The Long-Term Impacts of Mixing the Rich and Poor: Evidence from Conscript Dorms *

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Abstract

To what extent is economic success determined by with whom individuals interact socially? We tackle this question by exploiting a large-scale natural experiment in the Finnish conscription to estimate the effects of peers from different family backgrounds on long-term economic and educational outcomes. Our research design is based on the alphabetization of dorms within squadrons, which is shown to induce as good as random variation in peer composition. Using data on more than 50 thousand conscripts, we find that exposure to a dormmate from a high-income family has a positive impact on earnings, with the largest effect among individuals from high-income families. For them, a one-standard-deviation increase in parental income of dormmates increases long-term earnings by around 5.7%. Effects on earnings of individuals from poorer families are small. The results support labor market networks among the rich as the key mechanism. The findings imply that social sorting reinforces wage inequality between richer and poorer families.

Economic theories of social interaction emphasize the importance of social factors as determinants of economic outcomes. Social contacts may share information, aspirations, or behavioral norms with their peers. Importantly, the strength of these peer effects may depend on social sorting, whereby rich families interact more with other rich families and less with those who are poor. For example, if social interaction with the rich provides larger positive long-term earnings gains than interaction with the poor, then social sorting may reinforce economic inequality. Furthermore, conforming with the educational preferences of one's social reference group may lead to persistent poverty traps because some individuals from poorer

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families may acquire too little education compared to what would be intrinsically optimal for them (Bernheim, 1994; Akerlof, 1997). Exposure to high-income peers may also affect individuals differently. It could be that rich individuals benefit more from exposure to rich peers while the poor benefit less. Such heterogeneity would mean that policies that increase social contacts between the rich and poor may have limited scope for improving the economic prospects of the poor.

Theoretically, a combination of social sorting and social interactions can lead to persistent economic inequality (e.g., Montgomery, 1991; Durlauf, 1994; Bowles, Loury, and Sethi, 2014). Despite the potentially high economic and societal importance, causal evidence on the magnitude of social exposure effects generated by interactions between the rich and poor is scarce and even less is known about their persistence over time or mechanisms through which they shape educational and economic outcomes in the long run. In this paper, we examine these questions by studying the consequences of social exposure to peers from families with varying levels of income among more than 50 thousand dormmates conscripted into the Finnish military.

The Finnish conscription provides an exceptional opportunity to assess the long-term effects of social interaction between peers from richer and poorer families for two reasons. First, conscription service is mandatory for all men and around 80% of male cohorts relevant for our analysis serve in the military. Second, conscripts are assigned to live in dorms that mix individuals from a wide range of socioeconomic backgrounds. These features mean that the conscription exposes thousands of individuals from different family backgrounds to social interaction with each other. The analysis is made possible by our access to data in the Conscript Registry of the Finnish Defence Forces (FDF), which include administrative information on dorms and squadrons in which conscripts serve. We merge the data with a rich population panel data set on conscripts and their parents that allow us to follow conscripts' labor market and educational outcomes up to over 20 years after military service.

We focus on parental income as the primary measure of socioeconomic background and identify causal effects by exploiting variation stemming from the alphabetization of dorms within squadrons. As a result of the alphabetical dorm assignment, conscripts who are next to each other in their squadron's alphabetical ordering are likely to be allocated to the same dorm. We show that conditional on one's parental income and squadron fixed effects, the alphabetization generates as good as random variation in parental income of dormmates.

The first part of our analysis examines the effect of dormmates' socioeconomic background on earnings. We find that when a conscript is assigned a dormmate from a high-income family because of the alphabetical assignment rule, his earnings are higher in the long run. Our results suggest that when parental income of dormmates increases by one standard deviation (around €30k or \$33k), earnings at age 28-42 increase by around 3.1%. This effect is driven by gains among individuals who come from families with income above the median. For them, a one-standard-deviation increase in parental income of dormmates increases earnings by around 5.7% in the long run. This means that their discounted value of earnings until retirement increases by around €45k (or \$50k). The effect of dormmates' socioeconomic background on earnings of individuals from low-income families is economically small and statistically insignificant.

In the second part of our analysis, we examine the extent to which the effect on earnings is explained by changes in hourly wages, work hours, and employment. To do this, we amend our data with an extensive hourly wage database based on employers' human resources (HR) registers, covering around 63% of the conscripts in our sample. We find that exposure to a dormmate from a high-income family has a positive effect on hourly wage but affects employment and work hours only a little. The effect on hourly wages is of a similar magnitude as the effect on earnings and it is also driven by gains among individuals from high-income families.

The third part of our analysis examines the potential long-term mechanisms. We begin by assessing the effects on human capital. For individuals from high-income families, we find no evidence of dormmates' socioeconomic background affecting the quality or level of education programs in which they enroll. We also do not find effects on their study effort (measured by course credits) or long-term educational attainment. Conversely, individuals from low-income families enroll in education programs with higher expected economic returns when they are exposed to dormmates from higher-income families. While these effects are statistically significant, their magnitudes are modest. These findings suggest that individuals from low-income families benefit from interaction with peers from high-income families in terms of education, but the effects do not appear to be large enough to generate statistically detectable earnings gains in the long run. The findings also indicate that the positive long-term peer effects among individuals from high-income families are not explained by educational decisions, perhaps because baseline educational attainment is already at a high level in this group and they may be better informed about requirements of and returns to education programs by their families and acquaintances.

Economically beneficial labor market connections offer an alternative explanation for the earnings gains among individuals from high-income families (e.g., Montgomery, 1991; Granovetter, 1995). We examine this hypothesis by linking conscripts to their employers in a longitudinal employer-employee database. We find that persistent long-term social networks among individuals from high-income families are a key mechanism generating the positive peer effects on their earnings and wages. In particular, conscripts from high-income families who reside in the same dorm because of the alphabetical assignment rule are more likely to work in the same company several years after service. On the contrary, individuals from low-income families are not more likely to work in the same company with their dormmate in the long run, regardless of whether the dormmate is from a richer or poorer family background.

The main contribution of our study is twofold. First, we estimate the long-term causal effects of peers in a natural experiment where a large number of individuals from different family backgrounds are assigned to reside together. The obligatory nature of the conscription and FDF's dorm assignment practices generate a rare situation in which individuals from richer and poorer families are forced to live and train together over a prolonged period. This setting allows us to identify social exposure effects across an exceptionally wide family income range. Our analysis provides answers to the question of which groups benefit and which lose when individuals from richer and poorer families are socially integrated. We show that exposure to peers from high-income families economically benefits those who also come from high-income backgrounds, whereas it has little impact on the long-term economic outcomes of those who come from low-income backgrounds, with statistically significant effect heterogeneity across these groups. Second, we assess the importance of several potential mechanisms that could explain these findings. In particular, our results suggest that the lack of persistent labor market connections across the boundaries of socioeconomic groups appears to be a key explanation for why individuals from poorer families benefit little from exposure to peers from richer families in the long run.

Our study relates to several bodies of literature. First, it is linked to literature that has identified peer effects in causal settings where adults live in close social interaction with each other (Sacerdote, 2001; Carrell, Fullerton, and West, 2009; Carrell, Sacerdote, and West, 2013; Dahl, Kotsadam, and Rooth, 2021; Michelman, Price, and Zimmerman, 2022).[1] We complement this literature by providing causal evidence of the effects of social integration on long-term economic and educational outcomes in a setting where a large number of individuals representative of entire male cohorts and an exceptionally wide range of family incomes are mixed to live and train together. Second, this study is relevant for the body of research that has examined the existence and economic importance of labor market networks (Marmaros and Sacerdote, 2002; Bayer, Ross, and Topa, 2008; Hellerstein, McInerney, and Neumark, 2011; Schmutte, 2015; Zimmerman, 2019) and the effects of referrals on workers' wages (Burks et al., 2015; Brown, Setren, and Topa, 2016). We contribute to this literature by assessing the importance of peers' socioeconomic backgrounds for the formation and persistence of labor market connections in a setting where we observe both the company of employment of all workers and their labor market outcomes several years after the initial peer exposure. Importantly, our setting allows us to examine long-term labor market connections between individuals from low- and high-income families. Third, our study is linked to research on the long-term impacts on economic outcomes of policies that may increase social contact between adults from different socioeconomic backgrounds, such as housing policies that move individuals to better neighborhoods (e.g., Ludwig et al., 2012, 2013; Chetty, Hendren, and Katz, 2016). We provide causal evidence of the consequences of increased social exposure of individuals from poorer families to those from richer families, which is one potential channel through which many such policies could improve the economic outcomes of individuals from less-advantaged backgrounds. Fourth, our study is related to literature on the consequences of intra-group contact that suggests social integration of individuals from different backgrounds

[1] Gould, Lavy, and Paserman (2009), Black, Devereux, and Salvanes (2013), and Carrell, Hoekstra, and Kuka (2018) examine the long-term impacts of peer background among primary school students. Chetty et al. (2022a) use data on social connections in a large social media platform to examine the importance of cross-group connections for income mobility.

tends to improve social cohesion in the short run in settings with lower stakes (e.g., Allport, 1954; Rao, 2019; Mousa, 2020; Lowe, 2021). Methodologically, our study is closest to Rao (2019), who employs exogenous variation in peer background arising from the alphabetical assignment of poor students to study groups in elite schools in Delhi.

The paper is organized as follows. Section I summarizes the institutional features of the Finnish conscription. Section II describes data sources and reports summary statistics. Section III discusses the research design and reports results verifying its validity. The main results are presented in Section IV, and Section V examines the mechanisms. Section VI concludes.

I Conscription and military service in Finland

According to the Constitution of Finland, every Finnish citizen is obligated to participate in national defense. Conscription service is compulsory for all men, whereas women serve on a voluntary basis. Conscripts have two alternative ways to serve: they can complete either military or civil service. Around 80% of the male cohorts relevant for our analysis served in the military.

Every 18-year-old man is required to register for service. The first step in the conscription process is a call-up event where conscripts complete a questionnaire that assesses their cognitive and mental suitability for military service. Conscripts also go through a basic health check by military medical staff and an interview with a recruitment officer, in which they can express their desired military branch and service location. Based on the results of these assessments, conscripts are allocated to a branch and service location. A conscript who has enrolled in an education program may postpone the start of service. In addition, a small fraction of conscripts is exempt from service due to not being suitable for it. Women can serve voluntarily, but their fraction is small, around 1% in our data.

Military service begins with a basic training period, the duration of which is around six weeks. The purpose is to prepare and select the conscripts for the second phase of service, during which they go through a more specialized training. The third phase of their service aims to develop skills required to operate as a part of a task force and on joint operations of branches (FDF, 2022).

