Essays in Applied Economics

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Thesis submitted for assessment with a view to obtaining the degree of Doctor of Economics of the European University Institute

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European University Institute
Department of Economics

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Contents

Abstract

This thesis consists of three articles in applied economics.

In the first essay, I consider the extent to which informational frictions between workers and jobs can be alleviated with short-term contracts in the early career. I leverage a program at a Finnish university which gave out randomly selected students an internship subsidy for a three-month paid internship. I match these students to administrative data to track their transition to labor markets in the years around the program and find evidence that the program significantly improved early labor market success.

In the second essay, I study the effect of social sorting on family formation and inequality across households. I leverage the institutional features of Finnish high-school assignments to evaluate how exposure to high-skilled, high-socioeconomic -status peers affect the quality of social ties individuals form. I find that while high schools are an important meeting place for future spouses, but that exposure to higher quality peers will not affect the eventual partner characteristics. This suggests that policies aiming to mix individuals from various backgrounds may not always work anticipated.

In the third essay, I study with two co-authors the causes and consequences of broadly defined inequality and democratization using Finland as a natural experiment. We find evidence that the 19th famine affected inequality and labour coercion and thus the balance of political power. On the other hand, we find that these developments were critical in explaining both the increasing threat of revolution and participation in the Finnish civil war in the early 20th century and a subsequent shift to democratization. Areas that initially experienced higher growth in inequality, also experienced the most significant shift to redistribution in the aftermath of the war.

Internships and the Allocation of Talent

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Abstract

I study the role of internships in matching workers and jobs in the early career. I leverage on a university-funded program which gave randomly selected students a subsidy for a three-month internship. I match the program participants to an extensive university-employer-employee dataset and study their transition to labour markets. I find that the subsidy program successfully allows students to get professional work experience and has a positive effect on earnings growth. I then construct a direct measure of worker-job match quality using information on students and their labour market peers' skills. I show that subsidized students are working in workplaces and occupations in which their individual skills have between 5 to 10 percent higher return. These results suggest that internships provide an opportunity for students to demonstrate their skills on-the-job, and they may significantly reduce inefficiencies in entry-level labour markets.

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1 Introduction

Internships have become common among students aiming to enter the high skilled labour market. For example, in the United States, more than 60 percent of college students intern before graduation, and many graduates participate in multiple internships.[1] On the one hand, internships may increase efficiency in entry-level labour markets by diminishing asymmetric information between employers and entering workers. Both workers and employers may learn on-the-job about the suitability of workers to particular jobs and tasks. On the other hand, internships are often suggested to increase job precariousness and inequality in the early career.[2] Rather than offering stepping stones to young workers, employers may simply benefit from lower hiring costs and substitute full-time workers with temporary, skilled labour without helping them access more stable or higher ranked positions. Even if internship experience is associated with better labour market outcomes among recent graduates, it may simply reflect the fact that well connected and more skilled students are likely to get the most useful internship positions in the first place. Indeed, so far there is relatively little credible evidence on the potential benefits of internships on early labour market success.[3]

In this paper, I study whether internships help students find jobs that match their talents in the transition to high-skilled labour markets. To make progress, I leverage an internship subsidy program at a major Finnish university which allocates internship subsidies to randomly selected students. The program provides employers a subsidy if they hire a student for at least a 2-month paid internship. The subsidy covers about two thirds of the minimum hiring cost. In return, the employer should allow the student to work in expert duties under the supervision of a tenured employee and pay a minimum compensation. These features make the setting appealing to understand the role of internships in labour market transitions.

[1] About one half of these internships are paid and the other half unpaid (NACE 2014). While consistent and comparable estimates on the popularity of internships do not exist (in part due to lack of common definition of an internship) a wealth of reports from individual countries and studies on hiring practices suggests that the institution is widespread at least in most European countries.

[2] See e.g., Perlin (2011).

[3] Survey results suggests that students who intern during their studies fare better in the early labour markets: they are more likely to hold a job after graduation and earn a higher salary than those who did not intern (see f.e., Taylor (1988), Gault et al. (2000) and NACE (2014)) However, since there are potentially significant differences between the students who do internships and who do not (such as their motivation, effort, ability, and existing labour market networks) the descriptive evidence is not sufficient for understanding the independent role of internships in the early career. For example, see Knouse et al. (1999) for selection to internships among business students.

In the first part of the empirical analysis, I take advantage of a unique administrative dataset matching the program participants with student records and an extensive full-population employer-employee dataset. This allows me to track the students labour market transitions between occupations and employers before and after the program. I then show that students who are assigned a subsidy are more likely to have professional work experience and more exposure to high-skilled workplaces and jobs both during the subsidy program year and in the following years. This suggest that the subsidy program gives students a chance to demonstrate their skills, abilities and suitability to potential employers in a skilled environment.

Next, I use a unique measure of individual cognitive skill based on the Finnish Matriculation Exam scores to construct a direct measure of worker-job match quality. These data are available to for full population of high-school graduates in Finland since 1967 allowing me to measure the skills of both the program participants and their colleagues and co-workers. I measure match quality by estimating the return to individual skill-sets across jobs and comparing them to the market return to the same individual skill-set. I then show that subsidized students access jobs in which their individual skills have between 5 to 10 percent higher return when compared to the control group. I also find that the subsidized students experience higher earnings growth than the control group and that these earnings gains are concentrated on those students who had worse initial skill mismatches in their previous jobs.

Taken together, the results suggest that intenships provide students an opportunity to demonstrate their skills in tasks that are relevant to their later career. This opportunity allows students to access better jobs and workplaces in which they are likely to reap benefits later on. From a policy perspective, these results suggest that there is potential room improving the matching between workers and jobs in their early career.

The study is related to a large body of literature suggesting that imperfect information about worker productivity may generate important inefficiencies in early career labour markets.[4] In a baseline model in the literature, employers are not willing hire new workers if there is a risk that, after learning about their productivity, high productivity workers move to other firms. Hiring and experimenting with new workers is also constrained by regulation on temporary and fixed-term contracts which may not allow for sufficient

[4]See extensive survey by Acemoglu and Pischke (1999) on investment in training programs and Terviö (2009) for a model on the inefficiencies in hiring entry level workers. There is also extensive research in the literature on employer learning and informational asymmetries at entry, see f.e., Farber and Gibbons (1996) and Altonji and Pierret (2001).

flexibility from the employers side, increasing employment risk. On the other hand, new workers themselves may be unable to do unpaid work (or pay to be employed) to learn about their own productivity.[5] This may be due to credit constraints, laws prohibiting unpaid work contracts or the potential negative signals and stigma that such arrangements could cause to both parties. Social networks may alleviate these inefficiencies by allowing new workers to signal their type, but such networks only help workers with useful existing contacts and/or credible signals of their own type. These considerations suggest that there is potentially suboptimal level of public information on entrant productivity with potentially long lasting effects on young workers and inefficient matching in the early career hiring process. This gives rise to the potential benefits of public interventions to increase the available information.

In contrast, there are only a few existing studies aiming to directly study the role of internships, or similar interventions in early career labour market outcomes for high skilled workers.[6] First, and most directly related to this study, Saniter and Siedler (2014) study the effect of mandatory internships on labour market outcomes of students by taking advantage of introduction and abolishment of internships schemes across study programs over time at German universities. Using post-graduation survey data, they find evidence that internships have a positive effect on wages. Second, Nunley et al. (2016) study the effect of internships by a resumé audit and find that fictional college graduates with past internship experience are 14 percent more likely to be called for an interview still four years later than non-interns. Consistent with the above employer survey evidence, this suggest that internships are a way to signal student talent to potential employers. Third, Cerulli-Harms (2017) uses propensity score matching and finds evidence that interns are less likely to be employed and earn less than non-interns immediately after graduation, but catch up in the following years. She interprets this evidence as suggesting that internship experience sends a *negative* signal to prospective employers. Finally, Pallais (2014) studies the effects of providing potential employers with additional information on workers past performance in an online workplace. She finds evidence that additional information improves subsequent labour market outcomes in the entry level job market.

[5]Unpaid work is usually not allowed in most countries. Internships must usually be paid if employers derive any benefit from the work done by an intern. Yet, in practice these kind of arrangements seem to occur, potentially biasing the institution towards favouring those who can afford to work without pay.

[6]There exists a burgeoning empirical literature aiming to characterize the impacts of heterogeneous active labour market programs on wages and employment, but most of these studies concentrate on unemployed, disadvantaged or low-skilled workers (see Card et al. (2010)). Similarly, there exists an extensive literature on temporary-help jobs, which are also mostly targeted to low-skilled workers.

This study is also related to the recently emerging empirical literature on the role of matching in the labour markets. For example, Fredriksson et al. (2015) and Guvenen et al. (2018) show that idiosyncratic variation in worker-job match quality is a significant predictor of worker turnover and wage growth. However, there is still little evidence on the mechanisms through which better match quality emerges in the labour markets. I add to this emerging literature by showing studying the role of internships in decreasing mismatch in the early career.

This study adds to the emerging literature by providing the first analysis exploiting an individual level experimental design and taking advantage of extensive administrative data. In particular, the data allows to track students over time and across occupations and workplaces, which has not been possible in previous studies.

I proceed as follows. In Section 2, I describe the internships subsidy program. In Section 3, I describe the data and the methodology to measure worker-job match quality. In Section 4, I present the main empirical results. In Section 5, I conclude.

2 Institutional Background

2.1 Internships and the Subsidy Program

Internships and summer jobs are very common among university students and recent graduates in Finland and for many it provides the first opportunity to work in a professional work environment. Student surveys suggest that 75 percent of students intern before graduation and 25 percent do more than a single internship.[7] Many private companies, non-governmental organizations (NGOs) and public institutions offer internship opportunities to students in various educational fields. Majority (around 60 percent) of internships are in the public and non-profit sector. This is especially the case in the field of social sciences where many of the students can also be expected to work after graduation and where many of the sought-after internships exist.

Internships are also institutionally supported by Finnish universities. This happens mainly through two important ways. One the one hand, most universities aim to promote student attachment to labor markets by either giving academic credit to students who intern during their studies or by including compulsory internships into study programs. For example, in 2017, 90 percent of Finnish students could get academic credit for an

[7] See student survey by (Akava 2017)

internship. On the other hand, most universities also offer direct subsidies to employers who hire their students as paid interns. In 2017, 82 percent of students could apply for a subsidy and about one third actually received a subsidy. This

In this paper, I concentrate on the internship subsidy program at the University of Helsinki. The program is targeted to students typically enrolled in a 5-year master's program.[8] With limited financing, the subsidy program is commonly oversubscribed and participating faculties are forced to select which students to allocate the subsidies to.[9] I take advantage of the fact the Faculty of Social Sciences randomized the allocation of the subsidy between 2010 and 2013 among all the program applicants. This provides, in effect, a randomized controlled trial. However, the program itself has been completely administered and run by the faculty.

The internship subsidy program and the allocation procedure runs as follows. First, in November-December, students may apply for a subsidy for an internship that starts during the following calendar year. The subsidy amounts to around 3,000 euros, or about two thirds of the minimum nominal compensation, including employer contributions. Student may submit applications via a short online form on the faculty website. All enrolled students are eligible for the program, given that they do not already have academic study credit for an internship, and that they have not previously received a subsidy from the university.

In the second stage, the faculty randomly selects the students who get a subsidy among the applicants. The randomization takes place at the discipline level so that, conditional on the number of applicants, every applicant has an equal chance (about one half) of receiving the subsidy.

Third, students who are assigned a subsidy search for an internship position in a firm, government institution or a non-governmental organization. Typically this takes place in early spring when internships positions for the summer are announced. Many positions are marketed online on a university administered job openings platform where employers may post ads targeted to students in specific educational fields. But students may also approach any potential employer on their own. Some students may also already have a potential employer before applying for a subsidy. However, if the student is, for whatever

[8]Most students applying to a study program at a Finnish university are directly admitted to both the Bachelor's and Master's level degrees and courses. Most students are expected to finish with a Master's degree.

[9]Over-subscription results from the combination of fixed level of funds to run the program and a fixed and predetermined level of the subsidy paid per student.

reason, not able to find a job or she declines the subsidy, the subsidy is allocated to a person in a reserve list.

Finally, if a student finds an internship position which satisfies the program requirements, the university reimburses the employer after the internship is completed. As per the university rules, i) the employer needs to offer the student "expert duties that enable the trainee to apply and develop his or her skills to the greatest degree possible", ii) the employer needs to appoint a supervisor for the duration of the internship, iii) the internship needs to last for at least two months and iv) the employer "must pay a salary amounting to at least the minimum wage determined by the Social Insurance Institution" (Kela), which in 2017 was 1,187 euros.

3 Data and Descriptive Statistics

In the empirical analysis, I use information from multiple administrative sources to track the students transition to the labour markets. This section explains where these data come from and how I use these data to measure the impacts of the subsidy program.

3.1 Administrative Data

Subsidy lottery First, the most important information for the empirical analysis comes from the lottery results which list all the applicants to the subsidy program and those who were assigned a subsidy. I obtain this information and the student id's from the University of Helsinki Faculty of Social Sciences. Importantly, this data allows me to identify the students who were initially and randomly allocated a subsidy. If a student did not take up the subsidy, someone from the reserve list had an opportunity to use a subsidy instead, creating imperfect compliance. Thus, the main empirical analysis will be based on comparing students who were initially assigned a subsidy. However, based on aggregate figures released by the Faculty, at least 75 percent of students initially assigned the subsidy also used it. This suggests that at the minimum the difference between the control and the treatment group subsidy takeup was 50 percentage points, or 200 percent.[10]

Student records Second important dataset comes from the University of Helsinki Student Registry. In particular, I collect administrative data on the universe of students en-

[10]Unfortunately, I do not have the information which students actually used the subsidy or which students in the reserve list obtained the subsidy.

rolled at the Faculty of Social Sciences between 2007 and 2016. It includes comprehensive information on student enrolment status and graduation dates, field of study, courses and their instructors, grades and credits. This information is critical for the research design as it allows me to identify the application quota through which each student applied to the program. I can also use this information to control for student characteristics (such as grade point average and study credits) that are likely to be important drivers of labor market success. The data also includes information about university credited internships. I use this information to study the impact of the subsidy on the frequency of internships.

Labor market outcomes Third, to analyze student labour market outcomes, I match student registry data to the full population registries using anonymized students' social security numbers. The panel data includes information on all individuals residing in Finland between 1988 and 2017, thus in effect eliminating any meaningful attrition bias. It includes comprehensive information on annual labor earnings, employment and end-of-year occupation codes. The data also includes a complete job history, including establishment ids as well as start and end dates for every job contract the worker has had. The data thus provides the main individual labor market outcomes of interest as well as measures of characteristics of workplaces (coworkers and their characteristics) as well as occupations (other workers in the same occupation) in which the students work before, during and after applying to the subsidy program.

Internships Fourth, I use a combination of different administrative records to measure the frequency internships. The administrative work history data does not allow to directly discriminate whether a job is an internship as it only contains information on the starting and end dates of the contract. Thus, to measure internships, I use two different proxies for internship experience. On the one hand, guided by the knowledge from student surveys, we know that a significant majority of students do internships in the public sector. This is especially the case for students in social sciences where many of the prestigious jobs are indeed in the public sector. This includes internships in ministries, embassies and government research institutions, many of which typically hire a large amount of interns. Thus, my first (and preferred) measure of internship experience is to look at whether students were working in the public sector during the program year.

On the other hand, we have a direct measure of student internships derived from the student registries. In particular, I take the all course records and identify credits for

internships based on the course title using variants of the Finnish language word for "internship" (harjoittelu). However, not all the internship credits have a title that directly includes the word internship, and this results in measurement error and a downward bias in the empirical estimate.[11] This provides us with two alternative measures that are both imperfect, but as I will discuss later, provide consistent results.

Skill measures Finally, I measure individual cognitive skills using the Finnish Matriculation Exam Scores available from the Matriculation Exam Board. The Matriculation Exam is a standardized examination at the end of academic high-school which, in practice, qualifies students to apply to universities.[12] This allows us to match the majority of the students in our sample to their exam grades, again using their anonymized social security numbers.

Now, several features of the Matriculation Exam make it an appealing data source on student's skills for the purposes of my empirical analysis. First, students have real stakes in the examination. Most importantly, universities and other educational institutions use the results to select students to their study programs.[13] This is in contrast to many survey based skill measures used in the empirical literature where individuals might not have incentives to exert significant effort. Second, matriculation exams are graded in a standardized way so that we can compare individuals across schools and over time. In particular, all students in the country take the same exam in a given biannual examination at the end high school. Exams are assessed by external evaluators who are appointed by the Exam Board and who do not know the students and thus have no incentives to artificially inflate their scores. After evaluation, tests are graded on the curve so that the final grades are in effect determined by the relative ranking within the high-school cohort.

The coverage of the data also makes it a unique and appealing source of information. In particular, the exam scores are available for all high-school graduates since 1967. This allows me to measure the skills of the program participants, but also of the majority of working age, high-school educated population and practically the skills of every college graduate in the labour markets. This is important for our proposed measure of match quality, as will be detailed in the next section. Further, compared to other studies using

[11] There is also a possible upward bias in the internship credit measure as the incentives to register an internship with the university is likely to be biased towards the subsidized internships. In the worst case, it could be that only those who received the subsidy are willing to reger the internship. However, students also have the incentive to register other internships as they do get study credits for them.

[12] Matriculation is no longer a legal requirement to apply to universities.

[13] Universities may also arrange their entrance examinations to complement the selection process.

registry-based or population level datasets, the unique feature of the dataset is that it is available for both men and women. This is important, as a significant part of the program participants are, in fact, women.

In the empirical analysis, I concentrate on exam scores in two subjects that receive the highest weight in most university applications: Finnish language and Mathematics. The main benefit is that language and mathematical skills are likely to be capture complementary cognitive skills (math and verbal skills) that are rewarded in the labour markets.[14] Further, while students can choose which other subjects they take a test in, Finnish language and mathematics have been compulsory subjects for most high-school cohorts and we observe their test scores in our dataset.

3.2 Measuring Worker-Job Matching

In this paper, my preferred measures of skill mismatch are based on a simple concenptual framework. Suppose for simplicity that individual i has a skill set $S_i \in \{s_i^M, s_i^V\}$ where s_i^M is math and s_i^V is verbal skill vector. Now, we may decompose worker i's wage as

$$Y_i = S_i\beta + \epsilon_i \tag{1}$$

where β is the return to skill and ϵ_i includes other potentially unobservale factors that affect wages. Similarly, the return to skill in job j is defined as

$$Y_{i,j} = S_i\beta_j + \varepsilon_{i,j} \tag{2}$$

Next, I define worker-job match quality $M_{i,j}$ as the difference between market return and return in job j

$$M_{i,j} \equiv S_i\beta_j - S_i\beta \tag{3}$$

The match quality thus gives a measure of how well a worker is matched to a job. A negative match value suggest that a worker is employed in a job where the return to her skills is lower than on average (or lower than her expected return), and a positive match value suggests that a worker employed in a job where the return to her skills is higher

[14]The formats of these exams has also been fixed over time so that it is reasonable to assume that over time the exams measure the same type of skills. Another more practical benefit of using these exams is that up until 2005 both exams were compulsory, so that they cover most high school graduates. In 2005 it became optional to complete the mathematics exam, but still a great majority of all students complete the exam.

than on average (or higher than her expected return). In this framewrok, variation in match quality comes both from the variation in individual skill set S_i as well as variation in the return to these skills across jobs β_j.

In a typical empirical setting it is hard to measure both the skill of individual workers as well as the return to those skills. To make progress, I first take advantage of the individual level cognitive skill proxies from the Matriculation Exam Scores. While cognitive skills alone are not likely to capture all of the skills that are rewarded in the labour markets, a wealth of evidence suggest that they do play an important role in determining earnings, and their role remains especially important in professional labour markets that are the focus of this study.[15]

Second, it is even more demanding to know what the differences in return to skills β_j are across different jobs. To make progress, I use information on earnings and skills from out-of-sample workers in Finland. In particular, I estimate the market return to skills using a non-parametric specification

$$Y_i = \sum_{s \in S} \sum_{g \in G} I_i^s * I_i^{s,g} \beta^{s,g} + u_i \tag{4}$$

where Y_i is the (log) annual labour earnings, I_i^s is a dummy for skill s and $I_i^{s,g}$ is a dummy for having a level g skill of s, i.e., I regress the annual earnings on dummies for having a particular grade g in exam s. Having separate dummies allows for potentially important non-linearities in the skill return. I estimate this equation using the universe of workers in Finland between 2010 and 2015 excluding all the students in my main main empirical analysis.[16]

Next, I estimate the return to skills separately for every job j

$$Y_{i,j} = \sum_{s \in S} \sum_{g \in G} I_i^s * I_i^{s,g} \beta_j^{s,g} + u_{i,j}. \tag{5}$$

where the sample population is the same as above, but job j is defined either by 3 digit occupation code or an establishment id. The sample consist of all workers in every work-

[15] See f.e., Murnane et al. 1995, Heckman et al. 2006, Lindqvist and Vestman 2011, Castex and Dechter 2014, Deming 2017, Edin et al. 2017 and Jokela et al. 2017)

[16] Table 4 shows the results based on the above OLS specification for a pooled sample of workers in 2010 and 2015. The results suggest that both maths and language skills have a positive labour market return. The gradient is particularly steep for high advanced maths skills. There are two levels of maths in high-school and I treat them as separate subject so as to keep the specification simple. An alternative specification would be to interact maths skills with the level of maths.

place and in every occupation excluding all the students in my main main empirical analysis.

Finally, using the above empirical estimates in (4) and (5), I measure match quality by the difference between predicted market return and return in job j

$$\hat{M}_{i,j} \equiv S_i\hat{\beta}_j - S_i\hat{\beta} \tag{6}$$

This provides the measure of matching I use in the empirical analysis.

3.3 Mismatch of Talent in the Early Career

Before to main empirical analysis, it is useful to study what the above mismatch measures tell us about students transition to the labour market in general. Figures 1 (a)-(c) characterize the distribution of estimated worker-job mismatch as a function of time from the program participation. Both figures demonstrate that, at the time of applying to the internship program ($t-1$), there is a considerable mismatch between the jobs students hold and the skills they have. For example, Figure 1 (a) shows that a significant mass of students are placed in the to positive side of the mismatch distribution. In particular, 66 percent of students are working in a firm or a workplace where they can expect to earn less than the market return for their individual skill-set. On average this mismatch is around 54 percent of annual earnings as demonstrated in Figure 1 (c). Similarly, based on occupational mismatch measure, 70 percent of students are working in an occupation where they can expect to earn less than expected with their skill-set (Figure 1 (b)). On average, students earn 40 percent less than expected with their skills in their current occupation.

On the other hand, Figures 1 (a)-(c) also clearly demonstrate that there is a significant improvement in match quality over time as students transition to labour markets. First, as seen in both Figures (a) and (b), mismatch distribution shifts to to the left, i.e., the majority of students become better matched in the following years. Indeed, the share of mismatched students drop to 45 and 31 percent based on the firm and occupational measure respectively. Also, the average mismatch becomes significantly smaller 26 percent and -9 percent respectively.

Now it is useful to also compare how these mismatch numbers compare to other measures of labour market engagement. Consider first Figure 1 (d) which shows the share of students working in selected occupational categories. There are two important trends.

First, the share of students who are working in professional occupations increases from mere 10 percent before the subsidy program to over 45 percent in the following 5 years. In contrast, the share of students who are working in low skilled service and sales occupatins or clerical office jobs declines significantly. Thus, an important driver of the declining occupational mismatch is the shift of students away from low-skilled occupations to professional occupations.

These descriptive facts characterize quantitatively the fact that most students are working in jobs and workplaces in which their skills do not earn the highest or even the market return. However, as students enter the labour market, many can expect to get a hold of a job where they are. However, how exactly this transition happens remains still unexplored. This motivates the main empirical analysis in this paper that we are going to go next.

4 The Effect of Subsidies on Labour Market Outcomes

This section presents the empirical analysis of the effects of the subsidy program on students labour market outcomes. I begin studying the pre-treatment characteristics of the program applicants. I then specify the empirical strategy I will use in most of the empirical analysis and present the baseline results. Finally, I study how the subsidy program affected matching in the early career labour markets using alternative matching measures.

4.1 Pre-treatment Characteristics of Subsidy Applicants

Table 1 shows the pre-treatment characteristics of the 921 students who applied for an internship subsidy in 2010-2013. Columns (1) and (2) show the characteristics of the applicants who were initially assigned a subsidy and those who were initially not assigned a subsidy, respectively. Overall, the two groups are balanced and one cannot reject a joint null hypothesis test ($F = 0.91$, $p = 0.57$). Most individual statistics are also not statistically significantly different between (see Column 3) as expected under random allocation of subsidies.

The average applicant is 26 years old and 72 percent of applicants are women. These are typical numbers for university students in Finland and for the Faculty of Social Science majors, although the average age is also influenced by the fact that there are a few students above 50 years old. At the time of applying, average student has around 160

study credits, which is just below the requirement for a bachelor's degree (180). As the target degree for most of the students is a Master's, one can expect students to graduate within two years of the program.

The majority of applicants have some pre-existing labour market experience. At the time of application, more than 60 percent of the students have an active labour market contract and have worked on average almost nine months during the year. However, the majority of students are working in low skilled occupations and the most frequent occupational title is a sales worker. Many of the students are also working less than full time which is evident when looking at their annual earnings which are around 10,000 euros compared to the 25,000 euro average earnings of workers in the same occupations among the full population and compared to co-workers who also earn on average 26,000-29,000 euros.

4.2 Empirical Strategy

To test the effects of the subsidy on students labour market outcomes, I estimate intention to treat effects using an event-study -type framework. The motivation is that it allows me to fully take advantage of the panel dimension of the administrative data sources and transparently describe how students progress in their early career labour market transitions. In particular, I estimate models of the following form

$$Y_{it} = \alpha + \sum_{s=-12}^{60} (\tau_s + \beta_s * Assigned_i)\mathbf{1}\{S_{j(i)t} = s\} + X_i\gamma + \varepsilon_{it} \tag{7}$$

where $S_{j(i)t}$ is the time to subsidy application and $\mathbf{1}\{S_{j(i)t} = s\}$ is a set of dummies for student i having been assigned the subsidy s months ago, $Assigned_i$ is a dummy for students who were assigned a subsidy at $t-1$ and X_i includes application quota fixed effects. My main interests lies in the coefficients β_s which captures the difference between the students who were assigned a subsidy compared to workers that were not assigned a subsidy at time s.

In the majority of the analyses, the unit of analysis is an applicant-year or an applicant-month. The sample consist of 914 applicants. In the baseline analysis, I will focus on specifications where I include applicants who applied more than once for a subsidy, but cluster all standard errors at the individual applicant level. However, carrying out the analyses with a sample restricted to first-time applicants does not materially affect any of

the following results.

4.3 Internships, Peers and Job Characteristics

Figures 2 (a)-(d) characterize students' labour market transition during the subsidy program and the following four years using both individual level work-history data at the monthly frequency and annual registry based data.

First, Figures 2 (a) and 3 (a) show that students assigned the subsidy are significantly more likely to work for a public sector employer during the 12 months when they may benefit from the subsidy. Given that students in social sciences mostly do internships in the public sector, this is a clear indication that the subsidy helps the assigned students to land internships during the year when assigned the subsidy. On the one hand, Figure 2 (a) shows that summer months before and during the subsidy program are when a significant share of students work in the public secotr. However, during the subsidy program this spike is significantly higher for the treated students and starting from June when students are on a summer break and are also expected to do the majority of the internships. The difference between the treated and control group is 12 percentage points which is around 50 percent increase from the previous year and 40 percent increase compared to the control group. In contrast, what is also clear from the figure, is that the subsidy assignment seems not to be associated with any long-run impact on public sector employment as assigned and non-assigned students are equally likely to work in the public sector in the two years following the assignment.

Figures 2 (b) and 3 (b) shows a similar figures for monthly employment. The figures demonstrate two important regularities. First, in during their studies, students work mostly in the summer months, but start working during the whole years as their studies progress. Second, the effects of the subsidy on employment is negligible. While there is again a spike in employment during the summer months of the assignment, the difference is much smaller in both relative and absolute magnitude than for public sector employment. However, there appears to be no significant crowding out of private sector employment as there is no difference in private secotr employment. This suggests that most students hold on to additional private sector jobs in addition to the internships. This is consistent with working shorter hours in service sector jobs which are common among students as show in the previous section.

Figures 4 (a) and 5 (a) show that students are more likely to receive study credits for an internship when assigned a subsidy. The difference is more pronounced in the first

year, likely due to the fact that applicants who were not allocated a subsidy can re-apply again in the following year. However, this measure clearly misses a significant fraction of actual internships that actually took place. In particular, the Faculty reports that 75 percent of the treated student actually took up the subsidy, yet only 40 percent of these students are registered in the internship subsidy records by two years time.[17] One way to adjust for this discrepancy would be to scale these observed internship credits based on these aggregate take-up figures by a factor of 1.875. In this case, the difference in absolute terms between the treated and control would be around 28 percentage points and 215 percent in the first year and 20 percentage points and 72 percent in the following year.

Figures 2 (c)-(d) and 3 (c)-(d) characterise the coworkers students are exposed to over time and reveal two key findings. First, over time students are exposed to more and more skilled coworkers over time as the average earnings of their coworkers increases during the six years by around one fourth from below 30,000 to over 37,000 and the share of co-worker with a Master's degree almost doubles from 20 to 40 percent. This characterises the dramatic shift in the peers the students are exposed to once entering the labour markets. Second, during the program year, students assigned a subsidy have both higher earning and more educated coworkers. This suggest that the program pushes and exposes students to more prestigious and more skilled workplaces and away from low-skilled workplaces that they typically work in during their studies.

Another way to see how the program pushes students to more prestigious jobs is to look at the occupations they hold. Figures 4 (c)-(e) and 5 (c)-(e) address these issues. First, Figures 4 (c) and 5 (c) shows that treated students are significantly more likely to work in a professional job on the program year. The difference between the treated and control group is over 5 percentage points or 25 percent relative to the control group. These results suggest that the program was effective in achieving its primary purpose, which is to allow the students to get professional work experience before graduating. However, more interestingly, there is a significant persistent effect also in the following years where the treatment group is again 5 percentage points more likely to keep working in professional jobs. Figures (d) and (e) provide additional evidence that treated students were working in more prestigious and more skill-demanding occupations by showing that they were working in higher paid occupations with more skilled occupational peers.

