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Essays in Political Economy and Economic Sociology

by Adrian Mehic

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Essays in Political Economy and Economic Sociology

Abstract

This thesis consists of four self-contained papers within political economy and economic sociology. The first paper studies how the 2015 refugee wave impacted the vote share of the anti-immigration Sweden Democrats (SD) during the 2018 Swedish parliamentary elections. I find that while there is an overall positive effect of immigration on the SD votes, the magnitude of the effect differs considerably depending on pre-influx municipal characteristics.

In the second paper, I study how releases of radioactive fallout from the 1986 Chernobyl nuclear accident affected environmentalist voting in subsequent elections in Sweden. The results show that in municipalities affected by fallout, green voting increased. The Chernobyl premium om the green vote remained for around one decade after the accident. Detailed-individual level survey data suggests that opposition to nuclear power increased in affected areas after the accident.

The third paper examines how implementing blended learning at university affects grade outcomes when compared to online and campus teaching. The results show that female students with affluent parents were the relative winners of blended learning, both compared to other groups who were subject to blended learning, but also compared to their own performance when all teaching is on campus. Survey data suggests that this group of students have broader social networks, which facilitates communication with peers and improves grades.

In the fourth paper, I analyze how the academic performance of university students is affected by their own and their peers' socioeconomic status and beauty. The results suggest that a student's own socioeconomic status and beauty affect grades positively, and that these traits in peers also have a positive impact on grades. For the peer effects, I provide evidence for both a direct spillover channel, and for an indirect channel according to which peer beauty improves well-being among students.

Key words

voting; immigration; Chernobyl; nuclear power; COVID-19; blended learning; socioeconomy; beauty; peer effects

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Essays in Political Economy and Economic Sociology

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MADE IN SWEDEN

To my parents

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Abstract

This thesis consists of four self-contained papers on political economy and economic sociology. Below, I outline each paper briefly.

The first paper, *Regional aspects of immigration-related changes in political preferences*, studies how the 2015 refugee wave impacted the vote share of the Sweden Democrats (SD) during the 2018 Swedish parliamentary elections. To create exogenous variation in the share of refugees in municipalities, I utilize a placement program, in which municipalities cannot refuse participation. The results show that a higher share of refugees led to a higher share of SD votes, but that there were important differences between municipalities depending on pre-influx characteristics. These heterogeneous effects were only statistically significant when considering the immigration of a particular group of refugees dominated by young males. In municipalities with high crime rates before the refugee wave, and in municipalities with significant support for the populist New Democracy party during the 1992 refugee wave, the immigration of young males contributed to an additional increase in SD voting. On the other hand, in depopulating areas, the immigration-related growth in the SD vote was smaller.

The second paper, *The Electoral Consequences of Environmental Accidents: Evidence from Chernobyl*, discusses how radioactive fallout following the 1986 Chernobyl disaster affected the vote share of the Swedish Green Party (MP), which was elected into parliament in 1988. The fallout data comes from aerial measurements of ground contamination of cesium conducted by authorities in the universe of Swedish municipalities. The results suggest that the larger the level of fallout, the greater the MP support in the 1988 elections. However, the positive premium on the MP vote remained until the 1994 elections. By using

detailed, individual-level survey data, I show that the chief reasons behind the opposition to nuclear power in affected municipalities was not the risk of future accidents, but a fear of potential problems for future generations, as well as the risk of higher cancer incidence in conjunction with the operation of nuclear power stations. These results are driven by voters who followed local media closely.

The third paper, *Gender, Socioeconomic Status, and Student Performance When Education Is Partially Online*, examines a quasi-natural experiment at a Swedish technical university. The paper compares the grade outcomes of two student groups during the fall semester 2020; one group took all of their courses online, while the other group was treated with hybrid education, that is, both campus and online teaching. The paper focuses on the grades in the online courses, for which spillover effects from the parallel campus classes exist for the treated student group. The results show that female students with affluent parents benefited more from hybrid education than other student groups. In addition, this group of students fared better, even compared to students that experienced full campus teaching. By linking each student's social network position with data on their gender and socioeconomic background, we show that female students with parents of high socioeconomic status have broader social networks, which facilitated communication with classmates at a time when peer access was scarcely available.

The fourth paper, *Peer Desirability and Academic Achievement*, explores the relationship between students' own grades and peers' socioeconomic status and physical attractiveness, using data from university students in Sweden. The paper utilizes a natural experiment in conjunction with mandatory orientation weeks during the beginning of the five-year program, through which students are randomly allocated to peer groups. Consequently, this setting allows students to form friendship bonds with their classmates. Using these social ties, the paper aims to examine whether it is beneficial for the student's own grades if his or her classmates are attractive and have affluent parents. The results suggest that having more attractive and affluent classmates is positively related to academic performance. However, these results are only statistically significant when both traits are considered jointly, and not individually. In addition, the

results show that attractive students, as well as students with wealthy parents, are more likely to receive higher grades. To examine the mechanisms behind this finding, I design a field experiment using a similar group of students, for which I let the treatment group see pictures of ten attractive faces, while the control group is exposed to ten pictures of less attractive faces. Allocation to treatment and control groups is performed randomly. The respondents are then asked to answer a set of questions about their current study situation, and their plans for the future. The results of the field experiment show that singles in the treatment group reported higher levels of well-being, and were more optimistic about passing their exams, than the control group, indicating that even a relatively ephemeral exposure to attractive faces is associated with positive outcomes. Long-term exposure to attractive classmates, which is the case with friendship bonds that arise during the orientation weeks, is likely to have even greater impacts on the well-being of students, and thus, on their grades.

Svenskspråkig sammanfattning

Denna avhandling består av fyra artiklar inom områdena politisk ekonomi och ekonomisk sociologi. Nedan följer en sammanfattning av envar av de fyra artiklarna.

Den första artikeln, *Regional aspects of immigration-related changes in political preferences* (”Regionala aspekter av invandringsrelaterade förändringar i politiska preferenser”), handlar om hur flyktingvågen 2015 påverkade väljarstödet för Sverigedemokraterna (SD) under valet 2018. För att skapa exogen variation i andelen flyktingar i kommunerna används ett placeringsprogram, i vilket kommunerna inte kan vägra deltagande. Resultaten visar att en högre andel flyktingar ledde till högre andel SD-röster, men att det finns avgörande skillnader mellan kommunerna beroende på hur de såg ut före flyktingkrisen. Dessa heterogena effekter var enbart statistiskt signifikanta när det gällde invandring av ensamkommande flyktingbarn, en grupp som domineras av unga män. I kommuner med hög brottslighet före flyktingkrisen, samt i kommuner med påtagligt stöd för Ny demokrati i samband med den förra flyktingvågen 1992, ledde invandringen av ensamkommande till en extra stor ökning i SD-röster. I avfolkningskommuner var däremot inte SD-ökningen lika stor.

Den andra artikeln, *The Electoral Consequences of Environmental Accidents: Evidence from Chernobyl* (”Politiska konsekvenser av miljöolyckor: Bevis från Tjernobyl”), handlar om hur radioaktivt nedfall efter Tjernobylolyckan 1986 påverkade väljarstödet för Miljöpartiet (MP), som valdes in i riksdagen 1988. Datan över nedfall bygger på flygmätningar av cesiumnedfall i samtliga svenska kommuner. Resultaten visar att ju större nedfall av radioaktiva ämnen i kommunen, desto större stöd för MP i valet 1988. Den positiva effekten på MP:s

röstandel fanns emellertid kvar till och med valet 1994. Genom att använda detaljerad individbaserad enkätdata visar jag att boende i drabbade orter, som skäl för sitt motstånd mot kärnkraft, huvudsakligen angav oro för kommande generationer och cancerfall, snarare än en oro för framtida olyckor. Dessa resultat drivs huvudsakligen av väljare som uppgav att de ofta följde lokalmedia.

Den tredje artikeln, *Gender, Socioeconomic Status, and Student Performance When Education Is Partially Online* ("Genus, socioekonomisk status och studenters prestationer när undervisningen är delvis online") utvärderar ett kvasinaturligt experiment vid en svensk teknisk högskola, där betygsresultaten för två grupper av studenter under hösten 2020 jämförs. Den ena gruppen studenter hade enbart distansundervisning, medan den andra hade hybridundervisning, det vill säga både campus- och distansundervisning. Studien fokuserar på betygen i distanskurserna, för vilken det för en av studentgrupperna sålunda förelåg överspillningseffekter från campusundervisningen i parallella kurser. Resultaten visar att kvinnliga studenter med välbeställda föräldrar gynnades av hybridundervisningen relativt övriga studenter; denna grupp presterade till och med bättre än när undervisningen var helt på campus. Genom att länka varje individs sociala nätverksposition med kön och socioekonomisk status, visar vi att dessa studenter har bredare sociala nätverk, vilket gynnande dem när det fanns begränsade möjligheter att studera tillsammans med kursare.

Den fjärde artikeln, *Peer Desirability and Academic Achievement* ("Klasskompisars åtråvärdhet och akademiska utfall") handlar om hur klasskompisars socioekonomiska status och utseende påverkar betygsutfallen för universitetsstudenter i Sverige. Genom att utnyttja ett naturligt experiment i samband med nollningen i början av den femåriga utbildningen, medelst vilket studenterna slumpmässigt placeras i olika faddergrupper och således bildar vänskapsband med andra studenter i samma klass, försöker studien utvärdera huruvida det är gynnsamt för de egna betygen om gruppmedlemmarna är snygga och kommer från välbeställda familjer. Resultaten visar att bättre utseende och högre socioekonomisk status bland klasskamraterna gynnar den enskildes betyg, men att resultaten inte är statiskt signifikanta om hänsyn enbart tas till en av dessa två parametrar åt gången. Dessutom visar resultaten att studenter som själva är snygga och har välbeställda föräldrar får bättre betyg. För att undersöka vilka

mekanismer som driver resultaten designar jag ett fältexperiment med hjälp av en snarlik grupp studenter, där jag låter försöksgruppen se tio attraktiva personers ansikten, medan kontrollgruppen får se bilder på tio mindre attraktiva personer. Allokeringen till försöks- och kontrollgrupperna sker slumpmässigt. Respondenterna besvarar sedan ett antal frågor om sin studiesituation och sina framtidsplaner. Resultaten från fältexperimentet visar att singlar i försöksgruppen rapporterade bättre välbefinnande och var mer optimistiska kring att klara sina tentor visavi dem i kontrollgruppen, vilket indikerar att även en relativt kortvarig exponering mot attraktiva ansikten är förknippad med positiva utfall. En långvarig exponering mot attraktiva klasskompisar, vilket är fallet med vänskapsband som uppstår i samband med nollningen, finge sannolikt ännu större effekter på studenternas välbefinnande och därmed på deras betyg.

Acknowledgements

Alright, here's the deal: I've spent 11 years at Lund University, and a whopping 23 years in the Swedish educational system. This means: no time to waste, so let's start with the acknowledgements!

First, my main supervisor Joakim Westerlund. I first met you in 2017 when I was looking for someone to supervise my BS thesis (tee-hee-hee), and I was unsure of whom to contact. I remember writing a standard, slightly formal, e-mail, and you replied with *tjena!* and *plugget*.[1] I immediately knew I had found my guy! Of course, a supervisor needs to be more than a stand-up comedian. For instance, consider the famous expression "knock and the door will be opened" (Matt. 7:7). Well, in your case, there's no point in knocking, since you are always in your office, and you have always had time for my important and unimportant questions. This open-door approach is important for any PhD student, I think. In addition, your circadian rhythm implies that you are the only person at the Department that can reliably be hired to assist in transporting copious quantities of non-alcoholic champagne and *snittar* across the LTH campus at 8 am in the morning. Also, I don't know who decided that we end all our meetings with someone declaring "zurück zur Arbeit!", i.e., "back to work!", but it's a fun tradition.

Then, my second supervisor, Andreas Bergh. I first met you at the department mingle at the start of the fall semester in my first year. Our first meeting was supposed to be a lunch meeting. I came into your office like a snowstorm (without the earthenware jug, and it was in September, not in April like in the

[1] *Tjena!* = "Howdy!", and *plugget* is a somewhat archaic slang term normally reserved for elementary schools.

novel), only to find... Therese Nilsson. It turned out that you had forgotten about the meeting, and already had your lunch. So, there we were, at the dilapidated Restaurant Olympia: I was eating a pizza, and you were having an ice cream the size of a volleyball. More importantly, however, it was at this meeting that I came up with the idea of Paper I, which was very important to relieve some of the stress associated with starting research. I have also benefited immensely from your large network of researchers in Sweden and elsewhere.

Now, it's time to pay homage to the 2018 PhD cohort. I don't know who came up with the idea that we use nicknames from the, supposedly cheesy, German police sitcom *Alarm für Cobra 11* in our Messenger group chat, but it certainly spiced things up. Linn (Jennifer Dorn), you're a super nice person and one of the smartest people I know. I mean, your Stata commands and crazy individual-level register data were so complex, they caused several meltdowns in the University's hard drives. And, yes, of course you can join my "paper making factory". Let's start right away! Hopefully, we can soon try some new Scottish-Italian food hybrids as well, such as haggis lasagna. Devon (Kim Krüger), I'm sure that you will do great in your future academic career; you have many layers to your research, plenty of ideas, and a superb personality. It's also amazing how you managed to calculate the least-cost path from EC to the subpar Bryggan restaurant. I hope you can soon repeat those calculations for the slightly more sophisticated Grönt & Gott!

Moving on, what do you get if you take one of the most impressive beards I have ever seen, and fill it with raw econometrics talent and ricotta cheese? You get Marcus (Hotte Herzberger)! I'm truly impressed with how you always stood up for what's right; in everything from protesting against overambitious PhD courses in dynamic optimization, to fighting sneaky cheating Bachelor's students, you will be remembered as being on the right side of history. And it's always nice to have a wine expert in the cohort. Olga (Anna Engelhardt), I'm sorry we didn't manage to interact more, but you were always busy driving your crappy car to that mediocre fish restaurant whose name I can't remember (and that even Elon Musk's pocket would struggle paying for). But what I learned already from sharing an office with you in the first year, is that you have a saucy sense of humor that perfectly aligns with mine. Also, you taught me that

gooseberries are not unique to Sweden.[2] Ovi (Ben Jäger), although you were officially a member of the wiser 2017 cohort, I've always felt that you were part of our gang. Unfortunately, I abandoned theoretical econometrics relatively early, so I couldn't give you any intelligent advice about research. Still, I'm fascinated how you managed to mince so many big ideas into high-quality papers. When others tried hard to resemble Salieri, you were already Mozart! I'm also grateful for all the support around the job market.

Sometimes, people deviate from their intended paths. But, of course, once a Cobra 11 PhD, always a Cobra 11 PhD. So, let's give a shoutout to Charlotta (Andrea Schäfer) and Philipp (André Fux). Phil, there are few things I appreciate more than someone who au–tomato–icly says what they think, regardless of what everyone else thinks. Also, I will never forget you banging on everyone's doors at 12.00 every day proclaiming "FEEDING TIME!" After this signal, we all went to glorious Inspira, which happens to be the fifth restaurant mentioned in this Introduction. Those were the days. Charlotta, you are the only person to co-author with me in this thesis. I'm sure you would like to have contributed more to Paper III than you eventually did, but the paper could not have been written your input. The national anthem of your native Finland may proclaim that it is a "golden land" (I can't disagree with that), but you are even more golden.[3] Charlotta and Phil, I hope you two will eventually finish your PhD's somewhere, sometime. The Econ family needs you badly!

Then, of course, we have the other PhD students at the Department: Albert, David S, David W, Demid, Emelie, Erik, Hampus, Iker, James, Jonas, Josefin, Kajsa, Kristoffer, Ludvig, Lukas, Madeleine, Marco, Matthew, Najmeh, Natalie, Negar, Pelle, Pol, Prakriti, Sandra, Sanna, Sara, Shayan, Steve, Teppo, Thomas, Yousef, Zahra, and others. You guys are maximum fun!

[2]There is a saying in Swedish, *blott Sverige svenska krusbär har* ("only Sweden Swedish gooseberries yields"; originally from C.J.L. Almqvist, *Om svenska rim*, c. 1851), meaning that Sweden is a unique country; I did, however, believe that there was also a literal component to this. Olga's response: "You Swedes always think you're so special!"

[3]Swedish original by Johan Ludvig Runeberg, 1848: *För oss med moar, fjäll och skär // Ett guldland dock det är* ("In moor, and fell, and isle and wave, // A golden land, so brave", transl. by Anna Krook, 1904).

I want to thank Tommy Andersson and Erik Wengström for being excellent Directors of Studies, and to the two Department Heads during my time, namely Jerker Holm and Joakim Gullstrand. For awesome job market practice, I am grateful to Claudio Daminato, Simon Reese, and Talina Sondershaus. Among the more senior faculty that I have had the pleasure of interacting with over the last five years, I would like to mention Karin Bergman, David Edgerton, Daniel Ekeblom, Lina Maria Ellegård, Petter Lundborg, Sonja Opper, Dag Rydorff, Fredrik Sjöholm, Bengt Söderlund, Petra Thiemann, and Roel van Veldhuizen, among others. From the Department of Statistics, I want to mention Henrik Bengtsson, Peter Gustafsson, and Krzysztof Podgórski, and from the Department of Philosophy, Erik J. Olsson. Thank you for fruitful academic and non-academic discussions.

My time here would not have been the same had I not been blessed with the opportunity of teaching the introductory microeconomics course for engineering students. When I took over the course, the course literature was from 1974 (?!), which meant that students missed out on large swaths of the recent game theory revolution. Now, it has been transformed to a modern top-notch course, which happens to be one of the most highly rated at LTH. Thank you, Pontus Hansson, for giving me to opportunity to have full responsibility not only for this course, but for a Master's course as well. I also found it rewarding to TA for Hans Byström and Jens Forssbaeck in their famous Fixed Income Securities course. I mean, what second-year student wouldn't like to learn how to spend three months arbitraging with Swiss francs and U.S. dollars, only to get an amazing 0.5% annualized return on that investment? Apparently many of them!

If you have a problem, if no one else can help, and if you can find them, maybe you can hire... the A(dmin)-Team! This department is truly fortunate to have such an admin team. It almost feels like we have round-the-clock support here. Besides all work-related help, I'm grateful for your advice in life's more trivial issues, such as, how to optimally open a champagne bottle (have I mentioned that I like food and drink?), and of course, how to mediate in the slightly unexpected, but epic, struggle between a "retired" German exam invigilator and a 23-year-old micro teaching assistant. So, thank you, Anna, Azra, Jenny, Li, Mariana, Marie, Nathalie, Peter, and Ulf!

Speaking of Germans (again). Although I'm not sure I agree with the description of their language being "a maiden bred in the woods"[4], it was nevertheless a pleasure to take a BA in this subject simultaneously with my Econ PhD studies, although Joakim W remarked that I was probably "completely crazy" for even attempting to do this. But I succeeded! I thank Henrik Henriksson and Mikael Nystrand for thesis supervision, including tips on how to apply beauty research to research on the frequency of the genitive object in contemporary German, and Dorothea and Rebecka for being fun study partners.

I also want to thank Niclas Berggren, Caroline Hall, and Johannes Lindvall for reviewing the unpublished chapters of this thesis, and Therese Nilsson and Gunes Gokmen for reviewing the changes made in response. This has greatly improved the papers. Thank you, Johannes, for agreeing the be the opponent at the defense, and thanks to Daniela Andrén, Mikael Elinder, and Lars Jonung for taking your time to join the grading committee. Now, I just cross my fingers and quote one of my friends from my running group (more on that later), after she was almost disqualified after a race for some obscure attempt at cheating: "I mean, come on, be a little nice after you've given your maximum effort!"[5]

For financial support, I am grateful to the Arne Ryde Foundation, to Stiftelsen för främjande av ekonomisk forskning vid Lunds universitet, and to the Knut and Alice Wallenberg Foundation. The Arne Ryde Foundation deserves special praise, since it has contributed both to the financing of my research, as well as a dope seminar on the possibilities for PhD's outside academia that I organized, and that featured several inspiring speakers. Thanks to Fredrik Andersson for fast-tracking my application.

Is there anyone still reading? Good, we're almost done! A huge part of my life so far has been linked to sports, in particular running. Imagine Joakim W's surprise when, chillaxing in front of the TV and watching the Swedish

[4]Swed.: "en jungfru fostrad i skogen", Esaias Tegnér, *Språken*, 1817. Fun fact: The poem itself is virtually untranslatable into English as it is written using so-called elegiac couplets (in which a dactylic hexameter verse is followed by a dactylic pentameter verse), which does not match the rhythm of the English language.

[5]Swed.: "Liksom, kom igen, var lite snäll efter att man tagit ut sitt max!"

National Championships live from glorious Borås, all of a sudden he sees me not only appearing live, but also beating my personal best at the 800 meters (although broadcaster SVT claimed that this was a national record in their useless graphics). Naturally, to reach and maintain this level requires significant effort; over a five-year period, around 3,000 hours of practice. So, I thank my stellar coaches Rizak Dirshe and Ivan Premovski, and my banger training group, in particular Mattias, Sebastian, and Zeth, for being great friends both when running around in circles, and outside of practice.

This thesis would not have been written without support from my family and friends. Unfortunately, this Acknowledgement section is already way too long, so I restrict myself to two people, namely my parents, Aleksandra and Mirsad. Of course, being forced to leave your hometown of Travnik and ending up in exotic places such as Ljusdal, Sandviken, and later, Umeå, is already quite daunting. But, luckily for me, you managed to build a new life in Sweden. Mom, you are by some distance my greatest inspiration academically, since you even managed to get a Master's degree here.[6] Thank you for everything!

Lund, March 2023
Adrian

[6]Understandably, being equipped with a law degree from a non-existent country is usually not optimal if you want to maximize your chances on the Swedish labor market.

Introduction

Introduction

”Då de hade satt sig tillrätta i baksätet och hon hade talat om för chauffören vart de skulle åka, frågade hon: ’Men hur kunde du rita den där kartan?’
’Det var ingenting’, sade han. ’Det var rakt ingenting. Det var bara alldeles detsamma som att göra en mening.’ ”

– Torgny Lindgren (1938–2017),
västerbottnisk författare, ledamot av Svenska Akademien från 1991.

“When they had made themselves comfortable in the back seat and she had instructed the chauffeur where to go, she asked, ‘But how where you able to draw that map?’
‘It was nothing’, he said. ‘It was nothing at all. It was just exactly the same as constructing a sentence.’ ”

– Torgny Lindgren (1938–2017),
author from Västerbotten, member of the Swedish Academy from 1991.[7]

The 2010 laureate of the Nobel Memorial Prize in Economic Sciences, Dale T. Mortensen, remarked that “economics is a strange science” in his acceptance speech. With this in mind, it is not surprising that many people find

[7]Excerpt from *Pölsan* (2002). English-language title: *Hash* (2004, transl. by Tom Geddes). Pölsa (hash) is a Swedish dish closely resembling haggis.

it confusing that a thesis largely dealing with politics and sociology is considered an economics thesis. Part of the confusion is likely due to semantics. In Swedish and Norwegian, the science of economics is known as *nationalekonomi* and *samfunnsøkonomi*, respectively; that is, "national economics" or "societal economics". In German, the equivalent term is *Wirtschaftswissenschaft*, where the word *Wirtschaft* can mean "industry", "business", "farm", or even "restaurant". Hence, many laymen associate economics only with macro topics, such as inflation, business cycles, unemployment, trade, and taxation. These topics are generally not dealt with in other sciences. On the contrary, other topics in modern economics research, such as political economy, behavioral economics, and econometrics, overlap other sciences: political science, psychology, and statistics, respectively. This thesis is about the overlap between economics and two of the other major social sciences, namely political science and sociology.

While the subfield of economics that studies the interaction between economic systems and politics is widely known as political economy today, few know that the entire science of economics was called "political economy" well before it was renamed "economics" in the late 1800s.[8] For instance, Adam Smith wrote in *The Wealth of Nations*, "political economy, considered as a branch of the science of a statesman or legislator, proposes two distinct objects: first, to supply a plentiful revenue or product for the people [...] and secondly, to supply the state or commonwealth with a revenue" (Smith 1776). From this quote, it is easy to grasp the reason behind the historical overlap between political science and economics: the authority vested in the legislator (that is, in the absolute monarch) meant that he was directly responsible for all of the state's economic decisions. Obviously, there were no independent financial authorities around this time to balance any political influence.[9]

[8]However, the roots of political science are considerably older than the roots of economics; Aristotle and Plato both presented basic political theories (Miller 2017).

[9]As an example, the Swedish Riksbank, the oldest central bank in the world, was established by the parliament in 1668, well before Smith's book. However, it was not independent of the state or the monarch, since parliament controlled the day-to-day operations of the bank. Later, in 1680, King Charles XI abruptly transferred all meaningful powers from parliament to himself.

Although Smith is generally considered to be the first to treat economics, or political economy, as an academic discipline of its own, it was not until the publishing of Alfred Marshall's (1890) famous textbook, *Principles of Economics*, that the term "economics" gained widespread usage. Since then, the term "political economy" has been used chiefly in the same sense as in this thesis, that is, as the study of the link between economic systems and politics.

Another major social science is sociology. Again, this is a science with ancient roots. For instance, according to Chinese philosopher and sociologist Confucius, a society will be kept well-functioning, if in the following five relationships, the first person is respectful and the other is subservient: ruler–subject, father–son, husband–wife, elder brother–younger brother, and older friend–younger friend (Buckingham 2011). Confucius also stressed the importance of social norms, 禮 (Wade–Giles romanization: "li"), such as etiquette and proper conduct, in order for a society to be prosperous.

However, it was not until the Industrial Revolution that serious attempts were made to link economic outcomes with social outcomes, primarily through the works of Karl Marx and Max Weber. The Industrial Revolution created a new middle class (the bourgeoisie), along with the working class (the proletariat), and both Marx and Weber agreed that the living standard of the bourgeoisie was considerably better than that of the proletariat. According to Marx, this was a consequence of the bourgeoisie's ownership of the means of production, which is the sole factor upon which social class is based (Marx 1867). Weber, on the other hand, considered social class to be three-dimensional, consisting of wealth, prestige, and power, where the two latter dimensions relate to the social status of the individual (Weber 1921). Consequently, class, and by extension, economic outcomes, is partially a consequence of social ties.[10] Additionally, in the late 19$^{\text{th}}$ century, a methodological breakthrough occurred when Émile Durkheim published his book, *Le Suicide: Étude de sociologie* ("Suicide: A Study in Sociology", 1897), in which, using empirical observations, he established that

[10]However, it was not until several decades later that the term "social capital" was broadly accepted in economic sociology. Influential late 20$^{\text{th}}$ century contributions include Bordieu (1985) and Coleman (1988).

certain socioeconomic factors increased the risk of suicide. For example, men, singles, soldiers, and Protestants, were all groups for which the risk of suicide was higher.[11] Durkheim's work popularized empirical analysis in sociology.

Durkheim also foreshadowed a new generation of sociologists, who differentiated between theory-based sociology and more empirically-oriented sociology. In particular, Tönnies (1931) differentiates between "pure sociology" (German: *reine Soziologie*) and "empirical sociology" (*empirische Soziologie*). Crucial for empirical sociology is the analysis of social networks. Moreno (1934) introduced the sociogram, an example of which can be seen in Figure 1. In this sociogram, each person, alternatively referred to as a *node*, is represented by a blue circle; the lines connecting the nodes are called *edges*, and represent friendship bonds. Sociograms are important as they allow for the visualization of social ties. Concomitantly, there were developments in mathematics that facilitated social network analysis. To assign numbers to nodes, for instance, in order to identify the most influential person in a network, requires methods from graph theory. Important mathematical contributions in this field include works by Katz (1953), and Erdős and Rényi (1959).

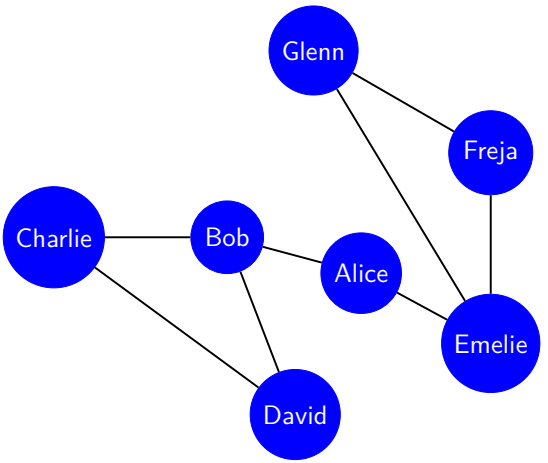

Figure 1: Diagram of a social network with seven nodes.

[11] Before Durkheim, it was generally assumed that suicide was mainly due to nervous breakdowns, and overall, unrelated to socioeconomic factors (Goldney et al. 2008).

In the same way that political economy attempts to link economic markets with political systems, economic sociology links markets with social networks. Of particular importance is the labor market, and an important contribution in this respect is the empirical work of Mark Granovetter (1973). According to Granovetter, the "weak ties" in the personal network are the most crucial for a job seeker's success in the labor market. In layman's terms, if a person wants to find a job, he or she is most likely to find it through acquaintances, not through good friends.

This idea about weak ties was expanded on by Burt (1992), who noted that individuals with social networks resembling Alice's in Figure 1 are usually more successful than others. The reason behind this is that, while Alice and David have the same number of friends (two), Alice's friends come from different social clusters, while David's friends are both friends with each other. Hence, the same information is "circulated" within the friendship cluster containing Bob, Charlie, and David, which is redundant. Alice, on the other hand, gets input both from the BCD cluster and the EFG cluster. We say that Alice has lower *network constraint*, or equivalently, higher *betweenness centrality*, than David. Having low network constraint is advantageous in many settings, for instance in the labor market, and for educational outcomes (Burt 2004; Calvó-Armengol et al. 2009). Social networks are also important for understanding transition economies, most notably China, as the quality of formal institutions in these countries tends be lower (Nee and Opper 2012, pp. 17–32).

But what is the difference between political science and sociology on one hand, and political economy and economic sociology on the other hand? In my experience, economists are generally less interested in political and sociological theory, and interested more in data and mathematical models. Using Tönnies' definition, sociologists are mainly interested in "pure sociology", whereas economists working in the intersection between economics and sociology specialize in "empirical sociology". The same is true for political scientists and political economists. There are also differences in the structure of academic journals in economics, and journals in political science and sociology, respectively.

In this thesis, while Paper I could have been published in a political science journal, it would certainly have to include an additional "theory" section, in which various political science theories on why immigration causes natives to vote in favor of anti-immigration parties would have been discussed. On the other hand, economists tend to be more rigorous in terms of methodology, especially when it comes to mathematical modelling and causality. In terms of mathematics, this is not unexpected considering that economists often work with questions about money and resources, which are quantitative by nature. Additionally, the introduction of mathematical models into economics came relatively soon after Smith; a mathematical model of agricultural land use was already developed by the early 19$^{\text{th}}$ century (von Thünen 1826). In a sense, the mathematical tradition is firmly rooted in economics, but less so in political science and sociology.

The question of causality is also more prominent in economics than in other social sciences. Importantly, causality provides a link between the four papers in this thesis, since all of them use natural experiments in some way. In the social sciences, it is often difficult to randomly assign research subjects into treatment and control groups, which contrasts medicine and most of the natural sciences. A natural experiment is a way of overcoming this. In principle, a natural experiment is an event or situation outside of the control of the researcher that generates variation in the variable of interest as if it had been randomly assigned. Although natural experiments are commonly used in today's research, the "natural experiment revolution" in economics occurred over a century after von Thünen's introduction of mathematics. The method was not popularized until the early 1990s, with influential contributions by Angrist (1990), Card (1990), and Card and Krueger (1994); Joshua Angrist and David Card were awarded the 2021 Nobel Memorial Prize in Economic Sciences for this type of research.[12]

However, only rarely will a researcher find a setting that produces a natural experiment completely resembling a laboratory setting. This is because perfect

[12]The prize was shared with Guido Imbens, who has also done important albeit more theoretical work related to natural experiments.

randomization implies that a lottery of some sort is used, which is seldom the case. An alternative approach is to use a quasi-natural experiment, that is, an empirical setting which resembles a randomized field experiment but lacks full random assignment. For a quasi-natural experiment to produce causal estimates, the researcher needs to know what parameters are used for the assignment, and should control for these parameters in the econometric framework. In this thesis, Paper IV provides the best example of a natural experiment, since allocation to treatment occurred randomly. In Paper II, randomization is mostly based on rainfall, which can be considered close to random but not as perfect as lottery-based randomization. Finally, Papers I and III use quasi-natural experiments. The following section provides a detailed description of each paper separately.

Paper I: Regional aspects of immigration-related changes in political preferences

This article studies the way the 2015 refugee crisis affected voting outcomes in Sweden. In particular, I examine whether the vote shares of the anti-immigration Sweden Democrats (SD) increased in municipalities with higher immigration rates. While the relationship between municipality-level immigration and voting outcomes has been examined in several previous studies (see Cools et al. 2021 for a review), less attention has been given to the way pre-influx regional characteristics affect changes in voting behavior associated with immigration. A common problem observed in this literature is that immigration rates are endogenous. This is because it is unlikely that immigrants choose municipalities completely randomly.

To overcome this issue, I use a placement program of refugees used in Sweden since early 2016. Under this scheme, refugees with a residence permit are allocated to municipalities based on four parameters: the population of the municipality, the number of previously allocated refugees, the number of asylum seekers residing in the municipality, and local labor market characteristics. Importantly, municipalities cannot refuse to participate in the program, nor can they refuse to accept an allocated refugee. Hence, the share of allocated refugees is used as an instrument for the endogenous immigration rate. A similar instrument

has been used in other papers, albeit answering questions other than the one posed in this paper (Edin et al. 2003; Dahlberg et al. 2012). In my regression model, the outcome variable is the percentage point change in the SD vote share between the 2018 and 2014 elections. The results suggest that, overall, higher immigration rates led to an increase in SD voting. However, there were significant regional heterogeneities: in municipalities with high crime rates before the refugee wave, as well in municipalities with significant right-wing populist support during the previous (1992) refugee wave, immigration boosted the SD vote even further. On the other hand, the immigration-related increase in the SD vote was smaller in areas subject to depopulation. Notably, these regional heterogeneities were only statistically significant when considering the immigration of a particular refugee group dominated by young males. This finding suggests that the immigration of young men is particularly polarizing.

Paper II: The Electoral Consequences of Environmental Accidents: Evidence from Chernobyl

This paper examines the way environmental accidents impact voting. Specifically, I examine to what extent radioactive fallout from the 1986 Chernobyl disaster contributed to the growth of the Swedish Green Party (MP). The MP was elected to parliament in 1988, more than tripling its vote share compared to the 1985 elections. A similar green wave was seen in other countries throughout Europe in the late 1980s and early 1990s.

To answer this question, I rely on aerial measurements of fallout carried out by authorities immediately after the reactor fire was extinguished. Since 99% of radioactive fallout is spread through rainfall, Chernobyl can be considered a natural experiment that exogenously produces variation in the ground deposition of radioactive materials. I then estimate a standard fixed effects model in which the MP vote shares from each election from 1982 to 2018 are regressed on the level of Chernobyl fallout interacted with indicator variables for election years. This allows us to estimate how the MP vote is affected by fallout, and, in addition, how fast the Chernobyl premium on the MP vote vanishes. The main finding of the paper is that the MP vote share increased significantly in areas affected by fallout. The premium on the MP vote declined in subsequent

elections, but remained statistically significant until the 1994 election; after this, there was no significant relationship between the 1986 Chernobyl fallout and MP voting. The finding that the support for the MP vanished relatively quickly is in line with previous research on the political effects of environmental accidents being relatively short-lived (Bechtel and Hainmueller 2011).

The second part of the paper examines the mechanisms behind these findings. Using an individual-level survey, which has been sent out randomly to households since 1986, it is possible to examine political attitudes at the municipality level. Using a 1980 referendum about nuclear power as the baseline year, I find that opposition to nuclear power increased compared to the pre-Chernobyl levels in areas affected by fallout, but that this opposition decreased considerably already after one election period. This finding is consistent with the election results. In addition, the survey results show that voters sceptical about nuclear power were primarily concerned about the problems for future generations associated with nuclear power, and the possible deterioration of air and water quality, rather than the risk of an accident in Sweden.

Paper III: Gender, Socioeconomic Status, and Student Performance When Education Is Partially Online (with Charlotta Olofsson)

During the COVID-19 pandemic, higher education institutions in many nations were temporarily closed or switched to an online or hybrid mode. In this paper, we evaluate an experiment at a Swedish technical university, in which students were quasi-randomized to either take all their courses online or take some courses online and some on campus. The latter form of education is otherwise referred to as *hybrid education* or *blended learning*. More specifically, we evaluate the grade outcomes of two online courses, in which around half the students took parallel online courses, while the other half took parallel campus courses. Hence, we compare the spillover effects on grades in online courses from having partial campus access in parallel classes. We hypothesize that spending more time on campus, albeit in conjunction with other course modules, improves well-being among students and facilitates peer communication. In the paper, we use official grade transcripts linked to detailed administrative data where

we proxy socioeconomic status using a student's parental income. To further evaluate the mechanisms behind our findings, we survey the same set of students a few months after the experiment. We have two main findings. First, having some access to campus was associated with a significantly higher level of self-assessed well-being. When education moved fully online after the onset of the second wave of the pandemic in November 2020, the difference between the two groups vanished, which strongly suggests that the difference observed earlier in the semester was due to the partial campus access in the treatment group. Second, we show that having classes partially in-person was associated with significant socioeconomic distortions. Specifically, blended learning, as opposed to full distance learning, only improved grades in the online courses among female students with affluent parents. In addition, blended learning was, in fact, associated with better grades than on-campus learning among women with a high socioeconomic status.

Our individual-level survey data suggests that there was no relationship between socioeconomic status and adverse mental health amid the COVID-19 pandemic. Instead, we turn our attention to social networks. Using the questionnaire, we estimate the network position of each student, matched with the same administrative data on parental income. The results suggest that female students with wealthy parents have significantly less constrained social networks, meaning that these students have more in-class friends and their friends, in turn, have fewer redundant links to one another. Stated differently, female students with wealthy parents are to a greater extent linked to different in-class social clusters, experience less information redundancy, and have access to broader information when communicating with peers, all of which have a positive impact on academic performance.

Paper IV: Peer Desirability and Academic Achievement

In this paper, I examine whether two specific peer traits considered important in the marriage market, namely socioeconomic background and physical attractiveness, are beneficial for the grades of university students. I introduce the term *desirability* to describe these two traits. Previous studies have suggested other "desirability pairs", including socioeconomic status and health, and attractive-

ness and sexual responsibility (Angelucci and Bennett 2021; Maralani and Portier 2021). To establish causality, I use the random assignment of engineering students to peer groups during introductory freshman weeks at a university in Sweden. Since introductory freshman weeks create persistent social ties between students, similar designs have been used in other studies (Fischer and Rode 2020; Thiemann 2022). There are two main results. First, a student's own socioeconomic status and beauty are associated with higher grades, although the effect size is larger for socioeconomic status. Second, the desirability of peers is also positively related to the student's own grades. This finding suggests that there are spillovers to peers from high-desirability students scoring better themselves. However, beauty in particular is likely to lead to indirect effects on grades, since being exposed to attractive individuals increases well-being through the so-called halo effect (Feingold 1992).

To evaluate the mechanisms behind the halo effect and its impact on peer effects, I design a field experiment. A group of university students is divided into treatment and control groups, where the treatment group is shown pictures of ten attractive faces, and the control group is shown ten not faces that are not conventionally attractive. Then, both the treatment and control groups are subjected to the same set of questions regarding their current well-being and their future careers. Singles in the treatment group report higher well-being and are more optimistic about their exams. This finding indicates that exposure to attractive faces increases student well-being, which is likely to improve grades.

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Paper I

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RESEARCH ARTICLE

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Regional aspects of immigration-related changes in political preferences

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Abstract

This paper investigates how right-wing populist voting following the 2015 refugee wave was affected by regional characteristics, using data from a quasi-natural experiment in Sweden. The results suggest that voting outcomes are heavily dependent on pre-influx municipal characteristics. In localities with strong anti-immigration sentiments during the 1990s refugee wave, as well as in areas with high crime rates before the refugee wave, there is a positive relationship between immigration rates and anti-immigration voting. However, the immigration-related increase in the nationalist vote is significantly smaller in depopulation areas. These polarizing effects on voting are exacerbated when considering immigration of young males.

KEYWORDS
immigration, persistence, quasi-natural experiment, right-wing populism, unaccompanied minors, urbanization, voting

1 | INTRODUCTION

Immigration has long been the dominating political issue in virtually every country in the Western world. Since the onset of the European migrant crisis in 2015, over three million people have applied for asylum in a European Union country. Sweden, a nation of ten million inhabitants, received some 163,000 asylum applications in 2015 alone. Between the 2014 parliamentary election, which took place just before the onset of the migrant crisis, and the 2018 election, the anti-immigration *Sweden Democrats* (SD) party increased its national vote share from 12.9% to 17.5%. However, there are large variations between municipalities both in terms of immigration levels and the

SD vote share, raising the question whether there is a causal relationship between immigration levels and the support for anti-immigration parties.

Given that anti-immigration parties have established themselves as the largest party in several European countries, the electoral base of these parties no longer consists of low-skilled workers only. Consequently, in the last years, there has been a growing interest in evaluating the causal impact of immigration on the right-wing populist vote. This literature has mainly focused on the electoral impact of immigration of refugees in particular, as immigration of refugees is considerably more controversial than high-skilled migration. Although numerical estimates vary between countries and time periods, most studies find a positive correlation between refugee migration and far-right voting. Recent contributions by Otto and Steinhardt (2014) on Germany, Halla et al. (2017) on Austria, Hangartner et al. (2019) on Greece, and Harmon (2018) and Dustmann et al. (2019) on Denmark, all point in this direction. A minority of studies conclude that there is no relationship between refugee migration and far-right voting (Dal Bó et al., 2022; Mendez & Cutillas, 2014), whereas a third set of studies gives opposite results (Lonsky, 2020; Steinmayr, 2021). Another strain in the literature notes that while there may be a positive impact on the right-wing populist vote from refugee migration, there is no, or even negative overall effects for immigration of highly educated individuals (Mayda et al., 2016; Moriconi et al., 2019).

However, while the impact of immigration on the right-wing populist vote is likely to differ between countries and time periods, an equally important question is whether there is regional within-country variation. Dustmann et al. (2019) find that the positive relationship between immigration rates and anti-immigration voting is exacerbated in rural areas, whereas right-wing populist voting decreased as a result of immigration in urban areas. The rural–urban divide on attitudes towards immigrants and migration is indeed the regional characteristic most carefully examined in the literature (Alba & Foner, 2017; Maxwell, 2019). However, there are likely additional local characteristics plausibly affecting voting outcomes.

Another gap in the literature concerns the persistence of anti-immigration sentiments across time. Although the 2015 influx to Sweden was exceptionally large, there was a similar influx in 1992, concurring with the collapse of Yugoslavia. At that time, another anti-immigration party was represented in parliament, only to disappear a few years later. Whereas a number of recent papers have emphasized the role played by historical events in shaping current economic and political outcomes (Alesina et al., 2013; Guiso et al., 2016; Pascali, 2016; Voigtländer & Voth, 2012), there are no studies on how past local anti-immigration sentiments affect current voting behavior in response to a refugee shock such as the one in 2015.

The chief purpose of this paper is, thus, to answer to what extent present regional characteristics affect anti-immigration voting. To answer this, I use data on municipality-level immigration rates during the 2015–2017 refugee crisis matched with data on municipal characteristics before the onset of the migrant crisis. Moreover, I use historical election results to examine the possibility of an additional effect on right-wing populist voting from latent anti-immigration sentiments. The question of interest here is, thus, whether voters in regions where anti-immigration parties were represented in local parliaments following the 1992 refugee wave were more likely to shift to the far-right in response to the 2015 influx. Finally, I use municipality-level data from the 1994 referendum on joining the European Union to examine whether past anti-EU sentiments affect electoral outcomes in response to immigration today.[1]

Using municipality-level immigration data leads to endogeneity problems, because immigrants can self-select into municipalities of their choice. This is particularly prominent in cases of family reunification, as most family migrants tend to settle in the same municipality as their relatives. Some asylum seekers may also choose this option, if they already have family or acquaintances in Sweden. To solve this, I use an instrumental variables (IV) strategy by utilizing data from a nationwide refugee placement program, through which refugees with a residence permit in Sweden are quasi-randomly allocated to Sweden's 290 municipalities. Although the number of municipalities is

[1]Several Brexit studies suggest that there is a correlation between voting for Leave and anti-immigration attitudes (Becker et al., 2017; Carreras et al., 2019).

relatively low, I argue that because the allocation scheme provides exogenous variation in the number of immigrants placed in the municipalities, it is possible to estimate the causal effect of migration on the vote share of the SD. A similar program implemented in the late 1980s and early 1990s has been used in other studies, albeit answering different questions than the one posed in this paper (Dahlberg & Edmark, 2008; Dahlberg et al., 2012; Edin et al., 2003).

Another major topic in the literature is to what extent different types of refugees give rise to differential attitudes among natives, hence affecting right-wing populist voting.[2] Given that this paper focuses on the regional aspects of right-wing populist voting in response to immigration, a natural question is, thus, whether regional heterogeneities in voting outcomes are affected by the characteristics of refugees. I address this question by focusing in particular on the differential effects arising from exposure to immigration of unaccompanied minors. This group, consisting mainly of Afghani nationals in their upper teens, was significantly over-represented among refugees arriving in Sweden during the 2015 wave. Since over 90% were male, members of this group fit well to the notion of migrants being crime-prone "young, strong men," spread by some mainstream politicians in Western Europe and the United States.

Controlling for other factors, the results show a positive and significant relationship between the immigration rate and the increase in the SD vote. In municipalities with high pre-influx crime rates, as well as in municipalities with relatively high shares of foreigners before the onset of the migrant crisis, immigration further exacerbated SD growth. However, the immigration-related increase in the right-wing populist vote was smaller in municipalities where the native population was declining before the migrant crisis. Both the positive and negative interaction effects were stronger for immigration of young men, compared to refugees as a whole. Finally, higher immigration rates significantly increased the SD margin in regions where right-wing populists were represented in local parliaments after the 1994 election, which was after the 1992 mass influx of mainly ex-Yugoslavian citizens. This result suggests a strong regional persistence in anti-immigration sentiments between refugee shocks. However, the immigration-related growth in the SD margin was lower in areas with historical anti-EU sentiments, which speaks against the existence of a positive correlation between anti-EU and anti-immigration preferences.

Using the results from an annual, nationwide survey, where respondents are selected randomly, I further show that the immigration-related SD growth is driven primarily by compositional amenities and cultural concerns.

The paper makes a number of contributions to the existing literature. First, the results provide further support to previous empirical research suggesting that immigration is a contributing factor to right-wing populist growth. In addition, I show that the results are heterogeneous both with respect to pre-influx regional characteristics and with respect to the characteristics of immigrants. Particularly, the finding that immigration has a dampening effect on the increase in right-wing populist voting in depopulation areas is new to the literature. However, previous research has shown that immigration is associated with increases in housing prices and economic activity in general (Gonzales & Ortega, 2012; Howard, 2020; Saiz, 2007; Sharpe, 2019). In regions adversely affected by urbanization, the possibility of such favorable effects are likely to be acknowledged by voters.

Moreover, I contribute to the recent literature on the electoral impact on the far-right vote stemming from reactions to different types of immigrants (Card et al., 2012; Hainmueller & Hopkins, 2014). Particularly, I show that the differential regional impact on far-right voting is considerably more pronounced when considering the influx of young, male refugees than for refugees as a whole. One plausible reason for this polarizing effect is because on one hand, there are favorable demographic characteristics associated with young men. These could contribute to a revitalization of, in particular, depopulation regions. On the other hand, young, male immigrants tend to be

[2]For instance, Edo et al. (2019) show that the immigration-related increase in right-wing populist voting is driven by low-educated immigrants from non-Western countries.

over-represented in crime statistics, which is a likely explanation to why immigration to municipalities with above-average crime rates before the crisis further exacerbated SD growth.