The total length of the military service depends on a conscript's branch and assignment. It has also changed over the years. From July 1998 onward, the shortest service time has been 165 days for conscripts who operate as infantrymen or in other basic tasks. Conscripts who undertake training for specialized tasks have served up to 270 days, whereas those who undertake reserve officer training have served up to 362 days. Before July 1998, these service times were 240, 285, and 330 days, respectively.[2]

Military training is organized in management units *(perusyksikkö)* led by senior command personnel. Management units belong to a branch and typically undertake tasks of that branch. Within management units, conscripts are allocated to squadrons and dorms, which follow a hierarchical structure. Squadrons within a management unit can undertake similar tasks or have some degree of specialization, and this may vary across management units. For this reason, we will focus in our analysis on within-squadron variation in exposure to dormmates from different family backgrounds and control for selection of conscripts at the squadron level with squadron fixed effects. Another advantage of our focus on squadrons is that the alphabetical dorm assignment rule, which we use as a natural experiment to account for confounding peer selection, is the strongest at the squadron level. This is because squadrons are typically divided to teams, which members are allocated to the same dorm.

In our sample, the mean dorm size is around eight, the mean squadron size around 19, and the mean management unit size around 100 conscripts. Conscripts in a squadron are allocated to three dorms, on average. For practical reasons, the dorm and bunk allocation follows typically the alphabetical rule (Section III.B provides details). Dormmates share living quarters, train together, and undertake tasks as a team, which exposes them to intensive social interaction with each other. The only exceptions are evenings at the garrison, when they are given free time (for example, to go to a canteen or gym), and some weekend vacations. Our dorm setting can be expected to provide strong variation in social exposure across dorms because dormmates spend most of their service time together, living together in the same room and carrying out training tasks together, while exposure between conscripts living in different dorms is limited.

[2]The duration of the non-military civil service has been from 347 to 395 days, which is considerably longer than the basic training in the military. This might be one explanation for the high fraction of men who undertake the military service.

II Data

II.A Data sources and variable definitions

Our analysis is made possible by our access to data in the FDF's Conscript Registry (CR). The CR data include information on the last dorm, last squadron, and management units in which a conscript has served. The CR also provides information on the start year and wave (there are two waves annually, one in early January and one in early July). Our data cover conscripts who started service between 1996 and 2006.

We merge the CR with population panel data covering the years 1994-2016, maintained by Statistics Finland (SF). Both the CR and population panel include the same unique identifier for each individual (social security numbers), which allows an exact match between the data sets. The population panel is based on tax and other administrative registers and includes information on demographic, economic, and education characteristics (including age, gender, income, number of days employed, and educational attainment), as well as child-parent links, which allows parent characteristics to be linked to conscripts. We next discuss the key variables constructed from the data.

Dorms and squadrons. We define conscripts in a squadron as conscripts who served in the same last squadron in the same wave. Similarly, we define dormmates as those who served in the same last dorm in the same wave. We observe dorms for more than 50 thousand individuals. Although the overall coverage is not perfect (around 25% of CR records), the data provide accurate information on dorm assignments at the management unit and squadron level because units that record the dorm code typically record it for all of their squadrons, dorms, and conscripts.[3] The CR does not include information on the length of the last dorm assignment, but time spent in the last management unit is available. In our sample, around 96% of conscripts spent at least 60 days in the last management unit.[4] This provides a reasonably reliable lower bound for the duration of peer exposure in the last dorm because

[3]Recording the dorm code in the common IT system is not mandatory for all management units and therefore information on it is available only for a subset of units.

[4]The majority of conscripts spend their whole service in the same management unit (52%). Seventy-one percent of them serve in 1-2 and 90 percent in 1-3 management units. Dormmates typically exit service around the same time. We also note that of dorms that include at least one female conscript, 58% have women as the majority, indicating some degree of gender segregation. This has little implications for our analysis, as women comprise only a small fraction of the conscript population.

unit and dorm assignments change little during the last months of service. We control for squadron fixed effects throughout our analysis to account for the potential selection of conscripts to the last squadron by unobserved characteristics.[5]

Parental income. Parental income is linked to conscripts with child-parent links, drawn from the national Population Information System. Our primary measure of income includes wage earnings, self-employment earnings, pension income, and capital income, as well as taxable social security benefits. We also include housing allowances, which are a major non-taxable benefit. We impute zeros for missing values of these variables and calculate income as their sum.[6] All income is measured prior to the deduction of taxes. Parental income is the sum of a conscript's parents' income. To reduce the influence of transitory shocks, we use the average parental income over three years before service.

Hourly wages and work hours. We draw information on hourly wages and work hours from the SF Structure of Earnings database. The SF compiles private sector wage data from its surveys as well as surveys conducted by employer organizations. Public sector wages are based on HR registers of public institutions and cover the entire public sector workforce. The data have some important advantages. First, the information covers an exceptionally large fraction of the workforce in Finland. We observe hourly wages and work hours for around 63% of the conscripts in our baseline sample. Second, because the data are based on HR registers of employers, wage information is not affected by the potential response bias that can be a concern in surveys targeting employees directly. Third, the surveys employ employer-level sampling, meaning that wages and hours are available for all employees within a respondent employer. This allows us to use average hourly wages by employer as outcomes in some extensions of our baseline regressions.

Completed years of education. Our primary measure of completed years of education is based on the highest degree an individual holds. We estimate the average time taken to complete an educational degree from administrative data drawn from the SF Education Register, which covers all Finnish students at the secondary (high school and vocational) and

[5]We note that controlling for squadron fixed effects is equivalent to controlling for squadron-by-wave fixed effects because conscripts in a squadron are in the same wave.

[6]Information on these variables is from the tax and social security records, which have full coverage of the working-age population. Thus, missing values can be treated as true zeros.

tertiary (university) levels. We calculate the duration of study for each completed degree in the Education Register from 1999 to 2016 as the difference between the start and graduation dates. The average time to complete a degree is then calculated as the mean of this variable by the first three digits of the education code (level, field, and subfield of the education program) and linked to conscripts by the first three digits of the degree code for the highest observed degree, which follows the same classification as the education program. For all individuals, we add nine years of schooling for compulsory primary education.[7] For degrees that require a high school diploma, we add three years, which is the standard length of high school studies. As an alternative outcome for educational attainment, we use a binary indicator for a university degree.

The data set also includes a conscript's alphabetical rank in the conscript population by family name and forenames (names are confidential and not included in the data set). We use this variable to construct alphabetical ranks for each conscript within his squadron and dorm. All monetary variables are deflated to 2012 with the CPI. To reduce the effect of outliers, we cap monetary variables at the upper tail of the distribution to the 99.5th percentile. Hourly wages are also winsorized to half of the minimum wage threshold of the national unemployment benefit scheme at the lower tail of the distribution.[8]

II.B Sample selection and summary statistics

In our analysis, we focus on the outcomes of conscripts who begin service at age 18 to 22, excluding those who begin service voluntarily at age 17 or postpone service to age 23 or later (together, their fraction is around 4.3%). We also exclude conscripts who have siblings serving in the same squadron (0.2%) and conscripts for whom no parent is observed (0.4%). However, we do not impose these restrictions when we measure the characteristics of one's dormmates in order not to induce measurement error in peer variables. Our baseline sample includes 50,578 conscripts for whom the dorm assignment is observed, residing in 6,756 dorms.

We next summarize some key summary statistics in the baseline sample (see Appendix Table A1 for a full set). Conscripts in our sample begin service at age 19.7, on average.

[7] In Finland, children are obligated by law to attend nine years of primary school.

[8] We use this measure as a lower bound for hourly wages because Finnish law does not define a general minimum wage.

Most men serve at ages 19 (34%) and 20 (58%) and there is little variation in service age distributions between individuals from low- and high-income backgrounds in our data (Appendix Table A2). Around 42% were employed during the year before service, with average annual earnings of around 5,000 euros (part-time jobs, especially during the summer holiday season, are typical at this age). The average completed years of schooling was 10.43, with very few having a university degree, which is expected in this age group. The average parental income was 59,960 euros, with a standard deviation of around 30 thousand euros.

Appendix Figure A1 demonstrates the representativeness of the conscript sample in terms of parental income. The figure shows the income distributions for conscripts' parents and the full Finnish population at age 35-65, separately for men and women. The distributions have very similar supports for both men and women, meaning that conscripts' parental backgrounds are well representative. The shapes of the distributions are also very similar. The population distribution shows larger mass in the income range of around 10-25 thousand euro, which disappears when pensioners are excluded. The fact that conscription collects together individuals from all parts of the income distribution is an important feature that enables us to assess the effects of exposing individuals from poorer and richer family backgrounds to social interaction with each other. We demonstrate below that this induces substantial variation in parental income of co-conscripts within squadrons.

III Empirical setting

III.A Challenges for identification

Our aim is to estimate the causal effect of the family background of peers with whom an individual is in close social interaction. We model peer effects by relating post-service outcome Y_{idt} of conscript i residing in dorm d to the average parental income of his dormmates, $\overline{PI}_{(i)d}$:

$$Y_{idt} = \alpha + \gamma \overline{PI}_{(i)d} + \beta_1 PI_i + \beta_2 x_i + \epsilon_i \ . \tag{1}$$

Here, PI_i is income of the conscripts' parents and x_i is a vector of his other predetermined characteristics. A key assumption for interpreting the estimate of γ as a causal peer effect is that, conditional on a conscript's parental income and other control variables, his unobserved characteristics correlated with Y_{idt}, incorporated in the error term ϵ_i, and the average parental income of his dormmates are uncorrelated. In a regression with post-service earnings as the outcome, this assumption is violated if, for example, conscripts with higher unobserved earnings potential, conditional on PI_i and x_i, are assigned dormmates with higher average parental income. Such a sorting would lead to a positive bias in the OLS estimate of γ. Conversely, if assignments within squadron aim to form balanced dorms, this can generate a negative correlation between the conscript's unobserved earnings potential and the average parental income of his dormmates, and the OLS estimate would be negatively biased.[9]

III.B Alphabetical dorm assignment

To identify causal peer effects, we employ variation in dormmates' family backgrounds induced by the alphabetization of dorms within squadrons. Conscripts in the same squadron are often assigned to dorms alphabetically. The alphabetization stems from the need to order the conscripts in a squadron in a logical way. Ordering by the family name, which the senior personnel uses to address a conscript, is a natural choice.