Finally, Figures 4 (b) and 5 (b) show that treated students out earn their control-peers

[17] There is no strict deadline to register an internship so, in theory, it may also be registered in the following year in my data.

by around 1000 euros in annual earnings. Importantly, both treated and control students see large increases in their annual earnings which is consistent with both increasing work hours and higher paid jobs as suggested above. However, treated students see faster earnings growth up to 3 years after the program after which the control students catch up.

4.4 The Effect of Subsidies on Matching Workers and Jobs

The previous evidence suggests that the subsidy program successfully provided students exposure to professional jobs with higher skill requirements and exposure to more skilled peers. Next, I will investigate whether the program was successful in helping students access jobs that were better suited for their skills. For this purpose, I will focus on skill mismatch measures developed in the previous sections.

The main empirical results are presented in Figures 2 (f) and Figures 3 (f) which show the firm-worker match quality around the subsidy assignment program. These figures show that the treated students were working in workplaces where their skills could earn up to 20 percent higher return than similar workers in the control group. While the monthly results are somewhat noisy, the effects is persistently positive and pooling the effects over time yields an estimate of around 12 percent higher expected earnings in the five years following treatment (see Table 2, Panel B).[18] Similar results follow from the analysis that concentrates on the occupation match measure (see Figure 4 (f) and 5 (f)). However, the mismatch effect is more pronounced in the initial program year when the effect is around 12 percent in expected earnings. On average, the reduction in occupational mismatch is around 5 percent in expected earnings during the five years following the program.

Another way to analyse the match between workers and jobs which does not rely on estimating skill returns is to simply look at the earnings premium of college graduates at the workplace that students work in (see Figures 2 (e) and 3 (e)). Indeed, the treated students are working in workplaces where a Master's degree has an up to 10 percentage point earnings premium.

[18]Firm match quality here is not mismatch as in the other figures but a match quality. This was not intentional, but it is a mistake and will be corrected. However, it will only change the sign of not the implication the results.

4.5 Subsidy, Mismatch and Earnings

Now, if the main channel through which the subsidy program is by allowing students to match to better jobs, we would expect that students who are initially most lacking experience, or who are working in jobs very far away from where their skills would provide the highest return, would be most likely to benefit from such a program.

To investigate this more formally, I will compare the effects of the subsidy program by the initial mismatch and consider its effects on students. Table 3 shows the results in a pooled specification which allows for an interaction between initial mismatch and the treatement status. Panel A shows that there is no significant heterogeneity in the effects of the subsidy on work experience in the public sector on on employment neither in the short or the long run. This suggest that initial misallocation did not affect the type of internships individuals went to during the program year. However, it also suggests that even those who were significantly mismatched initially, were not held back in terms of employment.

Things look different however, once one studies the heterogeneity in earnings responses in Panel B. In particular, students who were working in workplaces where they were earning little for their skill set, benefited more from the subsidy program in terms of future earnings. These earnings responses were more concentrated in the few years following the assignment. This suggests that the program improved exactly those students' worker-job matches who were initially most severely misallocated.

5 Conclusion

This paper studies the impact of internship subsidies on early career outcomes of university students in Finland using a unique administrative dataset, an experimental research design. The results suggests that the internship subsidy program improved students labour market attachment by promoting access them to higher ranked occupations, jobs in their own field and jobs in more prestigious workplaces as measured by co-worker characteristics. Further, by using a new measure of worker-job match quality based on individual skill measures, I show that the subsidy assignment improves the matching of students to jobs in which the students have a higher return.

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Table 1: Pre-Treatment Characteristics

	(1) Treated	(2) Control	(3) (1)-(2)	(4) *p-value*
Panel A: Student Characteristics				
Age	26.23	26.07	0.03	0.88
	(3.75)	(3.67)	(0.23)	
Women (%)	72.33	72.71	-0.45	0.87
	(44.79)	(44.59)	(2.84)	
GPA (0-5)	3.67	3.65	0.03	0.36
	(0.53)	(0.51)	(0.03)	
Study Credits	165.34	163.52	-0.66	0.90
	(88.44)	(78.53)	(5.28)	
On Student Benefits (%)	81.16	86.65	-4.44	0.06
	(39.15)	(34.04)	(2.38)	
Panel B: Job Characteristics				
Earnings (euros)	10243.15	9814.96	122.32	0.82
	(8947.67)	(8167.61)	(534.68)	
Work Months	8.80	8.63	0.11	0.68
	(3.71)	(3.85)	(0.27)	
Employed (%)	64.65	62.35	2.40	0.46
	(47.86)	(48.50)	(3.24)	
Professionals (%)	6.98	6.97	-1.41	0.39
	(25.51)	(25.49)	(1.64)	
Techn./Assoc. Prof (%)	7.91	6.57	1.61	0.36
	(27.02)	(24.81)	(1.76)	
Clerical (%)	15.58	13.55	2.46	0.30
	(36.31)	(34.26)	(2.39)	
Service and Sales (%)	19.30	21.71	-1.16	0.67
	(39.51)	(41.27)	(2.72)	
Occ. Earnings (euros)	24911.31	24579.66	-361.69	0.62
	(9185.37)	(9126.95)	(726.21)	
Panel C: Workplace Characteristics				
Coworker Earnings (euros)	29169.35	26381.31	2338.89	0.05
	(14792.30)	(13554.82	(1205.37)	
Secondary Degree (%)	44.39	47.21	-2.63	0.18
	(22.91)	(22.44)	(1.96)	
Bachelors's Degree (%)	13.53	14.90	-1.32	0.19
	(11.50)	(12.54)	(1.02)	
Master's Degree (%)	20.19	15.65	4.31	0.02
	(23.00)	(19.75)	(1.88)	
N	430	502	932	

Table 2: Pooled ITT Estimates of Employment and Worker-Job Mismatch

	(1)	(2)	(3)	(4)
Panel A: Employment				
	P(Works in Public Sector)		P(Has any job)	
	0-12m	0-60m	0-12m	0-60m
Subsidy	0.058***	0.004	0.007	-0.014
	(0.022)	(0.022)	(0.020)	(0.016)
N	11,622	54,534	11,622	54,534
Quota FE	Yes	Yes	Yes	Yes
Panel B: Match Quality				
	Workplace Mismatch		Occupation Mismatch	
	0-12m	0-60m	0-12m	0-60m
Subsidy	-0.52	-0.120*	-0.120**	-0.51*
	(0.070)	(0.072)	(0.048)	(0.028)
N	6,988	32,814	530	2,991
Quota FE	Yes	Yes	Yes	Yes

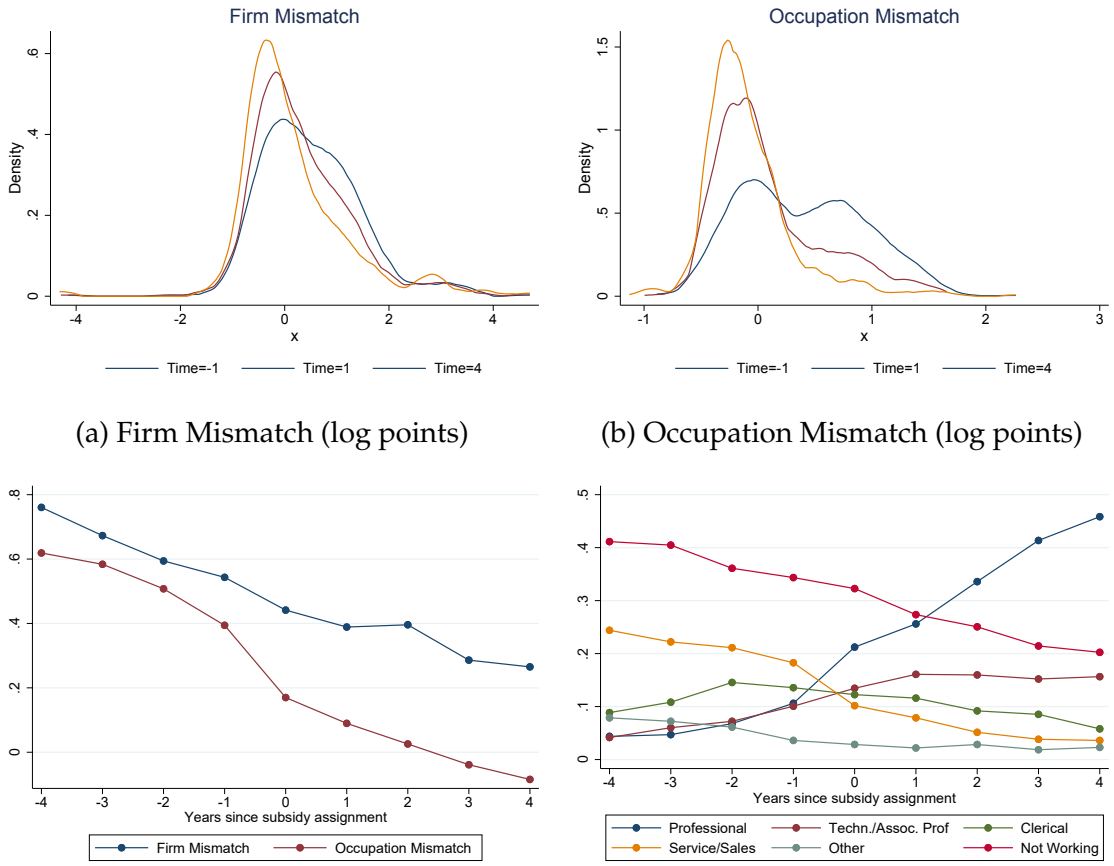
Table 3: Pre-Treatement Mismatch, Employment and Earnings

	(1)	(2)	(3)	(4)
Panel A: Mismatch and Employment				
	P(Works in Public Sector)		P(Has any job)	
	0-12m	0-60m	0-12m	0-60m
Subsidy	0.091***	0.003	0.016	-0.004
	(0.029)	(0.028)	(0.022)	(0.017)
Subsidy*Mismatch	-0.022	0.014	-0.004	0.000
	(0.025)	(0.023)	(0.018)	(0.014)
Mismatch	0.024	-0.010	-0.014	-0.007
	(0.020)	(0.017)	(0.015)	(0.012)
N	9,451	44,347	9,451	44,347
Quota FE	Yes	Yes	Yes	Yes
Panel C: Mismatch and Earnings				
	Annual Earnings ('000)		Annual Earnings ('000)	
	0-12m	0-60m	0-12m	0-60m
Subsidy	-0.576	-0.161	0.114	0.785
	(0.986)	(1.054)	(1.368)	(1.359)
Subsidy*Mismatch	1.902***	1.803*	0.362	0.995
	(0.819)	(0.985)	(1.716)	(1.887)
Mismatch	-2.086***	-2.052***	-3.034***	-1.051
	(0.663)	(0.785)	(1.080)	(1.182)
N	491	2,427	474	2,359
Quota FE	Yes	Yes	Yes	Yes

Table 4: Language and Math Skills Have a Positive Labour Market Return

		(1) Wage	
Language:	I	-0.258***	(0.009)
	A	-0.036***	(0.002)
	B	0.000	(0.001)
	C	0.040***	(0.001)
	M	0.059***	(0.001)
	L	0.067***	(0.001)
Adv. Math:	I	0.087***	(0.003)
	A	0.208***	(0.002)
	B	0.212***	(0.002)
	C	0.279***	(0.002)
	M	0.361***	(0.002)
	L	0.451***	(0.002)
Basic Math:	I	0.127***	(0.002)
	A	0.056***	(0.002)
	B	0.033***	(0.002)
	C	0.025***	(0.002)
	M	0.072***	(0.002)
	L	0.075***	(0.002)
Constant		9.999***	(0.000)
Observations		10,895,054	
R-squared		0.019	

Note: Robust standard errors in parentheses: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.



(a) Firm Mismatch (log points)

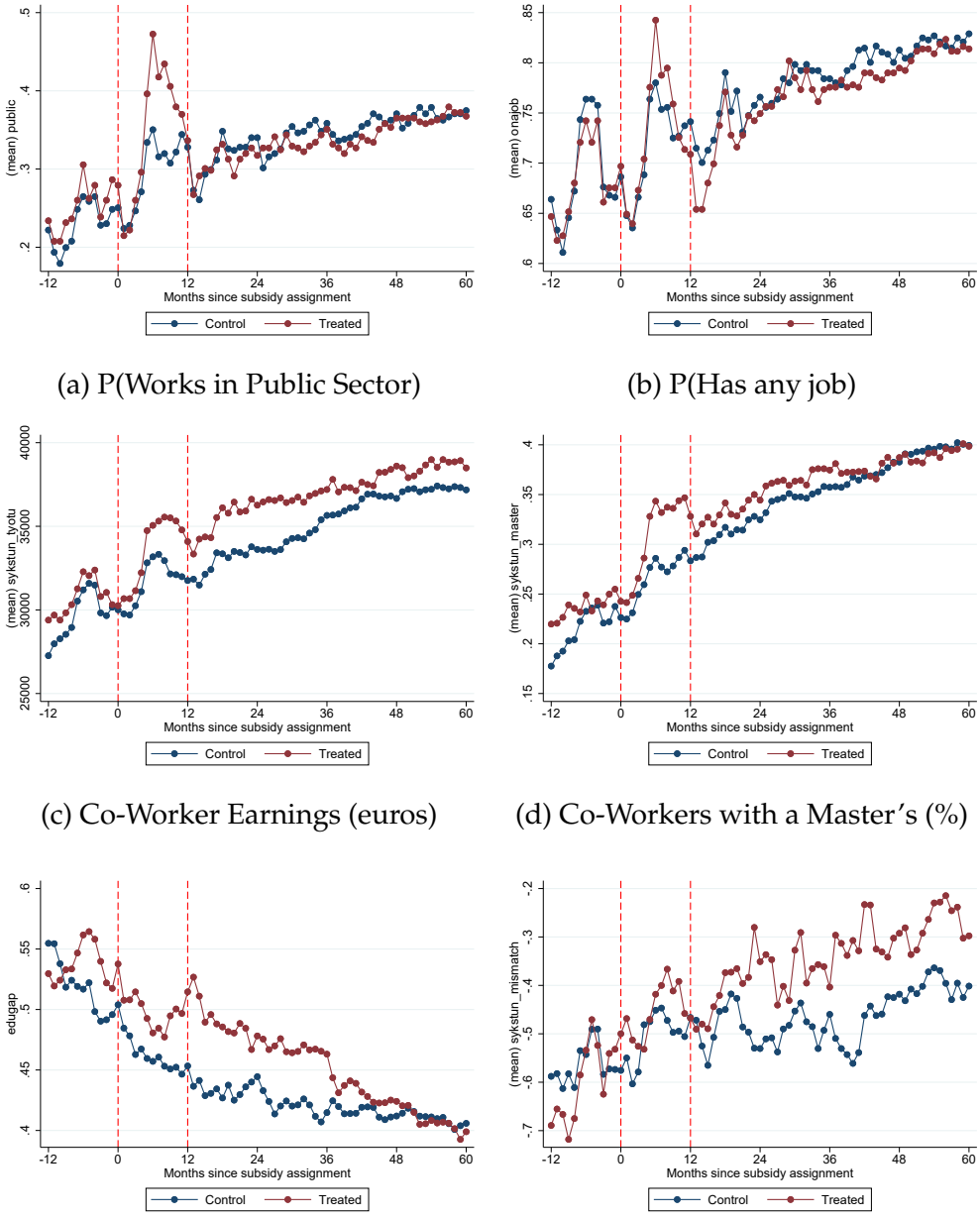
(b) Occupation Mismatch (log points)

(c) Mismatch over time (log points)

(d) Occupation shares over time

Figure 1: Mismatch over Time

Note: Panel (a) shows distribution of match quality before and after the subsidy assignment based on comparing returns skill at the workplace to the market return. Panel (b) shows distribution of match quality before and after the subsidy assignment based on comparing individual returns to skill in the occupation to the market return. Panel (c) shows the average mismatch as a function of time from aubsidy assignment. Panel (d) characterizes the share of students working in selected 1-digit occupational categories.



(a) P(Works in Public Sector)

(b) P(Has any job)

(c) Co-Worker Earnings (euros)

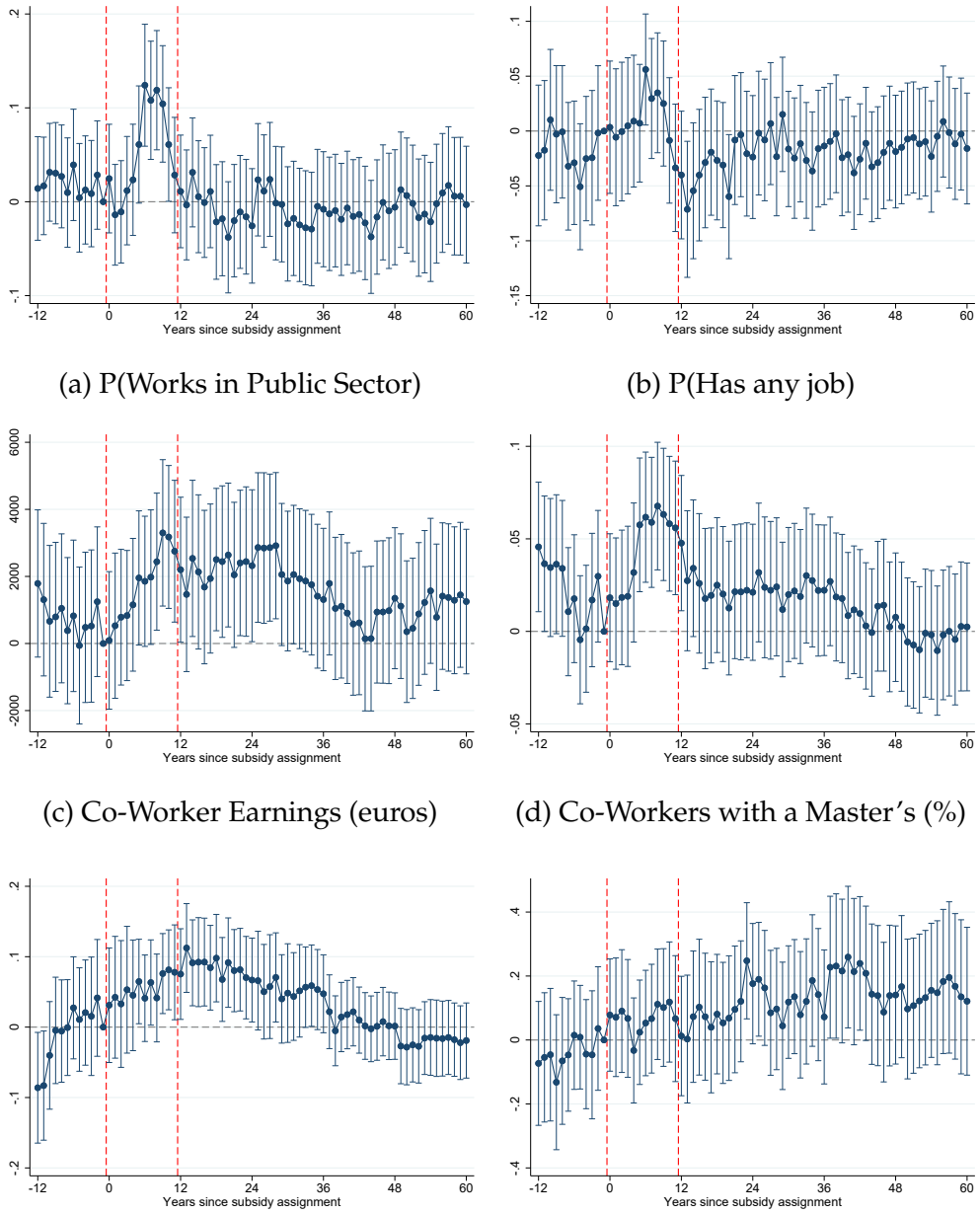
(d) Co-Workers with a Master's (%)

(e) Workplace College Premium

(f) Workplace Match

Figure 2: Trends in Workplace Outcomes by Subsidy Assignment Status

Note: Panel (a) shows the probability of working in the public sector as a function of time from the subsidy assignment by assignment status. Similarly, panel (b) shows the probability of working in any job, panel (c) shows the average earnings of coworkers in the same establishment, panel (d) shows the share of coworkers with a Master's degree. Panel (e) shows the earnings gap between the college graduates (Master's degree) vs non graduates at the workplace. Panel (f) shows the workplace match quality (negative of mismatch). All figures are based on individual work history files and use information on the contract dates and coworkers in the same establishment.



(a) P(Works in Public Sector)

(b) P(Has any job)

(c) Co-Worker Earnings (euros)

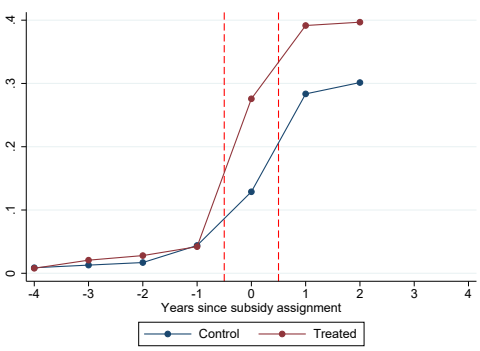
(d) Co-Workers with a Master's (%)

(e) Workplace College Premium

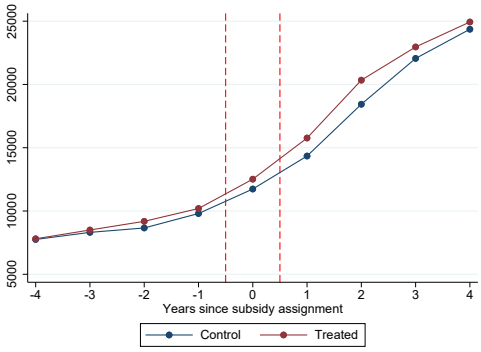
(f) Workplace Match

Figure 3: ITT Effects of Assignment on Workplace Outcomes

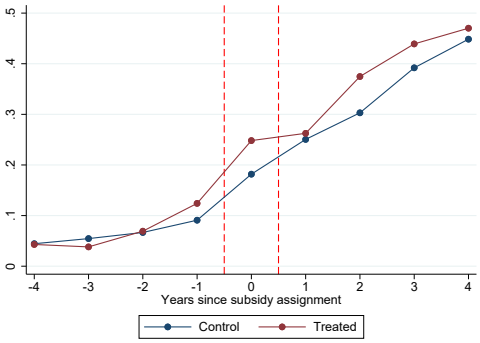
Note: These figures show event-study results comparing the outcomes of students who were initially assigned an internship subsidy vs those who were not. Panel (a) shows the probability of working in the public sector as a function of time from the subsidy assignment. Similarly, panel (b) shows the probability of working in any job, panel (c) shows the average earnings of coworkers in the same establishment, panel (d) shows the share of coworkers with a Master's degree. Panel (e) shows the earnings gap between the college graduates (Master's degree) vs non graduates at the workplace. Panel (f) shows the workplace match quality (negative of mismatch). All figures are based on individual work history files and use information on the contract dates and coworkers in the same establishment.



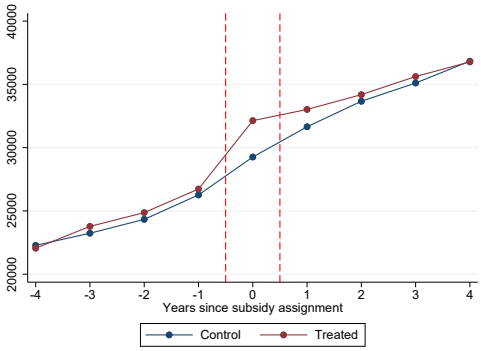
(a) P(Internship Study Credit)



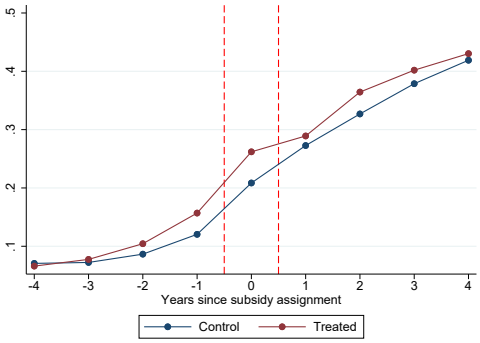
(b) Annual Earnings (euros)



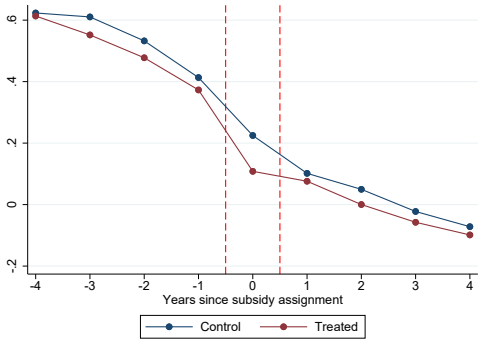
(c) P(Works as Professional)



(d) Earnings in Occupation (euros)



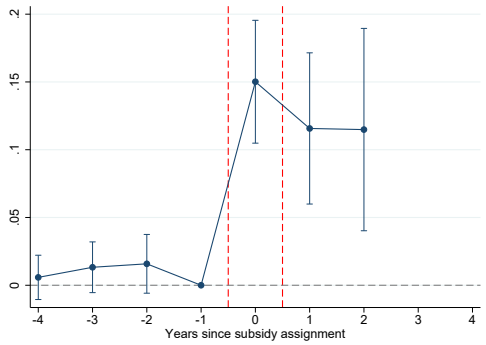
(e) Master's in Occupation (share)



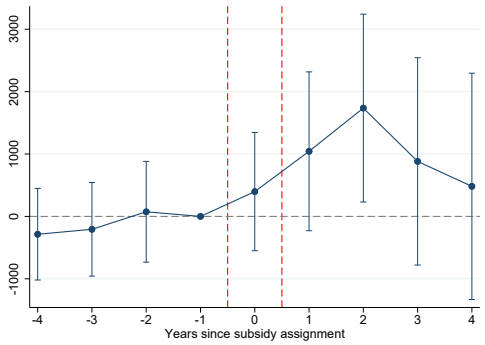
(f) Occupational Mismatch

Figure 4: The Effect of Assignment on Workplace Outcomes

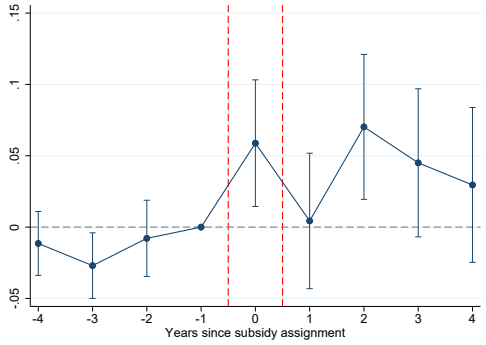
Note: These figures show trends in job characteristics as a function of years from the subsidy assignment by assignment status. Panel (a) shows the probability of registering an internship credit in the study registry as a function of time from the subsidy assignment. Similarly, panel (b) shows annual earnings, panel (c) shows the probability of working in a professional occupation, panel (d) shows the average earnings among all workers in the same occupational code and panel (e) shows the share college graduates (Master's degree) among all workers in the same occupational code. Panel (f) shows the occupational mismatch showing the gap between earnings in current occupation vs expected earnings with students skill-set.



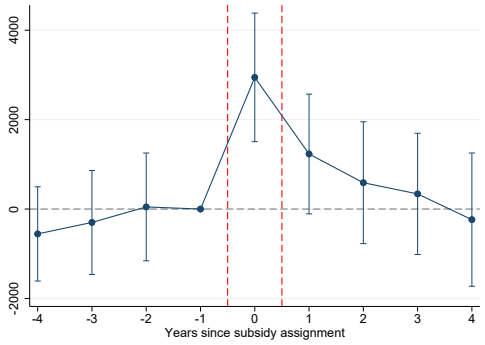
(a) P(Internship Study Credit)



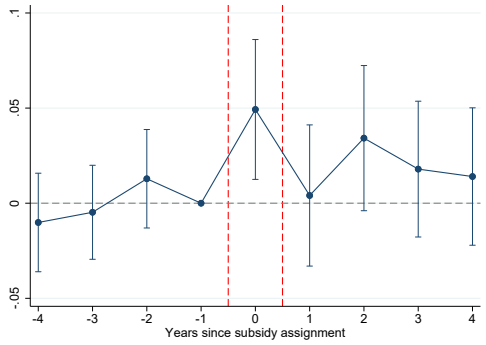
(b) Annual Earnings (euros)



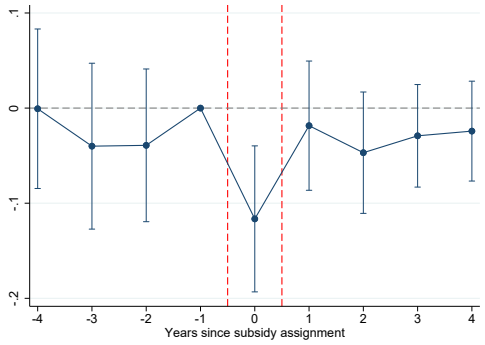
(c) P(Works as Professional)



(d) Earnings in Occupation (euros)



(e) Master's in Occupation (share)



(f) Occupational Mismatch

Figure 5: The Effect of Assignment on Workplace Outcomes

Note: These figures show event-study results comparing the outcomes of students who were initially assigned an internship subsidy vs those who were not. Panel (a) shows the probability of registering an internship credit in the study registry as a function of time from the subsidy assignment. Similarly, panel (b) shows annual earnings, panel (c) shows the probability of working in a professional occupation, panel (d) shows the average earnings among all workers in the same occupational code and panel (e) shows the share college graduates (Master's degree) among all workers in the same occupational code. Panel (f) shows the occupational mismatch showing the gap between earnings in current occupation vs expected earnings with students skill-set.