The final contribution is related to the finding that anti-immigration voting increased in response to the refugee shock in municipalities with right-wing populist presence following the 1992 refugee wave. These results suggest that latent anti-immigration sentiments are an important driver of the present-day growth of right-wing populism. The results are in line with those of Cantoni et al. (2019), who show that voters in German municipalities with strong support for the Nazi party in the 1930s are more likely to vote for the AfD today. My paper complements this analysis by focusing on changes in political preferences arising from the interaction between historical voting and current immigration levels, as an alternative to examining only cross-period correlations between voting patterns. This result provides further support to recent evidence of the role played by historical events in shaping current voting behavior (Ang, 2019; Fontana et al., 2017; Ochsner & Roesel, 2020). Additionally, the results regarding the relationship between previous anti-EU sentiments and current anti-immigration attitudes are new to the literature, and suggest that opposition to economic integration is not automatically associated with a subsequent opposition to refugee immigration.

The rest of the paper is structured as follows. Section 2 provides a number of stylized facts related to Swedish immigration policy, as well as the nature of the Swedish refugee allocation program. Section 3 discusses whether the refugee allocation program provides exogenous variation in the share of immigrants, and hence, whether the program produced a natural experiment. Section 4 describes the data. Section 5 presents the instrumental variables strategy, followed by the results. Section 6 provides a number of robustness checks. The paper concludes with Section 7.

2 | IMMIGRATION TO SWEDEN AND THE PLACEMENT PROGRAM

2.1 | Historical background

Until the 1980s, immigration to Sweden was mainly driven by labor demand. Most of the migrant workers had arrived from other Nordic countries, in particular Finland, and from other European countries, such as Greece and Italy. However, towards the end of the 1980s, migration for political reasons increased dramatically. Whereas Iranians were the largest nationality among asylum seekers during the 1980s, the wars in former Yugoslavia created an even larger refugee wave in the early 1990s. A temporary peak in asylum seekers occurred in 1992, when 84,000 refugees arrived. This figure was approximately 1% of the total population in that year. After declining during the late 1990s, the number of asylum seekers increased again in the early 2000s, hovering around 30,000 per year, as shown in Figure 1.

The 2015 wave saw the arrival of an additional 162,877 asylum seekers. Of these, 126,455 (78%) were from Syria, Afghanistan, Iraq, Eritrea or Somalia.[3] The EU-sponsored shutdown of the Western Balkans route in 2016, as well as harsher immigration policy in most countries, including Sweden, decreased the number of asylum seekers to a cumulative number of 75,000 for the years 2016, 2017, and 2018.

Due to Swedish laws being relatively more liberal vis-à-vis those of other nations for unaccompanied minors, a considerable share of asylum seekers belong to this group. As the name suggests, an unaccompanied minor is an individual below the age of 18 arriving without parents or guardians. In 2015, 35,369 out of the 162,877 asylum seekers belonged to this group. Of these, 92% were men, approximately 93% were above the age of 12, and around two-thirds were Afghan citizens.

[3]Data source: Swedish Migration Agency.

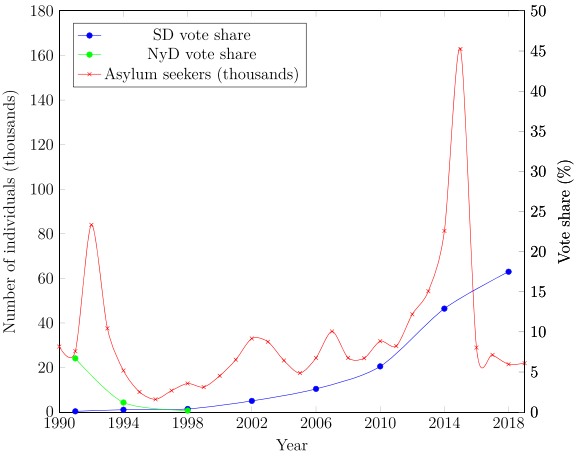

FIGURE 1 Number of asylum seekers, 1990–2019, and the vote shares of the Sweden Democrats and New Democracy 1991–2018. *Source*: Swedish Migration Agency and Statistics Sweden.

2.2 | The electoral system and the SD

Sweden is divided into 21 counties (Swedish: *län*), and 290 municipalities (*kommuner*). Parliamentary, county, and municipal elections are held every 4 years. All three elections take place on the same date, the second Sunday of September. The latest elections were on September 9, 2018. Sweden has a system of proportional representation, meaning that a party with x percent of the vote share obtains approximately x percent of the seats. This applies both at the national and local levels. The parliament, the Riksdag (*riksdagen*), has 349 seats and an election threshold of 4%, meaning that parties receiving a vote share below this threshold do not obtain any seats.

The SD were formed in 1988. Several of its founding members were involved in neo-Nazi groups before joining the newly-founded party. Since the late 1990s, the party has taken a more moderate stance and now identifies itself primarily as socially conservative. The policies of the party are similar to that of other right-wing populist parties in Europe, rejecting the idea of multiculturalism, and favoring significantly reduced immigration levels. The SD generally takes a tough stance on crime, and although not actively favoring a Swexit, it is the most Eurosceptic party in parliament.

The party was first elected into the Riksdag in 2010, when its vote share exceeded 4% for the first time. The strongholds of the SD are in the southern and western parts of Sweden, especially in rural areas. Additionally, the party has a relatively high vote share in "Rust Belt" areas in other parts of the country. It is relatively weak in the two largest cities, Stockholm and Gothenburg, as well as in rural areas of the northern part of the country, the latter being a traditional stronghold of the center-left Social Democrats. Table A1 of Supporting Information: Appendix A presents a brief overview of the eight parties currently represented in the Riksdag.

The SD are by far the most successful right-wing populist party in Swedish history. However, a similar party, New Democracy (*Ny demokrati*), was elected to the Riksdag in 1991 (around the time of the major refugee wave from Yugoslavia), when it received 6.7% of the national vote. Although reducing the number of immigrants was a key issue for New Democracy, the party was primarily anti-establishment. Amid internal disagreements, concomitant with the party moving towards hardcore anti-immigration, the party crashed out of the Riksdag in 1994, receiving only 1.2% of the vote. The more anti-immigration version of the party did, however, remain

represented in a number of municipalities post-1994, before being declared defunct in 2000. Figure 1 illustrates the historical development of the vote shares of both the SD and New Democracy.

2.3 | The Swedish refugee placement program

When an individual applies for asylum in Sweden, he or she is first placed in an asylum accommodation, until the application is either accepted or rejected. Alternatively, the asylum seeker may find his or her own accommodation. This is typically the case if the individual already has a social network in Sweden, such as relatives or friends. If the asylum application is accepted, the individual again has two alternatives: either to find own housing, or to take part in the placement program.

In the latter case, the immigrant is allocated to a municipality. Four parameters are used in deciding the quota for each municipality: Population, the labor market situation, the number of asylum seekers currently living in the municipality, as well as the number of immigrants with residence permit previously allocated to the municipality (cf. 7 § Swed. law 2016:38). Due to the large influx in 2015, a law change came into effect March 1, 2016, according to which municipalities cannot refuse to accept allocated immigrants. A similar law regarding unaccompanied minors has been in place since January 1, 2014. Furthermore, municipalities do not have any influence on the decision process, nor can they choose to be allocated a certain "type" of immigrants. Similarly, an individual cannot change municipal placement based on his or her own preferences.

Municipalities are economically compensated for the extra economic burden associated with accepting allocated immigrants. The compensation is in the form of government transfers to municipalities, of which the major share consists of a lump-sum transfer for each allocated refugee. Besides the lump-sum transfer for each allocated refugee, the municipality may apply for other types of economic compensation, for instance if an allocated individual requires costly health care. The lump-sum grant terminates 2 years after the individual has been allocated to a municipality. After this period, the costs of welfare benefits are shifted to municipalities.

Due to the large increase in the number of asylum seekers during the migrant crisis, the waiting time for an asylum decision has increased considerably. In 2012, the average waiting time was approximately 100 days; by 2017, the average waiting time had soared to 500 days.[4] Besides the time from asylum application to asylum decision, there is a time lag from asylum decision to municipality placement, if the individual chooses to participate in the program. The law mandates that municipality allocation take place within 2 months after the asylum decision (10 § Swed. law 2016:39). Hence, given the prolonged waiting times, some refugees arriving during the 2015 wave were allocated already later that year, whereas most were not allocated until 2016 or 2017. However, by the end of 2017, all but 5000 of the refugees arriving in 2015 that had received a residence permit had been allocated to a municipality.[5]

3 | VALIDITY AND STRENGTH OF INSTRUMENT

The main identifying assumption of this paper is that municipal placement is exogenous with respect to electoral outcomes. If it is not, the refugee placement program is invalid as an instrument and the IV estimates could be biased. Section 5.1 tests formally whether municipal placement was random with respect to political outcomes, conditional on the four parameters used for allocation of refugees. However, even if it is, there a number of issues with the placement program that could affect its validity and strength as an instrument for immigration. This section addresses briefly two potential sources of bias possibly arising from the placement program.

[4]Data source: Swedish Migration Agency.
[5]Data source: Swedish Migration Agency.

The first potential source of bias is the bias that may arise if municipalities refuse to participate in the placement program. The 2016 law change meant that municipalities can no longer refuse to participate in the allocation program. This was the first time in Swedish history that such a law was enacted, and since no municipality has breached the law after its enactment, there is no such bias from 2016 onward. In fact, even municipalities with a documented shortage of public housing were forced to find shelter for allocated refugees. In many cases, after depleting the entire stock of available public housing, municipalities turned to the private housing market by purchasing homes, and then acting as official second-hand landlords for allocated refugees. This is a clear contrast to the 1990s allocation scheme, where the availability of housing was the parameter that de facto decided how many were allocated (Dahlberg et al., 2012). Before 2016, it was possible for municipalities to refuse to participate in the placement program of adult refugees. However, as indicated by interviews with placement officials in Dahlberg et al. (2012), most municipalities considered immigration to be a national concern, requiring intermunicipal solidarity. By late-2014, only one municipality (Öckerö, outside Gothenburg) refused to participate in the placement program, citing a lack of housing as the reason for doing so (Astorga Diaz, 2014). Given that only one out of 290 municipalities refused to accept any refugees in 2015, municipal refusal is not a major issue in this study. Instead, the main argument behind the law change was to decrease the time from asylum decision to municipality placement (to reduce crowding at asylum accommodations), and to ensure that refugees were placed in municipalities with favorable labor market conditions. This had not previously been a parameter taken into consideration when deciding the quota for each municipality (Wennström & Öner, 2019).

Another potential source of bias is the bias that could arise when immigrants move from their original placement municipality. Individuals in the placement program have the opportunity to move to another municipality from day one, and it is well-known that immigrants and minority groups are over-represented in segregated areas in large urban areas (Borjas, 1998; Cutler & Glaeser, 1997; Cutler et al., 2008). Consequently, we can expect that some refugees will relocate between municipalities some time after their initial placement. In this case, the placement program underestimates the true immigrant share in metropolitan municipalities, and overestimates the immigrant share in small, rural municipalities.

There are no nationwide estimates on how many refugees moved from their municipality of placement. However, using register data from the Swedish Statistics Agency, Kangasharju and Brändström (2019) find that for the rural, northern county of Västerbotten, 73% of refugees arriving in 2015 were still living in their municipality of placement in August 2019. Secluded areas of northern Sweden are likely to have larger refugee outflow rates than urban counties in the south of the country. Additionally, August 2019 was 1 year after the 2018 elections, suggesting that the share of refugees living in their placement municipality at the time of the elections was even higher. During the 1990s refugee allocation, around 60% of refugees were still living in the original municipality 4 years after the initial placement (Dahlberg & Edmark, 2008). Given the sharp increase in real housing prices in Sweden during the last decade, it is reasonable to expect that current refugee mobility is lower than in the 1990s (Gustafsson et al., 2016). Thus, whereas many immigrants would prefer to settle in urban areas (where their compatriots are typically over-represented), it is often practically impossible to do so, because of the constrained access to housing associated with urban areas.

Finally, although refugees cannot change their municipality placement, one concern is whether there are compositional amenities of refugees influencing whether an individual chooses to participate in the allocation program, and if these characteristics are correlated with native voting outcomes. Interviews with refugees recently awarded residence permits in Lennartsson (2007) suggests that there are two main benefits of not participating in the program: First, choosing your own housing gives individuals a greater perception of independence, and secondly, that living in an asylum accommodation (and hence, not finding your own accommodation already in the beginning of the asylum process) is associated with boredom. However, the realized housing standard associated with private accommodation is often worse than in asylum facilities, mainly because of overcrowding in private apartments (Lennartsson, 2007). More importantly, there is no evidence that refugee characteristics, for instance education level or religion, affect the decision to participate in the placement program (National Board of Housing,

Building and Planning, 2008). Further supporting this claim, the summary statistics described in Section 4.2 show that the vast majority of both self-selected refugees and refugees participating in the allocation program hail from the Middle East and North Africa.

4 | DATA

4.1 | Data description

4.1.1 | Data on immigration

The variable immigrant inflow (*IM*) measures the total number of allocated refugees with a residence permit, as well as refugees who have self-selected into the municipality, and family migrants in 2015, 2016, and 2017. Since family migrants tend to locate in the same municipality as their relatives, they have also self-selected into the municipality. Since we cannot exclude the possibility that some migrants choose municipality based on the attitudes of natives, *IM* is potentially endogenous with respect to the right-wing vote share. Hence, I instrument *IM* with the variable allocated refugee inflow (*ARI*), which shows the cumulative number of individuals allocated to a municipality during 2015, 2016 and 2017. In total, 188,709 refugees (allocated and self-selected) and family migrants of refugees with a residence permit were registered in a municipality in 2015, 2016, and 2017 (*IM*). Of these, 60,855 were refugees[6] participating in the placement program (*ARI*), 81,300 were refugees self-selecting into a municipality and 46,554 were family migrants of refugees. Hence, among refugees, 43% participated in the placement program. Of these, 19,389 were unaccompanied minors, of which 14,387, or 74%, participated in the placement program.[7] Table A2 of Supporting Information: Appendix A provides some further insight into the characteristics of the different categories of immigrants with respect to countries of origin.

Note that *IM*, defined as the municipal-wise sum of refugees and family migrants, is a better proxy for refugee immigration than is the immigrant population share of each municipality. This is because the immigrant population share does not only reflect the refugee population of each municipality, but takes into account skilled labor immigration and immigration from EU countries in addition to refugee immigration and family migrants. This contrasts *IM*, which by construction only captures immigration of refugees and their relatives.

Let $ARI_{i,t}$ be the allocated refugee inflow in municipality i for each of the 3 years considered. Summarizing over all 3 years and averaging, yields the allocated refugee inflow rate (*ARIR*) for municipality i, defined as

$$ARIR_i = \frac{\sum_{t=2015}^{2017} ARI_{i,t}}{\overline{POP}_i} \times 100, \tag{1}$$

where the variable average population ($\overline{POP}$) is the average population in the municipality between 2015 and 2017. Analogously, the endogenous immigration rate (*IMR*) is calculated as

$$IMR_i = \frac{\sum_{t=2015}^{2017} IM_{i,t}}{\overline{POP}_i} \times 100. \tag{2}$$

For the unaccompanied minors, the allocated unaccompanied minors inflow rate is defined analogously to (1), and unaccompanied minors immigration rate is defined as (2), but with these variables considering only the unaccompanied minors in the numerator in lieu of total immigration.

[6]Note that family migrants of other immigrants, for instance relatives of labor migrants, are excluded from this figure.
[7]Data source: Swedish Migration Agency.

4.1.2 | Election data

The main analysis considers only parliamentary elections. The main reason behind this is that the SD have had large difficulties filling local seats, leading to many seats being left vacant. Since voters may fear "wasting" their votes, the SD usually fare worse in local elections compared to parliamentary elections. Moreover, many municipalities have small, local parties, who only run in the local elections. Some of these parties are anti-immigration or populist, which makes intermunicipal comparisons difficult for local elections.[8] The variable $\Delta SD_i^{2014-2018}$ is the percentage point difference in the SD vote share for municipality *i* between 2014 and 2018, and is our main outcome variable of interest. The maps in Figure A1 of Supporting Information: Appendix A show the relationship between allocated refugee inflow rates and the 2014–2018 growth of the SD.

One question that this paper tries to answer is whether past anti-immigration sentiments affect the current SD vote share increase. As a proxy for historical anti-immigration sentiments, the variable right-wing populisT 1994 is equal to unity if either New Democracy, the SD or some local anti-immigration party was represented in the municipal council after the 1994 election, which was the election following the Yugoslavian migration shock, and zero else.[9]

Similarly, to measure how current attitudes toward immigration are affected by historical opposition to the European Union, I define the variable EU "no" vote share 1994 as the share of "no" votes in each municipality at the 1994 EU referendum. Less than 2 months after the 1994 elections, there was a nonbinding referendum on whether Sweden should join the European Union, or remain outside of the bloc.[10] A majority of voters (52.3%) approved, and Sweden joined the EU in 1995.

4.1.3 | Data on municipal characteristics

In addition to the immigration variables, I deploy a set of control variables to characterize the municipality. All municipal controls are measured in 2014, which was the year preceding the migration wave. The municipal controls include population, the unemployment rate (the unemployed share of the workforce in the age group 15–64), the SD vote share in the 2014 parliamentary elections, the number of asylum seekers, the crime rate (defined as the number of reported crimes per capita), and the local tax rate. Further, the set of controls includes the share of residents with a foreign background, where an individual has a foreign background if the person is either born abroad or has at least one parent born abroad, the share of the workforce employed in manufacturing, as well as the net domestic outflow per capita. The latter is defined as the number of individuals moving to another municipality within Sweden minus the number of people moving in from another municipality in 2014, divided by the total population in that year. If this variable is positive, more individuals move out of the municipality than into the municipality. Moreover, I control for the number of refugees allocated to the municipality between 2012 and 2014. This is a proxy for previously allocated refugees, which is a parameter deciding the current number of allocated refugees.

As a geographical control, I use lands dummies, with Svealand (central Sweden) as the baseline category.[11] Finally, I include a dummy equal to unity if the municipality is located in Stockholm county or around the

[8]The results in Table A16 of Supporting Information: Appendix A show that the conclusions regarding the relationship between immigration rates and SD voting is robust when using local election results.

[9]Unfortunately, we do not have access to municipality-level vote shares of parties not currently represented in the Riksdag.

[10]The referendum took place November 13, whereas the parliamentary elections were on September 18. The turnout rates were similar: 83.3% in the referendum and 86.8% in the parliamentary elections.

[11]Similar to how the United States is divided into four census regions, namely the Northeast, Midwest, West and South, Sweden is similarly divided into three lands (*landsdelar*), representing the northern, central, and southern part of the country, respectively. This variable is included to capture the large differences in voter mobility across lands, with Northern Sweden in particular being a static center-left stronghold. See Supporting Information: Appendix B for additional details.

second-largest city of Gothenburg, and zero else. This is to account for the fact that the SD are weaker in urban areas.[12] Supporting Information: Appendix B provides additional details regarding the data sources for all variables used in the empirical analysis.

4.1.4 | Survey data

As a complement to election results, I use data from the annual *SOM*[13] survey to evaluate respondents' views on immigration. Using individual-level data facilitates our understanding of the mechanisms behind the results stemming from aggregate variables. The survey takes the form of a paper questionnaire.

The SOM survey has several key features making it suitable for our analysis. First, the respondents are chosen randomly among the Swedish adult population (aged 16–85), which is important for inference. Second, the relatively large sample size—around 3500 observations per year—allows for municipality-level breakdown of attitudes towards immigration. More specifically, starting in 2011, the survey contains a question in which respondents are asked to rank the top three societal problems facing Sweden. Thus, I calculate, for each municipality, the share of respondents naming "immigration" as the main, or one of the three main challenges, for the years 2011–2014, as well as for the years 2015–2018. I repeat this procedure for two other potential challenges, namely "crime" and "jobs." This allows us to compare the relative importance of immigration relative to other policy issues for the SD vote, before and after the refugee wave.

To complement the SOM survey, I use individual data from an additional survey: the so-called *VU* survey[14] The survey follows around 8000 respondents during the 2014–2018 period, asking respondents which party they voted for in the respective election. Hence, this provides us with individual-level data on party switching voters. This information is then matched with register data on demographic characteristics, such as income group and education. Since the VU survey only registers the home county of the respondent, which is coarser than the municipality level, I aggregate the population-weighted immigration rates at the county level and match this with the individual-level data.

4.2 | Summary statistics

Table 1 provides summary statistics of the variables used in the analysis. There is considerable variation both in the allocated refugee inflow rate, as well as in the immigration rate. The share of allocated refugees (*ARIR*) in the total population was 3% in the municipality with the highest rate, and a mere 0.08% in the municipality with the lowest allocated refugee inflow rate. The mean share of allocated refugees over the entire 3-year period is approximately corresponding to 0.77% of the average population in that period. When considering the total immigration rate (allocated refugees, self-selected refugees and family migrants, *IMR*), the average share of immigrants is about 2.5%.

One concern raised previously is the possibility of certain types of migrants (with characteristics that might have differential effects on native attitudes) choose not to take part in the allocation program, and that other types participate. Table A2 of Supporting Information: Appendix A provides data on the largest origin countries of all migrants arriving in a municipality between 2015 and 2017. The four largest immigrant groups are Syrians, Eritreans, Afghans and stateless individuals (mostly Palestinians). Citizens of Afghanistan and Eritrea are somewhat

[12]Several municipalities around Stockholm and Gothenburg are relatively small in population, meaning that the population size control fails to capture this effect.
[13]Shorthand for "Society, Opinion, Media" (*Samhälle, Opinion, Medier*).
[14]Shorthand for "The Election Survey" (*Valundersökningen*).

TABLE 1 Summary statistics

	Mean	Std. dev.	Min	Max
Exogenous immigration variables				
Allocated refugee inflow, 2015–2017	209.84	408.25	13	5943
Allocated refugee inflow rate, 2015–2017 (%)	0.773	0.379	0.083	3.00
Allocated unaccompanied minors inflow, 2015–2017	49.61	81.38	4	1,021
Allocated unaccompanied minors inflow rate, 2015–2017 (%)	0.210	0.142	0.030	0.917
Endogenous immigration variables				
Total immigration, 2015–2017	650.72	1010.40	66	11,281
Total immigration rate, 2015–2017 (%)	2.52	1.55	0.80	11.834
Unaccompanied minors immigration, 2015–2017	58.24	95.69	4	1238
Unaccompanied minors immigration rate, 2015–2017 (%)	0.279	0.170	0.048	1.076
Election variables				
$\Delta SD^{2014-2018}$ (p.p.)	5.62	1.85	0.99	15.21
SD vote share, 2014	15.34	4.47	5.30	29.96
Right-wing populist 1994	0.131	0.338	0	1
EU "no" vote share 1994	52.47	11.66	11.97	81.82
Allocation parameters				
Population, 2014	33,611.57	69,275.21	2,541	911,989
Unemployment rate, 2014 (%)	8.40	2.57	2.60	15.80
Number of asylum seekers, 2014	450.63	1062.25	0	9931
Allocated refugee inflow, 2012–2014	83.73	73.56	0	542
Municipal characteristic controls				
Reported crime rate, 2014	0.095	0.029	0.040	0.229
Local income tax rate, 2014 (%)	32.63	1.11	29.19	34.70
Share of residents with a foreign background, 2014 (%)	15.79	7.96	5.15	56.36
Net domestic outflow rate, 2014 (%)	0.282	1.002	−2.50	6.65
Share of the workforce employed in manufacturing, 2014	0.125	0.059	0.032	0.371
Northern Sweden dummy	0.161	0.368	0	1
Central Sweden dummy	0.283	0.451	0	1
Southern Sweden dummy	0.417	0.494	0	1
Urban area dummy	0.149	0.356	0	1
Additional election variables for panel analysis				
SD vote share, 2010	6.30	2.53	1.70	15.80
SD vote share, 2018	20.96	5.47	8.58	39.23
Total immigration rate, 2007–2010 (%)	0.908	0.747	0	7.29

TABLE 1 (Continued)

	Mean	Std. dev.	Min	Max
Total immigration rate, 2011–2014 (%)	1.56	1.17	0.028	6.85
Total immigration rate, 2015–2018 (%)	3.10	1.73	1.07	13.23
Allocated refugee inflow rate, 2007–2010 (%)	0.522	0.584	0	6.45
Allocated refugee inflow rate, 2011–2014 (%)	0.666	0.621	0.014	3.892
Allocated refugee inflow rate, 2015–2018 (%)	0.957	0.461	0.102	3.591
Unaccompanied minors immigration rate, 2007–2010 (%)	0.071	0.193	0.048	2.23
Unaccompanied minors immigration rate, 2011–2014 (%)	0.194	0.246	0	2.34
Unaccompanied minors immigration rate, 2015–2017 (%)	0.365	0.198	0.092	1.29
Allocated unaccompanied minors inflow rate, 2007–2010 (%)	0.057	0.186	0	2.23
Allocated unaccompanied minors inflow rate, 2011–2014 (%)	0.158	0.236	0	2.34
Allocated unaccompanied minors inflow rate, 2015–2018 (%)	0.241	0.158	0.048	1.05

more likely than other nationalities to participate, but overall, virtually all refugees, both allocated and self-selected, are from Muslim-majority countries in the Middle East and North Africa.

5 | THE IMPACT OF IMMIGRATION ON THE RIGHT-WING POPULIST VOTE

5.1 | Test of identifying assumptions

5.1.1 | Parameters deciding allocation

First, it is of importance to verify that past election results did not affect the allocation of refugees. According to law, the four parameters that are to be used in the allocation are population, the labor market situation, previously allocated refugees and the number of asylum seekers currently dwelling in the municipality. As described in Section 4.1, an intuitive way of quantifying previously allocated refugees is to use the cumulative number of allocated refugees during the preceding 3-year period (2012–2014). I use unemployment as a proxy for the labor market situation, and measure previous electoral outcomes by using the SD vote share in the 2014 elections. I thus estimate

$$\begin{aligned} \mathrm{ARI}_i^{2015-2017} = c_0 + \alpha_1 \overline{\mathrm{POP}}_i^{2014-2016} + \alpha_2 \overline{\mathrm{UNEMP}}_i^{2014-2016} + \alpha_3 \overline{\mathrm{ARI}}_i^{2012-2014} \\ + \alpha_4 \overline{\mathrm{ALS}}_i^{2014-2016} + \alpha_5 \mathrm{SD}_i^{2014} + u_i, \end{aligned} \tag{3}$$

where $\mathrm{ARI}_i^{2015-2017}$ is the cumulative number of allocated refugees between 2015 and 2017, c_0 is a constant, $\overline{\mathrm{POP}}_i^{2014-2016}$ is now the average population between 2014 and 2016, $\overline{\mathrm{UNEMP}}_i^{2014-2016}$ is the average unemployment rate over the same time period, $\overline{\mathrm{ARI}}_i^{2012-2014}$ is the average number of refugees allocated to the municipality over the preceding 3-year time period, $\overline{\mathrm{ALS}}_i^{2014-2016}$ is the average number of asylum seekers residing in the municipality between 2014 and 2016 (regardless of whether they live in an asylum accommodation or if they have arranged for private residence), SD_i^{2014} is the SD vote share in the 2014 election,[15] and u_i is an error term.

Because the allocation for the year t is done at time point $t-1$, the number of allocated refugees is calculated for the period 2015–2017, whereas the covariates on the right-hand side of (3) are for the period 2014–2016. If placement officials complied with the law when allocating refugees, $\hat{\alpha}_1$, $\hat{\alpha}_2$, $\hat{\alpha}_3$, and $\hat{\alpha}_4$ should all be significant, while $\hat{\alpha}_5$ should be insignificant if the allocation was random with respect to previous election outcomes.

Table 2 shows the results of this regression. The first results column is the baseline specification, using only the four variables defined by law to be used in the allocation, namely population, unemployment, the number of previously allocated refugees and the number of asylum seekers currently residing in the municipality. The center column is precisely Equation (3), namely the regression including the four parameters augmented with the SD vote share in 2014. This variable should have no impact on the allocation of refugees, and is added to exclude the possibility of biased placement officials. If the coefficient is significant, the allocation is not random with respect to the SD outcome in the last election. Finally, the rightmost column of Table 2 includes the pre-influx (2014) municipal controls defined in Section 4.1, namely the reported crime rate, tax rate, share of residents with a foreign background, net domestic outflow rate, the share of the workforce employed in manufacturing, as well as the lands dummies and the urban area dummy.[16]

The baseline specification with only the four allocation parameters explains approximately 95% of the variation in allocated refugees, suggesting that we can exclude the possibility of unobservable or unknown variables having a significant impact on the allocation quota for each municipality. The only significant variables in all three specifications are population, the unemployment rate and the number of asylum seekers currently living in the municipality. As expected, larger municipalities, municipalities with lower unemployment rates and municipalities with relatively few asylum seekers are allocated larger numbers of refugees. More importantly, however, the results suggest that there is no indication of biased placement officials systematically basing the allocation on election outcomes. This also holds when including the full set of municipality controls.

Although municipalities receive a yearly quota of allocated refugees, the actual instrument used in the empirical analysis is the cumulative share of allocated refugees, $ARIR_i$. Table A3 of Supporting Information: Appendix A presents the results when re-estimating (3) with the share of refugees as the left-hand side variable instead of the number of refugees.[17] The coefficient for the SD vote share is close to zero and statistically insignificant. Again, these results suggest that there is no evidence that municipalities with significant SD support received larger (or smaller) shares of refugees.

5.2 | Main results

5.2.1 | Causal estimates

Having established that there is no evidence of the allocation scheme not being random with respect to past electoral outcomes, I now move on to answer the question whether there is a causal effect of immigration on the right-wing vote share. Figure 2 illustrates the relationship between IMR_i, that is, the rate at which refugees and family migrants settle in municipality i over the 3-year period starting in 2015, and the percentage point change in the SD vote between 2014 and 2018.

This illustration suggests a positive relationship. However, as discussed previously, IMR_i is likely to be endogenous due to the possibility of self-selection into municipalities. To correct this, I use the exogenous variation

[15]Table A19 of Supporting Information: Appendix A presents the summary statistics for these variables.

[16]I exclude the 2014 population, unemployment rate and share of asylum seekers in this specification due to high colinearity with the 2014–2016 average population, unemployment rate and share of asylum seekers.

[17]For the right-hand side variables in this specification, I calculate the per capita shares of asylum seekers and the per capita share of allocated refugees 2012–2014, and drop population as allocation parameter.

TABLE 2 Test of identifying assumptions

	Baseline	2014 SD vote share included	Additional controls included
Average population 2014–2016	0.006***	0.006***	0.007***
	(0.001)	(0.001)	(0.001)
Average unemployment rate 2014–2016	−10.128***	−9.694***	−11.326***
	(2.367)	(2.641)	(2.740)
Average number of alloc. refugees 2012–2014	0.619*	0.583*	0.473
	(0.309)	(0.317)	(0.307)
Average number of asylum seekers 2014–2016	−0.176***	−0.176***	−0.230***
	(0.054)	(0.055)	(0.053)
SD vote share 2014		−0.719	0.230
		(1.635)	(1.916)
Observations	290	290	290
Mean dep. var.	209.84	209.84	209.84
R^2	0.9466	0.9468	0.9535

Notes: Dependent variable: Cumulative number of allocated refugees, 2015–2017. Standard errors clustered by municipality in brackets. * and *** denote significance at the 10% and 1% level, respectively.

induced by the municipal placement program. The two-stage least squares (2SLS) model for municipality i is specified as follows:

$$\text{IMR}_i^{2015-2017} = c_1 + \beta_1 \text{ARIR}_i^{2015-2017} + \boldsymbol{\beta}' \boldsymbol{X}_i + \varepsilon_{1i}, \tag{4}$$

$$\Delta \text{SD}_i^{2014-2018} = c_2 + \gamma_1 \widehat{\text{IMR}}_i^{2015-2017} + \boldsymbol{\gamma}' \boldsymbol{X}_i + \varepsilon_{2i}, \tag{5}$$

where c_1 and c_2 are constants in the first and second stage regressions, $ARIR_i$ is the cumulative number of refugees assigned to municipality i between 2015 and 2017 normalized by its average population, $\boldsymbol{X}_i$ is a vector of control variables, and ε_{1i} and ε_{2i} are error terms in the first and second stage regressions. To evaluate the effect of the unaccompanied minors immigration, it is straightforward to replace *ARIR* and *IMR* in (4) and (5) with their unaccompanied minors equivalents.

Table 3 presents the 2SLS results, where column (1) utilizes the lagged 2014 SD vote share as a control variable, in addition to the allocation parameters, namely the population, unemployment rate, number of asylum seekers, and the 2012–2014 number of allocated refugees.[18] To avoid any endogeneity, I use the 2014 values for population, unemployment rate, and number of asylum seekers, which was the year preceding the refugee wave. Column (2) includes the full set of municipality controls measured in 2014, that is, the crime rate, tax rate, share of

[18]Some readers might worry that the 2012–2014 number of allocated refugees induces multicollinearity with the share of allocated immigrants 2015–2017. Dropping this variable from the specification does not alter the conclusions in the following paragraphs. The results in Table 3 when excluding the 2012–2014 number of allocated refugees are available upon request.

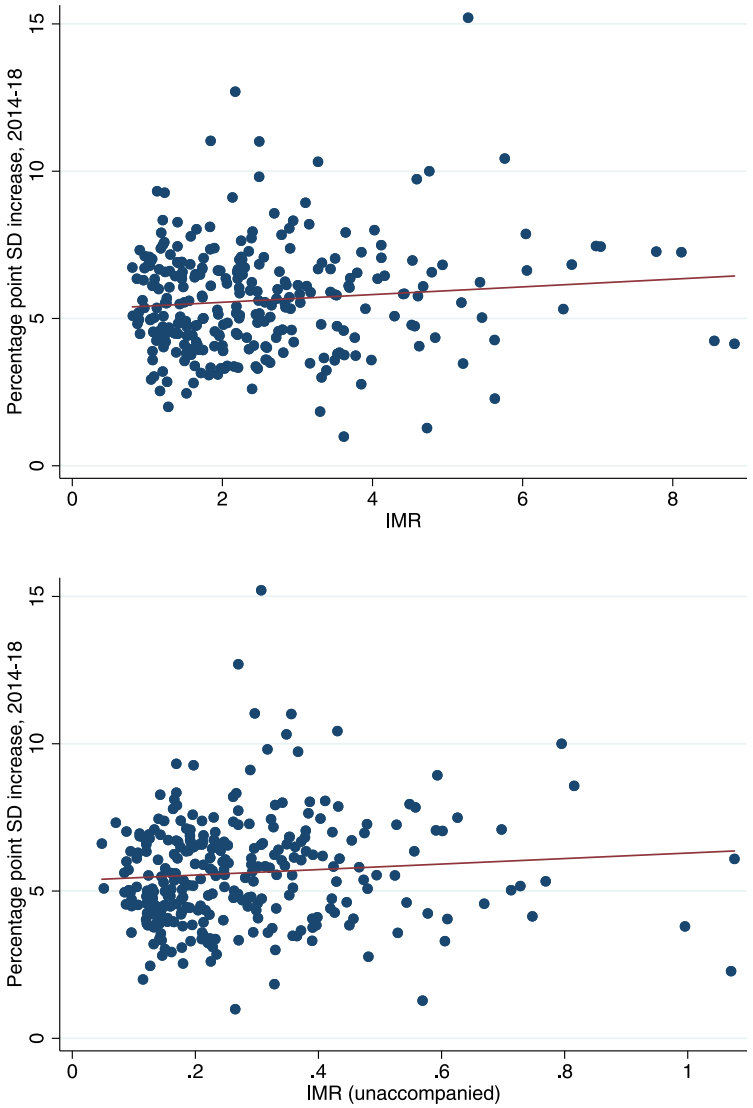

FIGURE 2 Top: Scatter plot with the immigration rate on the horizontal axis and percentage point increase in SD vote shares on the vertical axis. One municipality omitted for visual clarity reasons. Bottom: Scatter plot with the immigration rate of unaccompanied minors on the horizontal axis and percentage point increase in SD vote shares on the vertical axis.

oreigners, the net domestic outflow rate per capita, the share of the workforce employed in manufacturing, the lands dummies, and the urban area dummy.

Focusing on (2), we see that for each percentage point increase in the immigrant share, holding other variables constant, the SD margin increases by approximately 1.23 percentage points, which is significant at the 5% level. Similarly, for each percentage point increase in the unaccompanied minors share, the SD margin increases by

TABLE 3 Results, main specification

	All refugees		Unaccompanied minors	
	(1)	(2)	(1)	(2)
First stage				
Allocated refugee inflow rate	0.712***	0.731***	1.073***	1.067***
	(0.200)	(0.234)	(0.026)	(0.027)
Second stage				
Total immigration rate	0.653	1.233**	0.472	1.290*
	(0.480)	(0.559)	(0.755)	(0.769)
Allocation parameters included	Yes	Yes	Yes	Yes
Lagged SD vote share included	Yes	Yes	Yes	Yes
Municipal characteristic controls included	No	Yes	No	Yes
Observations	290	290	290	290
Mean dep. var.	5.62	5.62	5.62	5.62
F statistic of excl. instruments	12.40	12.64	1977.24	1836.48

Note: First stage dependent variable: Total immigration rate, 2015–2017. Second stage dependent variable: Percentage point change in the SD vote share, 2014–2018. Column (1), Controls included for 2014 values of population size, unemployment rate, number of asylum seekers, 2012–2014 number of allocated refugees, and the 2014 SD vote share. Column (2), All previous controls augmented with the 2014 crime rate, municipal tax rate, share of residents with a foreign background, net domestic outflow rate, share of the workforce employed in manufacturing, lands dummies, and an urban area dummy. The mean of the dependent variable refers to the dependent variable in the second stage, namely the percentage point difference in the SD vote share between 2014 and 2018. Standard errors clustered by municipality in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

approximately 1.29 percentage points. It is notable that both coefficients are significant only when including the pre-influx municipality controls, suggesting that there is considerable regional heterogeneity in immigration-driven changes in right-wing populist voting.

Additionally, the results show that the coefficient for the allocated refugee rate is highly significant in the first stage, suggesting that the instrument is relevant in the sense that it is a strong predictor of the endogenous variable. A caveat to note for the causal interpretation of the previous findings is that the IV strategy works particularly well for the unaccompanied minors, since members of this group are less likely than refugees in general to opt out of the allocation program. This is mirrored in the F statistic for excluded instruments, which is considerably higher for the unaccompanied minors. Table A4 of Supporting Information: Appendix A gives the ordinary least squares (OLS) estimates for comparison. Using OLS underestimates the true impact of immigration on the SD vote share, particularly when considering all refugees. In this case, the estimated $\hat{\gamma}_1$ is around one-tenth of the IV estimate when using the full set of controls. For the unaccompanied minors, the coefficient estimates are closer, which is expected considering that most of the unaccompanied minors participate in the placement program.

Finally, we examine the electoral impact of immigration on the two other main parties, namely the center-left Social Democrats, and the center-right Moderate Party. The results are presented in Table A5 of Supporting Information: Appendix A. Immigration had a significant dampening effect on the vote share of the Social Democrats, but not on the Moderate Party. The effect is also strong for immigration of unaccompanied minors.

5.2.2 | The impact of municipal characteristics

The results established previously suggest that pre-influx regional heterogeneity may impact how voters responded to the refugee shock. The following section tries to answer whether immigration affects electoral outcomes conditional on these municipal characteristics, focusing primarily on comparing the estimates for refugees in general with those for the unaccompanied minors. Alternatively stated, are there certain regions where voters were more likely than in others to shift to the SD in response to the refugee wave? I interact immigration rates with each of the pre-inflow (2014) municipal characteristics used as controls in the full specification of the previous regressions, thus estimating the second-stage equation[19] as

$$\Delta SD_i^{2014-2018} = c_2 + \gamma_1 \widehat{IMR}_i^{2015-2017} + \gamma_2 X_{1i} + \gamma_3 \left(\widehat{IMR}_i^{2015-2017} \times X_{1i} \right) + \boldsymbol{\gamma}' \tilde{\boldsymbol{X}}_i + \varepsilon_{2i}, \quad (6)$$

where X_{1i} is the pre-inflow municipality characteristic of interest for municipality i, and $\tilde{\boldsymbol{X}}_i$ is the vector of remaining controls. Due to the large number of municipal controls, this section presents the estimates for the most important interactions, all of which have significant interaction coefficients. Table A6 of Supporting Information: Appendix A provides the coefficient estimates for the interactions with the remaining municipal characteristic controls.[20] To facilitate the interpretation of the interaction terms, I calculate the corresponding z scores for each of the continuous controls by demeaning each variable and dividing by its standard deviation. Thus, the coefficient estimate $\hat{\gamma}_3$ can be interpreted as the additional effect on the SD margin by a one percentage-point increase in the immigrant share in municipalities with a one standard deviation higher value of the respective municipal characteristic. Note that the main effect for each municipal characteristic, $\hat{\gamma}_2$, is also standardized. In each of the regressions, I use the full set of remaining controls.

Panel A of Table 4 gives the results for the interaction with the pre-influx crime rate. The interaction term is statistically significant only for the unaccompanied minors, and is equal to around 1.7. Hence, for each percentage point increase in the unaccompanied minors share, the immigration-related increase in the SD margin increases from the baseline value of 1.6 percentage points to 2.7 percentage points.[21]

Continuing, the interaction between immigration of unaccompanied minors and the net domestic outflow rate has a significant and negative impact on the SD vote, as shown in Panel B of Table 4. Given a one standard deviation higher net domestic outflow rate, the increase in the SD margin that is due to immigration of minors is cut by around three quarters, from 2.4 to 0.6 percentage points.[22] Finally, the interaction between the immigration rate and the share of residents with a foreign background shown in Panel C is also significant only for the unaccompanied minors. For each percentage point increase in the unaccompanied minors share, one standard deviation higher share of residents with a foreign background is associated with an increase in the SD margin from 1.4 to 3.1 percentage points.

The finding on the negative impact of the pre-influx net domestic outflow rate on the SD vote is a notable contrast to Panel A (the crime rate) and C (the share of foreigners in the municipality). In areas with social problems, for instance relatively high crimes rates, an additional inflow of young immigrant men boosts the anti-immigration vote further. Considering that individuals with such characteristics are often assumed to be over-represented in crime statistics, this does not come as a major surprise. Conversely, a similar inflow of

[19]Note that when there are two endogenous variables (in this case, the immigration rate and the immigration rate multiplied by the pre-influx municipality characteristic), there are two first-stage regressions.

[20]That is, the 2014 SD vote share, 2014 tax rate, 2014 share of the workforce employed in manufacturing, and the urban area dummy. The lands dummies have statistically insignificant interactions and are highly Sweden-specific, and are available upon request.

[21]Calculated as 1.560 − 0.522 + 1.686 = 2.724. Note that a one standard deviation higher crime rate in 2014 actually decreased SD vote share growth between 2014 and 2018. This is likely because pre-influx crime rates were higher in urban areas.

[22]Calculated as taking the coefficient for the unaccompanied immigration rate minus the corresponding interaction term. The main effect for the net domestic outflow rate is not statistically different from zero.

TABLE 4 Municipal characteristics interactions

	All refugees	Unaccompanied minors
Panel A: Crime rate, 2014		
Immigration rate	2.084**	1.560**
	(0.936)	(0.739)
Crime rate	−3.347	−0.522*
	(2.648)	(0.275)
Immigration rate	1.351	1.686***
Crime rate, 2014	(0.965)	(0.581)
Panel B: Net domestic outflow rate, 2014		
Immigration rate	1.176	2.391***
	(1.095)	(0.714)
Net domestic outflow rate	−1.157	0.304
	(5.189)	(0.255)
Immigration rate × Net domestic outflow rate	0.061	−1.794***
	(1.332)	(0.563)
Panel C: Share of foreigners, 2014		
Immigration rate	1.821	1.430*
	(1.20)	(0.743)
Share of foreigners	−3.557	−0.219
	(3.650)	(0.202)
Immigration rate Share of foreigners	1.650	1.628**
	(1.584)	(0.807)
Allocation parameters included	Yes	Yes
Lagged SD vote share included	Yes	Yes
Municipal characteristic controls included	Yes	Yes
Observations	290	290
Mean dep. var.	5.62	5.62

Notes: Second stage 2SLS results. Dependent variable: Percentage point change in SD vote share, 2014–2018. Each specification uses the full set of controls. Standard errors clustered by municipality in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

naccompanied minors in depopulation areas decreases enthusiasm for the SD, possibly because of the favorable demographic characteristics of this immigrant group. As discussed in the Introduction, in regions adversely affected by urbanization, an influx of young residents may contribute to an increase in economic activity, possibly contributing to a revitalization of such regions. In regions with social problems, such as high crime rates already before the influx, a increase in young immigrants further boosts right-wing populist voting. Thus, pre-influx regional heterogeneity is crucial to understanding whether a sudden refugee wave will increase or decrease anti-immigration voting. For refugees in general, however, the estimated interaction

coefficients are insignificant, and in the case of the interaction with the pre-influx outflow rate, close to zero. This result suggests that immigration of refugees in general is not as regionally polarizing as immigration of young men.

5.3 | Historical persistence

5.3.1 | Historical anti-immigration attitudes

The previous section investigates how current local characteristics affect SD voting. A closely related question concerns to what extent regional historical voting outcomes affect modern-day political preferences, and how these are affected by immigration. Table 5 shows the second stage 2SLS results when extending the model to include the interaction between the immigration rate and right-wing populist 1994, with the change in the SD vote share as the outcome variable.

The main effect from local right-wing populist presence following the Yugoslavian immigration wave is positive and highly significant. In these municipalities, the increase in the SD margin was around 5 percentage points higher

TABLE 5 Interaction with historical voting

	All refugees	Unaccompanied minors
Panel A. Interaction with 1994 local presence of right-wing populists		
Immigration rate	1.310**	1.464*
	(0.569)	(0.784)
Right-wing populists 1994	4.767**	1.168
	(2.579)	(0.757)
Immigration rate × Right-wing populist 1994	−1.775	−2.583
	(1.131)	(3.370)
Panel B. Interaction with standardized 1994 EU "no" votes		
Immigration rate	3.000**	2.526***
	(1.253)	(0.788)
EU "no" vote share 1994	0.937	0.269
	(0.918)	(0.186)
Immigration rate EU "no" votes 1994	−0.939	−1.550***
	(0.557)	(0.588)
Allocation parameters included	Yes	Yes
Lagged SD vote share included	Yes	Yes
Municipal characteristic controls included	Yes	Yes
Observations	290	290
Mean dep. var.	5.62	5.62

Notes: Second stage 2SLS results when including interactions with historical voting outcomes. Each specification uses the full set of controls. Standard errors clustered by municipality in brackets. *, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

than in municipalities without nationalist representation in 1994. However, the interaction term with the immigration rate is negative. This provides some support to the contact hypothesis of Allport (1954), according to which prejudice among natives can be mitigated if natives interact with immigrants. However, this negative interaction effect is not numerically large enough to offset the large baseline effect from local right-wing populist presence in 1994. For the unaccompanied minors, the interaction coefficient is numerically larger than for refugees in general, although there is more variation around this estimate.

This result suggests that latent anti-immigration sentiments can explain some of the immigration-related increase in the right-wing populist vote. In addition, it further underlines the importance of regional aspects when assessing whether a refugee shock will affect the support for right-wing populists.

5.3.2 | Differential attitudes towards the European Union

Recently, there has been considerable interest in the regional and social aspects of Brexit voting patterns, especially in light of the strong correlation between support for Leave and anti-immigration attitudes (Alabrese et al., 2019; Colantone & Stanig, 2018; Fetzer, 2019; Liberini et al., 2019). Thus, a question of interest is whether there is a relationship between historical anti-EU attitudes and current scepticism towards immigration, similar to the relationship between right-wing populism in the 1990s and current SD voting. Panel B of Table 5 presents the results when interacting the standardized "no" votes from the 1994 EU referendum and the current immigration rate. The results suggest, perhaps surprisingly, that the interaction between current unaccompanied minors immigration rates and "no" votes in 1994 is significant and negative. Although the main effect from 1994 voting is close to zero, the interaction term is highly significant for the unaccompanied minors.