Our correspondence with the FDF indicated that in addition to being a logical choice, alphabetization of dorms is also practical in the daily headcounting process. During the headcount, conscripts stand by their bunks and the on-duty officer moves from dorm to dorm and records present and absent conscripts in a headcount book. To facilitate systematic and efficient counting, conscripts are often assigned in alphabetical order to dorms and listed in the same order in the headcount book. Within dorms, bunks are typically allocated in alphabetical order as well, so that subsequent conscripts in the listing reside in adjacent bunks. This arrangement facilitates efficient headcounting and detection of absent conscripts.[10]

Panel A of Figure 1 shows graphical evidence of alphabetization by plotting the likelihood of residing in the same dorm by the within-squadron alphabetical rank distance between two

[9]The tendency to form balanced or selective dorms by parental income can be described by calculating the ratio of means of absolute difference in parental income between pairs who are in the same dorm and who are in different dorms within a squadron (e.g., Shue, 2013). In our setting, the mean of this squadron commonality rate is 1.02, meaning that, in an average squadron, dormmates are, on average, slightly less similar than conscripts who reside in different dorms. This suggests that in squadrons with selective dorm assignments, the goal is more often to form balanced dorms.

[10]Appendix Figure A2 shows a standard page of an FDF headcount book.

conscripts. The figure employs data for all within-squadron pairs in our sample. Forty-four percent of conscripts who are adjacent to each other in their squadron's alphabetical ranking reside in the same dorm. As the alphabetical rank distance increases, the fraction declines sharply and is 16% for conscripts who are ten alphabetical ranks apart. The fact that the likelihood of residing in the same dorm increases sharply as the alphabetic rank distance between two conscripts decreases indicates that the alphabetic rule is prevalently used in assigning conscripts to dorms within squadrons. This is the second important feature of the conscription that enables us to assess the effects of exposing individuals from poorer and richer family backgrounds to social interaction with each other.

III.C Instrument and empirical model

The alphabetization of dorms within squadrons means that conscripts who are alphabetically more proximate in their squadron's alphabetical ranking are more likely to be assigned to the same dorm. This motivates an IV model that can be expressed with the following two-equation system:

$$\overline{PIalph}_{(i)d} = \alpha_s + \rho\overline{PIalph}_{(i)s} + \theta PI_i + \kappa_1 x_i + \nu_i \tag{2}$$

$$Y_{idt} = \mu_s + \gamma\overline{PIalph}_{(i)d} + \lambda PI_i + \kappa_2 x_i + u_i \ , \tag{3}$$

where the instrument, $\overline{PIalph}_{(i)s} = \frac{1}{M}\sum_{k(i)=1}^{M} PI_{k(i)}$, is the average parental income of M conscripts alphabetically nearest to conscript i in squadron s with $k(i)$ indexing the conscripts by the within-squadron alphabetical rank distance to conscript i. The peer mean, $\overline{PIalph}_{(i)d} = \frac{1}{M}\sum_{j(i)=1}^{M} PI_{j(i)}$, is the average parental income of M dormmates alphabetically nearest to conscript i with $j(i)$ indexing the conscripts by the within-dorm alphabetical rank distance to conscript i. We include squadron fixed effects, α_s and μ_s, and dummies for the year of outcome measurement in all specifications.[11] We also run specifications where we control for the location of the conscript in the population alphabetical ranking and include in x_i a rich set of pre-determined characteristics of the conscript and his parents. We cluster standard errors by squadron, which is the level at which conscripts are quasi-randomly assigned to dorms.

[11] We note that conscripts in a squadron are in the same wave. Therefore, s indexes equivalently squadron-by-wave cells. We also note that separate dummies for wave or start year are redundant.

The model estimates the peer effect γ on a conscript stemming from his M alphabetically nearest dormmates. In our baseline model, we focus on the two alphabetically nearest conscripts. We prefer this specification because alphabetically nearest dormmates are typically assigned to adjacent bunks and are thus exposed to intensive social interaction with each other. It also provides the strongest first stage in the alphabetical setting because when conscripts are next to each other in their squadron's alphabetical ranking, they are most likely to be assigned to the same dorm (Panel A of Figure 1); when they are assigned to the same dorm, they are by construction next to each other in the dorm's alphabetical ranking. This generates a strong first-stage correlation between the average parental income of the conscript's two alphabetically nearest conscripts in the squadron and the average parental income of his two alphabetically nearest dormmates. We also report results for all dormmates and for dormmates who are more distant in the alphabetical ranking.

Because the incomes of peers' parents are measured and realized before the conscripts enter service, the estimate of the peer effect γ is not affected by the reflection problem, which arises in situations where the measure of peer characteristic is affected by peer interaction (Manski, 1993). Furthermore, because parental income is based on administrative records and measured with high accuracy, our estimates are unlikely to be affected by measurement error in the peer variable, which could attenuate peer effect estimates in quasi-experimental settings (Feld and Zölitz, 2017). We also note that controlling for own characteristic in a peer regression is a standard practice to eliminate the mechanical correlation between it and the peer mean (Angrist, 2014).[12]

Exogeneity of the instrument. Throughout the analysis, we control for squadron fixed effects (α_s and μ_s) that account for the potential selection of conscripts by unobserved characteristics at the squadron level. Including a term for parental income in the regression means that our estimation employs variation in peer composition across individuals with the same level of parental income. In our baseline model, we also control for the location of the conscript in the alphabetical ranking by adding dummies for each tenth of a population

[12]This mechanical correlation is likely to be negative in (quasi-) random peer group settings (Angrist, 2014). We show below that such a negative correlation is also present in our data. As a result, because parental income is correlated with many of the outcomes of interest, omitting it from the peer regression would lead to an omitted-variable bias in the estimates of peer effects. We quantify this bias in Section IV.A.

alphabetical rank percentile. Consequently, alphabetical clustering of conscripts by their parents' income does not induce bias in our estimates of γ.

The key identifying assumption is that conditional on an individual's parental income and squadron fixed effects, the alphabetical assignment generates as good as random variation in who is whose close dormmate. We provide support for this by showing that the instrument is uncorrelated with predetermined characteristics of a conscript and his parents. We also show that excluding cases where a conscript and a peer are proximate in the population's alphabetical ranking does not affect the results. This refinement of the alphabetical design is facilitated by the fact that conscripts who are alphabetically close to each other in their squadron's alphabetical ranking are often relatively far away from each other in the population's alphabetical ranking. These specifications provide further support for the robustness of the design against alphabetical clustering by unobservable characteristics.[13]

Exclusion restriction. To interpret the estimate of γ as the causal effect of the two alphabetically nearest dormmates, we must assume that the parental income of alphabetically nearest conscripts in the squadron affects the conscript's post-service outcomes only because they are assigned to the same dorm, and not because they interact with alphabetically nearest conscripts who reside in other dorms. This assumption is plausible because dormmates are exposed to intensive social interaction with each other and spend the majority of service time training and living together, whereas they have relatively little exposure to those who live in other dorms. We also show that parental income of the alphabetically nearest conscripts in the squadron does not predict outcomes in squadrons within which the alphabetical dorm assignment rule is weak; if there were social spillovers across dorm boundaries, parental income of alphabetically nearest conscripts in these squadron should predict outcomes, irrespective of whether they are assigned to the same dorm.

Relevance of the instrument. Panel B in Figure 1 shows graphically the first-stage relationship that we utilize (ρ in Equation [2]). The figure plots residuals from separate regressions of the average parental income of the two alphabetically nearest conscripts within the squadron and within the dorm on parental income and squadron fixed effects. The

[13] We also show in Appendix Figure A3 that a conscript's alphabetical rank is uncorrelated with his parental income.

regressions are weighted by the number of observations available in the baseline panel when individuals are at age 28-42 to correspond to the panel estimates presented below.[14] The figure shows substantial within-squadron variation in parental income of the two alphabetically nearest conscripts in the squadron and a strong first-stage correlation between it and parental income of the two alphabetically nearest dormmates.[15] The slope in the figure is 0.314 with a standard error of 0.009 (the first-stage F statistic is 1259). This strong first-stage correlation is generated by the prevalence of the alphabetical dorm assignment within squadrons.

III.D Verifying the alphabetical research design

To investigate whether the alphabetical design generates exogenous variation in peer composition, we run reduced-form regressions of pre-service characteristics on the instrument, parental income, and squadron fixed effects.[16] Table 1 shows the results. Each row shows estimates for the outcome specified by the row label. Although parental income is highly correlated with most pre-service characteristics, the average parental income of the two alphabetically nearest conscripts in the squadron is not statistically related to any of them: The coefficient on it is statistically insignificant in all regressions. Panel B shows the results for predicted earnings, where the outcome is the best linear prediction of earnings at age 28-42 based on the pre-service characteristics in Panel A. The correlation between the instrument and predicted earnings is very small and statistically insignificant. These findings provide strong support for the assumption that conditional on parental income and squadron fixed effects, the alphabetization of dorms within squadrons induces as good as random variation in the composition of alphabetically nearest dormmates.

[14] Unweighted regressions provide very similar results (reported below).

[15] The standard deviation of the average parental income of the two nearest conscripts in the squadron, conditional on parental income and squadron fixed effects, is 17.78. Panel C in Appendix Figure A1 shows the distribution.

[16] We note that including parental income in the validation regressions is necessary to account for the mechanical correlation between it and the peer mean (Angrist, 2014). In a regression of own parental income on the average parental income of the two alphabetically nearest conscripts in the squadron and squadron fixed effects, this correlation is -0.14 (Appendix Table A3, Column 3). Section IV.A provides further discussion of the induced omitted-variable bias when own baseline characteristic is excluded from the peer regression.

IV The long-term effects of peers' family backgrounds

In this section, we examine the effects of peers' parental income on earnings and show several robustness tests providing further support for the validity of the research design and interpretation of the results as causal peer effects generated by social interactions within dorms. We then examine the importance of hourly wage, work hours, and employment margins. We also assess heterogeneity of effects by parental income, provide results for alternative definitions of peer groups, and discuss the differences between the IV and OLS estimates.

IV.A Earnings

We start by examining the long-term effects of peers' family backgrounds on earnings. Panel A of Table 2 shows results from an IV regression of earnings at age 28-42 on the average parental income of the two alphabetically nearest dormmates, using the average parental income of the two alphabetically nearest conscripts in the squadron as an instrument and conditioning on parental income, squadron fixed effects, and dummies for the year of outcome measurement (Equations [2] and [3]). The IV estimate of the peer effect is 0.0308 and it is statistically significant ($p < 0.05$). Panels B and C of Figure 1 show the corresponding first-stage and reduced-form coefficients graphically. Together, the graphs provide an illustration of the key components of the IV estimate, which is the ratio of the two coefficients. Panel C also shows graphically the coefficient in Table 1 for predicted earnings based on the pre-service characteristics, which is almost zero and statistically insignificant.