Social Sorting, Family Formation and Inequality*

Tuomo Virkola†

January 18, 2021

Abstract

This paper studies whether sorting of individuals to segregated environments contributes to assortative mating. Taking advantage of extensive Finnish administrative data, I document that i) there is a strong positive assortative mating pattern by socioeconomic status and skill and ii) partner choices are stronly associated with the social environments individuals are exposed to. Next, leveraging discontinuities in high-school assignments, I show that students assigned to a specific school at age 16 are two times more likely to cohabit with or marry a person from that school at age 26 compared to those who were assigned to a ex-ante similar close-by school. However, even large jumps in high-school peer characteristics do not translate to persistent changes in partner characteristics. These results suggest that although sorting may be important for meeting up potential partners, sorting in itself may not be sufficient for generating significant assortative mating patterns.

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Partner choices play an important role in explaining earnings differences between households. A wealth of evidence in social science documents that individuals marry and partner up with people who are in economically meaningful ways similar to themselves.[1] This assortative mating pattern has become increasingly relevant as it amplifies how recent changes in *individual level* earnings distribution translate to *household level* earnings inequality.[2] One plausible factor that can lead to assortative mating is the sorting of individuals into homogenous environments, such as schools, workplaces, or neighborhoods, where interactions with partner candidates are more likely to take place. However, individuals typically sort into these environments based on their earnings, skills, family background or preferences making it hard to distinguish between different mechanisms that could lead to observed levels of assortative mating or household inequality.

In this paper, I leverage extensive administrative data from Finland and the features of the Finnish high-school assignment system to make progress in understanding the role of social sorting on assortative mating. The administrative data allows me to overcome two key difficulties that typically make it hard to evaluate causal drivers of partner choices. The first difficulty is to be able to accurately measure and identify the social environments that individuals are part of or exposed to. I use high-schools as a clearly defined social environment in which a student is expected interact daily for three years with their close peers. Second difficulty is to isolate differences in individual preferences and the social environment, as preferences are likely to affect both social sorting and mating preferences.[3] Thus, wealthy families are likely to cluster into similar neighbourhoods, professionals are likely to sort into similar workplaces, and students sort into schools and colleges based on their aspirations. To make progress, I use Finnish administrative data to identify when and to to which high-schools individuals were allocated to and match these data to data on the universe of couples. Second, I exploit the fact that high-school assignments are based on a transparent system where students elementary school GPA is used to select students to their schools of preferences. Using the combination of these data and a way to allows to provide new results on the potential factors leading to assortative mating.

[1] See Berscheid et al. (1971), Schwartz and Mare (2005), Li et al. (2013), Schwartz (2013), Fisman et al. (2006), Fisman et al. (2008), Hitsch et al. (2010b), Hitsch et al. (2010a) andBelot and Francesconi (2013) for mating choices.

[2] See Kremer (1997), Fernández and Rogerson (2001), Fernández et al. (2005) for older and Eika et al. (2018), Greenwood et al. (2014), Greenwood et al. (2016a), Greenwood et al. (2016b) Greenwood et al. (2017) for more recent takes on the distributional consequences.

[3] It is also possible that social environment affects mating preferences.

I provide three main empirical results. First, I show that there is a significant degree of assortative mating in Finland that is comparable to other Western countries. I start by documenting that individuals tend to partner up with others who have a similar level of educational outcomes and similar socioeconomic background.

Second, I present evidence that sorting into social environments in adolescence is an important determinant of partner choices moving into adulthood. I document that, on average, around 10 percent of Finnish adult couples are formed between men and women who went to the same high-school and at the same time between age 16 to 18. Next, leveraging an RDD from high-school assignments, I show that a significant share of this observed pattern is causally driven by high-school assignments rather than individual preferences. In particular, I show that being just barely assigned to the same high-school increases the probability of partnering up and living together at age 26 by fifty percent (4.5 p.p. vs 3 p.p.) relative to those who were assigned to a similar nearby school due to marginally missing the selection cut-off.

Third, I leverage heterogeneity in school and peer characteristics to study the importance of social environment on the quality of partners. I show that being assigned to a more selective school increases exposure to higher skilled and higher socioeconomic status peers. However, this exposure does not translate to higher partner skills of higher partner socieconomic status. Instead, I find that while schools are an important meeting place for social interactions and partner formation, other factors are more important when it comes to assortativ mating patterns. These results suggest that social mixing policies may not help in alleviating concerns related to assortative mating and its effects on household level income inequality.

This paper contributes to the literature on the nature of assortative mating and its consequences on family income inequality (Greenwood et al. (2014), Eika et al. (2018), Gihleb and Lang (2016), Schwartz and Mare (2005), Breen and Salazar (2011), Schwartz (2013) Bratsberg et al. (2018)). The closest paper to ours is recent work by Mogstadt et al .. 2018 which uses a similar decomposition exercise to study the contribution of different factors to family income inequality over time. Our study adds to that literature by examining changes in.. etc.

This paper also complements previous literature on determinants of family formation, which has aimed to characterize how much of the observed aggregate sorting patterns could be explained by preferences (Berscheid et al. (1971), Li et al. (2013)Hitsch et al. (2010a)Hitsch et al. (2010b)Eastwick and Finkel (2008) Gautier et al. (2010) Fisman et al.

(2006), Fisman et al. (2008) Belot and Francesconi (2013)). This literature has exploited the richness of the data on preferences and matches that are available from from the early stages of family formation, including online dating and speeddating contexts. I concentrate on the more long run outcomes to complement this literature. I provide evidence which suggest that in addition to preferences, much of the observed sorting pattersn coul be driven by the environment in which individuals spend their time.

This paper also contributes to the literatue on the effects of high-school assignments on students. On the one hand, much of this literature suggests that assignments to even the most prestiguous and sought-after schooling environments do not readily translate to private human capital gains (seee e.g., Abdulkadiroğlu et al. (2014) and the following literature). On the other hand, there are many other reasons to prefer more prestiguous schooling environments, one of which is to prefer interactions with higher quality peers. My analysis suggest that this is also likely to have consequences also for family formation.

The paper that is closest to my analysis is the one by Artmann et al. (2018) who study how university admission loteries in the Netherlands affect partner's earnings and the probability of marrying someone from the same field.[4] My analysis complements their analysis in several important ways that allow to understand the mechanisms of sorting in greater detail. First, I am able to isolate sorting in educational institutions from those due to labor market overlap as I am able to identify the whether individuals went to the same detailed school-program at the same time. I can also provide predictions that apply to large share of the population compared toany specific educational degree such as medical students.

I proceed as follows. In Section 2, I describe the Finnish educational system and its relevant aspects for my analysis. In Section 3, I describe the data and variables used through out the paper. In Section 4, I characterize assortative mating in Finland using measrues based on education, earnings and skill. In section 5, I present the empirical strategy to evaluate the role fo social sorting on partner choices. In section 6, I conclude with a discussion of the results and next steps.

1 Institutional Background

To understand high-school assignments and the data I use in the empirical analysis, it is helpful to describe Finnish educational institutions in some detail. The system con-

[4] Kirkeboen et al. (2016)

sits of three main levels: basic education, secondary education and tertiary education and it is illustrated in Figure **??**. Basic education consists of 9 years of coursework in the elementary school. All students follow the same core curriculum and are typically assigned to schools based on their residential address and school catchment areas.[5] After compulsory education, students may apply to three-year programs in high-schools (formally called secondary schools in Finland). High-schools are divided to vocational and academic tracks, providing different types of skill and training. Academic track schools consist of general education in sciences and humanities. The primary aim is to prepare students to apply to tertiary education. Vocational programs concentrate on specific skills and practical labour market training, and to provide skills to enter labour markets.[6]

High-school assignments are centralized at the national level and based on a tranparent ranking system, which makes it useful for our empirical analysis. First, the applications open during the 9th grade spring spring term (around February-March), students select up to five programs in the order of their preference and submit their preferences to the Joint Application System (Yhteishakujärjestelmä), a nationwide clearing house. Most students apply to programs in their own municipality of residence, but students are free to apply to all programs in Finland. A program is a study track in a given school. Vocational schools often have multiple tracks that provide specialized training in a given speciality, but Academic schools typically only include a single track per school as they are required to offer the same basic curriculum to all students.

Second, in June when students receive their elementary school graduating diplomas, students are preliminarily ranked in each school they applied to and offered a spot in the first school their preliminary ranking allows them to get into. In the majority of academic high-schools, these rankings are based solely on compulsory school final GPA.[7] In vocational schools, schools are free to choose the weights attached to each elementary school subject, but acceptance is still primarily a function of the GPA. After some student have declined their initial offer, there is room for accepting leftover spots in the schools where students applied, but were not initially accepted. This last stage takes about two weeks after which the final allocations are announced.

[5]There are special track schools to which students can apply to, but these are a small minority. Further, in more recent years, there has been more freedom to choose a schhol that is different from the schhol intake area.

[6]To be clear, from both schools students may apply to Bachelor's -level programs in univesities of applied sciences and polytechnics from which they can continue al the way to university level programs.

[7]Some special track schools may also use additional tests and pre-assignements to select their students, but even in these cases, GPA still typically enters the ranking criterion.

2 Data and Descriptive Statistics

This section describes the administrative data sources I use in the empirical analysis. I first describe the population level data and then describe the high-school application data.

Partners Identifying partners in the registry data is central to the empirical analysis. To identify partners, I use extensive data from Finnish population census and registers. In particular, I use data starting from year 1970 to 2015 which includes identifiers for families and their household members. Family identifiers are based on the housing unit which is, in princple, derived from information on the housing address. Partners are identified from living in the same address, being opposite sex and having amaximum of 18 years of age difference. Additional adjustments are made using information child births so that partners who do not live in the same address but have a child, are identified a partners. It also includes married couples living in the same address as well as partners who are simply cohabiting.

The strength of using cohabitation as the starting point in our analysis has the benefit that we can identify couples early on before they get married (which usually happens after some years of cohabitation) and over time as marriage rates have gone down even if probability of partnering up has not. However, a possible concern with cohabitation data is that we may mistakenly classify flatmates as couples in cases where the couple do not have children and are not married or in a registered relationship.

Parental Earnings I use parental taxable income averaged over the 5 years prior to high-school applications as the primary measure of socieconomic status. This is beneficial as it has been measured consistently over time and is based on tax registry data. Thus, I avoid bias and measurement error due to potential misreporting prevalent in survey based datasets. To abstract from changes in the distribution of earnings, I transform parental earnings into earnings percentiles by student cohorts.

High-School Choices My empirical analysis relies on a wealth of information from the Join Application Registry (Yhteishakurekisteri). The data contain information on the universe of secondary school applicants, their ranking of each program, up to 5 programs per student, the initially assigned school slots, and their final school allocation. The data also contains accompanying information that determines students acceptance to each program, including elementary school GPA and prior educational attainment. I restrict the

baseline sample to first-time applicants with no missing information on key variables that I exploit in the empirical analysis. I concentrate on students who were 15-17 year olds to focus on those who truly were first time applicants from those that have applied in previous years which we do not observe in the dataset. I drop students whose GPA is missing as we cannot run the RDD in these cases and it must be the case that selection was not based on GPA.

Descriptive statistics Table 1 characterizies the high-school applicants and selections after the above restrictions. Panel B shows that applicants are on average 16.11 years old, 46 percent are women and on average are placed in the 58th percentile of the parental income distribution. Women tend to have higher pre-highschool educational attainment as the GPA is around 0.5 standard deviations higher than that for men.

Panel B shows that the majority of applicants are offered a spot to their preferred study program. On average, 59 percent get accepted to their first choice. On the other hand, between 15 to 10 percent of applicants are offered a slot also in the other programs. Panel C shows that almost half of applicants apply to an academic track program while the other half applies to vocational track programs. Peer characteristics reflect population level averages, but it is notable that men apply to programs with more men, lower GPA and lower socieconomic status peers than women.

3 Descriptive Analysis of Assortative Mating

To meausure assortative mating so that it is are not sensitive to changes in the income distributions between men and women nor endogenous labour market participation choices, I concentrate on parental income rank correlations and standardized test scores. In particular, I estimate equations of the form

$$P_{i,t}^{W} = \alpha_t + \beta_t P_{i,t}^{M} + \varepsilon_{i,t} \tag{1}$$

where $P_{i,t}^{M}$ is the man's characteristic and $P_{i,t}^{W}$ is the woman's characteristic score. In these specifications, β_t measures the degree of relative assortative mating at time t. With these estimates, the expected difference between the highest ranked and the lowest ranked woman's partner's rank is 100 x β_t (and analoguously for the men). Thus, for an estimate of $\beta = 0.2$ we expect that that the partner of the top woman is ranked 20 percentiles

higher than the bottom woman's.

Figures 2a and 2b and Table 2 show strong assortative mating patterns based on two alternative measures in binned scatter plots. First, panel (a) shows that the higher the (standardized) GPA, the higher is partner's (standardized) GPA. Overall, a one standard deviation increase in GPA predicts a 0.39 increase in partner's GPA (see Table 2, column 1). On the other hand, there is a similar strong assortative mating pattern based on parental income ranks (Panel b). A 10 percentage point increase in parental income rank predicts a 2 percentage point higher partner's rank (see Table 2, column 1). To put this figure in percpective, it is comparable in magnitude to the intergenerational earnings correlation measured using Finnish administrative data.

Table 2 columns (2) and (4) suggest that partner choices and the school environment are strongly associated. In particular, controlling for high-school peer characteristics accounts for a significant part of the partner correlations. A naive interpretation would suggest that up to 50 percent of the partner correlation could be explained by high-school peers. However, this estimate does not have any causal interpretation, as individuals can select into schools based on their preferences.

4 Empirical Strategy

To understand the mechanisms that drive assortative mating, I take advantage of the local random assignement of students to high-schools. The setting exploits the fact that admissions to Finnish high-schools are a function of elementary school GPA which, with limited student intake and oversubscription, generates unpredictable cutoff points in student admissions. Next, I will lay out the main equation of interest to make clear what we aim to identify.

4.1 School Admissions and Partner Choice

The first question we would like to answer in this section is whether being assigned to a particular social environment (high-school) affects who an indivial partners up with later in adulthood. Thus, the first equation of interest is

$$\text{Partner}_{j(i),t} = \beta \text{Admission}_{j,i,t} + f(X_i) + \varepsilon_i \tag{2}$$

where $\text{Admission}_{j,i,t}$ is an indicator for individual i being admitted to school-program j in year t and $\text{Partner}_{j(i),t}$ is an indicator for individual i having a partner who also went to school j. However, clearly β will be biased estimate of the effect as long as selection to schools is not randomly assigned. Students who have a preference for particular school are more likely to come from the same neighborhood or have similar pre-exsting preferences that may or may not be correlated with partner choices. Further, these preferences are likely to be related to individual skill and predicted earnings: skilled students are likely to apply to similar programs and less skilled students are more likely to apply to similar programs. Thus, naive correlations with participation or admission to the same program can give spurious results and maynot help making progres in understanding the mecanisms of assortative mating.

To avoid selection issues, I exploit discontinuities in acceptance to individual school programs. In particular, the first stage takes the form

$$\text{Admission}_{i,j,t} = g(X_i) + \pi_1 \mathbf{1}(X_i \geq X_{j,0}) + \epsilon_i \tag{3}$$

where $X_{j,0}$ is the lowest GPA that guarantees acceptance to program j, X_i is student i elementray school GPA and $g(X_i)$ is a flexible function of GPA. Now, since the exact cutoff which guarantees acceptance X_0 is unknown ex-ante (more evidence to support this is shown below), there is no reason to expect that preferences for school programs and, more importantly, for partners changes at the cutoff. Thus, close to the cutoff we have random variation in admissions, and as a result, random variation in the social environment.

Next, I can study the effects of assignment discontinuities in a reduced form framework which are helpful especially for demonstratin some of the key in a graphical setting,

$$\text{Partner}_{j(i),t} = h(X_i) + \pi_1 \mathbf{1}(X_i \geq X_{j,0}) + \mu_i \tag{4}$$

where $\text{Partner}_{j(i),t}$ is an indicator for individual i having a partner who went to school j.

Now, the RDD outlined above allows us to ask questions of how the assigned school environment afects later life partner choices. This is helpful in order to make progress in understanding observed sorting patters. On the one hand, high-schools are an interesting case, because at ages 16 to 18 individuals are going through formative times when it comes to romantic relationships. It is also an environement, where individuals interact with other students for an extended period of time on a daily basis. It is thus not unlikely

that many or even most students get to know each other in the school that they enroll in. This is in contrast to larger insitutions such as universities where there are potentially thousands of other students most of whom students never meet directly via the school environment. This is helpful as it helps us to pinpoint where the interactions are likely to take place.

4.2 Peer Composition and Partner Choices

The second question we would like to answer is how exposure to different types of peers may translate to partner choices and partner characteristics. For example, does exposure to high performing or high socio-economic status peers also affect the characteristics of your partner? In particular, we would like to know how much of the observed assortative mating pattern observed in Figures 2a and 2b could be explained by the shared social environment. Ideally, we would like to estimate the following type of a relationship

$$\text{Partner Skill}_{j(i),t} = \beta \text{Peer Skill}_{j(i),t} + \varepsilon_i \tag{5}$$

Now, similar to the case just mentioned above, peer skill is again endogenous. To make progress, we need to control for the fact that individuals can select into peer groups.

In the empirical analysis, we will take advantage of the fact that students place higher GPA schools higher in their ranking (there is always a chance to withdraw application and get to the lower GPA school). In particular, we will exploit the fact that being admitted to your first choice school is a significant predictor of the peer characteristics.

5 The Effect of Social Sorting on Partner Choices

This section discusses the main empirical results on how being assigned to a preferred high-school affects partner choices at age 26. I first show graphical evidence on the reduced form relationship and then proceed to main IV results. These results are based on those applicants who were on the margin of being admitted to an academic high-school.

Figure 3 (a) shows the clear discontinuity in the probability of enrolling in your preferred school once a student passes the minimum acceptance threshold. Enrolment jumps by almost 50 percent exactly at the cut-off. Figure 3 (b) on the other hand shows the probability of partnering up with someone from your preferred school as a function of you GPA. Probability of partnering up with someone from you preferred school increases

from 3 to 4.5 percent at the admittance threshold. Thus, on average being admitted to the same school increases the probability of partnering up with someone from that school by 100 percent. This finding is strongly consistent with the idea that high-schools are an empirically important meeting place for potential future partners.

Figure 4 shows the results on how being assigned to a preferred school affects the peers individuals are exposed in high-schools and the partner characteristics. Both Panels (a) and (b) show that being assigned to your preferred school increases exposure to high-performing and high-socio-economic status peers. However, Panels (c) and (d) shows that this exposure does not translate to partner characteristics. This is despite the finding that partnering up is significantly driven by the school environment.

Table 3 shows the main IV results based on the RDD design. I concentrate on a simple specification with a small symmetrical bandwith of 0.25 around the admission cut-offs across all specifications to make them comparable. First, Panel A shows that an admission to a preferred track increases the likelihood of partnering up with someone from the same high-school by 2.9 p.p. which is a 82 percent increase over the baseline. The point estimate is similar for both men and women, but for men the baseline probability of partnering up with a school-mate is almost 50 percent higher.

Next, Panel B shows that there is a 0.9 standard deviation increase is peer GPA when an applicant is admitted to a preferred program. This effect is similar in magnitude for both men and women. However, Panel C shows that there is no significant increase in partner's GPA. The point estimate is 0.012 and statistically insignificant. This is very much in contrast to the OLS results described above. The partial OLS coefficient in Table 2 would suggest that a similar increase in peer characteristics would predict a 0.151 x 0.907 = 0.137 increase in partner GPA holding individual GPA constant. These IV results thus allow us to rule out an effect that is an order of magnitude smaller.

Panel D and E shows analogous effects based on parental income rank. First, admission to a preferred program has a significantly positive effect on peer socio-economic characteristics. Admitted students are exposed to peers whose parents are almost 5 percentiles higher in the income rank. However, the effect on partner's socioeconomic status is not significant and the point estimate is even negative. Again, we can rule out an effect that is significantly smaller than predicted by the partial OLS coefficient from Table 2 which would predict an increase of 2.37 on the partner's income percentile.

Taken together, these IV results support the results from the reduced form relationships shown graphically.

6 Conclusion

This paper studies how the social environment individuals are exposed to affects family formation moving into adulthood. Using extensive administrative data sources and taking advantage of the Finnish high/school assignments, I show that high-schools are a meeting place for potential partners and that these meeting places are to an important degree segregated by skill and socio economic status. Yet, these results suggest that segregation in the educational environment may not be an important driver of assortative mating.

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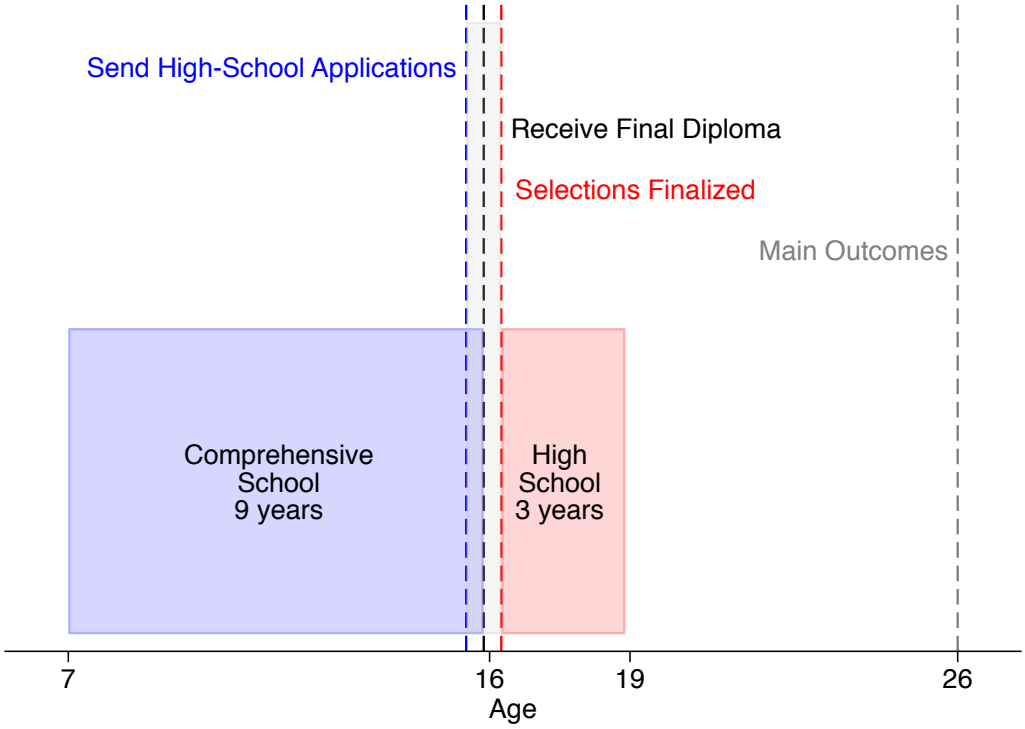

Figure 1: Timeline of Applications

Note: This figure illustrates the timeline of high-school assignments and measurement of main outcome variables. Prospective students send high-school applications during the final comprehensive school term in Febrary-March. Students receive the Final diploma at the end of comprehensive school by end of May or early June. Preliminary student selections are made in June and are based primarily on GPA. Between June and of August, students may accept or reject school offers before the selections are finalized in end of August. Partner choices are measured by age 26 whcih is the final year when the last 2007 high-school cohort is observed in our administrative data.

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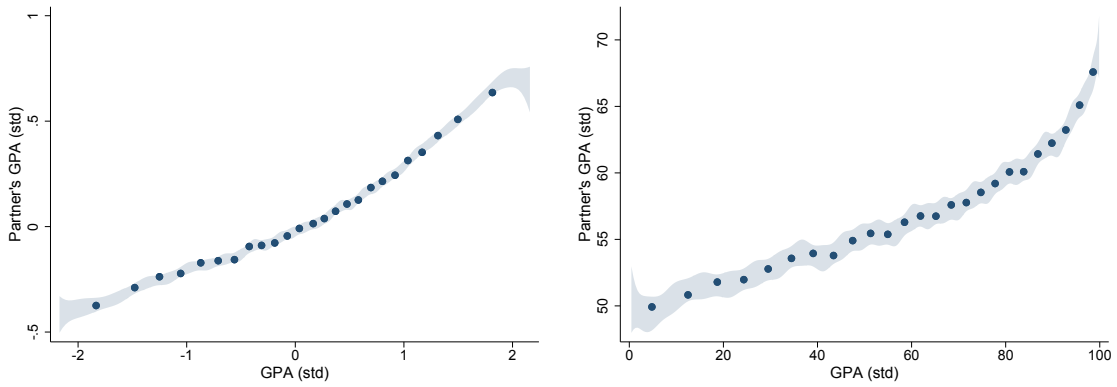
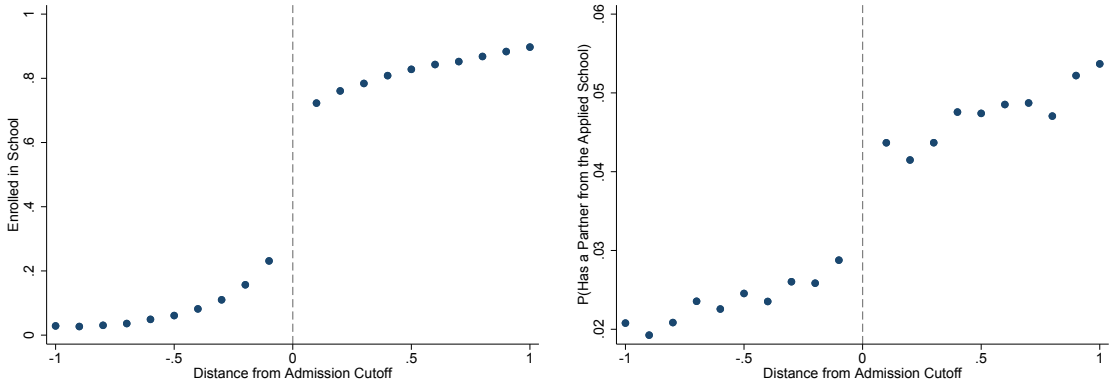


(a) GPA (b) Parent Income Rank

Figure 2: Assortative Mating

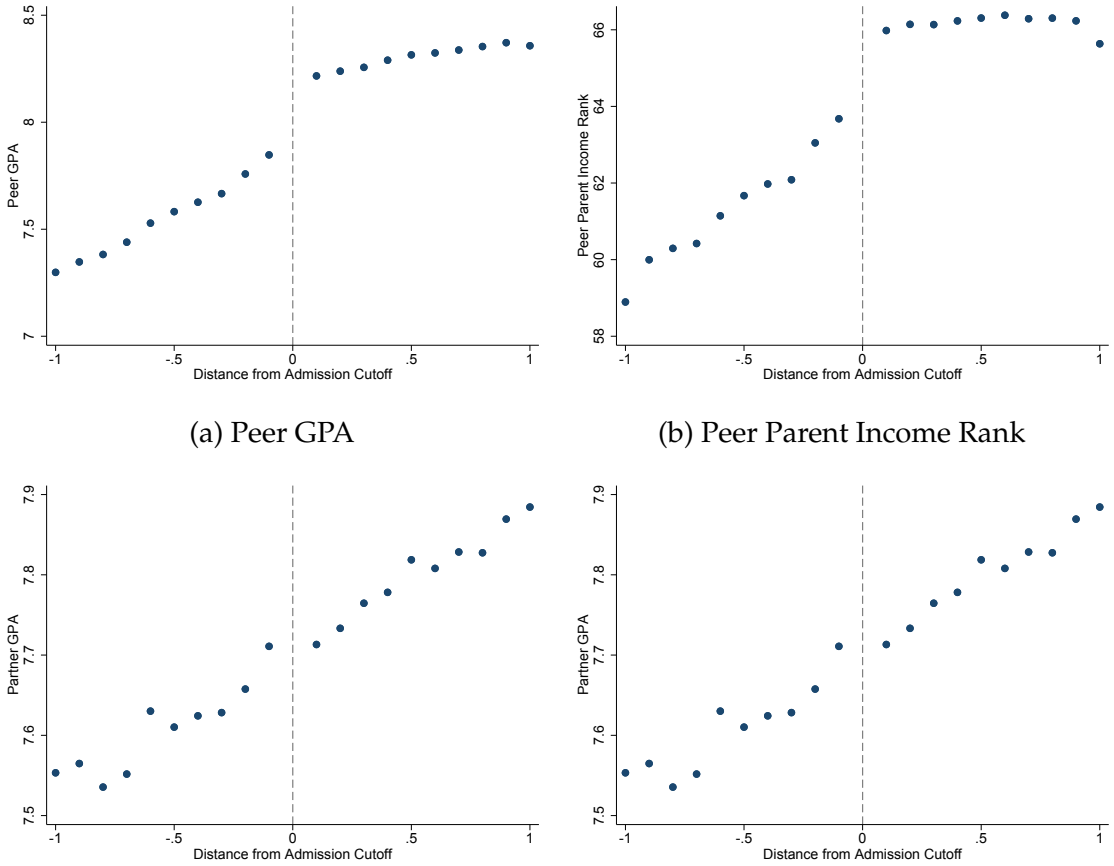
Note: These figures show binned scatterplots of partner's characteristics. Panel (a) shows the association between individuals own standardized GPA and her partner's standardized GPA at age 26. Panel (b) shows the association between individuals own parental income rank (measured at age 10-15) and her partner's parental income rank. Bands show 95% confidence intervals.



(a) Enrolled in school (b) Partner from applied school

Figure 3: The Effect of School Assignment on Partner Choice 10 Years Later

Note: These figures show the baseline reduced form effects from the RD design outlined in Section 5. Panel (a) shows the probability of enrolling into first-choice school as a function of distance to the acceptance treshold. Panel (b) shows the probability that applicant was cohabiting with a person who was accepted to the first-choice high-school as a function of distance to the acceptance treshold to the first-choice school. All graphs show averages in equal sized bins of 0.05 of the GPA.