Although a confounding factor is the possibility of a recent shift in voter preferences towards the EU, these results argue against a relationship between anti-immigration and anti-EU attitudes on the regional level. Instead, the unaccompanied immigration-related increase in the SD vote between 2014 and 2018 was larger in areas with anti-immigration, but pro-EU, sentiments in the 1990s. The importance of this group of traditionally center-left voters—positively oriented towards immigration for political reasons, but negative towards free trade and economic integration—has been noted in several other studies (Beaudonnet & Gomez, 2017; van der Waal & de Koster, 2018).[23] A possible explanation is that the largest concentration of "no" votes were in Social Democratic-dominated rural areas where voters feared job losses due to economic integration (Gilljam & Holmberg, 1996). Many of these municipalities have been adversely affected by urbanization in the last years, meaning that a boost of young immigrants could contribute to a local revitalization. This is similar to the argument made previously regarding the lower SD support in areas with high levels of domestic outflow.[24] In any case, this result gives further support our previous claim that immigration of minors is particularly polarizing.

6 | ALTERNATIVE MECHANISMS AND ROBUSTNESS

6.1 | Native response to immigration

With respect to the identification strategy, one additional concern is related to the response of natives to immigration. A number of studies suggest that there is a positive relationship between immigration rates and native

[23]Many of Europe's center-left parties, as well as the US Democratic Party, have traditionally been sceptical of immigration of workers, but more inclined to support immigration of refugees and family migrants (Welch, 2015). Additional anecdotal evidence of this can be found in the 2020 Democratic primaries, where candidate Bernie Sanders was highly opposed to visa workers, while taking a pro-refugee stance.

[24]The correlation between the EU "no" votes and the net domestic outflow rate is around 0.45, which provides some support to this line of thought.

internal migration (Borjas, 2006; Card, 2009; Mocetti & Porello, 2010; Saiz & Wachter, 2011). If better-off and more liberal Swedes moved out in response to immigration, it would lead to an upward bias in the results. On the other hand, if individuals sceptical towards immigration moved out, it would create a downward bias. To test whether there was significant exodus of natives from municipalities with high rates of immigration, I estimate the following model. The first stage is the same as before, and the second stage is described by

$$\text{Net native outflow rate}_i^{2016-2018} = c_2 + \gamma_1 \widehat{\text{IMR}}_i^{2015-2017} + \boldsymbol{\gamma}'\mathbf{X}_i + \varepsilon_{2i}. \quad (7)$$

The left-hand side variable Net native outflow rate$_i^{2016-2018}$ measures the net outflow of Swedish-born individuals from municipality i between 2016 and 2018 divided by the average population during this time period.[25] The variable differs in two ways from the depopulation variable net domestic outflow rate. First, we now measure only the outflow of Swedish-born individuals, and secondly, include overseas relocations as well. The results are presented in the left-hand column of Table A7 of Supporting Information: Appendix A. The estimate of the coefficient is statistically insignificant, suggesting that there was no significant native outflow in municipalities with high immigration rates during 2015–2017. This result is in line with recent studies arguing against native flight in response to immigration (Moraga et al., 2019; Peri & Sparber, 2011).

Similarly, immigration could have an impact on turnout rates, that is, voter participation. If turnout is different in areas with high immigration rates compared to areas with low immigration rates, this could bias the results. Thus, I re-estimate the second stage described by (7) with the difference in turnout rates between the elections in 2014 and 2018 as the dependent variable. The results are presented in right-hand column of Supporting Information: Table A6. Again, the coefficient estimate is negative, although statistically insignificant. This result suggests that there is no significant impact of immigration on electoral turnout.

6.2 | Alternative measures of regional disparity

One of the main findings of this paper is that in depopulation areas, the increase in the SD margin due to immigration was lower than in municipalities with high levels of population growth just before the onset of the refugee crisis. As an alternative measure of depopulation, Table A8 of Supporting Information: Appendix A provides the results when the immigration rate is interacted with the variable Net native outflow rate$_i^{2014}$. Again, this is the Swedish-born outflow rate including overseas allocations, immediately before the refugee crisis. This contrasts the measure used in Panel B of Table 4, which includes the domestic outflow of both foreign-born individuals, as well as Swedish-born. However, as before, the interaction effect is only significant for the unaccompanied minors. Whereas the baseline increase in the SD margin was 1.6 percentage points for each percentage point increase in unaccompanied minors immigration, one standard deviation higher net native outflow rate decreases SD votes by around one percentage point. This result further supports the finding that for voters in regions adversely impacted by urbanization, immigration of young men decreased right-wing populist voting.

There is a strong relationship between the net domestic outflow rate and the age structure of the municipality in 2014. The results in Table A9 of Supporting Information: Appendix A suggest that the interaction between immigration rates and the average age of residents in the population is negative, but the coefficient is only significant for the unaccompanied minors. The interpretation is straightforward: in municipalities where the average age was higher before the refugee wave, the immigration-related increase in the SD vote was dampened. This provides further support to the result established in Section 5.2, namely that in depopulation municipalities, immigration of young men was seen embraced as a way of revitalizing the local community.

[25]Since it takes time to relocate in response to a refugee wave, I allow for a 1-year lag with respect to the immigration variable.

Similarly, we may re-examine the results presented in Panel C of Table 4, namely the interaction between immigration and the pre-influx share of foreigners. Since naturalized immigrants can vote, it could affect voting outcomes if this group is more likely to vote against the SD. Replacing the share of foreign-born and first-generation immigrants with the share of naturalized immigrants, the results in Table A10 of Supporting Information: Appendix A are similar to those reported in Table 4, with SD support increasing following immigration of unaccompanied minors in municipalities with a large share of naturalized immigrants before 2015. Hence, any anti-SD voting from naturalized immigrants[26] is likely to be more than offset by the pro-SD native vote in these municipalities.

Finally, we may examine the role played by the pre-influx education level. Table A11 of Supporting Information: Appendix A interacts immigration rates with the 2014 share of residents with at least a 3-year college degree. The interaction terms are statistically insignificant, both for refugees and general and for unaccompanied minors. The baseline coefficients for the share of highly educated residents are negative, but significant only for the unaccompanied minors. This finding provides further support for the claim that immigration of minors is particularly polarizing.[27]

6.3 | Labor market outcomes

The political science literature identifies two primary mechanisms explaining the growth of right-wing populism: immigration and adverse native labor market outcomes.[28] Studies from Sweden using pre-2018 SD election outcomes show that adverse native labor market outcomes were important drivers of the rapid right-wing populist growth in Sweden, especially between 2006 and 2010 (Dal Bó et al., 2022; Dehdari, 2021).

Since the municipality-level change in unemployment is correlated with the immigration rate, I instead use the change in the *native* employment rate between 2014 and 2018, together with the same municipality controls utilized previously, to examine whether adverse labor market outcomes impacted SD growth. The OLS results of this regression are given in Table A12 of Supporting Information: Appendix A, suggesting that there is no significant relationship between the change in native employment and the increase in the SD margin.[29] Hence, adverse labor market outcomes of natives cannot explain the sharp growth of the right-wing populist vote between 2014 and 2018.

6.4 | Individual-level evidence

6.4.1 | Potential mechanisms

Another question concerns what channels could explain our results. The SOM survey described in Section 4.1 allows us to complement our previous findings with individual-level evidence. Respondents are asked to rank the top three societal problems facing Sweden, with which we can calculate the share of respondents (in percent) viewing immigration, crime or jobs as the main problem, or one of the three main problems facing the country. For each municipality, I use the difference in shares between the sample years 2011–2014 and 2015–2018 as the

[26]Twice a year, the Swedish Statistics Agency asks voters for their favorite party (which is not necessarily identical to the party they would vote for in an election). In May 2018, 11.3% of naturalized immigrants considered SD as their favorite party, compared to 15.2% of Sweden-born. The sample size was $N = 4632$ (Swedish Statistics Agency, 2018, p. 37).

[27]Note that the share of college graduates is correlated with two of the municipality-specific controls, namely the manufacturing share (−0.53) and the unemployment rate (−0.40). This inflates the standard errors, causing the coefficient estimate for the immigration rate in Table A11 of Supporting Information: Appendix A to become statistically insignificant.

[28]See Arzheimer (2009) for a summary.

[29]Using 2SLS with the unemployment rate as endogenous variable and the change in native employment as the instrument gives the same conclusion.

outcome variable, and regress this on the immigration rate and the controls utilized previously. For instance, if 5 and 8 percent of respondents viewed immigration as the main problem before and after the refugee wave, respectively, the increase is 3 percentage points.

Table A13 of Supporting Information: Appendix A presents the results of the second-stage 2SLS.[30] We see that the refugee wave led to an increase in the share of respondents viewing immigration as the top problem, but not as one of the three main problems. There are weaker evidence for concerns about crime rates or crowding-out effects on the labor market, suggesting that compositional amenities and cultural concerns are salient factors in explaining our findings. However, the question regarding concerns about immigration in the survey reflects the views of respondents on immigrants broadly defined, which may capture some statistical discrimination vis-à-vis immigrants not captured in the coefficients for concerns about crime rates and jobs, where "immigrants," broadly defined, are associated with an increased propensity to commit crimes.[31] This could explain our findings from Section 5.2, namely that the SD margin increased significantly in municipalities with high crime rates before the refugee wave.

6.4.2 | Individual-level relationship between immigration exposure and voting

This subsection uses individual-level evidence to examine the relationship between immigration exposure and voting, using the VU survey discussed in Section 4.1. The survey asks individuals what party they voted for in 2014 and 2018, allowing the identification of party switchers. Since the VU survey identifies respondents only on the county level, I match the individual responses with aggregated county-level data on immigration rates. I then create a binary variable taking the value unity if a person switched parties to the SD between 2014 and 2018, and zero else. This variable is the outcome variable of interest, whereas the main independent variable is the county-level immigration rate instrumented with the county-level allocated refugee inflow rate.

Supporting Information: Table A14 presents the results, estimated using an instrumental variables probit model. The results are broadly consistent with previous findings: there is a significant relationship between immigration exposure at the county level and the probability of switching to the SD, whereas there is no such relationship when considering immigration of unaccompanied minors. The latter finding is likely to be related to the polarizing regional-level effects of immigration of unaccompanied minors.

6.5 | Panel evidence

As further evidence on one of the main results of this paper, namely that immigration has a positive effect on SD support both for refugees in general and for unaccompanied minors specifically, it is possible to expand the data set to include more time periods. The earliest year for which there is municipality-level data on refugee allocation is 2005. Although municipalities were not obligated by law to accept allocated refugees until 2016, most municipalities did not contest their allocation before the law change. In 2014, immediately before the law change, 289 out of 290 municipalities accepted allocated refugees, and all municipalities accepted allocated unaccompanied minors.[32] Thus, we can include more time periods and estimate the fixed-effects IV model

$$SD_{it} = \alpha_i + \beta_1 IMR_{it} + \boldsymbol{\beta}' \boldsymbol{X}_{it} + \eta_t + \epsilon_{it} \tag{8}$$

[30]Note that the first-stage coefficients are the same as before, since the allocated refugee inflow rate is still used as an instrument for the total immigration rate.

[31]See Kaddoura (2019) for a discussion about the relationship between immigration rates and crime rates in the Swedish context.

[32]Allocation of unaccompanied minors had been made non-contestable in 2014. In 2013, 244 municipalities accepted allocated unaccompanied minors, and 283 municipalities accepted allocated refugees.

TABLE 6 Panel estimates

	All refugees		Unaccompanied minors	
	(1)	(2)	(1)	(2)
Total immigration rate	1.557*	1.371*	2.544***	2.794***
	(0.852)	(0.735)	(0.635)	(0.681)
Time FE included	Yes	Yes	Yes	Yes
Allocation parameters included	Yes	Yes	Yes	Yes
Municipal characteristic controls included	No	Yes	No	Yes
Municipalities	290	290	290	290
Observations	870	870	870	870
Mean dep. var.	14.20	14.20	14.20	14.20

Notes: Panel estimates. Dependent variable: SD vote shares in 2010, 2014, and 2018 elections. Column (1), Municipality fixed effects, time fixed effects, and allocation parameters. Column (2), All previous controls augmented with the time-variant controls used in previous regressions, namely the crime rate, municipal tax rate, share of residents with a foreign background, net domestic outflow rate, and share of the workforce employed in manufacturing. Standard errors clustered by municipality in brackets.

for municipalities $i = 1, ..., 290$ and election years $t = 2010, 2014, 2018$. In (8), SD_{it} denotes the SD vote share, α_i is a municipality-specific fixed effect, IMR_{it} is the immigration rate between the elections at time points t and $t-1$,[33] $\boldsymbol{X}_{it}$ are the remaining municipality-specific time-variant controls, η_t is an election fixed effect, and ϵ_{it} is an idiosyncratic error term. As before, the endogenous variable IMR_{it} is instrumented with ARI_{it}.[34]

Table 6 presents the results. The coefficient for all refugees is positive and significant at the 10% level. When utilizing all controls in Column (2), the coefficient estimate of around 1.4 percentage points SD increase for each percentage point increase in immigration is very close to the estimate of 1.2 presented in Table 3, which was for the 2014–18 period only. For the unaccompanied minors, the coefficient estimate is around double the magnitude compared to the estimate in Table 3. However, a caveat to note is that around 15% of municipalities were non-participants in the allocation program of unaccompanied minors before the law change, meaning that these estimates are likely to be less precise than the estimates for the later time periods.

6.6 | Additional robustness checks

In this section, I perform a number of additional robustness checks.

6.6.1 | Percentages instead of percentage points

Table A15 of Supporting Information: Appendix A shows the results when re-estimating Equation (5) with the 2014–2018 percentage change in the SD margin instead of percentage points. The overall results on the positive effect of immigration on right-wing populist voting are robust to these changes.

[33]Hence, for the 2010 election, we use cumulative immigration rates for 2007, 2008, 2009 and 2010 to form IMR. For the 2014 election, years 2011, 2012, 2013, and 2014 are used, and for the 2018 election, 2015, 2016, 2017, and 2018.

[34]Table 1 gives the summary statistics for these variables. The summary statistics for the municipality-specific controls for years other than 2014 are available upon request.

6.6.2 | Results in local elections

So far, the results are for parliamentary elections only. Did immigration affect SD performance in local elections as well? Table A16 of Supporting Information: Appendix A shows the estimates when re-estimating the 2SLS model given by (4) and (5) with the percentage point change in the SD margin in municipal instead of parliamentary elections. The resulting point estimates are higher both for refugees in general and for unaccompanied minors, although with slightly higher variation around the estimates. While Swedish municipalities have no influence over immigration policy, there is high correlation between votes in parliamentary and local elections, meaning that the conclusions reached previously do not change significantly when examining municipal election outcomes.

6.6.3 | Further tests of identifying assumptions

Section 5.1. of this paper establishes that municipal placement between 2015 and 2017 was not affected by the 2014 level of SD support. To address the concern that the vote shares of other parties had an impact on refugee placement, Panels B and C in the left-hand column of Table A17 of Supporting Information: Appendix A present the results of additional regressions with the number of allocated refugees as the dependent variable, and the electoral outcomes of the center-left Social Democrats and center-right Moderate Party as independent variables.[35] In addition, the results in Panels A–C of the right-hand column of Supporting Information: Table A16 show the estimates when the 2010–2014 change in the vote share of the SD, Social Democrats and Moderate Party are used as regressors in lieu of the 2014 levels. Again, the results suggest that placement of refugees was not significantly related to previous electoral outcomes.

6.6.4 | Allocation of asylum seekers

One final threat to identification is that, given that the allocation mechanism is a function of the number of asylum seekers currently living in the municipality, similar concerns as those raised in Section 3 may apply. That is, that share of asylum seekers in a municipality is related to local amenities and voting. Table A18 of Supporting Information: Appendix A presents OLS results from regressing the share of asylum seekers living in the municipality in 2014, immediately before the refugee wave, on the municipality-specific controls used in the paper. There is no significant relationship between the share of asylum seekers and the municipality-specific controls, suggesting that local amenities did not impact the share of asylum seekers. Importantly, akin to the findings on the share of allocated refugees with a residence permit, there is nothing to suggest that allocation of asylum seekers was related to pre-influx SD voting.[36]

7 | CONCLUDING REMARKS

By exploiting the exogenous variation in allocated refugees between municipalities induced by the Swedish refugee placement program during 2015–2017, this paper evaluates the immigration-related changes in voter preferences depending on differential regional characteristics. I provide estimates both for immigration of refugees as whole, as well as for unaccompanied minors, a group dominated by young men.

[35]Note that the coefficient estimate in Panel A is the same as in Table 2, since the SD vote share is used as the explanatory variable of interest.
[36]Additionally, it is possible to show that there is no relationship between the share of asylum seekers in 2014 and the 2010 SD vote share (coefficient estimate −0.074, p = 0.20).

The results suggest a positive but modest overall impact of immigration on the right-wing populist vote share growth, a result in line with recent findings by other authors (Dustmann et al., 2019; Halla et al., 2017). However, the main contribution of this paper is instead the results regarding the significant impact on voting behavior stemming from pre-influx regional characteristics. The results suggest a significant heterogeneity between geographical areas: in municipalities with right-wing populist party presence around the time of the 1990s refugee wave, the SD margin increased significantly more than in other areas, suggesting that latent anti-immigration sentiments are an important explanation for current voting trends. Moreover, in municipalities with high pre-influx crime rates, as well as in municipalities where relatively many residents were of non-Swedish descent before the refugee influx, immigration further exacerbated SD growth. The increase was considerably larger for immigration of young men, compared to immigration of refugees as a whole.

However, in regions adversely affected by urbanization, immigration had a dampening effect on the SD vote. I relate this finding to recent research on the positive impact of immigration on housing prices and economic activity. Again, the negative impact on the right-wing populist vote was more prominent for immigration of young men. Thus, in geographical areas where the negative effects of immigration are more prominent (for instance, in the form of higher crime rates) the SD margin increased further, whereas the opposite effect exists in areas where the positive effects of immigration are more likely to be appreciated by voters. This is particularly true for the unaccompanied minors: whereas young men are a potential asset to the labor force in depopulation areas, they are associated with higher crime rates and social problems. This finding on the regional polarization of an immigration shock further extends the current literature on the political geography of voting (Alabrese et al., 2019; Becker et al., 2017; Hersh & Nall, 2015).

Given the strong polarizing effects of immigration, native attitudes are likely to have a major impact on the future economic and social performance of immigrants in host countries. Thus, to what extent attitudes of natives affect the assimilation of refugees, and whether this is affected by regional characteristics, is an interesting question for further research.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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Online Appendix [Not for Publication]

A. Additional empirical results

Table A1
Overview of the eight parties represented in the Riksdag.

Abb.	Party name	Ideology	Vote share 2018	Vote share 2014	Seats 2018 (2014)	Gov./opposition 2014 – 2018
	Center-left coalition					
S	Social Democrats	Social democracy	28.26	31.01	100 (113)	Government
V	Left Party	Socialism	8.00	5.72	28 (21)	Supporting government
MP	Green Party	Green politics	4.41	6.89	16 (25)	Government
	Center-right coalition					
M	Moderate Party	Liberal conservatism	19.84	23.33	70 (84)	Opposition
C	Center Party	Centrism	8.61	6.11	31 (22)	Opposition
KD	Christian Democrats	Conservatism	6.32	4.57	22 (16)	Opposition
L	Liberals	Liberalism	5.49	5.42	20 (19)	Opposition
SD	Sweden Democrats	Social conservatism, Swedish nationalism	17.53	12.86	62 (49)	Opposition

Table A2
Immigrant characteristics

	Number	Largest origin countries
Total Immigration (*IM*)	188,709	Syria (53.4%) Eritrea (11.5%) Stateless (9.1%) Afghanistan (9.1%) *Other* (16.9%)
Allocated refugee inflow (*ARI*)	60,855	Syria (46.4%) Afghanistan (16.7%) Eritrea (14.2%) Stateless (6.2%) *Other* (16.5%)
Self-selected refugees	81,300	Syria (58.3%) Eritrea (11.0%) Stateless (9.9%) Afghanistan (3.6%) *Other* (17.2%)
Family migrants	46,554	Syria (54.1%) Stateless (11.7%) Somalia (10.7%) Eritrea (8.8%) *Other* (14.7%)

Notes. Characteristics of all immigrants arriving in a municipality 2015–17 (excluding labor immigrants, EU immigrants and students). Data source: Swedish Migration Agency.

Table A3
Additional tests of identifying assumptions

	Baseline	Last election change in SD vote share included	Additional controls included
Average unemployment rate 2014–16	−0.152***	−0.148***	−0.171***
	(0.013)	(0.014)	(0.017)
Capita share of allocated refugees 2012–14	0.310***	0.308***	0.307***
	(0.025)	(0.024)	(0.031)
Capita share of asylum seekers 2014–16	−0.035*	−0.033	−0.045*
	(0.020)	(0.020)	(0.027)
SD vote share 2014		−0.003	0.002
		(0.003)	(0.004)
Observations	290	290	290
Mean dep. var.	0.773	0.773	0.773
R^2	0.6050	0.6059	0.6225

Notes. Dependent variable: Share of allocated refugees, 2015–17. Allocation parameters and the SD vote share are standardized with mean zero and standard deviation equal to one. Standard errors clustered by municipality in brackets. * and *** denote significance at the 10% and 1% level, respectively.

Table A4
Results, main specification (OLS estimates)

	All refugees		Unaccompanied minors	
	(1)	(2)	(1)	(2)
Total Immigration rate	0.012	0.132*	0.023	0.962
	(0.074)	(0.080)	(0.665)	(0.696)
Allocation parameters included	Yes	Yes	Yes	Yes
Lagged SD vote share included	Yes	Yes	Yes	Yes
Municipal characteristic controls included	No	Yes	No	Yes
Observations	290	290	290	290
Mean dep. var.	5.62	5.62	5.62	5.62
R^2	0.219	0.269	0.219	0.268

Notes. Dependent variable: Percentage point change in the SD vote share, 2014–18. Column (1), Controls included for 2014 values of population size, unemployment rate, number of asylum seekers, 2012–14 number of allocated refugees, and the 2014 SD vote share. Column (2), All previous controls augmented with the 2014 crime rate, municipal tax rate, share of residents with a foreign background, net domestic outflow rate, share of the workforce employed in manufacturing, lands dummies, and an urban area dummy. Standard errors clustered by municipality in brackets. * denotes significance at the 10% level.

Table A5
Effects on other parties

	All refugees		Unaccompanied minors	
	Center-left	Center-right	Center-left	Center-right
Total Immigration rate	−1.690**	−0.054	−1.739**	0.811
	(0.697)	(0.335)	(0.844)	(0.673)
Allocation parameters included	Yes	Yes	Yes	Yes
Lagged SD vote share included	Yes	Yes	Yes	Yes
Municipal characteristic controls included	Yes	Yes	Yes	Yes
Observations	290	290	290	290
Mean dep. var.	−4.77	−2.84	−4.77	−2.84

Notes. Second stage 2SLS results. Dependent variable: Change in Social Democratic (center-left), and Moderate Party (center-right) vote shares, 2014–18. All columns use the full set of controls. Standard errors clustered by municipality in brackets. ** denotes significance at the 5% level.

Table A6
Additional municipal characteristics interactions

	All refugees	Unaccompanied minors
Panel D: SD vote share, 2014		
Immigration rate	−0.529	1.324*
	(3.412)	(0.743)
2014 SD vote share	7.221	0.488**
	(14.797)	(0.201)
Immigration rate ×	−2.767	0.758
2014 SD vote share	(6.193)	(0.550)
Panel E: Tax rate, 2014		
Immigration rate	2.3354	1.293*
	(8.144)	(0.770)
Tax rate	−9.264	−0.097
	(73.211)	(0.192)
Immigration rate ×	3.880	0.365
Tax rate	(30.886)	(0.555)
Panel F: Share of workforce in manufacturing, 2014		
Immigration rate	0.125	1.270
	(2.400)	(0.787)
Share of workforce in manufacturing	7.901	−0.100
	(22.248)	(0.227)
Immigration rate ×	−2.823	0.193
Share of workforce in manufacturing	(7.764)	(0.696)
Panel G: Urban area dummy		
Immigration rate	1.217*	1.254
	(0.585)	(0.791)
Urban area	3.800	0.185
	(4.593)	(0.457)
Immigration rate ×	−2.091	0.632
Urban area	(2.282)	(2.060)
Allocation parameters included	Yes	Yes
Lagged SD vote share included	Yes	Yes
Municipal characteristic controls included	Yes	Yes
Observations	290	290
Mean dep. var.	5.62	5.62

Notes. Dependent variable: Percentage point change in SD vote share, 2014–18. Each specification uses the full set of controls. Standard errors clustered by municipality in brackets. * and ** denote significance at the 10% and 5% level, respectively.

Table A7
Placebo tests

	Dependent variable: Net native outflow rate$^{2016-2018}$		Dependent variable: ΔTurnout rate$^{2014-2018}$	
	(1)	(2)	(1)	(2)
Total Immigration rate	0.065	−0.120	−0.131	−0.090
	(0.246)	(0.197)	(0.195)	(0.202)
Allocation parameters included	Yes	Yes	Yes	Yes
Lagged SD vote share included	Yes	Yes	Yes	Yes
Municipal characteristic controls included	No	Yes	No	Yes
Observations	290	290	290	290
Mean dep. var.	−0.063	−0.063	1.32	1.32

Notes. Second stage 2SLS results. Dependent variable: Percentage point change in the net native outflow rate and turnout rate in response to immigration. Column (1), Controls included for 2014 values of population size, unemployment rate, number of asylum seekers, 2012–14 number of allocated refugees, and the 2014 SD vote share. Column (2), All previous controls augmented with the 2014 crime rate, municipal tax rate, share of residents with a foreign background, net domestic outflow rate (when turnout is the dependent variable), share of the workforce employed in manufacturing, lands dummies, and an urban area dummy. The municipal control variable Net domestic outflow rate is dropped for the left-hand regression to avoid colinearity. Standard errors clustered by municipality in brackets.

Table A8
Interaction with outflow rate of Swedish-born individuals

	All refugees	Unaccompanied minors
Immigration rate	1.215**	1.627**
	(0.531)	(0.764)
Net native outflow rate	0.540	0.252
	(0.650)	(0.171)
Immigration rate × Net native outflow rate	−0.391	−1.075**
	(0.270)	(0.439)
Allocation parameters included	Yes	Yes
Lagged SD vote share included	Yes	Yes
Municipal characteristic controls included	Yes	Yes
Observations	290	290
Mean dep. var.	5.62	5.62

Notes. Second stage 2SLS results when interacting immigration with the net outflow rate of Swedish-born individuals only. Dependent variable: Percentage point change in SD vote share, 2014–18. Column (1), Controls included for 2014 values of population size, unemployment rate, number of asylum seekers, 2012–14 number of allocated refugees, and the 2014 SD vote share. Column (2), All previous controls augmented with the 2014 crime rate, municipal tax rate, net domestic outflow rate, share of the workforce employed in manufacturing, lands dummies, and an urban area dummy. Standard errors clustered by municipality in brackets. * and ** denote significance at the 10% and 5% level, respectively.

Table A9
Interaction with the average age of the population

	All refugees	Unaccompanied minors
Immigration rate	1.569**	1.195
	(0.802)	(0.933)
Average age	1.464	0.709***
	(2.100)	(0.221)
Immigration rate × Average age	−0.786	−1.330**
	(0.962)	(0.624)
Allocation parameters included	Yes	Yes
Lagged SD vote share included	Yes	Yes
Municipal characteristic controls included	Yes	Yes
Observations	290	290
Mean dep. var.	5.62	5.62

Notes. Second stage 2SLS results when interacting immigration with the 2014 average age of residents in municipality. Dependent variable: Percentage point change in SD vote share, 2014–18. Column (1), Controls included for 2014 values of population size, unemployment rate, number of asylum seekers, 2012–14 number of allocated refugees, and the 2014 SD vote share. Column (2), All previous controls augmented with the 2014 crime rate, municipal tax rate, share of the workforce employed in manufacturing, lands dummies, and an urban area dummy. Standard errors clustered by municipality in brackets. * and ** denote significance at the 10% and 5% level, respectively.

Table A10
Interaction with the share of naturalized immigrants

	All refugees	Unaccompanied minors
Immigration rate	2.614	1.906***
	(1.758)	(0.730)
Share of naturalized immigrants	−4.031	−0.473**
	(4.867)	(0.210)
Immigration rate × Share of naturalized immigrants	1.999	1.965**
	(2.228)	(0.862)
Allocation parameters included	Yes	Yes
Lagged SD vote share included	Yes	Yes
Municipal characteristic controls included	Yes	Yes
Observations	290	290
Mean dep. var.	5.62	5.62

Notes. Second stage 2SLS results when interacting immigration with the 2014 share of naturalized immigrants. Dependent variable: Percentage point change in SD vote share, 2014–18. Column (1), Controls included for 2014 values of population size, unemployment rate, number of asylum seekers, 2012–14 number of allocated refugees, and the 2014 SD vote share. Column (2), All previous controls augmented with the 2014 crime rate, municipal tax rate, share of residents with a foreign background, net domestic outflow rate, share of the workforce employed in manufacturing, lands dummies, and an urban area dummy. Standard errors clustered by municipality in brackets. * and ** denote significance at the 10% and 5% level, respectively.

Table A11
Interaction with the share of highly-educated residents

	All refugees	Unaccompanied minors
Immigration rate	1.454	0.344
	(1.719)	(0.967)
Share of highly educated residents	−1.037	−0.455**
	(2.340)	(0.182)
Immigration rate × highly educated residents	0.489	−0.792
	(2.008)	(0.947)
Allocation parameters included	Yes	Yes
Lagged SD vote share included	Yes	Yes
Municipal characteristic controls included	Yes	Yes
Observations	290	290
Mean dep. var.	5.62	5.62

Notes. Second stage 2SLS results when interacting immigration with the 2014 share of highly educated residents, defined as the share of the population with at least a three-year college degree. Dependent variable: Percentage point change in SD vote share, 2014–18. Controls included for 2014 values of population size, unemployment rate, number of asylum seekers, 2012–14 number of allocated refugees, 2014 SD vote share, crime rate, municipal tax rate, share of residents with a foreign background, net domestic outflow rate, share of the workforce employed in manufacturing, lands dummies, and an urban area dummy. Standard errors clustered by municipality in brackets. ** denotes significance at the 5% level.

Table A12
Native employment rate as the main independent variable

	(1)	(2)
Δ Native employment rate	0.146	0.190*
	(0.099)	(0.107)
Allocation parameters included	Yes	Yes
Lagged SD vote share included	Yes	Yes
Municipal characteristic controls included	No	Yes
Observations	290	290
Mean dep. var.	5.62	5.62
R^2	0.2237	0.2694

Notes. OLS results with the difference in the native employment rate as the main independent variable of interest. Dependent variable: Percentage point change in SD vote share, 2014–18. Column (1), Controls included for 2014 values of population size, unemployment rate, number of asylum seekers, 2012–14 number of allocated refugees, and the 2014 SD vote share. Column (2), All previous controls augmented with the 2014 crime rate, municipal tax rate, share of residents with a foreign background, net domestic outflow rate, share of the workforce employed in manufacturing, lands dummies, and an urban area dummy. Standard errors clustered by municipality in brackets. * denotes significance at the 10% level.

Table A13
Potential channels

	Immigration:		Crime:		Jobs:	
	Largest problem	Top three problem	Largest problem	Top three problem	Largest problem	Top three problem
Immigration rate	2.675*	−0.318	0.208	−0.146	0.241	1.369
	(1.503)	(1.697)	(0.200)	(0.379)	(1.260)	(1.749)
Allocation parameters included	Yes	Yes	Yes	Yes	Yes	Yes
Lagged SD vote share included	Yes	Yes	Yes	Yes	Yes	Yes
Municipal characteristic controls included	Yes	Yes	Yes	Yes	Yes	Yes
Observations	290	290	290	290	290	290
Mean dep. var.	2.68	2.60	0.130	0.291	−7.70	−11.60

Notes: Second stage 2SLS results. The dependent variable is the difference in the share of respondents viewing each societal problem as one of the top three problems facing Sweden, where the difference is between 2011–2014 and 2015–2018, expressed in percentage points. A constant is included in all regressions. Standard errors clustered at the municipality level in brackets. * and *** denote significance at the 10% and 1% level, respectively.

Table A14
Party switchers and immigration rates

	All refugees	Unaccompanied minors
Immigration rate	0.567**	–0.118
	(0.230)	(0.595)
Controls	No	No
Observations	7,999	7,999
Mean dep. var.	0.006	0.006

Notes. Instrumental-variable probit estimates. First stage dependent variable: Total immigration rate (aggregated at the county level), 2015–17. Second stage dependent variable: A binary variable taking the value one if the individual switched to the SD between 2014 and 2018, and zero else. White heteroscedasticity robust standard errors are in brackets. ** denotes significance at the 5% level.

Table A15
Results with percentages instead of percentage points

	All refugees	Unaccompanied minors
First stage:		
Allocated refugee rate	0.731***	1.067***
	(0.234)	(0.027)
Second stage:		
Total immigration rate	6.957*	8.593
	(3.953)	(6.380)
Allocation parameters included	Yes	Yes
Lagged SD vote share included	Yes	Yes
Municipal characteristic controls included	Yes	Yes
Observations	290	290
Mean dep. var.	38.80	38.80
F statistic of excl. instruments	12.64	1,836.48

Notes. 2SLS results. First stage dependent variable: Total immigration rate, 2015–17. Second stage dependent variable: Percentage point change in the SD vote share, 2014–18. Each specification uses the full set of controls. The mean of the dependent variable refers to the dependent variable in the second stage, namely the percentage point difference in the SD vote share between 2014 and 2018. Standard errors clustered by municipality in brackets. * and *** denote significance at the 10% and 1% level, respectively.

Table A16
Results in local elections

	All refugees	Unaccompanied minors
First stage:		
Allocated refugee rate	0.725***	1.070***
	(0.249)	(0.027)
Second stage:		
Total immigration rate	2.183**	2.028*
	(0.966)	(1.080)
Allocation parameters included	Yes	Yes
Lagged SD vote share included	Yes	Yes
Municipal characteristic controls included	Yes	Yes
Observations	290	290
Mean dep. var.	5.62	5.62
F statistic of excl. instruments	12.08	1857.50

Notes. 2SLS results. First stage dependent variable: Total immigration rate, 2015–17. Second stage dependent variable: Percentage point change in the SD vote share in municipal elections, 2014–18. Each specification uses the full set of controls. The mean of the dependent variable refers to the dependent variable in the second stage, namely the percentage point difference in the SD vote share between 2014 and 2018. Standard errors clustered by municipality in brackets. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table A17
Additional tests of identifying assumptions

	2014 level	2010–14 difference
Panel A: Sweden Democrats		
Sweden Democrats vote share	0.230	1.115
	(1.916)	(3.902)
R^2	0.9535	0.9535
Panel B: Social Democrats		
Social Democrats vote share	0.777	4.013
	(1.020)	(3.390)
R^2	0.9536	0.9538
Panel C: Moderate Party		
Moderate Party vote share	−0.650	−3.035
	(1.250)	(2.560)
R^2	0.9535	0.9536
Allocation parameters included	Yes	Yes
Municipal characteristic controls included	Yes	Yes
Observations	290	290
Mean dep. var.	209.84	209.84

Notes. OLS results with the cumulative number of allocated refugees 2015–17 as the dependent variable. Each specification uses the full set of controls. Standard errors clustered by municipality in brackets.

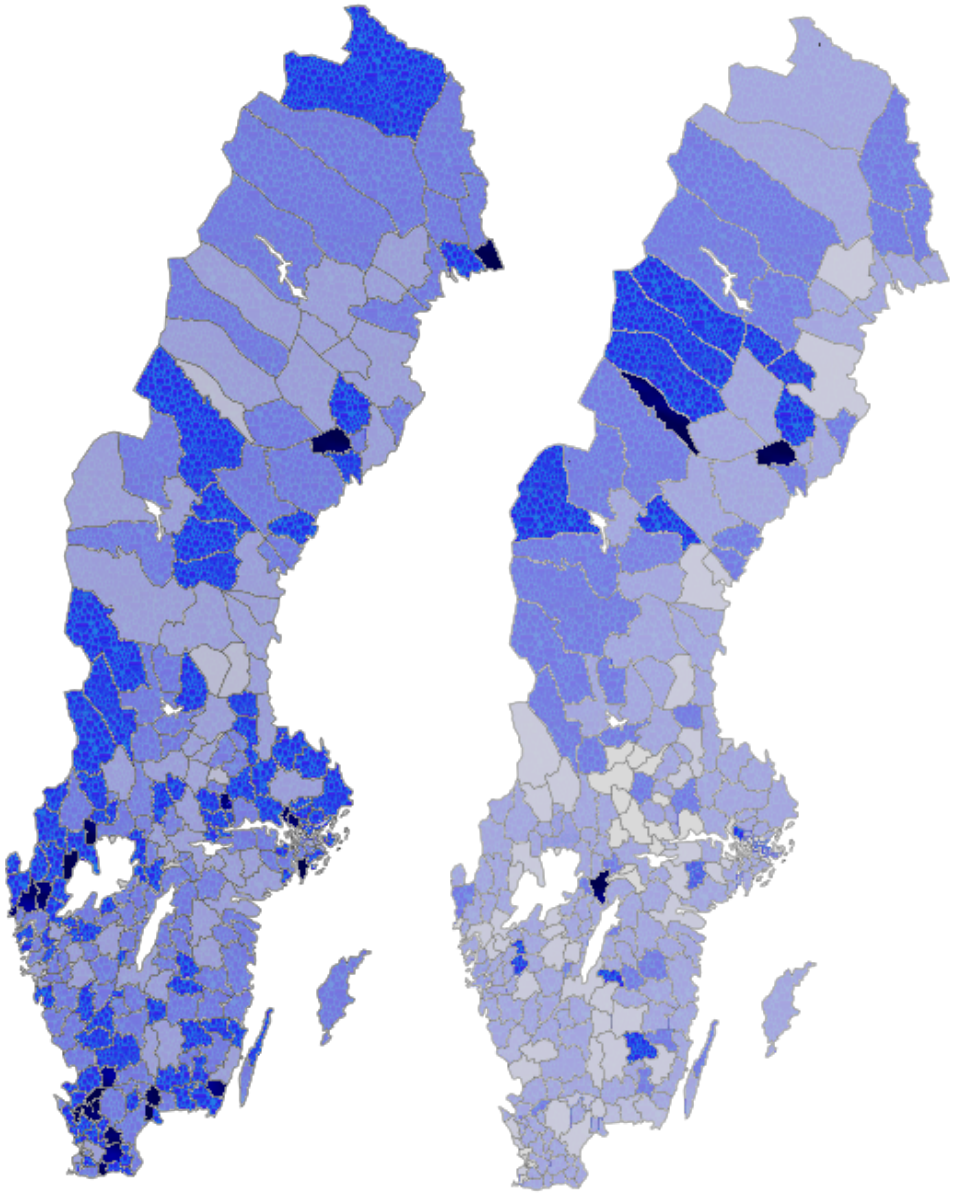
Table A18
Share of asylum seekers and local amenities

	(1)	(2)
SD vote share, 2014	−0.058	−0.085
	(0.059)	(0.064)
Unemployment rate, 2014		0.109
		(0.071)
Allocated refugee inflow, 2012–14		−0.145
		(0.091)
Reported crime rate, 2014		0.040
		(0.040)
Local income tax rate, 2014		0.050
		(0.063)
Share of residents with a foreign background, 2014		−0.081
		(0.049)
Net domestic outflow rate, 2014		−0.034
		(0.031)
Share of the workforce employed in manufacturing, 2014		0.000
		(0.050)
Northern Sweden dummy		0.410
		(0.298)
Southern Sweden dummy		0.078
		(0.068)
Observations	290	290
Mean dep. var.	0.000	0.000
R^2	0.003	0.044

Notes. OLS results with the share of asylum seekers, expressed as percentage of the total population, living in the municipality in 2014 (prior to the refugee wave) as the dependent variable, and the allocation parameters and the remaining municipality-specific controls as independent variables. All continuous variables are standardized with mean zero, and standard deviation equal to unity. For the lands dummies, Central Sweden is the baseline category. Column (1), Only the 2014 SD vote share as independent variable. Column (2), All municipality-specific controls as independent variables.

Table A19
Summary statistics for auxiliary variables

	Mean	Std.dev.	Min	Max
Variables used for testing identifying assumptions:				
Average population, 2014–16	34,015.55	70,204.36	2,453	923,708
Average unemployment rate, 2014–16 (%)	7.73	2.78	2.37	15.14
Average number of allocated refugees, 2012–14	27.91	24.52	0	180.67
Average number of asylum seekers in the municip., 2014–16	309.62	436.34	12	4,731.68
Variables used when calculating immigration rates:				
Average population, 2015–17	34,444.15	71,237.36	2,453	936,299
Variables used in Tables A7–A16:				
Net native outflow rate, 2016–18 (%)	−0.063	1.34	−6.14	3.20
Δ Turnout (p.p.)	1.316	0.661	−1.20	3.70
Net native outflow rate, 2014 (%)	0.111	0.505	−2.05	1.95
Average age, 2014 (years)	43.34	2.63	36.70	49.40
Share of naturalized immigrants, 2014 (%)	5.97	3.39	1.42	22.46
Δ Native employment rate, 2014–2018 (p.p.)	2.21	0.95	−1.20	5.80
Immigration: Largest problem (difference between periods, p.p.)	2.68	4.66	−20.00	16.67
Immigration: Top three problem (difference between periods, p.p.)	2.60	6.22	−20.00	40.00
Crime: Largest problem (difference between periods, p.p.)	0.130	0.698	−3.85	4.76
Crime: Top three problem (difference between periods, p.p.)	0.291	1.39	−7.14	7.69
Jobs: Largest problem (difference between periods, p.p.)	−7.70	5.16	−25.00	8.33
Jobs: Top three problem (difference between periods, p.p.)	−11.60	6.58	−42.86	10.16
Δ Party switcher to the SD (binary, 1 if yes)	0.006	0.08	0	1
Δ $SD^{2014-2018}$ (%)	38.80	14.35	5.33	94.91
Δ $SD^{2014-2018}$ (p.p., municipal council elections)	3.80	2.97	−6.25	20.50
Variables used in Table A17:				
Social Democratic party vote share, 2014 (%)	35.03	8.73	6.80	58.10
Moderate party vote share, 2014 (%)	20.30	7.18	6.90	50.00
Δ $SD^{2010-2014}$ (p.p.)	9.04	2.46	3.35	15.41
Δ $S^{2010-2014}$ (p.p.)	0.028	2.17	−10.10	3.90
Δ $M^{2010-2014}$ (p.p.)	−6.76	2.09	−13.80	0



(a) Percentage point (p.p) growth of the SD vote share, 2014–18.

(b) Allocated refugee inflow rate, 2015–17.

Figure A1: The relationship between the percentage point growth of the SD vote share and the allocated refugee inflow rate at the municipality level. The percentages for the allocated refugee inflow rates refer to shares of the pre-influx population.

B. Data sources and variables construction

This section describes the construction of the variables used in the empirical analysis in additional detail.

B.A. Immigration data

I use annual municipality-level immigration data provided by the Swedish Migration Agency. This dataset excludes asylum seekers, labor market immigrants and EU citizens. Instead, the data provides information on the yearly number of refugees (IM_i) with a residence permit divided into four groups: quota refugees, allocated refugees, self-selected refugees, and family migrants.[37] Quota refugees are otherwise referred to as *resettlement refugees*, and are a part of the predetermined yearly quota of refugees that Sweden is accepting through the UN Refugee Agency (UNHCR). Individuals belonging to this group are allocated into municipalities already before arriving in Sweden. Together with the allocated refugees, they form the allocated refugee inflow (ARI_i).

B.B. Data on allocation parameters

In the specifications used in Sections 5.2 and 5.3, four of the control variables are used as allocation parameters, namely the population, unemployment rate, number of previously allocated refugees, and current asylum seekers residing in the municipality. In order to avoid any endogeneity, I use the 2014 values of these variables when estimating equations (4), (5), (6), and (7). A brief description of the sources for these variables follows below.

Population. The 2014 population of the municipality, using data from the Swedish Statistics Agency.

Unemployment rate. This is the unemployment rate for individuals between 16 and 64 years of age. The data source is the Swedish Public Employment Service.

Allocated refugee inflow, 2012–14. This data is due to the Swedish Migration Agency, and I use the cumulative number of allocated refugees to each municipality for the years 2012, 2013 and 2014, which is the three-year period preceding the period of interest (2015–17) in this paper. This is one of the parameters deciding allocation of refugees to municipalities.

Number of asylum seekers. This is the number of asylum seekers residing in the municipality in 2014. It includes both those living in asylum accommodations provided by the Swedish Migration Agency, as well as individuals in other types of accommodations (for instance, those staying with relatives).

[37] Note that family migrants refer only to relatives of refugees. Relatives of other categories of immigrants, for instance of visa workers, are excluded.

B.C. Election and municipal characteristic data

All election data are due to the Swedish Statistics Agency. A brief discussion about data sources and definitions for the other municipal controls follows below.

Reported crime rate. This data is from Swedish National Council for Crime Prevention and is the total number of reported crimes in 2014, divided by the population in that year. This variable is included to control for the possibility that residents in high-crime areas are more likely to support the SD.

Local tax rate. In Sweden, the bulk of income taxes is collected by the municipalities and counties. This produces variation in income tax rates between municipalities. It is reasonable to assume some level of discontent in high-tax municipalities, which could benefit populist parties such as the SD. As shown in Table 1, however, the variation around the mean of 32.63% is relatively modest. The data source is the Swedish Statistics Agency.

Share of residents with a foreign background. This is the sum of foreign-born individuals and Swedish-born individuals with both parents born abroad, divided by the total population in 2014. Foreign-born individuals are less likely to support the SD, however, if the share of immigrants in a municipality is perceived as too large, right-wing populist support could increase among natives. The data source is the Swedish Statistics Agency.

Net domestic outflow rate. The net domestic outflow is the number of individuals moving from municipality i to another municipality in Sweden, divided by the 2014 population. This variable is included in order to identify depopulation municipalities. The SD could benefit by reaping citizen discontent in these areas. Alternatively, residents in such municipalities could view immigration as a possibility to provide at least temporary boost to community life by increasing population. The data source is the Swedish Statistics Agency.

Share of the workforce employed in manufacturing. The share of the workforce employed in manufacturing is the number of individuals employed in manufacturing in 2014 divided by the total number of residents aged 15–64. This variable is included to control for the possibility that competition from low-skilled immigrants is a greater threat to blue-collar workers, which would increase the potential of the SD. The data source is the Swedish Statistics Agency.

Lands dummies. I use the three traditional "lands" of Sweden as geographic controls. Northern Sweden (*Norrland*) consists of five counties, namely Gävleborg, Jämtland, Norrbotten, Västerbotten, and Västernorrland. Central Sweden (*Svealand*) consists of the counties of Dalarna, Örebro, Stockholm, Södermanland, Uppsala, Värmland, and Västmanland. Finally, southern Sweden (*Götaland*) consists of Blekinge, Gotland, Halland, Jönköping, Kalmar, Kronoberg, Östergötland, Scania and Västra Götaland counties. Northern Sweden is a stronghold for the center-left Social Democratic party, which also has a relatively low voter mobility, whereas southern Sweden has a lager share of center-right and right-wing voters (Wesslén 2015).

Notably, all of the 30 municipalities in which the SD was the largest party in the 2018 elections are located in Götaland. In the empirical analysis, I use Svealand as the baseline category. This land has the most diverse political views, comprising both left- and right-wing dominated municipalities.

