFA	-0.198		0.544	
	(0.687)		(0.859)	
MFB		-1.537**		-0.097
		(0.680)		(0.783)
FFB		0.511		-0.081
		(0.570)		(0.749)
PVT Score	0.535***	0.531***	0.449***	0.449***
	(0.064)	(0.064)	(0.070)	(0.070)
Fraction Female	0.307	0.539**	-0.611**	-0.592**
	(0.257)	(0.209)	(0.286)	(0.267)
School, Grade FE	Yes	Yes	Yes	Yes
School Linear TT	Yes	Yes	Yes	Yes
Individual Characteristics Controls	Yes	Yes	Yes	Yes
Peer Characteristics Controls	Yes	Yes	Yes	Yes
Observations	5650	5650	4703	4703
R^2	0.333	0.333	0.328	0.328
Adjusted R^2	0.300	0.300	0.287	0.287

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of bachelor's degree attainment on individual and peer characteristics. The dependent variable is equal to 1 if the individual has completed a bachelor's (four-year college) degree and 0 otherwise. MFA (respectively, FFA) represents male (respectively, female) "high achievers" (those with at least one post-college parent) as a fraction of all students in the grade. MFB (respectively, FFB) is represents male (respectively, female) "high achievers" (those with two post-college parents) as a fraction of males (respectively, females) in the grade. All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Individual controls include race dummies (Black, Latino, Asian, and other races), age in months, mother and father's education (dummies for each parent for high school, some college but no degree, college degree, and post college), and log family income. Peer characteristics controls include fraction foreign born, Black, Latino, Asian, and other races. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Coefficient on PVT score multiplied by 100. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

3.7).

We can also examine the robustness of the results by eliminating outliers. As discussed in Section 4, the great majority of schools in our sample have variation in MF and FF similar to with what would be expected by chance. For about 10 percent of schools, however, the variation lies outside the 90 percent confidence interval obtained through the Monte Carlo simulation. To test whether these outliers are driving the results, we re-run our main specifications [columns (5)-(6) of Table 3.7] excluding the schools whose within-school standard deviations of MF and FF lie outside of the 90 percent confidence interval. The results are shown in columns (3)-(4) (respectively, columns (7)-(8) for boys) of Table 3.17. As shown, the magnitude and significance of the coefficients are similar to what we obtained previously.

Lastly, we can also examine whether the results are due to differential attrition between Wave I and Wave IV as this could generate bias in the results. To do so, we regress a dummy for whether the individual remained in the sample between Wave I and Wave IV on the main variables. As shown in Appendix Table 5, there is no significant association between these and the dummy, suggesting that the results are unlikely to be driven by selective attrition of specific types of girls and boys from the sample.

Table 3.17: High Achievers and Bachelor's Degree Attainment: Robustness

	Dependent Variable: Bachelor's degree							
	Females				Males			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MF	-0.851***	-0.823***	-1.148***	-1.001***	0.201	0.178	0.151	0.108
	(0.272)	(0.247)	(0.355)	(0.325)	(0.295)	(0.298)	(0.465)	(0.482)
FF	0.047	0.055	0.163	0.194	-0.042	-0.111	0.375	0.389
	(0.266)	(0.262)	(0.326)	(0.323)	(0.370)	(0.365)	(0.530)	(0.528)
PVT Score	0.544***		0.527***		0.469***		0.426***	
	(0.064)		(0.067)		(0.069)		(0.075)	
Fraction Female	0.388***	0.291**	0.461**	0.343	0.011	-0.001	-0.611**	-0.592**
	(0.143)	(0.143)	(0.231)	(0.229)	(0.214)	(0.216)	(0.293)	(0.292)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Linear TT	No	No	Yes	Yes	No	No	Yes	Yes
Individual Characteristics Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Peer Characteristics Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5650	5898	5098	5326	4703	4948	4135	4364
R^2	0.308	0.292	0.337	0.321	0.289	0.274	0.328	0.313
Adjusted R^2	0.289	0.274	0.303	0.288	0.266	0.251	0.286	0.273