(a) Peer GPA

(b) Peer Parent Income Rank

(c) Partner GPA

(d) Partner Income Rank

Figure 4: The Effect of School Assignment on Peer and Partner Characteristics

Note: These figures show the baseline reduced form effects from the RD design outlined in Section 5. Panel (a) shows peer compulsory school GPA as a function of distance to the acceptance treshold to preferred school. Similarly, panel (b) shows peer parent income rank. Panels (c) and (d) show partner GPA and parent income rank for partners 10 years after school assignment (age 26). All graphs show averages in equal sized bins of 0.05 of the GPA.

Table 1: Descriptive Statistics on High-School Applicants

	(1) All	(2) Men	(3) Women
Panel A: Pre-Assignment Characteristics			
Age	16.11	16.13	16.09
	(0.67)	(0.66)	(0.67)
Woman	0.46	0.00	1.00
	(0.50)	(0.00)	(0.00)
GPA	7.39	7.13	7.70
	(1.15)	(1.12)	(1.11)
GPA (standardized)	-0.00	-0.25	0.29
	(1.00)	(0.98)	(0.94)
Parent Income Rank	58.25	58.49	57.98
	(27.38)	(27.37)	(27.38)
Panel B: Offers			
Accepted to 1st Choice	0.59	0.57	0.62
	(0.49)	(0.50	(0.48)
Accepted to 2nd Choice	0.14	0.14	0.13
	(0.35)	(0.35	(0.34)
Accepted to 3rd Choice	0.11	0.12	0.10
	(0.31)	(0.32	(0.29)
Accepted to 4th Choice	0.10	0.11	0.10
	(0.30)	(0.31	(0.29)
Accepted to 5th Choice	0.09	0.10	0.09
	(0.29)	(0.29	(0.29)
Panel C: Program Characteristics			
Academic Track	0.48	0.39	0.57
	(0.50)	(0.49	(0.49)
Peer GPA	7.60	7.39	7.83
	(0.86)	(0.90	(0.76)
Peer GPA (standardized)	-0.02	-0.26	0.24
	(0.99)	(1.03	(0.87)
Women Peers	0.49	0.35	0.65
	(0.28)	(0.28	(0.17)
Peer Parent Income Rank	58.54	57.46	59.72
	(12.61)	(12.61	(12.50)
N	1482039	770,089	711,950

Note: Robust standard errors in parentheses: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2: Partner Associations and Peer Characteristics

	Partner's GPA		Partner's Parent Income Rank	
	(1)	(2)	(3)	(4)
Panel A: Partner Correlations				
GPA	0.385***	0.274***		
	[0.002]	[0.002]		
Peer GPA		0.151***		
		[0.002]		
Parent Income Rank			0.198***	0.094***
			[0.002]	[0.003]
Peer Parent Income Rank				0.509***
				[0.005]
N	372388	372388	186268	186268
Cohort FE	Yes	Yes	Yes	Yes
Gender FE	Yes	Yes	Yes	Yes

Note: Robust standard errors in parentheses: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3: The Effects of Academic School Assignment on Peers and Partner Characteristics

	All (1)	Men (2)	Women (3)
Panel A: P(Has a Partner from the Applied School)			
Admitted to Program	0.029***	0.030***	0.028***
	[0.003]	[0.004]	[0.004]
Mean	.035	.043	.027
N	74350	38711	35639
Bandwith	0.25	0.25	0.25
p	0	0	0
Panel B: Peer GPA (standardized)			
Admitted to Program	0.907***	0.936***	0.875***
	[0.011]	[0.016]	[0.016]
Mean	.535	.516	.555
N	63088	33027	30061
Bandwith	0.25	0.25	0.25
p	0	0	0
Panel C: Partner GPA (standardized)			
Admitted to Program	0.012	0.012	0.042
	[0.026]	[0.032]	[0.037]
Mean	.079	.437	-.264
N	26133	12785	13348
Bandwith	0.25	0.25	0.25
p	0	0	0
Panel D: Peer Parent Income Rank			
Admitted to Program	4.733***	3.821***	5.759***
	[0.223]	[0.304]	[0.326]
Mean	65.013	65.354	64.635
N	46723	24545	22178
Bandwith	0.25	0.25	0.25
p	0	0	0
Panel E: Partner Parent Income Rank			
Admitted to Program	-1.120	-1.849	-0.536
	[0.786]	[1.141]	[1.081]
Mean	60.597	61.845	59.487
N	21950	10329	11621
Bandwith	0.25	0.25	0.25
p	0	0	0

Note: Robust standard errors in parentheses: * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

The Violent Origins of Finnish Equality*

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January 18, 2021

Abstract

Why is Finland today an equal country? We employ newly collected historical data to document that Finland was extremely unequal in terms of income and land distribution until a violent uprising in 1918 which was a major turning point in Finnish inequality. We show that high inequality partly originated from the famine of 1866-1868 which increased the concentration of land and power to large landowners. Regions with more exposure to the famine also had worse labor market outcomes and more coercion by the early 1900s. Using unique micro-data on casualties of the Finnish Civil War, we demonstrate that the famine contributed to insurgency participation through these factors. Although unsuccessful in replacing the government, the insurgency led to significant policy changes, including radical land redistribution and a full extension of franchise. A more drastic shift towards equality occurred in locations that were more affected by the famine and that had higher levels of pre-conflict inequality and more insurgents. These results indicate that equality has not been a persistent feature of the Finnish society inherent in its culture or values but is instead an outcome of institutional changes.

Keywords: Civil war, democratization, famine, inequality, redistribution, threat of revolution
JEL: D63, D74, H11, J47

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1 Introduction

The Nordic countries are known for their redistributive policies and low levels of inequality. An important but poorly answered question is whether equality is an innate part of these societies, or if it is an outcome of institutional changes made in the more recent past.[1]

In this paper, we present historical evidence that aligns with the latter explanation. Using newly collected municipality-level wealth and income statistics from Finland, we document that Finnish inequality was considerable until a civil war that the country experienced in 1918. The primary purpose of our paper is to use these data, combined with micro-data on insurgency participation, to show that, within Finland, (i) high inequality and associated grievances directly led to insurgent activity, and (ii) municipalities with more insurgents had a more sizable shift toward equality after the conflict than other municipalities. These two main results are visible in Figure 1 where Panel A illustrates the relationship between inequality in 1904 and insurgency in 1918, and Panel B plots the connection between the insurgency and post-war decline in inequality.

To provide exogenous variation and explain high inequality in land ownership and income, we start our empirical analysis from another historical turning point: the famine of 1866-1868. According to historical accounts from the era, land ownership became persistently more concentrated in areas more affected by the famine relative to other regions due to speculation and foreclosures of farms and land of heavily indebted farmers (Gylling 1917). We use crop failures in 1867 as an exogenous driver for the famine and find a strong and robust relationship between later high inequality and measures of famine exposure.

In addition to the concentration of land ownership, the famine led to more labor market coercion. Instead of rising workers' wages, a likely result of labor scarcity in a free labor market, the famine increased coercive tenant farming contracts, where the landowner can decide how much work the tenant has to perform to rent land.[2] As the prices of agricultural products increased after the famine and there was upward pressure on wages due to labor scarcity, it was logical for the land owners to have more coercive tenant-farming contracts (Domar 1970; Acemoglu and Wolitzky 2011).[3] Consistent with these mechanisms, we show that locations

[1] Some scholars of comparative economic development have suggested that the roots of modern-day outcomes are in factors such as culture and social norms (Guiso et al. 2006; Luttmer and Singhal 2011; Berger and Engzell 2019), or population heterogeneity (Alesina and Ferrara 2005; Ashraf and Galor 2013). Others have highlighted the role of institutions in shaping countries' long-run development (North 1990; Hall and Jones 1999; Porta et al. 1999; Robinson et al. 2005).

[2] Tenant farmers usually paid their rent in workdays instead of money, and the number of days was generally at the discretion of the land owner (Peltonen 1990; Peltonen 1992). As the land owners could ask the tenant to perform tasks at will, there was less need for wage labor, putting downward pressure on wages. Peltonen (1990) argues that the tenant farming institution hindered the emergence of the labor market.

[3] The large land owners were left with considerable market power, as rural workers had few outside options due to mobility restrictions and lack of industrialization. Land was rarely available for purchase as the large landowners

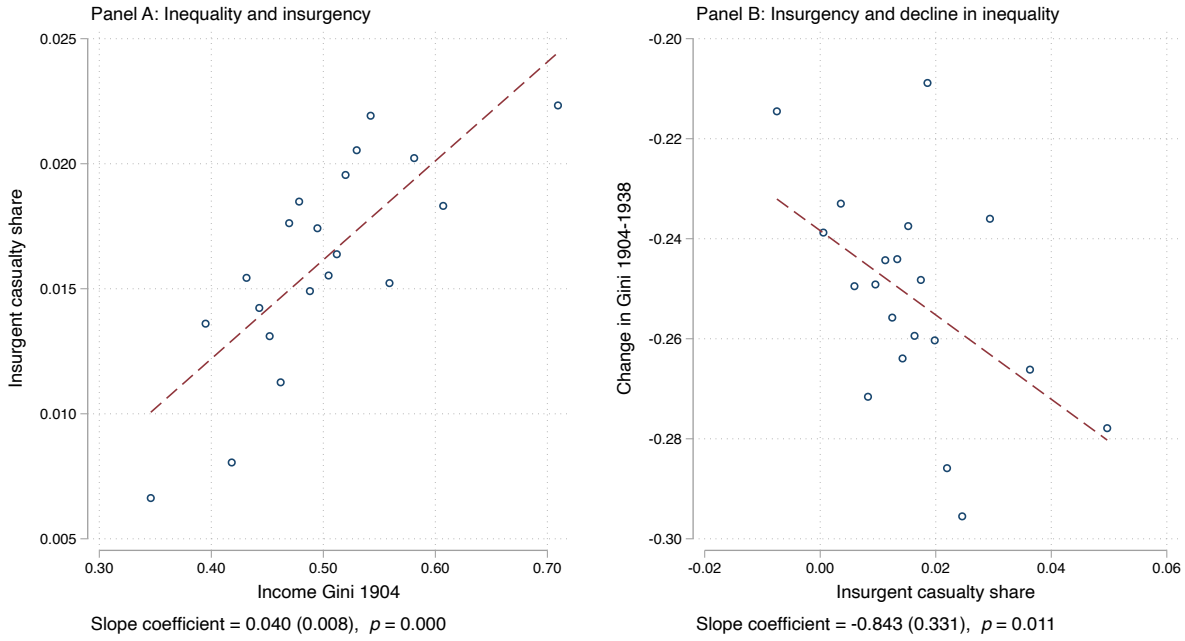

Figure 1. Relationship between economic inequality, insurgency and redistribution.

Notes: The figures plot the relationship between economic inequality in 1904 and insurgency (Panel A) and insurgency and decline in inequality (Panel B). Robust standard errors of the slope coefficients are reported in parentheses. Dots are binned averages computed within twenty bins with an equal number of observations. We net out baseline controls (latitude, longitude, log suitability for rye, log suitability for barley, log elevation, and log population in 1865) and region fixed effects. For further information on our data, see Section 3.

where a larger share of the population perished during the famine years, or that experienced an exogenous crop failure shock in 1867, had more tenant farmers and lower agricultural wages. This further bolstered the increasing inequalities of the era.

We propose that economic inequality and associated grievances catalyzed the Finnish Civil War of 1918. This reasoning mainly follows the so-called *discontent theories* of civil conflict (Moore 1966; Huntington 1968; Gurr 1970; Paige 1975; Muller and Seligson 1987). In the 1918 Finnish Civil War, which started as an offshoot of the Russian revolution, socialist insurgents unsuccessfully fought conservative Senate-led forces over the control of the newly-independent nation. The conflict lasted four months and resulted in about 39,000 casualties, three-fourths of whom were insurgents. To match the insurgents to locations and to understand who joined the conflict and the insurgency, we use a unique individual-level dataset that contains information on all civil-war casualties, including their place of birth. Our data indicate that municipalities with a

preferred tenant farming contracts (Peltonen 1990; Peltonen 1992). Free movement of labor was banned until 1898 (Happonen 2004).

higher inequality in the early 1900s (and those that had faced a greater famine shock fifty years prior) experienced more insurgent deaths in 1918.[4]

After the conflict, inequality fell rapidly. We show that a key mechanism behind the sudden decrease in inequality was the land reform enacted shortly after the conflict. We further argue that extending the franchise played a critical role in keeping the peace. Until the late 1910s, suffrage in Finnish local elections was highly restricted, and instead of following a "one man, one vote" rule, the number of votes of each voter depended on the amount of taxes paid. This system accumulated political power to the economic elites and completely excluded many citizens from the electorate, limiting their chances of influencing, for instance, poor relief and health care.[5] The country introduced equal voting rights after the civil war with the first fully democratic municipal election held merely a year after the conflict. We find that municipal spending policies reacted to the larger and poorer electorate accordingly.

Taken together, these results clearly show that equality has not been a stable part of Finnish history, that is, something inherent in its "Nordic values". Instead, Finland has experienced both a radical rise and a drastic fall of inequality. We emphasize and provide evidence that the famine of 1866-1868, the Civil War of 1918, and the following democratization and redistribution contributed to these developments. However, we do not claim that these were the only factors contributing to inequality—just that they had an impact. Altogether, the Finnish experience shows similarities to multiple other settings. While taxation and redistribution are the most common tools in the battle against inequality in present-day societies, many scholars argue that violent revolutions have played an important role in shaping the distribution of prosperity and power historically (Acemoglu and Robinson 2000; Wood 2003; Moore 2016; Scheidel 2018).[6]

With our novel data set and a within-country setting, we bring in new results and contribute to several large debates in social sciences on the causes and consequences of inequality. First, we provide novel evidence on the societal impacts of famines by investigating how labor shortages

[4]We further document evidence of non-violent expressions of discontent in the more affected municipalities before the civil war. They voted more for the Social Democratic Party and had more workers' association members.

[5]See also Bengtsson (2019) who describes economic inequality and political exclusion in Sweden around the same time. The Swedish circumstances were very similar to those in Finland. Having weighted voting rights not unique to the Nordic countries. For instance, a similar rule was used in Prussia (Becker and Hornung 2020). Furthermore, franchise restrictions that were based on income were commonplace in many other parts of the world, such as the United States (Keyssar 2009).

[6]In particular, Finland is not a unique case in that the height of its inequality coincides with social unrest and the extension of the franchise. Our findings are in fact in tally with the seminal work by Acemoglu and Robinson (2000) which establishes a potential link between democratization and the Kuznets curve:

> *Rising inequality often associated with industrialization increases social unrest and induces democratization. Democratization in turn opens the way for redistribution and mass education, and reduces inequality. Interestingly, in line with this approach, in a number of Western economies, the peak of the Kuznets curve coincides with the extension of the franchise.*

affect economic activity and living standards under coercive labor market institutions. Earlier work has explored the origins of labor market coercion. For instance, Naidu and Yuchtman (2013), Bobonis and Morrow (2014), and Saleh (2020) find that export booms may tighten labor market coercion. We complement this research by illustrating that a tightening labor coercion may also stem from negative population shocks. These results are in line with a famous remark of Brenner (1976), according to whom labor markets became more coercive in Eastern Europe after the Black Death wiped out a significant share of the population. Another related strand of literature focuses on the consequences of labor coercion, in particular, how it adversely affects various long-run outcomes of countries (Dell 2010; Dell and Olken 2020; Nunn and Wantchekon 2011).

The aforementioned research also touches on our second contribution: we provide new evidence on how inequality and labor coercion can trigger a civil conflict. The existing empirical research on inequality and civil conflict offers mixed results, and much of the evidence comes from analyses of cross-country data (Muller and Seligson 1987; Collier and Hoeffler 1998, 2004; Nafziger and Auvinen 2002; Fearon and Laitin 2003). Some closely related studies focus on the relationship between land inequality and civil unrest. For example, Albertus et al. (2018) use municipality-level data from Brazil's extensive land reform program to study the conditional relationship between landholding inequality and unrest.[7] Being able to link greater economic inequality with agricultural labor coercion (stemming from the famine) and strengthened political exclusion, we can explore some potential mechanisms that could explain the connection between inequality and conflict. Moreover, our data on the civil war casualties allow us to advance the literature by going beyond the mere occurrence of conflict since we can measure participation and the economic status of the participants. By focusing on institutional factors that contribute to the emergence of a civil conflict, we complement work by authors such as Miguel et al. (2004) and Dube and Vargas (2013) that focuses on the relationship between short-term shocks and conflict.

Third, our findings speak to theories of inequality and democratization. Prominent redistributive theories of democratization proposed by Boix (2003) and Acemoglu and Robinson (2000, 2006b) suggest a foundational link between democratization and inequality. In these theories, the distribution of wealth shapes elites' incentives to prevent or enable a more even distribution of political power.[8] Whereas Boix (2003) proposes a linear relationship between inequality and democratization, Acemoglu and Robinson (2000, 2006b) theorize that

[7]Similarly, Hidalgo et al. (2010) study Brazilian municipalities. Their focus is on the economic predictors of land invasions. They show that in highly unequal municipalities, negative income shocks cause twice as many land invasions as in municipalities with average land inequality.

[8]The ideas regarding the connection between inequality and democracy are not new. They can be traced back to great thinkers such as Aristotle, who wrote in *The Politics* that "where one set of people possesses a great deal and the other nothing, the result is either extreme democracy or unmixed oligarchy, or tyranny due to the excesses of either"—or Alexis de Tocqueville, who praised an "equality of conditions" as the most important precondition for democracy in *Democracy in America* (1835).

democratization often happens when an economy is at the peak of the Kuznets curve. Our findings go hand-in-hand with the conjecture of Acemoglu and Robinson. Their *threat of revolution hypothesis* suggests that extending the franchise can act as a commitment to future redistribution, which again prevents future social unrest.

Previous studies—namely Przeworski (2009), Aidt and Jensen (2014), and Aidt and Franck (2015)—have shown that the threat of revolution can catalyze the democratization process. Our fourth contribution is to take this analysis a step further by showing that democratization induced by a threat of revolution can also affect redistribution and inequality. Many historical studies document a positive relationship between democracy and redistribution or the size of the public sector (Husted and Kenny 1997; Lott and Kenny 1999; Aidt et al. 2006; Miller 2008). The link between democratization and inequality has especially been studied by political scientists and sociologists.[9] In their survey, Scheve and Stasavage (2017) show that cross-country data do not support the idea of democracy producing wealth equality. Acemoglu et al. (2015) document that, in a panel of countries, democracy is positively associated with tax revenues as a fraction of GDP as well as secondary school enrollment. The authors, however, find much more limited evidence of an effect of democracy on inequality. They conclude that the literature is still inconclusive on the impacts and hypothesize that the effects may depend on initial conditions. We offer an empirical characterization of one case where economic equality and increased redistribution followed the adoption of democratic institutions in the aftermath of a civil conflict.

In the following section, we discuss the historical background of our case through a conceptual framework that links the events to trends in inequality. We also lay out empirically testable hypotheses based on the existing literature. We then present our newly collected data and our findings from the empirical investigation. Finally, we discuss some additional remarks, such as alternative mechanisms that could drive the development of Finnish inequality, before concluding.

2 Historical Background and Conceptual Framework

This section gives a brief overview of the historical background of our study. More precisely, we discuss the famine of 1866-1868, the Civil War of 1918 and its aftermath, and how these events impacted Finnish inequality. We connect these events to the related theoretical and empirical literature and lay out empirical predictions that we can test with our data.

[9]Much of the existing research uses cross-country data, and especially the older research suffers from econometric problems as pointed out by Acemoglu et al. (2015). For example, there is the possibility that omitted factors are affecting both inequality and democracy, and that reverse causation from inequality to democracy may be present. Our within-country setting is perhaps cleaner and less vulnerable to such concerns.

2.1 Famine and Inequality

The famine of 1866-1868 originated from catastrophic weather conditions which caused severe crop failures across the country. Due to excessive rain during the summer of 1866, root vegetables rotted in the fields, and sowing rye became extremely difficult in the autumn.[10] The rainy summer and autumn were followed by a hard and long winter which severely affected the 1867 harvest in large parts of the country.[11] Overall, the harvest of rye was cut in half relative to previous years' normal harvest (Panel A in Figure 2). While the weather and harvests returned back to normal by 1868, contagious diseases spread and killed thousands more (Turpeinen 1986).

The famine resulted in about 270,000 deaths. This was about 150,000 in excess relative to the normal mortality, that is, almost 8% of the entire Finnish population (Panel C in Figure 2). However, there was significant variation in the severity of famine deaths and crop failures across the country that we will exploit in our empirical analyses. As shown in Panels C and D of Figure 2, areas in central and southern Finland were hit harder by the famine. In some heavily affected municipalities, as much as one-third of the population died during the famine years. It is also clear that excess deaths occur often in the same municipalities that experienced crop failures. Still, some municipalities experienced large population shocks despite not having a crop failure, at least partly due to contagious diseases that spread during the famine.

The famine induced a considerable decrease in labor supply. Not only did it impact the current labor force: typical to famines, children were a particularly vulnerable group. This meant that the availability of labor force was affected for an extended period of time. Now, for our analysis, it is important to understand what happened in the labor markets, given the negative labor supply shock? Standard economic theory would tend to suggest that if labor market institutions are inclusive, negative supply shocks would increase workers' bargaining power. As a consequence, wages should increase and the income distribution become more equal., which This is, for example, what seems to have happened in Western Europe after the Black Death in the mid-1300s. The negative labor shock improved the living standards of workers.[12] However, this standard economic logic is not likely to apply in the presence of coercive labor markets which we consider more descriptive of the Finnish labor markets in the era. In coercive labor markets, the

[10] Rye was the by far most important source of calories in Finland in the 1800s. However, also other, less important types of crops such as barley were similarly affected by the poor weather conditions. We illustrate this in Appendix Figure A1. Appendix Figure A2 shows that the output of other agricultural products was less affected but did not increase either during the famine years.

[11] Turpeinen (1986) describes the weather conditions during the famine years in detail. Karadja and Prawitz (2019) study the consequences of the same weather shock in Sweden. They show that emigration from Sweden to the United States was greater in locations that were hit harder by the poor weather. A fundamental difference between Finland and Sweden is that the significantly richer Sweden did not experience a famine.

[12] As discussed in Scheidel (2018), a key difference between famines and pandemics is that famines tend to have a lower mortality.

effects of a population decline depend on the market price of the output and workers' outside options (Domar 1970; Acemoglu and Wolitzky 2011). For example, in Acemoglu and Wolitzky's model of labor market coercion, labor scarcity can increase labor coercion if the market price of the output increases but workers' outside options do not. A prominent historical example consitent with this view, as noted by Brenner (1976), is that labor coercion actually increased in many parts of Eastern Europe after the population shock caused by the Black Death, leading to the so-called Second Serfdom.

The Finnish labor market institutions at the time were coercive. A particular feature was the prevalence of tenant farming, which was an essential part of rural labor markets in the late 19th and early 20th century and which was among the most contentious policy issues in the public debate. The number of tenant farms increased through the late 1800s and early 1900s, tenant farms constituting nearly half of all farms in 1912.[13] In many cases demand for farmable land was high but no land was made available for sale, making tenant farming the only option (Peltonen 1990; Peltonen 1992). While tenant farming was not serfdom, it was a coercive labor relationship as the large land owners had significant power over their tenants.[14] The tenant farmers paid their rent mainly in day labor (*taksvärkki*), and paying a monetary rent was rare.[15] Tenant farmers seldom had written contracts, and the amount of day labor and contract termination was entirely at the discretion of land owners. Particularly taxing was the land owners' right to require extra day labor (*ylipäivät*) and employing the tenant farmer's family. Tenant farming also hindered the emergence of the rural wage labor market as the land owners could use the tenant farmers as labor (Peltonen 1990). Such monopsony power was arguably higher in locations with more concentrated ownership of land.

The underlying conditions further bolstered tightening coercion in the Finnish labor markets. The prices of agricultural products increased due to famine, and workers lacked outside options. This latter point was because there were merely a few cities and little employment opportunities in industrial production. Even when the country started industrializing eventually, pursuing outside opportunities was difficult due to negative attitudes towards migration, low social mobility, and various geographical mobility restrictions, such as passport requirements (Lento 1951). According to the theory of Acemoglu and Wolitzky (2011), these factors would have allowed labor coercion to increase. This would have introduced a downward pressure in wages, increased tenant farming,

[13]There were three broad types of tenants: *lampuoti* who rented an entire farm, *torppari* who rented a part of farm, and *mäkitupalainen* who rented land to live on without any farmable land. The increase in tenant farming was mainly driven by severe restrictions on land division and ownership in the 19th century. A landowner could only pass on their property to a single descendant and while the others were forced to rent part of that land for themselves. As the population grew, this resulted in the increase in tenant farming contracts.

[14]Naidu and Yuchtman (2013) discuss how such "intermediate" forms of labor coercion have been common.

[15]Monetization of the Finnish agriculture was slowly increasing through the 1800s. But still in the year 1912, 72% of the tenant farmers paid their rent in day labor according to the land rent statistics of Statistics Finland.

and potentially increased economic inequality. At the same time, those with enough land to be net producers would have gained from higher food prices (see also Ravallion 1997 and the references therein).

Furthermore, land ownership became increasingly concentrated after the famine. In particular, many households were either forced to sell their farms and other property to feed their family, abandon their homes to find food elsewhere, or their farms had to be auctioned off to pay off debts accumulated during these successive bad harvest years (Häkkinen et al. 1991). This mechanism is well illustrated by Dr. Edvard Gylling who writes in the *Workers' Almanac* of 1918 (Gylling 1917):

> *Wheat was largely bought with debt money. Farms and houses were used as a collateral. When the famine continued, farmers could not pay back their debts. On the contrary, new debt would have been needed. Payments were dunned despite the extreme distress. [...] Hundreds and thousands of houses were foreclosed because of even small debts, unpaid rents, or unpaid taxes. [...] Many farms changed hands, and ownership became more concentrated than before.*

As the banks refused to lend during the extreme situation, the largest land owners and speculators with liquid wealth were able to increase their holdings more in municipalities which were hit harder by the famine.[16] The concentration of land ownership and the prevalence of tenant farming interacted in potentially significant ways, increasing inequality and labor coercion in the early 1900s.[17]

Based on the characteristics of the Finnish labor markets and the theoretical predictions discussed above, we make two empirically testable claims regarding the effects of the famine of 1866-1868 on inequality and labor coercion:

> **Claim 1.** The famine caused a persistent increase in inequality.
>
> **Claim 2.** There was more labor coercion in locations that were hit harder by the famine.

[16] Using data on the universe of Finnish newspapers, we provide descriptive evidence which is consistent with this mechanism. Appendix Figure A3 shows that there were much more newspaper mentions of bankruptcy and auctions after the famine. The share of pages containing hits for bankruptcy and auction increased five and two fold, respectively. We also find evidence suggesting that a gap in advertisements on sales and purchases emerges around the famine years.

[17] An additional reason why the famine may have shaped inequality in the long run is that exposure to famine at young ages shapes adult health and labor market outcomes. These further influence the same outcomes of future offspring. For instance, Meng and Qian (2011) provide empirical evidence of the long-run impacts of China's Great Famine. They argue that famine affects long-run development outcomes through two main channels: by adversely affecting childhood health, or by reducing the quality or quantity of investment into children by deteriorating their parents' health. If the impacts are differential by parental socio-economic background, this could also contribute to increases in inequality.

2.2 Inequality and Civil Conflict

We propose that the famine of 1866-1868 contributed to the emergence of the Finnish Civil War of 1918 by boosting economic inequalities and political exclusion in society. The civil war was a conflict for the control of Finland following its declaration of independence from Russia.[18] In 1917, the February Revolution dethroned Czar Nicholas II, and the political power over the Grand Duchy of Finland was handed over to the weak Finnish Parliament. This transition created a power vacuum both politically and in law enforcement, as the Russian-backed police were dissolved. Simultaneously, both the bourgeois elite and the working class began to organize their own security guards. There were frequent strikes and demonstrations across the country demanding better working conditions, land reform, and an extension of the franchise. While the conservative government had made significant concessions by November, the conflict had already escalated to the point where both sides had started arming their forces, and the revolution attempt was on its way.

The social backgrounds of their members distinguished the two sides of the civil conflict. The Red Guards were led by a section of the Social Democratic Party, and they mainly consisted of industrial and agricultural workers. Soviet Russia provided the Red Guard with arms, and many Russians also fought along with the Reds. The insurgents controlled most of Southern Finland and many of its industrial centers. The conservative Senate led the White Guards, and also supported by the German Imperial Army. The Whites were mostly volunteering members of the middle and upper social classes, but they also organized a draft in their controlled areas during the war. The hostilities of 1918 lasted for about four months, from late January until mid-May. In total, about 39,000 people died, and most of the casualties were insurgents. Many of these casualties were not battle deaths but either executed or died of diseases in prison camps after the war.

Economists and political scientists have established that economic underdevelopment and poverty are important predictors of civil conflict (Collier and Hoeffler 1998, 2004; Fearon and Laitin 2003; Miguel et al. 2004; Blattman and Miguel 2010). In discontent theories of conflict, economic inequality is considered to be among the fundamental economic preconditions of insurgency and revolution (Huntington 1968; Paige 1975; Muller and Seligson 1987; Midlarsky 1988). While the first notion has strong support in the data, the empirical support for the latter is more mixed (Muller and Seligson 1987; Collier and Hoeffler 1998, 2004; Nafziger and Auvinen

[18] See Alapuro (1988) and Upton (1980) for more in-depth accounts of the civil war.

2002; Fearon and Laitin 2003).[19] We present the geographical distribution of insurgency, based on insurgent casualties' place of birth, alongside with pre-conflict inequality in Figure 3.

In our context, economic inequality also directly translates to political inequality. A universal suffrage was adopted in the national elections already in 1906, but the voting rights in municipal elections were still tied to income. In most municipalities, voters would get one vote for every 100 Marks of taxes paid. Individual voting power was capped at $1/6$ of the total municipality votes. Only about one fifth of the working-age population paid enough local taxes to be granted voting rights.[20] A large share of the working class was not allowed to vote and was left without representation in many municipalities. For example, Hjerppe and Lefgren (1974) suggest that 70 percent of the population did not pay a minimum amount of taxes to be eligible to vote in the early 1900s.[21] Their chances of influencing policies decided at the municipal level—poor relief, taking care of unemployment, public safety, primary education, primary health care, and so forth—were extremely limited.