Urban area dummy. In order to capture the relatively weak support of the SD in urban areas, I let this binary variable take the value one if the municipality is either in Stockholm or Södermanland counties, or in the area of Västra Götaland county closest to Gothenburg. Since the latter county was formed by merging three counties in 1998 (Älvsborg, Göteborg and Bohus, and Skaraborg counties), I assign unity value to municipalities previously in Göteborg and Bohus County, which consisted of the city of Gothenburg and its outskirts.

Paper II

The Electoral Consequences of Environmental Accidents: Evidence from Chernobyl

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Abstract

This paper examines the relationship between environmental accidents and voting. Following the 1986 Chernobyl disaster, environmentalist parties entered parliaments in several nations. This paper uses Chernobyl as a natural experiment creating variation in radioactive fallout exposure over Sweden. I match municipality-level data on cesium ground contamination with election results for the environmentalist Green Party, which was elected to parliament in 1988. After adjusting for pre-Chernobyl views on nuclear power, the results show that voters in high-fallout areas were more likely to vote for the Greens. However, there is no evidence of a persistent effect on green voting. Instead, detailed individual-level survey data suggests that that resistance to nuclear energy in fallout-effected areas decreased sharply after a relatively short time period.

JEL classification codes: D72, Q48, Q53, Q58
Keywords: Chernobyl; pollution; voting

1. Introduction

One of the major trends in Western politics in recent years is the growth of green parties. At the same time, amid climate change, environmental accidents and natural disasters have become more common. What is the relationship between environmental accidents and political outcomes? Previous findings suggest that wildfire exposure increases voter support for pro-environmental measures, and unexpected temperature fluctuations increase voter concerns about climate change (Egan and Mullin 2012; Herrnstadt and Muehlegger 2014; Hazlett and Mildenberger 2020). Still, there is relatively little previous research on this subject.

In this article, I use the natural experiment generated by the most disastrous environmental disaster in modern history, the 1986 Chernobyl nuclear disaster, to evaluate the impacts on voting outcomes. More specifically, I use between-municipality variation in radioactive fallout over Sweden caused by Chernobyl in order to examine the causal effect of exposure to fallout on voting, focusing on votes for the Green Party (*Miljöpartiet*, MP), which was elected into parliament in 1988, two years after the incident. The rise of the MP mirrors a similar development in other Western nations in the years following Chernobyl. For the identification strategy, I use an important property of radiation deposition, namely that virtually all of the Chernobyl releases were spread though rainfall (Clark and Smith 1988). Hence, due to differences in precipitation levels, there were large variations between municipalities in terms of concentrations of fallout. Immediately after the reactor fire at Chernobyl was extinguished, authorities conducted large-scale aerial measurements of radioactive fallout in each municipality.

Controlling for other factors, the results suggest that the increase in the Green Party vote share was higher in municipalities with higher fallout levels. The positive impact on the green vote was particularly noticeable in municipalities with very high levels of fallout exposure. Additionally, the results show that the premium on the green vote resulting from the accident disappeared after three elections. Hence, these results suggest that there was no long-term effect on green voting from Chernobyl. Generalizing, any positive effects on environmentalist voting following a natural disaster is likely to hold only for a relatively short period of time.

To evaluate the mechanisms behind the changes in the green vote, I use the results from an annual, nationwide survey, where the respondents are selected randomly, matched with data on fallout levels in respondents' home municipalities. Comparing with pre-1986 attitudes to nuclear power, I find that there was significantly lower support for nuclear power in fallout-affected areas immediately after the accident. However, beginning already in the late 1980s, this fallout-driven heterogeneity in attitudes towards nuclear power began gradually eroding. This finding is consistent with the voting results.

In addition, the survey results show that the negative attitudes toward nuclear power in contaminated municipalities in the years immediately following the accident were primarily a consequence of fears of higher cancer incidence and deteriorated local environments, rather than concerns about the risk of nuclear accidents. Additionally, pro-environmentalist sentiments were considerably more pronounced among individuals who frequently read local newspapers. This finding is consistent with previous research showing that media exposure, particularly local as opposed to national media, can significantly alter voter preferences (Gentzkow 2006;

Enikolopov et al. 2011; Kendall et al. 2015; Repetto 2018).

It is common that the growth of new political movements leads to significant policy changes (Tabellini 2019). The election of the Green Party into parliament was no exception. For instance, it contributed to a 1990 decision of the Social Democratic government to implement a carbon tax, one of the first of its kind in the world (Hildingsson and Knaggård 2022). After implementation of the tax, carbon dioxide emissions from transport declined by around 11% (Andersson 2019). However, the success of environmentalist parties in the late 1980s was not limited to Sweden. In several European nations, green parties were elected into parliament following the accident.[1] These electoral successes also translated to considerable political power in many European countries; already in the mid-1990s, green parties were represented in coalition governments in both France and Germany.

The paper makes a number of contributions. First, it adds to the wider social science literature on how exposure to environmental changes affects short-term policy preferences. Most of the previous studies focus on the electoral consequences for the incumbent party. A 2002 flood in Germany increased support for the incumbent party, as did a 2010 wildfire in Russia (Bechtel and Hainmueller 2011; Lazarev et al. 2014). A plausible reason for the success of incumbents following natural disasters is that such events provides opportunities for governing parties to signal their quality to voters, causing voters to update their views about incumbents in a favorable direction (Ashworth et al. 2018). Contrary to this view, other papers studying the electoral consequences of natural disasters have found no effects on support for the incumbent (Bodet et al. 2016), or that the political effects depend on how vigorously the governing party responds to the event (Cole et al. 2012).

Second, it contributes to an extensive literature on other aspects of pollution. Exposure to air pollution contributes to lower birthweight and shorter stature among infants (Currie and Walker 2011; Rosales-Rueda and Triyana 2019), whereas early-life exposure to pollution has negative outcomes on future education outcomes and labor force participation (Currie et al. 2009; Isen et al. 2017). In areas with close proximity to a toxic industrial plant, housing prices and productivity levels are lower, and mortality rates are higher (Currie et al. 2015; Ebenstein et al. 2015; He et al. 2019). While numerous studies focus on the adverse effects of pollution, another strain of the literature points toward the economic benefits of pollution-generating activities. Oil and gas investments generate significant economic effects, including increased real wages, lower unemployment rates, and higher fertility rates (Feyrer et al. 2017; Allcott and Keniston 2018; Kearney and Wilson 2018). Understanding the political consequences of environmental accidents and natural disasters is important, considering that the frequency of such events is likely to increase as a result of global warming.

Finally, the natural experiment induced by regional variation in Chernobyl fallout levels has been used in several other studies, albeit answering different questions than the one posed in this paper. For instance, a study using the same setting and treatment, shows that children born in Swedish regions with high fallout exposure perform worse in secondary school, in particular

[1] Following the disaster, green parties were elected into national parliaments in Austria (1986), Sweden (1988), Netherlands (1989) and East Germany (March 1990). In the December 1990 elections in reunified Germany, green parties received 8 out of 662 seats.

in mathematics (Almond et al. 2009), even though the fallout level in affected municipalities were considered harmless by authorities. A further insight from the same paper is that babies born in high-fallout municipalities were less likely to graduate from high school, but that there were no effects on birth weight or neonatal mortality, suggesting that Chernobyl fallout chiefly impacted cognitive ability. Other studies have shown that more exposed individuals exhibit higher depression rates, lower subjective well-being, and lower labor market participation rates (Lehmann and Wadsworth 2011; Danzer and Danzer 2016). This article is the first to evaluate political outcomes of the disaster.

The reminder of the paper is structured as follows. Section 2 summarizes the Chernobyl disaster, and provides a brief background to the green movement in Sweden and elsewhere. Section 3 describes the data. Section 4 presents the empirical strategy, and presents the main results. Section 5 provides individual-level evidence about mechanisms, and Section 6 concludes.

2. Background

2.1. The Chernobyl disaster

On April 26, 1986, an explosion in reactor 4 of the Chernobyl nuclear power plant caused the release of large amounts of radioactive particles. The blaze burned for ten days, sending a plume of radiation across Europe. While the first radioactive cloud reached Sweden on April 27, the most significant rainfall was on the night between April 28 and 29. A nuclear accident leads to the release of many different radioactive particles, however, cesium-137, with a half-life of 30 years, is the most stable radioactive particle released during the disaster. Sweden received approximately 5% of total cesium fallout released during the disaster (Moberg 1991).

To mitigate the risks to public health, restrictions limiting food consumption were enacted immediately after the meltdown in areas heavily affected by fallout. Restrictions were mostly in the form of threshold limits for consumption of meat, berries, fish and mushrooms, severely impacting day to day life in rural areas. The restrictions caused significant damage to animal life, as thousands of reindeer and other wild animals had to be destroyed due to contamination. As an additional side effect and regardless of formal restrictions, many people were too afraid to even go out in the open. There are numerous anecdotal accounts of this phenomenon. As one farmer from Delsbo in Gävleborg County, one of the most hard-hit areas in Sweden, describes it (Mörtberg 2016):

> "Before Chernobyl, my wife used to pick blueberries, lingonberries and raspberries. But we quit that immediately. It probably took us ten years before we dared to do that again. And we did not start mushroom hunting until five years ago [in 2011]"

Notwithstanding the restrictions on food consumption, the impact on public health was significant. While the adverse health effects were considerably more pronounced in the Soviet Union, several studies suggest a positive relationship between fallout exposure in Sweden and cancer incidence (Tondel et al. 2006; Alinaghizadeh et al. 2016). The Swedish Radiation Safety Authority estimates that in the 50-year period following 1986, approximately 300 excess cancer deaths will occur in Sweden due to exposure to Chernobyl fallout (Hult 2011). However, there was considerable geographical variation in exposure to fallout. In the most affected areas,

ground deposition was close to that outside the Chernobyl exclusion zone, whereas other parts of Sweden were essentially spared (Almond et al. 2009). Importantly, regional variation in fallout exposure provides a natural experiment enabling us to assess the political consequences of the disaster.

2.2. Environmental issues and politics in Sweden

In the late 1970s, Sweden had six reactors in operation. Although peaceful nuclear power was fairly uncontroversial during the 1960s and early 1970s, the 1979 Three Mile Island accident in Harrisburg, Pennsylvania, led to a surge in anti-nuclear activism in the Western world. At this time, Sweden was governed by a center-right government consisting of three parties with highly divergent views on nuclear power: the pro-nuclear Moderate Party, the Center Party, which was against, and the People's Party somewhere in between.[2] Amid the public debate following Harrisburg, the nuclear power issue caused internal government disagreement. To mitigate this, as well to accommodate public pressure to abandon nuclear power altogether, a nonbinding referendum on the future of nuclear power in Sweden was held in 1980.

Nonstandard for a referendum, there were three options available to voters: Options 1 and 2 favored the use of nuclear power until other energy sources could replace it, whereas Option 3 favored abolishment within ten years. The chief difference between Options 1 and 2 was that the latter specified that nuclear plants be owned by the government, whereas the former did not. Hence, Option 1 was supported by the center-right Moderate Party, and Option 2 was supported by the center-left Social Democrats, as well as the People's Party. Option 3 was supported by the Center Party, and a sizable faction within the Social Democrats. Option 2 won a plurality (39.1%), followed by Option 3 (38.7%) and Option 1 (18.9%), with 3.3% of the votes cast blank. Hence, there was no majority for an abolishment, and six reactors that were already under construction were commissioned between 1981 and 1985, increasing the total number of reactors to 12. As of 2023, there are still six reactors in operation in Sweden.

Despite large levels of radiation affecting significant parts of Sweden, the established parties showed no major signs of reconsidering their nuclear power policies. The Moderate Party was still staunchly pro-nuclear power, and the anti-nuclear Center Party was aiming to form a coalition with the Moderates, meaning that they were forced to play down their anti-nuclear rhetoric.[3] The Social Democrats, which received 45% of the votes in 1985 and managed to form a government, was nominally pro-abolishment both before and after the disaster. However, they had close ties to Sweden's largest blue-collar union, the Swedish Trade Union Confederation, whose members dominated electricity-dependent sectors such as steelmaking and in the pulp and

[2]The Center Party was the first major party in Sweden to demand that nuclear power be abolished (Asp and Holmberg 1984, p. 34). It had been against nuclear power already before the Three Mile Island accident. However, the contrast vis-à-vis the MP was considerable: The Center Party was originally an agrarian party focusing on farmer interests, with energy policy being just one issue amongst others. Moreover, it had been in government on several occasions and was, thus, more of an "establishment" party, and consequently, less radical than the MP with respect to nuclear power.

[3]In addition, the Moderate Party had received around twice as many votes as the Center Party in the 1985 election (21.3% and 12.4%, respectively).

paper industry. Considering that nuclear power represented around 50% of Sweden's electricity production by the time of the disaster, there were fears within the Social Democratic party that any abrupt abolishment of reactors would lead to significant adverse effects on blue-collar jobs. Consequently, there was significant potential for an environmentalist anti-nuclear political party.

One year after the referendum, the MP was formed. From its founding, the party has been highly sceptical of nuclear power, advocating a transition to renewable energy sources. Its national vote share in the 1982 parliamentary elections was 1.7 percent, followed by 1.5 percent in 1985 and 5.5 percent in 1988. Sweden has a system of proportional representation, meaning that a party with x percent of the national vote share obtains approximately x percent of the seats. In order to claim any seats in parliament, a party must receive a higher vote share than the election threshold of 4%. Hence, it was not until the 1988 election–the first following Chernobyl–that the MP won seats in the national parliament, the *Riksdag*. Before their election to the Riksdag, the MP had limited financial resources, and did not conduct any targeted political campaigns in fallout-affected areas.[4] Besides Chernobyl, political scientists regard the sudden mass death of thousands of harbor seals (*Phoca vitulina*) along the Atlantic coast of Sweden in 1988 as contributing factor to the success of the MP. It was initially thought to be related to marine pollution, the regulation of which was a major issue for the MP.[5] The only major political event around this time was the February 1986 assassination of Social Democratic prime minister Olof Palme, which led to sympathy votes for the Social Democrats (Esaiasson and Granberg 1996).

The breakthrough of the MP mirrored a similar development in other Western nations. In 1983, the West German Green Party won representation in the *Bundestag*, becoming the first major green party represented in a national parliament. In addition, the 1980s saw an increase in public awareness of other environmental issues, most notably regarding the depletion of the Earth's ozone layer (Christoff 1994). The re-escalation of the Cold War in the early 1980s provided further boosts for green movements, as most green parties emphasized both environmental and peace issues (Rüdig 2019). By 1998, green parties were junior coalition members in Finland, France, and Germany.

In recent years, the global upswing of green parties has accelerated due to mounting cross-country concern regarding the impact of climate change. In Sweden, however, the vote share of the MP has hovered around four to six percent since the early 1990s, as seen in Figure 1.

[4] In Sweden, government subsidies are given to parties conditional on having received at least 2.5% in either of the two most recent parliamentary elections. Since the MP did not live up to this criterion before 1988, it did not receive public funding at the time of the disaster. Hence, there were little resources to conduct targeted campaigns. However, the co-chair of the MP at the time, Eva Goës, lived in Härnösand, one of the most fallout-affected municipalities. She was actively campaigning by speaking to voters and local media, even conducting radiation measurements using her personal Geiger counter. According to co-chair Birger Schlaug, this was not the result of some pre-agreed political strategy, but merely a consequence of Goës residing in the area. *Source*: Author's email correspondence with Schlaug.

[5] It was later concluded that the mass death of seals was caused by the virus *Phocine morbillivirus*, and was unrelated to pollution or eutrophication.

Over the years, the MP has de-emphasized nuclear power, and increased its focus on climate change and energy transition. Still, in the exit poll following the 2022 elections, a plurality of voters still regarded the MP as having the best policy on environmental issues.[6] Akin to its Green sister parties in other European countries, the MP is progressive in social issues, particularly with respect to immigration. Its liberal stance on immigration contrasts the views of the majority of the electorate, which is in favor of a more restrictive immigration policy (Bolin and Aylott 2019). It had a supply-and-confidence agreement with the Social Democratic government during 2002–06, and were junior members in a centre-left coalition with the Social Democrats between 2014 and 2021.

3. Data

3.1. Radiation data

To estimate Chernobyl fallout exposure in each municipality, I rely on aerial measurements of ground deposition of cesium conducted by authorities immediately after the accident, which commenced May 9, and lasted until June 3 (Bennerstedt et al. 1986). Figure 2 illustrates the significant geographical variation in fallout levels observed following the accident. The northernmost parts of Sweden were essentially spared, as was most of southern Sweden. Instead, the highest concentration of ground cesium deposition was in coastal areas in the central parts of the country. The measure of ground contamination is in kilobequerels per square meter (kBq/m^2).

In all, there were 284 municipalities (*kommuner*) in Sweden at the time of the Chernobyl disaster. These were divided into 24 counties (*län*). I then proceed by using ArcGIS to calculate the average ground deposition of cesium for each municipality. An alternative approach is to construct a dummy variable taking the value one if ground deposition of cesium was greater than 37 kBq/m^2, and zero else.[7] This cut-off is often used by to define an area as contaminated, and many research papers examining the consequences of Chernobyl use this measure (cf. Almond et al. 2009; Bauer et al. 2017).

3.2. Election data and survey design

To test whether exposure to radioactive fallout affected electoral outcomes, our main explanatory variable of interest is the evolution of the MP vote share in parliamentary elections. Specifically, I use the municipality-level MP vote share, denoted MP_{it}, as the dependent variable.[8]

[6] The question was: "Which party has the best policy on environmental issues?". The MP received 26%. No other parties had above 15% for this question. Numbers are according to the poll by national public television broadcaster SVT.

[7] This threshold follows from an alternative measure of contamination (*curie*, Ci), where 1 Ci/km^2 = 37 kBq/m^2 (Maskalchuk 2012).

[8] The reason for performing the analysis using coarser municipality-level data, rather than to use precinct-level observations, is that that individual-level data is only available at the municipality level. In addition, Swedish municipal borders have been more or less unchanged since the 1980s, whereas precinct borders are changed frequently, usually before each election. If precinct-level data were to be used, this feature would make it it difficult to compare results across elections.

Online Appendix B provides further details into the data sources and definitions for all variables in the empirical analysis.

In addition to the election data, I use survey data from the annual *SOM*[9] survey in order to evaluate respondents' views on nuclear power, and whether exposure to Chernobyl fallout affected those views. The survey takes the form of a paper questionnaire, and I use survey data from 1986 to 2018.[10] As the name suggests, the questions survey respondents' views on politics, society, and media. A question on respondents' views on nuclear power has been asked every year, which allows us to investigate differences in attitudes towards nuclear power, both between municipalities, and between time periods.

The survey has two key features making it suitable for our analysis. First, the respondents are chosen randomly among the Swedish adult population (aged 16–85), which is important for inference. Secondly, the relatively large sample size – around 3,500 observations per year – allows for municipality-level breakdown of attitudes towards nuclear power. However, many small municipalities will typically have relatively few observations for a given year. To overcome this obstacle, and to allow for consistency with the election results, I merge several survey waves to match the corresponding election years. For instance, one subsample corresponds to 1986–88, followed by 1989–91, 1992–1994, and so on.[11] This allows us to construct the variable *Support nuclear power*$_{it}$ for municipality i and election period t as the share of respondents supporting the long-term use of nuclear power in Sweden. Matching survey data with fallout data allows us to examine whether there was a change in respondents' attitudes towards nuclear power between subsample periods. Table A.1 of Online Appendix A presents the summary statistics for all variables used in the empirical analysis. Online Appendix B provides additional details on definitions and data sources for the variables used in the empirical analysis. Online Appendix C provides additional details on the structure of the questionnaire, as well as the exact wording of the questions and answers available to respondents.

4. The impact of fallout on the green vote

4.1. Year by year estimates

This section presents the voting results. First, to assess the impact of the Chernobyl disaster on the MP vote, I estimate the specification

$$\text{MP}_{it} = \beta_0 + \sum_{t \neq 1985} \beta_t \left(\text{Fallout}_i \times I_t\right) + \boldsymbol{\beta}' \boldsymbol{X}_{it} + \gamma_t + \eta_i + \varepsilon_{it} \tag{1}$$

where MP_{it} is the year by year z-score of the MP vote share for municipality i and time t, β_0 is a constant, Fallout_i is the ground deposition of cesium in the municipality, which is interacted with the corresponding election year dummies, denoted I_t for $t = 1982 \ldots 2018$. In addition, $\boldsymbol{X}_{it}$ is a vector of municipality-specific controls, γ_t is a year fixed effect, η_i is a municipality fixed

[9] Shorthand for "Society, Opinion, Media" (*Samhälle, Opinion, Medier*).

[10] The 1986 survey, which was the first one, was sent to households in October 1986, six months after the Chernobyl accident.

[11] Starting in 1994, Sweden has used four-year terms.

effect, and ε_{it} is an idiosyncratic error term. The 1985 election is omitted for comparison. The reason behind the year by year standardization of the MP vote share is that the local MP vote share tends to vary with the average national support of the party. For the fallout measure, I consider both the standardized average fallout level, as well as the binary variable equal to unity if the ground deposition of cesium was greater than 37 kBq/m^2, and zero else.

I begin by presenting the results without additional municipality-specific controls. The estimated β_t:s are plotted in Figure 3, while Table A.2 of Online Appendix A gives the full results table.[12] To account for potential dependence based on spatial proximity, I use spatial corrected standard errors (Conley 1999). As expected, there was no impact from fallout on the MP vote at the 1982 election, taking place several years before the accident. In the 1988, 1991, and 1994 elections, there were positive effects on the MP vote. For the 1988 election, one standard deviation higher ground deposition was associated with a 0.19 standard deviation higher MP vote share. In the 1991 and 1994 elections, the magnitude of this coefficient decreased to approximately 0.13 standard deviations. After the 1994 election, none of the estimated β_t:s were statistically different from zero. Alternatively, we may consider the municipalities with ground deposition above 37 kBq/m^2. In these municipalities, the MP vote share in the 1988 election was around one standard deviation higher. In subsequent elections, the high ground deposition dummy was statistically insignificant.

To account for potential confounding factors not captured by the municipality fixed effects, Table A.2 of Online Appendix A includes the 1980 referendum results for Option 3, as well as the indicator variable for municipalities heavily affected by the 1986 seal virus epidemic, the latter being the other major environmental issue in the 1988 election.[13] There are only minor changes in the coefficients after including these variables, suggesting that neither pre-Chernobyl views about nuclear power nor the seal virus epidemic are driving our results.[14] The latter finding is consistent with recent research downplaying the role of the seal epidemic in explaining the growth of the MP in the late 1980s (Ljunggren 2010). Finally, Figure 3 illustrates that there was no increase in the MP vote share between the 2010 and the 2014 elections, suggesting that the 2011 Fukushima disaster did not affect MP voting.[15]

While we cannot fully exclude that the vanishing Chernobyl premium on the MP vote was partially due to the party shifting towards other issues, there are at least two arguments against. First, the findings on the long-term effects are consistent with previous research. A regional stimulus program enacted by the German government following severe flooding in 2002 increased voter support for the incumbent party in affected areas, however, more than half of

[12] As there were only 279 municipalities by the time of the 1980 referendum (as opposed to 284 in 1986), this is the number of observations in our model.

[13] Since both of these controls are time-independent, they are also interacted with the time dummies in (1).

[14] Additionally, the results are robust to the inclusion of an additional set of municipality controls, including the population, employment rate, and the share of residents with a college degree. These results are available upon request.

[15] To confirm this, Table A.3 of Online Appendix A presents the OLS estimates from regressing the percentage point change in the MP vote share between 2010 and 2014 on 1986 fallout levels. The coefficient estimate for 1986 fallout is statistically insignificant. This finding is expected considering that 25 years had passed between the two accidents.

the gains vanished in the following election, suggesting that voter memory is fairly short-lived (Bechtel and Hainmueller 2011). Second, the rival Center Party abolished their anti-nuclear stance after the 2002 elections, making the MP the largest party that retained a significant focus on anti-nuclear issues (Forssblad 2008).

4.2. Short-term effects on other parties and turnout

1. Effects on other parties in the 1988 election

The results presented previously suggest that it was primarily in the 1988 election, the first after Chernobyl, that fallout exposure led to significant changes in voter preferences in favor of the MP. To evaluate the effects on other parties in the 1988 election, I estimate

$$\Delta\text{Vote share}_i^{1985-88} = \beta_0 + \beta_1 \text{Fallout}_i + \boldsymbol{\beta}' \boldsymbol{X}_i + \varepsilon_i \tag{2}$$

Tables A.4–A.5 of Online Appendix A present the results when re-estimating (1) with the vote shares of the incumbent Social Democrats, and the anti-nuclear Center Party.

The results show that fallout was significantly negatively related to Social Democratic vote shares. One standard deviation higher average ground deposition rates decreased the Social Democratic vote share by approximately 0.12 standard deviations. In municipalities subject to more than 37 kBq/m^2 of fallout, the incumbent vote share was approximately 0.75 standard deviations lower in the 1988 election. There were no significant effects on the Center Party vote shares.

2. Effects on turnout in the 1988 election

Did the accident lead to a mobilization of new voters? To examine the effect on turnout between 1985 and 1988, I estimate a similar model to the one above, namely

$$\Delta\text{Turnout}_i^{1985-88} = \beta_0 + \beta_1 \text{Fallout}_i + \boldsymbol{\beta}' \boldsymbol{X}_i + \varepsilon_i \tag{3}$$

where $\Delta\text{Turnout}^{1985-88}$ denotes the standardized percentage point difference in the turnout rate between the 1985 and 1988 elections for municipality i.

The results are presented in Table A.6 of Online Appendix A. The coefficient estimate $\hat{\beta}_1$ is statistically insignificant, suggesting that the increase of the MP vote share was solely due to shifting party preferences among existing voters. As discussed above, this voter shift was chiefly due to voters abandoning the incumbent party, the Social Democrats.

4.3. Robustness

1. Parallel trends assumption

I proceed by testing the sensitivity of the main estimates in Figure 3 from deviations from the parallel trend assumption, using the Rambachan-Roth approach (Rambachan and Roth 2022). This method relaxes the parallel trends assumption by allowing deviations from linearity up to a parameter M, where larger values of M lead to wider confidence sets. Figure A.1 of Online Appendix A presents the estimated confidence intervals for the coefficient associated with

Fallout $\times I^{1988}$, which is the main variable of interest in the paper. The estimated coefficient is statistically significant even when allowing for large deviations from the linear trend. Online Appendix D discusses the method in additional detail.

2. Placebo test

As a test of the identification strategy, Table A.7 of Online Appendix A gives placebo estimates, re-estimating (1) as a simple linear regression with the MP vote share difference between the 1982 and 1985 elections as the dependent variable. Since the 1982–85 term terminated one year before Chernobyl, the coefficient estimate for fallout should be zero. As expected, both when using the average ground deposition and the high-fallout dummy, the coefficient estimate for fallout is close to zero, and statistically insignificant.

5. Individual-level evidence

Did the positive effect on MP voting in 1988 mirror a change in attitudes towards nuclear power in fallout-affected areas? If the increased MP voting in response to fallout exposure was a result of concern about environmental accidents, we would expect public opinion to become more sceptical towards nuclear power after the accident. Another question arising is whether the lack of a long-term Chernobyl premium on the MP vote is similarly reflected in a change in attitudes toward nuclear power over time. To answer these questions, I use the results from the survey described in Section 3.2, allowing us to examine more closely the relationship between MP voting, fallout exposure and pro-environmentalist attitudes.

5.1. Relationship between fallout exposure and support for nuclear power

I start by considering the municipality-level relationship between fallout exposure and support for nuclear power. I use the same specification as the main analysis, replacing the outcome variable with z-scores of the share supporting nuclear power in each election period t for each municipality i. That is, the data is collapsed to match the election period level: 1986–1988, 1988–1991, 1991–1994, and so on. I let the 1980 referendum results serve as the reference category[16], and estimate

$$\text{Support nuclear power}_{it} = \beta_0 + \sum_{t \neq 1980} \beta_t \left(\text{Fallout}_i \times I_t\right) + \boldsymbol{\beta}' \boldsymbol{X}_{it} + \gamma_t + \eta_i + \varepsilon_{it} \tag{4}$$

Interpretation is straightforward: Negative coefficient estimates $\widehat{\beta}_t$ suggest that voters in affected areas were less inclined to support nuclear power, compared to the 1980 referendum. The results are presented in Figure 4, and the full results table is presented in Table A.8 of Online Appendix A. One standard deviation higher fallout decreases support for nuclear power by around 0.1 standard deviations for the 1986–88 period, relative to pre-Chernobyl support. The magnitudes of the estimated coefficients are similar for the 1988–91 and 1991–94 periods,

[16] Formally, the inverse of the vote share for Option 3 (abolishment as soon as possible) is used as a proxy for the pre-Chernobyl share supporting nuclear power.

although the coefficient is statistically insignificant for the former. After 1994, there is no significant relationship between fallout exposure and support for nuclear power. When the high fallout dummy is used, only the results for the 1986–88 period is significant. In highly affected municipalities, support for nuclear power decreased by around 0.4 standard deviations compared to the 1980 referendum. A caveat to note is that for some smaller municipalities, the number of respondents may be low, even when several survey waves are collapsed into election periods. However, overall, the survey results are consistent with the voting results.

5.2. Media and voter information

1. Local media coverage depending on fallout levels

Another question is related to the process through which process voters gather information regarding local fallout levels. If voters were uninformed about the levels of fallout in their home municipality, it is unlikely that we would see variation in MP voting across municipalities. One potential channel through which voters update their knowledge about regional fallout levels is through local media. To answer whether there was a variation in newspaper coverage based on fallout levels, I use data from scanned print versions of the universe of Swedish newspapers from 2013 to 2019; in all, approximately 250 newspapers.[17] I then regress the number of times the words "cesium" and "Chernobyl" appear in the most-circulated newspaper for each municipality on the maximum and average fallout levels, after adjusting for the number of days per week the newspaper circulates.

The results are presented in Table A.9 of Online Appendix A, suggesting that the words "cesium" and "Chernobyl" are significantly more common in local papers of municipalities with higher fallout exposure. This is particularly evident for "cesium", and in municipalities outside the largest urban areas. Given this, it is likely that there was a regional difference in newspaper coverage of Chernobyl in the months and years immediately after the accident as well. Considering that cesium fallout is associated with significant health hazards, we would expect the MP to have higher potential among informed voters, that is, voters who frequently read the papers. This would, thus, provide a plausible channel for explaining the variation in the MP vote share across municipalities. I examine in the next section whether this was the case.

2. Media exposure and perceptions of nuclear power

Finally, the results of this paper have shown that support for nuclear power was lower in fallout-affected areas. The survey enables us to examine the mechanisms behind this stance in additional detail. Specifically, the survey asks respondents to evaluate a number of potential hazards related to nuclear power, and rate from 1 to 10 how worried he or she is about the hazard in question. Here, 1 is "not at all worried", and 10 is "very worried". I consider the survey years 1986–1988, that is, between the accident and the election. For each question, I calculate the share of very worried respondents (10 on the 1–10 scale), and regress this share on fallout levels and the municipality-specific controls. In addition, I consider both the entire sample, as well as limiting the sample to include only respondents who state that they read the local newspaper a minimum of six times per week. The hypothesis here is that newspaper

[17] Unfortunately, this data is not available for the years immediately after Chernobyl.

readers are likely to be better informed about local fallout levels, impacting perceived risks and, eventually, party preferences.

The results are presented in Table 1. Respondents are more worried about cancer incidence, deteriorated air and water quality, and the implications for future generations, than accidents or unsafe disposal of radioactive waste. Additionally, the estimated effect sizes tend to be larger when considering only the frequent paper readers. For instance, the share of respondents very worried about cancer incidence was around 0.4 standard deviations higher in high-fallout municipalities when the full sample is used, but around 0.5 standard deviations higher when only the frequent newspaper readers are considered. Consistent with the hypothesis outlined above, this finding suggest that negative opinions about nuclear energy, and consequently, increased MP voting, was driven primarily by well-informed voters.

Since respondents were not particularly worried about the risk of a nuclear accident in Sweden, the results about the fear of higher cancer incidence and the deterioration of air and water quality, which are commonly associated with nuclear accidents, can be interpreted as voters being worried about accidents outside Sweden, for instance in the Soviet Union. It could also be the case that respondents are worried about other incidents that do not constitute full meltdowns, or that operating nuclear power plants can deteriorate local environments and lead to releases of carcinogenic waste.

6. Concluding remarks

Climate change is likely the largest threat to human well-being, and is a considerable policy challenge. Particularly, in light of global warming, natural disasters and environmental accidents will become more prevalent in the future. This paper provides causal evidence that local exposure to radioactive fallout from Chernobyl, one of the most disastrous environmental accidents in modern history, caused major changes in voter preferences in hard-hit areas in Sweden.

The findings of the paper show that the environmentalist vote share increased in areas with significant ground contamination of cesium, with voters punishing the incumbent party. Individual-level survey data suggests that these changes are due to voters in affected areas becoming more sceptical towards nuclear power. However, these positive effects on the green vote disappeared after three elections, indicating that voter memory is fairly short-lived.

Fortunately, major accidents in nuclear reactors are rare. However, when they do happen, the the consequences are catastrophic. Local environments can be contaminated for centuries, and there are significant long-term ramifications for public health, and also on voting, as shown in this paper. The results of the paper are generalizable to other types of environmental disasters, for instance floods, droughts, and temperature shocks. However, the political effects of an environmental accident are likely to depend heavily on government actions, as shown in previous research (Bechtel and Hainmueller 2011). With this in mind, more research is likely needed to establish how the short- and long-term effects of environmental accidents on electoral outcomes.

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Figure 1: Plot of the MP vote share for each election year between 1982 and 2018. *Data source:* Swedish Statistics Agency.

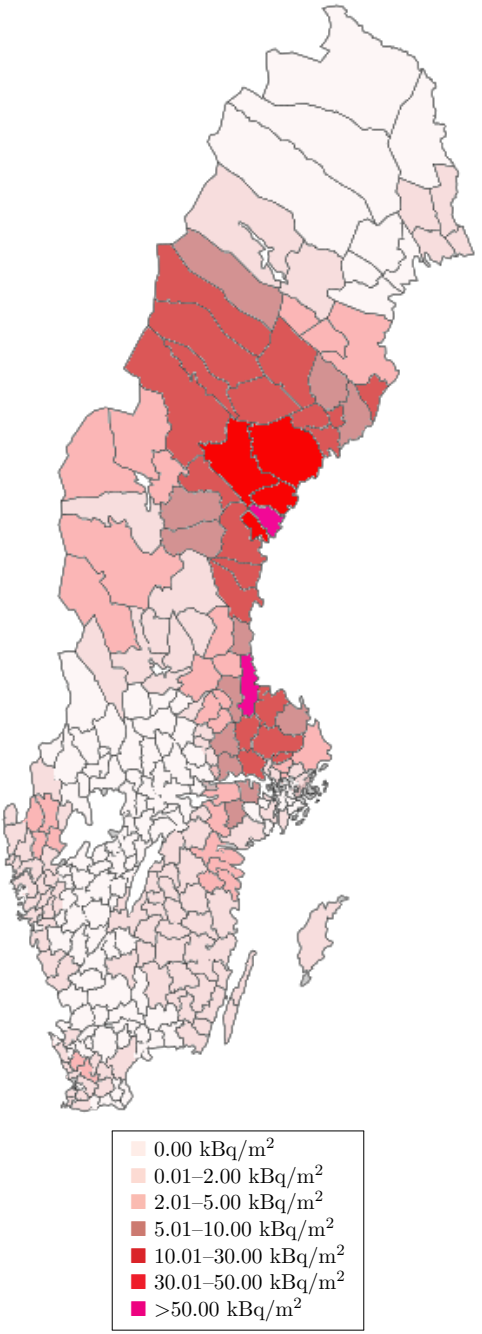

Figure 2: Variation in average ground deposition at the municipality level.

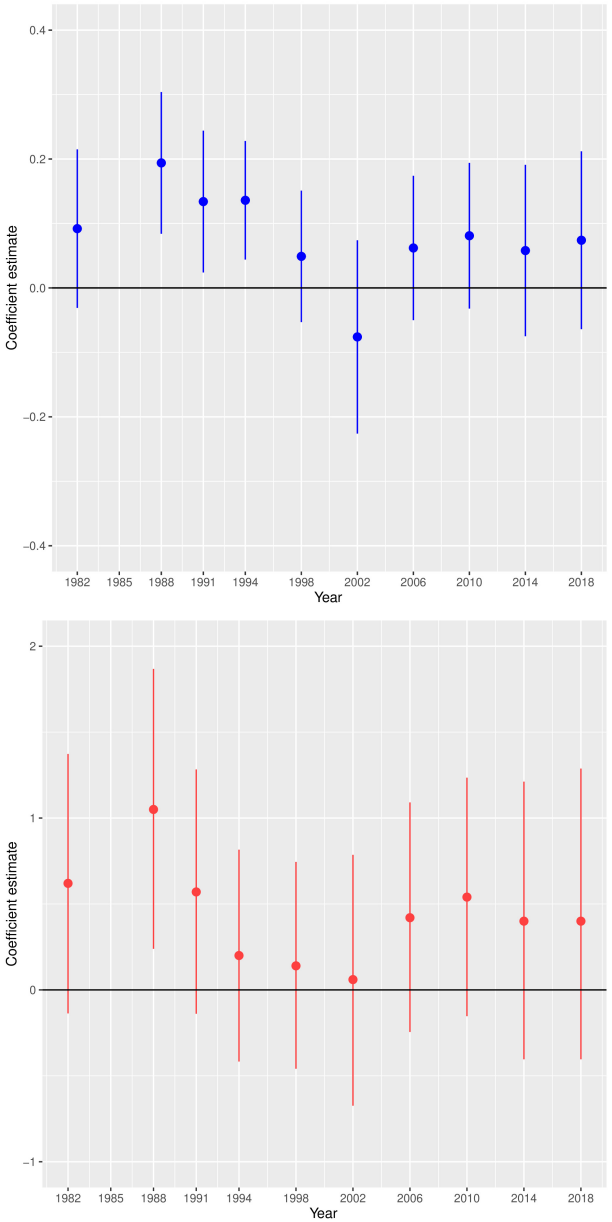

Figure 3: Estimated Chernobyl effect on the MP vote share for each election 1982–2018, with the 1985 election omitted for comparison. The top panel uses the continuous measure, the bottom panel uses the high fallout dummy. 90% confidence intervals are used. No controls included.

TABLE 1
CONCERN ABOUT NUCLEAR RISKS AND FALLOUT EXPOSURE

Outcome variable: Share very worried	Average ground deposition				High ground deposition dummy			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
Panel A. Risk: "Major accident in Sweden"								
Fallout	0.002	0.015	−0.014	0.012	−0.099	0.033	−0.160	0.006
	(0.041)	(0.041)	(0.042)	(0.043)	(0.267)	(0.261)	(0.250)	(0.255)
Panel B. Risk: "Higher incidence of cancer"								
Fallout	0.107**	0.112**	0.079	0.087*	0.503**	0.621***	0.401**	0.503***
	(0.053)	(0.051)	(0.051)	(0.048)	(0.231)	(0.226)	(0.201)	(0.199)
Panel C. Risk: "Deteriorated air and water quality"								
Fallout	0.142**	0.139**	0.116*	0.123**	0.268	0.413	0.166	0.336
	(0.066)	(0.063)	(0.063)	(0.061)	(0.456)	(0.430)	(0.435)	(0.415)
Panel D. Risk: "Problems for future generations"								
Fallout	0.121**	0.104**	0.077*	0.069	0.315	0.440*	0.236	0.383
	(0.049)	(0.045)	(0.045)	(0.044)	(0.258)	(0.264)	(0.245)	(0.258)
Panel E. Risk: "Unsafe disposal of nuclear waste"								
Fallout	−0.018	0.016	−0.046	−0.004	−0.065	0.107	−0.113	0.073
	(0.043)	(0.043)	(0.044)	(0.045)	(0.203)	(0.149)	(0.141)	(0.141)
Only paper readers	No	Yes	No	Yes	No	Yes	No	Yes
Municipality-specific controls	No	No	Yes	Yes	No	No	Yes	Yes
Municipalities	284	279	284	279	284	279	284	279
Observations	4,614	3,675	4,614	3,675	4,614	3,675	4,614	3,675
Mean dep. var.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Note. Outcome variable: Standardized share of respondents claiming to be "very worried" about each of the five risks associated with nuclear power use in Sweden. Column (1): No controls, full sample. Column (2): No controls, sample restricted to include only those who read a newspaper at least six times per week. Column (3): Controls for 1980 referendum results and 1986 seal virus deaths, full sample. Column (4): Controls for 1980 referendum results and 1986 seal virus deaths, restricted sample. A constant is included in all regressions. Spatial corrected standard errors in brackets. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

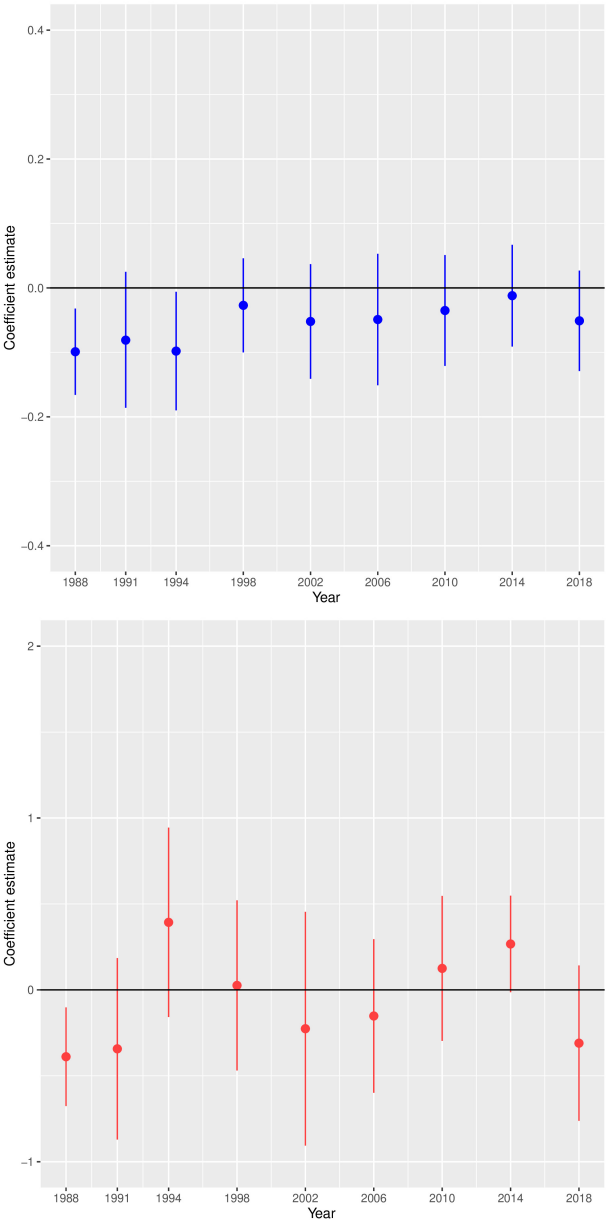

Figure 4: Estimated Chernobyl effect on the share supporting nuclear power for each election 1988–2018. The baseline year is 1980, for which municipality-level variation in the pro-nuclear options in the referendum is utilized. The top panel uses the continuous measure, the bottom panel uses the high fallout dummy. 90% confidence intervals are used. No controls included.

Online Appendix [Not for Publication]

A. Additional empirical results

Table A.1
Summary statistics

Fallout variable	Mean	Std.dev.	Min	Max
Average ground deposition (kBq/m^2)	3.512	8.110	0	73.516
High-fallout municipality	0.018	0.318	0	1
Election variables				
MP vote share, all time periods	3.697	1.817	0.3	12.8
Social Democratic vote share, all time periods	40.43	10.47	5.8	72.8
Center Party vote share, all time periods	11.29	6.656	1.1	40.3
$\Delta S^{1985-1988}$ (p.p.)	−0.655	1.433	−5.1	2.8
$\Delta C^{1985-1988}$ (p.p.)	−1.715	2.293	−11.8	5.2
$\Delta Turnout^{1985-1988}$ (p.p.)	−3.841	.871	−6.1	0.4
Survey variables				
Supporting nuclear power, all time periods (%)	31.39	7.32	10.04	53.56
Share very worried about major accident (%)	11.98	13.50	0	100
Share very worried about higher cancer incidence (%)	16.54	17.46	0	100
Share very worried about deteriorated air and water quality (%)	19.59	17.90	0	100
Share very worried about problems for future generations (%)	19.73	17.45	0	100
Share very worried about unsafe disposal of nuclear waste (%)	33.87	18.80	0	100
Municipal characteristic controls				
Coastal municipality (indicator)	0.080	0.271	0	1
Option 3 vote share, 1980 (%)	40.823	8.584	17.644	64.13

Note. Abbreviations: MP = Green Party, S = Social Democrats, C = Center Party.

Table A.2
The impact of fallout on the green vote

Outcome variable:	Average ground		High-deposition	
MP vote share	deposition		dummy	
	(1)	(2)	(1)	(2)
Fallout $\times I^{1982}$	0.092	0.090	0.618	0.618
	(0.075)	(0.074)	(0.459)	(0.459)
Fallout $\times I^{1988}$	0.194***	0.193***	1.054**	1.034**
	(0.067)	(0.064)	(0.495)	(0.481)
Fallout $\times I^{1991}$	0.134**	0.131**	0.572	0.567
	(0.067)	(0.064)	(0.432)	(0.418)
Fallout $\times I^{1994}$	0.136**	0.122**	0.199	0.273
	(0.056)	(0.048)	(0.375)	(0.346)
Fallout $\times I^{1998}$	0.049	0.048	0.143	0.154
	(0.062)	(0.059)	(0.366)	(0.368)
Fallout $\times I^{2002}$	−0.076	−0.072	0.056	−0.023
	(0.091)	(0.088)	(0.444)	(0.442)
Fallout $\times I^{2006}$	0.062	0.060	0.423	0.378
	(0.068)	(0.067)	(0.406)	(0.407)
Fallout $\times I^{2010}$	0.081	0.086	0.541	0.480
	(0.069)	(0.067)	(0.422)	(0.423)
Fallout $\times I^{2014}$	0.058	0.065	0.404	0.331
	(0.081)	(0.080)	(0.491)	(0.502)
Fallout $\times I^{2018}$	0.074	0.076	0.404	0.345
	(0.084)	(0.084)	(0.537)	(0.545)
Time FE	Yes	Yes	Yes	Yes
Municipality-specific controls	No	Yes	No	Yes
Municipalities	284	284	284	284
Observations	3,124	3,074	3,124	3,074
Mean dep. var.	0.000	0.000	0.000	0.000

Note. Outcome variable: MP vote share. A constant is included in all regressions. Column (1): No controls. Column (2): Controls for 1980 referendum results and 1986 seal virus deaths. Spatial corrected standard errors in brackets. ** and *** denote significance at the 5% and 1% level, respectively.

Table A.3
The impact of the Fukushima accident

Outcome variable:	Average ground		High-deposition	
Difference in MP vote share, 2010–14	deposition		dummy	
	(1)	(2)	(1)	(2)
Fallout	−0.104	−0.065	−0.538	−0.433
	(0.077)	(0.077)	(0.442)	(0.479)
Controls	No	Yes	No	Yes
Observations	284	279	284	279
R^2	0.011	0.097	0.001	0.006
Mean dep. var.	0.000	0.000	0.000	0.000

Note. Outcome variable: Standardized difference in the MP vote share, 1982–85. A constant is included in all regressions. Column (1): No controls. Column (2): Controls for 1980 referendum results and 1986 seal virus deaths. Spatial corrected standard errors in brackets.