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of bachelor's degree attainment on individual and peer characteristics. The dependent variable is equal to 1 if the individual has completed a bachelor's (four-year college) degree and 0 otherwise. Columns (1)-(2) (respectively (5)-(6) for males) exclude linear time trends. Columns (3)-(4) (respectively (7)-(8) for males) exclude schools with variation in MF and FF outside of the 90 percent confidence interval obtained in the Monte Carlo simulations. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent). All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Individual controls include race dummies (Black, Latino, Asian, and other races), age in months, mother and father's education (dummies for each parent for high school, some college but no degree, college degree, and post college), and log family income. Peer characteristics controls include fraction foreign born, Black, Latino, Asian, and other races. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Coefficient on PVT score multiplied by 100.

3.9 Conclusion

This paper investigated the long-run effects of exposure to "high-achieving" boys versus girls in high school. Using a predetermined student characteristic – whether at least one of their parents has some post-college education – to proxy for a bundle of student characteristics, we investigated the consequences of quasi-random variation in the proportion of "high-achieving" girls and boys, separately, across grades within the same school. As shown, we found a very strong asymmetric gender effect: the proportion of "high-achieving" boys has a statistically and economically significant negative effect on the probability that girls will end up with a bachelor's degree some 14 years later. Boys, on the other hand, are not affected by their exposure to "high-achievers" of either gender.

Our paper suggests that in the future it would be useful to conduct experiments that examine how female subjects vs male subjects are affected by competing with varying proportions of high-performing males vs high-performing females. Are asymmetric gender effects also present there? Are any particular interventions helpful? It would also be of interest to examine whether the results the paper obtained are present in other data sets and in contexts other than high schools.

The data does not allow us to distinguish among various potential mechanisms. It could be that interacting with "high-achieving" boys has a direct negative effect on fellow female students. Or, the effect could be more indirect, e.g., arising from how teachers react to these students or even from how the parents of these boys affect teachers or the allocation of resources at the grade level. Nonetheless, we can identify some of the pathways. In particular, we show that greater exposure to "high-achieving" boys decreases girls' self confidence and aspirations and increases their risky behavior including increasing teen-age motherhood. The girls especially affected are those in the lower half of the ability distribution as measured by their PVT score. Policies that target these more marginal girls – that increase their ambition and self-confidence or that decrease their exposure to these boys – are likely to have beneficial effects. Furthermore, our findings suggest that exposure to "high-achieving" girls has a positive effect of essentially the same absolute magnitude indicating that the negative effects of "high-achieving" boys can be counterbalanced through exposure to their female counterparts.

APPENDIX A

APPENDIX TO CHAPTER 1

Table A.1: Difference in CPS Voting Rates and Official Ballots Counted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
State Unemployment Rate	-0.012								
	(0.077)								
State E-P Ratio, W M LEHS		-0.010							
		(0.049)							
State E-P Ratio, W F LEHS			-0.010						
			(0.036)						
State E-P Ratio, B M LEHS				-0.005					
				(0.010)					
State E-P Ratio, B F LEHS					0.003				
					(0.008)				
State E-P Ratio, W M Any College						-0.046			
						(0.092)			
State E-P Ratio, W F Any College							-0.026		
							(0.043)		
State E-P Ratio, B M Any College								-0.004	
								(0.013)	
State E-P Ratio, B F Any College									-0.012
									(0.012)
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	850	700	700	683	670	700	700	694	691
R^2	0.594	0.598	0.598	0.596	0.593	0.598	0.598	0.597	0.597

Note: This table shows parameter estimates and standard errors (in parentheses) from an OLS regression in which the dependent variable is the difference between the CPS estimated voter turnout and estimates based on the official counts of votes for the highest office. Observations are at the state-year level. All regressions also include indicators for concurrent governor and senate elections. Regressions are weighted by the population over age 18 in each state-year cell. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