The scholarship on inequality sometimes argues that simple economic inequalities between the rich and the poor may not be enough to trigger a civil war. What might be more provoking is so-called horizontal inequality, i.e., power and resources being unequally distributed between groups that are different also in other characteristics. The political exclusion of the working classes in the pre-civil war Finland is one example of such an inequality that could have served as a catalyst for the civil conflict (Østby 2008; Buhaug et al. 2008; Wimmer et al. 2009).

We illustrate the political inequality in local elections in Figure 4. Panel A shows the amount of people in each tax bracket, and the relative weight of that group in local decision making. Note first that a large share of the population did not pay any taxes and were not allowed to vote. However, we also see that the much smaller high-income groups have nearly the same vote share as the much larger lower-income groups. In Panel B, we present the inequality in vote weights across the tax brackets. Assuming that the cap on individual voting power was not binding, the vote of a person

[19] Scholars such as Collier and Hoeffler (1998, 2004) link the emergence of rebellion to the availability of financing and potential recruits. Fearon and Laitin (2003) argue that grievances alone are not enough to generate political violence. Their central thesis is that civil wars happen in "fragile states with limited administrative control of their peripheries". These arguments align with early theoretical work by sociologists such as Tilly (1978) and Skocpol (1979). For further discussion, see also Boix (2008).

[20] The unit of taxation was the household, meaning that a larger part of the population was actually taxed.

[21] In an essay from 1932, future Finnish President Urho Kekkonen describes the municipal voting rights before universal suffrage as follows:

> *[...] only a handful of municipality's wealthiest citizens became elected, and it could even be the case that the richest few percent could overrule everyone else in this voting system. Participation in municipal decision-making was the right of merely a few, and working and middle class members in the countryside and cities had no way of influencing municipal policy-making. The public opinion was strictly against voting rights based on income for a good reason [...]*

in the highest income bracket would have weighed 800 times the vote of a person in the lowest income bracket, for example.

Taking these arguments together, we make the following claim regarding the correlational relationship between inequality and insurgency, and the reduced-form effect of the famine on insurgency:

> **Claim 3.** Insurgency was higher in locations with more inequality, and in locations that were hit harder by the famine.

2.3 Conflict and Redistribution

After the civil war, there were important policy changes that affected both political and economic inequality. In particular, the central government implemented a major electoral reform that established universal suffrage in municipal elections. The law was passed in the parliament after the declaration of independence, some months before the conflict onset in late 1917, and after years of debate in the parliament and significant resistance from the elite and landowners. In practice, however, municipal elections with universal suffrage were not organized until after the conflict.[22]

Why did the winning side of the conflict decide to carry out a reform that especially benefited the losing side? The threat of revolution hypothesis put forward by Acemoglu and Robinson (2000) suggests that extending the franchise can act as a commitment to future redistribution to prevent social unrest. Extended suffrage will eventually result in increased redistribution and schooling, and consequently lower inequality.[23] The ruling elites may also have to resort to full-scale democratization since lesser concessions might be viewed as a sign of weakness which could lead to further unrest (Acemoglu and Robinson 2006a). Przeworski (2009), Aidt and Jensen (2014), and Aidt and Franck (2015) provide empirical evidence in support of the hypothesis. In

[22]There had been several attempts to reform the franchise already before Finland's independence of Russia (see, e.g., Mylly 2006). The Finnish Parliament first voted in favor of universal suffrage in municipal elections in the year 1906. The Senate was strongly against this, and described the new legislation as "unfair". It took until the year 1914 before the legislation was brought to the Russian emperor to be ratified which he did not do in the end. Right before Finland's independence in the year 1917, the Parliament again voted in favor of universal suffrage. This legislation was not ratified by the provisional government of Russia. Once Finland became independent of Russia, the constraint of having to have legislation accepted by Russia naturally ceased to exist.

[23]Acemoglu and Robinson (2006b) present case studies that follow such patterns. For example, in Germany, the United Kingdom, and France, franchise reforms were preceded by increasing inequality. This eventually led to social unrest and then to direct or indirect democratization. Bourguignon and Verdier (2000) construct a model that generates a similar pattern. In their model, the governing elite can subsidize the education of the non-educated. This has positive externalities but it comes at a cost of losing political control to the newly educated citizens. In their model, a partial investment in schooling is linked to an increase in inequality. With partial democratization there is little redistribution. But when more individuals get educated and the elite sees its political power diluted to the now larger voting group, they vote for further redistribution which yields a reduction in inequality.

related work, Weingast (1997) and Wantchekon and Neeman (2002) construct formal arguments that it might not be until a civil conflict escalates that the elites concede to democratize.

The first democratic municipal elections with universal suffrage were held only half a year after the civil war. The reform extended municipal voting rights to some of those who fought with the government forces, but the poorer insurgent side was undoubtedly most affected. The final question that this paper seeks to answer is how this affected real outcomes.

The Meltzer and Richard (1981) model predicts that extending political power to poorer segments of society leads to more redistribution and less inequality.[24] But in general, democracy may not always change fiscal policy and the distribution of income. Counteracting forces such as the capture of democracy (Acemoglu and Robinson 2008; Acemoglu et al. 2013; Acemoglu et al. 2013) or a middle-class bias (Acemoglu et al. 2015) may also play an important role.[25] However, in Finland in 1918, the conditions were ripe for changes—and the overall consensus was that changes were needed to prevent a new conflict from emerging. After the civil war and franchise extension, previously unrepresented segments of society gained political representation in local governments. Decisions in local councils were made by a two-third supermajority, which would have made capture more costly. The conservatives, who typically represented the land owners and other members of upper social classes, rarely had enough seats to get decisions through on their own. This served as a catalyst for collaboration between the conservatives and the left (Kettunen 1986; Aatsinki 2009; Lintunen 2017).

A particularly important form of redistribution was the large land reform implemented after the civil war (Jörgensen 2006). Legislation passed in the year 1918 made it possible for the tenant farmers to buy the land they were renting at a discounted price. Four years later in 1922, the Parliament approved the *Act on the acquisition of land for settlement purposes* (278/1922) that gave the state and local authorities the right to facilitate the acquisition of land for citizens for cultivation and residential settlement. The government could redeem fractions of larger farms and redistribute the land to individuals who did not own any land before, but still had the knowledge and resources to start practicing agriculture on the newly acquired land. The local governments had significant discretion in how the land was reallocated.

[24]For empirical evidence that franchise extension leads to changes in public policies, see for example Husted and Kenny (1997), Lott and Kenny (1999), and Miller (2008). On the other hand, Mulligan et al. (2004) offer cross-country evidence suggesting that democracies and non-democracies are not that different in terms of their economic policies. Similarly, Castañeda Dower et al. (2018) find little evidence of more redistribution after increased representation of peasants in Imperial Russia.

[25]If the democracy is captured, rich segments of society can take other actions to offset the reallocation of power by increasing their de facto power. For example, they could try to control the political agenda of parties by lobbying or repression. Second, early franchise reforms often also gave voting rights to many members of the middle classes. It is possible that the middle class could then have used its power to tax the poor and redistribute to itself. The implications for inequality are ambiguous in a simple median-voter framework discussed by Acemoglu et al. (2015).

Our final claim concerns the evolution of inequality and redistribution after franchise extension. As before, we consider the relationship between the famine and changes in redistribution and inequality causal. The other relationships are correlational, but we believe that they are likely mechanisms through which the famine could matter for later outcomes.

> **Claim 4.** Inequality decreased and redistribution increased more in locations that were hit harder by the famine, that had a higher inequality before the civil war, and that had more insurgents.

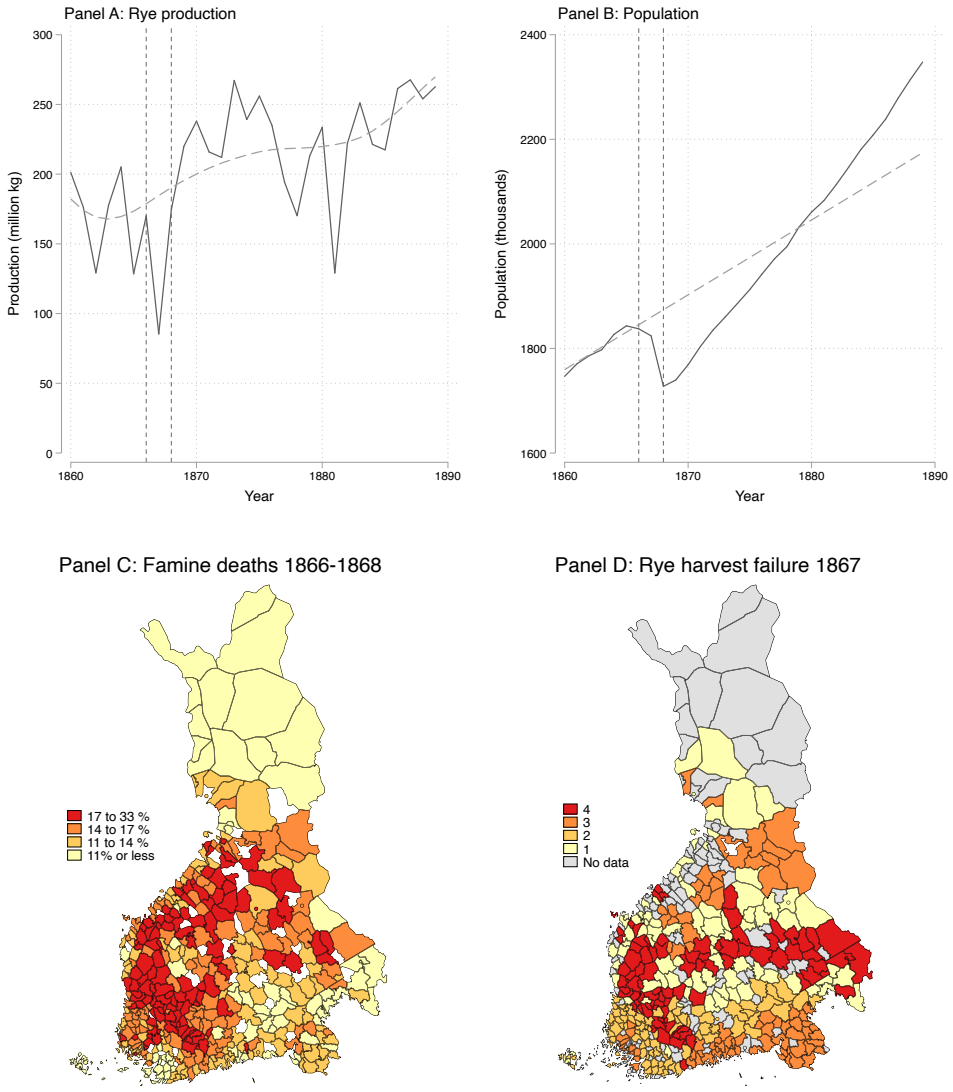

Figure 2. Geographical distribution of famine exposure.

Notes: Panel A shows rye production in Finland as documented in Viita (1965). Panel B shows the Finnish population between 1860-1890 and a linear prediction based on the prior four decades of population growth. The population decrease was 7.8% during the famine years 1866-1868 relative to the predicted population. Panel C plots the famine casualties relative to the 1865 population in each municipality. The map in Panel D plots the geographical distribution of the severity of rye harvest failure. A higher number reflects a poorer harvest. We construct this measure based on the harvest quality data reported by Hirvonen (2013). We net out rye suitability, and show the residuals divided into quartiles.

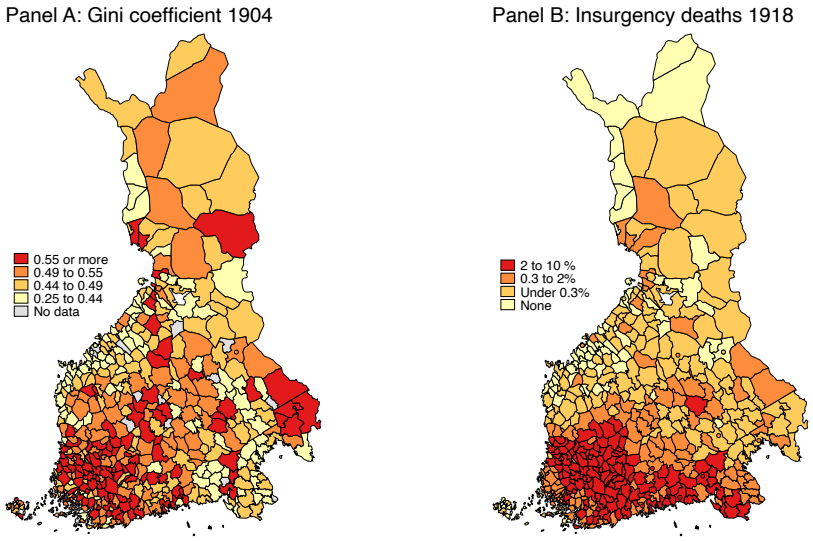

Figure 3. Geographical distribution of pre-civil war inequality and insurgent deaths.

Notes: The map in Panel A shows income inequality in 1904 in each municipality. The map in Panel B shows insurgency deaths in each municipality relative to the male population.

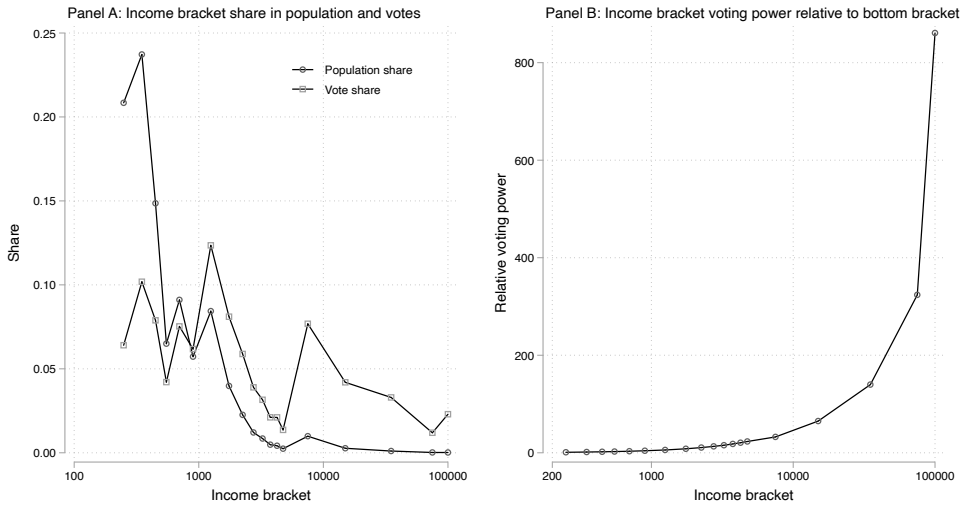

Figure 4. Taxable income and voting power in local elections

Notes: Panel A plots the share of the population and the vote share of each income bracket in 1904. Panel B plots the voting power of a person in an income bracket relative to a person in the lowest income bracket.

3 Data

We have collected and digitized an extensive amount of historical data to test whether our claims have empirical support. This section discusses these data and their sources. We also assess the exogeneity of the famine exposure, as this will be crucial for our empirical investigation.

3.1 Main Variables

We start by characterizing our main dependent and independent variables. Summary statistics on the data that we use in our main analyses are presented in Table 1.

Exposure to the Famine We construct two measures to capture the geographical variation in exposure to the famine. Our first measure is the number of deaths between 1866 and 1868 relative to the 1865 population. The number of deaths in each municipality is documented in the Official Statistics of Finland publication *Syntyneet, vihityt ja kuolleet vuosina 1865-1868* from the year 1871. On average, around 15% of population died between these years.[26] We consider the famine deaths to serve as a proxy for the local severity of the famine shock, including economic and social hardship and not only the demographic change. Second, we use information on the quality of the 1867 rye harvest from Hirvonen (2013). These data were originally collected for reports sent to the Russian Finance Ministry by regional governors of Finland. Hirvonen (2013) quantifies the information in the reports on a scale from 1 to 5, where 1 corresponds to a complete crop failure, and 5 corresponds to a good harvest. We create an indicator variable for a crop failure using these data. It gets the value 1 if a municipality had a crop failure in 1867, and it is equal to 0 otherwise.[27] Overall, a striking 35% of the municipalities covered in our data experienced a crop failure in 1867.[28]

Inequality Measures To measure both economic and political inequality, we use extensive data on taxes and land ownership. First, we use the Official Statistics of Finland tables on taxes for the years 1865, 1881, 1932, 1935, and 1938 to measure income inequality in each municipality. These statistical tables include the number of people in each income bracket in a given municipality

[26] This figure is larger than the 8% reported by, e.g., Turpeinen (1986). This is because our measure includes all deaths during the period, instead of capturing only excess mortality.

[27] Municipalities can have multiple reports for the year 1867. We take the average of these reports and consider any value below 2 as a crop failure. In that case, at least one report must have stated that a municipality experienced a complete crop failure (value 1).

[28] As we discussed earlier, the main reason for the famine were poor weather conditions. Finland had only two weather stations in 1867, so measuring weather shocks credibly at the municipal level is not possible.

which allows us to estimate a Gini coefficient for each municipality.[29] While Statistics Finland did not publish any data on income and property between 1881-1920, we are able to estimate income inequality closer to the 1918 civil war by using municipal income tax statistics drawn from Hjelt and Broms (1904). These municipal income statistics are also binned by income group, which allows a consistent estimation of the Gini coefficients. The Hjelt and Broms data only include information on rural municipalities. On average, the income Gini was 0.50 in 1904, and the average change between the years 1904-1938 was -0.25. Given that Finland at the time relied heavily on agriculture, much of the income inequality was closely linked with land inequality. As an alternative measure of inequality measured slightly closer to the conflict, we estimate land inequality for each municipality using farm size information from the 1910 Finnish Agricultural Census. The Gini coefficient for land ownership averaged at 0.39 in 1910.

Labor Coercion Outcomes We measure the extent of labor market coercion in the Finnish rural municipalities by using the information on the share of tenant farmers, the share of terminated tenant contracts, and daily agricultural wages. Tenant farming was a prevalent form of farming, and the lack of land ownership is often attributed as a sign of labor coercion as discussed above. In our data, 45% of all farms were tenant farms. These data come from the 1910 Finnish Agricultural Census. Frequently, the quality of land given to the tenant farmers was poor and making the land suitable for cultivation required additional work and effort. Historical accounts suggests that land owners sometimes terminated tenant farmers' contracts after they had put lots of effort into turning their rental land productive (Rasila 1970). We thus also measure agricultural labor coercion by the amount of terminated tenant leases. The amount of terminated tenant leases in each municipality comes from Haataja (1916). We scale the number of terminated contracts by all farms in 1910. The resulting variable averages at 4 percent. We also collect information on the average daily wages paid to farm laborers. These are available in Statistics Finland's publication on farming and animal husbandry in the year 1908. We use the daily male wage during the summer as a proxy for farm labor wages. On average, this wage was 3.07 Finnish Marks.[30]

Insurgency Participation We use a unique dataset of all Finnish Civil War casualties, collected by the Finnish government in the *Suomen sotasurmat 1914-22* project, to approximate Civil War participation. These data include the side of the conflict (Red or White), the occupation, and the municipality of birth of each individual who died in the civil war. There are about $39,550$

[29]The income statistics are binned by tax groups and a large part of the population did not pay any taxes. We estimate the Gini coefficient using a robust Pareto midpoint estimator suggested by von Hippel et al. (2016) which takes into account the binned nature of the data and the open-ended largest tax bracket. This implementation is available in the `rpme` package in Stata. Alternative metrics for income inequality are highly correlated with the Gini coefficient for the years for which we can measure them (see Appendix B).

[30]One Finnish Mark in 1908 is roughly equal to four U.S. dollars today.

individuals in the dataset.[31] Since the database separates the casualties by the side, we can use the revolutionary side deaths as a precise measure of insurgency participation. We use the municipality of birth of the insurgents to calculate the number of causalities in each municipality and divide this number by the municipal male population in 1910 to obtain the insurgent death rate. Using the municipality of birth allows us to capture also those rural workers who migrated to the cities before the civil war. On average, 2% of the male population died in the conflict. As the majority of the insurgency casualties documented in the dataset occurred in prison camps after the cease fire, and as we are using the municipality of birth rather than the municipality of residence, the casualties capture the local insurgency participation more broadly and not only the severity of local battles.[32]

Redistribution We measure redistribution after the conflict using information on the inter-war land reform, and Statistics Finland's statistics on municipal spending. The land reform data are taken from Statistics Finland's land survey for the years 1919-1923. This publication includes a statistical map of new farms created in the land reform in each municipality. We have digitized and georeferenced the contents of this map and measure the land reform intensity by the number of new farms created by the land reform during the years 1919-1923, relative to the total number of farms in 1910. For public spending, we have gathered the information for the years 1912 and 1932. 1912 is the earliest available information about municipal spending, and 1932 is the first year after the civil war when we can measure spending. These publications report how much each municipal government spent on poor relief, health care, and schooling, and thus they cover the main redistributive policies of local governments. Spending in these areas did not rely on the national budget, and decisions on the policies were entirely up to the municipal governments. We scale the spending variables by population and take logarithms. Our main variable of interest is the change in (log) per capita spending in a given municipality between 1912 and 1932.

Covariates Finally, we use an extensive set of covariates to capture potentially important differences in municipal labor markets and other characteristics. In particular, we have collected information on population in 1865 from Statistics Finland's publication *Suomen väestö joulukuun 31 p:nä 1865*. We measure the soil suitability for different crops in each municipality using data from the Global Agro-Ecological Zones (GAEZ) database.[33] We also use information on terrain slope taken from the same database. Finally, our econometric analyses also control for the latitude and longitude of the centroid of each municipality.

[31]The Finnish National Archive has made these data publicly available online at `http://vesta.narc.fi/cgi-bin/db2www/sotasurmaetusivu/main` (accessed May 31, 2019).

[32]Most active insurgents were imprisoned even though not all were eventually sentenced. The majority of the prisoners died of diseases such as the 1918 Spanish flu pandemic.

[33]We take a logarithm of crop suitability without adding a constant to zeros. This means that areas that are not suitable for cultivation are left out of our analyses.

Table 1. Summary statistics.

	N	Mean	Std. dev.	Min.	Max.
Famine					
Famine deaths 1866-1868	409	0.15	0.05	0.05	0.34
Crop failure 1867	334	0.34	0.47	0.00	1.00
Grievances					
Income Gini 1904	451	0.50	0.09	0.25	0.87
Land Gini 1910	442	0.39	0.08	0.15	0.70
Tenant farm share	442	0.46	0.24	0.00	0.95
Terminated lease share	429	0.04	0.06	0.00	0.30
Daily wage 1908	422	3.07	0.58	2.00	5.00
Civil war					
Insurgency per capita	450	0.02	0.02	0.00	0.10
Redistribution outcomes					
Δ Gini 1904-1938	444	-0.25	0.09	-0.62	0.28
Land reform share	437	0.27	0.19	0.00	0.86
Δ Welfare spending 1912-1938	438	3.24	0.41	2.05	4.64
Δ School spending 1912-1938	436	1.87	0.42	0.38	3.24
Δ Health spending 1912-1938	434	3.78	0.80	1.73	5.97

3.2 Pre-Famine Characteristics

Before proceeding to the main analysis, we study whether the municipalities that were more exposed to the famine were somehow different from those that were less affected already before the famine. Table 2 describes the various pre-famine characteristics of municipalities. First, we report the means and standard deviations of the pre-determined covariates in column (1). Column (2) presents regression coefficients (and corresponding standard errors) from univariate regressions where we regress pre-famine characteristics one by one on the population share of famine deaths. We see that there are some potentially important pre-existing differences. For example, municipalities that experienced more deaths were, on average, more populous, poorer, located in particular parts of the country, and less suitable for barley and rye farming. These associations suggest that it is important to control for these characteristics in the empirical analysis.

Next, columns (3)-(5) compare municipalities by their crop failure status. According to our classification, 112 municipalities in our data experienced a crop failure, and 223 did not. Columns (3) and (4) present the means (and standard deviations) of our covariates for these two groups. We compare the means in the groups by regressing each covariate on the crop failure dummy. We condition these comparisons on the suitability for rye cultivation. The logic behind this is that many locations were less suitable for rye cultivation and frequently experienced crop failures.

Ideally, we would like to capture an exogenous shock. As we do not have any baseline trends available, we proxy the commonness of crop failures by controlling for the general suitability for rye cultivation. Even after adjusting for rye suitability, we see that crop failure captures something meaningful in terms of famine exposure. Overall, we find municipalities that experienced a crop failure had a significantly higher death rate during the famine years. The difference is around 4 percentage points which is about half of the excess mortality observed at the national level. In contrast, we do not detect any statistically significant differences in pre-famine characteristics between municipalities that experienced a crop failure and those that did not. While none of the individual differences are statistically significant, the F-statistic for the test of joint significance of the differences in column (5) is 1.07. Therefore, we fail to reject the hypothesis that crop failures are uncorrelated with pre-famine characteristics ($p = 0.39$). This provides support for our empirical strategy.

Table 2. Famine deaths and pre-famine municipal characteristics.

	Mean	$\beta^{Famine\,deaths}$	No failure	Crop failure	$\beta^{Crop\,failure}$	Observations
	(1)	(2)	(3)	(4)	(5)	(6)
Famine deaths 1866-1868	0.15		0.14	0.18	0.04***	409
	[0.05]		[0.04]	[0.04]	(0.01)	
log(Population 1865)	8.06	0.19**	8.24	8.17	-0.10	420
	[0.74]	(0.08)	[0.69]	[0.60]	(0.08)	
Pop. density 1865	1.57	0.02	1.73	1.84	0.07	420
	[1.92]	(0.15)	[2.01]	[2.18]	(0.28)	
Rainfall	5.96	-0.09**	6.07	5.92	-0.04	451
	[0.42]	(0.05)	[0.38]	[0.34]	(0.06)	
ln(Slope)	9.00	0.01	8.99	8.99	0.00	451
	[0.08]	(0.01)	[0.08]	[0.08]	(0.01)	
Income Gini 1865	0.31	-0.03**	0.32	0.30	-0.00	349
	[0.12]	(0.01)	[0.11]	[0.10]	(0.01)	
Mean income 1865	14.40	-2.69**	14.26	13.51	-0.22	349
	[6.58]	(1.05)	[5.99]	[4.24]	(0.64)	
ln(Dist. to Helsinki)	5.39	0.13**	5.24	5.57	0.07	451
	[0.65]	(0.05)	[0.65]	[0.57]	(0.10)	
ln(Dist. to Turku)	5.26	0.17**	5.14	5.56	0.15	451
	[0.91]	(0.08)	[0.94]	[0.57]	(0.11)	
Latitude	61.98	0.75***	61.51	62.53	0.30	451
	[1.49]	(0.15)	[1.30]	[1.14]	(0.22)	
Longitude	24.80	-0.33	25.01	25.17	-0.05	451
	[2.70]	(0.28)	[2.88]	[2.70]	(0.32)	
ln(Barley suitability)	8.23	-0.24***	8.39	8.00	-0.00	451
	[0.57]	(0.05)	[0.61]	[0.40]	(0.00)	
ln(Cereal suitability)	7.70	-0.01	7.77	7.69	0.02	450
	[0.31]	(0.03)	[0.33]	[0.26]	(0.03)	
ln(Rye suitability)	6.62	-0.22***	6.79	6.39		451
	[0.57]	(0.05)	[0.61]	[0.41]		

Notes: Column (1) shows the mean of famine deaths and other pre-famine characteristics across municipalities. Column (2) shows the correlation between famine deaths and pre-famine characteristics. Columns (3) and (4) show the famine deaths and pre-famine characteristics by the occurrence of crop failure in 1867. Column (5) shows the correlation of famine deaths and pre-treatment characteristics with crop failure, conditional on rye suitability. Standard deviations and robust standard errors are reported in brackets and parentheses, respectively. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

4 Main Findings

This section presents the main results. We first study the effects of the famine shock on inequality and labor coercion in the long run. Then, we proceed to analyze how these factors influenced participation in the Finnish Civil War. We conclude our main analyses by examining changes in inequality and redistribution after the conflict and the subsequent adoption of democratic local government institutions.

4.1 Persistent Effects of Famine on Inequality and Labor Coercion

Evolution of Inequality We begin our empirical investigation by showing the dynamic relationship between inequality and the famine of 1866-1868. To do so, we estimate an event-study specification of the form:

$$Gini_{mt} = \sum_{s \neq 1865} \delta_s \left(Famine\ exposure_m \times \mathbf{1}(t = s) \right) + \lambda_m + \lambda_t + \eta_{mt}, \tag{1}$$

where $Gini_{mt}$ is income inequality in municipality m in the year t, and $Famine\ exposure_m$ is either the death rate during the famine years or a dummy for a crop failure in 1867. λ_t and λ_m are the county-specific year and municipality fixed effects, respectively, and η_{mt} is the error term. δ_t are year-specific coefficients that we plot in Figure 5, setting 1865 as the base year. We present more detailed regression results from the event-study specification in Appendix Table C1. We cluster the standard errors at the municipal level. We include our baseline controls (rye suitability, latitude, longitude, the logarithm of the population in 1865, and land ruggedness) interacted with year fixed effects.[34]

The pattern of the event-study estimates shows that the famine caused an inverted U-shaped response in Finnish inequality, where the turning point appears after the 1918 Civil War and after the extension of the franchise. Panel A of Figure 5 illustrates that inequality was first increasing more in areas with more deaths during the famine years. Furthermore, inequality also decreased the most in these areas after the Civil War of 1918 was over. The estimated impact of famine deaths on inequality in 1904, relative to the base-year 1865, is 0.533. This means that a one-standard-deviation increase in the famine severity is associated with approximately a 0.027 point increase in the Gini index. Panel B shows a similar relationship between inequality and the crop failure in 1867, with a crop failure causing a 0.031 points higher change in inequality. We can reject the

[34] We choose these baseline controls to maximize the number of observations that we have available for our estimations, including the subsequent analyses. The results that are reported in this paper are robust to alternative sets of control variables.

hypothesis that $\delta_{1904} = \delta_{1931}$ for both measures, meaning that inequality declined more in areas with a more severe famine exposure.