Table A.4
The impact of fallout on the Social Democratic vote share

Outcome variable:	Average ground		High-deposition	
Diff. in Social Dem. vote share	deposition		dummy	
	(1)	(2)	(1)	(2)
Fallout	-0.126^{***}	-0.156^{***}	-0.742^{***}	-0.767^{***}
	(0.046)	(0.046)	(0.130)	(0.120)
Controls	No	Yes	No	Yes
Observations	284	279	284	279
R^2	0.002	0.007	0.001	0.006
Mean dep. var.	0.000	0.000	0.000	0.000

Note. Outcome variable: Standardized difference in the Social democratic vote share, 1985–88. A constant is included in all regressions. Column (1): No controls. Column (2): Controls for 1980 referendum results and 1986 seal virus deaths. Spatial corrected standard errors in brackets. *** denotes significance at the 1% level

Table A.5
The impact of fallout on the Center Party vote share

Outcome variable:	Average ground		High-deposition	
Diff. in Center Party vote share	deposition		dummy	
	(1)	(2)	(1)	(2)
Fallout	−0.093*	−0.039	−0.118	−0.031
	(0.051)	(0.045)	(0.198)	(0.179)
Controls	No	Yes	No	Yes
Observations	284	279	284	279
R^2	0.002	0.007	0.001	0.006
Mean dep. var.	0.000	0.000	0.000	0.000

Note. Outcome variable: Standardized difference in the Center Party vote share, 1985–88. A constant is included in all regressions. Column (1): No controls. Column (2): Controls for 1980 referendum results and 1986 seal virus deaths. Spatial corrected standard errors in brackets. * denotes significance at the 10% level

Table A.6
The impact of fallout on turnout

Outcome variable:	Average ground		High-deposition	
Difference in turnout rate	deposition		dummy	
	(1)	(2)	(1)	(2)
Fallout	0.049	0.033	−0.186	−0.148
	(0.065)	(0.059)	(0.373)	(0.372)
Controls	No	Yes	No	Yes
Observations	284	279	284	279
R^2	0.002	0.007	0.001	0.006
Mean dep. var.	0.000	0.000	0.000	0.000

Note. Outcome variable: Standardized difference in turnout rates, 1985–88. A constant is included in all regressions. Column (1): No controls. Column (2): Controls for 1980 referendum results and 1986 seal virus deaths. Spatial corrected standard errors in brackets.

Table A.7
Placebo regression: MP vote share 1982–85

Outcome variable:	Average ground		High-deposition	
Difference in MP vote share, 1982–85	deposition		dummy	
	(1)	(2)	(1)	(2)
Fallout	0.011	−0.017	−0.108	−0.126
	(0.056)	(0.053)	(0.314)	(0.323)
Controls	No	Yes	No	Yes
Observations	284	279	284	279
R^2	0.000	0.034	0.000	0.034
Mean dep. var.	0.000	0.000	0.000	0.000

Note. Outcome variable: Standardized difference in the MP vote share, 1982–85. A constant is included in all regressions. Column (1): No controls. Column (2): Controls for 1980 referendum results and 1986 seal virus deaths. Spatial corrected standard errors in brackets.

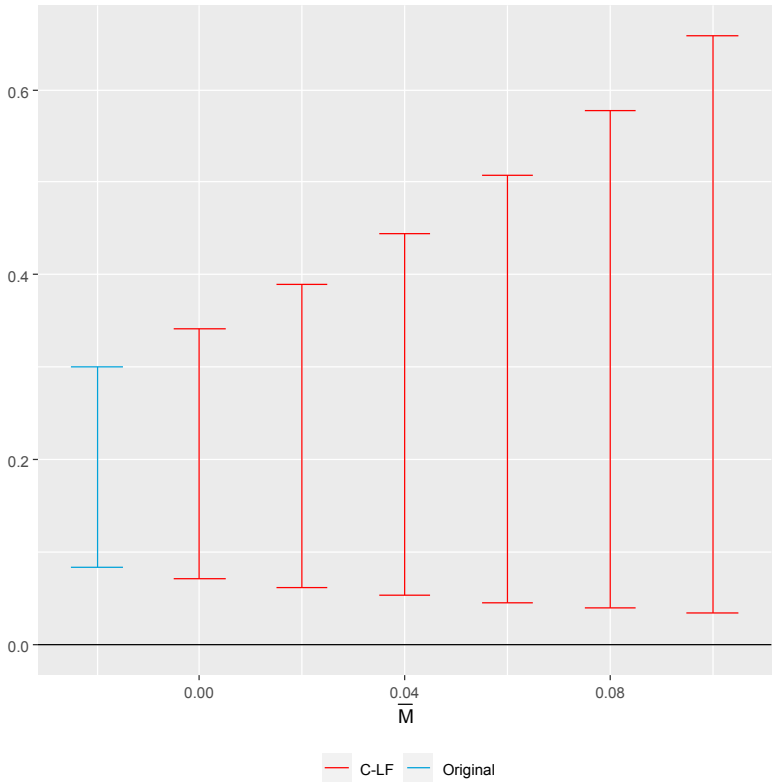

Figure A.1: Sensitivity analysis. Estimated 90% confidence intervals for various levels of M between 0 and 1 for the coefficient associated with Fallout $\times$ I^{1988}.

Table A.8
The impact of fallout on the share supporting nuclear power

Outcome variable: Share supporting nuclear power	Average ground deposition	High-deposition dummy
Fallout $\times I^{1988}$	-0.099^{**}	-0.389^{**}
	(0.041)	(0.174)
Fallout $\times I^{1991}$	-0.081	-0.343
	(0.064)	(0.320)
Fallout $\times I^{1994}$	-0.098^{*}	0.393
	(0.056)	(0.334)
Fallout $\times I^{1998}$	-0.027	0.026
	(0.044)	(0.300)
Fallout $\times I^{2002}$	-0.052	-0.226
	(0.054)	(0.412)
Fallout $\times I^{2006}$	-0.049	-0.152
	(0.062)	(0.271)
Fallout $\times I^{2010}$	-0.035	0.125
	(0.052)	(0.256)
Fallout $\times I^{2014}$	-0.012	0.267
	(0.048)	(0.170)
Fallout $\times I^{2018}$	-0.051	-0.310
	(0.047)	(0.274)
Year FE	Yes	Yes
Municipality-specific controls	No	No
Municipalities	284	284
Observations	2,821	2,821
Mean dep. var.	0.000	0.000

Note. Outcome variable: Standardized share supporting nuclear power. A constant is included in all regressions. Spatial corrected standard errors in brackets. * and ** denote significance at the 10% and 5% level, respectively.

Table A.9
Occurrence of Chernobyl-related words in media

Outcome variable:	"Cesium"		"Chernobyl"	
Cumulative number each word appears in most circulated newspaper	(1)	(2)	(1)	(2)
Panel A. Average ground deposition				
Fallout	0.151***	0.234***	−0.043	0.050***
	(0.038)	(0.026)	(0.033)	(0.013)
Panel B. High ground deposition dummy				
Fallout	1.210***	1.521***	−0.150	0.208***
	(0.104)	(0.072)	(0.097)	(0.051)
Controls included	No	No	No	No
Only local papers	No	Yes	No	Yes
Observations	284	242	284	242
Mean dep. var.	0.000	0.000	0.000	0.000

Note. Outcome variable: Standardized cumulative number of times the words "cesium" and "Chernobyl" appear in the most circulated newspaper in each municipality from January 1, 2013, to December 31, 2019. Column (2) drops municipalities where the largest paper is either *Dagens Nyheter*, *Svenska Dagbladet*, *Göteborgs-Posten*, or *Sydsvenskan*, which have full national national coverage. A constant is included in all regressions. Spatial corrected standard errors in brackets. *** denotes significance at the 1% level.

B. Data sources and variables construction

This section describes the construction of the variables used in the empirical analysis in additional detail.

Election variables. All election data is due to the Swedish Statistics Agency. This includes the historical vote shares of the MP and other parties, as well as the municipality-level results of the 1980 referendum.

Data on fallout As described in Sections 2 and 3, the fallout data comes from aerial measurements conducted by Swedish authorities. The variable for average ground deposition is standardized, so that the sample mean is equal to zero, and the sample standard deviation is equal to unity. The high fallout dummy is equal to unity if average fallout was above 37 kBq/m^2, and zero else.

Coastal municipality dummy. The virus epidemic affected most of the western coast of Sweden, specifically areas of the Kattegat and Skagerrak coastlines. The combined Kattegat and Skagerrak straits are occasionally known as the *Jutland sea*. In terms of counties, all coastal municipalities of Gothenburg and Bohus County and Halland County were affected, as was the western coast of Kristianstad County and a small part of Malmöhus County.

In all, 20 municipalities are classified as being on the western coast: Gothenburg, Kungälv, Lysekil, Munkedal, Orust, Sotenäs, Stenungsund, Strömstad, Tanum, Tjörn, Uddevalla, and Öckerö municipalities in Gothenburg and Bohus County, Falkenberg, Halmstad, Kungsbacka, Laholm, and Varberg municipalities in Halland County, Båstad and Ängelholm municipalities in Kristianstad County, and Höganäs municipality in Malmöhus County.

C. Survey construction

The following section describes in detail the formulation of the question related to nuclear power in each of the SOM surveys. Checked boxes denote the responses used to construct the variable *Support nuclear power*. The responses to these questions are collapsed to match the election years.

1986 version

- ☐ Abolish nuclear power immediately.
- ☐ Abolish nuclear power before 2010.
- ☐ Abolish nuclear power by 2010.
- ☐ Abolish nuclear power, but not as early as 2010.
- ☑ Use nuclear power, do not abolish it.

1987–1995 version

What is your view on the use of nuclear power in Sweden?

- ☐ Abolish nuclear power immediately.
- ☐ Abolish nuclear power before 2010.
- ☐ Abolish nuclear power by 2010.
- ☐ Abolish nuclear power, but not that early.
- ☑ Use nuclear power, do not abolish it.
- ☐ I have no particular opinion on the matter.

1995–1999 version

What is your view on the use of nuclear power in Sweden?

- ☐ Abolish nuclear power by 2010.
- ☐ Abolish nuclear power, but use the 12 reactors we have until the end of their life cycle.
- ☑ Use nuclear power and renew the 12 reactors we have, so that Sweden in the future will have 12 reactors in operation.
- ☑ Use nuclear power and invest in more reactors than 12 in the future.
- ☐ I have no particular opinion on the matter.

2000–2004 version

What is your view on the long-term use of nuclear power in Sweden?

☐ Abolish nuclear power by 2010.
☐ Abolish nuclear power, but use the reactors we have until the end of their life cycle.
☑ Use nuclear power and renew the current nuclear reactors, but do not build any new reactors.
☑ Use nuclear power and invest in more reactors in the future.
☐ I have no particular opinion on the matter.

2005–2011 version

What is your view on the long-term use of nuclear power as a source of energy in Sweden?

☐ Abolish nuclear power as soon as possible.
☐ Abolish nuclear power, but use the reactors we have until the end of their life cycle.
☑ Use nuclear power and renew the current nuclear reactors, but do not build any new reactors.
☑ Use nuclear power and invest in more reactors in the future.
☐ I have no particular opinion on the matter.

2011–2018 version

What is your view on the long-term use of nuclear power as a source of energy in Sweden?

☐ Abolish nuclear power as soon as possible.
☐ Abolish nuclear power, but use the 10 reactors we have until the end of their life cycle.
☑ Use nuclear power and replace the current reactors with at most 10 new reactors.
☑ Use nuclear power and build more reactors than the current 10 in the future.
☐ No opinion.

D. Assessing the parallel trends assumption

To examine the sensitivity of the main results to potential violation of the parallel trends assumption, I implement the Rambachan-Roth approach (Rambachan and Roth 2022; henceforth referred to as R&R), a method widely used in recent empirical studies (cf. Biasi and Sarsons 2022; Dustmann et al. 2022). R&R's main robustness test involves constructing a set Δ of potential deviations from the parallel trends assumption, and then, constructing confidence intervals associated with these deviations for the parameters of interest. Using the same notation as in the paper by R&R, I denote by δ the trend, and by $M \geq 0$ the amount by which the slope of δ can change between consecutive periods. Following R&R, and using their notation $\Delta^{SD}(M)$ for the set of potential deviations (the abbreviation SD is for "second differences" or "second derivative"), we have

$$\Delta^{SD}(M) := \{\delta : |(\delta_{t+1} - \delta_t) - (\delta - \delta_{t-1})| \leq M, \forall t\} \tag{D.1}$$

In the special case when $M = 0$, the difference in trends are exactly linear. While there is nothing in the data that can place an upper bound on the value of M, R&R suggest 0.5*standard error as the default maximum value. For the coefficient associated with Fallout $\times I^{1988}$, this is equivalent to approximately $M = 0.035$. Figure A.1 of Online Appendix A presents the estimated confidence intervals for the coefficient associated with Fallout $\times I^{1988}$ for values of M ranging from 0 to a considerably more conservative estimate of 0.10. As the maximum deviation from the linear trend increases, the confidence intervals around the estimates become wider, as expected. However, regardless of the value of M, we see that there is a positive relationship between the fallout level and the MP vote share in the 1988 elections.

Paper III

Gender, Socioeconomic Status, and Student Performance When Education Is Partially Online [1]

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Abstract

We evaluate an experiment at a Swedish university, in which students were quasi-randomized to either taking all their courses online, or to have some courses online and some on campus. We show that having some courses in person improved grades only among female students with affluent parents. Detailed individual-level survey data suggests that there was no relationship between socioeconomy and adverse mental health amid the COVID-19 pandemic. Instead, by estimating each student's network position, linked with administrative data on parental income, we show that female students with wealthy parents have significantly less constrained social networks, facilitating communication with peers.

JEL classification codes: I23; I28; J16; Z13
Keywords: online learning; COVID-19; social networks

I. Introduction

In recent years, online coursework has gained considerable ground in higher education. This trend has been exacerbated by the COVID-19 pandemic, during which higher education facilities in most nations were temporarily closed, and in-person classes largely replaced by online teaching.

A growing body of literature examines the academic consequences of taking college courses online instead of in the traditional in-person format, with most studies finding a negative relationship between distance education and test scores (Figlio et al. 2013; Alpert et al. 2016; Bettinger et al. 2017). Some studies also find a negative effect on grades from blended learning, that is, mixing online and in-person teaching, while others are more positive (Kozakowski 2019; Escueta et al. 2020). Others have noted that online education increases college enrollment, particularly among mid-career individuals who would not otherwise have pursued higher education, and that colleges with higher shares of online courses charge lower tuition fees (Deming et al. 2015; Goodman et al. 2019).

In this paper, we evaluate a quasi-natural experiment among a set of second-year engineering students at Lund University, Sweden, during the fall semester of 2020. Students were quasi-randomly assigned to either taking all of their mandatory courses online, or to have some courses fully online and some fully on campus. This produces two student groups for the mandatory courses taken online: one group having no access to campus teaching whatsoever, and one group having some, albeit unrelated, coursework on campus. We hypothesize that on-campus meetings and informal chats with peers are likely to improve learning outcomes in online courses, even if meetings take place in conjunction with classes in other courses. This hypothesis is consistent with previous studies finding significant pedagogical benefits of peer discussion and small-group learning (Springer et al. 1999; Smith et al. 2009). Communicating with classmates is likely to be facilitated by campus access, especially peers that the student is not very close friends with. In addition, campus access is likely to improve students' mental health, which is beneficial for performance in both the campus and online courses (Eisenberg et al. 2009; Cornaglia et al. 2015). We call these the spillover effects of campus teaching, and would, thus, expect positive spillover effects in online courses for students treated with parallel campus classes.

To explore whether there were any heterogeneous effects on academic outcomes depending on students' socioeconomic status, we link data on grade outcomes with detailed administrative data on parental taxable income for each student, and use this as a proxy for socioeconomic background. We utilize a number of additional individual-level controls to further isolate the effect played by socioeconomic status. Our results show that there were spillover effects of campus education only for female students with affluent parents, with the relationship increasing linearly with income.

This finding raises an important question: Are socioeconomic distortions increasing linearly with time on campus? If this were the case, we would expect that a complete return to full campus education is associated with significant socioeconomic distortions to the benefit of female students with wealthy parents. We show that there were no socioeconomic differences in

grade outcomes for the previous cohort, when all education was given on campus. This result suggests that it is the blended learning setting that causes the socioeconomic heterogeneity in grade outcomes.

What can explain this relationship between gender, parents' income and the grade outcomes of blended learning? Several recent papers have highlighted the importance of environmental rather than biological factors in intergenerational transmission, for example with respect to children's entrepreneurial success (Lindquist et al. 2015; Bell et al. 2019; Black et al. 2020). These studies often point to network effects as one of the keys in explaining the relative importance of "nurture" in this context. In our setting, one potential network-related channel is structural embeddedness, which is defined as the degree of overlap between the social networks of two individuals (Granovetter 1985). In essence, the lower the number of mutual friends shared by two people, the more open is the social network around these two individuals. Being connected across groups improves access to novel ideas, reduces information redundancy, and promotes alternative ways of thinking (Burt 2004). Such traits are likely to have a positive impact on academic outcomes. Given previous research, it is plausible that the network status of parents[1] extends to their children (Kohn et al. 1986; Coleman 1988; Conti and Heckman 2010). An alternative explanation to our findings is that female students from wealthy backgrounds are less stressed or anxious about the pandemic, and can make more of the campus experience.

Consequently, the second set of results concerns the mechanisms behind the finding that gender and socioeconomic status are positively correlated with campus spillovers under blended learning. Immediately following the end of the semester, we survey the same set of students. In our survey, we combine a standard questionnaire on studying habits and mental health with questions about students' social networks. We show that students treated with campus classes report lower levels of mental distress vis-à-vis students without in-person classes. These results hold even after controlling for pre-pandemic quality of life. After education for all students moved fully online towards the end of the fall semester, there was no difference between treated and untreated students with respect to self-reported mental health, suggesting that the difference observed earlier in the semester was indeed due to campus presence. Importantly, however, we find no relationship between mental health and gender or parental stratification status, meaning that our results are unlikely to be explained by differences in anxiety about the pandemic.

Instead, we turn our attention to the role played by social networks. Using the results from our questionnaire, we graph the classmate social network among students treated with campus education, and use the estimated network positions to compute network constraint for each individual student. By linking estimated network constraint with the same administrative data on parental income, we show that students with affluent parents have more open social networks, suggesting that these students can more efficiently bridge "holes" in the social structure.[2] We additionally show that female students with wealthy parents treated with campus

[1] Recent research underscores that both parents' stratification status impacts children's social networks, and by extension also the potential for intergenerational mobility, as opposed to previous theories focusing only on the role of fathers (Beller 2009; Mare 2015).

[2] An absence of a direct tie between two individuals in a social network is referred to as a *structural hole*.

classes spend more of their study time physically meeting peers, compared with other student groups. Consistent with our theory, this result suggests that campus communication acts as a catalyst for students stretching across network clusters, and that female students whose parents are at the highest end of the stratification hierarchy are the most efficient network "brokers". Considering that even treated students had limited access to campus, our results are consistent with recent findings that network brokerage is particularly useful under time constraints, and in our setting, time for socializing with peers is scarcely available (Mullainathan and Shafir 2013; Burt 2017; Opper and Burt 2021). Alternatively stated, limited access to peers in a blended learning setting raises transaction costs for social interactions, making it difficult for less connected students to interact with peers.

This paper makes a number of contributions. First, we add to the growing literature on the heterogeneous effects of the closing of educational facilities during the COVID-19 pandemic. In the United States, the adverse learning outcomes associated with the closing of K-12 schools was disproportionately skewed towards low-income students, whereas internet search frequency for online educational resources was higher in affluent areas (Chetty et al. 2020a; Bacher-Hicks et al. 2021). Similar conclusions can be reached when considering studies from European nations (Cacault et al. 2021; Grewenig et al. 2021). Another strain in the literature focuses on the heterogeneous consequences of lockdown on mental health. Two studies from the U.S., and Greece, respectively, show that self-reported anxiety among university students was higher among females than males (Kecojevic et al. 2020; Patsali et al. 2020). Similarly, students with access to a yard or garden experienced lower levels of anxiety during the lockdown in France (Husky et al. 2020). We add to this literature by showing that having some campus classes is beneficial for mental health compared to full online mode, however, distance education was not associated with an overall deterioration of grade outcomes.

Second, we contribute to the literature on heterogeneity in social networks depending on socioeconomic characteristics. Previous research has shown that having wealthy parents is associated with relatively higher shares of acquaintances in individual social networks, concomitant with a higher rate of socializing with friends, but lower rates of socializing with relatives (Andersson 2018). Among university students, those coming from privileged backgrounds are more likely to communicate and interact with faculty (Kim and Sax 2009). In terms of gender, while it has not been established that women have broader networks overall, being embedded in networks that are diverse, for instance with respect to the gender and socioeconomic status of group members, is more likely to benefit women than men (Lutter 2015; Mengel 2020).

We add to the literature on heterogeneity in social networks in two ways: First, by showing that parental socioeconomic status is one important channel in explaining the heterogeneity in network constraint observed among students, suggesting that there is significant intergenerational transmission of network status. Second, we show that network constraint is an important mechanism behind the observed spillover effects of campus education in a blended learning setting, and that network constraint is lower among females and students with affluent parents.

Finally, we contribute to the broader literature on social networks and their role in shaping economic outcomes. Previous research has shown that network centrality is positively associated with academic performance (Calvó-Armengol et al. 2009; De Paola et al. 2019), that

individuals with more open networks have higher savings rates and shorter unemployment spells (Breza and Chandrasekhar 2019; Cingano and Rosolia 2012). In addition, individuals with more open networks are more likely to be elected into political office (Cruz et al. 2017), and CEOs with higher network centrality are more successful in finalizing merger and acquisition deals (El-Khatib et al. 2015). We add to this literature by showing that network structure is an important mechanism in explaining variations in academic outcomes when education is partially online.

The rest of the paper is structured as follows. Section II provides additional details on the experiment setting. Section III describes the data. Section IV presents the results, while Section V discusses potential mechanisms. Section VI concludes.

II. Setting

II.A. Background

We study a quasi-natural experiment conducted at the Faculty of Engineering at Lund University, Sweden, during the fall semester of 2020. Starting March 17, 2020, higher education facilities were "strongly recommended" by the Public Health Agency of Sweden to switch to online-based teaching in order to mitigate the spread of COVID-19. Hence, the reminder of the Spring semester was fully online at all public universities in Sweden, including Lund. The recommendation about online education lapsed on June 15, 2020, but was reinstated in early November.

Since public universities in Sweden are their own government agencies, they have significant leeway in interpreting regulations and recommendations from other government agencies. Following the summer holidays, teachers[3] at the Faculty of Engineering could decide for themselves whether to continue with online-based education, or return to campus. The only prerequisite for in-person teaching was that student groups could not be larger than 50 individuals, and that the number of seats in lecture halls were required to be twice the number of students in class, in order to ensure social distancing. However, as the number of students enrolled in most of the Faculty's undergraduate programs exceeds 50 by some margin, the former requirement meant that the bulk of courses were given online, in order for instructors to avoid the extra teaching burden associated with splitting students into two or more lecture groups.[4] Whether a course was to be held online or at campus was unknown to students until around one week before the start of the fall semester.

Swedish universities follow the standardized system for comparing academic credits across the European Union, the so-called *European Credit Transfer System* (ECTS). One academic year is equal to 60 ECTS credits, which corresponds to 1600 hours of full-time studies. There are two semesters in an academic year (Fall and Spring), with 30 ECTS worth of coursework in each. In addition, each semester is divided into two terms; for the fall semester, the terms are September–October, and November–December. Most courses run for one term only, which

[3]Throughout this paper, "teacher", or "instructor", refers to the person responsible for giving lectures and planning exercise sessions, regardless of academic rank.

[4]Teaching credits provided to instructors were the same regardless of teaching mode.

places a relatively high emphasis on final exams. However, courses running for an entire semester typically have mid-term exams, to avoid examining four months of coursework in a single day.

II.B. The Experiment

We consider the academic results of second-year students enrolled in four separate undergraduate engineering programs: Engineering Mathematics (EM), Engineering Physics (EP), Industrial Engineering (I), and Mechanical Engineering (M).[5] All are five-year degree programs leading to an M.S. in Engineering. All four are among the most competitive engineering programs in Sweden, with most of the students having scored grade A in all of their subjects in high school.[6] During the fall semester of 2020, students enrolled in EM and I took the same mandatory course in introductory microeconomic theory, which was given in person with students split into two lecture groups based on surnames. Hence, students in these two programs constitute the treatment group, as they had at least some campus classes during this time period. Students enrolled in EP and M had no campus classes whatsoever during the fall semester, meaning that these students constitute the control group.

We proceed by using data on student performance in two courses taken during the same semester as the microeconomics course: a mathematics course for students enrolled in EM and EP, as well as an introductory course in supply chain management for students in I and M. Consequently, this design creates one treated and one untreated student group for each course, where the treatment is access to campus teaching in other courses.[7]

Table 1 describes the course structure used in the experiment in some additional detail. We are interested in grade outcomes for the two online courses "Complex Analysis" for EM and EP students, and "Supply Chain Management" for students enrolled in I and M. As the name suggests, the course in complex analysis deals with complex-valued functions, with first-year courses in calculus and linear algebra being prerequisites. The course in supply chain management builds directly on a course in operations management taken by I and M students during their first year. During the mathematics course, EM students are treated with in-person classes in their parallel microeconomics course, whereas EP students have no access to campus whatsoever in their parallel courses in Dyanamics and Statistical Thermodynamics. Similarly, for the course in supply chain management, students in the I program have the online mathematical statistics course and the on-campus microeconomics course in parallel, whereas M students have no campus access in their parallel courses. If it is the case that there are spillover effects from campus teaching, we would expect the treated students to perform relatively better in the online courses than their non-treated peers.

Our experiment design has numerous advantages. First, there is no selection into courses, since all courses in the first and second years are mandatory. Second, admission to the four

[5] Table A.1 of Online Appendix A provides the first-year course structure for each program.

[6] Admission to Swedish universities is based solely on either high school grades or on the standardized entrance exams (equivalent to the SAT in the United States), and not on interviews or covers letters. Around two-thirds of applicants are admitted through high school grades, and the reminder via the standardized admissions test.

[7] Although students without in-person classes were not explicitly banned from university premises, students were advised not to visit campus unless they had scheduled classes.

engineering programs is almost equally difficult, so there should not be any program fixed effects in terms of academic ability. Finally, the design is not subject to any teacher fixed effects, because the instructor for each of the two spillover courses is the same individual, regardless of student group. Thus, although the decision by individual teachers whether to remain online or to switch to campus teaching is plausibly affected by unobservable teacher characteristics, the quality of the online course will be the same regardless of whether the student group received campus treatment in their parallel course or not. A potential drawback with our experiment design is that the students have different parallel courses. Thus, a potential concern is that some students will have easier parallel courses impacting their performance in the courses of interest for the analysis. More specifically, EM students take a course in mathematical statistics parallel to the Complex Analysis course, which could benefit their performance vis-à-vis the EP students, which have only physics courses in parallel. Section IV.A presents placebo tests for the previous cohort, when all education was on campus, and Section IV.B presents further robustness checks.

III. Data

III.A. Data Overview

At the Faculty of Engineering, passing grades are given by 3, 4, and 5, with 5 being the top grade. The grading scale is absolute, meaning that the cutoff level for each grade is determined before the start of the course, and is not affected by the relative performance of students.

In order to isolate the effect of campus access on academic outcomes, we use a set of control variables. Since we are particularly interested in the role played by socioeconomic factors, we use administrative data from the Swedish Tax Authority to calculate the taxable income of each parent for the year 2019.[8] We then calculate the average of each parent's income and use this as a proxy for the student's socioeconomic background. Overall, the parents of our sampled students are considerably wealthier than the median in Sweden, with the median parental income at SEK 567,350.[9] However, there is still significant variation in terms of parental income among our sampled students. Figure A.1 of Online Appendix A illustrates the box-and-whisker diagrams of average parental income for students in each engineering program, measured in SEK.

We employ a number of additional student-specific controls, namely age and the median income of the student's home municipality. Online Appendix B presents the data sources for all variables used in the empirical analysis, and provides additional definitions.

[8] Sweden has no joint family taxation.

[9] The median income for individuals aged 20–64 was SEK 337,400 in 2019 (*Data source*: Swedish Statistics Agency.). It is well-known that top-ranked universities tend to disproportionally enroll students from high-income families (Chetty et al. 2020b). However, since there are no tuition fees in Sweden, it is likely that this gap is smaller than for many other universities of similar standing.

III.B. Survey Construction

To examine mechanisms and to perform additional robustness checks, we survey students from the four engineering programs immediately after the end of the fall semester, by constructing an online survey consisting of 21 questions. We emailed an online link to each of the 333 students enrolled in EM, EP, I, and M, followed by two reminder emails after 48 and 96 hours, respectively. Each respondent was awarded a gift card worth SEK 50. In total, we received 151 responses, corresponding to a response rate of 45 percent. The survey questions fall into three categories: socioeconomy, opinions about coursework in the fall semester, and questions about mental health and social networks.

In the final question, we ask students to name up to five of their closest classmates. On average, students participating in the survey named 4.07 friends. Here, we encounter a frequent problem in social network analysis, namely tie non-response. Because ties represent social interactions between individuals, estimates of network strength are likely to be biased even with relatively low rates of non-response (Kossinets 2006; Smith and Moody 2013). To correct for non-response, we use that during the microeconomics course for EM and I, students self-selected into groups of 3–4 classmates when writing a mandatory group assignment. Of the 36 groups in total, there were 15 three-person groups and 21 four-person groups. This allows us to impute up to three friends for the non-responding students. A major advantage of this procedure is that it enables us to fully eliminate non-response among the EM and I students, as well as to link students' network positions to parental income. We can also show that the share of non-responding students was random between groups.[10] However, since we are only able to perform the imputation for EM and I, we drop this question for the remaining students.

Table A.2 of Online Appendix A presents a balance test, comparing the universe of students in our dataset with our survey sample with respect to the share of treated (I and EM) students, the share of female students, and the median income of the students' home municipalities. There are no statistically significant differences between the survey sample and the full sample with respect to these student characteristics, suggesting that the survey sample is likely to be a representative cross-section of the student population.

III.C. Operationalization of Parental Stratification Status

One of our research questions is related to whether there is variation in academic and non-academic outcomes depending on parental socioeconomic status. However, it can be difficult for students participating in the survey to precisely estimate their parents' income. To tackle this issue, we construct a socioeconomic status index based on four questions in the survey,

[10] We construct a contingency table with two frequency columns (the number of group members participating in the survey, and the number of group members for which the friendship connections were imputed), and 36 rows, corresponding to the number of student groups. The test statistic for testing the null hypothesis that non-response is random between groups is $\sum_{i=1}^{r}\sum_{j=1}^{c}\frac{[O_{i,j}-E_{i,j}]^2}{E_{i,j}} \overset{\text{asy.}}{\sim} \chi^2(35)$, where for outcomes i and j, $O_{i,j}$ is the observed frequency, and $E_{i.j}$ is the expected frequency if non-response is random. The observed chi-square score is 33.10, which is equivalent to a p-value of 0.56. A concern would otherwise be that, for instance, female-only groups were more likely to have a larger share of members participating in the survey.

each asking the respondent to state, respectively: (i) in which municipality he or she lived just before starting university, (ii) in what type of dwelling he or she mainly lived during childhood, (iii) whether any of their parents has a college or university degree, and whether (iv) anyone of their parents has been the CEO or a board member of a publicly listed company during the lifetime of the respondent.

For each of the above questions, we proceed by assigning a numerical value to each response.[11] Respondents from more affluent municipalities receive a higher score, where the score is proportional to the median disposable income of the municipality. Similarly, respondents who grew up in a house receive a higher score than those living in rental apartments during their childhood, as do respondents for which both parents have a college degree. Finally, respondents where at least one parent has been the CEO or a board member of a publicly listed company receive the score 4, compared to 1 for those without a CEO or board member parent. The latter variable is the one most likely to capture those with the highest-earning parents, as the data presented in Table A.4 of Online Appendix A shows that out of the 20 parents with the highest reported taxable income in our sample, 16 had at least one CEO position or board assignment during the lifetime of their children, and 15 of these had at least one current assignment. Of the top 7 parents, all had at least one CEO or board assignment.

To construct the index, we denote questions by $j = 1, \ldots, 4$, and sum the numerical scores obtained in each question to form the *Socioeconomic status index* for student $i = 1, \ldots, 151$ as

$$\text{Socioeconomic status index}_i = \sum_{j=1}^{4} \text{Score}_{ij} \tag{1}$$

To facilitate interpretation, we standardize the index so that its sample mean is equal to zero and its sample standard deviation is equal to unity. Hence, the higher the z-score associated with the respondent's socioeconomic status index, the higher is the socioeconomic background of the respondent.

Online Appendix C provides additional details on the structure of the questionnaire, as well as the exact wording of the questions and answers available to respondents. Online Appendix D presents the full results for each question in the survey.

III.D. Summary Statistics

Table A.5 of Online Appendix A presents the summary statistics for the 2020 cohort. We have previously showed that the parents of our sampled students are wealthier than average. The proportion of females is around 28%. There is less variation in age, the average age being around 21. Table A.6 of Online Appendix A gives the same summary statistics for the previous (2019) cohort, the academic outcomes of which are also utilized in the empirical analysis. Apart from a somewhat higher share of women in the 2019 cohort, there are no major differences between the two cohorts in terms of the variables of interest.

[11] Table A.3 of Online Appendix A presents the contribution of each question to the total index value.

IV. Spillover Effects of Campus Access on Online Coursework

In this section, we examine the grade outcomes of students depending on campus treatment in parallel courses. As a placebo test, we evaluate the grade effects in the current cohort compared to last year's, when all instruction was in-person, as well as preforming a number of additional robustness tests.

IV.A. Main Results: Blended Learning versus Full Online

1. Estimates for the Current Cohort

Denote by $y_i \in \{3, 4, 5\}$ the grade obtained by student i in either the mathematics course (EM and EP) or supply chain management course (I and M). Both courses build heavily on first-year courses: Complex Analysis on first-year mathematics courses in calculus and linear algebra, and Supply Chain Management on the first-year course in operations management. Consequently, for EM and EP students, we let Δy_i be the difference between the logarithm of y_i and the logarithm of first-year mathematics GPA, and for I and M students, Δy_i is the difference between the logarithm of y_i and the log grade in the first-year operations management course. Finally, we standardize Δy_i so that its mean is equal to zero and its standard deviation is equal to unity.

In this paper, we are particularly interested in the role played by parental position in the stratification hierarchy, and whether the effects were particularly strong for male or female students. Thus, we estimate

$$\begin{aligned}\Delta y_i = {} & \alpha_0 + \beta_1 \text{Treated}_i + \beta_2 \text{Inc}_i + \beta_3 \text{Gender}_i + \beta_4 \left(\text{Treated} \times \text{Inc}\right)_i \\ & + \beta_5 \left(\text{Treated} \times \text{Gender}\right)_i + \beta_6 \left(\text{Gender} \times \text{Inc}\right)_i + \beta_7 \left(\text{Treated} \times \text{Gender} \times \text{Inc}\right)_i \\ & + \boldsymbol{\gamma}' \boldsymbol{X}_i + \varepsilon_i \end{aligned} \tag{2}$$

where $\text{Treated}_i \in \{0, 1\}$ denotes whether the student was treated with the parallel campus course or not, Inc_i denotes the average annual income of parents, $\text{Gender}_i \in \{0, 1\}$ is zero for males and unity for females, $\boldsymbol{X}_i$ is a vector of student-specific controls (age and median income of home municipality), and ε_i is an error term. Hence, if there are spillover effects of campus education, we would expect the coefficient estimate $\hat{\beta}_1$ of β_1 to be positive.

Table 2 presents the results. Since we cluster standard errors at the program level, using clustered standard errors when the number of clusters is low tends to over-reject the null hypothesis $\beta_j = 0$. In our case, there are only four clusters, so we adjust the standard errors with a wild cluster bootstrap (Cameron et al. 2008) with bootstrap weights drawn from the Webb distribution, which has been shown to work well in settings when the number of clusters is below 10 (Webb 2014; Cameron and Miller 2015). In Table 2, the p-values for the null hypothesis that the parameter corresponding to the coefficient estimate is equal to zero is in square brackets.[12] We see that the coefficient estimate for treatment with campus access in parallel courses,

[12] Note that inference in the wild cluster bootstrap is based on p-values only, with the bootstrap p-value being the share of the bootstrap statistics that are more extreme than the one from the original sample. Hence, the algorithm does not produce any standard errors.

$\hat{\beta}_1$ is positive, although statistically insignificant. When using the full model as described by Equation (2), corresponding to Columns (2)–(3) in Table 2, we see that the only statistically significant coefficient is the triple interaction term $\hat{\beta}_7$ between treatment, female gender and average parental income. The triple interaction coefficient estimate is positive, suggesting that the effect is increasing linearly with income. If average household income increases by SEK 100,000, which is approximately equal to USD 12,000 (so that the average income of each parent increases by SEK 50,000), academic performance of treated female students increases by 0.035 standard deviations.[13] The triple interaction term is significant at the 5% level, whereas the remaining coefficients are all statistically insignificant.

Although the effect is relatively small when parental income is included in the model linearly, the magnitude of the triple interaction term increases when we instead consider a binary variable taking the value unity when parental income is in the highest decile (top 10% of the empirical income distribution), and zero else. The results in Table A.7 of Online Appendix A suggest that for females treated with parallel campus classes, grades are around 0.5 standard deviations higher for those with parents above the top 10% income percentile, compared to those whose parents are at the bottom 90%. Again, the triple interaction term is significant at the 5% level.

It can be challenging to interpret the coefficients when there are three-way interactions. To facilitate interpretation, we plot the estimated coefficients from the above regression models for each of the four subgroups, varying average parental income between 0 and $3,000,000$.[14] Figure 1 presents this plot, with the standardized grade change between years 1 and 2 on the vertical axis. For for females in the blended learning group, grades improved with the average income of parents. Both for males, and for women in the online group, parental income had relatively minor effects on grades. Section V explores various potential mechanisms behind these findings.

2. Estimates for the Previous Cohort

It is important to establish that there were no spillover effects the last time the courses were given, namely during the fall semester of 2019, when all instruction was on campus. We thus re-estimate (2) using the values for the previous cohort of students. Since it did not exist any "treated" or "untreated" students in 2019, we check whether females from affluent backgrounds benefited from full campus education before the pandemic. Figure A.2 of Online Appendix A plots the estimated coefficients. There are no indications that socioeconomy impacted the grade change between years 1 and 2 when education was fully on-campus.

Hence, we may summarize our main results as follows. Female students with affluent parents benefited from campus access in 2020, when EM and I students were treated with hybrid education and EP and M students had online teaching only. However, there was no socioeconomic heterogeneity with respect to grade outcomes in the 2019 cohort, when all education was given on campus. This result implies that it is the hybrid setting under blended learning that causes the socioeconomic distortions, not campus education per se.

[13] We use the standardized values from Column (3), and since parental income is measured in tens of thousands of SEK, $0.007 \times 5 = 0.035$.

[14] Note that SEK 3,500,000 is slightly above the sample maximum for average parental income.

IV.B. Robustness Checks

In this subsection, we run a number of robustness checks to address possible concerns with our identification strategy.

1. Balance Tests for Treated and Untreated Students

One drawback of our approach is that, while it is random which engineering programs had parallel online courses, students do not randomly select into programs. Thus, it would be problematic if treated and untreated students differed with respect to certain traits of interest for our analysis, for instance gender and socioeconomic background. Table A.8 of Online Appendix A provides the results of the balance test comparing treated and non-treated students with respect to gender, socioeconomic status of parents, age, and the grade for the first mathematics course (Calculus in One Variable), which is mandatory for all engineering students regardless of track. We note that there are no statistically significant differences between these characteristics between treated and untreated students. Consequently, the non-random selection into engineering programs does not pose a threat to the causal interpretation of our results.

2. Heterogeneous Effects by Initial Achievement

Another concern related to our identification strategy is the possibility of high socioeconomic-status females having had higher test scores at baseline. To exclude this possibility, we re-estimate equation (2) replacing the left-hand side with the standardized log grades in the first-year courses in mathematics and operations management, respectively. The results in Table A.9 of Online Appendix A show that all coefficients are statistically insignificant. Additionally, it is reassuring that the treatment variable is insignificant, considering that these first-year courses took place before the pandemic, and hence, there could not have been any "treated" and "untreated" students at that time.

3. Difficulty of Parallel Courses

Another concern of our study relates to how students allocate time between courses. A feature of the design is that students take up to three courses in parallel, and parallel courses differ between programs, and hence, between treated and untreated students. Although students take 30 ECTS credits per semester regardless of program, it could be the case that some course for one of the student groups is significantly more time-consuming than it "should" be. To pass the more difficult course, students would likely allocate time away from the spillover course, the grade outcomes of which are of interest in our study. In order to check whether this was the case, we use a question of our survey asking students to estimate the share of their total study time allocated to each course. For our courses of interest, it suffices that there is no significant difference in study times between students in different programs.

Table A.10 of Online Appendix A presents the results. The p-values for the difference in mean allocated time was 0.46 for Complex Analysis (that is, between EM and EP students), and 0.64 for Supply Chain Management (I and M). Hence, we find no evidence to suggest that some of the student groups found their parallel courses disproportionally time-consuming. Although we do not explicitly ask students how many hours per week they spend studying, several

university-run surveys have shown that a vast majority of students at the Faculty spend the recommended 40 hours per week, regardless of program (Lund University 2005; Holmström 2018).

4. Additional Robustness Checks

Besides time spent studying, it is important to ensure that both the treated and untreated students have similar opinions about how interesting their coursework is, since any heterogeneity could affect grades as well as non-grade outcomes. In our survey, we ask students to quantify on a scale from 1 to 5 how interesting each course is. By calculating the average score over treated and untreated students, with courses weighted by the number of ECTS credits, we find no difference between student groups in terms of how interesting students found their coursework.[15] This finding also suggests that having online education, as opposed to blended learning, did not affect motivation for this group of students. However, it is still possible that the 2019 course offerings of Complex Analysis and Supply Chain Management were an exception, and that EM and I students have higher rates of grade progression between basic and more advanced courses. This could be a concern for the causal interpretation of our findings, since it would be difficult to disentangle the effect of treatment with parallel hybrid classes during Fall 2020 from a general trend were EM and I students perform better at more advanced courses.

To exclude this possibility, we again estimate a model similar to (2), with the left-hand side replaced by the grade difference between two freshman mathematics courses for the 2019 and 2020 cohorts, with both cohorts taking the courses before the pandemic. The results of this regression are presented in Table A.11 of Online Appendix A. The coefficient for EM and I, that is, the student groups treated with hybrid classes during Fall 2020, is close to zero in magnitude and statistically insignificant. Moreover, none of the interaction terms with second-year treatment are significant.[16] Hence, there are no signs of a general grade progression trend favoring EM and I students at the Faculty of Engineering.

Finally, since we are interested in non-grade outcomes related to campus treatment, it could be problematic if students in either the treated or untreated group had higher reported quality of life before the pandemic. Question 15 of the survey asks students to rate their pre-pandemic quality of life between 1 and 5. We regress the results on the treatment variable and its interactions with gender and the socioeconomic status index. Table A.12 of Online Appendix A presents the results. We find no indication that treated students reported higher levels of pre-pandemic satisfaction. After including the control variable for self-estimated popularity, as well as the interactions with gender and parental socioeconomy, we find that the coefficient for treatment is close to zero, and that there is no heterogeneity with respect to gender and parental socioeconomic status.

[15] The average score was 3.52 for treated students with a sample standard deviation of 2.24 (53 observations), and 3.35 for untreated students with a sample standard deviation of 2.20 (96 observations). With $N = 149$, we have $t_{147} = 0.45$, which is equivalent to a p-value of mean differences equal to 0.65.

[16] Adding cohort fixed effects has only minor effects on the magnitude of the estimated coefficients, and is available on request.

V. Evidence on Mechanisms

So far, we have established that female students with affluent parents were significantly more likely to benefit from treatment with blended learning. We now consider the role played by network constraint in explaining this finding. We are also able to exclude other potential mechanisms, such as students with parents at the higher end of the stratification hierarchy being less affected by the pandemic.

V.A. Social Networks

1. Theoretical Framework

We first examine the role played by social networks and its relationship to socioeconomic status. We start by defining network constraint, which is our primary measure of network status. The idea is straightforward. If Alice spends all of her time with her friend Bob, a person meeting Alice also meets Bob. In this social network, the same information is shared across all members of the clique, and the network is said to suffer from a high level of network constraint. Alternatively, if Alice's friends do not know each other very well (even if they are classmates), she is more likely to access novel information when interacting with friends. This is because the first of Alice's friends gets her input from clique A, the second from clique B, and so on. Hence, Alice acts as a "broker" between networks. People linked to multiple social clusters have less information redundancy, and access to broader information, which should positively impact course performance.

We may formalize this line of thinking slightly. Let $\boldsymbol{A}$ be the square adjacency matrix associated with the social network. The elements $\{a_{ij}\}$ of $\boldsymbol{A}$ are equal to unity if individuals (vertices) i and j are connected, and zero otherwise. Here, "connected" means that there is an edge from vertex i to vertex j. Note that an individual cannot be connected to herself, implying that the graph associated with the adjacency matrix is loop-free, and $\text{tr}(\boldsymbol{A}) = 0$. Denoting i's ego network by V_i, define the tie strength p_{ij} between i and j as

$$p_{ij} = \frac{a_{ij} + a_{ji}}{\sum_{k \in V_i \setminus \{i\}} (a_{ik} + a_{ki})}$$

We then calculate the network constraint (Burt 1992) associated with vertex i as

$$C_i = \sum_{k \in V_i \setminus \{i\}} \left(p_{ij} + \sum_{k \in V_i \setminus \{i,j\}} p_{iq} p_{qj} \right)^2 \tag{3}$$

Note that network constraint is undefined for isolated vertices, that is, if the vertex is not an endpoint of any edge. In our case, this would arise if the respondent did not have any friends at all. The higher the value of C_i, the higher is the constraint on i's social network. That is, an individual with a low value of C_i has a relatively low level of network constraint, and thus a more open social network, allowing the person to access different network clusters. If female students from affluent backgrounds have lower levels of network constraint, it could explain our

findings on the role played by socioeconomic status for grade spillovers.[17]

2. Network Constraint and Social Stratification

Using the up to five connections named by students in Question 21 of our survey, we construct two separate adjacency matrices: one for students in EM, and one for students in I. This allows us to estimate network constraint for each of the 112 students in EM and I.[18] Figure 2 shows a detail of the social network for the I students.[19] As an example of heterogeneity in network constraint, individual 86 (in the top right corner) has a relatively closed network, whereas individual 73 (in the bottom of the figure) has a considerably more open network. Using our previous notation for network constraint, $C_{73} < C_{86}$. Proceeding from here, we calculate the network constraint multiplied by 100 for each student, and estimate

$$100 \times C_i = \alpha_0 + \beta_1 \text{Inc}_i + \beta_2 \text{Gender}_i + \beta_3 \left(\text{Gender} \times \text{Inc}\right)_i + \boldsymbol{\gamma}' \boldsymbol{X}_i + \varepsilon_i \tag{4}$$

In this specification, we divide annual parental income by 10,000 to avoid extremely small numbers for the coefficient estimates. Thus, the coefficient estimate $\hat{\beta}_1$ can be interpreted as the change in network constraint associated with a SEK 10,000 increase in average annual income of parents, keeping other variables constant. Figure A.3 of Online Appendix A illustrates visually the relationship between network constraint and average parental income, indicating that students with high-earning parents have lower values of network constraint, and thus, more open networks.

Table 3 presents the results when estimating (4). The results confirm that both higher parental income, as well as female gender, are significantly associated with lower network constraint. Similarly, the interaction between these two variables is also statistically significant. The latter finding suggest that high socioeconomic status females indeed have less constrained networks than females with less affluent parents. Augmenting the model to include controls in Column (6) of Table 3 barely changes the magnitude of the coefficient estimates.

3. Alternative Channels

In this subsection, we exclude two potential alternative reasons for the variation in network constraint among students. First, students hailing from affluent municipalities may be influenced by their surroundings rather than their own, or their parents' social status. It could also be the case that network constraint is lower for students from larger cities, as these typically have more sports clubs, religious organizations, and so on, thus contributing to network openness. Table

[17] We use second-year students, and previous research has shown that in 4-year degree programs, there are only minor changes in network centrality after nine months (Overgoor et al. 2020). Additionally, almost all students participate in the Faculty's orientation weeks, and peer groups formed during orientation weeks tend to be strong predictors of friendship over time (Thiemann 2022). Given this, it is unlikely that the timing between the courses of interest and our survey had any impact on the friendship networks among our sampled students.