In Panel B of Table 2, we include additional control variables for pre-service characteristics of conscripts' and their parents listed in Panel A of Table 1 and dummies for age in the service start year and in the year of outcome measurement. We control for the location of the conscript in the alphabetical ranking with 1,000 dummies for each tenth of a population alphabetical rank percentile and include the average parental income of other dormmates, who are not the two alphabetically nearest, to ensure that their composition does not affect the results. Overall, adding these control variables has little impact on the estimates. The IV estimate of 0.0333 ($p < 0.05$) means that when the parental income of the two alphabetically nearest dormmates increases by one standard deviation, which is equivalent to around 30 thousand euros (or 33 thousand USD), the conscript's annual earnings will be around 999 euros higher at age 28-42 (a 3.1% increase compared to the sample mean of 32,080 euros). This corresponds to an increase of around 22,880 euros (or 25,168 USD) in the discounted value of earnings until retirement at age 65.[17]

Appendix Table A3 provides additional reduced-form estimates for specifications excluding own parental income as a control variable from the baseline IV model. Because own parental income is positively correlated with a conscript's earnings at age 28-42 and it has a negative mechanical correlation with parental income of the two alphabetically nearest conscripts in the squadron, as can be expected in a (quasi-) random peer group setting (Angrist, 2014), omitting it from the peer regression induces a negative bias in the estimate of peer effect.[18]

Internal validity. In Table 1, we have provided support for the validity of the research design by showing that parental income of peers to whom a conscript is exposed due to the alphabetization is uncorrelated with the conscript's and his parents' background characteristics. In Table 3, we provide additional support for the validity of the alphabetical research design by estimating alternative regression models. Panel A replicates the baseline estimates. In Panel B, we exclude conscripts for whom either of the two alphabetically nearest conscripts in the squadron is less than one percentile from him in the population alphabetical rank distribution. This specification provides further support for our argument that alphabetical clustering by unobserved characteristics is not a concern in our design. If alphabetical clustering generated the positive estimate of the peer effect, this specification should provide smaller estimates than the baseline estimate in Panel A because it employs only variation in peer composition stemming from peers who are relatively distant from the

[17] The increase in the discounted value of earnings is calculated for age 28 through 65 by assuming that the discount rate is 3% and the effect on earnings is equal to the point estimate of 0.0333 over this age interval.

[18] In the baseline model, the coefficient on own parental income is 0.0306 (Column 1 of Panel B in Appendix Table A3), while the mechanical correlation is –0.052 (Column 3). The bias from omitting own parental income from the baseline regression is determined by these two estimates and is $-0.052 \times 0.0306 \approx -0.0016$. The corresponding bias for the specification without additional control variables is larger than this because some of the additional control variables are correlated with own parental income and therefore account partly for its omission. We also note that the correlation between own parental income and parental income of the two alphabetically nearest conscripts in the squadron in a regression without squadron fixed effects is 0.440. This positive correlation is driven by between-squadron variation in parental income which has a standard deviation of 16.7 (Appendix Figure A4 shows the distribution).

subject in the population alphabetical ranking but close to him in the squadron alphabetical ranking. Although the estimated effect from this specification is less precise due to the smaller sample size, it is larger than the baseline estimate and statistically significant.

Panels C and D show results separately for squadrons with strong and weak alphabetical dorm assignment rule. We define squadrons with strong alphabetical rule as those whose within-squadron correlation between the average parental income of the two alphabetically nearest conscripts within the squadron and within the dorm is above the median. If alphabetically nearest conscripts in the squadron affect a conscript's earnings only when he is assigned to the same dorm with them, the reduced-form estimate should be close to zero in the sample of squadrons with weak alphabetization of dorms. However, if there are social spillovers between alphabetically nearest conscripts at the squadron level, irrespective of whether they reside in the same dorm, we would expect to reject the null hypothesis of the reduced-form estimate being equal to zero in this sample. The reduced-form estimate for squadrons with weak alphabetization is small (0.0027; $p = 0.659$) compared to the estimate for squadrons with strong alphabetization (0.0152; $p = 0.025$), and it is statistically insignificant, indicating that social spillovers between alphabetically proximate conscripts are limited at the squadron level when dorm assignment does not follow the alphabetical rule. This result provides strong support for the exclusion restriction that the identified peer effects are generated among conscripts who are assigned to the same dorm as a result of the alphabetical assignment rule, and not due to social contacts across dorm boundaries. In squadrons with weak alphabetization, within which alphabetically assigned dormmates are less common, the alphabetical research design leads to IV estimates with extremely low precision: The IV standard error in this sample is more than 20 times larger compared to squadrons with strong alphabetization.

Panels E and F show that the estimates do not change appreciably when we restrict the sample to individuals at age 30-35 or exclude female conscripts. Panels G, H, and I show that excluding individuals whose primary language is not Finnish and limiting the dorm size to 20 and 10 conscripts provides slightly larger estimates of peer effects compared to the baseline. Only a small fraction of conscripts serve in dorms with more than 20 conscripts, leading to imprecise estimates for this sample (Panel J). Panels K and L show that the results change little when we interact the dummies for each tenth of a population alphabetical rank percentile with the population alphabetical rank and the distance to the nearest peer in the population alphabetical ranking. Finally, Panel M shows that results are very similar when the IV regression is weighted by the inverse of the number of observations available for each conscript at age 28-42, accounting for the larger weight of conscripts who have a larger number of earnings observations available from age 28 onward (for example, those in older conscript cohorts).

IV.B Hourly wages and work hours

We next turn to examine the extent to which changes in hourly wages and work hours explain the identified long-term peer effects on earnings. The results are reported in Table 4. As discussed above, hourly wages and work hours are observed for a restricted sample of conscripts. Therefore, in the first column, we begin by testing whether the instrument induces selection into the wage sample by using the full baseline sample and estimating a reduced-form peer regression with a binary indicator equal to one if hourly wage is observed, and zero otherwise, as the outcome. The coefficient on the instrument is small and statistically insignificant, suggesting that instrument-driven selection is not a concern in our setting.

The second column of Table 4 shows the first-stage coefficient in the hourly wage sample, which is of a similar magnitude as in the baseline sample. The IV estimate of the peer effect for hourly wage is 0.0242 and statistically significant ($p < 0.01$). The estimate means that when parental income of the two alphabetically nearest dormmates increases by one standard deviation, hourly wage increases by around 72 cents (or by 3.6% from the sample mean). With around 163 work hours per month (Column 5) and 315 days of employment per year (Appendix Table A1), the annualized magnitude of the effect on hourly wage of increasing parental income of the two nearest dormmates by one standard deviation (€30k) is $0.0242 \times 30 \times 163 \times 12 \times (315/365) \approx 1,226$ euros. To compare this magnitude to the estimated peer effect on earnings, we use the estimate for earnings in the wage sample, which is 0.0608 with a standard error of 0.0148 (Panel A of Appendix Table A4). This estimate means that when parental income of the two alphabetically nearest dormmates increases by one standard deviation, annual earnings increase by around 1,824 euros. These results together suggest that the estimated peer effect on hourly wages explains $100 \times (1,226/1,824) \approx 67\%$ of the estimated peer effect on earnings.[19] Finally, the effect on work hours is small and statistically insignificant.

IV.C Heterogeneity

We now turn to examining the heterogeneity of peer effects on earnings and wages by parental income. The results are presented in Panels A and B of Table 5. The estimated peer effect on earnings is the largest for individuals who come from high-income families (0.0670; $p < 0.01$). For them, a one-standard-deviation increase in parental income of the two alphabetically nearest dormmates increases annual earnings by around 2,010 euros, or by around 5.7% compared to the mean of 35,070 euros in this group (from Panel C of Appendix Table A1). This effect corresponds to an increase of around €45k (or around $50k) in the discounted value of earnings until retirement at age 65.[20]

We find no statistically detectable impact on earnings of individuals who come from low-income families. The IV estimate for them is positive, but it is small and statistically insignificant. The point estimate of 0.0032 means that a one-standard-deviation increase in parental income of peers increases their earnings by around 0.3% with respect to the mean of 29,100 euros in this group. The difference in the estimated peer effect between individuals from high- and low-income families is 0.0638 and it is statistically significant ($p < 0.05$).

Panel B examines heterogeneity of effects on hourly wages. The estimate for conscripts who come from high-income families is 0.0363 ($p < 0.01$) and implies that when parental income of the two alphabetically nearest dormmates increases by one standard deviation, the hourly wage will be around 1.09 euros higher, or increases by around 5.2% compared to the mean of 20.86 euros in this group. The estimate for conscripts from low-income families is statistically insignificant and implies a relative effect of around 1.4% with respect to the

[19] Appendix Figure A5 presents graphically the first-stage and reduced-form estimates for hourly wage, mirroring Panels B and C in Figure 1. It also shows the coefficient for predicted hourly wage based on the pre-service characteristics, which is small and statistically insignificant (-0.005 with a standard error of 0.004).

[20] Appendix Table A5 reports the corresponding first-stage estimates, which range from 0.30 to 0.32 and are statistically significant at the 1% risk level across specifications. The estimates vary little across samples, indicating that the strength of the first stage is not related to parental income. Appendix Figures A6 and A7 present graphically the reduced-form and first-stage estimates for earnings and hourly wages separately for the low- and high-income groups.

sample mean of 18.70 euros in this group.

Peer effects and the wage gap. We next provide a simple example to illustrate the degree to which the estimated peer effects can explain the wage gap between socioeconomic groups. We consider low- and high-socioeconomic status (SES) peer groups, with the former group including three conscripts whose parents' incomes are in the 10th percentile (27,410 euros) and the latter group including three conscripts whose parents' incomes are in the 90th percentile (96,250 euros). Suppose that a conscript in the high-SES group swaps places with a conscript in the low-SES group. The average parental income of peers of the high-SES mover reduces by 68,840 euros, whereas it increases by the same amount for the low-SES mover. Moreover, the average parental income of peers of the two conscripts who stay in the low-SES group increases by 34,420 euros and reduces by the same amount for the two conscripts who stay in the high-SES group. The point estimates of the peer effect on hourly wages imply that this change in group composition reduces the average hourly wage of the high-SES individuals by $0.0363 \times (-68.840 - 2 \times 34.420)/3 \approx -1.66$ euros and increases it for the low-SES individuals by $0.0090 \times (68.840 + 2 \times 34.420)/3 \approx 0.41$ euros. The net reduction in the average wage differential between these groups is then 2.07 euros, which is around 14% of the 90-10 percentile wage gap of 14.30 euros in the conscript population at age 28-42.[21] Therefore, differences in social exposure to high-SES peers can be a fairly important explanation for wage inequality between high- and low-SES groups. This example also demonstrates that the FDF's dorm assignment practices can have considerable impacts on the long-term economic inequality among conscripted cohorts.