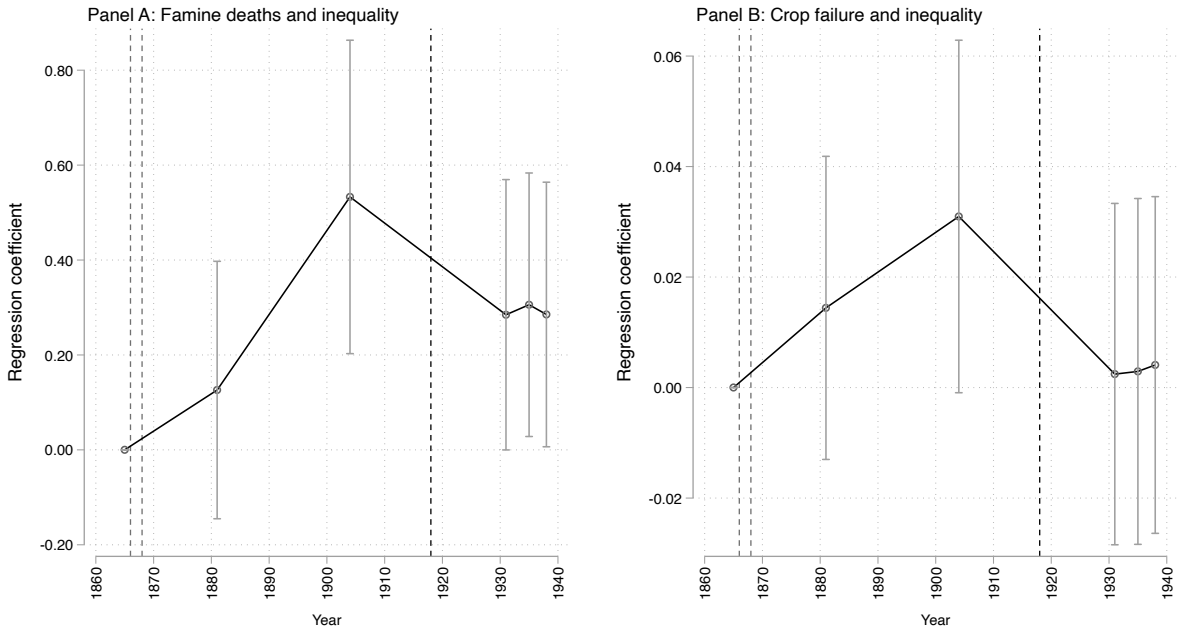

Figure 5. Event-study estimates.

Notes: The figures plot the raw differences from an event-study specification and their 95% confidence intervals that are constructed using standard errors clustered at the municipality level. We use 1865 as the base year. Estimations include county fixed effects and baseline controls interacted with year fixed effects. The first two vertical lines mark the famine years, and the third line shows the civil war year.

Effect of the Famine on Inequality and Labor Coercion before the Civil War of 1918 We continue our empirical investigation by showing that the famine of 1868 was associated with higher inequality and worse labor market outcomes in the early 1900s. Our main specification takes the form

$$Grievance_m = \alpha + \beta Famine\ exposure_m + X_m'\gamma + \varepsilon_m \tag{2}$$

where $Grievance_m$ is a grievance outcome in municipality m, measured by income inequality, inequality in land ownership, tenant farmer share, terminated contract share, or daily wage. $Famine\ exposure_m$ is again either the death rate during the famine years or an indicator for a crop failure in 1867, X_m' is a vector of controls, and ε_m is an error term. The coefficient of our interest is β which captures the effect of the famine shock on the grievance outcome. All our specifications include a set of baseline control variables. We also report results from a

specification that is augmented with county (*lääni*) fixed effects to control for any unobservable region-specific attributes. During the period of our study, there were eight counties.[35] We use heteroskedasticity robust standard errors throughout the paper, although we also report Conley (1999) standard errors that correct for possible spatial autocorrelation among municipalities within 50 kilometers of each other. These two standard errors are similar.

We first look at the effects of the famine on income and land inequality. Panel A of Table 3 shows our estimates of the effect of the famine on the Gini coefficient for income inequality in 1904. These results corroborate our event-study findings in Figure 5. Panel B reports estimates for land inequality in 1910. When we control for region fixed effects, the estimates for the famine severity are 0.214 for income inequality (Panel A, column 2) and 0.239 for land inequality (Panel B, column 2). This means that a negative population shock equal to one standard deviation (or 5% of the population) is associated with an increase of about 0.01 in both income and land inequality. In columns (3) and (4), we report the direct impact of the 1867 crop failure on the inequality outcomes. The results from regressions that control for county fixed effects suggest that experiencing the crop failure increased the income Gini coefficient by 0.022 points and the land Gini coefficient by 0.020 points. These point estimates are statistically significant across specifications and economically meaningful, but not very large relative to the means of 0.51 (income Gini) and 0.40 (land Gini).[36]

In Table 4, we study how the famine shock affected labor markets in the early 1900s. We find that greater famine exposure increased the share of tenant farms substantially (Panel A). The OLS estimate reported in column (2) is 1.107. This estimate means that a one-standard-deviation increase in famine severity is associated with a 0.055 higher tenant farmer share in 1910. This is a significant shift also relative to the mean of 0.47. Column (4) shows that the impact of a crop failure is 0.058. We find less strong evidence on other measures of labor coercion. For instance, the statistically non-significant impact of a crop failure on the share of terminated leases is around 0.002, the average share being about 5%. The daily wage of workers was about 0.085 Finnish Marks lower in locations that experienced a crop failure. This effects is relatively small compared to the average daily wage which was about three Finnish Marks. Furthermore, the most conservative estimates with county fixed effects are not statistically significant at any conventional levels. The results on wages are robust to scaling the daily wages by land prices (hectares per 100 Finnish Marks). Running a regression with the full set of controls yields a point estimate of

[35]Appendix Tables C2-C6 document that all our main results are robust to including all the predetermined covariates that we observe as control variables in our regressions (see Table 2). However, this considerably cuts down the number of observations that we have available. Our results are also robust to an approach that uses the crop failure indicator as an instrumental variable for famine deaths. We refer to Appendix Tables C7-C11 and the associated discussion for further information.

[36]In the supplementary materials, we show that all our results for the crop failures are robust to using a randomization inference approach (see Appendix Figure C1).

-5.898 ($p = 0.003$) for famine deaths per capita, and a point estimate of -0.395 ($p = 0.056$) for crop failure.

We interpret these results as evidence of an increase in labor coercion. This interpretation aligns with previous work by, for example, Naidu (2010), Naidu and Yuchtman (2013), and Tyrefors Hinnerich et al. (2017) in other historical contexts. As we discussed earlier, our results go against the standard economic logic in which negative labor supply shocks would improve bargaining position of the working class. In our case, the rising inequality interacted with coercive rural labor market institutions, and this resulted in even worse conditions for the agricultural workers.

Table 3. Famine and inequality in the early 1900s.

	(1)	(2)	(3)	(4)
Panel A: Income Gini 1904				
Famine deaths per capita	0.315***	0.214**		
	(0.086)	(0.085)		
Crop failure			0.022**	0.022**
			(0.010)	(0.011)
Conley SE	0.106	0.110	0.012	0.011
N	409	409	328	328
R^2	0.212	0.287	0.138	0.233
Outcome mean	0.496	0.496	0.507	0.507
Panel B: Land Gini 1910				
Famine deaths per capita	0.252***	0.239***		
	(0.075)	(0.079)		
Crop failure			0.022***	0.020**
			(0.008)	(0.008)
Conley SE	0.089	0.083	0.009	0.009
N	403	403	324	324
R^2	0.132	0.225	0.133	0.212
Outcome mean	0.392	0.392	0.396	0.396
Controls	✓	✓	✓	✓
County FE		✓		✓

Notes: Baseline controls included in all estimations are latitude, longitude, log suitability for rye, log suitability for barley, log elevation, and log population in 1865. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table 4. Famine and labor coercion in the early 1900s.

	(1)	(2)	(3)	(4)
Panel A: Tenant farm share				
Famine deaths per capita	1.359***	1.107***		
	(0.195)	(0.171)		
Crop failure			0.087***	0.058***
			(0.024)	(0.021)
Conley SE	0.312	0.295	0.029	0.024
N	403	403	324	324
R^2	0.382	0.560	0.415	0.594
Outcome mean	0.464	0.464	0.468	0.468
Panel B: Terminated leases share				
Famine deaths per capita	0.073	0.039		
	(0.056)	(0.058)		
Crop failure			0.008	0.002
			(0.006)	(0.006)
Conley SE	0.064	0.058	0.007	0.006
N	390	390	314	314
R^2	0.269	0.326	0.248	0.317
Outcome mean	0.046	0.046	0.048	0.048
Panel C: Daily wage				
Famine deaths per capita	-4.105***	-3.473***		
	(0.612)	(0.626)		
Crop failure			-0.204***	-0.085
			(0.078)	(0.072)
Conley SE	0.697	0.633	0.086	0.080
N	382	382	306	306
R^2	0.279	0.389	0.146	0.275
Outcome mean	3.076	3.076	3.011	3.011
Controls	✓	✓	✓	✓
County FE		✓		✓

Notes: Baseline controls included in all estimations are latitude, longitude, log suitability for rye, log suitability for barley, log elevation, and log population in 1865. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

4.2 Determinants of Insurgency Participation

Explaining Civil War Participation Next, we turn to the effects of the the famine on insurgency in 1918, proxied by the insurgency casualty rate. Panel A Table 5 reports our estimates on determinants of insurgency participation. It shows that municipalities that were more severely affected by the famine experienced more insurgent deaths in the 1918 Civil War. The regression results for famine deaths per capita reported in column (1) and (2) suggest that an increase of one standard deviation in famine deaths is associated with $0.003 - 0.005$ more per capita insurgent deaths, respectively. The magnitude of these estimates is meaningful compared to the mean of 0.016. The results in columns (3) and (4) imply that experiencing a crop failure in 1867 resulted in 0.004 more insurgent deaths per capita. The size of the effect is again considerable relative to the mean, about one quarter. The last two columns in Table 5 look at the correlation between income inequality in the early 1900s and insurgency. It is possible that the famine affected insurgency through its effect on inequality which we demonstrated in the previous section. When we include all control variables, we see that increasing inequality by one standard deviation is also associated with about 0.004 more per capita deaths. The findings are robust to including different control variables, to using the logarithm of the dependent variable, and to a randomization inference approach. Therefore, our results supports our third claim regarding the origins of insurgency.

To provide further evidence, we also study the relationship between insurgency deaths and land inequality as well as insurgency deaths and different measures of labor coercion. These analyses are reported in Appendix Table C12. As we would expect, land inequality and labor coercion are associated with a higher insurgent death rates. The regression results are significant both statistically and economically. A one-standard-deviation increase in land inequality is linked with 0.003 more insurgent deaths. Similar shifts in the share of tenant farmers or the share of terminated tenant farmer contracts are associated with an increase of 0.005 in the insurgent death rate, i.e., about one third of the mean. The regression results on wages are less robust in terms of statistical significance, but they follow an expected pattern: locations with higher wages before the civil war had less insurgency deaths

In Panel B of Table 5, we then assess the association between the famine shock or inequality in the early 1900s, and White casualties. By examining casualties on different sides of the conflict, we are not only measuring those who had less to lose in general, or were born in somehow more conflict-prone areas. While columns (1) and (2) indicate a positive correlation between famine casualties and White casualties, columns (3) and (4) suggest that crop failures had no impact on civil war participation on the government side. Similarly, economic inequality in the early 1900s is not associated with White casualties.

Our findings go somewhat against prior explanations offered by Finnish scholars of the conflict. For example, Alapuro (1988) argues, in line with Tilly (1978) and Skocpol (1979), that dissatisfaction with the economic circumstances did not cause the rebellion. Instead, he stresses the importance of abundant resources and armed organizations of the Red and White guards. Alapuro (1988) also highlights the role of limited state capacity and the power vacuum that the end of the Russian rule created. These were undoubtedly elementary factors, but they cannot explain the geographical variation in insurgency within Finland that we observe.

Table 5. Famine, inequality, and insurgency.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Insurgent casualty share						
Famine deaths per capita	0.103***	0.079***				
	(0.018)	(0.015)				
Crop failure			0.004**	0.004**		
			(0.002)	(0.002)		
Income Gini					0.054***	0.040***
					(0.009)	(0.008)
Conley SE	0.036	0.024	0.003	0.002	0.015	0.011
N	408	408	327	327	419	419
R^2	0.441	0.620	0.404	0.583	0.440	0.617
Outcome mean	0.016	0.016	0.018	0.018	0.016	0.016
Panel B: White casualty share						
Famine deaths per capita	0.008***	0.009***				
	(0.003)	(0.003)				
Crop failure			0.000	0.000		
			(0.000)	(0.000)		
Income Gini					-0.003	0.000
					(0.002)	(0.002)
Conley SE	0.003	0.003	0.000	0.000	0.002	0.002
N	408	408	327	327	419	419
R^2	0.107	0.291	0.072	0.229	0.096	0.267
Outcome mean	0.003	0.003	0.003	0.003	0.003	0.003
Controls	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓

Notes: Baseline controls included in all estimations are latitude, longitude, log suitability for rye, log suitability for barley, log elevation, and log population in 1865. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Who Joined the Insurgency? We take advantage of the rich individual-level dataset to further describe the socioeconomic status of those who perished—and thus participated in—the civil conflict. The occupations of the deceased are potentially a good proxy for the occupations of those who participated in the conflict. If the conflict truly was about demanding more redistribution and equality, we would expect that the insurgents disproportionately represent lower socioeconomic groups.[37]

We present the occupational structure of the deceased in Panel A of Figure 6.[38] It clearly shows that the deaths on the revolutionary side of the conflict originated in larger numbers from less-skilled and lower-wage occupations. The largest share of casualties were from the unskilled manual labor group which mainly consisted of lower-level farm (including tenant farmers) and factory workers. Land laborers were also an important part of the Red casualties. On the government side, land owners were the largest group among the civil war casualties. Arguably, they would have had the most to lose if the redistributional policies demanded by the insurgents had been passed. In Panel B of Figure 6, we show the share of deaths in the occupational groups by the side of the conflict. It shows that approximately 90% of the high grade professionals who died in the conflict were on the White side, whereas over 90% of the unskilled manual laborers were on the Red side. In the group of non-manual employees, the shares are almost equal between the two sides of the civil war.

4.3 Inequality and Redistribution after the Civil War

The last step of our empirical analysis concerns the turn in inequality and the increase in redistribution after the Civil War of 1918. After the conflict, the national government implemented large scale land reforms that allowed tenant farmers to buy the land they occupied and other lands they required. In addition to the land reform, the franchise was extended in the municipal councils to all citizens and votes were no longer tied to taxable income. This plausibly affected differently regions with different pre-conflict distributions of income and voting rights.[39] In this section, we provide evidence on how these national policies (together with the earlier violent uprising) could explain the downward turn in Finnish inequality.

[37] In related work, Arosalo (1998) uses aggregate-level data to study how the economic and political situation of different social strata is associated with political violence during the Finnish Civil War. She also quotes reports from the hearings of the high treason courts of the year 1918 which suggest that about one third of the insurgents were motivated by shortage of work and low wages.

[38] We assign the occupations to larger socioeconomic classes that broadly follow the commonly used Eriksson and Goldthorpe (1992) classification scheme. Note that we only use information on civil war casualties who came from municipalities included in the sample that we use in our main analyses.

[39] Tyrefors Hinnerich et al. (2017) study a similar reform in Sweden.

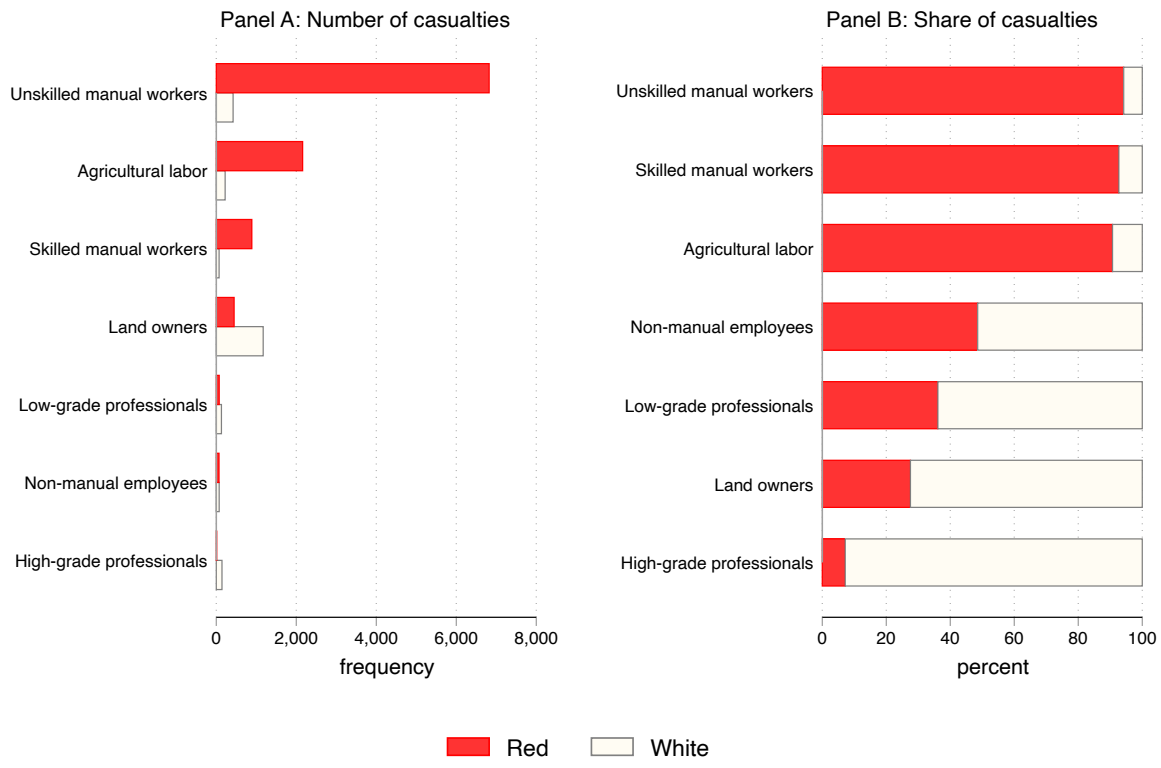

Figure 6. Civil war casualties by occupational class and conflict side.

Evolution of Redistribution We start by looking at changes in the Gini coefficient between the years 1904-1938. Panel A of Table 6 reports the regression results on the association between the change in inequality and the famine, initial inequality, and insurgency. As already established in Figure 5, inequality decreased more after the conflict in areas that were most affected by the famine shock. The estimates in columns (1) and (2) of Panel A show that the Gini coefficient dropped approximately 0.01 points more between 1904 and 1938 in locations that experienced one standard deviation more famine deaths. Similarly, municipalities that experienced a crop failure also saw about $0.02 - 0.03$ points larger decreases in income inequality (columns 3 and 4). This impact is equal to about one-tenth of the mean.

We then look at the association between changes in inequality and the initial level in columns (5) and (6). Municipalities with more inequality in the early 1900s had a more concentrated distribution of political power. Thus, they ought to be affected the most by the municipal franchise reform. Indeed, columns (7) and (8) of Panel A reveal that a one-standard-deviation increase in income Gini 1904 is associated with about a 0.08 points larger change in the Gini coefficient. This change is almost 32% of the mean. Furthermore, the last two columns in Panel A show that inequality decreased the most in locations that had more insurgent deaths. A change

equal to one standard deviation in insurgency per capita is associated with a 0.018 point decrease in inequality. The effect size is about 7% relative to the mean.

We turn to explain this decrease in inequality by changes in different forms of redistribution in Table 7. We measure redistribution by the large land reforms implemented in the 1920s as well as by per capita municipal spending changes between the years 1912 and 1932. We see an increase in redistributive policy outcomes, especially in health care spending, but most of the large and sudden shift in inequality after the conflict can be attributed to the land reform. Panel A of Table 7 shows that the number of farms created in the 1920s land reforms (relative to the number of farms in 1910) is significantly higher in municipalities that were hit by a more severe famine shock, that had a more unequal distribution of income before the conflict, and that saw more insurgency. For instance, the estimate in column (4) implies that experiencing a crop failure led to an increase of $0.05 - 0.07$ in land redistribution which is about one fourth of the mean. Column (6) implies that a similar increase in the 1904 Gini coefficient is associated with a 0.035 increase in the dependent variable (13% of the mean), and based on column (8) we can infer that a one-standard deviation increase in insurgency per capita is associated with a 0.065 higher land redistribution (24% of the mean).

In the remaining panels of Table 7, we assess the relationship between changes in municipal spending and the famine shock, income inequality before the conflict, and insurgency. Columns (1)-(4) look at the association between the famine shock and municipal spending growth. The estimates are positive and statistically significant for the famine deaths per capita, but the evidence is weaker in the case of the crop failure variable. In Panel D, we see that municipalities that were more exposed to the famine might have experienced greater growth of municipal health care spending. Income inequality in 1904 has a clear positive association with the change in welfare spending (columns 5 and 6 of Panel B). There is no statistically significant relationship between initial inequality and municipal school spending growth (Panel C), although the point estimates are positive. However, it appears that increases in health spending are greater in municipalities that were more unequal before the civil war (Panel D). The regression results for insurgency per capita (columns 7 and 8) are largely in line with the other point estimates in terms of their sign and statistical significance.

While there is some indication that policies changed after the democratization, the changes in public spending were not overwhelming. But as discussed by Acemoglu et al. (2015), democracy does not change inequality quickly by affecting policies. The reason inequality decreased rapidly in Finland seems to be because of the extensive land reforms which plausibly had a more immediate effect on inequality in an agrarian society. Yet, our results do suggest some changes in social policies, especially in health spending at the municipal level which can be expected to be targeted to the very lowest socioeconomic classes.

Table 6. Regression results for change in inequality after the civil war.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable: Δ Gini 1904-1938								
Famine deaths per capita	-0.275***	-0.220**						
	(0.094)	(0.100)						
Crop failure			-0.021**	-0.025**				
			(0.010)	(0.011)				
Income Gini					-0.810***	-0.847***		
					(0.048)	(0.049)		
Insurgency per capita							-0.778***	-0.843***
							(0.245)	(0.325)
Conley SE	0.099	0.111	0.011	0.011	0.047	0.040	0.222	0.356
N	404	404	326	326	415	415	415	415
R^2	0.050	0.074	0.024	0.104	0.605	0.637	0.032	0.079
Outcome mean	-0.250	-0.250	-0.255	-0.255	-0.252	-0.252	-0.252	-0.252
Controls	✓	✓	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓		✓

Notes: Baseline controls included in all estimations are latitude, longitude, log suitability for rye, log suitability for barley, log elevation, and log population in 1865. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table 7. Regression results for redistribution after the civil war.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Land redistribution								
Famine deaths per capita	1.073***	0.777***						
	(0.175)	(0.177)						
Crop failure			0.067***	0.050**				
			(0.019)	(0.021)				
Income Gini					0.584***	0.398***		
					(0.088)	(0.092)		
Insurgency per capita							4.008***	3.264***
							(0.611)	(0.705)
Conley SE	0.275	0.249	0.021	0.020	0.113	0.099	0.594	0.686
N	398	398	320	320	409	409	409	409
R^2	0.339	0.419	0.416	0.498	0.339	0.418	0.373	0.435
Outcome mean	0.276	0.276	0.278	0.278	0.272	0.272	0.272	0.272
Panel B: Δ Municipal welfare spending 1912-1932								
Famine deaths per capita	0.827*	0.951**						
	(0.445)	(0.470)						
Crop failure			-0.036	-0.005				
			(0.052)	(0.054)				
Income Gini					0.722***	0.667***		
					(0.244)	(0.246)		
Insurgency per capita							2.428*	0.924
							(1.362)	(1.608)
Conley SE	0.450	0.410	0.051	0.051	0.330	0.302	1.844	1.679
N	398	398	322	322	409	409	409	409
R^2	0.082	0.143	0.082	0.155	0.114	0.178	0.101	0.163
Outcome mean	3.220	3.220	3.248	3.248	3.229	3.229	3.229	3.229
Panel C: Δ Municipal school spending 1912-1932								
Famine deaths per capita	1.123**	0.887*						
	(0.475)	(0.517)						
Crop failure			0.048	0.061				
			(0.045)	(0.051)				
Income Gini					0.206	0.004		
					(0.245)	(0.258)		
Insurgency per capita							0.413	0.606
							(1.133)	(1.305)
Conley SE	0.573	0.615	0.046	0.048	0.176	0.183	1.399	1.576
N	397	397	321	321	408	408	408	408
R^2	0.106	0.133	0.112	0.141	0.093	0.127	0.091	0.127
Outcome mean	1.871	1.871	1.853	1.853	1.865	1.865	1.865	1.865
Panel D: Δ Municipal health spending 1912-1932								
Famine deaths per capita	2.998***	2.517***						
	(0.818)	(0.901)						
Crop failure			0.186**	0.174*				
			(0.093)	(0.100)				
Income Gini					1.182**	1.011*		
					(0.506)	(0.533)		
Insurgency per capita							7.381***	8.252***
							(2.511)	(3.062)
Conley SE	0.895	0.997	0.130	0.135	0.503	0.514	2.598	2.853
N	394	394	322	322	405	405	405	405
R^2	0.042	0.062	0.059	0.082	0.027	0.049	0.032	0.056
Outcome mean	3.791	3.791	3.824	3.824	3.790	3.790	3.790	3.790
Controls	✓	✓	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓		✓

Notes: Baseline controls included in all estimations are latitude, longitude, log suitability for rye, log suitability for barley, log elevation, and log population in 1865. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

5 Discussion

Compared to other European countries and the major political reforms carried out at the national level in Finland, the municipal suffrage reform was implemented relatively late. Scholars have speculated that if concessions had been made sooner, the Finnish Civil War could have been avoided (see, e.g., Katajisto 2018). In light of our empirical illustration, these speculations seem warranted: the conflict was most intense in locations that had higher underlying (political and economic) inequality. There were signs of discontent already before the civil war which we discuss first in this section. We next try to shed some light on why inequality triggered a civil war in Finland. For instance, Finland's western neighbor Sweden had high levels of inequality and similar voting institutions until the year 1919 (see Bengtsson 2019). Yet the country did not experience a civil war, but reformed its institutions in more peacefully. In Norway, the state successfully suffocated the perceived revolutionary threat by reducing working hours and expanding social transfer programs (Bergli Rasmussen and Knutsen 2020). Lastly, we rule out some alternative mechanisms through which the civil war could have plausibly influenced inequality and redistribution.

5.1 Discontent before the Civil War

There were signs of resentment already before the civil war. For instance, industrial action and strikes became more and more common alongside with the increasing popularity of the labor movement. For some time, the elites were able to make concessions to hold major conflicts at bay. The general strike of 1905 is a good example (see, for instance, Alapuro 1988). In October 1905, many factories, government offices, schools and stores were closed for a few days in different parts of the country. It has been argued that to appease the working classes who were disgruntled by poor working and living conditions, the elites carried out major reforms such as ending the era of Russification in Finland and granting universal suffrage in the Parliamentary elections.[40]

Economic Grievances and Voting The first Finnish Parliament was elected in 1907 in an election where almost all men and women older than 24 years were allowed to vote. While the reform did not change the fact the Russian Emperor held the ultimate political power in the country, it allowed, for the first time in history, parts of the population to vote and express their political preferences. Did the voting behavior in pre-conflict elections exhibit any signs of discontent? It is well known

[40]Russification was a policy aimed at limiting the autonomic status of the Grand Duchy of Finland during the years 1899–1905. During this era, young Finnish men were conscripted in the Russian military, Finnish newspapers were under strict censorship, and public officials were fired and replaced with Russians, among other things. The country experienced another period of Russification right before the civil war, in 1908–1917. See, for instance, Haltzel et al. (1981).

that voters may reward or punish the incumbent based on their personal economic stance, or they may use past outcomes as a basis for their expectations for future outcomes (Fiorina 1981; Key 1966).

To test for this possibility, we have collected data on the results of parliamentary elections by municipalities to measure variation in political preferences. We concentrate on votes for the Social Democratic Party. Due to the universal suffrage, the Social Democratic vote share also capturates more moderate tendencies than direct insurgency participation; some individuals may have been supporters of a non-violent revolution. In the early 1900s, the Social Democratic Party was the largest individual party but the bourgeois parties held more than half of the seats for most of the first two decades. The Social Democratic Party was actively advocating for tenant farmers' rights, a land reform, improving the working conditions of laborers, and limiting the number of working hours, among other things.

In Panel A of Table 8, we quantify the relationship between Social Democrats' average vote share in elections held between 1910 and 1917, our measures of famine exposure, and income inequality in 1904. The positive connection is clear. For instance, the column (4) shows that Social Democrats' vote share was about $6-9\%$ higher in municipalities that had a crop failure in the year 1867. Focusing on column (6), we see that a one-standard-deviation shift in the 1904 Gini coefficient is associated with an increase of about 6% in Social Democrats' vote share. The magnitude of these point estimates seems sizeable also compared to the average vote share of about 40%. We complement these findings with Appendix Table D1 that explores the link between different land inequality and labor coercion measures, and Social Democrats' electoral performance. In all of these estimations, we see a correlation that we would expect to see: Social Democrats performed better in municipalities where land inequality was higher, or if the local labor market conditions were more coercive.

Workers' Associations Close to the Social Democratic party were so-called workers' associations that became increasingly popular in the late 1800s and early 1900s among agricultural, industrial, and other workers. They were organizations that were distinct from labor unions, and they were intended to foster cooperation among the working classes and improve their working conditions. Alapuro (1988), among others, has highlighted the importance of workers' associations in organizing the insurgent groups and arming them during the civil war.