Table A.2: Characteristics of Demographic-State Groups by Industry Composition

	(1) Employment Demand Index	(2) Share Operator B Man	(3) Share Operator LT Man	(4) Share Trans B Man	(5) Share Trans LT Man
Fraction Married 1984	-0.446	-0.0462	0.0178	-0.0104	0.00958
	(0.561)	(0.0320)	(0.0467)	(0.0185)	(0.0187)
Average Number of Children 1984	-0.210	0.0154	-0.00153	0.00684	0.00480
	(0.178)	(0.0133)	(0.0122)	(0.00517)	(0.00422)
Fraction Unionized 1984	-0.654	0.0327	-0.00842	0.0190	-0.0141
	(0.427)	(0.0290)	(0.0168)	(0.0145)	(0.0102)
Group x Year FE	Yes	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes	Yes
Observations	5117	5117	5117	5117	5117
R^2	0.835	0.800	0.654	0.822	0.773

Note: This table shows parameter estimates and standard errors (in parentheses) from an OLS regression in which the dependent variables are listed in the columns. The labor demand index is at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for the fraction of the population in five-year ago groups. Regressions are weighted by the population in each demographic group-state-year cell. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

Table A.3: Placebo Tests

	(1)	(2)	(3)	(4)
t+2 Employment Demand Index	0.393	0.0171		
	(0.389)	(0.411)		
t+4 Employment Demand Index			0.387	0.0877
			(0.520)	(0.521)
Employment Demand Index		0.732*		0.714*
		(0.433)		(0.399)
Group x State FE	Yes	Yes	Yes	Yes
Group x Year FE	Yes	Yes	Yes	Yes
State x Year FE	Yes	Yes	Yes	Yes
Observations	4816	4816	4515	4515
R^2	0.948	0.948	0.948	0.948
Adjusted R^2	0.932	0.932	0.931	0.931

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (5). The labor demand index is at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include controls for whether voting behavior is reported by self or proxy and whether the individual has a high school degree and a bachelor's degree (or, in columns (4)-(5) the group average of these values). Regressions are weighted by the population in each demographic group-state-year cell. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

Table A.4: Predictive Power of State Labor Demand Index

	(1) State Unemployment Rate	(2) State Prime Age E-P Ratio
State Labor Demand Index	-0.864***	0.171
	(0.256)	(0.432)
State FE	Yes	Yes
Year FE	Yes	Yes
Observations	850	850
R^2	0.808	0.822
Adjusted R^2	0.790	0.805

Note: This table shows parameter estimates and standard errors (in parentheses) from OLS regressions in which the dependent variable is listed in the column heading on the state labor demand index. Observations are at the state-year level and regressions are weighted by the population in each state-year cell. All columns include controls for the fraction ages 25-54, 55-64, and 65 and over; the fraction black, Hispanic, and white; and the fraction with a high school degree, some college, and a college degree. Coefficients multiplied by 100. Regressions are weighted by the population in each state-year cell. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

Table A.5: Effect of Labor Demand Index on Employment and Voting in ANES Data

	(1) Employed	(2) Voted
Labor Demand Index	3.062*	4.668**
	(1.698)	(2.262)
State Unemployment Rate	176.148	148.500
	(114.241)	(100.270)
Group x State FE	Yes	Yes
Education x Year FE	Yes	Yes
Observations	7106	6753
R^2	0.125	0.226
Years	1990-2016	1990-2016

Note: This table shows parameter estimates and standard errors (in parentheses) from estimating equation (5) with the dependent variables listed in the column headings. The labor demand index is at the demographic group-state-year level. The demographic-state groups are defined by education (less than or equal to high school, any college) x race (black, white) x gender (male, female), and state. All columns also include a quadratic control for age. Coefficients multiplied by 100. Standard errors clustered at the state level. * p<0.1 ** p<0.05 *** p<0.01