IV.D Other outcomes

Panel D of Table 5 shows results for a binary employment outcome, which is equal to one if a person worked during the year and zero otherwise. We convert this variable to percentages by multiplying it by 100. Panel E shows results for the number of days an individual is employed during the year, which is based on compulsory pension insurance contribution

[21] Alternatively, the calculation can be based on the distribution of dorm mean parental income. If the corresponding moves are made between a low-SES group in the 10th dorm percentile (42,700 euros) and a high-SES group in the 90th dorm percentile (79,900 euros), this would lead to a net reduction in the average wage differential between these groups of around 1.11 euros, which is around 8% of the 90-10 percentile wage gap.

records and available in the population panel from 2005 onward. All IV estimates for these employment outcomes are relatively small and statistically insignificant. Panels F and G show estimates for unemployment benefits and general housing allowance. Consistent with the positive earnings effects, the point estimates for both of these social transfers are negative across samples, with statistically significant estimates at the 5% risk level for both outcomes in the full sample and for housing allowance in the high-parental-income sample. The results are broadly similar for the specification excluding additional control variables from the IV model (Appendix Table A6) and for the sample of squadrons with strong alphabetical rule (Appendix Tables A7).[22]

IV.E Alternative peer specifications

We next turn to results for specifications based on alternative definitions of peer groups. We start with estimates based on an IV model where the peer variable is the average parental income of $K_d - 1$ dormmates of a conscript and the instrument for it is the average parental income of his $K_d - 1$ alphabetically nearest conscripts in the squadron, where K_d is the size of his dorm. Table 6 shows the results.[23] Overall, the results for earnings and hourly wages are consistent with the main specification based on the two alphabetically nearest dormmates, indicating strong heterogeneity of peer effects. The estimates are also of a similar magnitude as for the main specification, although less precise. In the all-dormmates specification, adding to the peer mean 30 thousand euros means that, on average, seven dormmates (one subtracted from the mean dorm size) have 30 thousand euros higher parental income. Converting the estimate for earnings in Column 1 to correspond to an increase in parental income of two dormmates gives $(2/7) \times 0.1286 \approx 0.0367$, which is close to the estimate of 0.0333 for the two alphabetically nearest dormmates. The corresponding magnitudes for hourly wage are $(2/7) \times 0.0817 \approx 0.0233$ and 0.0242, respectively. While the estimate for employment

[22] For squadrons with weak alphabetical rule, the reduced-form coefficients on the instrument are relatively small across samples and outcomes and none of them is significant at the 5% risk level (Appendix Table A9). Only one out of 21 coefficients is significant at the 10% risk level, which could occur by chance due to sampling variance. These results together with the corresponding reduced-form estimates for squadrons with strong alphabetical assignment (Appendix Table A8) provide strong support for the interpretation that the identified heterogeneous peer effects are generated among conscripts who are assigned to the same dorm as a result of the alphabetical assignment rule, and not due to social contacts across dorm boundaries.

[23] In Appendix Table A10, validation regressions do not detect statistically significant coefficients on the $K_d - 1$ -nearest instrument. The first-stage coefficients range from 0.136 to 0.151.

is positive for individuals from high-income families, the corresponding estimate is only marginally significant for employment days (p<0.10) and insignificant for work hours.[24]

The first-stage coefficients for the $K_d - 1$ -nearest instrument are around 50% smaller compared to the baseline specification. This is in line with the fact that the further away conscripts are from each other in the squadron's alphabetical ranking, the less likely they are to be assigned to the same dorm (Panel A of Figure 1). As a result, adding in the peer mean parental income of conscripts who are further away from each other in the alphabetical ranking reduces variation induced by the instrument in the peer mean, leading to lower precision of the IV estimates.[25] Appendix Figure A8 illustrates this by plotting the first-stage estimates and 95% confidence intervals by the alphabetical rank distance between the conscript and his peers. The first stage is the strongest for the two alphabetically nearest conscripts in the squadron. Therefore, in the all-dormmates specification, the majority of identifying variation in peer parental income induced by the alphabetical rule comes from the parental income of the two alphabetically nearest conscripts in the squadron, and the magnitudes of the estimates adjusted for the size of the peer group can be expected to be similar for the specifications based on the two alphabetically nearest and all dormmates. The figure also shows that the reduced-form estimates for peers who are further away in the alphabetical ranking are all very small. This is expected for two reasons. First, fewer of them are in the same dorm and influence each other. Second, when assigned to the same dorm, they are likely to influence each other less because alphabetically distant conscripts are rarely assigned to adjacent bunks.[26]

We find little evidence of nonlinearity by peers being above or below own parental income

[24] Appendix Table A11 shows result for an alternative IV model with an instrument based on alphabetically simulated dorms. This model has smaller first-stage coefficients on the instrument (from 0.100 to 0.108) than the model based on $K_d - 1$ nearest conscripts and provides broadly similar results compared to our baseline model.

[25] We note that the less precise estimates for $K_d - 1$ dormmates could also partly stem from the reduced variation in the peer mean because the group mean converges toward the population mean as the size of the peer group increases (Angrist, 2014).

[26] Results from an IV specification including both the parental income of the two alphabetically nearest dormmates and of all dormmates lead to similar conclusions. When both of these variables are included as independent variables in the IV model for earnings, using parental income of the two nearest conscripts and $K_d - 1$ nearest conscript in the squadron as instruments, the IV coefficient (standard error) for the two nearest dormmates is 0.0306 (0.0095) and highly significant (p<0.01), whereas the IV coefficient (standard error) for all dormmates is 0.0429 (0.0361), statistically insignificant, and only around 40% ($\approx 100 \times (2/7) \times 0.0429/0.0306$) in magnitude compared to the baseline estimate.

explaining the key heterogeneity results for earnings and hourly wages (Appendix Table A12).

Although the estimates for all dormmates provide broadly similar qualitative conclusions, we prefer the specification for the two alphabetically nearest dormmates because 1) they can be expected to have the largest influence on each other due to the alphabetical assignment of bunks within dorms, 2) a higher fraction of them are assigned to the same dorm, which allows us to recover substantially more precise IV estimates of peer effects for them, and 3) because the two alphabetically nearest dormmates account for the majority of variation induced by the instrument in the alphabetical IV setting, the two approaches recover broadly similar magnitudes for peer effects.

Peer parental education. Appendix Table A13 provides results for dormmates' parental education. The results lead to similar conclusions as the baseline specification based on dormmates' parental income. This is expected as the correlation between parental income and parental education of the two alphabetically nearest dormmates is 0.63, and the correlation between the corresponding instruments is the same. Due to these high correlations, the IV estimations based on peer parental income and peer parental education can be expected to recover broadly similar results. Indeed, the IV coefficient of 0.1454 ($p < 0.05$) in Panel A of Appendix Table A13 means that when the years of education of the parents of the two alphabetically nearest dormmates increases by one standard deviation, which is equivalent to around 6 years, the conscript's annual earnings will be around 872 euros higher at age 28-42 (a 2.7% increase compared to the sample mean), which is of a similar magnitude as the corresponding effect of peer parental income (999 euros). The results indicate also similar heterogeneity of peer effects, with the largest effects on earnings and hourly wages of individuals from high-income families.

IV.F Comparison to OLS estimates

Appendix Table A14 shows OLS estimates of γ based on Equation (3). The control variables are the same as in the baseline IV specification. The OLS estimates for earnings and hourly wages are positive and statistically significant but smaller than the corresponding IV estimates. One potential explanation for this is that in squadrons with selective dorm assignment, the goal is more often to form balanced dorms, rather than specialized dorms, by individual characteristics that are correlated with future outcomes.[27] This explanation is also supported by OLS results for squadrons with weak and strong alphabetical assignment rule: For squadrons with weak alphabetical assignment, within which selective dorm assignment can be expected to be applied more often, OLS estimates are lower and further away from the baseline IV estimates than for squadrons with strong alphabetical assignment.

V Mechanisms

We now examine several potential mechanisms through which the identified peer effects on earnings and hourly wages could operate. Our analysis is divided into three parts. We begin by investigating the role of human capital investment by examining rich data on post-service educational trajectories and attainment. In the second part, we assess the importance of long-term labor market connections by employing linked employer-employee data covering the whole working-age population. In the third part, we examine the role of sorting to employers associated with higher compensation levels.

V.A Human capital investment

Conscripts in our sample enter service at age 18-22. At this age, individuals typically decide whether to apply for post-secondary education programs at a university and for which field to apply. Mechanisms that operate through educational choices could therefore be an explanation for the identified peer effects on earnings and hourly wages. For example, conscripts might influence educational choices of others by affecting their attitudes toward higher education, by sharing information about the difficulty and requirements for entering specific education programs, or by providing information on the expected returns to specific degrees.

The results for educational outcomes are reported in Table 7.[28] Our primary short-term outcome is a measure of the economic quality of the education program attended 1-3

[27]OLS will underestimate the peer effect on earnings if individuals with high earnings potential are more likely assigned a dormmate whose parents have a low income than in a case where dorms are randomly assigned. This could occur, for example, if a common goal of selective dorm assignment is to create balanced dorms (e.g., with similar ability and skill distributions). The commonality rate above one is consistent with balancing in squadrons with selective assignments (see footnote 9). Conversely, if dorms were specialized, the selection bias in the OLS estimates would be likely positive.

[28]First-stage coefficients on the instrument range from 0.283 to 0.309 and are statistically significant at the 1% risk level across specifications in the table. For brevity, they are not reported.

years after service, based on hourly wages at age 31 of individuals who have studied in the program.[29] Focusing first on individuals from high-income families, we find no statistically significant impacts on the quality of education programs they attend 1-3 years after service. Conversely, we find that exposure to peers from higher-income families has a positive and statistically significant impact on the program quality among individuals from low-income families. However, the magnitude of this effect is modest. The estimate of 0.0051 ($p < 0.05$) means that as parental income of peers increases by one standard deviation, an individual from a low-income family enrolls to an education program with 15 cents higher expected hourly wage, on average, which is only a 0.9% increase from the sample mean of 17.60 euros in this group. Long-term effects on completed years of schooling and a binary indicator for holding a university degree are statistically indistinguishable from zero across samples. We also find no impacts on the choice of study field, study credits, or the likelihood of study credits being observed (Appendix Tables A15, A16, and A17).

In sum, our results for educational outcomes suggest that the identified positive peer effect on earnings of individuals from richer families do not appear to operate through the human capital investment channel, perhaps because baseline educational attainment is already at a high level in this group and they may be better informed about requirements of and returns to education programs by their families and acquaintances. Individuals from poorer families enroll in economically more valuable programs when they are exposed to peers from high-income backgrounds, which could partly explain the positive but relatively small and statistically insignificant estimated peer effects on their earnings and hourly wages.

V.B Labor market networks

Economically beneficial labor market connections offer an alternative potential explanation for the earnings gains among individuals from high-income families. They may, for instance, share valuable information on job opportunities or provide job referrals to each other (e.g.,

[29] We follow Chetty et al. (2011) and construct indexes of program quality by taking the sample of all individuals observed at age 21 in the population panel from 1999 to 2006 and grouping them by education program (one group includes individuals who are not attending a program). The hourly-wage-based index of program quality is then calculated as the average hourly wage at age 31 in these groups. We also construct a similar index based on earnings. The indexes are merged to each conscript by the program attended 1-3 years after service.