We have collected data on local workers' associations and their membership numbers from statistics published by the Social Democratic Party (see Turkia 1907). Panel B of Table 8 indicates that there were more workers' association members per capita in municipalities that were hit harder by the famine (columns 1-4). It is also clear that a larger share of the municipal population belonged to a workers' association in more unequal locations (columns 5 and 6). The

specification that controls for county fixed effects suggests that a one-standard-deviation increase in the 1904 Gini coefficient is associated with a 2% higher membership rate. We corroborate these findings in the auxiliary materials by showing that land inequality and labor coercion measures were also positively associated with workers' association membership rates (Appendix Table D2). Appendix Tables D3 and D4 show that the relationship between the presence of workers' associations, inequality and labor coercion follows the same pattern.

Strikes in 1917 Workers' associations also had an important role in mobilizing workers during strikes. Strike activities continued actively even after the general strike of 1905, and the year preceding the Civil War the number of strikes hit a new record. For example, during the previous peak year of 1907, there had been slightly less than two hundred strikes with around 20,000 participants. In the year 1917, the number of strikes rose close to five hundred. Almost 140,000 workers joined the protests, and about one and a half million working days were lost in total (Tilastokeskus 1990). According to Aatsinki (2009), the largest occupational groups that took part in strikes were agricultural workers (about 16,000 participants) and log drivers (about 17,000 participants) whose demands were typically about lowering the number of working hours and increasing wages.

We find some evidence suggesting that agricultural workers were participating more in municipalities that were hit harder by the famine and that had higher levels of inequality and labor coercion. In Panel C of Table 8, we illustrate the relationship between the total number of strike days per capita, and the famine and economic inequality. While systematically positive and robust to different specifications, the regression results do not suggest that there is a statistically significant relationship between the famine and strike participation. The results for inequality are clearer. For example, column (6) indicates that an increase of one standard deviation in economic inequality was associated with an increase of 0.09 in the strike days per capita. We also investigate the correlation between land inequality, labor coercion, and strikes in Appendix Table D5. We see that there were more strike days per capita in locations with more land inequality, with more tenant farmers, with more terminated leases, or with lower wages.[41]

In November 1917, just a few months after the Russian revolution and a few months before the beginning of the civil conflict, the discontent culminated in another general strike. The elites did

[41] We use data collected from 1917 and 1918 issues of *Työtilastollinen aikakauslehti* and *Sosialinen aikakauskirja* (the Journal of Employment Statistics and the Journal of Social Issues) that report the occurrence of strikes and participation in them for the year 1917. Note that there were several region- and nation-wide strikes during the year. We only use data on strikes the exact location of which can be pinned down. We corroborate the results reported here by demonstrating that the famine, inequality, and labor coercion mattered for the occurrence of strikes in an expected way in Appendix Tables D6 and D7. Furthermore, Appendix Tables D8-D11 show that we obtain similar results if we use data on all strikes and not just those of agricultural workers.

try to tackle the unrest with concessions: they limited the working day length to eight hours and reformed the municipal franchise, for instance.

Civil War as a Bargaining Failure It is possible that inequality would have dropped even without an armed conflict. While in other Nordic countries the state renegotiated the social contract making many social changes without any violent conflict, the social unrest persisted in Finland and escalated into a full-blown conflict in January 1918. Why did the conflict arise despite the government's concessions and relatively rational expectation that there would be more redistribution?[42] Undoubtedly, it was important that Finland had just become independent of Russia that also experienced a revolution. But it is likely that other forces were also at play.

First, it could be that promises of changes came too late. Even if the government made appeasing concessions, the lasting inequality and labor coercion could have led to grievances that were hard to forget. Some of the violence in 1918 was likely due to this less "rationalist" motivation. In Panel A of Table 5, we saw that White casualties were not associated with inequality. However, this is only part of the story. If we focus on Whites who were murdered during the war, we see a strong positive correlation between underlying inequality and the casualty rate (Panel A of Figure 7). This further supports the argument that people in more unequal localities took part in the insurgency because of their discontent. While also the rate of murdered Reds is correlated with initial inequality, there is a crucial distinction between the murders of Reds and Whites. Panel B of Figure 7 plots the number of murdered Reds and Whites by date. We see that Whites were murdered in particular at the beginning of the conflict, whereas the murders of Reds peak towards the end of the civil war when many of them were imprisoned and executed. Battle deaths do not exhibit a similar distinction. Instead, the number of daily deaths on the two sides almost fully coincide with each other.

Second, it could also be that the government's promises were simply not enough to calm the working classes. For instance, social scientist Aksel Warén wrote an influential piece *Torpparioloista Suomessa* (1898) stating that the largest social problem in rural Finland was the uncertainty associated with tenant farming. He stressed the need for juridicial rights and written contracts for the tenants. However, he did not even imagine a complete eradication of the institution which would allow farmers to buy their own land, suggesting that the grievances in rural Finland and the concessions that were needed to avoid violence were still considerably underestimated (Peltonen 1990).

Third, it is possible that there was a lack of proper commitment to the reforms. Without democracy at the local level, it is hard to believe that redistributive policies, if implemented,

[42] A large literature in conflict studies highlights the importance of bargaining failures as a cause of civil conflict (see Walter 2009 for a review).

Table 8. Determinants of pre-civil war discontent.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: SDP vote share						
Famine deaths per capita	1.346***	0.966***				
	(0.202)	(0.196)				
Crop failure			0.089***	0.055**		
			(0.023)	(0.022)		
Income Gini					0.905***	0.717***
					(0.096)	(0.091)
Conley SE	0.282	0.267	0.030	0.028	0.139	0.118
N	363	363	294	294	374	374
R^2	0.385	0.497	0.266	0.423	0.424	0.527
Outcome mean	0.409	0.409	0.442	0.442	0.410	0.410
Panel B: Workers' association members						
Famine deaths per capita	0.186***	0.152**				
	(0.067)	(0.071)				
Crop failure			0.014*	0.014		
			(0.007)	(0.008)		
Income Gini					0.227***	0.216***
					(0.034)	(0.038)
Conley SE	0.090	0.085	0.008	0.009	0.041	0.044
N	404	404	324	324	415	415
R^2	0.170	0.210	0.164	0.197	0.236	0.270
Outcome mean	0.056	0.056	0.060	0.060	0.056	0.056
Panel C: Strike participation						
Famine deaths per capita	1.914	1.586				
	(1.233)	(1.268)				
Crop failure			0.280	0.263		
			(0.250)	(0.217)		
Income Gini					0.881**	1.007*
					(0.341)	(0.530)
Conley SE	0.890	1.125	0.234	0.189	0.240	0.347
N	404	404	324	324	415	415
R^2	0.015	0.028	0.018	0.036	0.014	0.028
Outcome mean	0.163	0.163	0.161	0.161	0.159	0.159
Baseline controls	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓

Notes: Baseline controls included in all estimations are latitude, longitude, log suitability for rye, log suitability for barley, log elevation, and log population in 1865. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

would be permanent. It is easy for the elites not to commit. In fact, this is a key part of the theoretical model of Acemoglu and Robinson (2000). Moreover, the lack of state power and the monopoly of violence could have expedited the commitment problem (Alapuro 1988).

Our fourth and final remark is related to the circle of violence that emerged in the year 1918. The civil war was not a centrally planned uprising against the elites. On the contrary, it was the result of local skirmishes that escalated to violence in many municipalities. Such a process would potentially have been hard to stop non-violently, even if the government offered more redistribution.

5.2 Some Alternative Mechanisms

There are a number of factors that could explain the increasing and decreasing trends of inequality in Finland. We will next discuss a few alternative mechanisms and illustrate that the famine was less likely to matter for later outcomes through these channels.

Industrialization before the Conflict The original argument of Kuznets (1955) was that inequality first rises with economic development as countries industrialize and then falls as the fruits of industrialization and productivity growth trickle down to the working classes. For instance, this could happen because wealthier people initially have profitable investment opportunities, while an inflow of cheap labor from rural areas to cities keeps wages down. We believe that this is unlikely to explain the pattern that we see in Finland. First, Appendix Figure D3 shows the share of workers across main sectors and the share of value added by sector. It demonstrates two key features of the Finnish economy. Finland was primarily an agrarian country in the early 20th century. Almost 70 percent of households were employed in agriculture, and almost half of the value added was generated in the agricultural sector. The rise in manufacturing was modest before the civil war. Large-scale industrialization followed only much later, in part due to the war reparations that Finland had to pay to the Soviet Union after the WWII (see Mitrunen 2020). Thus, if industrialization was the driver of the income inequality, it should have had a tremendous influence on the economy very early on in the industrialization process. Second, we do not find any evidence that the famine would have been associated with industrialization in Finland (Appendix Tables D13 and D14). Thus, it is unlikely that industrialization would be driving our results in the way that in Kuznets argued. This could have been the case, for instance, if the famine had stimulated the migration of cheap rural labor force to cities.

Famine and Emigration Another alternative explanation relating famines to democratization emphasizes the role of emigration and outside options. Indeed, in many parts of the European

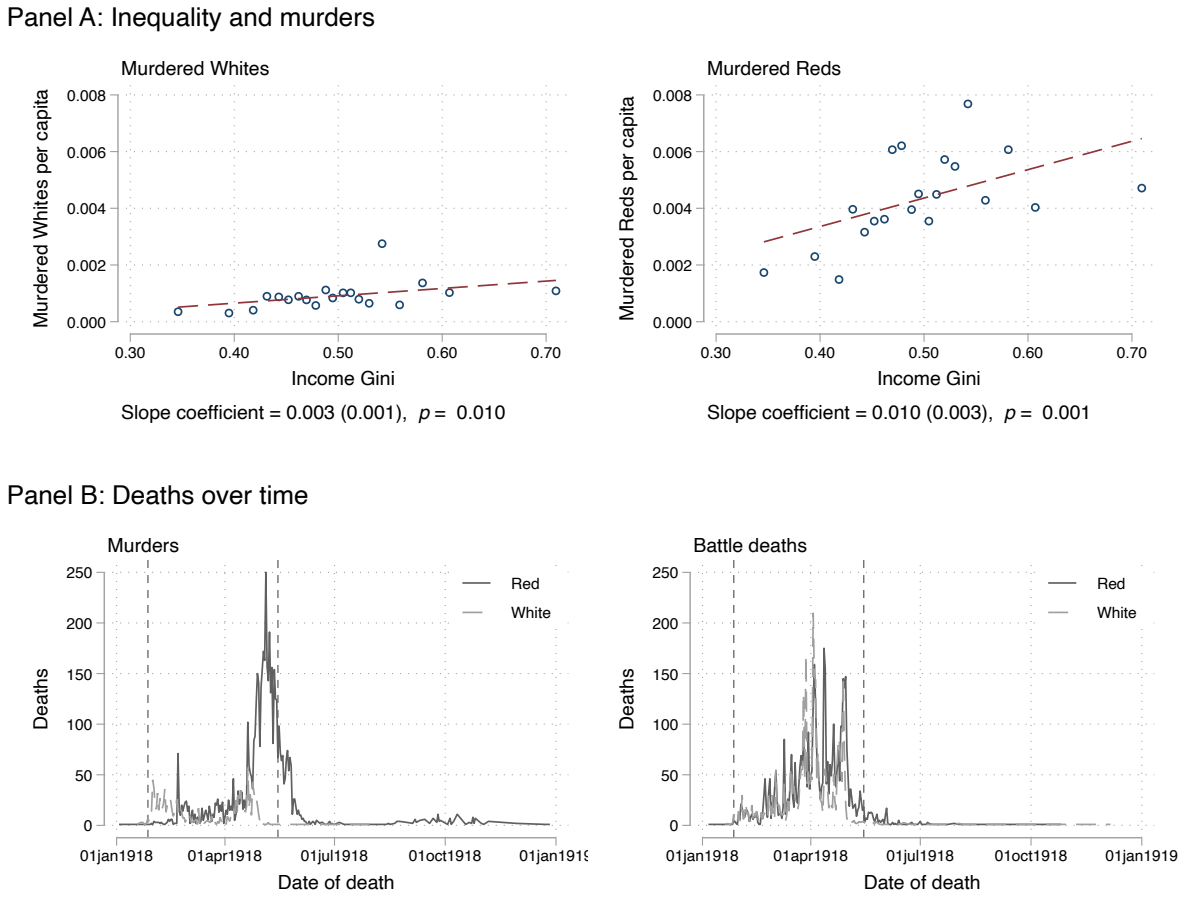

Figure 7. Murders during the Finnish Civil War.

Notes: Panel A of the figure plots the relationship between economic inequality in 1904 and murdered Whites (left-hand side graph) or Reds (right-hand side graph) per capita. Robust standard errors of the slope coefficients are reported in parentheses. Dots are binned averages computed within twenty bins with an equal number of observations. We net out baseline controls and region fixed effects. Panel B of the figure shows the daily number of deaths for individuals who were murdered (left-hand side graph) or who fell in a battle (right-hand side graph). The dashed lines mark the starting and ending dates of the Finnish Civil War.

continent, people expressed discontent after large negative shocks by emigrating to the Americas, in particular to the United States. Narciso et al. (2020) find that the Great Irish Famine, highly lethal mass starvation that took place about four decades before the Finnish famine, was a significant long-run driver of Irish individuals' migration choices. In Sweden, the same weather shock that caused the Finnish famine triggered the first wave of emigration to North America. Karadja and Prawitz (2019) show that the mass emigration increased the remaining workers' bargaining power and paved the way for increased demands for political change. Importantly, welfare expenditures went up in Swedish municipalities that also became more likely to adopt more inclusive political institutions.

A large number of Finns also emigrated during the Age of Mass Migration. However, emigration from Finland truly kicked off much later than in Sweden: there was no first wave in the aftermath of the famine, and the peak of emigration was only in 1902 when a total of 25,000 emigrants left the country. More importantly, looking at the geographic distribution of emigrants across municipalities, we do not find any compelling evidence that emigration would have been driven by the famine shock (Appendix Table D12). To investigate this possibility, we use annual emigration statistics to measure the number of emigrants from each municipality between 1893 and 1914.[43] While the regression coefficients are positive throughout the table, they are small in magnitude and mostly statistically insignificant. Thus, our results do not support the idea that the famine would have induced a massive flow of emigration, which in turn could have induced an increase in workers' bargaining power.[44]

Political Behavior before and after 1918 We have argued that the leveling of income distribution was at least partially due to the municipal franchise extension that happened after the 1918 Civil War. Also other types of political forces might have influenced the declining trend in inequality. It could be that there was a shift in the demand for left-wing politicians. Appendix Figure D1 shows regression results from an event-study specification where we look at the connection between inequality, famine, and insurgency on voting for the Social Democratic Party. If anything, this relationship appears to be negative. In particular, Social Democrats' vote share decreases the most in locations that were more exposed to the insurgency. This relationship may be merely mechanical, as insurgents were more likely to vote for the Social Democrats and also more likely to die in the war. Be it as it may, we see more redistribution in these locations despite the drop in votes for Social Democrats. Thus, it seems more likely that other parties moved closer

[43]Unfortunately, Statistics Finland did not publish emigration statistics by municipality before 1893. However, these data capture most of the variation in emigration by municipality, as emigration from Finland prior to 1890s was relatively modest and decreased dramatically during the First World War.

[44]This is in tally with the remarks made by Alapuro (1990). He argues that those individuals who were exposed to the famine simply did not have sufficient resources to protest. According to Alapuro, there were vast similarities in the famines that Finland and Ireland experienced, but the reactions in these two countries were dramatically different.

to the new median voter and this increased redistribution. However, an important limitation of this analysis is that our policies are determined by local politicians but we do not have data on local government elections. We only observe electoral behavior in the parliamentary elections.

There could also be effects on voter turnout which could have downstream effects on policymakers' incentives and their policy output. On the one hand, one might expect that a greater exposure to a civil conflict could hinder political participation, for instance, by undermining trust in the government. On the other hand, the little evidence that we have points to the opposite direction. For example, Blattman (2009) documents that forced recruitment leads to notably greater postwar political participation in Uganda. Appendix Figure D2 again presents point estimates from an event-study specification. We do not find any apparent indications that voter turnout would have reacted notably to the underlying inequality or the civil war after 1918. Thus, effects on voter turnout also seem like a less plausible channel.

6 Concluding Remarks

Inequality and conflict are both closely tied to the economic development of countries. But can inequality lead to violent conflict, and can insurgency promote a more equal distribution of wealth and income? These questions have received vast scholarly attention (see, e.g., Acemoglu and Robinson 2000; Wood 2003; Moore 2016; Scheidel 2018), but empirical work establishing these links and exploring the underlying mechanisms still remains limited. We address this deficit by using historical data from Finland. As of today, Finland is considered to be among the most equal and the most democratic societies. For instance, the World Bank's most recent estimate of Finland's Gini coefficient was 0.27, making Finland the 8th-most equal country in the world. In the Polity IV classification scheme, the country has been defined as fully democratic for more than seven decades.

Contrary to popular belief, this equality has not always been the case. Merely a hundred years ago, extreme economic inequality and exclusive political institutions were defining characteristics of the Finnish society. This paper first demonstrates that the famine of 1866-1868 had a persistent impact on inequality and labor coercion in Finland in the early 1900s. We then argue that these inequalities were a substantial trigger for the Finnish Civil War in 1918. Exploiting individual-level data on civil war casualties, we document that insurgency was higher in places that were more affected by the famine and that had more inequality and labor coercion. Finally, we show that after the civil war and the adoption of democratic practices in local elections, the shift towards equality and higher levels of redistribution was more substantial in municipalities with higher levels of inequality before the conflict.

Our study offers an empirical illustration of a case in which an insurgency and subsequent redistribution boosted equality.[45] Particularly important was the land reform that was heavily influenced by the underlying inequalities and the insurgency. Furthermore, after the Civil War previously unrepresented segments of society gained political representation in local governments and were able to participate in decision-making. Forces such as middle class bias or elite capture were less likely to play an important role, paving the path for more equality and redistribution.

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[45] Despite the seemingly fast recovery after the Civil War of 1918, the wounds of the conflict are still present in subtle ways in Finnish society. The Finnish public broadcasting company YLE surveyed a number of Finns one hundred years after the conflict in 2018. For instance, 68% of respondents said the conflict still divides Finns at least to some extent. The survey results are summarized in English at `https://yle.fi/uutiset/osasto/news/civil_war_still_divides_finland_after_100_years_poll_suggests/10025538` (accessed May 31, 2019).

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Supplementary Information

A Additional Backround on the Famine

While rye was the most important source of calories and the most important crop in Finland around the famine years, also other types of crops were affected by the shock. We illustrate this in Figure A1 where Panel A shows grain production for four types of crops (rye, barley, oats, and wheat) over time. We see that besides rye, barley crop was particularly affected by the same underlying wether conditions. Oats and wheat were less important for calorific intake. The producer prices of all crops hiked up around the famine years, as we show in Panel B. In Figure A2, we demonstrate that other types of agricultural products were less affected by the famine. On the other hand, the figure suggests that the production of these crops did not increase in response to the famine.

We provide some descriptive evidence on the economic problems caused by the famine in Figure A3. These figures use data on the universe of Finnish newspapers to illustrate that the prevalence of newspaper mentions of bankruptcy and auctions increased after the famine. The share of pages containing hits for bankruptcy and auction increased five and two fold, respectively. We also find evidence suggesting that a gap in advertisements on sales and purchases emerges around the famine years. The newspaper data are available through the National Library of Finland at `https://digi.kansalliskirjasto.fi/search?formats=NEWSPAPER&set_language=en` (accessed May 27, 2020).

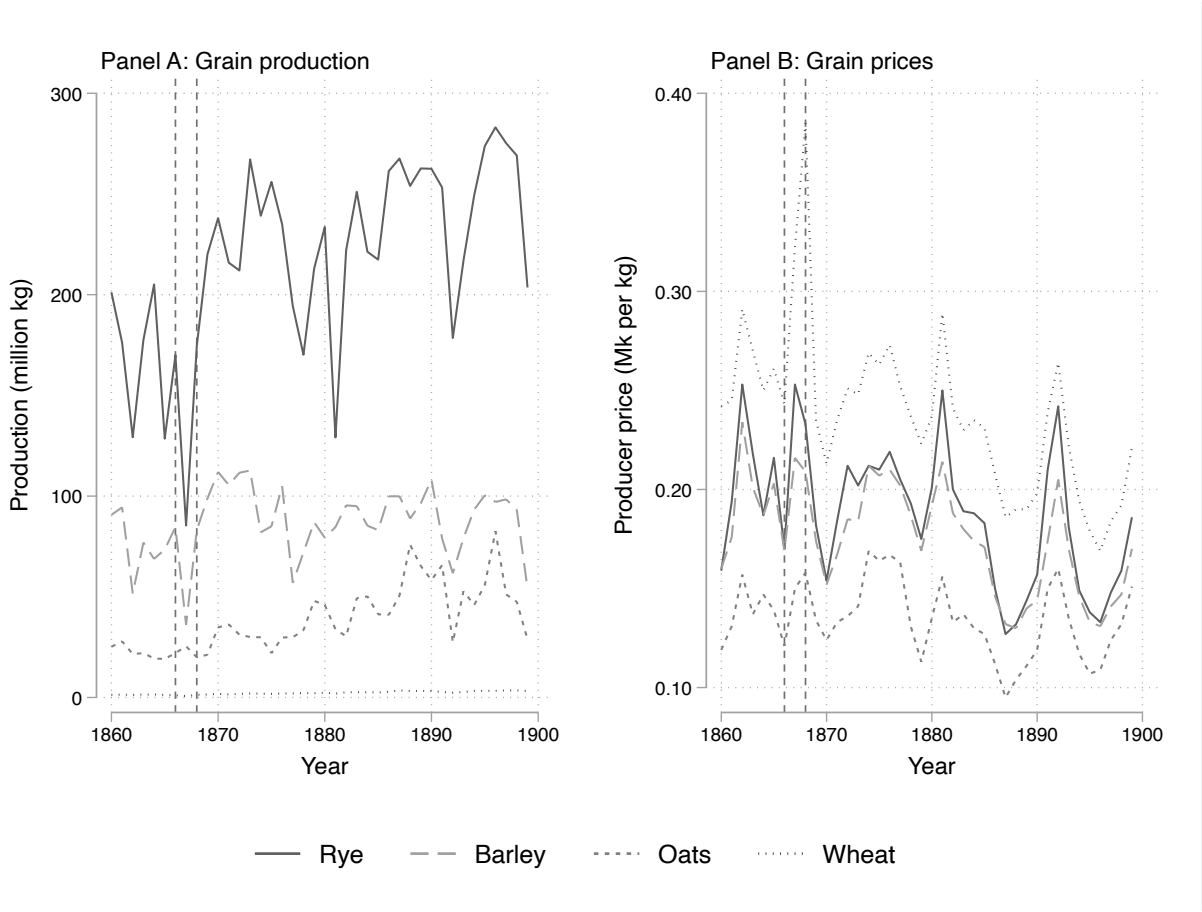

Figure A1. Grain production and prices over time.

Notes: The dashed lines mark the famine years. The left-hand side graph shows grain production in millions of kilograms, and the right-hand side graph reports the producer prices (Finnish Marks per kilogram). The data come from Viita (1965).

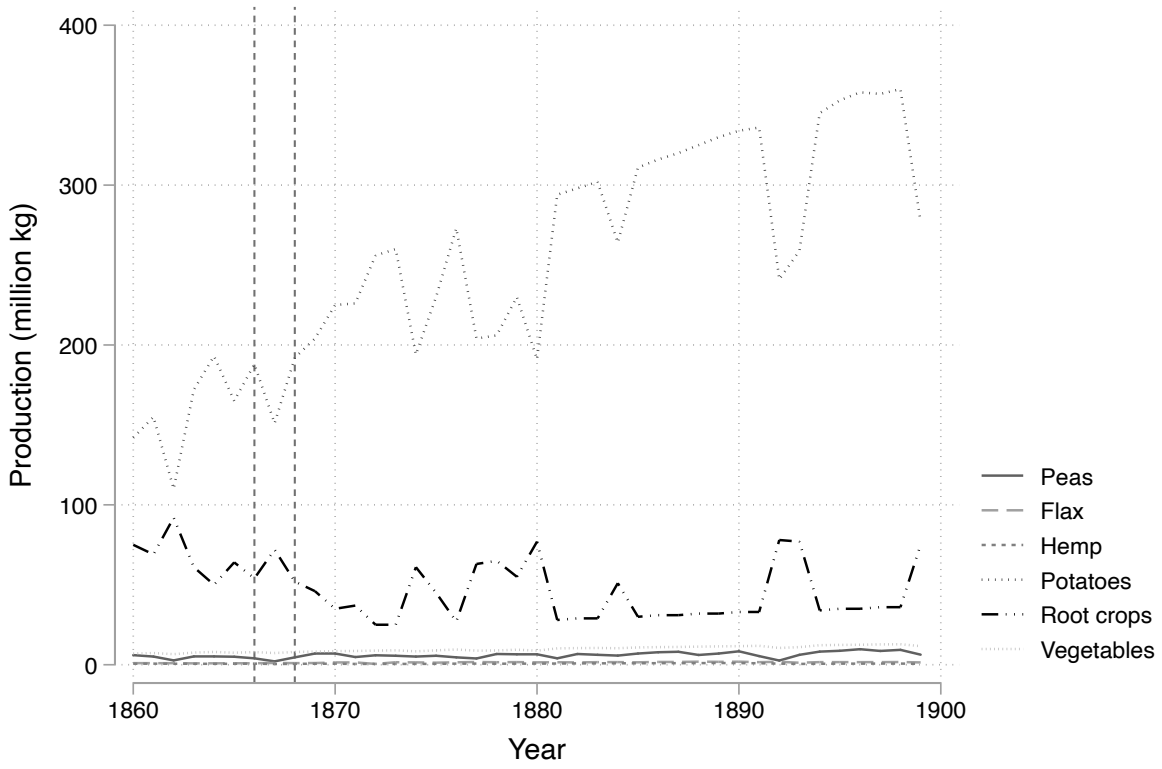

Figure A2. Other agricultural production over time.

Notes: The dashed lines mark the famine years. The graph shows agricultural production in millions of kilograms. The data come from Viita (1965).

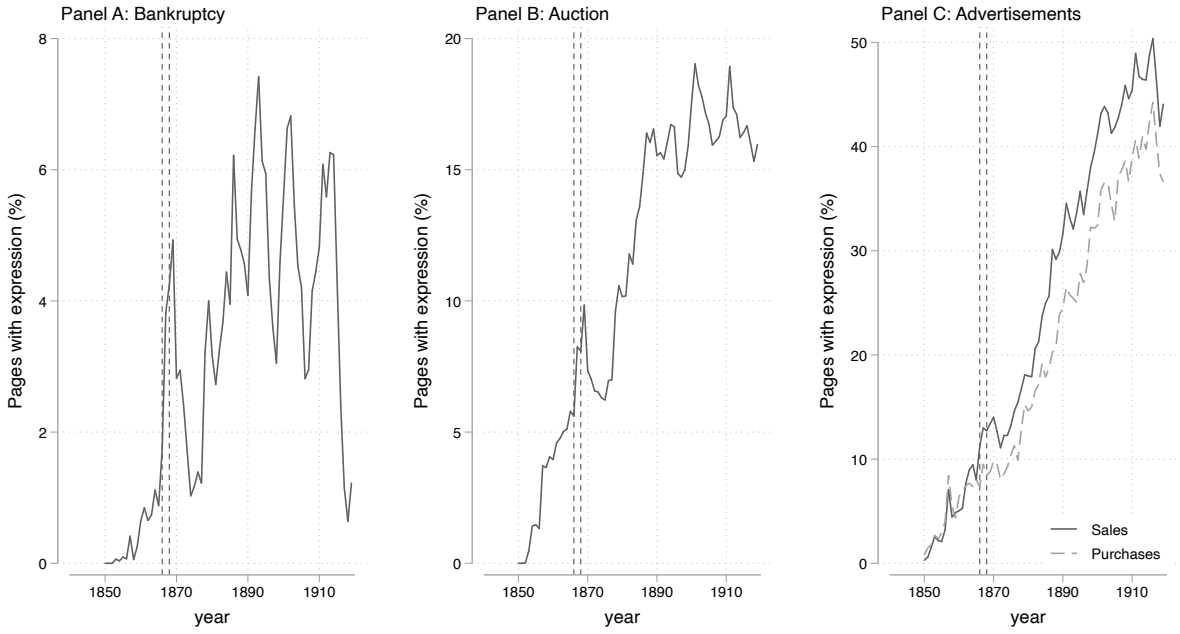

Figure A3. Famine in newspapers.

Notes: The dashed lines mark the famine years. The figures are based on data from the National Library of Finland. The data are available online at `https://digi.kansalliskirjasto.fi/search?formats=NEWSPAPER&set_language=en` (accessed May 27, 2020).

B Measurement of Inequality

The municipality level income statistics for years 1865, 1881, 1932, 1935, and 1938 are binned by tax groups and a large part of the population did not pay any taxes. To estimate Gini coefficients for each municipality we use robust Pareto midpoint estimator suggested by von Hippel et al. (2016) which takes into account the binned nature of the data and the open-ended largest tax bracket. This implementation is available in the `rpme` package in Stata.

Our income data allow us to estimate a number of other inequality measures. They are all strongly correlated with the Gini coefficient. We illustrate this point graphically in Figure B1 where we plot the Gini coefficient against eight different inequality measures. The first inequality measure in the is the median-mean ratio. It is particularly interesting to see a close connection here, as in the Meltzer-Richard model that we use as one of the theoretical motivations behind our study. The other figures correlate the Gini coefficient with other occasionally used inequality measures: relative mean deviation; Mehran, Piesch, Kakwani, Theil and generalized entropy indices; and coefficient of variation. The relationship appears to be linear in most cases, and we also obtain high R^2.