[18] We drop a total of 15 students who transferred to Lund from other universities after the first year, or who did not actively participate in the first year.

[19] This is a so-called *directed graph*, because A naming B as one of her top 5 friends need not imply that B will name A as a top 5 friend. Consequently, there are friendship "directions".

A.13 of Online Appendix A regresses, in turn, network constraint on the median income of the home municipality of each student, and the population of the home municipality. Neither the coefficient for municipality income, nor the coefficient for municipality population, are statistically significant. In addition, the explanatory power is near-zero for both specifications, further suggesting that parental income is likely to be a more plausible channel behind our findings.

V.B. Non-Grade Outcomes and Details About Channels

In this section, we use the results from our survey to examine non-grade outcomes of treatment with campus classes. These results help us exclude the possibility of our findings being driven by heterogeneity in attitudes towards the pandemic. Finally, we use the results of the survey to further investigate the mechanisms relating network constraint to realized peer interactions.

1. Non-Grade Outcomes

We use the survey results to examine whether there is heterogeneity in non-grade outcome responses depending on campus treatment. First, we ask the respondent to quantify, from 1 to 5, how negatively he or she was affected by the pandemic. The latter question focuses on the non-medical consequences of the pandemic, for instance increased boredom due to lack of social gatherings. Since education was fully online from November onward (including for treated students), we use subquestions for September–October, and November–December.

Columns (1)–(3) of Table 4 present the results for September–October. Column (2) includes controls for pre-pandemic life satisfaction and self-estimated popularity.[20] Finally, Column (3) adds interactions with the standardized socioeconomic status index and female gender. Not surprisingly, students treated with campus education report lower levels of adverse mental effects amid the pandemic; around 0.35 units lower on the 1–5 scale. There was no heterogeneity with respect to gender or socioeconomic status. Columns (1)–(3) of Table A.14 of Online Appendix A show the results when the values for the second half of the semester are regressed on the same set of variables. Here, the coefficient for treatment is statistically insignificant and close to zero in magnitude. This suggests that there was no difference in self-reported mental health between student groups in the period November–December, when education for all four programs was online. This finding strongly suggests that the observed differences in self-reported mental health between treated and untreated students was indeed due to campus presence.

Columns (4)–(6) of Table 4 present the results when asking students to quantify on a scale from 1 to 5 whether he or she was worried about getting infected with the coronavirus during the first half of the semester, when some education was on campus for EM and I. The coefficient for treatment is positive and statistically significant when controlling for pre-pandemic life satisfaction and popularity in Column (5), however, it is insignificant in the full model as specified in (6). The interaction between campus treatment, female gender and the socioeconomic status index is significant and positive. This provides some support to our theory about network constraint: female students with wealthy parents seem to be aware that they are indirectly exposed to more virus transmission chains, because the openness of their social networks implies that

[20] More popular students are more likely to be involved in student life. Due to the restrictions imposed during the pandemic, this may disproportionally affect the well-being of this group of students.

their on-campus contacts are more likely to come from different social cliques. Finally, Columns (4)–(6) of Table A.14 of Online Appendix A shows that there was no variation in fear of being infected in the second half of the semester, when all education was online.

2. Time Spent on Campus Outside Classes

So far, we have concluded that differences in non-grade outcomes surrounding the pandemic cannot explain why female students with affluent parents benefited from campus access in parallel courses. Instead, this group of students has lower network constraint, which should reduce information redundancy and facilitate communication across social clusters during in-person classes. With this said, it remains a mystery why there was no income or gender effects in grade spillovers in 2019, when all education was on campus. It seems unlikely that female students with affluent parents in the previous cohort had more closed networks than students in the current cohort. An alternative explanation relates to time scarcity: a sizable proportion of learning takes place outside lectures, and if students with high levels of social capital are better at utilizing limited campus time to plan group learning activities with peers, it would benefit their grade performance. Previous research has failed to find an association between socioeconomic status and study behaviors when education is fully on campus (Delaney et al. 2013). However, given that access to peers is limited under blended learning, these results may need to be reconsidered.

Elaborating, we ask respondents to quantify from 1 to 5 how often they studied together with their classmates for the spillover course, and whether those meetings took place in-person or online. Here, 1 means that the respondent never studied together with classmates in-person, whereas 5 means that all study sessions were in person. Since we believe that campus study time may vary both with respect to treatment, gender and socioeconomic characteristics, we interact treatment both with the indicator for female gender, as well as with the standardized socioeconomic status index. The results reported in Table 5 show that the triple interaction coefficient between treatment, female gender, and socioeconomic status is statistically significant with a p-value of 0.056. Additionally, both the main effect coefficient of the socioeconomic status index and the interaction coefficient between treatment and female gender are significant, the latter being negative but smaller in magnitude than the triple interaction coefficient. Hence, treated females with wealthy parents spend a relatively larger share of their study time meeting classmates in-person compared to treated females with less affluent parents.

We may also choose to vary only treatment status: Comparing two female students with relatively wealthy parents (say, a z-score of 2), who differ only in terms of treatment, the student treated with with blended learning will spend around 0.4 units more time studying on campus for the spillover course, based on the estimated coefficients in Table 5. Similarly, untreated male students with relatively poor parents are the group spending the least time studying with classmates on campus, which is consistent with our results on network constraint.

Finally, circumstances surrounding the pandemic may have influenced the decision to study with friends. Although Sweden did not impose a full lockdown during the pandemic, there were limited opening hours in coffee shops and restaurants, and student life was largely restricted. Since physical lectures provided one of few opportunities to meet with friends, students may

have studied more in groups than they would have done otherwise. Again, since students with high level of social capital are the ones with the greatest preferences for socializing with peers, it is plausible that members of this group substituted student activities with group study sessions to a large extent. This may have influenced the grades of female students with wealthy parents through the significant benefits of small-group learning.

VI. Concluding Remarks

It remains to be seen whether the pandemic will profoundly change academic education. Globally, the supply of online courses has been increasing for several years, and multiple universities have played with the idea of replacing on-campus teaching with at least some degree of blended education, or to completely outsource courses to other universities through Massive Open Online Courses (Styles 2020).

In this paper, we show that grade outcomes under blended learning are heavily dependent both on gender and socioeconomic characteristics. In particular, partial access to campus under blended learning leads to positive grade spillovers for the online courses taken in parallel, but only for female students. The effect is increasing linearly with parental income. Conversely, both the traditional in-person setting as well as full online classes, do not cause socioeconomic distortions. We show that the relative winners of blended learning, namely female students with affluent parents, have broader social networks enabling them to take advantage of scarcely available campus time to interact with peers. However, in terms of mental health, blended learning is still preferred to full online teaching. We show that partial campus access mitigates the pandemic-related adverse effects on mental health for all students, regardless of gender or socioeconomic background.

Our findings have broader implications. For decades, intergenerational mobility has been higher for individuals with college education, suggesting that the relative benefits of higher education are skewed towards those least likely to attend college (Torche 2011). All of these studies have assumed that access to peers is fully available, with students being on campus virtually around-the-clock. If face-to-face meetings with peers become scarcely available under blended learning, these results may need to be reconsidered.

In the paper, we have used data from one of Sweden's most competitive engineering programs. Although there is significant variation in terms of parental incomes among our sampled students, most of the students are from high socioeconomic status backgrounds. Thus, additional research is required to examine the consequences of hybrid education among other student groups, for instance those from lower socioeconomic status backgrounds, or in developing countries.

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Table 1
Course structure, fall semester, second year

Course name	Subject classification	Sep–Oct	Nov–Dec
Microeconomic Theory (C, 6)	Economics	EM, I	
Complex Analysis (O, 7)	Mathematics	EM, EP	
Supply Chain Management (O, 5)	Operations Management	I	M
Mathematical Statistics (O, 9)	Mathematics	EM, I	EM, I
Systems and Transforms (O, 7)	Mathematics		EM, EP
Dynamics (O, 6)	Physics	EP	
Mechanics (O, 15)	Physics	M	M
Solid Mechanics (O, 4.5)	Physics		EP
Statistical Thermodynamics (O, 6)	Physics	EP	EP
Thermodynamics and Fluid Mechanics (O, 11)	Physics	M	M
Marketing and Globalization (O, 4.5)	Business Administration		I
Programming (O, 4.5)	Computer Science		I

Note. Mandatory courses during the fall semester for second-year students enrolled in Engineering Mathematics (EM), Engineering Physics (EP), Industrial Engineering (I), and Mechanical Engineering (M). Spillover courses underlined. In brackets: "C" and "O" denote campus and online courses, respectively, whereas the number refers to number of ECTS credits awarded for passing the course in question.

Table 2
Main results

	(1)	(2)	(3)
Treated	0.223	0.189	0.159
	[0.552]	[0.752]	[0.768]
Average parental income		−0.001	−0.001
(SEK, 10,000s)		[0.756]	[0.232]
Female gender		−0.109	−0.101
		[0.776]	[0.742]
Treated		0.000	0.000
× Average parental income		[0.946]	[0.682]
Treated		−0.383	−0.383
× Female gender		[0.510]	[0.410]
Average parental income		−0.001	−0.001
× Female gender		[0.728]	[0.702]
Treated × Female gender		0.007**	0.007**
× Average parental income		[0.040]	[0.036]
Student characteristic controls	No	No	Yes
Observations	321	321	319
Mean dep. var.	0.000	0.000	0.000
R^2	0.012	0.028	0.035

Note. Dependent variable: Change in achieved grade between spillover course and equivalent first-year course. A constant is included in all regressions. Columns (1), (2), (4), and (5): No controls. Columns (3) and (6): Controls for age and median income of home municipality. Standard errors clustered by program in brackets, with Columns (4)–(6) reporting wild cluster bootstrap-adjusted p-values in square brackets, computed using 500 replications and bootstrap weights drawn from the Webb distribution. ** denotes significance at the 5% level.

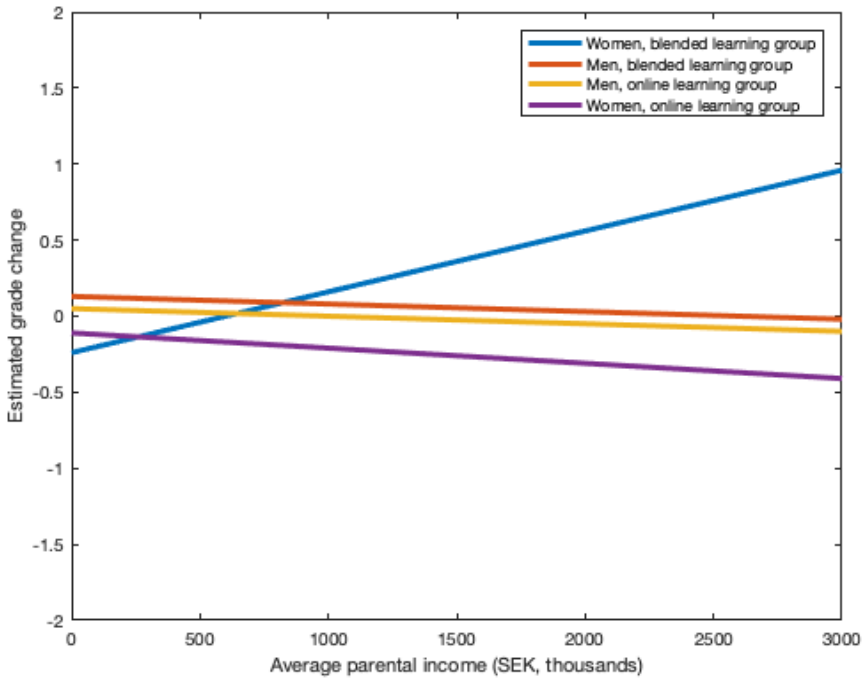


Figure 1: Plot of the estimated regression coefficients from the main specification. The estimated grade change on the vertical axis refers to the difference (in standard deviations) between the second year course and the corresponding first year course.

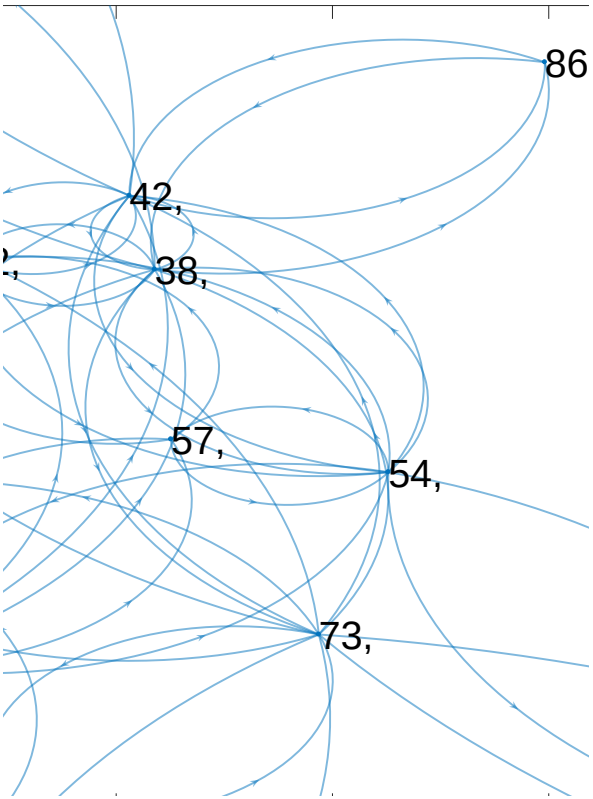


Figure 2: Detail of the estimated social network for the I students.

Table 3
Network constraint, gender, and parental income

	(1)	(2)	(3)	(4)	(5)	(6)
Average parental income (SEK, 10,000s)	−0.098***		−0.100***		−0.073*	−0.094**
	(0.037)		(0.029)		(0.042)	(0.047)
Female gender		−11.551***	−11.171***		−12.073***	−12.660***
		(4.061)	(3.894)		(3.784)	(4.016)
Average parental income (SEK, 10,000s)				−0.265***	−0.207**	−0.224**
× Female gender				(0.077)	(0.087)	(0.089)
Student characteristic controls	No	No	No	No	No	Yes
Observations	112	112	112	112	112	112
Mean dep. var.	58.81	58.81	58.81	58.81	58.81	58.81
R^2	0.045	0.061	0.109	0.048	0.132	0.145

Note. Dependent variable: Network constraint (×100). A constant is included in all regressions. Columns (1)–(5): No controls. Column (6): Controls for age and median income of home municipality. Heteroscedasticity-robust standard errors in brackets. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table 4
Non-grade outcomes, Sep–Oct

	Adverse mental effects			Afraid of contracting virus		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	−0.353**	−0.471**	−0.526**	−0.026	0.125**	−0.019
	[0.050]	[0.048]	[0.050]	[0.698]	[0.048]	[0.902]
Socioeconomic status index			−0.004			−0.027
			[0.938]			[0.670]
Female gender			0.361			−0.321
			[0.154]			[0.298]
Treated			0.096			0.128
× Socioeconomic status index			[0.670]			[0.730]
Treated			−0.091			0.147
× Female gender			[0.726]			[0.350]
Socioeconomic status index			−0.294			−0.141
× Female gender			[0.698]			[0.766]
Treated × Female gender			0.344			0.537*
× Socioeconomic status index			[0.598]			[0.070]
Student characteristic controls	No	Yes	Yes	No	Yes	Yes
Observations	150	135	135	150	135	135
Mean dep. var.	2.81	2.81	2.81	1.96	1.96	1.96
R^2	0.024	0.045	0.083	0.000	0.058	0.100

Note. Dependent variable: "How was your mental health affected by the pandemic?", and "How worried were you about contracting COVID-19?", respectively. Both variables are measured on a scale from 1 to 5, for the period September–October. A constant is included in all regressions. Columns (1) and (4): No controls. Columns (2)–(3) and (5)–(6): Controls for pre-pandemic life satisfaction and self-estimated popularity. *P*-values are in square brackets and computed using wild cluster bootstrap with 500 replications, with bootstrap weights drawn from the Webb distribution. * and ** denote significance at the 10% and 5% level, respectively.

Table 5
Self-estimated study time on campus

	(1)	(2)	(3)
Treated	0.068	−0.127	−0.006
	[0.772]	[0.668]	[0.970]
Socioeconomic status index			0.309*
			[0.072]
Female gender			0.793
			[0.276]
Treated			−0.435
× Socioeconomic status index			[0.196]
Treated			−0.600*
× Female gender			[0.098]
Socioeconomic status index			−0.834
× Female gender			[0.330]
Treated × Female gender			0.927*
× Socioeconomic status index			[0.056]
Student characteristic controls	No	Yes	Yes
Observations	151	135	135
Mean dep. var.	2.18	2.18	2.18
R^2	0.001	0.122	0.229

Note. Dependent variable: "On a scale from 1 to 5, where 1 is almost never, and 5 is daily, how often did you study together with your classmates when studying Complex Analysis or Supply Chain Management? Now, we mean physical meetings only." A constant is included in all regressions. Column (1): No controls. Columns (2)–(3): Controls for pre-pandemic life satisfaction, self-estimated popularity and estimated interest in the spillover course on a scale from 1 to 5. *P*-values are in square brackets and computed using wild cluster bootstrap with 500 replications, with bootstrap weights drawn from the Webb distribution. * denotes significance at the 10% level.

Online Appendix [Not for Publication]

A. Additional Empirical Results

Table A.1
Course structure, first year

Subject classification	EM	EP	I	M	Comments
Mathematics	28.5	28.5	27	27	Divided into 15 credits single variable calculus, 6 linear algebra, 6 multivariable calculus. An additional 1.5 credits multivariable calculus is added for EM and EP students.
Physics	13.5	22.5	18	6	
Programming	18	9	–	12	
Operations Management	–	–	9	9	
Business Administration	–	–	6	–	
Mechanical Engineering	–	–	–	6	
Overall first year	60	60	60	60	

Note. First-year course structure for the four engineering programs evaluated in the experiment. The numbers refer to ECTS (European Credit Transfer System) credits. Note that 1.5 ECTS credits is equal to one week (40 hours) of full-time studies. Overall, 60 ECTS corresponds to a total workload of 1600 hours per annum. All courses are mandatory for students in each program.

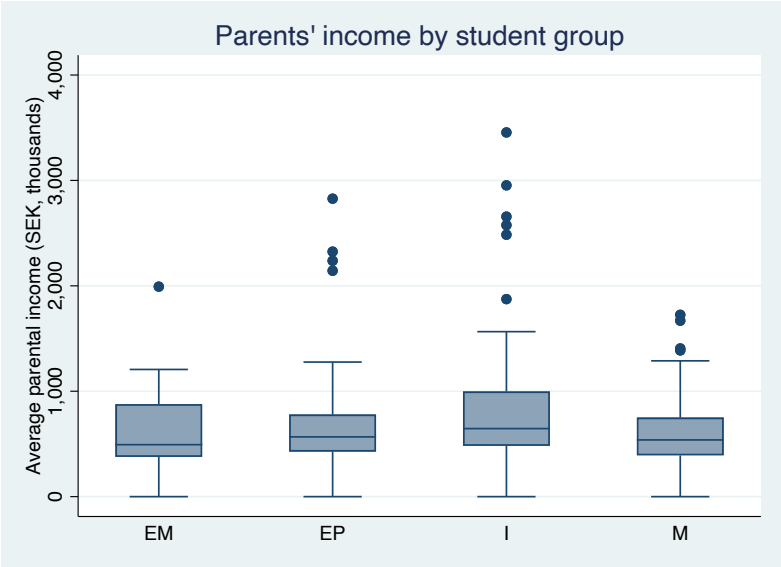


Figure A.1: Box-and-whisker diagram of average annual parental income in 2019 measured in thousands of SEK for each of the four student groups: Engineering Mathematics (EM), Engineering Physics (EP), Industrial Engineering (I), and Mechanical Engineering (M).

Table A.2
Balance tests

Student characteristic	Full sample	Survey sample	p-value for equality of proportions/means
Treated (%)	39.0	35.8	[0.50]
Female (%)	27.6	32.4	[0.28]
Median income of home municip. (SEK, thousands)	337.29	334.48	[0.49]

Note. Balance test comparing the full sample ($N = 333$) and the survey sample ($N = 151$) with respect to the share of treated students (EM and I), the share of females, as well as the median income of the student's home municipality.

Table A.3
Construction of the socioeconomic status index

Question number	Statement	Alternative	Score
2	In which municipality did you live at the time of your graduation from high school?	290 municipalities	Range: [2.78, 4.43] (the variable Median income of home municipality divided by 100)
3	When growing up, in what type of dwelling did you mainly live?	House	4
		Townhouse	3
		Housing cooperative	2
		Rental apartment	1
4	Does anyone of your parents have a college or university degree?	Yes, both	3
		Yes, but only one parent.	2
		No	1
5	Has anyone of your parents been the CEO or a board member of a publicly listed company?	Yes	4
		No	1
		Don't know	1

Note. The table shows, for each question, the contribution of each response to the socioeconomic status index for student i.

Table A.4
CEO position or board assignment of parents, 2019

Id	Gender	Taxable income (SEK, 2019)	Current assignments (2019)
1	M	5,654,100	1×CEO, 1×board member
2	W	5,241,400	4×CEO, 7×board member
3	M	4,982,400	4×board member
4	M	4,906,000	3×board member
5	M	4,432,000	2×CEO, 3×board member
6	M	3,987,700	1×CEO, 3×board member
7	M	3,857,500	1×board member
8	M	3,602,600	*no current or previous assignments in Sweden*
9	M	3,445,200	*no current or previous assignments in Sweden*
10	W	3,063,200	1×CEO, 2×board member
11	M	3,047,500	1×CEO, 15×board member
12	M	2,839,700	1×board member
13	M	2,652,000	*no current assignment*; until 2003: 1×board member
14	W	2,484,600	1×board member
15	M	2,453,100	7×board member
16	M	2,316,300	*no current or previous assignments in Sweden*
17	W	2,253,800	3×board member
18	M	2,231,500	*no current or previous assignments in Sweden*
19	M	2,022,500	2×board member
20	M	1,937,200	2×CEO, 2×board member

Note. The table shows, for the 20 highest earning parents, whether the individual is a CEO, or has any current (2019) board assignments. If the person had no CEO position or board assignments in 2019, the table shows the year of the last registered CEO position or board assignment.

Table A.5
Summary statistics

Main outcome variable	Mean	Std.dev.	Min	Max
Grade difference (Δy_i)	−0.043	0.689	−1.792	1.609
Second-year grade variables				
Grade, Supply Chain Management	3.989	0.588	3	5
Grade, Complex Analysis	3.989	0.796	3	5
First-year grade variables				
Grade, Operations Management	3.800	0.801	3	5
First-year mathematics GPA	4.002	0.707	3	5
Student-specific variables				
Average parental income (SEK)	687,007.6	465,717.6	0	3,455,200
Female gender	0.281	0.450	0	1
Age	21.059	1.096	19	28
Median income of home municip. (SEK) (thousands, SEK)	337.83	41.51	278.1	443.1

Note. The variable Grade difference is the difference between the second-year grade in Supply Chain Management and first-year grade in Operations Management for I and M students, and the difference between the grade in Complex Analysis and first-year mathematics GPA for EM and EP students.

Table A.6
Summary statistics: 2019 cohort

Main outcome variable	Mean	Std.dev.	Min	Max
Grade difference (Δy_i)	0.164	0.844	−5	2
Second-year grade variables				
Grade, Supply Chain Management	4.317	0.778	3	5
Grade, Complex Analysis	3.958	0.815	3	5
First-year grade variables				
Grade, Operations Management	3.963	0.795	3	5
First-year mathematics GPA	4.013	0.698	3	5
Student-specific variables				
Average parental income (SEK)	693,571.1	502,553.0	0	4,040,300
Female gender	0.374	0.485	0	1
Age	21.164	1.262	19	29
Median income of home municip. (thousands, SEK)	339.585	41.035	278.1	443.4

Note. The variable Grade difference is the difference between the second-year grade in Supply Chain Management and first-year grade in Operations Management for I and M students, and the difference between the grade in Complex Analysis and first-year mathematics GPA for EM and EP students.

Table A.7
Main results with high income dummy

	(1)	(2)	(3)
Treated	0.223	0.170	0.159
	[0.552]	[0.676]	[0.716]
High parental income		−0.263	−0.248
		[0.772]	[0.586]
Female gender		−0.193	−0.186
		[0.730]	[0.574]
Treated		0.158	0.167
× High parental income		[0.606]	[0.574]
Treated		0.045	0.040
× Female gender		[0.836]	[0.824]
High parental income		0.377	0.311
× Female gender		[0.218]	[0.206]
Treated × Female gender		0.454**	0.505**
× High parental income		[0.032]	[0.040]
Student characteristic controls	No	No	Yes
Observations	321	321	319
Mean dep. var.	0.000	0.000	0.000
R^2	0.001	0.012	0.035

Note. Dependent variable: Change in achieved grade between spillover course and equivalent first-year course, 2019 cohort. "High income" referents to students with parents above the top 10% income percentile. A constant is included in all regressions. Columns (1) and (2): No controls. Column (3): Controls for age and median income of home municipality. *P*-values are in square brackets and computed using wild cluster bootstrap with 500 replications, with bootstrap weights drawn from the Webb distribution.

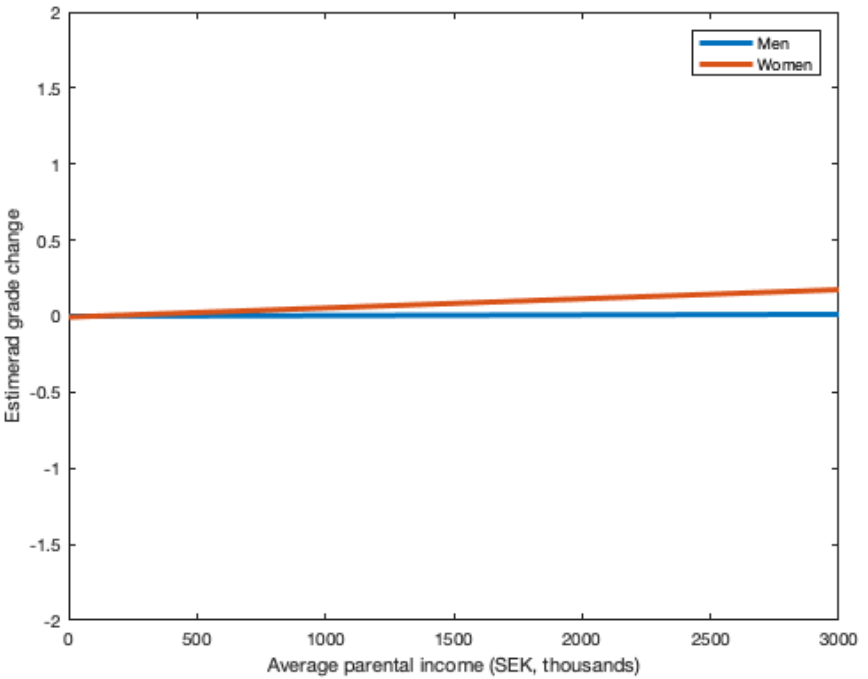


Figure A.2: Plot of the estimated regression coefficients from the main specification for the 2018 cohort, which took both their first and second year courses before the pandemic. The estimated grade change on the vertical axis refers to the difference (in standard deviations) between the second year course and the corresponding first year course.

Table A.8
Balance tests

Student characteristic	Untreated students	Treated students	p-value for equality of proportions/means
Female (%)	25.6	30.8	[0.31]
Median income of home municip. (SEK, thousands)	334.46	341.79	[0.12]
Average age	21.10	20.95	[0.22]
Average parental income (SEK, thousands)	550.40	629.85	[0.12]
Grade, first mathematics course	3.79	3.73	[0.53]

Note. Balance test comparing the full sample ($N = 333$) and the survey sample ($N = 151$) with respect to the share of treated students (EM and I), the share of females, as well as the median income of the student's home municipality.

Table A.9
Baseline grades

	(1)	(2)	(3)
Treated	0.585	0.548	0.598
	[0.346]	[0.340]	[0.306]
Average parental income		0.000	0.000
(SEK, 10,000s)		[0.298]	[0.184]
Female gender		−0.377	−0.087
		[0.152]	[0.610]
Treated		0.000	0.000
× Average parental income		[0.540]	[0.414]
Treated		0.603	0.335
× Female gender		[0.112]	[0.374]
Average parental income		0.00	0.000
× Female gender		[0.602]	[0.260]
Treated × Female gender		0.000	0.008
× Average parental income		[0.232]	[0.302]
Student characteristic controls	No	No	Yes
Observations	321	321	319
Mean dep. var.	0.000	0.000	0.000
R^2	0.080	0.095	0.133

Note. Dependent variable: Change in achieved grade between spillover course and equivalent first-year course, 2019 cohort. A constant is included in all regressions. Columns (1) and (2): No controls. Column (3): Controls for age and median income of home municipality. *P*-values are in square brackets and computed using wild cluster bootstrap with 500 replications, with bootstrap weights drawn from the Webb distribution.

Table A.10
Share of study time allocated to courses

Course	Treated groups	Untreated groups	F-stat.	N
Complex Analysis	50.417	54.400	–0.55	53
	(5.092)	(1.769)	[0.46]	
Supply Chain Management	23.048	22.018	0.22	98
	(1.465)	(1.511)	[0.64]	
N	54	97		

Note. Self-estimated study time (in percent) allocated to Complex Analysis (for EM and EM), and Supply Chain Management (for I and M) with standard errors in brackets. The column entitled "F-stat" refers to the F-statistic for the difference in means between the treated and untreated student groups, with p-values in square brackets.

Table A.11
Spillover effects, first year

	(1)	(2)
Treated	0.060	0.158
	[0.710]	[0.664]
Average parental income		0.004
(SEK, 10,000s)		[0.298]
Female gender		−0.018
		[0.824]
Treated		−0.002
× Average parental income		[0.418]
Treated		0.166
× Female gender		[0.584]
Average parental income		−0.001
× Female gender		[0.886]
Treated × Female gender		−0.001
× Average parental income		[0.584]
Student characteristic controls	No	Yes
Observations	464	462
Mean dep. var.	0.000	0.000
R^2	0.001	0.024

Note. Dependent variable: Change in achieved grade between second semester and first semester mathematics courses for both the 2019 and 2020 cohorts. A constant is included in all regressions. Column (1): No controls. Column (2): Controls for age and median income of home municipality. *P*-values are in square brackets and computed using wild cluster bootstrap with 500 replications, with bootstrap weights drawn from the Webb distribution.

Table A.12
Pre-pandemic satisfaction

	(1)	(2)
Treated	0.133	0.036
	[0.132]	[0.794]
Socioeconomic status index		−0.036
		[0.358]
Female gender		0.018
		[0.690]
Treated		−0.056
× Socioeconomic status index		[0.788]
Treated		−0.007
× Female gender		[0.946]
Socioeconomic status index		−0.002
× Female gender		[0.934]
Treated × Female gender		−0.130
× Socioeconomic status index		[0.688]
Student characteristic controls	No	Yes
Observations	149	135
Mean dep. var.	4.07	4.07
R^2	0.006	0.053

Note. Dependent variable: "On a scale from 1 to 5, where 1 is very dissatisfied, and 5 is very satisfied, how satisfied were you with the quality of your life during the period immediately before the onset of the pandemic (February/March 2020)? Here, we mean well-being broadly speaking, joy of life, view of the future, and so on." A constant is included in all regressions. Column (1): No controls. Column (2): Control for self-estimated popularity. *P*-values are in square brackets and computed using wild cluster bootstrap with 500 replications, with bootstrap weights drawn from the Webb distribution.

Table A.13
Network constraint: Alternative channels

	(1)	(2)
Panel A:		
Median income of home municipality (SEK, thousands)	−0.010	−0.006
	(0.044)	(0.046)
R^2	0.000	0.002
Panel B:		
Population of home municipality (thousands)	−0.001	−0.001
	(0.008)	(0.008)
R^2	0.000	0.002
Student characteristic controls	No	Yes
Observations	112	112
Mean dep. var.	58.81	58.81

Note. Dependent variable: Network constraint (×100). A constant is included in all regressions. Column (1): No controls. Column (2), Panel A: Controls for age. Column (2), Panel B: Controls for age and median income of home municipality. Heteroscedasticity-robust standard errors in brackets.

Table A.14
Non-grade outcomes, Nov–Dec

	Adverse mental effects			Afraid of contracting virus		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	−0.179	−0.086*	−0.296	−0.026	0.125**	0.223
	[0.468]	[0.086]	[0.440]	[0.698]	[0.048]	[0.464]
Socioeconomic status index			−0.183			0.092
			[0.188]			[0.782]
Female gender			0.534			0.622
			[0.204]			[0.214]
Treated			−0.184			−0.314
× Socioeconomic status index			[0.428]			[0.286]
Treated			−0.083			−0.205
× Female gender			[0.654]			[0.774]
Socioeconomic status index			−0.411			−0.090*
× Female gender			[0.756]			[0.100]
Treated × Female gender			0.035			0.745
× Socioeconomic status index			[0.876]			[0.464]
Student characteristic controls	No	Yes	Yes	No	Yes	Yes
Observations	149	134	134	150	135	135
Mean dep. var.	3.53	3.53	3.53	2.63	2.63	2.63
R^2	0.006	0.034	0.093	0.000	0.058	0.126

Note. Dependent variable: "How was your mental health affected by the pandemic", and "How worried were you about contracting COVID-19", respectively. Both variables are measured on a scale from 1 to 5, for the period November–December. A constant is included in all regressions. Columns (1) and (4): No controls. Columns (2)–(3) and (5)–(6): Controls for pre-pandemic life satisfaction and self-estimated popularity. *P*-values are in square brackets and computed using wild cluster bootstrap with 500 replications, with bootstrap weights drawn from the Webb distribution. * and ** denote significance at the 10% and 5% level, respectively.

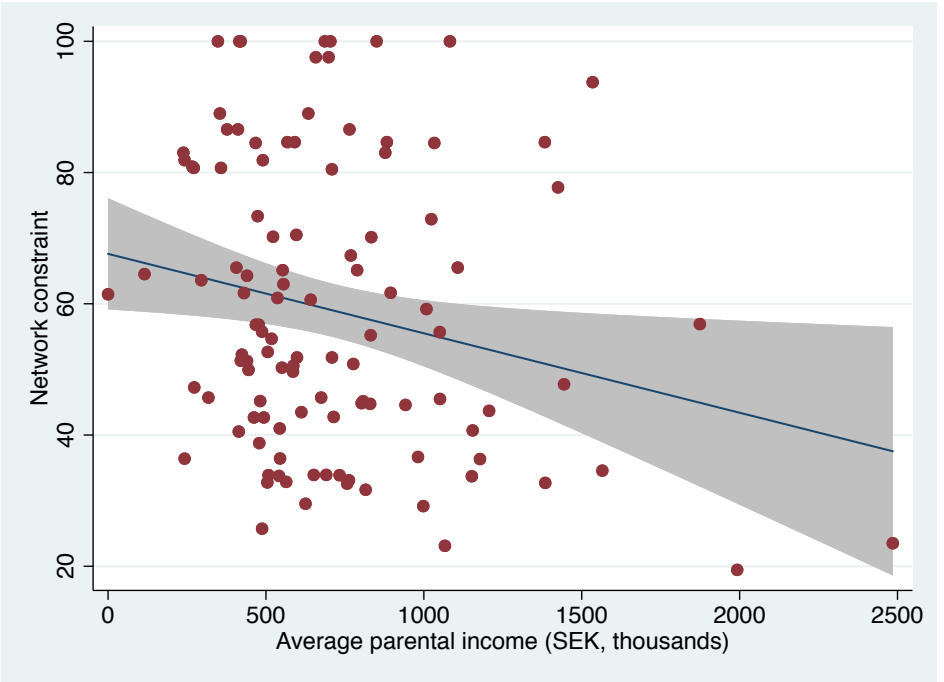


Figure A.3: Scatter plot of the relationship between average parental income on the horizontal axis and network constraint (× 100) on the vertical axis, with 95% confidence bands around the estimated regression line. *Note.* Three observations, each with average parental income above SEK 2,500,000, were omitted from the figure for reasons of visual clarity.

B. Data Description and Data Sources

This subsection describes the construction of the variables used in the empirical analysis in additional detail.

Academic outcomes. All grade data comes from LADOK, which is the student administration system used at Lund University.

Parental income. To obtain the data on parents' income, we first retrieved the personal identity numbers (social security numbers) of both parents using the population registry. Then, we proceeded by using the Tax Agency's data on taxable income for the latest available year, 2019.[21] This figure includes earned income, but excludes capital gains. In accordance with the Swedish Constitution, both the personal identity numbers and the tax records are publicly available information.

Additional personal data. Using the population registry, it is straightforward to retrieve additional demographic characteristics for our sampled students. In this paper, we use gender, as well as the name of the municipality where students resided before starting university. The penultimate digit in the 12-digit personal identity number gives the gender at birth, being odd for men and even for women.

Municipality median income and population. As a control variable in our regressions in Section IV, we use the median disposable income for each municipality for the latest available year, 2018, and for individuals aged 20–64. In Section V.B, we additionally utilize data on the population of students' home municipalities for robustness checks. The data source for both of these variables is the Swedish Statistics Agency.

Parental CEO and/or board assignments. The robustness check presented in Table A.4 of Online Appendix A confirms that most of the wealthiest parents had either a CEO position or board assignment in 2019. This data comes from the Swedish Companies Registration Office, a government agency.

[21] In the robustness checks for the previous cohort, we used tax records from 2018.

C. Survey Construction

The following section describes each question used in our survey in additional detail.

1. What is your gender?

- ☐ Male
- ☐ Female
- ☐ Prefer not to specify.

2. In which municipality did you live at the time of your graduation from high school?

_____

3. When growing up, in what type of dwelling did you mainly live?

- ☐ House
- ☐ Housing cooperative
- ☐ Rental apartment
- ☐ Townhouse

4. Does anyone of your parents have a college or university degree?

- ☐ Yes, both.
- ☐ Yes, but only one parent.
- ☐ No

5. Has anyone of your parents been the CEO or a board member of a publicly listed company?

- ☐ Yes
- ☐ No
- ☐ Don't know.

6. Much of last semester[22] was online. Did you at any point during the semester move back to live with your parents because of this?

- ☐ Yes
- ☐ No
- ☐ I already live with my parents.

7. During the period September–October[23] last semester, how large a share (in %) of your total

[22] Refers to Fall 2020.

[23] Since M students took the course in supply chain management in the second half of the semester, this changes to November–December M students.

time spent studying, did you spend on each of the following courses? By "time spent studying", we mean the sum of lectures, exercise sessions, self-study, exam cramming, and so on. It should sum to 100.

8. On a scale from 1 to 5, where 1 is very uninteresting, and 5 is super-interesting, how would you rate each of the following courses?

9. On a scale from 1 to 5, where 1 is almost never, and 5 is daily, how often did you study together with your classmates when studying the following courses? Here, we mean both physical meetings, as well as group chats through Messenger, Zoom, and so on.

10. On a scale from 1 to 5, where 1 is almost never, and 5 is daily, how often did you study together with your classmates when studying the following courses? Now, we mean physical meetings only.

11. On a scale from 1 to 5, where 1 is not worried at all, and 5 is very worried, how worried were you about contracting COVID-19?

12. On a scale from 1 to 5, where 1 is not negatively at all, and 5 is very negatively, how was your mental health affected by the pandemic? Here, we refer to the lack of social contacts, fewer in-person lectures, boredom, and so on.

13. On a scale from 1 to 5, where 1 is not motivated at all, and 5 is extremely motivated, how motivated were you in your studies last semester, generally speaking?

[24] This differs between programs as follows:

EM: Complex Analysis, Mathematical Statistics, Microeconomic Theory

EP: Complex Analysis, Dynamics, Statistical Thermodynamics

I: Mathematical Statistics, Microeconomic Theory, Supply Chain Management

M: Mechanics, Supply Chain Management, Thermodynamics and Fluid Mechanics

During September–October.

During November–December.

14. On a scale from 1 to 5, where 1 is very dissatisfied, and 5 is very satisfied, how satisfied were you with the quality of your life last semester? Here, we mean well-being broadly speaking, joy of life, view of the future, and so on.

During September–October.

During November–December.

15. Same question as above, only referring to the period immediately before the onset of the pandemic (February/March 2020).

16. Immediately before the pandemic, how active were you in student life? Here, 1 means not active at all, and 5 means very active.

17. What is your view on the restrictions imposed in Sweden in response to the spread of the virus?

- ☐ Well-balanced[25]
- ☐ Too harsh.
- ☐ Too lenient.

18. A question regarding people you know (friends and acquaintances). On a scale from 1 to 5, how many of them know each other? Here, 1 means that none of my friends and acquaintances know each other, and 5 means that almost all of my friends and acquaintances know each other.

19. How large a share (in %) of your classmates are in each of the following categories? It should sum to 100.

They are my close friends, and we can talk about anything!

They are my friends, but I do not know them well enough to talk about deeper stuff.

I know them, and maybe talk to them when I meet them, but I would never use social media to communicate with them.

I do not know them, and I never communicate with them.

20. On a scale from 1 to 5, where 1 is not popular at all, and 5 is extremely popular, how do you think your classmates view you?

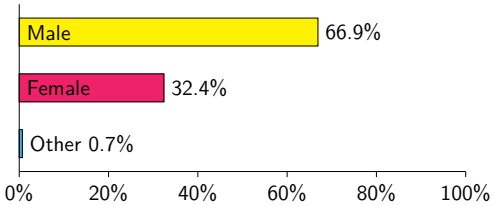
[25] The Swedish adverb used here, *lagom*, has no one-word translation into English. Other suggestions include "about right", or "just enough".

21. Write down the initials of your five closest classmates.[26]

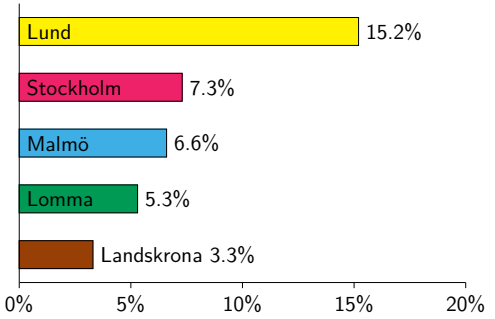
[26] We drop this question for the non-treated students.

D. Survey Results

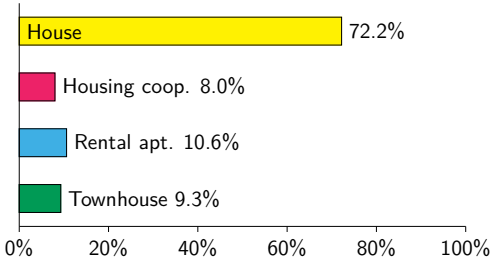
1. What is your gender?



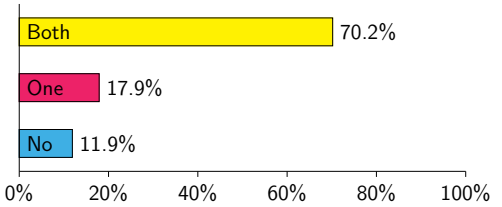
2. In which municipality did you live at the time of your graduation from high school?
   Five most prevalent municipalities:



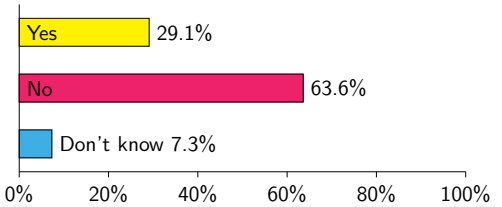
3. When growing up, in what type of dwelling did you mainly live?



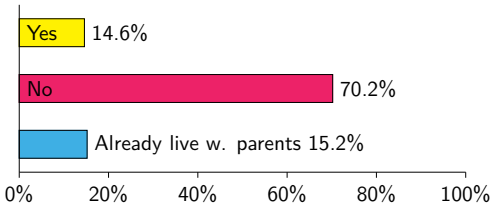
4. Does anyone of your parents have a college or university degree?



5. Has anyone of your parents been the CEO or a board member of a publicly listed company?

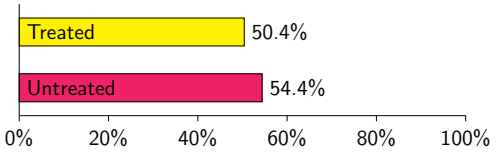


6. Much of last semester was online. Did you at any point during the semester move back to live with your parents because of this?

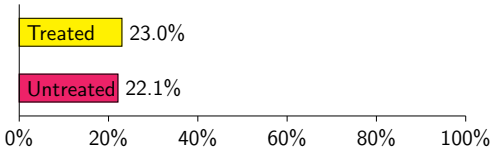


7. During the period September–October last semester, how large a share (in %) of your total time spent studying, did you spend on each of the following courses? By "time spent studying", we mean the sum of lectures, exercise sessions, self-study, exam cramming, and so on. It should sum to 100. *Note.* Only the results for the spillover courses (Complex Analysis and Supply Chain Management) are presented.

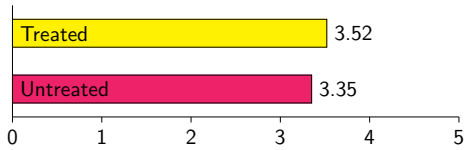
Complex Analysis:



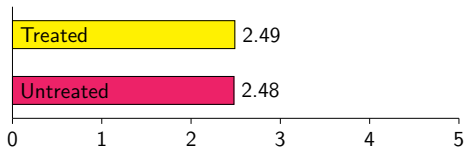
Supply Chain Management:



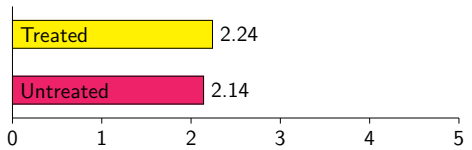
8. On a scale from 1 to 5, where 1 is very uninteresting, and 5 is super-interesting, how would you rate each of the following courses? *Note.* The table averages over all three courses weighted by ECTS credits, for treated and untreated students.



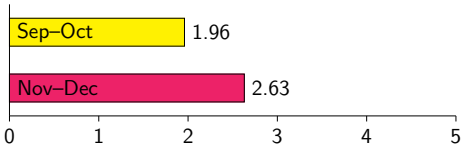
9. On a scale from 1 to 5, where 1 is almost never, and 5 is daily, how often did you study together with your classmates when studying the following courses? Here, we mean both physical meetings, as well as group chats through Messenger, Zoom, and so on. *Note.* Only the results for the spillover courses (Complex Analysis and Supply Chain Management) are presented.



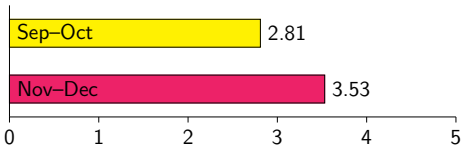
10. On a scale from 1 to 5, where 1 is almost never, and 5 is daily, how often did you study together with your classmates when studying the following courses? Now, we mean physical meetings only. *Note.* Only the spillover courses (Complex Analysis and Supply Chain Management) are presented.



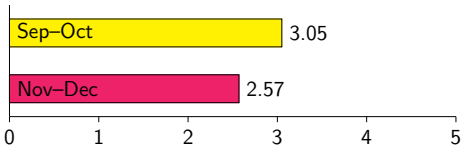
11. On a scale from 1 to 5, where 1 is not worried at all, and 5 is very worried, how worried were you about contracting COVID-19?



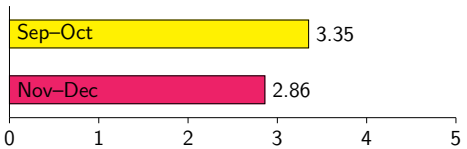
12. On a scale from 1 to 5, where 1 is not negatively at all, and 5 is very negatively, how was your mental health affected by the pandemic? Here, we refer to the lack of social contacts, fewer in-person lectures, boredom, and so on.