Granovetter, 1995; Topa, 2011). Moreover, referrals within networks comprised of high-productivity employed workers and job candidates can lead to better labor market matches for the candidates as a result of which they can hold job positions in which their productivity and compensation is higher (e.g., Montgomery, 1991; Galenianos, 2013).

Motivated by these considerations, we now assess the importance of social connections by employing linked employer-employee data drawn from the National Pension Insurance System. This information is based on mandatory individual pension insurance filings made by the employer and covers the entire workforce. We use the unique identifier for the employer an employee is associated with in the last week of the year. To empirically test for labor market networks, we build on the empirical model in Bayer, Ross, and Topa (2008) and estimate the following dyadic regression:

$$Y_{ii'} = \alpha_{s(ii')} + \phi D_{ii'} + \beta_{(x_i,x_{i'})} + \epsilon_{ii'} \ , \tag{4}$$

where $Y_{ii'}$ is a binary indicator equal to one if conscripts i and i' work for the same employer after their service in any of the years when at least one of the pair members is at age 28-42, and zero otherwise; $D_{ii'}$ is a binary indicator equal to one if conscript i' is among the two alphabetically nearest dormmates of conscript i and zero otherwise; and $\epsilon_{ii'}$ is an error term. We run the regression by using data for all conscript pairs serving in the same squadron and include squadron fixed effects $\alpha_{s(ii')}$. To account for the potential within-squadron selection of conscripts to dorms by unobserved characteristics, we instrument $D_{ii'}$ with a binary indicator equal to one if conscript i' is among the two alphabetically nearest conscripts of conscript i in squadron s and zero otherwise. This IV approach employs the same within-squadron variation across the squadron alphabetical ranking as the peer IV regression model in Equations (2) and (3). The instrument induces variation in who is a conscript's alphabetically nearest dormmate because alphabetically nearest conscripts in the squadron are more likely to be assigned to the same dorm than those who are further away from each other in the alphabetical ranking. We also control for pair characteristics by including fixed effects $\beta_{(x_i,x_{i'})}$ that control for combinations of pair members' locations in the distributions of parental income, population alphabetical ranking, and pre-service characteristics listed in Panel A of Table 1.[30] As before,

[30]For parental income, population alphabetical rank, and each continuous pre-service characteristic listed in Panel A of Table 1, we include fixed effects for all pairwise combinations by two-percentile bins (that is, fixed effects for $0.5 \cdot [50 \cdot 50 + 50] = 1,275$ categories for each variable). For other (discrete) pre-service

we cluster standard errors at the squadron level and multiply binary outcomes by 100.

To check the validity of the dyadic IV regression, we start by estimating Equation (4) for pre-service pairwise outcomes. We find no significant difference in the likelihood of working for the same employer before the service between pairs of alphabetically close dormmates and others pairs in the squadron (Appendix Table A18).[31] The findings are similar for a binary indicator for working in the same establishment before service, which is based on the SF establishment indicator, constructed from company identifier, workplace address, and industry.

Turning to results for post-service outcomes, the first column in Table 8 shows that conscripts who are close peers are more likely to work for the same employer and in the same establishment after service. In the short term (within three years after serving), the fraction of pairs of alphabetically close dormmates working for the same employer is around 0.4 pp higher ($p < 0.05$) compared to other pairs in the squadron. In the long term (either of the pair member at age 28-42), the difference is still around 0.3 pp. While this effect is not statistically significant at the conventional significance levels ($p = 10.2$), the corresponding estimate for working in the same establishment is of a very similar magnitude and statistically significant ($p < 0.05$).

Columns 2-4 display the estimates by pair type. We find evidence of strong short-term labor market connections for pairs where both members are from low-income families. However, these ties appear to vanish completely over time. Conversely, although alphabetically close dormmates from high-income families do not appear to work more often for the same employer compared to other similar pairs in the squadron 1-3 years after service, Column 4 shows strong evidence of long-term labor market connections among them: In this sample, the fraction of pairs of alphabetically close dormmates working for the same employer is around 2 pp higher compared to other similar pairs in the squadron, which is a considerable effect compared to the mean of around 0.5% in this sample. This finding suggests that persistent, long-term labor market networks are an important mechanism through which beneficial social exposure effects among individuals from more affluent backgrounds operate. Finally, we

characteristics, we include fixed effects for all pairwise combinations of the variable values.

[31] First-stage coefficients on the instrument range from 0.18 to 0.20 and are statistically significant at the 1% risk level across specifications reported in this section. For brevity, they are not reported in the tables.

do not detect statistically significant effects for mixed pairs where one member is from a low-income family and the other is from a high-income family.

We believe that one potential explanation for the strong short-term labor market connections among individuals from lower-income families is that they may be better connected to the segments of the labor markets relevant for young adults with low education and little experience, which could be particularly important right after service. The lack of evidence of labor market connections among mixed pairs suggests that intensive social exposure does not generate significant social ties across socioeconomic group boundaries in the conscripted cohorts. This appears to be a key explanation for why earnings of individuals from low-income families are little affected in the long term by exposure to peers from high-income families. Overall, the results in this section suggest that an intervention as extreme as the mixing of conscripts from a wide range of family backgrounds to live and train together for a prolonged period does not break the tendency for individuals to connect socially more with those who are similar and less with those who are different.

V.C Sorting by Employer Compensation Levels

The findings in the previous section raise the question of whether the earnings and hourly wage gains among individuals from high-income families stem from a higher fraction of them working for employers associated with higher compensation levels. We examine this possibility by estimating the effects of peer parental income on the average hourly wages payed by an employer to its continuing employees one year before a conscript starts working for the employer. The results are presented in Appendix Table A19.

We do not find evidence of sorting to employers associated with higher compensation levels among individuals from high-income families. This conclusion holds also when we use the average hourly wages of less- and highly-educated employees as the outcomes. Conversely, we do find evidence of sorting to employers associated with higher wages among individuals from low-income families, which could be another explanation for the positive but relatively small and statistically insignificant estimated peer effects on their earnings and hourly wages. Finally, we do not find evidence of either of these conscript groups working for larger employers or of instrument-induced sample selection (based on binary indicators for the average wage outcomes being observed).[32]

Our key conclusion from the analysis in this section is that the hourly wage gains among individuals from high-income families are not driven by jobs at employers associated with higher compensation levels. Overall, combined with the evidence in the previous section, the findings are in line with the hypothesis that informal hiring networks among individuals from richer families generate wage advantages for them within a given employer.

VI Conclusions

This paper examines how mixing individuals from high- and low-income families shapes long-term educational and economic outcomes. We provide causal evidence by exploiting a large-scale natural experiment generated by the alphabetization of conscript dorms in the Finnish military. An important feature of our research setting is that it allows us to provide novel evidence of the consequences of increased social exposure of individuals from poorer families to those from richer families. We find that social exposure to peers from richer families increases long-term earnings of individuals who are also from richer families, but not of those who are from poorer families.

Our study highlights the role of social factors in generating and reinforcing economic inequality by showing sizable and statistically significant heterogeneity in the effects of social exposure by socioeconomic background. Our results suggest that differences in exposure to high-SES peers can significantly contribute to economic inequality over time. In the context of our study, such differentials can explain a considerable fraction of wage inequality between the 90th percentile richest and 10th percentile poorest social groups, corresponding to around 14% of the 90-10 percentile wage gap in the cohorts relevant for our study.

Our results for individuals from more affluent backgrounds provide new evidence of the important role of social connections for their long-term economic success. Our findings are in line with Zimmerman (2019) and Michelman, Price, and Zimmerman (2022), who

[32]We have focused on hourly wages and not on earnings, because we observe earnings of an individual by year, not by employer, and thus cannot calculate the exact employer-specific average employee earnings. For completeness, Appendix Table A20 shows results for the average employee daily earnings. Consistent with the results for the average employee hourly wages, the estimate for the average employee daily earnings is statistically insignificant for individuals from high-income families and positive for individuals from low-income families.

provide evidence of the importance of social connections among students from high-status backgrounds at elite US and Chilean universities. We complement and extend this line of research by examining the consequences of cohort-wide social integration. Our analysis of labor market networks suggests that income-based social integration does not break the tendency for individuals from richer families to connect more with those who are also from richer families and less with those who are from poorer families.

Our study also has some limitations. First, our setting is based on compulsory military service of male citizens, which means that we can only assess the consequences of cohort-wide social mixing for men. Second, our study focuses on peer exposure during a particular age range, 18 to 22. While this is an important phase during which individuals typically choose their educational tracks leading to final degrees and make occupational choices, our results might not be generalizable to other age groups. Third, social integration might have different consequences in countries with different social cultures, educational institutions, and labor markets compared to Finland. We believe that the context of our study is particularly interesting for studying the long-term impacts of social mixing. Finland provides publicly funded education from primary school to university and has high levels of equality of opportunity and income mobility (Corak, 2013; Narayan et al., 2018). Therefore, if the social and economic trajectories of the poor were affected by exposure to high-SES peers, we would expect that to show up, especially in Finland, where other, non-social barriers to income mobility are less likely to dampen the effects of income-based social integration.

Our findings are not entirely pessimistic for the poor. An important finding is that exposure to peers from richer families raises the quality of their education. However, this effect is not sufficiently large to induce a statistically detectable impact on their long-term earnings. Our results suggests that a key explanation for why individuals from poorer families receive only small economic gains from social interaction with individuals from richer families is the lack of cross-group labor market connections. This finding supports the view that the effectiveness of policies that aim to reduce economic inequality by increasing social exposure between individuals from different socioeconomic backgrounds may be improved by combining them with interventions that strengthen persistent cross-group social ties, such as mentorships, internships, and affirmative action (e.g., Jackson, forthcoming; Chetty et al., 2022b).

Our findings also raise the question of the extent to which exposure to dormmates from high-income backgrounds affects the economic outcomes of conscripts' children. We leave the empirical assessment of such an intergenerational transmission of peer effects for the future, when data on the educational and economic trajectories of conscripts' children become available.