Table A.6: Summary Statistics for CPS Volunteer Supplement

	All		Males		Females	
	mean	sd	mean	sd	mean	sd
Age	40.08	8.60	40.07	8.61	40.09	8.58
Non-Hispanic Black	0.06	0.23	0.04	0.19	0.07	0.26
Any College	0.61	0.49	0.58	0.49	0.64	0.48
Married	0.66	0.47	0.65	0.48	0.67	0.47
Any Child in HH	0.58	0.49	0.53	0.50	0.62	0.48
Number of Children in HH	1.14	1.24	1.05	1.23	1.23	1.24
Employed	0.83	0.38	0.90	0.30	0.76	0.43
Employed 35+ Hours	0.67	0.47	0.80	0.40	0.57	0.50
Weekly Hours	32.94	18.18	38.52	16.02	27.96	18.54
No. of Volunteer Orgs	0.47	0.85	0.39	0.78	0.54	0.91
Hours Volunteered	21.76	48.62	18.85	46.10	24.43	50.67
No. of Civic or Political Orgs	0.02	0.17	0.02	0.17	0.02	0.17
Public Meeting	0.10	0.30	0.10	0.30	0.11	0.31
Community Project	0.09	0.29	0.09	0.29	0.09	0.28
Observations	727353		347612		374770	

Source: Author calculations from Current Population Survey Volunteer Supplement 2002-2015, downloaded from Flood et al. (2015).

APPENDIX B

APPENDIX TO CHAPTER 2

Table B.1: Data Description, Females with a Younger Sibling

Variable	Mean	Std Deviation	Description
Wave I			
White	0.67	0.47	Dummy variable equal to 1 if respondent reported being white
Black	0.18	0.39	Dummy variable equal to 1 if respondent reported being black
Latino	0.11	0.32	Dummy variable equal to 1 if respondent reported being Hispanic/Latino
Asian	0.01	0.1	Dummy variable equal to 1 if respondent reported being Asian
Other Race	0.03	0.16	Dummy variable equal to 1 if respondent reported being of another race (not white, black, Latino, or Asian)
Mother is Immigrant	0.08	0.26	Dummy variable equal to 1 if residential mother is an immigrant
Father is Immigrant	0.09	0.29	Dummy variable equal to 1 if residential father is an immigrant
Immigrant Parent	0.12	0.32	Dummy variable equal to 1 if at least one parent is an immigrant
Age	15.14	1.72	Respondent's age in years as of June 1, 1995
Mother's Age	38.52	4.69	Respondent's mother's age in years
Mother HS Graduate	0.79	0.4	Dummy variable equal to 1 if respondent's mother has a high school diploma
Mother College Graduate	0.22	0.41	Dummy variable equal to 1 if respondent's mother has a college degree
Father HS Graduate	0.81	0.39	Dummy variable equal to 1 if respondent's father has a high school diploma
Father College Graduate	0.21	0.41	Dummy variable equal to 1 if respondent's father has a college degree
Birth Order	1.45	0.83	Birth order (among children of biological parents)
Total Siblings in HH	1.91	1.1	Total Siblings in HH in Wave I
Wave IV			
High School Graduate	0.92	0.27	Dummy variable equal to 1 if respondent has a high school diploma
College Graduate	0.33	0.47	Dummy variable equal to 1 if respondent has a college degree
Weekly Hours Worked	40.95	10.51	Weely Hours Worked in Current or Most Recent Job (if over 10 Hours)
Works 35+ Hours	0.62	0.48	Dummy variable equal to 1 if respondent has current job and works at least 35 hours per week
Any Earnings	0.87		Dummy variable equal to 1 if respondent reports any earnings over $2000 in the previous year
Average Earnings	31808.16	33145.95	Average personal earnings in the previous year (if respondent reports any earnings over $2000)
Median Earnings	28000		Median personal earnings in the previous year (if respondent reports any earnings over $2000)
Observations	3600		