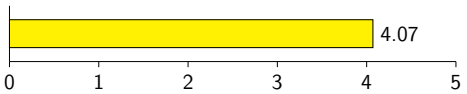
13. On a scale from 1 to 5, where 1 is not motivated at all, and 5 is extremely motivated, how motivated were you in your studies last semester, generally speaking?



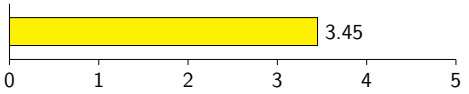
14. On a scale from 1 to 5, where 1 is very dissatisfied, and 5 is very satisfied, how satisfied were you with the quality of your life last semester? Here, we mean well-being broadly speaking, joy of life, view of the future, and so on.



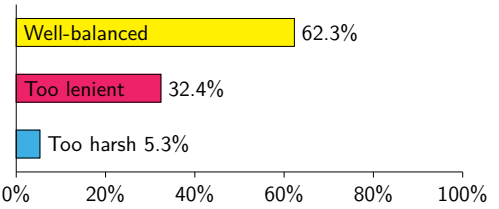
15. Same question as above, only referring to the period immediately before the onset of the pandemic (February/March 2020).



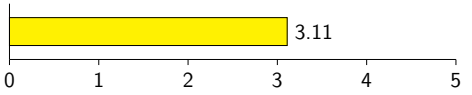
16. Immediately before the pandemic, how active were you in student life? Here, 1 means not active at all, and 5 means very active.



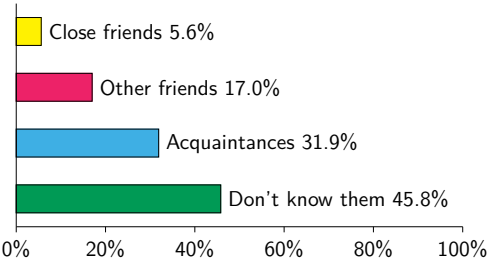
17. What is your view on the restrictions imposed in Sweden in response to the spread of the virus?



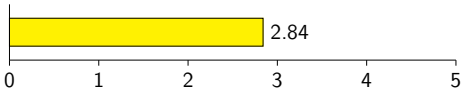
18. A question regarding people you know (friends and acquaintances). On a scale from 1 to 5, how many of them know each other? Here, 1 means that none of my friends and acquaintances know each other, and 5 means that almost all of my friends and acquaintances know each other.



19. How large a share (in %) of your classmates are in each of the following categories? It should sum to 100.



20. On a scale from 1 to 5, where 1 is not popular at all, and 5 is extremely popular, how do you think your classmates view you?



Paper IV

Peer Desirability and Academic Achievement

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Abstract

On the marriage market, two of the most desired partner traits are socioeconomic status and physical beauty. Using the random assignment of university engineering students to peer groups during introductory freshmen weeks, this paper studies how a student's parental income and facial attractiveness affect the grade outcomes of peers. The results show that exposure to highly desirable peers with respect to socioeconomic background and beauty improves grades. The results are particularly strong when the two traits are considered jointly. A field experiment shows that exposure to attractive faces improves optimism and well-being among students not currently in a romantic relationship.

JEL classification codes: C93; D91; I23; J12; Z13
Keywords: beauty; peer effects

I. Introduction

Peer effects in education is one of the most studied phenomena in economics. In its most narrow sense, it refers to the observation that students perform better if exposed to classrooms with high-achieving classmates. Recently, economists have begun to explore how personality traits of peers affect academic outcomes. Examples of such traits include grit, self-confidence and anxiety (Gerhards and Gravert 2020; Golsteyn et al. 2021). In this paper, I examine how university students' socioeconomic status (SES), and physical beauty, affect the academic performance of peers.

There are at least two reasons for why socioeconomic status and beauty would affect peer grades. First, socioeconomic status and physical attractiveness are significantly linked to academic outcomes. As an example of this, students with wealthy parents are more likely to interact with faculty, positively impacting grades by widening their information set (Kim and Sax 2009). More generally, students from high-status backgrounds have broader social networks and higher network centrality, both of which are likely to have positive impacts on academic performance (Calvó-Armengol et al. 2009; Mehic and Olofsson 2022). This is because having a broad social network means that a person is linked to different social clusters, which facilitates access to more diverse and novel information. Other research has suggested that high-SES status students are more motivated to study, and more altruistic than others (Falk et al. 2021). In terms of beauty, attractive college students receive higher grades than non-attractive peers (Landy and Sigall 1974; Hernández-Julián and Peters 2017). There is similar evidence about beautiful people being more likely to have broker positions in social networks (O'Connor and Gladstone 2019). Any such positive effects on grades, regardless of whether driven by socioeconomy, beauty, or both, can cause positive spillovers to peers. Examples of propagation mechanisms include group assignments and informal study sessions.

Secondly, socioeconomic status and beauty are also seen as highly desirable traits on the marriage market.[1] Recent sociological evidence suggests that singles take into account these traits jointly when screening for a potential romantic partner, and that couples are formed by matching on total desirability (McClintock 2014).[2] Relatedly, besides being obvious predictors of dating success, social stratification and beauty are both status markers, and many people attribute desirable interpersonal qualities to high-SES and physically attractive individuals, even if they have no intent of pursuing a sexual relationship with that individual (Webster and Driskell 1983; Feingold 1992; Lemay et al. 2010). Put simply, people enjoy being in the presence of beautiful and wealthy people, and individuals associate beauty and wealth with a wide range of positive attributes (Marks et al. 1981; Eagly et al. 1991; Paulus 2016).[3] Additional evidence shows that exposure to facial attractiveness increases neural activity (Chatterjee et al. 2009). Hence, be-

[1] Considering that college graduates are significantly more likely to be married to one another, higher education institutions are increasingly regarded as local marriage markets (Eika et al. 2019; Kirkebøen et al. 2022).

[2] This theory of assortative mating contrasts the more traditional beauty-status exchange model, under which it is assumed that one partner "exchanges" a high level of one trait for a high level of another trait (Buss 1998; Juhn and McCue 2017). Specifically, wealthy men desire attractive women, whereas physically attractive women trade beauty for a man's stratification status.

[3] In psychology, this is sometimes known as the halo effect.

ing surrounded by highly desirable peers improves well-being, which should, in turn, positively affect academic performance (Cornaglia et al. 2015).[4]

Building on this theory, I construct a measure of a student's total desirability with respect to socioeconomic status and beauty.[5] In the main model, these two traits are given equal weight in determining a student's desirability. For the identification strategy, I use data from the random assignment of undergraduate engineering freshmen into peer groups during orientation weeks at a Swedish university. The purpose of the orientation weeks is to familiarize newly admitted students with the university environment, and to facilitate the creation of friendship bonds between the students. Since students are allocated to peer groups randomly, the setting eliminates the problem of students self-selecting into peer groups.

By matching data on peer group affiliation with two years of grade data for each student, detailed administrative records on parental income, as well as survey responses grading the physical beauty of each individual, I can causally identify the role played by peer desirability in terms of socioeconomic status and beauty on academic performance. Since all courses during the first two years of the engineering program are mandatory, there is no self-selection bias in the empirical design. Another advantage of the identification strategy is that students admitted to the engineering program in question are all in the top 1.5% of university applicants nationally, suggesting that students are on approximately the same academic level before commencing their studies. To the best of my knowledge, this is both the first study examining the role of peer beauty in shaping academic outcomes, and the first to use detailed administrative records to examine the academic consequences of peer socioeconomic status.

I have two main findings. First, a student's own desirability improves academic performance. This effect is particularly strong for mathematics courses, and is driven primarily by socioeconomic status, although a student's own beauty also affects grades positively. Second, the desirability of peers impacts academic achievement, with exposure to desirable peers significantly improving grades, again chiefly in mathematics. The estimated peer effects are attenuated when considering the desirability characteristics individually, suggesting that these traits are to be considered jointly not to underestimate peer effects. Additionally, this finding implies that socioeconomic background and beauty are approximately of equal importance when determining peer effects.

The finding that a student's own socioeocomic status and beauty impact grades positively implies the existence of direct spillover effects from peers. However, since peer effects are magnified when considered jointly, an indirect marriage market channel is likely to be an important driver behind our results. I proceed by shedding light on the mechanisms behind this link. I design a field experiment among university students, in which the treatment group is shown a number of pictures of individuals perceived as beautiful, whereas the control group is shown a set of less attractive faces. The treatment is followed by a questionnaire, in which respondents

[4] In addition to socioeconomy and beauty, a variety of other potential "desirability pairs" have been suggested in recent research. These include couples matching on joint socioeconomy and health (Maralani and Portier 2021), or in regions where HIV is endemic, on joint beauty and sexual safety (Angelucci and Bennett 2021).

[5] The term "desirability" reflects the sociological term "desirable traits".

are asked to rate their agreement with various statements on a five-point Likert scale. Singles in the treatment group were significantly more optimistic about their subjective probability of passing the next exam, and also rated their well-being higher vis-à-vis singles in the control group. This finding suggests that even ephemeral exposure to beauty has significant positive impacts on well-being, at least among those who are not currently in a romantic relationship. These positive effects on contentment are likely to cause academic spillovers, positively impacting in-class performance. Mirroring the grade findings, the survey results suggest that respondents consider a person with moderately high values of both socioeconomic status and beauty as more attractive than someone with very high values on one of these characteristics only.

This paper contributes to three different strands of work in economics and sociology. First, I contribute to the wide literature on peer effects in education (Sacerdote 2001; Lavy and Schlosser 2011; Carrell et al. 2013; Hill 2015; Feld and Zölitz 2017). Although most studies evaluate long-term interventions, a number of these papers have used a similar short-term intervention where students are randomly allocated to peer groups during freshman orientation weeks, albeit answering different questions than the ones posed in this article (Fischer and Rode 2020; Thiemann 2022). Most studies focus on the peer effect on grades, whereas a minority of papers examine other outcomes, most notably major choice (Aneli and Peri 2019; Brenøe and Zölitz 2020; Zölitz and Feld 2021). My paper adds to this literature by examining the role played by the socioeconomic status and beauty of peers, and how the interplay between these traits is shaping peer academic outcomes.

Secondly, the paper contributes to research on how student achievement varies by socioeconomic status (Willms 1986; Rivkin 2001; McEwan 2004). However, these studies are oftentimes plagued with endogeneity concerns, or unclear definitions of socioeconomic status (van Ewijk and Sleegers 2010). Two recent random interventions from Peru and India, respectively, show that for low-SES students, exposure to high-status peers improves academic outcomes, whereas the effects on high-SES students are more modest (Adrianzén et al. 2019; Rao 2019). Contrary to these findings, having high-status college peers in an elite U.S. university in the early 1900s increased educational inequality through exposure to on-campus cliques that positively impact the academic and social outcomes of high-SES students, but not those from more frugal backgrounds (Michelman et al. 2022). I contribute to this literature by illustrating that peer socioeconomic status positively impacts grades, however, the results are considerably stronger when considering socioeconomic status jointly with peer beauty.

Finally, this paper complements the wide literature on the importance of beauty in shaping various outcomes, both in education and elsewhere. Being physically attractive is associated with a range of advantages in life: workers who are more attractive are more likely to be called back to an interview (Bóo et al. 2013), earn more money (Biddle and Hamermesh 1998; Scholz and Sicinski 2015), and are more satisfied with their careers (Hosoda et al. 2003). In addition, attractive teachers receive higher ratings from students (Hamermesh and Parker 2005), good-looking students are more likely to be enrolled in high-status colleges (Ong 2022), beautiful politicians are more likely be elected to office (Berggren et al. 2010; Berggren et al. 2017), while being unattractive increases the a person's propensity for criminal activity (Mocan and Tekin 2010). I add to this literature by illustrating the importance of peer beauty in shaping academic out-

comes, at least when combined with peer socioeconomic status. The finding that peer effects from socioeconomic status and beauty are stronger for more difficult mathematics courses is consistent with previous research suggesting that the beauty premium is highly context-specific (Mobius and Rosenblat 2006; Deryugina and Shurkov 2015). Finally, if the positive grade effect stemming from peer beauty is a result of improved productivity, this finding has broader labor market implications, because it implies that hiring a physically attractive worker improves the productivity of that person's colleagues. Although increased productivity is generally considered advantageous for the firm, it may be considered discriminatory to hire based on beauty even if it boots coworker productivity. This finding, thus, sheds new light on the origins and consequences of labor market discrimination.

The reminder of the paper is structured as follows. Section II describes the orientation weeks, and Section III presents the data. Section IV outlines the empirical strategy, presents the main results, and discusses potential mechanisms. The paper concludes with Section V.

II. Setting

II.A. The Industrial Engineering Program

At the Lund University Faculty of Engineering, most undergraduate engineering programs are five-year programs leading to an MA in Engineering. The most competitive engineering program is the Industrial Engineering (denoted by I) program. Admission to Swedish universities is based solely on either high school grades or on the standardized entrance exams (equivalent to the SAT in the United States), and not on interviews or cover letters.[6] Regardless of whether a student is admitted through grades or through the standardized exam, all admitted students are approximately in the top 1.5% of university applicants nationally. This feature ensures that all students admitted to the program are on roughly the same academic level before starting their studies.

The first two years consist of mandatory courses in mathematics, physics, computer science, business administration, and economics, after which students choose one out of five possible specializations, for instance finance or supply chain management. Table A.1 of Online Appendix A shows the year-by-year structure for the I program. The focus of this paper is on years 1 and 2, since all courses in the first two years are mandatory, and there is no selection into courses.

Swedish universities follow the standardized system for comparing academic credits across the European Union, the so-called *European Credit Transfer System* (ECTS). One academic year is equal to 60 ECTS credits, which corresponds to 1600 hours of full-time studies. There are two semesters in an academic year (Fall and Spring), with 30 ECTS worth of coursework in each. In addition, each semester is divided into two terms; for the Fall semester, the terms are September–October, and November–December. For the Spring semester, the terms are January–March, and April–June. Online Appendix B provides further details on the setting, including the admissions procedure described previously.

[6] Around two-thirds of applicants are admitted through high school grades, and the reminder via the standardized admissions test.

II.B. The Orientation Weeks

The total number of students enrolled in the Industrial Engineering program is around 100 each year. To familiarize incoming freshmen with their future classmates, with the city of Lund, and with university life in general, the program starts with four student-led orientation weeks. When receiving their acceptance letter, admitted students are informed that the induction meeting is one week before the start of lectures. This date is, in fact, the commencement of the orientation weeks. Hence, despite the orientation weeks being organized by the student union, the university facilitates and encourages participation. As a consequence, participation is *de facto* mandatory. At the start of the orientation weeks, students are allocated randomly to peer groups, the number of peer groups being around seven per year. In our sample, the average number of students per peer group is approximately 11. A student at the program describes the first day of the orientation weeks as follows:

> "It's kind of a lit thing really. On the first day, we have the introduction, we kind of meet all the teachers, or, rather, they present themselves. Then, directly afterwards, all the mentors enter and read out the names [of the peer group allocations] and then people run out, and they have pounding music outside and welcome everyone with hugs . . . "[7]

As suggested by the quote, each peer group is allocated a number of senior students to act as mentors. The mentors are also allocated randomly among senior students interested in becoming mentors.[8] Importantly for the identification strategy, freshmen are not allowed to switch between different peer groups. Also, the freshmen have classes only with their fellow students from the same cohort, and not with their mentors. Hence, it is unlikely that the socioeconomic status and beauty of the mentors will affect the grade outcomes of the freshmen.

Table A.2 of Online Appendix A presents the schedule of a typical orientation week. The orientation weeks commence in Week 0, one week before the start of lectures. Whereas Week 1 is relatively "soft" in terms of scheduled classes, in Weeks 2 and 3, students are expected to spend around 40 hours per week on their studies, so the number of freshmen activities is reduced. The end of the orientation weeks is marked by a large prom in Week 3. As indicated by Table A.2, there are some activities in which all freshmen regardless of peer group participate, but the lion's share of activities are separated by peer group. Since the bulk of the orientation weeks activities take place before the start of lectures and assignments, there are no academic activities associated with the orientation weeks. Also, since the orientation weeks are strictly student-organized, the peer groups are unrelated to group allocations done by teachers and course administrators for educational purposes.[9] Thus, the only way that the orientation weeks can impact academic outcomes is through social interactions, not through variations in educational quality.

As the peer group teammates are the first classmates the freshman encounters, ties formed during the orientation weeks have potential to persist over time. Supporting this claim, several

[7] Transcription from an episode of a student-run podcast (Marhaug and Stefansson 2020).

[8] Typically, a large majority (80–90%) of the students in the previous cohort volunteer to be mentors for the younger cohort. Figure A.1 of Online Appendix A illustrates the partition into peer groups during the first day of the orientation weeks for one of the cohorts.

[9] For instance, allocations to TA sessions is typically based on surnames.

recent papers have found that friendship bonds formed early are predictive of persistent social ties throughout university (Back et al. 2008; Giese et al. 2020). Having a formal orientation week similar to the one in our setting reinforces these dynamics (Thiemann 2022). In addition, in four-year degree programs, there are only minor changes in inter-student social networks after nine months (Overgoor et al. 2020), again pointing to the importance of early friendship connections.

III. Data

III.A. Sample

At the Faculty of Engineering, passing grades are given by 3, 4, and 5, with 5 being the top grade.[10] The grading scale is absolute, meaning that the cutoff level for each grade is determined before the start of the course, and is not affected by the relative performance of students. I use data from five cohorts, namely from students starting their studies in 2015, 2016, 2017, 2018, and 2019. The cohorts are thus denoted I-15, I-16, I-17, I-18, and I-19, respectively. In total, the full sample includes 307 students.

As discussed previously, during the first two years, all courses are mandatory, meaning that students cannot self-select into courses. Hence, I restrict the sample to include all courses in the first two years of the program. In all, students take 15 courses during the first two years. Most courses run for one term only, and students always have two or three courses in parallel. Another feature is that every second exam during the first two years is typically in a mathematics course, whereas the other is in business administration, economics, or physics. Although there are a number exceptions to this principle, it will be of some importance for the econometric framework.

III.B. Measure of Desirability and Control Variables

I consider two distinct traits as being crucial for an individual's desirability, namely socioeconomic status and physical beauty. To operationalize socioeconomic status, I use register data from the Swedish Tax Authority to calculate the average labor income of both parents, measured in the same year that their children started their studies. The average income is then standardized at the total sample level (that is, when including all five cohorts), so that the average income is equal to zero, and the standard deviation is equal to unity.

To quantify physical beauty, I recruited a jury consisting of 74 individuals. Since the number of students in the full sample is over 300, each jury member rates one-half of the sample only. Thus, each face receives an average of 37 independent ratings. The jury members are selected so that their average age and gender composition is not statistically different from the values observed in sample.[11] By using publicly available pictures of all students, I let each juror grade

[10] The failing mark is "U" (Swed. *underkänd*, meaning "failed"). If a student did not take an exam, that particular course is treated as a missing observation. If the student did take the exam, but failed, I assign the value 1 to the course in question.

[11] The average age of the raters is 19.73 years, which is not statistically different from the average age of the students when starting the program (20.06 years, $p = 0.14$). Among jury members, 44.6% were women, compared

the faces using a scale from 1 to 10, where 1 is extremely unattractive, and 10 is extremely attractive. Intercoder reliability was excellent (Cronbach's alpha = 0.94). Again, I standardize the measure of physical beauty so that the mean is zero, and the standard deviation is equal to one.

Figure A.2 of Online Appendix A illustrates the box-and-whisker diagrams of the non-standardized values of parental income and beauty for each of the five cohorts. There are no major differences across cohorts in terms of these characteristics.[12] Additionally, there are no differences between cohorts in terms of the share of students with missing personal data (one-way ANOVA F statistic 0.51, $p = 0.73$).[13] Figure A.3 of Online Appendix A shows the scatter plot of average parental income and beauty, showing a positive, albeit numerically insignificant correlation between the two traits. Since attractiveness is associated with a wage premium, and because beauty is at least partially a genetic attribute, a positive relationship between parental income and student beauty is thus to be expected. However, it is possible that this effect is partially mitigated by, for instance, attractive mothers having married more well-off (but less attractive) men, diluting the relationship between parental income and the beauty of their children. The average of a student's standardized parental income and her standardized physical beauty is then the individual's total desirability. This variable is also the main independent variable of interest in the paper. As control variables, I include data on the students' gender at birth and age. Table A.3 of Online Appendix A presents the summary statistics for all variables used in the empirical analysis. The average face rating was 4.72, and the average pre-tax parental income was around SEK 800,000.[14] In addition, we can see that the share of females is relatively large for being an engineering program, close to 40%. Online Appendix C presents the data sources for all variables, and provides additional definitions.

III.C. Survey Data

To examine potential mechanisms, I performed an online survey combined with a field experiment. The respondents are students at the Lund University School of Economics and Management, as well as students at the Industrial Engineering program at Uppsala University, Sweden. To increase the sample size, I used students enrolled in several different courses (not all majoring in Economics), and include course fixed effects to account for differences between cohorts. Two versions of the questionnaire are distributed. Allocation to treatment and control groups is done as good as randomly, and is described in additional detail in Online Appendix C. The exact wording of the questions and answers available to respondents are presented in Online Appendix D, whereas Online Appendix E presents the full results for each question in the survey.

to 37.0% of the students in the sample ($p = 0.23$).

[12] The robustness checks in Section IV.C show that the key finding regarding peer effects are not altered when removing the outlier in terms of income in the I-17 cohort.

[13] The most common reason behind this is if the student's parents are non-residents of Sweden. In that case, the parent has no personal identity number in the population registry, and it is not possible to retrieve any income data.

[14] 1 USD = 9,96 SEK at the time of writing.

1. Questionnaire Outline

Both versions of the survey begin by asking respondents a number of basic questions about gender, socioeconomy, and relationship status.[15] In addition, this part of the questionnaire asks students about what traits they find important in romantic and non-romantic relationships, where each respondent rates the importance of various traits on a five-step Likert scale from "not at all important" to "very important". I further include questions about study habits, namely to what extend students participate in lectures and TA sessions, and to what extent they communicate with teachers outside of class.

2. Experiment Design

The second part of the online survey differs between treatment and control groups. Approximately half the sample grades ten faces of individuals widely perceived as beautiful, whereas the other half rates ten non-attractive faces. Both for the treatment and control group, there are five female and five male faces. Akin to the rating of the student pictures, the field experiment photos are rated on the 1–10 scale, where 1 is extremely unattractive, and 10 is extremely attractive. The final section of the survey is again identical between groups. It asks about future career prospects (dream sector and starting salary), a yes/no question on whether the respondent would like to be a CEO in the future, and a number of questions where respondent is asked to rate their agreement with various statements on a 1–5 Likert scale. These include statements about the probability of passing the next exam, whether the respondent thinks that grades are important for future prospects, and a general statement on future professional life and present well-being. The purpose of these questions if to examine whether a short-term exposure to attractive faces can increase confidence about academic and labor market outcomes, and whether these effects differ between singles and those in a romantic relationship. If even ephemeral exposure to aesthetically pleasing faces increases self-reported grit and well-being, long-term exposure to beautiful classmates is likely to have even greater effects on academic performance.

IV. Empirical Strategy and Results

IV.A. Own Desirability and Grade Outcomes

Before introducing peer effects, I begin with examining whether a student's own desirability affects academic performance. This can be done by estimating

$$y_{ict} = \rho_1 y_{ic,t-1} + \rho_2 y_{ic,t-2} + \xi X_{ic} + \boldsymbol{\lambda}' \boldsymbol{W}_{ic} + \psi_c + \omega_t + \epsilon_{ict} \tag{1}$$

where y_{ict} is the grade of student i in cohort c in the course (subject) t, X_{ic} is the desirability of individual i, $\boldsymbol{W}_{ic}$ is a vector of individual-level controls, ψ_c are cohort fixed effects, ω_t are course fixed effects, and ϵ_{ict} is an idiosyncratic error term. The coefficient of interest is ξ.

[15] Since it can be difficult for respondents to precisely estimate their parents' income, I construct a socioeconomic index based on student's demographic characteristics, standardized so that its mean is zero, and standard deviation is equal to unity. See Online Appendix C for details.

In this specification, the AR(1) term, namely the one-period lagged grade $y_{ic,t-1}$, denotes the grade in the previous exam. Most applications of the dynamic panel model in economics make use of the one-period lagged dependent variable only. However, due to the course structure of the program discussed in Section III.A, if y_{ict} is the grade in a mathematics course, the AR(2) term $y_{ic,t-2}$ often represents the grade in a previous mathematics course, and vice versa for non-mathematics courses. Considering that some students are innately better at mathematics, whereas others are not, we can expect significant persistence over two time periods.[16]

Table 1 gives the results of this regression. Columns (1)–(2) show the results when estimating the model using pooled OLS, columns (3)–(4) utilize the Arelleno-Bover system GMM, and (5)–(6) use the Blundell-Bond system GMM (Arellano and Bover 1995; Blundell and Bond 1998). Since we have not yet introduced peer groups, I cluster standard errors at the cohort level. First, both the AR(1) and AR(2) coefficients are highly significant, suggesting that there is persistence both over one and two periods. Second, a student's own desirability significantly improves academic achievement; one standard deviation higher desirability improves grades by around 0.045 standard deviations when using GMM with the full set of controls. There are only minor differences between the two versions of the GMM in terms of coefficient magnitude. Although the OLS and GMM estimates are numerically close, the OLS estimates are both biased and inconsistent, and should be interpreted with some caution. Section IV.B addresses a number of potential econometric concerns with my empirical strategy.

IV.B. Peer Desirability Characteristics and Grade Outcomes

1. Main Results

Having established the link between a student's own desirability and academic outcomes, this section examines the role of desirability on peer grade outcomes. Here, I use the following AR(2) dynamic panel variation of the standard linear-in-means peer effects model (Manski 1993):

$$y_{icgt} = \phi_1 y_{icg,t-1} + \phi_2 y_{icg,t-2} + \beta X_{icg} + \gamma \overline{X}_{(i)cg} + \delta S_g + \boldsymbol{\theta'} \boldsymbol{W}_{icg} + \eta_c + \pi_t + \varepsilon_{icgt} \qquad (2)$$

where y_{icgt} denotes the grade of student i in cohort c, and in peer group g in the course t, X_{icg} is the desirability of student i in cohort c and in peer group g, $\overline{X}_{(i)cg}$ is the average value of desirability of all members of group g in cohort c excluding i, S_g is the number of students in group g, $\boldsymbol{W}_{icg}$ is a vector of individual-level controls, η_c are cohort fixed effects, π_t are the course fixed effects, and ε_{icgt} is an idiosyncratic error term. In this specification, the coefficient of interest is γ.

Table 2 presents the results. Again, columns (1)–(2) give the pooled OLS estimates, columns (3)–(4) and (5)–(6) use the Arellano-Bover and Blundell-Bond GMM estimates, respectively. We see that both own desirability, and importantly, peer desirability, have a positive impact on academic outcomes. The main result from this part of the paper is that exposure to a one standard deviation higher average peer desirability raises grades by around 3.5 percent of a

[16] In addition to this intuitive explanation behind including the AR(2) term, dropping it from (1) causes the Arellano-Bond test for serial correlation (Arellano and Bond 1991) to indicate that the idiosyncratic error term in levels is not serially uncorrelated. This technical point further motivates the inclusion of the AR(2) term.

standard deviation. The coefficient, denoted $\hat{\gamma}$, is statistically significant at the 5% level, and the result holds regardless of estimation method. The effect size is consistent with previous research on peer effects (cf. Thiemann 2022). Additionally, the estimate of a student's own desirability $\hat{\beta}$ is also positive. The estimates are only marginally impacted by the inclusion of the controls for age, gender, suggesting that these traits are not driving the findings.[17]

Before examining heterogeneous effects, I will briefly address two potential econometric concerns. First, one might worry that the number of clusters is too low. In the specification in Section IV.A, I cluster standard errors by cohort, of which there are only five. In the current specification with peer effects, there are 32 orientation week peer groups. Although the latter number is larger, it could be considered low in certain situations (Cameron and Miller 2015). Tables A.4 and A.5 of Online Appendix A replicate results in Tables 1 and 2, correcting for potentially low cluster sizes by providing adjusted p-values using the wild clustered bootstrap technique (Cameron et al. 2008).[18] I use bootstrap weights drawn from the Webb distribution, which has been shown to perform well when the number of clusters is low (Webb 2014). After correction, the estimated p-values increase slightly, particularly for the case with only five clusters. Importantly, however, none of our previous conclusions are altered by this procedure.

Second, considering that the number of observations is fairly large, there will be a large number of instruments as the GMM generates one instrument for each available lag and time period, the latter being equivalent to courses in our case. The "too good to be true" p-value of the Hansen J test in specifications (3)–(6) of Tables 1 and 2 is a possible indication of instrument proliferation. Two potential solutions include to limit the number of lags used as instruments, or to collapse the instrument matrix (Roodman 2009). Collapsing the instrument set in specifications (4) and (6) in Table 2 gives similar estimates of the impact of peer desirability ($\hat{\gamma} = 0.041, p = 0.032$ for Arellano-Bover, and $\hat{\gamma} = 0.041, p = 0.040$ for Blundell-Bond GMM). Returning to the results in Table 1, namely when considering a student's own desirability only, collapsing the instrument matrix yields similar estimates of the impact of own desirability ($\hat{\xi} = 0.051, p = 0.000$ for Arellano-Bover, and $\hat{\xi} = 0.056, p = 0.000$ for Blundell-Bond GMM, using the full set of controls in both cases). In sum, the main findings are robust to several alternative econometric specifications.

2. Heterogeneous Effects Depending on Own Desirability

An important question is whether there are heterogeneous effects depending on a student's own desirability. I investigate this by interacting peer desirability with own desirability. Since our main results are driven chiefly by grades in mathematics, I perform additional estimates by dropping the non-mathematics courses, and estimating an AR(1) dynamic panel model using only mathematics modules. The results are presented in Table A.6 of Online Appendix A. The estimated coefficient for the interaction between peer desirability and own desirability is

[17] The relatively small impact of these controls suggest that the addition of further controls, for instance cognitive ability and psychological factors, would not impact the results other than marginally.

[18] Inference in the wild cluster bootstrap is based on p-values only. The algorithm does not produce any standard errors, instead, the bootstrap p-value is the share of the bootstrap statistics that are more extreme than the one from the original sample.

negative, and statistically insignificant. If we instead consider only the mathematics courses, the absolute value of the coefficient is more than three times larger in magnitude vis-à-vis the specification with all courses included. One standard deviation higher value of own desirability reduces the peer effect from being in a group with one standard deviation higher peer desirability by around 0.1 standard deviations. The estimated interaction term is highly significant.

3. Relative Importance of Peer Socioeconomic Status and Beauty for Grades

What is the relative importance for peer effects of each of the two characteristics that constitute a student's total desirability? In the previous analyses, parental income and beauty were given equal weight when defining desirability. This section examines the effects on peer effects when giving different weight to socioeconomic status and beauty.

Figure 1 illustrates the point estimates $\hat{\gamma}$ and the 90% confidence bands associated with $\hat{\gamma}$ when re-estimating equation (2) for various combinations of socioeconomic status and beauty. The horizontal axis shows the relative weight of beauty in constructing the standardized desirability measure. Hence, a value of 0 means that the measure is based solely on income, whereas 100 means that it is based fully on beauty. The coefficients are estimated using Blundell-Bond system GMM including the full set of controls, corresponding to column (6) in Table 2 when parental income and beauty were given equal weight. The results suggest that the largest peer effects occur when the relative weight of beauty is around 40–60%, that is, when we give approximately equal weight to socioeconomic status and beauty. Conversely, there were no statistically significant peer effects when considering only parental income or beauty. Focusing on these measures individually instead of considering them jointly, is thus likely to underestimate peer effects. This finding is broadly consistent with similar research suggesting that considering traits individually instead of jointly risks underestimating marriage market sorting (Maralani and Portier 2021).

We can perform a similar decomposition when considering the impacts of a student's own characteristics. Table 4 reports the results when re-estimating (1) separately for parental income and beauty, using bootstrap-adjusted p-values. Both coefficients estimates of ξ are positive and significant at the 10% level, however, the estimate of 0.041σ ($p = 0.064$) for parental income is close to the 0.045σ ($p = 0.032$) estimated when considering total desirability. The estimated coefficient for beauty is lower at 0.020σ ($p = 0.080$). Taken together, these results imply that physically attractive students, as well as high-SES students, perform better academically. Since the same traits are associated with positive peer effects, this finding indicates the presence of direct spillover effects to peers. Both when giving equal weight to income and beauty, as well as when considering income and beauty separately, the main effect is driven by mathematics courses.

IV.C. Robustness Checks

In addition to the concerns about cluster size and instrument proliferation, this section briefly performs a number of other robustness checks.

1. *Test for Random Assignment*

The identification strategy relies heavily on the assumption that assignment to peer groups was random with respect to peer desirability. Intuitively, since the students responsible for the allocation to peer groups have no information on these traits, it seems unlikely that beauty and parental income would effect allocation.[19] To test this claim formally, I perform a standard randomization check. It involves regressing the students' own desirability on the leave-out-one mean of the desirability in the assigned peer group. However, a problem with this procedure is that randomization induces a negative correlation between own and peer desirability. The reason for this bias is that students cannot be their own peers. Thus, students with a high value of desirability draw peers from an urn of students with, on average, lower values of the trait. I correct for this bias by using a standard approach in the peer effects literature, namely controlling for the leave-out-one mean at the level of randomization (Guryan et al. 2009; Feld and Zölitz 2017; Golsteyn et al. 2021). In our case, randomization is performed at the cohort level. To further increase the precision of the estimates, I include controls for student age, gender, as well as the group size variable S_g, and the cohort fixed effect η_c. In addition to total desirability, I perform the same randomization check for parental income and beauty separately. Table A.7 of Online Appendix A presents the results. The small and statistically insignificant coefficients are consistent with assignment to peer groups being random with respect to desirability, and also with respect to the two traits separately.

2. *The Importance of Peer Group Member for Future Friendship Bonds*

Another key assumption is that the peer groups are predictive of friendship bonds. In other words, that students remain friends with their orientation week group members. If this were not the case, we would not be able to credibly estimate any peer effects. To test this claim more formally, I use that during one course early in the second year of the program, students self-select into groups of 3–4 classmates when writing a mandatory group assignment. If peer group members are disproportionately represented in student's assignment groups, it is possible to conclude that the peer groups formed during the orientation week predict persisting friendship bonds. For each student, I calculate the number of assignment group members that were also in the student's orientation week peer group. With this information, it is straightforward to calculate the empirical distribution of overlapping friends. That is, the share of students with no peer group members in their assignment group, the share with one friend, and so on. Using basic combinatorics, I proceed by calculating the corresponding shares if assignment group members were chosen randomly.[20]

Figure 2 plots the observed and approximate theoretical (expected) distributions of overlapping friends. We can see that a majority of students, 54%, chose to partner with at least one of their peer group friends, which significantly contrasts the expected share: if assignment group

[19] The peer group allocation is done before the mentors meet the students.

[20] See Online Appendix B for the derivation of the expected shares. It is reasonable to expect that students meet some of the peer group friends more often than others, for instance through to TA sessions. However, as discussed previously, allocation to TA sessions is unrelated to the peer group allocation. Thus, this does not threaten the validity of the results in this section.

members were chosen randomly, only 32% would have at least one peer group friend in their assignment team. There are similar discrepancies for other values of the number of overlapping friends. For example, the expected share of students partnering with exactly two friends was around 11%, which is considerably higher than the predicted 3.5%. Overall, the difference between the empirical and theoretical distributions is highly significant, with the p-value of Fisher's exact test equal to 0.00036. This finding suggests that the orientation weeks are indeed predictive of future within-cohort friendship bonds. Additionally, this finding suggests that the mentors, that is, the senior students assigned to lead the orientation weeks, are likely of less importance for the main findings compared to the group peers.

IV.D. Evidence on Mechanisms

The main finding of the paper is that socioeconomic status and beauty are jointly associated with significant peer effects. Although both socioeconomic status and physical attractiveness are positively related to a student's own academic performance, the spillover to peers is considerably larger when socioeconomy and beauty are considered jointly. In the following section, I shed light on the channels behind this finding, primarily by means of the field experiment discussed previously.

1. *Preliminary Analyses*

Before analyzing the role of exposure to beautiful faces in creating positive externalities, I begin by evaluating descriptively what characteristics respondents find important in a romantic partner. Notably, over 72% respondents reported that beauty was "somewhat important" in a potential romantic partner, while 11% found it to be "very important". If we consider the rating scale as a 1–5 Likert scale instead of a non-numeric scale, where "not at all important" is given the value 1, "not so important" the value 2, and so on, we may note that male students placed significantly more weight on beauty than females (coefficient estimate 0.258, $p = 0.002$). Examining the views on our second trait of interest, a plurality of respondents, 39.6%, regarded the income of a potential partner as "neither important nor unimportant", although 31.5% found it to be at least somewhat important. Again converting to the 1–5 scale, women were significantly more likely than men to find income important in a romantic partner (coefficient estimate 0.627, $p < 0.001$).

Taken together, these findings suggest that a potential romantic partner with relatively high values of both these traits is likely to be more attractive on the marriage market, compared to a person with an extremely high value of one characteristic, and a low value on the other. This result mirrors the findings on peer effects, which were particularly large in magnitude when socioeconomic status and beauty were considered jointly.

2. *Main Experiment Results*

Proceeding from here, I examine the role of treatment, that is, exposure to aesthetically pleasing faces, in shaping views about future academic and professional outcomes. Table A.8 of Online Appendix A presents the balance tests between treatment and control groups with respect to socioeconomic status, gender, and relationship status. There are no significant differences between

the treatment and control groups in terms of these traits. As expected, the average beauty of all ten faces was estimated to be significantly higher in the treatment group compared to the control group (6.59 and 3.79, respectively, $p < 0.001$). However, there is nothing to suggest that that singles were more generous in their ratings.[21]

Table 4 reports the OLS results when regressing the outcome on the 1–5 scale on the treatment dummy and a set of student-specific controls. Both for the question about future exams, as well as for the general question about well-being, there is a positive and significant effect of treatment when interacted with the indicator for being single. This finding is key for understanding why grades improved when students were exposed to attractive classmates, as it suggests that even ephemeral exposure to beautiful faces increases optimism and self-estimated well-being among single respondents.[22] Since students in the program spend considerable time with their peer group members, significantly contrasting the short-term exposure in the experiment, the true underlying effect is likely to be larger. Table A.9 of Online Appendix A includes an indicator variable taking the value 1 if the respondent claimed that beauty is "very important" in a romantic partner. The results are somewhat stronger when augmenting with this variable, although the numerical differences are relatively minor.

A concern when using OLS in this case is that in the outcome variable, the distance between the options available to respondents are all assumed to be equal, which is a strong assumption if the outcome variable is a rating scale score.[23] Relaxing this assumption, Table A.10 of Online Appendix A shows the results when using ordered logit in lieu of OLS. The results are robust to this modification.

Moving on to evaluating the students' views about their future careers, as reported in Table A.11 of Online Appendix A, there is no effect of treatment on estimated starting salaries, nor on dream sector or the willingness to become a manager. These findings suggest that long-term career plans are not affected by exposure to physically attractive faces. These results are consistent with a finding reported in Table 4, namely that there is no impact of treatment on students' views of their future professional lives. Here, a caveat to note is that a large majority of respondents preferred the private sector, and an even larger share would be interested in becoming a manager in the future (68% and 83%, respectively). This means that variation is relatively low between treatment and control groups for these two questions.

Nonetheless, these findings suggest that exposure to attractive classmates improves exam confidence and general well-being among singles. Together with the finding that socioeconomic status and beauty individually improve a student's own performance, we can conclude that the peer effects of desirability operate through two mechanisms. First, there is a *direct academic channel*, where the higher academic performance of high-SES and physically attractive students

[21] A simple linear regression with the average rating as the dependent variable on a constant and the dummy for being single gives a coefficient estimate of 0.267, and a corresponding robust standard error equal to 0.252 ($p = 0.290$). Additionally controlling for treatment gives a corresponding coefficient estimate of 0.042, and a robust standard error equal to 0.151 ($p = 0.780$).

[22] Although overconfidence may be detrimental to achievement (for instance, through reducing study hours), studies generally show a positive association between motivational beliefs and grades (Blackwell et al. 2007; Morisano et al. 2010; Corkin et al. 2017).

[23] Often, respondents tend to place themselves in the "middle" of the option set, in this case around 3.

cause spillover effects to peers. Second, there is an indirect link, through which exposure to peer beauty creates positive externalities, improving a student's exam optimism and well-being. We may interpret this as a *marriage market channel*. Although we cannot exclude such a link in the opposite direction, where exposure to high-SES classmates improves well-being, this channel is likely to be of less importance, due to the relatively lower weight that students place on the socioeconomic status of a romantic partner.

V. Concluding Remarks

In this paper, I study the role of peer desirability with respect to socioeconomy and beauty in shaping academic outcomes, using the random allocation of engineering students to peer groups during freshman orientation weeks. I show that students allocated to peer groups with highly desirable classmates with respect to socioeconomic status and beauty have better grades. In addition, a student's own socioeconomic status and beauty impact grades positively. Using a field experiment, I further show that single respondents exposed to physically attractive faces are more optimistic about passing exams, and report higher levels of well-being. This finding suggest that peer effects operate, at least partially, by an indirect channel, through which variations in peer beauty affect well-being.

One possible extension of this paper is to evaluate how the distribution of desirability within a group affects grades. That is, whether the results hold in a situation in which only one or two individuals in the peer group are highly desirable, while most others are somewhat unattractive and have low socioeconomic status. Another extension is to examine the role of peer desirability in other settings, for instance in the labor market. Improvements in grades reflect, at least to some extent, increases in student productivity. Thus, the results of this paper imply that hiring a high-SES or physically attractive worker, improves the productivity of that person's co-workers. These effects are likely to be particularly strong for unmarried junior professionals, and can have significant long-term ramifications (Hamermesh 2011).

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Table 1
Impact of own desirability on grades

Outcome variable:						
Standardized grades	(1)	(2)	(3)	(4)	(5)	(6)
Grade_{t-1}	0.222^{***}	0.222^{***}	0.301^{***}	0.307^{***}	0.267^{***}	0.272^{***}
	(0.028)	(0.030)	(0.059)	(0.064)	(0.055)	(0.059)
Grade_{t-2}	0.323^{***}	0.318^{***}	0.162^{***}	0.162^{***}	0.169^{***}	0.168^{***}
	(0.018)	(0.019)	(0.033)	(0.031)	(0.031)	(0.035)
Own desirability	0.036^{***}	0.039^{***}	0.043^{***}	0.042^{***}	0.045^{***}	0.045^{***}
	(0.005)	(0.004)	(0.004)	(0.008)	(0.003)	(0.006)
Course FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Method	OLS	OLS	AB GMM	AB GMM	BB GMM	BB GMM
Observations	3,645	3,645	3,645	3,645	3,645	3,645
R^2	0.311	0.313				
Mean dep. var.	0.000	0.000	0.000	0.000	0.000	0.000
Hansen J test p-value			[1.00]	[1.00]	[1.00]	[1.00]
AR(3) test p-value			[0.51]	[0.52]	[0.39]	[0.40]

Note. Outcome variable: Standardized grades. Controls: The student's gender, and age. Standard errors clustered by cohort in brackets. *** denotes significance at the 1% level.

Table 2
Impact of own and peer desirability on grades

Outcome variable:						
Standardized grades	(1)	(2)	(3)	(4)	(5)	(6)
Grade_{t-1}	0.221***	0.217***	0.300***	0.301***	0.267***	0.270***
	(0.026)	(0.028)	(0.086)	(0.087)	(0.066)	(0.068)
Grade_{t-2}	0.321***	0.317***	0.163***	0.162***	0.169***	0.169***
	(0.019)	(0.019)	(0.023)	(0.022)	(0.023)	(0.023)
Peer desirability	0.029**	0.028*	0.034**	0.031**	0.036**	0.033**
	(0.014)	(0.014)	(0.015)	(0.015)	(0.017)	(0.016)
Own desirability	0.029*	0.030**	0.035*	0.034**	0.037**	0.036**
	(0.016)	(0.014)	(0.018)	(0.016)	(0.018)	(0.016)
Course FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Method	OLS	OLS	AB GMM	AB GMM	BB GMM	BB GMM
Observations	3,645	3,645	3,645	3,645	3,645	3,645
R^2	0.312	0.314				
Mean dep. var.	0.000	0.000	0.000	0.000	0.000	0.000
Hansen J test p-value			[1.00]	[1.00]	[1.00]	[1.00]
AR(3) test p-value			[0.48]	[0.49]	[0.35]	[0.36]

Note. Outcome variable: Standardized grades. Controls: The student's gender, and age. Standard errors clustered by peer group in brackets. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

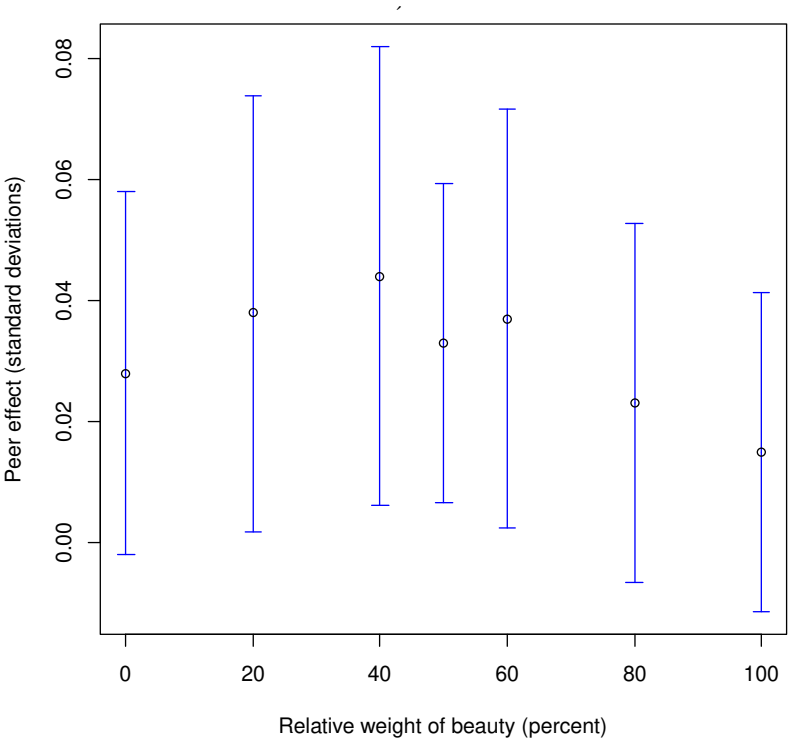


Figure 1: Coefficient estimates ($\hat{\gamma}$) and 90% confidence bands corresponding to re-estimating equation (2) using Blundell-Bond GMM with the full set of controls, varying the relative weight of beauty compared to parental income in determining peer desirability.