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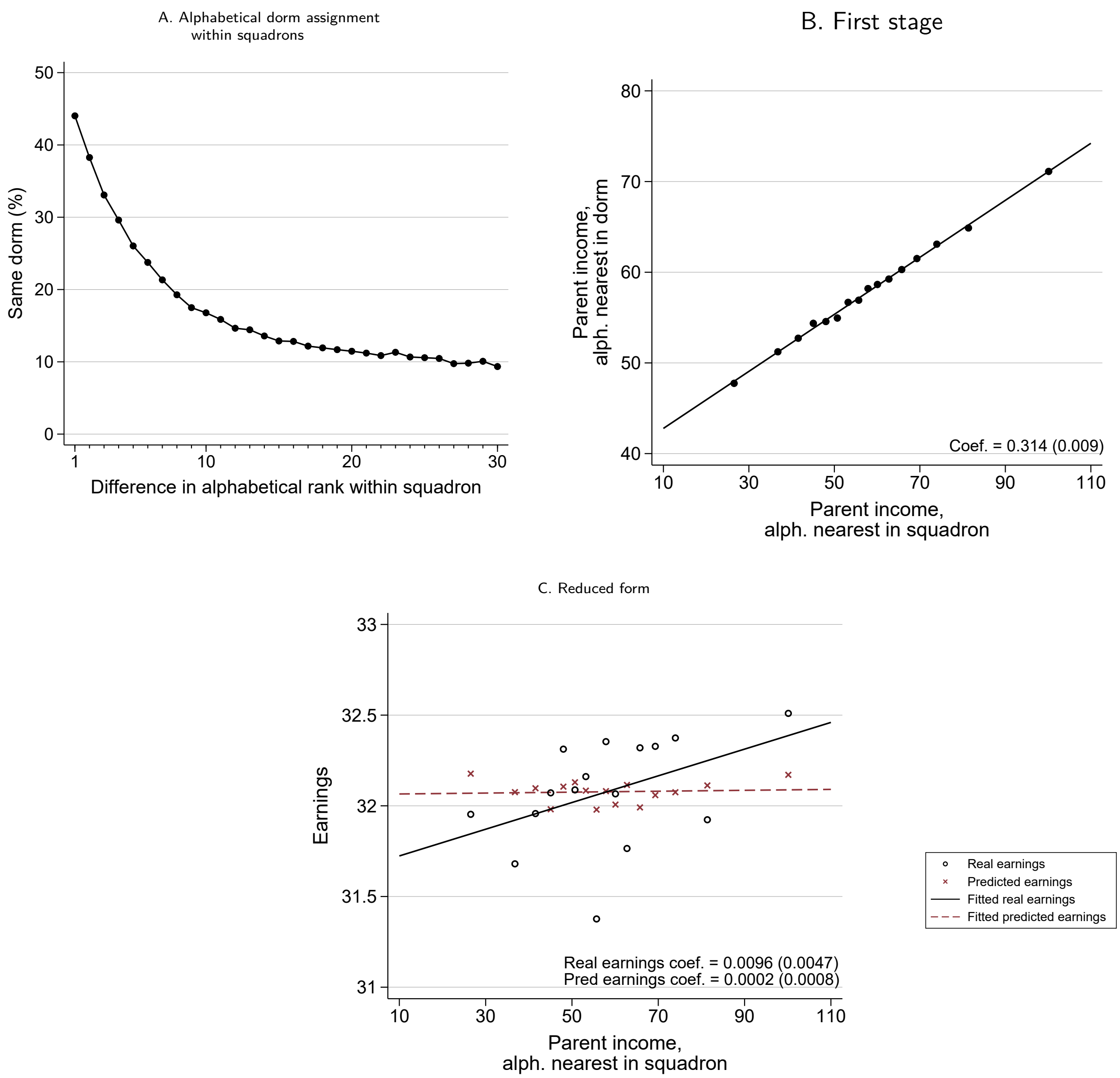
Figure 1: Alphabetical dorm assignment, peers' parental income, and long-run earnings



Notes: **Panel A:** The figure shows the fraction of pairs residing in the same dorm within a squadron by the within-squadron alphabetical rank distance between the pair members using data for pairs of conscripts in the same squadron. The number of pairs in the data shown is 715,225. Values for bins with alphabetical distance above 30 are not shown. **Panels B and C:** Panel B displays the first-stage effect of the average parental income of the two alphabetically nearest conscripts in the squadron on the average parental income of the two alphabetically nearest dormmates. Panel C displays the reduced-form effect of the average parental income of the two alphabetically nearest conscripts in the squadron on realized and predicted earnings at age 28-42. Predicted earnings are constructed as the best linear prediction of earnings at age 28-42 based on the pre-service characteristics in Panel A of Table 1. The figures plot the residuals from separate regressions of the x- and y-axis variables on parental income, dummies for calendar year, and squadron fixed effects. The lines pass through coordinates corresponding to the sample means of the variables on the horizontal and vertical axes. Data include 376,963 conscript-year observations (50,578 conscripts). Income and earnings are in thousand euros. Standard errors allowing for clustering at the level of squadron are in parentheses.

Table 1: Validation regressions

	Independent variable				
	Parental income		Parental income of the two alphabetically nearest conscripts in the squadron		Dependent
Dependent variable	coef.	s.e.	coef.	s.e.	mean
			A. Pre-service		
Earnings	0.0020	(0.0010)	-0.0010	(0.0012)	5.00
Age	0.0003	(0.0001)	-0.0001	(0.0005)	19.74
Employed (%)	0.0178	(0.0080)	-0.0102	(0.0113)	42.29
Years of schooling	0.0027	(0.0002)	0.0004	(0.0003)	10.43
Married (%)	-0.0022	(0.0008)	0.0018	(0.0011)	0.27
Foreign (%)	-0.0001	(0.0001)	0.0001	(0.0001)	0.00
Primary language Finnish (%)	0.0128	(0.0028)	-0.0022	(0.0036)	95.78
Unemployment benefits	-0.0023	(0.0001)	0.0001	(0.0002)	0.28
General housing allowance	-0.0043	(0.0002)	-0.0001	(0.0002)	0.26
Number of parents employed	0.9374	(0.0141)	-0.0056	(0.0142)	1.49
Parents' years of schooling	0.1189	(0.0012)	0.0005	(0.0012)	23.96
Parental pension income	-0.0210	(0.0012)	-0.0001	(0.0014)	2.23
Parental unemployment benefits	-0.0299	(0.0006)	0.0010	(0.0009)	1.89
Parental housing allowance	-0.0080	(0.0002)	-0.0002	(0.0003)	0.39
			B. Predicted		
Predicted earnings at age 28-42	0.0937	(0.0010)	0.0002	(0.0008)	32.08

Notes: The data include 50,578 conscripts. Each row presents coefficients from a separate regression on the dependent variable denoted by the row label. All regressions include the following independent variables: parental income, parental income of the two alphabetically nearest conscripts in the squadron, and squadron fixed effects. Conscripts in the same squadron are in the same wave and thus dummies for the service year and wave are redundant and not included. Dependent variables in Panel A are measured one year before service, except age, which is measured in the initial service year. Predicted earnings in Panel B are constructed as the best linear prediction of earnings at age 28-42 based on the pre-service characteristics in Panel A. Monetary variables are in thousand euros. Standard errors allowing for clustering at the level of squadron are in parentheses.

Table 2: Peer effects on long-run earnings

	First stage	Reduced form	IV
	A. No additional controls		
Parental income:			
two alphabetically nearest in the squadron	0.3136	0.0096	
	(0.0088)	(0.0047)	
two alphabetically nearest dormmates			0.0308
			(0.0150)
	B. With additional controls		
Parental income:			
two alphabetically nearest in the squadron	0.3066	0.0102	
	(0.0087)	(0.0046)	
two alphabetically nearest dormmates			0.0333
			(0.0149)
Dependent mean	29.00	32.08	32.08
Observations		376,963	
Conscripts		50,578	
Dorms		6,756	
Squadrons		4,836	

Notes: The table displays estimates from a regression of earnings at age 28-42 on the average parental income of the two alphabetically nearest dormmates, using the average parental income of the two alphabetically nearest conscripts in the squadron as the instrument. All regressions include parental income, dummies for the year of outcome measurement, and squadron fixed effects. Conscripts in the same squadron are in the same wave and thus dummies for the service year and wave are redundant and not included. The specification with additional control variables in Panel B also includes pre-service characteristics for a conscript and his parents listed in Panel A of Table 1, average parental income of dormmates who are not the two alphabetically nearest, and dummies for each tenth of a population alphabetical rank percentile, age in the service start year, and age in the year of outcome measurement. Income and earnings are in thousand euros. The first-stage F statistic is 1259 in Panel A and 1253 in Panel B. Standard errors allowing for clustering at the level of squadron are in parentheses.

Table 3: Specification checks

	First stage	Reduced form	IV	N
A. Baseline specification	0.3066	0.0102	0.0333	376,963
	(0.0087)	(0.0046)	(0.0149)	
B. Exclude conscripts with a peer within 1 population alphabetical rank percentile	0.2786	0.0133	0.0479	204,781
	(0.0100)	(0.0068)	(0.0242)	
C. Squadrons with strong alphabetical assignment	0.6074	0.0152	0.0251	184,182
	(0.0080)	(0.0068)	(0.0111)	
D. Squadrons with weak alphabetical assignment	0.0265	0.0027	0.1037	192,781
	(0.0071)	(0.0061)	(0.2309)	
E. Age 30-35	0.3072	0.0109	0.0355	224,801
	(0.0090)	(0.0049)	(0.0159)	
F. Exclude women	0.3086	0.0104	0.0336	374,560
	(0.0087)	(0.0046)	(0.0148)	
G. Primary language Finnish	0.3041	0.0121	0.0399	360,991
	(0.0087)	(0.0046)	(0.0150)	
H. Dorm size ≤ 20	0.3050	0.0116	0.0382	369,463
	(0.0087)	(0.0046)	(0.0151)	
I. Dorm size ≤ 10	0.2715	0.0111	0.0410	251,868
	(0.0098)	(0.0057)	(0.0209)	
J. Dorm size > 20	0.4097	0.0046	0.0113	7,500
	(0.0583)	(0.0337)	(0.0779)	
K. Interact alph. rank dummies with linear term for alph. rank	0.3049	0.0105	0.0344	376,963
	(0.0085)	(0.0046)	(0.0150)	
L. + interact alph. rank dummies with linear term for distance to nearest peer in the alph. ranking	0.3054	0.0101	0.0330	376,963
	(0.0084)	(0.0046)	(0.0149)	
M. Weighted	0.2896	0.0099	0.0343	376,963
	(0.0100)	(0.0042)	(0.0144)	

Notes: The table shows estimates for alternative specifications of the baseline model in Panel B of Table 2. Panel B runs a similar specification but excludes conscripts for whom either of the two alphabetically nearest conscripts in the squadron is less than one percentile from him in the population alphabetical rank distribution. Panels C and D show results separately for squadrons with strong and weak alphabetical dorm assignment rule. We define squadrons with strong (weak) alphabetical rule as those whose within-squadron correlation between the average parental income of the two alphabetically nearest conscripts within the squadron and within the dorm is above (below) the median. Panel E restricts the sample to individuals at age 30-35, Panel F excludes female conscripts, and Panel G excludes conscripts whose primary language is not Finnish. Panels H and I limit the dorm size to 20 and 10 conscripts, while Panel J reports estimates for dorms with more than 20 conscripts. Panel K includes interaction terms between the dummies for each tenth of a population alphabetical rank percentile and the population alphabetical rank. Panel L runs a similar specification but adds interaction terms between the dummies for each tenth of a population alphabetical rank percentile and the distance to the nearest peer in the population alphabetical ranking. In Panel M, regressions are weighted by the inverse of the number of observations available for each conscript at age 28-42. Standard errors allowing for clustering at the level of squadron are in parentheses.