Note: See notes to Table 2.2

APPENDIX C

APPENDIX TO CHAPTER 3

Table C.1: Balance Tests, MN and FN

Panel A, Females						
	Log Family Income	PVT Score	Mother Not in HH	Father Not in HH	Black	Age in Months
IHS MN	-0.0143	-0.462	0.000	-0.030	0.015	0.232
	(0.057)	(0.822)	(0.014)	(0.028)	(0.021)	(0.554)
IHS FN	0.003	-0.464	0.017	-0.023	0.023*	-0.197
	(0.038)	(0.735)	(0.012)	(0.026)	(0.013)	(0.372)
Own Parent Post College	0.310***	5.170***	-0.009	-0.018	0.004	-1.133***
	(0.036)	(0.715)	(0.007)	(0.019)	(0.019)	(0.295)
Panel B, Males						
	Log Family Income	PVT Score	Mother Not in HH	Father Not in HH	Black	Age in Months
IHS MN	0.009	0.859	0.022	0.011	0.026	-0.626
	(0.101)	(1.033)	(0.017)	(0.042)	(0.023)	(0.562)
Own Parent Post College	0.278***	4.200***	-0.014	-0.007	0.029*	-1.170***
	(0.043)	(0.616)	(0.010)	(0.019)	(0.015)	(0.293)
IHS FN	-0.012	-0.208	-0.015	-0.046*	0.011	0.147
	(0.040)	(0.780)	(0.016)	(0.024)	(0.012)	(0.449)

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of MN and FN on individual characteristics. The estimates displayed in each row are for separate regressions in which the dependent variable is the variable name in the column and the independent variable is displayed in the row. MN (respectively, FN) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent) as defined in the text and IHS is the inverse hyperbolic sine transformation. The regressions of FN in Panel A and MN in Panel B include a control for whether the individual has at least one post-college parent. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. All regressions are unweighted. Standard errors clustered at the school level. * $p<0.1$ ** $p<0.05$ *** $p<0.01$

Table C.2: High Achievers and Confidence and Motivation

	Females			Males		
	(1)	(2)	(3)	(4)	(5)	(6)
	Very Intelligent	College Likely	Want College	Very Intelligent	College Likely	Want College
MF	-0.288	-0.485	-0.545*	0.251	-0.589	0.507
	(0.358)	(0.322)	(0.297)	(0.426)	(0.399)	(0.456)
FF	-0.125	0.113	0.289	0.802**	0.785	-0.015
	(0.407)	(0.335)	(0.335)	(0.404)	(0.481)	(0.380)
PVT Score	0.649***	0.433***	0.317***	0.818***	0.120	0.226**
	(0.094)	(0.076)	(0.072)	(0.084)	(0.086)	(0.089)
Fraction Female	0.004	-0.064	-0.127	-0.002	-0.324	-0.174
	(0.229)	(0.275)	(0.282)	(0.283)	(0.328)	(0.331)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes
School Linear TT	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes
Peer Characteristics Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5642	5636	5641	4695	4691	4694
R^2	0.167	0.224	0.144	0.212	0.219	0.191
Adjusted R^2	0.125	0.185	0.102	0.164	0.171	0.142

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of measures of confidence and motivation on individual and peer characteristics. "Want College" equals 1 if the student reports that they want to go to college as a 5 on a scale of 1-5 and equals 0 otherwise; "College likely" equals 1 if the student says the likelihood that they go to college is a 5 on a scale of 1-5 and equals 0 otherwise. "Very intelligent" equals 1 if the student reports that their intelligence level is "moderately above average" or "extremely above average" relative to others their own age and equals 0 otherwise. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent). All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Individual controls include race dummies (Black, Latino, Asian, and other races), age in months, mother and father's education (dummies for each parent for high school, some college but no degree, college degree, and post college), and log family income. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Peer characteristics controls include fraction foreign born, Black, Latino, Asian, and other races. Coefficient on PVT score multiplied by 100. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01.