Table 3
Impact of own desirability on grades (sep. for income and beauty)

Outcome variable:	Equal weight		Only parental income		Only beauty	
Standardized grades						
	(1)	(2)	(3)	(4)	(5)	(6)
Grade_{t-1}	0.272^{***}	0.288^{***}	0.269^{***}	0.278^{***}	0.285^{***}	0.292^{***}
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Grade_{t-2}	0.168^{***}	0.164^{***}	0.169^{***}	0.168^{***}	0.172^{***}	0.169^{***}
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Own desirability	0.045^{**}	-0.041^{*}	0.041^{*}	-0.051	0.020^{*}	-0.011
	[0.032]	[0.080]	[0.064]	[0.240]	[0.080]	[0.389]
Mathematics course		-0.645^{***}		-0.664^{***}		-0.635^{***}
		[0.000]		[0.000]		[0.000]
Own desirability		0.221^{***}		0.234^{**}		0.084^{*}
$\times$ Mathematics course		[0.000]		[0.028]		[0.093]
Course FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Method	BB GMM	BB GMM	BB GMM	BB GMM	BB GMM	BB GMM
Observations	3,645	3,645	3,645	3,645	3,645	3,645
Mean dep. var.	0.000	0.000	0.000	0.000	0.000	0.000
Hansen J test p-value	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
AR(3) test p-value	[0.40]	[0.24]	[0.39]	[0.21]	[0.40]	[0.40]

Note. Outcome variable: Standardized grades. Controls: The student's gender, and age. Standard errors clustered by peer group in brackets. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

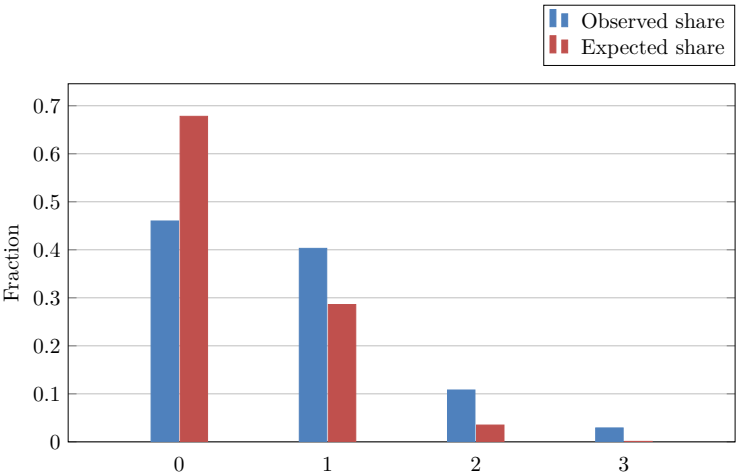


Figure 2: The observed and expected frequencies of the number of friends from the freshman week in the student's assignment group. The observed shares are based from the universe of available observations from cohorts I-18 and I-19; $N = 139$. The Fisher exact p-value is 0.00036 under the null hypothesis of equal distributions. For additional details, see Online Appendix B.

Table 4
Experiment results

	Probability of passing upcoming exams		Importance of grades for future career		View on future professional life		"How do you feel right now?"	
Treatment	0.092	−0.285	0.114	−0.078	0.022	−0.116	−0.066	−0.447*
	(0.135)	(0.199)	(0.200)	(0.153)	(0.129)	(0.182)	(0.156)	(0.235)
Single		−0.316		0.103		−0.381**		−0.605**
		(1.697)		(0.229)		(0.177)		(0.235)
Treatment × Single		0.548**		0.292		0.200		0.611**
		(0.256)		(0.309)		(0.251)		(0.308)
Controls included	No	Yes	No	Yes	No	Yes	No	Yes
Observations	195	195	194	194	197	197	196	196
R^2	0.002	0.156	0.003	0.036	0.000	0.072	0.001	0.080
Mean dep. var.	3.15	3.15	3.49	3.49	3.98	3.98	3.43	3.43

Note: OLS estimates. Outcome variable: Agreement on a 1–5 Likert scale. Controls: Gender, socioeconomic status, relationship status, and course and university fixed effects. White heteroscedasticity robust standard errors are in brackets. * and ** denote significance at the 10% and 5% level, respectively.

Online Appendix [Not for Publication]

A. Additional Tables and Empirical Results

Table A.1
Program structure for the Industrial Engineering program

Subject	First year	Second year	Third year	Fourth year	Fifth year	Overall
Mathematics	27	23				50
Physics	18		7.5			25.5
Business Administration	15	22	18			55
Economics		6	6			12
Computer Science		9				9
Elective courses			28.5	60	60	148.5
Overall	60	60	60	60	60	300

Note. Swedish universities follow the standardized system for comparing academic credits across the European Union, the so-called *European Credit Transfer System* (ECTS). One academic year is equal to 60 ECTS credits, which corresponds to 1600 hours of full-time studies. The table shows the number of ECTS credits per subject and year for students enrolled in the I program.

Figure A.1: Cohort I-19 and their mentors during during the orientation weeks. Different peer groups are identified by different colored T-shirts and/or headgear. The picture is published pursuant to the Swedish Act on Copyright in Literary and Artistic Works (Swed. law 1960:729), ch. 2, § 23(1).

Table A.2
Typical orientation week schedule

Weekday	Week 0	Week 1	Week 2	Week 3
Sunday	X	Breakfast	X	Boat Race
Monday	Welcome, Mentor Snuggle[a]	Team building	X	Sittning
Tuesday	Team building	Sittning	Sittning	X
Wednesday	Sittning[b]	Misc. games	Pub	Brännboll tourn.[c]
Thursday	Mentor Snuggle	Pub	Misc. games	Mentor Snuggle
Friday	Misc. games	Friday Snuggle	Misc. games	X
Saturday	City tour	Beach Day	Sittning	Gasque[d]

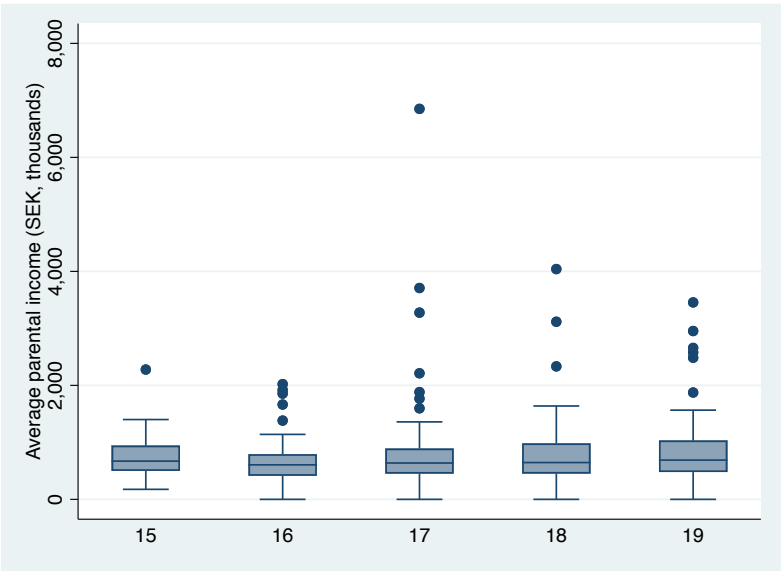
Note. Underlined activities take place with all freshmen. X indicates that there are no scheduled activities on that particular day.

[a] An informal house party in peer groups, organized by mentors.

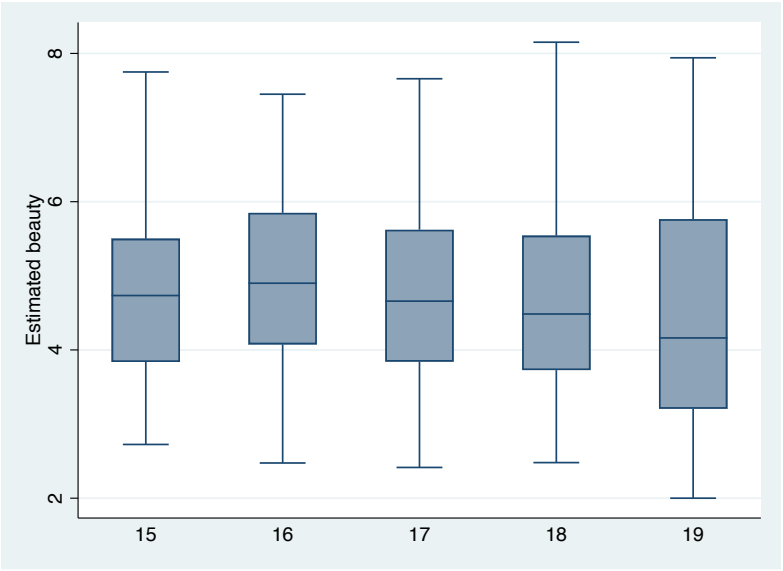
[b] A dinner party at which the dress code is typically suit. More formal than Mentor/Friday Snuggle, but less formal than Gasque. Etymologically cognate with German *Sitzung* ("sitting", "meeting").

[c] A Swedish sport, somewhat similar to baseball.

[d] A prom at which the dress code is black tie. More formal than Sittning, and considerably more formal than Mentor/Friday Snuggle. Faux-French spelling of archaic Swedish *gask* ("party"); originally from Italian *casco* ("helmet"), itself from Latin *quassō* ("I shake", "I strike").



(a) Box-and-whisker diagram of average parental income (in thousands of SEK).



(b) Box-and-whisker diagram of beauty (on the 1–10 scale).

Figure A.2: Box-and-whisker diagrams of the two components of desirability, over cohorts.

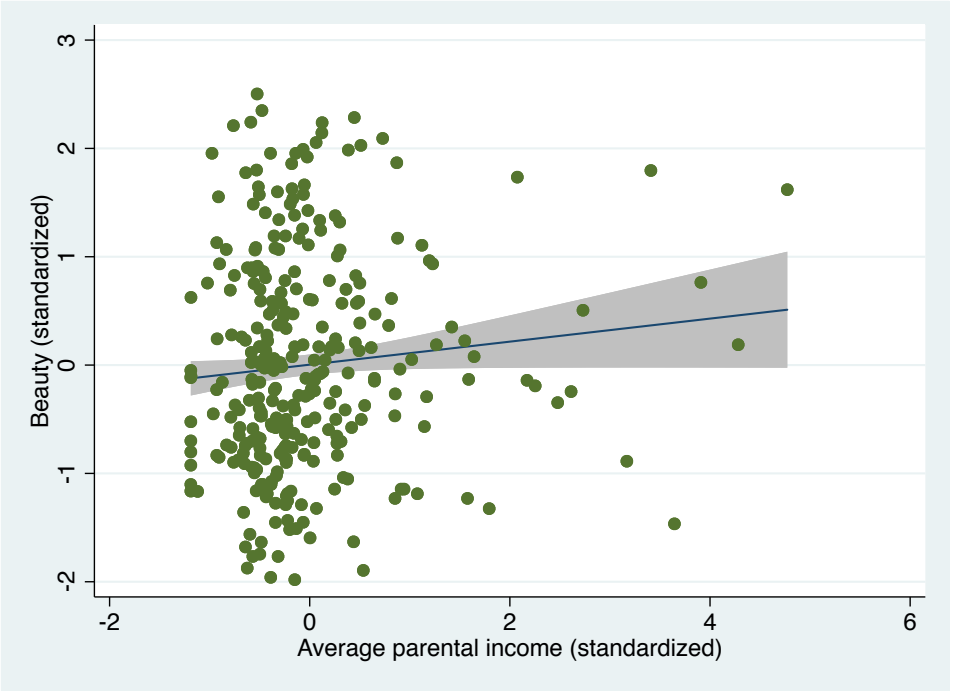


Figure A.3: Scatter plot of standardized annual parental income on the horizontal axis, and standardized beauty on the vertical axis, with 90% confidence bands around the estimated regression line. The Pearson correlation coefficient between the two traits is 0.08. *Note.* One observation is excluded for reasons of visual clarity.

Table A.3
Summary statistics

Main dependent variable	**Mean**	**Std.dev.**	**Min**	**Max**
Grade (all courses)	4.054	0.890	1	5
Measures of desirability				
Average parental income (SEK)	805,341.80	676,845.50	0	6,851,000
Facial attractiveness	4.718	1.370	2	8.152
Student-specific controls				
Female gender	0.370	0.483	0	1
Age at program start	20.06	0.998	18	25

Note. Summary statistics. The measures of grades and desirability are non-standardized.

Table A.4
Impact of own desirability on grades (bootstrap adj.)

Outcome variable:						
Standardized grades	(1)	(2)	(3)	(4)	(5)	(6)
Grade_{t-1}	0.222***	0.222***	0.301***	0.307***	0.267***	0.272***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Grade_{t-2}	0.323***	0.318***	0.162***	0.162***	0.169***	0.168***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Own desirability	0.036**	0.039**	0.043**	0.042**	0.045**	0.045**
	[0.028]	[0.024]	[0.020]	[0.016]	[0.032]	[0.032]
Course FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Method	OLS	OLS	AB GMM	AB GMM	BB GMM	BB GMM
Observations	3,645	3,645	3,645	3,645	3,645	3,645
R^2	0.311	0.313				
Mean dep. var.	0.000	0.000	0.000	0.000	0.000	0.000
Hansen J test p-value			[1.00]	[1.00]	[1.00]	[1.00]
AR(3) test p-value			[0.51]	[0.52]	[0.39]	[0.40]

Note. Outcome variable: Standardized grades. Controls: The student's gender and age. Bootstrap-adjusted p-values in brackets, with the bootstrap based on 250 replications and wild weights drawn from the Webb distribution. ** and *** denote significance at the 5% and 1% level, respectively.

Table A.5
Impact of own and peer desirability on grades (Bootstrap adj.)

Outcome variable:						
Standardized grades	(1)	(2)	(3)	(4)	(5)	(6)
Grade_{t-1}	0.221***	0.217***	0.300***	0.301***	0.267***	0.270***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Grade_{t-2}	0.321***	0.317***	0.163***	0.162***	0.169***	0.169***
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
Peer desirability	0.029*	0.028*	0.034**	0.031**	0.036**	0.033**
	[0.064]	[0.092]	[0.040]	[0.048]	[0.040]	[0.048]
Own desirability	0.029**	0.030***	0.035**	0.034**	0.037**	0.036**
	[0.032]	[0.004]	[0.040]	[0.020]	[0.040]	[0.020]
Course FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Method	OLS	OLS	AB GMM	AB GMM	BB GMM	BB GMM
Observations	3,645	3,645	3,645	3,645	3,645	3,645
R^2	0.312	0.314				
Mean dep. var.	0.000	0.000	0.000	0.000	0.000	0.000
Hansen J test p-value	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]	[1.00]
AR(3) test p-value			[0.48]	[0.49]	[0.35]	[0.36]

Note. Outcome variable: Standardized grades. Controls: The student's gender and age. Bootstrap-adjusted p-values in brackets, with the bootstrap based on 250 replications and wild weights drawn from the Webb distribution. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table A.6
Impact of own desirability on peer effects

Outcome variable:	All courses		Quantitative only	
Standardized grades	(1)	(2)	(1)	(2)
Grade_{t-1}	0.270^{***}	0.262^{***}	-0.154	-0.175
	(0.068)	(0.068)	(0.122)	(0.118)
Grade_{t-2}	0.169^{***}	0.167^{***}		
	(0.023)	(0.023)		
Peer desirability	0.033^{**}	0.035^{**}	0.067	0.073^{*}
	(0.016)	(0.017)	(0.043)	(0.043)
Own desirability	0.036^{**}	0.045^{***}	0.084^{**}	0.116^{***}
	(0.016)	(0.017)	(0.042)	(0.044)
Peer desirability × Own desirability		-0.029		-0.104^{***}
		(0.018)		(0.040)
Course FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	3,645	3,645	3,645	3,645
Mean dep. var.	0.000	0.000	0.000	0.000
Hansen J test p-value	[1.00]	[1.00]	[0.55]	[0.35]
AR(2) test p-value	[0.09]	[0.11]	[0.78]	[0.89]
AR(3) test p-value	[0.36]	[0.35]		

Note. Outcome variable: Standardized grades. Controls: The student's gender and age. Standard errors clustered by peer group in brackets. *, ** and *** denote significance at the 10%, 5% and 1% level, respectively.

Table A.7
Test for random assignment

	Peer total desirability	Peer socioec. status	Peer beauty
Total desirability	−0.003		
	[0.968]		
Socioeconomic status		0.037	
		[0.672]	
Beauty			0.014
			[0.916]
Controls	Yes	Yes	Yes
Observations	307	307	307
R^2	0.154	0.029	0.265
Mean dep. var.	0.000	0.000	0.000

Note. Test of random assignment. The dependent variable is the standardized leave-out-mean of total desirability in the allocated peer group, as well as the leave-out-mean of each of the two traits. Standard errors are clustered at the cohort level, with bootstrap-adjusted p-values in brackets, and the bootstrap based on 250 replications and wild weights drawn from the Webb distribution.

Table A.8
Balance test

Trait	Treatment group ($N = 113$)	Control group ($N = 115$)	p-value for equality of means/proportions
Socioeconomic status index (standardized values)	0.03	–0.03	[0.82]
Male (%)	54.0	56.5	[0.70]
Single (%)	54.7	64.0	[0.17]

Note. Balance test comparing the treatment and control groups with respect to socioeconomic status, gender, and relationship status.

Table A.9
Experiment results (augmented control set)

	Probability of passing upcoming exams		Importance of grades for future career		View on future professional life		"How do you feel right now?"	
Treatment	0.092	−0.285	0.114	−0.078	0.022	−0.116	−0.066	−0.447*
	(0.135)	(0.199)	(0.200)	(0.153)	(0.129)	(0.182)	(0.156)	(0.235)
Single		−0.316		0.103		−0.381**		−0.605**
		(1.697)		(0.229)		(0.177)		(0.235)
Treatment × Single		0.548**		0.292		0.200		0.611**
		(0.256)		(0.309)		(0.251)		(0.308)
Controls included	No	Yes	No	Yes	No	Yes	No	Yes
Observations	195	195	194	194	197	197	196	196
R^2	0.002	0.156	0.003	0.036	0.000	0.072	0.001	0.080
Mean dep. var.	3.15	3.15	3.49	3.49	3.98	3.98	3.43	3.43

Note: OLS estimates. Outcome variable: Agreement on a 1–5 Likert scale. Controls: Gender, socioeconomic status, relationship status, a dummy indicating that the person found physical appearance to be "very important" in a romantic partner, and course and university fixed effects. White heteroscedasticity robust standard errors are in brackets. * and ** denote significance at the 10% and 5% level, respectively.

Table A.10
Experiment results (Ordered logit estimates)

	Probability of passing upcoming exams		Importance of grades for future career		View on future professional life		"How do you feel right now?"	
Treatment	0.293	−0.398	0.172	−0.094	0.022	−0.308	−0.169	−0.881**
	(0.267)	(0.412)	(0.260)	(0.404)	(0.129)	(0.399)	(0.263)	(0.422)
Single		−0.560		0.144		−0.762**		−1.101**
		(0.390)		(0.394)		(0.373)		(0.435)
Treatment × Single		0.993*		0.466		0.457		1.114**
		(0.536)		(0.522)		(0.536)		(0.547)
Controls included	No	Yes	No	Yes	No	Yes	No	Yes
Observations	195	195	194	194	197	197	196	196
Mean dep. var.	3.15	3.15	3.49	3.49	3.98	3.98	3.43	3.43

Note: Ordered logit estimates. Outcome variable: Agreement on a 1–5 Likert scale. Controls: Gender, socioeconomic status, relationship status, and course and university fixed effects. White heteroscedasticity robust standard errors are in brackets. * and ** denote significance at the 10% and 5% level, respectively.

Table A.11
Additional effects of treatment

	Prefers private sect.		Would consider being a manager		Starting salary > SEK 40,000	
	(1)	(2)	(1)	(2)	(1)	(2)
Treatment	−0.036	−0.263	0.158	−0.232	−0.367	−0.078
	(0.307)	(0.515)	(0.383)	(0.553)	(0.332)	(0.543)
Single		−0.525		0.000		0.169
		(0.469)		(0.529)		(0.489)
Treatment × Single		0.439		0.776		−0.534
		(0.673)		(0.771)		(0.697)
Controls	No	Yes	No	Yes	No	Yes
Observations	197	197	197	197	197	197
Pseudo R^2	0.000	0.094	0.001	0.046	0.006	0.034
Mean dep. var.	0.680	0.680	0.832	0.832	0.249	0.249

Note. Logit estimates. Outcome variable: Agreement on a 1–5 Likert scale. Controls: Gender, socioeconomic status, relationship status, and course and university fixed effects. White heteroscedasticity robust standard errors are in brackets.

B. Additional Background

B.A. Admissions Procedure

As discussed in the main body of the text, admission to Swedish universities is based only on high-school grades or on the standardized national entrance exams and not on interviews, cover letters or extracurricular activities. The following section provides evidence that regardless of whether a student was admitted through grades or through the entrance exam, the rank of admitted students is approximately similar relative to other college applicants.

1. The Standardized Admissions Test

The standardized admissions test (*Högskoleprovet*, "The Higher Education Test") consists of two parts: one verbal and one quantitative segment. The test is given twice per year. There is no age limit, nor is there a limit on how many times an individual can sit the test. It is the most recent test score that counts towards admission, meaning that it is very common to sit the test several times.

Scores for each segment are between 0.0 and 2.0, and given in intervals of 0.1, that is, 0.0, 0.1, ..., 1.9, 2.0. The mean of the score in the verbal part and the quantitative part gives the final score. Hence, the final test score is also a number between 0.0 and 2.0, given in intervals of 0.05. The score distribution for each of the two segments is predetermined, that is, x percent of the examinees always receive 0.0, y percent receive 0.1, and so on. The purpose of this is to mitigate the potential unfairness stemming from exam fixed effects. Additionally, it ensures that the mean test score will be very similar regardless of year. Although the final test score is a discrete random variable, the distribution can be approximated well by the normal distribution. Between 2015 and 2019, the mean final score varied between 0.87 and 0.90. The standard deviation was approximately 0.40 for the entire time period.[24]

For the I program, the average minimum entrance score for the years 2015–19 was 1.74. The c.d.f. of the truncated $\mathcal{N}(0.9, 0.4)$, with left-truncation at 0.0, and right-truncation at 2.0, is equal to 0.985 at a test score of 1.74. This finding suggests that students admitted to the program through the standardized test were in the top 1.5% of applicants nationwide.

2. High School Grades

Sweden has a mandatory nine-year comprehensive school (*grundskolan*), followed by three years of upper secondary school (*gymnasieskolan*). High school grades are given using the A–F scale,

[24] *Data source*: Swedish Council for Higher Education, which is the government agency responsible for tertiary education.

where A–E are passing grades, whereas F is the failing grade. A student scoring "A" in all subjects all three years receives a final score of 20.0 points, whereas an "E" gives 10.0 points. Failing all courses all three years gives zero points. In addition, there are 2.5 extra credits available to students who take more advanced subjects, for instance third-year mathematics. Hence, the maximum score is 22.5. The average score nationally for the years 2015–19 has varied between 14.0 and 14.5, according to data from the Swedish National Agency for Education, which is the government agency responsible for overseeing both public and private high schools. The sample mean was 14.2 for the 2015–19 period. There is no data on the standard deviation of final grade scores, however, in a 2009 representative sample of Stockholm high schools, the mean score was 14.7, with a standard deviation of 5.42 (Ernbo 2010). Hence, we can expect a national standard deviation close to this estimate. For the I program, the 2015–19 average minimum admission score was approximately 21.85 on the 0–22.5 scale.

The corresponding c.d.f. of the truncated $\mathcal{N}(14.2, 5.4)$, with left-truncation at 0.0, and right-truncation at 22.5, is 0.983 at a grade score of 21.85. Hence, these students were in the top 1.7% of graduating high school students nationwide, which is very close to the relative rank of those admitted through the standardized tests.

B.B. Observed and Expected Frequencies of Peer Members in Assignment Groups

The following section derives the theoretical expressions for the expected frequencies in Figure 2, and provides further details on the structure of the group assignment.

1. The Group Assignment

The assignment gives 15% of the final grade in the mandatory introductory course in microeconomics, which is given in the second year of the program. All students in both cohorts did the assignment, even though the assignment itself was not strictly mandatory. In its current form, the assignment was introduced in the Fall term of 2019, meaning that I have data only for the cohorts I-18 and I-19. However, since the structure of orientation weeks was exactly the same for all five cohorts in our sample, it is unlikely that the long-term structure of social ties was much different in the cohorts I-15–17.

As mentioned in the main body of the paper, students could form a group consisting of 3 or 4 students. In the I-18 cohort, there were 11 three-student groups, and 13 four-student groups. In the I-19 cohort, there were 12 three-student groups, and 15 four-student groups. This corresponds to an average of 45.1% three-person groups, and 54.9% four-person groups. Cumulatively over the two years, a total of 64 students had no overlapping classmates, that is, no peer group members in their assignment group. A total of 56, 15, and 4 students had 1, 2, or 3 overlapping friends, respectively. In terms of shares of the total number of students ($N = 139$), these frequencies correspond to 46.0%, 40.3%, 10.8%, and 2.9%, respectively. The theoretical distribution (see below for derivations) is 67.8%, 28.6%, 3.5%, and 0.1% for 0, 1, 2, and 3 overlapping friends, respectively. Converting to frequencies using the observed sample size of 139 students, and using Fisher's exact test for testing the null hypothesis of equal distributions gives a p-value of 0.00036. This result suggests that the orientation week peer groups

are highly predictive of second year friendship bonds.

2. Mathematical Derivations

This subsection approximates the theoretical distribution of the number of overlapping friends under the assumption that the assignment members are chosen randomly among classmates. That is, that students do not take into account the social ties formed during the orientation weeks. The goal is to reject the null hypothesis that there is no difference between this distribution and the observed shares presented above. I first calculate the corresponding theoretical distributions if all groups were three- or four-person groups. Introduce the following notation:

$$\begin{aligned} N &= \text{total cohort size} \\ S &= \text{peer group size} \\ k &= \text{number of members in assignment group} \\ m &= \text{number of assignment groups } (\stackrel{\text{def}}{=} N/k) \end{aligned}$$

I assume the following parameter values, where the values of N and S are very close to those observed in I-18 and I-19: $N = 72$, $S = 11$, $k \in \{3, 4\}$, and $m \in \{18, 24\}$. Letting the random variable X be the number of overlapping friends, straightforward calculations show that the probability of each outcome x is distributed as follows for given k:

$$\mathbb{P}(X = x) = \begin{cases} \frac{\binom{N-S}{k-1}}{\binom{N}{k}/m} & \text{if } x = 0 \\ \frac{\binom{S-1}{x}\binom{N-S}{k-x-1}}{\binom{N}{k}/m} & \text{if } 0 < x < k-1 \\ \frac{\binom{S-1}{x}}{\binom{N}{k}/m} & \text{if } x = k-1 \\ 0 & \text{if } x = k \end{cases}$$

Using the parameter values above, straightforward calculations yields the following distribution for $k = 3$ and $k = 4$:

Outcome	$k = 3$	$k = 4$
$\mathbb{P}(X = 0)$	73.6%	62.9%
$\mathbb{P}(X = 1)$	24.5%	32.0%
$\mathbb{P}(X = 2)$	1.8%	4.8%
$\mathbb{P}(X = 3)$	0%	0.2%
$\mathbb{P}(X = 4)$	0%	0%

The above distribution would be observed if all groups were either three- or four-person groups. Using the empirical shares of 45% three-person groups and 55% four-person groups observed in

I-18 and I-19, the weighted average of the two distributions is 67.8%, 28.6%, 3.5%, and 0.1% for $x = 0$, 1, 2, and 3, respectively. This corresponds to the "expected shares" in Figure 2.

C. Data Description

C.A. Data Sources

This subsection describes the construction of the variables used in the empirical analysis in additional detail.

Academic outcomes. All grade data comes from LADOK, which is the student administration system used at Lund University.

Parental income. To obtain the data on parents' income, I first retrieve the personal identity numbers (social security numbers) of both parents using the population registry. This is done irrespective of whether the parents have divorced or not.[25] Then, I proceed by using the Tax Agency's data on taxable income for the year during which their children started their studies. This figure includes earned income, but excludes capital gains. In accordance with the Swedish Constitution, both the personal identity numbers and the tax records are publicly available information.

Additional personal data. Using the population registry, it is straightforward to retrieve additional demographic characteristics for our sampled students. In this paper, I use gender and age. The penultimate digit in the 12-digit personal identity number gives the gender at birth, being odd for men and even for women.

Facial attractiveness. I recruit a jury consisting of 74 individuals, where the youngest person is 17 years, and the oldest is 32 years. I proceed by splitting the entire sample of 307 student pictures into two subsamples, so that each face receives, on average, 37 ratings. As described in Section III.B, the raters were recruited so that their personal characteristics with respect to age and gender. Specifically, to match the age of the students when starting the program, most of the jury members were recruited among high school students. To lower the risk of raters recognizing any photo, virtually all of the raters resided outside of Lund. To account for the potential problem of photo quality and clothes impacting jury ratings (Hamermesh et al. 2002), all photos were in color and of similar quality. To the extent possible, formal wear was avoided. In the instructions, jury members were asked to focus specifically on the person's face when providing their ratings. Each rater was paid SEK 100 (approx. USD 10) in cash.

[25] In Sweden, half of all newly separated couples choose joint custody, which is a higher share than in most other countries (Lernstad 2023).

C.B. Experimental Procedure

The following subsection provides some additional details on the design of the field experiment. The experiment consists of an online questionnaire, which comes in two versions. The first 14 questions are equal for both the treatment and control group. [26] This part asks about basic demographic characteristics, including gender, socioeconomy, relationship status, questions about participation in student life, and finally a question about social networks. Additionally, I ask about how often students skip non-mandatory lectures and TA sessions, as well as characteristics that students find important in romantic and non-romantic friends.

The second part is different for the treatment and control groups. Allocation to the two groups is as good as random, and is done as follows. The sampling frame consists of all students from three cohorts: first-semester students at the Introductory Microeconomics course at Lund University (which is mandatory for all Business Administration and Economics students, regardless of intended major), as well as first- and second-year students at the Industrial Engineering program at Uppsala University. In total, the sampling frame consists of approximately 500 students. In all, I received 228 responses, corresponding to a response rate of approximately 45 percent. The response rate is in line with similar studies (Yun and Trumbo 2000). For the Uppsala cohorts, students with surnames beginning in the letters A–K are given a link to the treatment questionnaire (that is, containing beautiful pictures), and those with surnames beginning in L–Ö are linked to the questionnaire with unattractive faces, with Ö being the last letter of the Swedish alphabet. Thus, the group exposed to unattractive faces constitutes the control group. For larger Lund cohort, those with surnames A–K are the control group, and those with surnames L–Ö are the treatment group. There was no indication that the response rates differed between the treatment and control groups.

The treatment group are shown ten faces, five male and five female, widely perceived as beautiful. For the treatment group, I use pictures of the following individuals, presented in the same order in which they appear in the survey: American influencer Cameron Dallas, Hungarian model Barbara Palvin, Swedish model Elvir Aljicevic, South African model Candice Swanepoel, English actor Robert Pattinson, Swedish Olympic high jumper Maja Nilsson, Italian model and researcher Pietro Boselli, Swedish musician Julia Kedhammar, English musician Harry Styles, and Swedish influencer Timea Sigården. For the control group, I again use ten faces, namely those of American actor DJ Qualls, Norwegian actress Ina Svenningdal, English football (soccer) player Wayne Rooney, British royal family member Louise Windsor, American basketball player Frank Kaminsky, German political activist Carola Rackete, Croatian football player Ognjen Vukojević, American comedian Rosie O'Donnell, Spanish football player Iván Campo, as well German politician Annegret Kramp-Karrenbauer.

The pictures used in the field experiment are from individuals who are, at lest to some degree, public figures. A potential problem could arise if raters associate the pictures with the achievements of those depicted, and not with their appearances. To avoid any systematic differences between treatment and control groups in this respect, two people in each category (Robert Pattinson and Harry Styles in the attractive group, and Wayne Rooney and Ina Svenningdal in the non-attractive group) are plausibly identified by most raters, while the remaining

[26] Online Appendix D gives the exact formulation of the questions.

are likely not. Hence, a majority of pictures are of public figures that cannot be described as "celeberties", and thus, are unknown to most raters.

Finally, the last part of the survey is identical for treatment and control groups. Here, I ask about the student's dream sector, estimated starting salary, whether the individual would like to become a manager in the future, as well as a number of questions about the student's current situation. In the latter category, students are asked to estimate on a five-point Likert scale their subjective probability of passing the upcoming exams, whether university grades are important, and generally, how the student view their coming work lives, as well her well-being. Online Appendix D provides the exact wording of the questions. Online Appendix E presents the full results for each question.

C.C. Socioeconomic Status Index

It can be difficult for students participating in the survey to precisely estimate their parents' income. To tackle this issue, I construct a socioeconomic status index (cf. Mehic and Olofsson 2022) based on four questions in the survey, each asking the respondent to state, respectively: (i) in which municipality he or she lived just before starting university, (ii) in what type of dwelling he or she mainly lived during childhood, (iii) whether any of their parents has a college or university degree, and whether (iv) anyone of their parents has been the CEO or a board member of a publicly listed company during the lifetime of the respondent.

Respondents from more affluent municipalities receive a higher score, where the score is proportional to the median disposable income of the municipality. Similarly, respondents who grew up in a house receive a higher score than those living in rental apartments during their childhood, as do respondents for which both parents have a college degree. Finally, respondents where at least one parent has been the CEO or board member of a publicly listed company receive a higher score.

To construct the index, I denote questions by $j = 1, \ldots, 4$, and sum the numerical scores obtained in each question to form the *Socioeconomic status index* for student i as

$$\text{Socioeconomic status index}_i = \sum_{j=1}^{4} \text{Score}_{ij} \tag{3}$$

To facilitate interpretation, I standardize the index so that its sample mean is equal to zero and its sample standard deviation is equal to unity. Hence, the higher the z-score associated with the respondent's socioeconomic status index, the higher is the socioeconomic background of the respondent.

D. Questionnaire

The following section describes each question used in the survey in additional detail.

1. What is your gender?

- ☐ Male
- ☐ Female
- ☐ Other

2. Does anyone of your parents have a college or university degree?

- ☐ Yes, both.
- ☐ Yes, but only one parent.
- ☐ No

3. When growing up, in what type of dwelling did you mainly live?

- ☐ House
- ☐ Housing cooperative
- ☐ Rental apartment
- ☐ Townhouse

4. Has anyone of your parents been the CEO or a board member of a publicly listed company?

- ☐ Yes
- ☐ No
- ☐ Don't know.

5. In which municipality did you live at the time of your graduation from high school?

---

6. What is your current relationship status?

- ☐ Single
- ☐ In a relationship
- ☐ It's complicated...

7. Are you active in any of the following? You may choose more than one option.

- ☐ Student union
- ☐ Nation
- ☐ Other student society (theater, choir, etc.)

☐ Sports club
☐ Political or religious organization

8. On a scale from 1 to 5, how active are you in student life? Here, 1 means not active at all, and 5 means very active.

9. On a scale from 1 to 5, how many of your friends know each other? Here, 1 means that none of my friends know each other, and 5 means that almost all of my friends know each other.

10. How large a share of the lectures do you attend on a typical course?

☐ 0–50%
☐ 51–70%
☐ 71–90%
☐ Above 90%

11. How large a share of the exercise sessions do you attend on a typical course?

☐ 0–50%
☐ 51–70%
☐ 71–90%
☐ Above 90%

12. How often do you contact lecturers/TAs about questions about course material that you have not fully grasped?

☐ Several times per course
☐ Once or twice per course
☐ Almost never

13. How important do you find the following traits in a partner (boyfriend/girlfriend)?

Trait	Very important	Fairly important	Neither important nor unimportant	Not so important	Not at all important
Personality	☐	☐	☐	☐	☐
Appearance	☐	☐	☐	☐	☐
Humour	☐	☐	☐	☐	☐
Intelligence/Education	☐	☐	☐	☐	☐
Fitness	☐	☐	☐	☐	☐
Income	☐	☐	☐	☐	☐
Same interests as me	☐	☐	☐	☐	☐

14. How important do you find the following traits in a friend?

Trait	Very important	Fairly important	Neither important nor unimportant	Not so important	Not at all important
Personality	☐	☐	☐	☐	☐
Appearance	☐	☐	☐	☐	☐
Humour	☐	☐	☐	☐	☐
Intelligence/Education	☐	☐	☐	☐	☐
Fitness	☐	☐	☐	☐	☐
Income	☐	☐	☐	☐	☐
Same interests as me	☐	☐	☐	☐	☐

15. Rate the facial attractiveness of the following individual, where 1 is extremely unattractive, and 10 extremely attractive.

[Treatment: Picture of Cameron Dallas. Control: Picture of DJ Qualls.]

16. Rate the facial attractiveness of the following individual, where 1 is extremely unattractive, and 10 extremely attractive.

[Treatment: Picture of Barbara Palvin. Control: Picture of Ina Svenningdal.]

17. Rate the facial attractiveness of the following individual, where 1 is extremely unattractive, and 10 extremely attractive.

[Treatment: Picture of Elvir Aljicevic. Control: Picture of Wayne Rooney.]

18. Rate the facial attractiveness of the following individual, where 1 is extremely unattractive, and 10 extremely attractive.

[Treatment: Picture of Candice Swanepoel. Control: Picture of Louise Windsor.]

19. Rate the facial attractiveness of the following individual, where 1 is extremely unattractive, and 10 extremely attractive.

[Treatment: Picture of Robert Pattinson. Control: Picture of Frank Kaminsky.]

20. Rate the facial attractiveness of the following individual, where 1 is extremely unattractive, and 10 extremely attractive.

[Treatment: Picture of Maja Nilsson. Control: Picture of Carola Rackete.]

21. Rate the facial attractiveness of the following individual, where 1 is extremely unattractive, and 10 extremely attractive.

[Treatment: Picture of Pietro Boselli. Control: Picture of Ognjen Vukojević.]

22. Rate the facial attractiveness of the following individual, where 1 is extremely unattractive, and 10 extremely attractive.

[Treatment: Picture of Julia Kedhammar. Control: Picture of Rosie O'Donnell.]

23. Rate the facial attractiveness of the following individual, where 1 is extremely unattractive, and 10 extremely attractive.

[Treatment: Picture of Harry Styles. Control: Picture of Ivan Cámpo.]

24. Rate the facial attractiveness of the following individual, where 1 is extremely unattractive, and 10 extremely attractive.

[Treatment: Picture of Timea Sigården. Control: Picture of Annegret Kramp-Karrenbauer.]

25. In what sector would you like to work in the future?

- ☐ Private company
- ☐ International organization
- ☐ Public sector

26. How high do you think your starting salary[27] will be?

- ☐ Below SEK 30,000
- ☐ SEK 30,000–35,000
- ☐ SEK 35,000–40,000
- ☐ SEK 40,000–45,000
- ☐ Above SEK 45,000

27. Can you imagine being a manager in the future?

- ☐ Yes
- ☐ No
- ☐ Not sure

28. On a scale from 1 to 5, how is your view on your upcoming exams? 1 means that it will be difficult to pass them, and 5 means that I will easily pass.

29. On a scale from 1 to 5, how important do you think that grades are for your education? 1 means that they are totally non-important, and 5 means that they are very important.

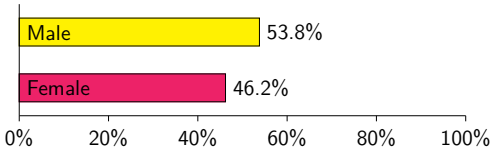
30. On a scale from 1 to 5, how do you view your future professional life? 1 means "very pessimistically", and 5 means "very optimistically".

31. On a scale from 1 to 5, how do you feel right now? 1 means "I do not feel well at all", while 5 means "I feel very well."

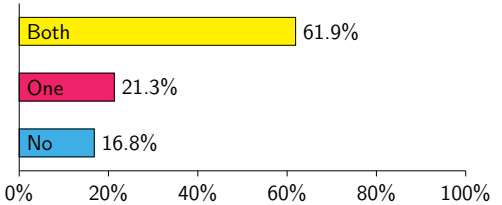
[27] It is implicitly assumed that the figures are monthly salaries before tax.

E. Survey Results

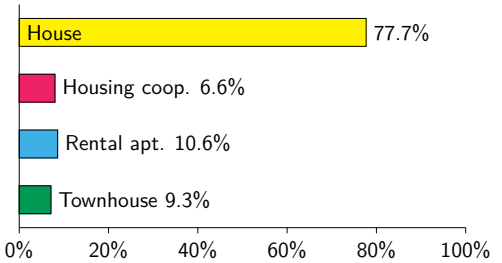
1. What is your gender?



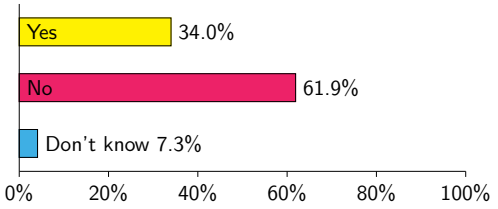
2. Does anyone of your parents have a college or university degree?



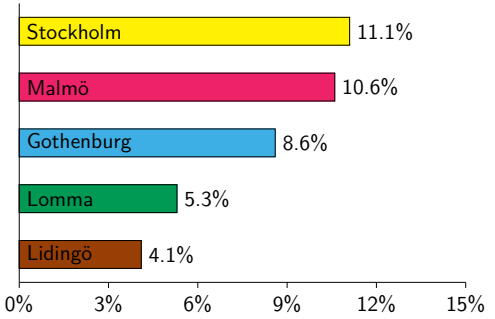
3. When growing up, in what type of dwelling did you mainly live?



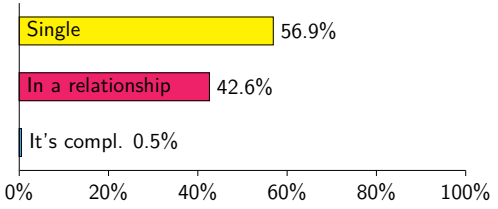
4. Has anyone of your parents been the CEO or a board member of a publicly listed company?



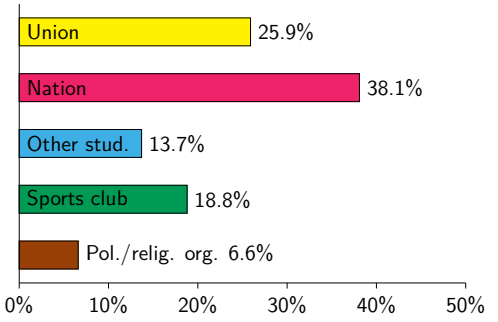
5. In which municipality did you live at the time of your graduation from high school?
   Five most prevalent municipalities:



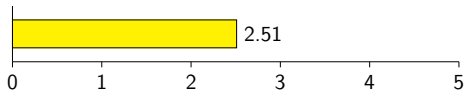
6. What is your current relationship status?



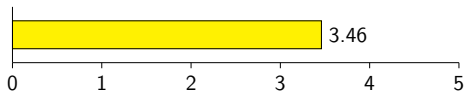
7. Are you active in any of the following? You may choose more than one option.



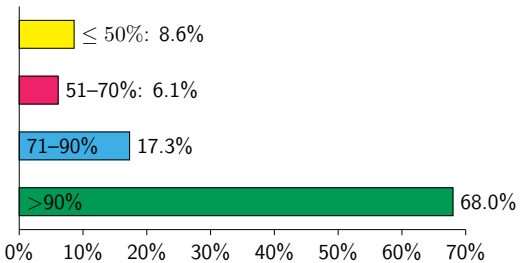
8. On a scale from 1 to 5, how active are you in student life? Here, 1 means not active at all, and 5 means very active.



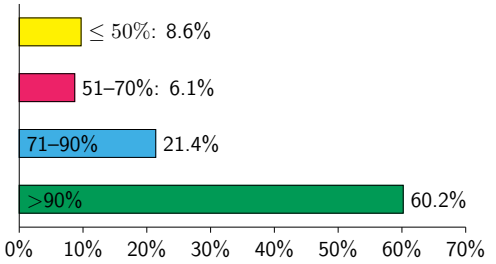
9. On a scale from 1 to 5, how many of your friends know each other? Here, 1 means that none of my friends know each other, and 5 means that almost all of my friends know each other.



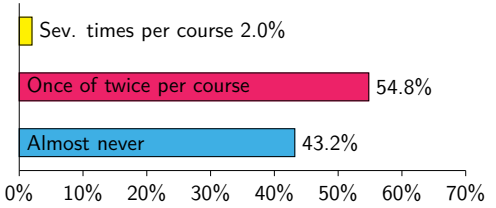
10. How large a share of the lectures do you attend on a typical course?



11. How large a share of the exercise sessions do you attend on a typical course?



12. How often do you contact lecturers/TAs about questions about course material that you have not fully grasped?



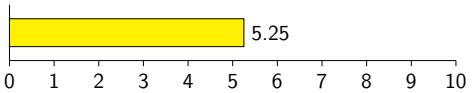
13. How important do you find the following traits in a partner (boyfriend/girlfriend)? *Note.* All values in percent. All rows may not sum to 100 due to rounding.

Trait	Very important	Fairly important	Neither important nor unimportant	Not so important	Not at all important
Personality	96.5	3.1	0.5	0.0	0.0
Appearance	11.2	72.1	14.2	2.5	0.0
Humour	62.9	31.5	5.6	0.0	0.0
Intelligence/Education	30.0	56.9	9.1	3.6	0.5
Fitness	3.1	33.0	42.1	18.3	3.6
Income	2.5	19.8	39.6	19.8	9.1
Same interests as me	5.1	54.3	28.9	11.7	0.0

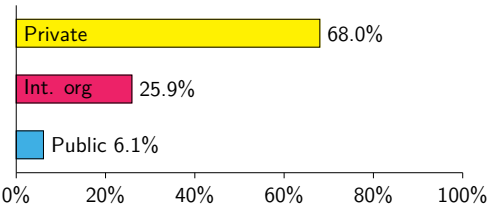
14. How important do you find the following traits in a friend? *Note.* All values in percent. All rows may not sum to 100 due to rounding.

Trait	Very important	Fairly important	Neither important nor unimportant	Not so important	Not at all important
Personality	85.8	12.1	1.0	1.0	0.0
Appearance	0.0	3.1	19.9	28.1	50.0
Humour	58.9	36.0	4.6	0.0	0.5
Intelligence/Education	8.1	41.6	32.0	10.2	8.1
Fitness	0.5	5.1	28.4	17.4	56.6
Income	0.0	5.1	25.4	19.8	49.8
Same interests as me	23.9	57.9	14.7	2.5	23.9

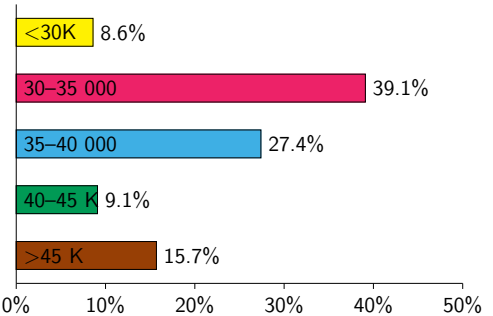
15–24. Rate the facial attractiveness of the following individual, where 1 is extremely unattractive, and 10 extremely attractive. *Note*: Face-specific ratings available upon request.



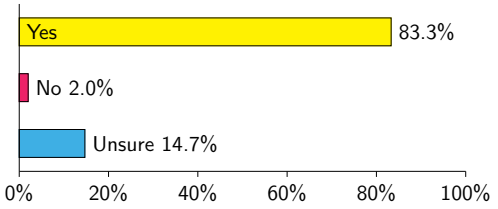
25. In what sector would you like to work in the future?



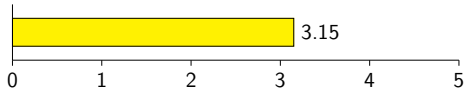
26. How high do you think your starting salary will be?



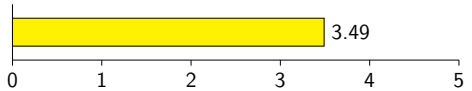
27. Can you imagine being a manager in the future?



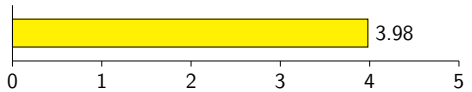
28. On a scale from 1 to 5, how is your view on your upcoming exams? 1 means that it will be difficult to pass them, and 5 means that I will easily pass.



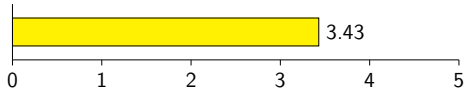
29. On a scale from 1 to 5, how important do you think that grades are for your education? 1 means that they are totally non-important, and 5 means that they are very important



30. On a scale from 1 to 5, how do you view your future professional life? 1 means "very pessimistically", and 5 means "very optimistically".



31. On a scale from 1 to 5, how do you feel right now? 1 means "I do not feel well at all", while 5 means "I feel very well."



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