Table 4: Peer effects on hourly wage and work hours

	Baseline sample	Wage sample				
	Hourly wage observed		**Hourly wage**		**Work hours**	
	Reduced form	First stage	Reduced form	IV	Reduced form	IV
Parental income:						
two alphabetically nearest in the squadron	0.00010	0.3090	0.0075		0.0085	
	(0.00014)	(0.0103)	(0.0019)		(0.0063)	
two alphabetically nearest dormmates				0.0242		0.0275
				(0.0061)		(0.0203)
Dependent mean	0.44	58.87	19.87	19.87	163.42	163.42
Observations	376,963	165,180				
Conscripts	50,578	32,688				
Dorms	6,756	6,693				
Squadrons	4,836	4,519				

Notes: The table displays IV estimates of peer effects on hourly wage and work hours. The number of observations is lower in the wage sample compared to the baseline sample due to the limited coverage of the wage data (see Section II.A for details). In the first column, we test whether the instrument induces selection into the wage sample by using the full baseline sample and estimating a reduced-form peer regression with a binary indicator equal to one if hourly wage is observed, and zero otherwise, as the outcome. The other columns display estimates for hourly wage and work hours for specifications based on the IV model with additional controls, corresponding to Panel B of Table 2. The first-stage F statistic for the wage sample is 907. Standard errors allowing for clustering at the level of squadron are in parentheses.

Table 5: Heterogeneity of peer effects by parental income

Dependent variable	All	Low parental income	High parental income	Difference high - low	Dependent mean, all
A. Earnings	0.0333	0.0032	0.0670	0.0638	32.08
	(0.0149)	(0.0201)	(0.0229)	(0.0301)	
B. Hourly wage	0.0242	0.0090	0.0363	0.0273	19.87
	(0.0061)	(0.0083)	(0.0091)	(0.0123)	
C. Work hours	0.0275	0.0174	0.0439	0.0265	163.42
	(0.0203)	(0.0284)	(0.0316)	(0.0426)	
D. Employed (%)	0.0251	0.0113	0.0289	0.0176	84.27
	(0.0217)	(0.0357)	(0.0290)	(0.0458)	
E. Employment days	0.0646	-0.0077	0.1079	0.1156	315.09
	(0.0734)	(0.1181)	(0.0989)	(0.1526)	
F. Unemployment benefits	-0.0031	-0.0023	-0.0036	-0.0013	1.04
	(0.0015)	(0.0026)	(0.0019)	(0.0032)	
G. General housing allowance	-0.0011	-0.0009	-0.0012	-0.0004	0.19
	(0.0005)	(0.0008)	(0.0006)	(0.0010)	
Observations:					
Panels A, D, F, and G	376,963	188,853	188,110		
Panels B and C	165,180	75,470	89,710		
Panel E	374,769	187,743	187,026		

Notes: The table shows IV estimates of peer effects on long-term outcomes. Specifications are based on the IV model with additional controls, corresponding to Panel B of Table 2. Each cell in Columns 1-3 displays an IV estimate of the impact of the average parental income of the two alphabetically nearest dormmates on an outcome denoted by the row title in a sample denoted by the column title. The sample of individuals with low (high) parental income includes conscripts whose parental income is below (above) the median. Column 4 shows the difference in the estimated peer effect between the high- and low-parental-income samples. It is the coefficient on the interaction term between the average parental income of the two alphabetically nearest dormmates and a dummy for high parental income in a specification using the full sample and interacting all right-hand-side variables and fixed effects with the dummy for high parental income. We note that this specification recovers estimates for the subsamples that are equivalent to the estimates from separate IV regressions by subsample. The numbers of observations vary across rows due to differences in availability of data for the outcome variables: Panels B and C are based on the wage sample; in Panel E, employment days are available from 2005 onward. The last column shows the full sample means of the dependent variables. First-stage estimates range from 0.299 to 0.323 across specifications and are all statistically significant at the 1% risk level (Appendix Table A5). Means for subsamples are reported in Panel C of Appendix Table A1. Income, earnings, and benefits are in thousand euros. Standard errors allowing for clustering at the level of squadron are in parentheses.

Table 6: Heterogeneity of peer effects by parental income, all dormmates with $K_d - 1$ -nearest instrument

Dependent variable	All	Low parental income	High parental income	Dependent mean, all
A. Earnings	0.1286	-0.0256	0.3378	32.08
	(0.0736)	(0.1018)	(0.1116)	
B. Hourly wage	0.0817	0.0234	0.1277	19.87
	(0.0285)	(0.0463)	(0.0435)	
C. Work hours	0.0906	0.1601	0.0785	163.42
	(0.0910)	(0.1628)	(0.1413)	
D. Employed (%)	0.0980	-0.0674	0.2745	84.27
	(0.1082)	(0.1834)	(0.1395)	
E. Employment days	0.1460	-0.5113	0.8391	315.09
	(0.3745)	(0.6102)	(0.4859)	
F. Unemployment benefits	-0.0018	0.0091	-0.0110	1.04
	(0.0076)	(0.0140)	(0.0092)	
G. General housing allowance	0.0006	0.0044	-0.0020	0.19
	(0.0022)	(0.0041)	(0.0029)	
Observations:				
Panels A, D, F, and G	376,963	188,853	188,110	
Panels B and C	165,180	75,470	89,710	
Panel E	374,769	187,743	187,026	

Notes: The table shows IV estimates of peer effects on long-term outcomes. Each cell in Columns 1-3 displays an IV estimate of the impact of the average parental income of a conscript's all dormmates on an outcome denoted by the row title in a sample denoted by the column title. Specifications are based on the IV model with additional controls, using as the instrument the average parental income of a conscript's $K_d - 1$ alphabetical nearest conscripts in the squadron, where K_d is the size of the conscript's dorm. The sample of individuals with low (high) parental income includes conscripts whose parental income is below (above) the median. The numbers of observations vary across rows due to differences in availability of data for the outcome variables: Panels B and C are based on the wage sample; in Panel E, employment days are available from 2005 onward. The last column shows the full sample means of the dependent variables. First-stage coefficients on the instrument range from 0.136 to 0.151 across specifications and are all statistically significant at the 1% risk level. Income, earnings, and benefits are in thousand euros. Standard errors allowing for clustering at the level of squadron are in parentheses.

Table 7: Peer effects on education

Dependent variable	All	Low parental income	High parental income	Difference high - low	Dependent mean, all
1-3 years after service					
A. Hourly-wage-based index of program quality	0.0007	0.0051	-0.0035	-0.0086	18.16
	(0.0017)	(0.0022)	(0.0028)	(0.0036)	
B. Earnings-based index of program quality	0.0005	0.0103	-0.0101	-0.0204	26.57
	(0.0042)	(0.0055)	(0.0069)	(0.0088)	
Observations	148,742	74,377	74,365		
At age 28-42					
C. Holds a university degree (%)	0.0335	0.0476	0.0290	-0.0186	34.70
	(0.0349)	(0.0530)	(0.0530)	(0.0763)	
D. Years of schooling	0.0027	0.0053	0.0006	0.0053	13.77
	(0.0022)	(0.0033)	(0.0034)	(0.0033)	
Observations	376,963	188,853	188,110		

Notes: The table shows IV estimates of peer effects on educational outcomes. Specifications are based on the IV model with additional controls, corresponding to Panel B of Table 2. Each cell in Columns 1-3 displays an IV estimate of the impact of the average parental income of the two alphabetically nearest dormmates on an outcome denoted by the row title in a sample denoted by the column title. The sample of individuals with low (high) parental income includes conscripts whose parental income is below (above) the median. Column 4 shows the difference in the estimated peer effect between the high- and low-parental-income samples. It is the coefficient on the interaction term between the average parental income of the two alphabetically nearest dormmates and a dummy for high parental income in a specification using the full sample and interacting all right-hand-side variables and fixed effects with the dummy for high parental income. First-stage estimates range from 0.283 to 0.309 across specifications and are all statistically significant at the 1% risk level. The indexes of the study program quality are based on the average hourly wages and earnings at age 31 of individuals attending a program at age 21 (footnote 29 provides details). The data for education programs cover the years 1999-2013. Income and earnings are in thousand euros. Standard errors allowing for clustering at the level of squadron are in parentheses.

Table 8: Peers and the likelihood of working for the same employer

Dependent variable	All Pairs	Both have low parental income	Mixed pairs	Both have high parental income
1-3 years after service				
Work for the same employer (%)	0.4061	0.7461	0.3327	0.2873
	(0.1875)	(0.3350)	(0.2888)	(0.4604)
	[0.390]	[0.316]	[0.400]	[0.452]
Work in the same establishment (%)	0.336	0.739	0.2945	-0.0346
	(0.157)	(0.256)	(0.2405)	(0.4070)
	[0.254]	[0.173]	[0.260]	[0.333]
At age 28-42				
Work for the same employer (%)	0.3163	-0.2275	-0.2103	2.1878
	(0.1936)	(0.3168)	(0.2676)	(0.5824)
	[0.356]	[0.268]	[0.335]	[0.494]
Work in the same establishment (%)	0.3451	0.0780	0.2261	1.0502
	(0.1581)	(0.2474)	(0.2186)	(0.4757)
	[0.232]	[0.163]	[0.216]	[0.336]
Pairwise observations	982,728	266,628	468,317	247,783

Notes: Pairwise data for all within-squadron pairs of 50,578 conscripts in the baseline sample. The table displays estimates from a regression of a binary indicator for working for the same employer on a binary indicator for pair members being among the two alphabetically nearest dormmates, using a binary indicator for pair members being among the two alphabetically nearest conscripts in the squadron as the instrument. The table also reports results for a binary indicator for working in the same establishment. All outcome variables are converted to percentages by multiplying them by 100. All regressions include squadron fixed effects and fixed effects for pair characteristics. The fixed effects for pair characteristics are constructed as follows. For parental income, alphabetical population rank, and each continuous pre-service characteristic listed in Panel A of Table 1, we include fixed effects for all pairwise combinations by two-percentile bins (that is, fixed effects for $0.5 \cdot [50 \cdot 50 + 50] = 1,275$ categories for each variable). For other (discrete) pre-service characteristics, we include fixed effects for all pairwise combinations of the variable values. First-stage estimates range from 0.180 to 0.200 across specifications and are all statistically significant at the 1% risk level. Standard errors allowing for clustering at the level of squadron are in parentheses. Sample means of dependent variables are in brackets.