Table C.3: High Achievers and Risky Behaviors

	Females								Males							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
	Cigarettes	Alcohol	Drunk	Binge	Marijuana	Fight	Arrest	Unpr Sex	Cigarettes	Alcohol	Drunk	Binge	Marijuana	Fight	Arrest	Unpr Sex
MF	1.004**	0.315	0.656*	0.433	0.267	0.368	-0.031	0.202	0.618	0.184	-0.217	-0.183	-0.029	-0.532	-0.206	-0.126
	(0.414)	(0.307)	(0.395)	(0.322)	(0.249)	(0.247)	(0.124)	(0.342)	(0.428)	(0.330)	(0.359)	(0.357)	(0.303)	(0.470)	(0.238)	(0.197)
FF	-0.088	-0.718*	-0.824**	-0.491	-0.151	-0.106	0.201*	0.720**	-0.752	-0.518*	-0.454	-0.650	-0.354	-0.902*	-0.462*	-0.189
	(0.410)	(0.397)	(0.361)	(0.352)	(0.266)	(0.248)	(0.108)	(0.342)	(0.522)	(0.295)	(0.351)	(0.458)	(0.377)	(0.491)	(0.269)	(0.272)
PVT Score	-0.209**	-0.091	-0.097	-0.120	-0.055	-0.261***	0.021	-0.129**	-0.086	0.185**	0.042	0.013	0.012	-0.098	0.004	-0.139**
	(0.088)	(0.083)	(0.086)	(0.080)	(0.052)	(0.062)	(0.032)	(0.055)	(0.088)	(0.091)	(0.066)	(0.072)	(0.053)	(0.083)	(0.055)	(0.065)
Fraction Female	-0.058	-0.324	-0.156	-0.247	0.061	-0.704***	0.012	0.052	0.782*	0.202	0.022	-0.013	0.044	-0.036	-0.140	-0.078
	(0.264)	(0.218)	(0.280)	(0.257)	(0.155)	(0.206)	(0.091)	(0.222)	(0.413)	(0.339)	(0.350)	(0.365)	(0.214)	(0.295)	(0.198)	(0.174)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Linear TT	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Peer Characteristics Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5630	5626	5612	5608	5594	5629	5628	5598	4678	4681	4652	4656	4626	4676	4666	4642
R^2	0.166	0.198	0.191	0.171	0.135	0.156	0.122	0.143	0.173	0.202	0.240	0.261	0.157	0.156	0.159	0.149
Adjusted R^2	0.124	0.158	0.151	0.130	0.091	0.113	0.078	0.100	0.122	0.154	0.193	0.216	0.106	0.105	0.108	0.097
Dep Var Mean	0.586	0.563	0.275	0.237	0.121	0.218	0.027	0.126	0.574	0.565	0.279	0.275	0.135	0.406	0.111	0.102

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of measures of risky behaviors on individual and peer characteristics. The dependent variables are defined as follows: "any cigarettes" equals 1 if the individual has ever smoked cigarettes and equals 0 otherwise; "any alcohol" equals 1 if the individual has ever had more than a "couple of sips" of alcohol and equals 0 otherwise; "drunk" equals 1 if the individual reports being drunk in the past year and equals 0 otherwise; "binge drinking" equals 1 if the individual has had 5 or more drinks "in a row" in the past year and equals 0 otherwise; "any marijuana" equals 1 if the individual has smoked any marijuana in the past 30 days and equals 0 otherwise; "fight" equals 1 if the individual reports getting in a "serious physical fight" in the past year and 0 otherwise; "arrest before 18" equals 1 if the individual was arrested before age 18 and 0 otherwise; and "unprotected sex," equals 1 if the individual did not use any form of birth control the most recent time she had sex and 0 otherwise. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent) as described in the text. All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Individual controls include race dummies (Black, Latino, Asian, and other races), age in months, mother and father's education (dummies for each parent for high school, some college but no degree, college degree, and post college), and log family income. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Peer characteristics controls include fraction foreign born, Black, Latino, Asian, and other races. Coefficient on PVT score multiplied by 100. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01.

Table C.4: Confidence and Risky Behaviors with Controls for PVT Rank

	Females (1) Confidence Index	(2) Risky Index 1	(3) Risky Index 2	(4) Birth Before 18	Males (5) Confidence Index	(6) Risky Index 1	(7) Risky Index 2	(8) Birth Before 18
MF	-1.283*	1.325*	0.628	0.457***	0.113	0.170	-0.874	0.111
	(0.676)	(0.685)	(0.760)	(0.161)	(0.912)	(0.720)	(0.786)	(0.104)
FF	0.413	-1.440**	1.261**	0.343*	1.378*	-1.597**	-1.974**	-0.179**
	(0.707)	(0.680)	(0.602)	(0.185)	(0.787)	(0.698)	(0.913)	(0.082)
PVT Score	0.471	0.913***	0.210	0.027	0.261	0.777*	0.150	-0.099
	(0.375)	(0.341)	(0.522)	(0.097)	(0.481)	(0.441)	(0.422)	(0.073)
Fraction Female	-0.340	-0.064	-0.607	0.024	-0.568	0.626	-0.202	0.104
	(0.542)	(0.511)	(0.562)	(0.121)	(0.658)	(0.789)	(0.689)	(0.125)
PVT Rank	0.352**	-0.540***	-0.255	-0.068*	0.309	-0.303*	-0.185	0.016
	(0.153)	(0.148)	(0.182)	(0.040)	(0.190)	(0.179)	(0.171)	(0.030)
School, Grade FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
School Linear TT	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Peer Characteristics Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5630	5512	5512	5547	4685	4505	4505	4610
R^2	0.237	0.239	0.175	0.131	0.246	0.293	0.191	0.154
Adjusted R^2	0.199	0.200	0.133	0.087	0.200	0.248	0.140	0.102

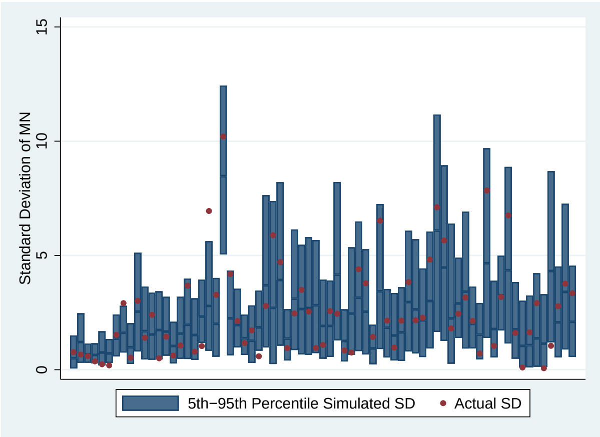
Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of measures of confidence and motivation and risky behaviors on individual and peer characteristics. The Confidence Index is the first factor from a factor analysis of three variables measuring self-perceptions of intelligence, desire to go to college, and likelihood of going to college. The Risky Index 1 (respectively, 2) is the first (respectively, second) factor from a factor analysis of 8 variables measuring risky behaviors. First Birth Before 18 takes a value of 1 if the individual has had a child by the time she turns age 18 and 0 otherwise. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent). All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Individual controls include race dummies (Black, Latino, Asian, and other races), age in months, mother's and father's education (dummies for high school, some college but no degree, college degree, post college for each parent), and log family income. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Peer characteristics controls include fraction foreign born, Black, Latino, Asian, and other races. Coefficient on PVT score multiplied by 100. The PVT percentile rank of the student is calculated by taking the absolute rank of each student relative to others in her grade and school in the in-home sample (with the worst-performing student having a value of 1) and then converting into a percentile. Wave IV weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01

Table C.5: Sample Attrition

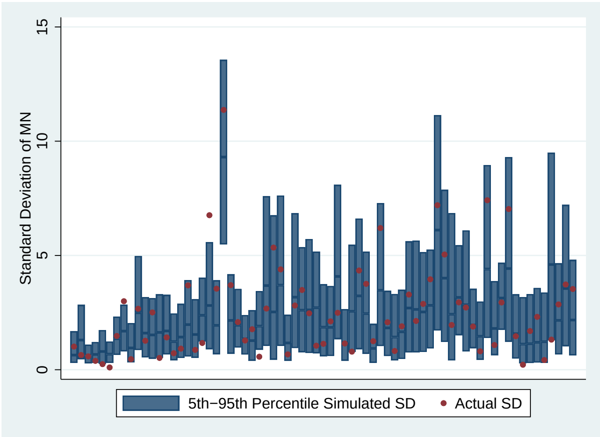
	Dependent Variable: In Wave IV Sample			
	Females		Males	
	(1)	(2)	(3)	(4)
MF	0.216		0.333	
	(0.259)		(0.310)	
FF	0.339		-0.503	
	(0.248)		(0.433)	
MN		0.029		0.028
		(0.027)		(0.032)
FN		0.023		-0.052
		(0.022)		(0.032)
PVT Score	0.076	0.076	0.162**	0.163**
	(0.057)	(0.057)	(0.068)	(0.068)
Fraction of Peers who are Female	0.099	0.090	0.341	0.457*
	(0.178)	(0.172)	(0.256)	(0.274)
School, Grade FE	Yes	Yes	Yes	Yes
School Linear TT	Yes	Yes	Yes	Yes
Individual Controls	Yes	Yes	Yes	Yes
Peer Characteristics Controls	Yes	Yes	Yes	Yes
Observations	6878	6876	6250	6250
R^2	0.152	0.146	0.144	0.145

Note: This table reports parameter estimates and standard errors (in parentheses) for regressions of being in the Wave IV sample (conditional on being in Wave I) on individual and peer characteristics. MF (respectively, FF) is the fraction of male (respectively, female) "high achievers" (those with at least one post-college parent) as described in the text. All columns include a dummy for whether Wave I interview took place in 1994-1995 or 1995-1996 school year. Individual controls include race dummies (Black, Latino, Asian, and other races), age in months, mother and father's education (dummies for each parent for high school, some college but no degree, college degree, and post college), and log family income. If mother's (respectively, father's) education is missing, all mother's (respectively, father's) education dummies are set to zero and a dummy is included for missing mother's (respectively, father's) education. If family income is missing, family income is set to the mean value for the school and a dummy is included for missing family income. Peer characteristics controls include fraction foreign born, Black, Latino, Asian, and other races in columns (1) and (3) and the IHS transformation of the count of peers who are foreign born, Black, Latino, Asian, and other races in columns (2) and (4). Wave I weights used. Standard errors clustered at the school level. * p<0.1 ** p<0.05 *** p<0.01.

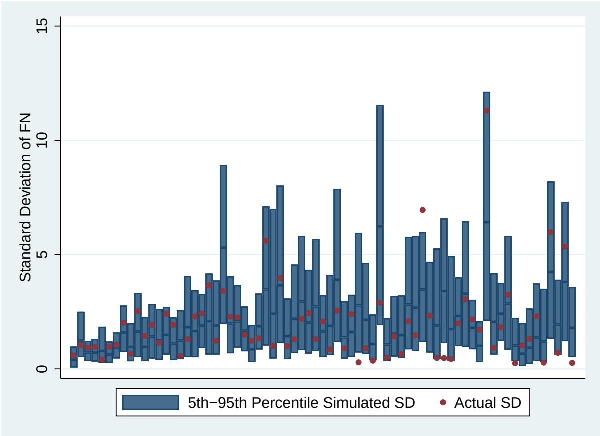
Figure C.1: Monte Carlo Estimates of MN and FN



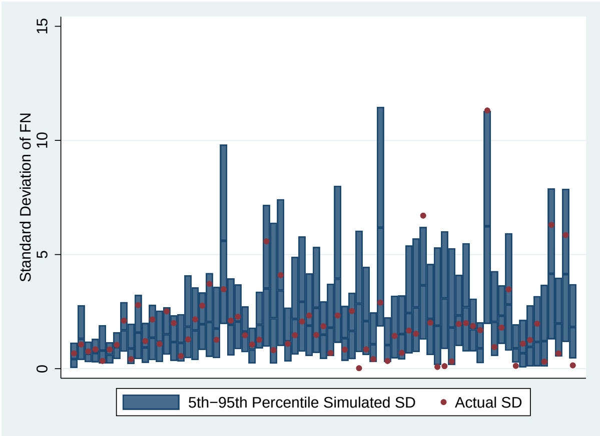
(a) Females: MN



(b) Males: MN



(c) Females: FN



(d) Males: FN

Note: These figures display simulated and actual standard deviations for schools in the sample with at least three grades, with each bar representing a different school. Upper and lower edges of the bar represent the 5th and 95th percentiles respectively of the simulated within-school standard deviation of MN (or FN). The dot represents the empirical standard deviation.

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