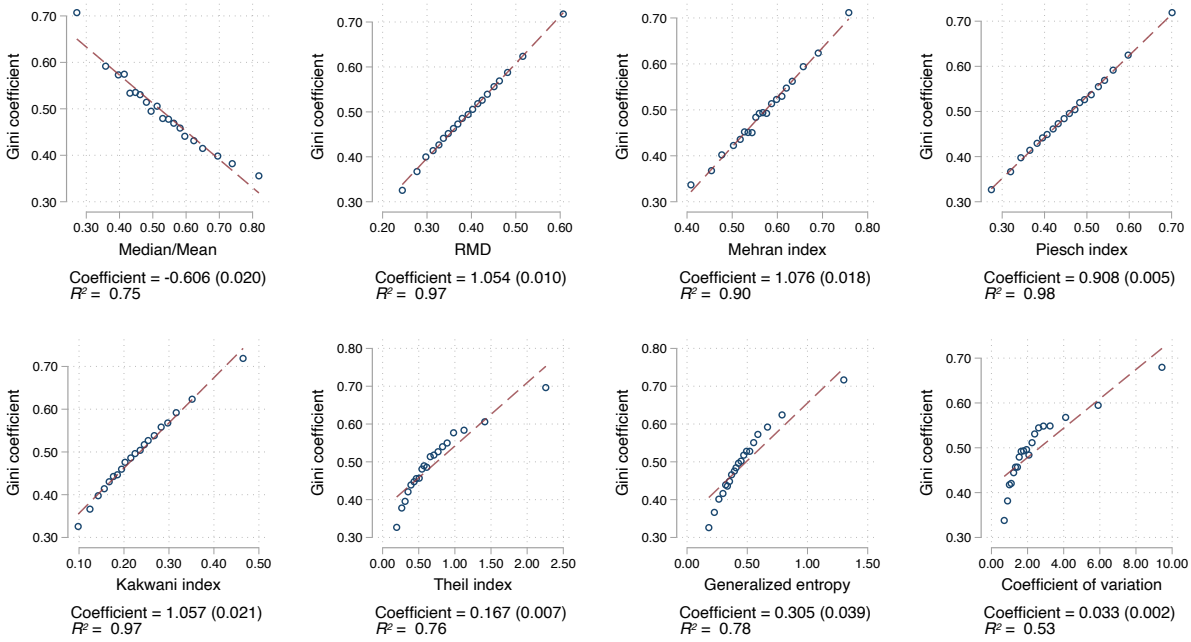

Figure B1. Relationship between the Gini coefficient and other inequality measures.

Notes: The figures show the relationship between the Gini coefficient and other inequality measures. We use data from the year 1904. Observations have been binned within twenty bins with an equal number of observations. Robust standard errors are indicated in parentheses.

C Additional Tables and Figures for the Main Results Section

C.1 Event-Study Estimates

As a starting point for our empirical analyses, we estimated the following event-study model:

$$Gini_{mt} = \sum_{s \neq 1865} \delta_s \left(Famine\ exposure_m \times \mathbf{1}(t = s) \right) + \lambda_m + \lambda_t + \eta_{mt}.$$

Here $Gini_{mt}$ is income inequality in municipality m in the year t and $Famine\ exposure_m$ is either the death rate during the famine yearsor a dummy for crop failure in 1867. λ_t and λ_m are year and municipality fixed effects, respectively, and η_{mt} is the error term. δ_t are year-specific coefficients that we plotted in the main text Figure 5. We present more detailed estimates in Table C1, again setting 1865 as the base year. We cluster standard errors at the municipal level and include county fixed effects and our baseline controls (rye suitability, latitude, longitude, logarithm of population in 1865, and land ruggedness) interacted with year fixed effects. One important notion from this table is that we can reject the hypothesis that $\delta_{1904} = \delta_{1931}$ for both measures, meaning that inequality declined more in areas with more severe famine. Furthermore, we see that the point estimates obtained for post-civil war years decline compared to the years before. This gives support to our main argument that the Civil War was an important turning point in inequality.

Table C1. Flexible effects of famine on income inequality.

	(1)	(2)	(3)
Panel A: Famine deaths and inequality			
δ_{1881} × Famine death share	0.309**	0.224	0.126
	(0.133)	(0.138)	(0.138)
δ_{1904} × Famine death share	0.654***	0.613***	0.533***
	(0.150)	(0.162)	(0.168)
δ_{1931} × Famine death share	0.371***	0.314**	0.285*
	(0.141)	(0.150)	(0.145)
δ_{1935} × Famine death share	0.377***	0.353**	0.306**
	(0.145)	(0.153)	(0.141)
δ_{1938} × Famine death share	0.378***	0.344**	0.285**
	(0.146)	(0.154)	(0.142)
N	2037	2037	1996
R^2	0.769	0.786	0.798
$\delta_{1904} = \delta_{1931}$ (p-value)	0.001	0.002	0.027
Panel B: Crop failure and inequality			
δ_{1881} × Crop failure	0.015	0.014	0.014
	(0.012)	(0.014)	(0.014)
δ_{1904} × Crop failure	0.030**	0.038**	0.031*
	(0.014)	(0.016)	(0.016)
δ_{1931} × Crop failure	0.008	0.008	0.002
	(0.013)	(0.016)	(0.016)
δ_{1935} × Crop failure	0.007	0.008	0.003
	(0.013)	(0.016)	(0.016)
δ_{1938} × Crop failure	0.010	0.013	0.004
	(0.013)	(0.016)	(0.015)
N	1762	1762	1732
R^2	0.775	0.789	0.801
$\delta_{1904} = \delta_{1931}$ (p-value)	0.048	0.019	0.020
Year × County FE		✓	✓
Year × Controls			✓

Notes: All regressions include municipality fixed effects. Robust standard errors clustered at the municipality level are reported in parentheses. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

C.2 Robustness to Additional Controls

In our main analyses, we chose the set of control variables to maximize the number of observations we have available. However, as Table 2 reveals, we have a more extensive set of pre-treatment covariates available for a more limited sample. We repeat our main analyses by augmenting the set of baseline controls (logarithm of population in 1865, rye suitability, barley suitability, land ruggedness, latitude, and longitude) with the other variables that we have: population density, rainfall, income Gini in 1865, mean income in 1865, distance to Helsinki, distance to Turku, and cereal suitability.

Table C2 reports the regression results for the impact of the famine on income and land inequality in the early 1900s. We see that the estimates for the relationship between the famine death share or the crop failure and the 1904 Gini index or the 1910 land Gini are statistically significant and in the same ballpark as the results that we report in the main text. We also find strong evidence that the famine exposure is linked with increasing prevalence of tenant farming (Panel A of Table C3). There does not appear to be a relationship between the famine and terminated leases (Panel B), but there is weak evidence of famine exposure affecting wages negatively (Panel C).

We then assess the robustness of the insurgency results in Table C4. The results suggest that insurgency participation was higher where the famine caused more deaths or in municipalities that lost their rye harvest. Furthermore, the correlation between pre-conflict inequality and insurgency persists even after we include the additional control variables (columns 5 and 5).

Our results on inequality and redistribution after the civil war remain similarly unaffected by the inclusion of the extended set of controls. Table C5 still indicates that inequality went down the most in locations that were the most exposed to the famine (columns 1-4), that had higher levels of inequality in the early 1900s (columns 5 and 6), or that had more insurgency participation in the Civil War (columns 7 and 8). As in the main text, the robustness check for Table C6 shows that we can attribute the decrease in inequality mostly to the land reform (Panel A). The regression results for spending growth are less clear (Panels B, C, and D), although there is some indication of health spending increasing more in municipalities that were more severely affected by the famine, that initially had more inequality, or that had more insurgency participation.

Table C2. Famine and inequality in the early 1900s (additional control variables included).

	(1)	(2)	(3)	(4)
Panel A: Income Gini				
Famine deaths per capita	0.307***	0.195*		
	(0.105)	(0.110)		
Crop failure			0.023**	0.031***
			(0.011)	(0.012)
Conley SE	0.112	0.123	0.010	0.011
N	333	333	290	290
R^2	0.310	0.349	0.217	0.305
Outcome mean	0.499	0.499	0.508	0.508
Panel B: Land Gini				
Famine deaths per capita	0.226***	0.194**		
	(0.079)	(0.092)		
Crop failure			0.020**	0.017**
			(0.008)	(0.008)
Conley SE	0.084	0.088	0.007	0.008
N	329	329	288	288
R^2	0.316	0.324	0.352	0.368
Outcome mean	0.398	0.398	0.400	0.400
Controls	✓	✓	✓	✓
County FE		✓		✓

Notes: Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table C3. Famine and labor coercion in the early 1900s (additional control variables included).

	(1)	(2)	(3)	(4)
Panel A: Tenant farmer share				
Famine deaths per capita	1.369***	0.924***		
	(0.212)	(0.206)		
Crop failure			0.079***	0.057***
			(0.023)	(0.022)
Conley SE	0.297	0.292	0.025	0.023
N	329	329	288	288
R^2	0.584	0.663	0.583	0.681
Outcome mean	0.468	0.468	0.467	0.467
Panel B: Terminated leases share				
Famine deaths per capita	0.070	0.016		
	(0.070)	(0.073)		
Crop failure			0.009	0.001
			(0.007)	(0.008)
Conley SE	0.064	0.053	0.006	0.006
N	317	317	278	278
R^2	0.342	0.383	0.335	0.382
Outcome mean	0.049	0.049	0.049	0.049
Panel C: Daily wage				
Famine deaths per capita	-4.010***	-3.139***		
	(0.720)	(0.749)		
Crop failure			-0.171**	-0.077
			(0.073)	(0.071)
Conley SE	0.904	0.862	0.068	0.071
N	316	316	275	275
R^2	0.361	0.411	0.272	0.369
Outcome mean	3.014	3.014	3.006	3.006
Controls	✓	✓	✓	✓
County FE		✓		✓

Notes: Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table C4. Famine, inequality, and insurgency (additional control variables included).

	(1)	(2)	(3)	(4)	(5)	(6)
Famine deaths per capita	0.114***	0.096***				
	(0.022)	(0.019)				
Crop failure			0.004*	0.004*		
			(0.002)	(0.002)		
Income Gini					0.047***	0.033***
					(0.011)	(0.010)
Conley SE	0.039	0.026	0.004	0.003	0.018	0.012
N	333	333	290	290	342	342
R^2	0.483	0.628	0.462	0.597	0.468	0.615
Outcome mean	0.018	0.018	0.018	0.018	0.018	0.018
Controls	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓

Notes: Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table C5. Regression results for change in inequality after the franchise reform (additional control variables included).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Famine deaths per capita	-0.228**	-0.190						
	(0.115)	(0.126)						
Crop failure			-0.017	-0.029**				
			(0.010)	(0.012)				
Income Gini					-0.842***	-0.850***		
					(0.058)	(0.061)		
Insurgency per capita							-0.778***	-0.771**
							(0.282)	(0.351)
Conley SE	0.101	0.116	0.010	0.011	0.058	0.057	0.299	0.391
N	330	330	289	289	339	339	339	339
R^2	0.085	0.106	0.068	0.160	0.635	0.642	0.067	0.111
Outcome mean	-0.249	-0.249	-0.254	-0.254	-0.251	-0.251	-0.251	-0.251

Notes: The dependent variable is change in Gini coefficient between the years 1904 and 1938. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table C6. Regression results for redistribution after the franchise reform (additional control variables included).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Land redistribution								
Famine deaths per capita	0.958***	0.612***						
	(0.194)	(0.205)						
Crop failure			0.059***	0.050**				
			(0.020)	(0.022)				
Income Gini					0.483***	0.338***		
					(0.099)	(0.101)		
Insurgency per capita							3.295***	2.632***
							(0.672)	(0.731)
Conley SE	0.235	0.222	0.017	0.017	0.096	0.078	0.404	0.509
N	325	325	285	285	334	334	334	334
R^2	0.456	0.517	0.476	0.547	0.454	0.526	0.478	0.538
Outcome mean	0.280	0.280	0.276	0.276	0.275	0.275	0.275	0.275
Panel B: Δ Municipal welfare spending 1912-1932								
Famine deaths per capita	0.518	0.868						
	(0.564)	(0.585)						
Crop failure			-0.058	-0.014				
			(0.052)	(0.056)				
Income Gini					0.442	0.376		
					(0.326)	(0.313)		
Insurgency per capita							0.716	-0.234
							(1.583)	(1.791)
Conley SE	0.526	0.451	0.051	0.051	0.417	0.360	1.781	1.799
N	324	324	285	285	333	333	333	333
R^2	0.154	0.207	0.155	0.225	0.175	0.239	0.168	0.234
Outcome mean	3.212	3.212	3.250	3.250	3.223	3.223	3.223	3.223
Panel C: Δ Municipal school spending 1912-1932								
Famine deaths per capita	1.214**	0.932						
	(0.561)	(0.623)						
Crop failure			0.048	0.060				
			(0.049)	(0.056)				
Income Gini					0.167	-0.064		
					(0.300)	(0.316)		
Insurgency per capita							1.032	0.721
							(1.258)	(1.393)
Conley SE	0.643	0.697	0.042	0.044	0.229	0.239	1.593	1.675
N	323	323	284	284	332	332	332	332
R^2	0.149	0.171	0.149	0.175	0.133	0.163	0.133	0.164
Outcome mean	1.849	1.849	1.838	1.838	1.845	1.845	1.845	1.845
Panel D: Δ Municipal health spending 1912-1932								
Famine deaths per capita	2.472**	2.063*						
	(1.019)	(1.149)						
Crop failure			0.122	0.117				
			(0.102)	(0.112)				
Income Gini					1.225**	1.159*		
					(0.617)	(0.678)		
Insurgency per capita							6.380**	8.197**
							(2.617)	(3.176)
Conley SE	0.985	1.174	0.126	0.133	0.633	0.627	2.167	2.539
N	324	324	286	286	333	333	333	333
R^2	0.088	0.107	0.108	0.135	0.083	0.102	0.083	0.107
Outcome mean	3.811	3.811	3.822	3.822	3.813	3.813	3.813	3.813
Controls	✓	✓	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓		✓

Notes: The dependent variables are indicated in the table. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

C.3 IV Results

Our main tables report OLS results measuring famine exposure with famine deaths and crop failures. As Table 2 in the main text indicates, famine deaths were correlated with various predetermined characteristics. We take this into account by always including control variables in our OLS regressions, but there may still be other, potentially unobservable confounders. If that was the case, our OLS estimates capture a conditional correlation between famine exposure and economic grievances. In order to deal with the endogeneity concerns and to quantify the impact of famine deaths on subsequent outcomes, we also estimate a 2SLS model and instrument the famine severity with rye crop failure in 1867. As we show in Table 2, whether a municipality experienced a crop failure or not in the year 1867 is orthogonal to various pre-famine characteristics once we condition on rye suitability.

The first-stage regression takes the form

$$Death\ share_m = \pi + \rho Crop\ failure_m + X_m'\vartheta + \mu_m.$$

Consider, for example, the first stage regression for income inequality in 1904, for which we have 320 observations. A regression with our baseline controls controls yields a coefficient $\rho = 0.039$ ($p < 0.001$, Kleibergen-Paap first-stage $F = 42.30$). Our first-stage regression coefficient is barely affected when we include county fixed effects: $\rho = 0.034$ ($p < 0.001$, Kleibergen-Paap first-stage $F = 26.58$). Therefore, $Crop\ failure_m$ is a relevant and strong instrument for $Death\ share_m$. The identifying assumption here is $Cov(Crop\ failure_m, \varepsilon_m \mid Rye\ suitability_m) = 0$. In other words, once we control for rye suitability, the crop failure shock is not correlated with pre-existing municipal characteristics. We consider the famine year deaths to serve as a proxy for the local severity of the famine shock, including economic and social hardship and not only the demographic change. With this in mind, the crop failure instrument should also fulfill the exclusion restriction, as it is unlikely that a crop failure shock would affect inequality or labor market coercion via other channels than the famine.

Tables C7-C11 show regression results corresponding to our main analyses, using the instrumental variables approach. The 2SLS estimates are in line with the corresponding OLS and reduced-form results that we report in the main text. Finally, note that even if the OLS captured the causal effect of the famine on later outcomes, the OLS and IV estimates measure different effects. While the IV esimates give us a local average treatment effect of famine severity (proxied by the population change) in localities that experienced a crop failure, the OLS estimation yields an average treatment effect. Many municipalities that did not experience a crop failure saw large drops in population, for example, due to contagious diseases killing people.

Table C7. Famine and inequality in the early 1900s: 2SLS estimates.

	Income Gini		Land Gini	
	(1)	(2)	(3)	(4)
Famine deaths per capita	0.464*	0.513*	0.545**	0.566**
	(0.253)	(0.309)	(0.222)	(0.263)
Conley SE	0.267	0.327	0.243	0.288
N	322	322	317	317
First stage *F*	42.296	26.578	41.951	24.988
R^2	0.199	0.248	0.125	0.186
Outcome mean	0.506	0.506	0.397	0.397
Controls	✓	✓	✓	✓
County FE		✓		✓

Notes: Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table C8. Famine and labor coercion in the early 1900s: 2SLS estimates.

	Tenant farms %		Terminated leases %		Daily wage	
	(1)	(2)	(3)	(4)	(5)	(6)
Famine deaths per capita	2.214***	1.767***	0.187	0.038	-5.567***	-2.510
	(0.598)	(0.627)	(0.165)	(0.190)	(1.960)	(2.031)
Conley SE	0.635	0.633	0.185	0.205	2.178	2.244
N	317	317	307	307	299	299
First stage *F*	41.951	24.988	41.119	23.935	33.263	20.646
R^2	0.402	0.578	0.249	0.317	0.191	0.331
Outcome mean	0.474	0.474	0.049	0.049	3.013	3.013
Controls	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓

Notes: Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table C9. Famine, and insurgency: 2SLS estimates.

	(1)	(2)
Famine deaths per capita	0.110**	0.123**
	(0.054)	(0.054)
Conley SE	0.069	0.060
N	320	320
First stage *F*	42.384	25.566
R^2	0.465	0.608
Outcome mean	0.018	0.018
Controls	✓	✓
County FE		✓

Notes: Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table C10. Regression results for change in inequality after the franchise reform: 2SLS estimates.

	(1)	(2)
Famine deaths per capita	-0.471**	-0.616**
	(0.229)	(0.308)
Conley SE	0.251	0.342
N	319	319
First stage *F*	42.335	25.586
R^2	0.037	0.039
Outcome mean	-0.252	-0.252
Controls	✓	✓
County FE		✓

Notes: The dependent variable is change in Gini coefficient between the years 1904 and 1938. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table C11. Regression results for redistribution after the franchise reform: 2SLS estimates.

	Land redistribution		Δ Welfare spending		Δ School spending		Δ Health spending	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Famine deaths per capita	1.679***	1.493**	-1.005	-0.269	1.080	1.721	5.234**	5.899*
	(0.494)	(0.631)	(1.309)	(1.576)	(1.176)	(1.536)	(2.512)	(3.147)
Conley SE	0.550	0.706	1.298	1.499	1.298	1.641	2.769	3.271
N	313	313	315	315	314	314	315	315
First stage *F*	40.119	23.998	43.030	26.488	41.324	24.570	39.784	24.116
R^2	0.426	0.474	0.025	0.120	0.118	0.132	0.053	0.057
Outcome mean	0.281	0.281	3.240	3.240	1.855	1.855	3.828	3.828
Controls	✓	✓	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓		✓

Notes: The dependent variables are indicated in the table. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

C.4 Results from Randomization Inference

Figure C1 demonstrates that our results for the crop failure variable are robust to a randomization inference approach. We conduct 1,000 permutations in which we randomly allocate municipalities to the control (no crop failure) and treatment (crop failure) groups. We then estimate the relationship between our outcome variables and the simulated crop failure dummy, and compute the share of placebo coefficients that are larger in magnitude than the absolute coefficient for the actual point estimate (our *p* value). There is a positive and statistically significant effect of the famine on income inequality (Panel A), land inequality (Panel B), the share of tenant farmers (Panel C), insurgency participation (Panel F), land reform (Panel H), and health care spending (Panel K). There is also a negative and statistically significant effect on change in inequality after the Civil War (Panel G).

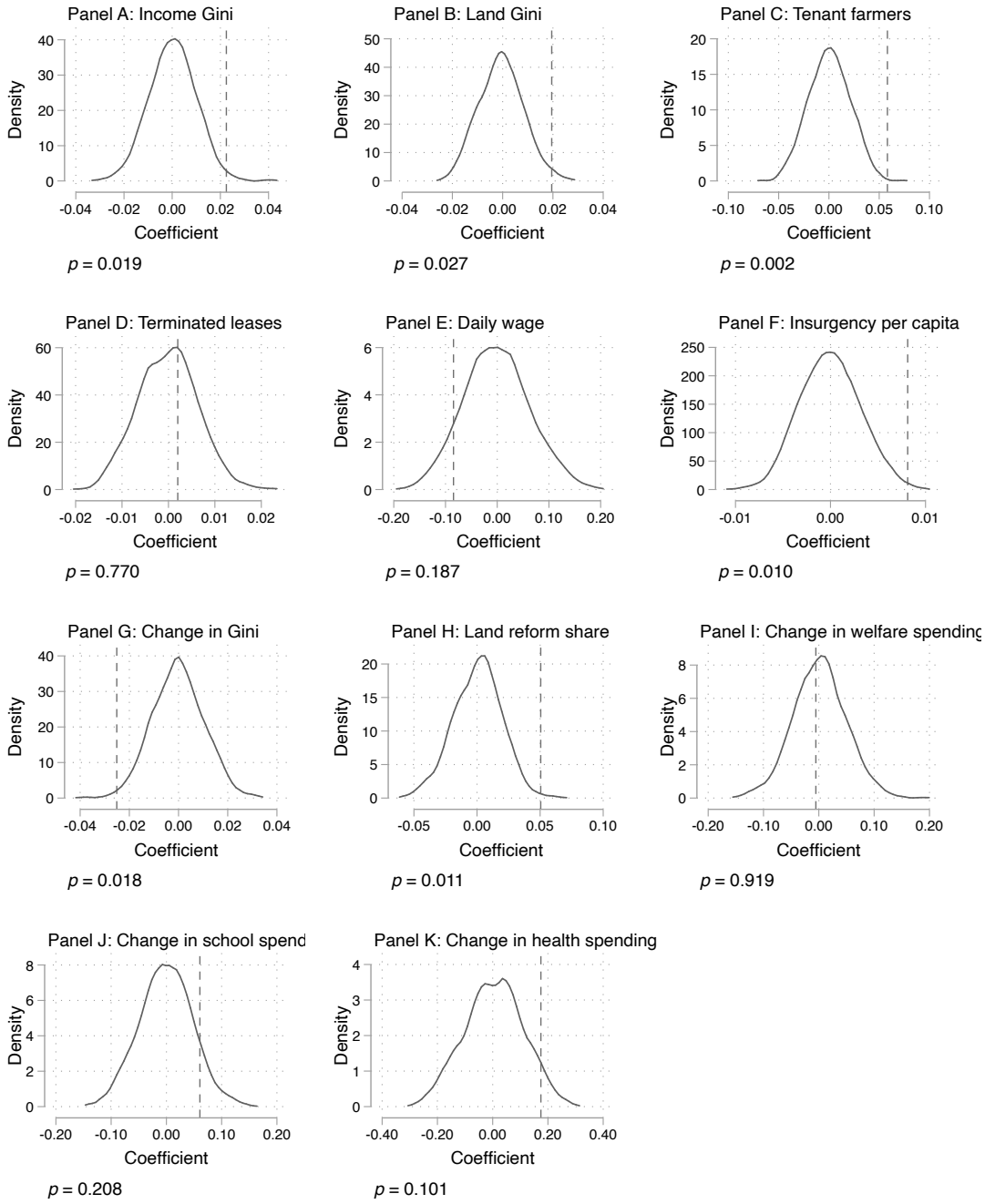

Figure C1. Results from a randomization inference approach.

Notes: The figures show the distribution of estimates from 1,000 permutations in which observations are randomly assigned to the treatment and control groups (crop failure vs. no crop failure). The dashed gray lines mark the estimated effect of experiencing a crop failure. The *p*-values give the share of 1,000 placebo coefficients that are larger in magnitude than the absolute coefficient for the actual point estimate. Estimations include the full set of controls.

C.5 Regression Results on Land Inequality, Coercion, and Insurgency

Table C12 complements the results in our main text (where we show that locations with more income inequality or a greater exposure to the famine had more insurgency) by showing that insurgency is also associated with land inequality and labor coercion. Column (2) suggests that a one-standard-deviation increase in land inequality is associated with a 0.003 higher insurgent casualty rate. Similarly, increasing the share of tenant farmers by one standard deviation is linked with a 0.006 higher insurgent casualty rate (column 4). The same number is 0.004 for the share of terminated leases (column 6). The regression coefficients for wages are negative, and the point estimate is only marginally significant when we include region fixed effects.

Table C12. Land inequality, labor coercion, and insurgency.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Land Gini	0.065***	0.042***						
	(0.010)	(0.009)						
Tenant farm share			0.021***	0.023***				
			(0.003)	(0.003)				
Terminated lease share					0.089***	0.067***		
					(0.017)	(0.014)		
Daily wage							-0.001	-0.002*
							(0.001)	(0.001)
Conley SE	0.013	0.012	0.006	0.005	0.022	0.014	0.002	0.001
N	414	414	414	414	401	401	393	393
R^2	0.442	0.613	0.426	0.628	0.457	0.614	0.371	0.588
Outcome mean	0.016	0.016	0.016	0.016	0.015	0.015	0.017	0.017
Controls	✓	✓	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓		✓

Notes: Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

D Additional Tables and Figures for the Discussion Section

D.1 Regression Results on Elections

In the discussion section of our main text, we show that the Social Democratic Party performed better in the Parliamentary elections in municipalities that either were more exposed to the famine or that had higher levels of economic inequality in the early 1900s. Table D1 shows that also labor coercion correlates with Social Democrats' electoral performance, all the reported point estimates being higly statistically significant. Let us again focus on the specifications with control variables. The regression coefficient for land Gini is 0.690 which means that a one-standard-deviation increase in land inequality is associated with almost 5% higher vote shares for the Social Democrats. The point estimate for the tenant farm share suggests that a shift of the same magnitude is linked with over 13% higher vote shares, while this figure is about 6% for the share of terminated leases. The support for Social Democrats was lower in locations where the wages were higher: an increase of one standard deviation in the daily wage is associated with about 4% less votes for the Social Democratic Party.

We have also used electoral data for a more extensive time period. We first use data on Social Democrats' vote shares over time to look at how it is associated with the famine, inequality, or insurgency. Figure D1 reports regression coefficients from an event-study specification. We set the pre-civil war year (1917) as the baseline. As before, all regressions control for municipality fixed effects, county-year fixed effects, and baseline controls interacted with year dummies. First, we see that there is no clear relationship between the famine and voting for the Social Democrats (Panels A and B). Interestingly, the point estimates for inequality (Panel C) and insurgency (Panel D) are negative. This relationship could be mechanical. When insurgents die—and more so in municipalities that were more unequal, as we show in the main text—there are simply less party supporters left.

We also assess whether any of the effects we observe could be attributed to increased voter turnout. Figure D2 reveals that this is not the case: the regression coefficients are virtually always indistinguishable from zero.

Table D1. Labor coercion and Social Democrats' electoral performance.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Land Gini	0.843***	0.690***						
	(0.120)	(0.112)						
Tenant farm share			0.606***	0.561***				
			(0.036)	(0.042)				
Terminated lease share					1.362***	1.077***		
					(0.154)	(0.145)		
Daily wage							-0.099***	-0.076***
							(0.019)	(0.018)
Conley SE	0.179	0.131	0.058	0.062	0.239	0.198	0.026	0.018
N	372	372	372	372	361	361	352	352
R^2	0.380	0.504	0.600	0.632	0.410	0.515	0.363	0.482
Outcome mean	0.412	0.412	0.412	0.412	0.405	0.405	0.416	0.416
Controls	✓	✓	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓		✓

Notes: Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

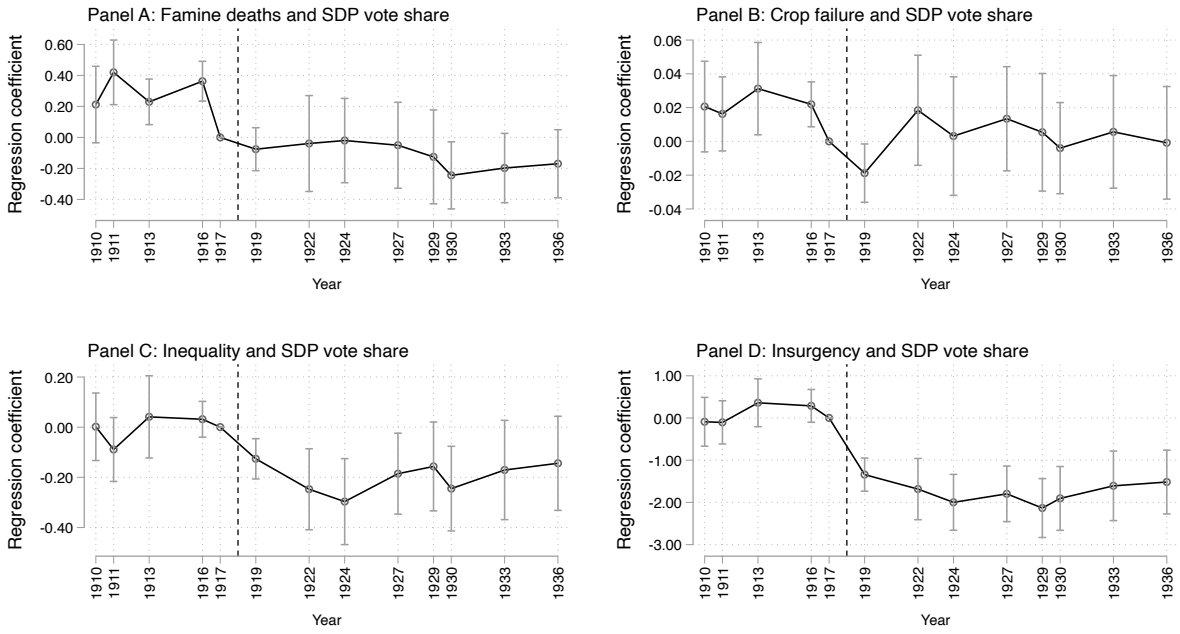

Figure D1. Event-study estimates: SDP vote share.

Notes: The dependent variable is the Social Democratic Party vote share. The figures plot the raw differences from an event-study specification and their 95% confidence intervals that are constructed using standard errors clustered at the municipality level. We use 1917 as the base year. The vertical line marks the civil war year. All regressions control for municipality fixed effects, county-year fixed effects, and baseline controls interacted with year dummies.

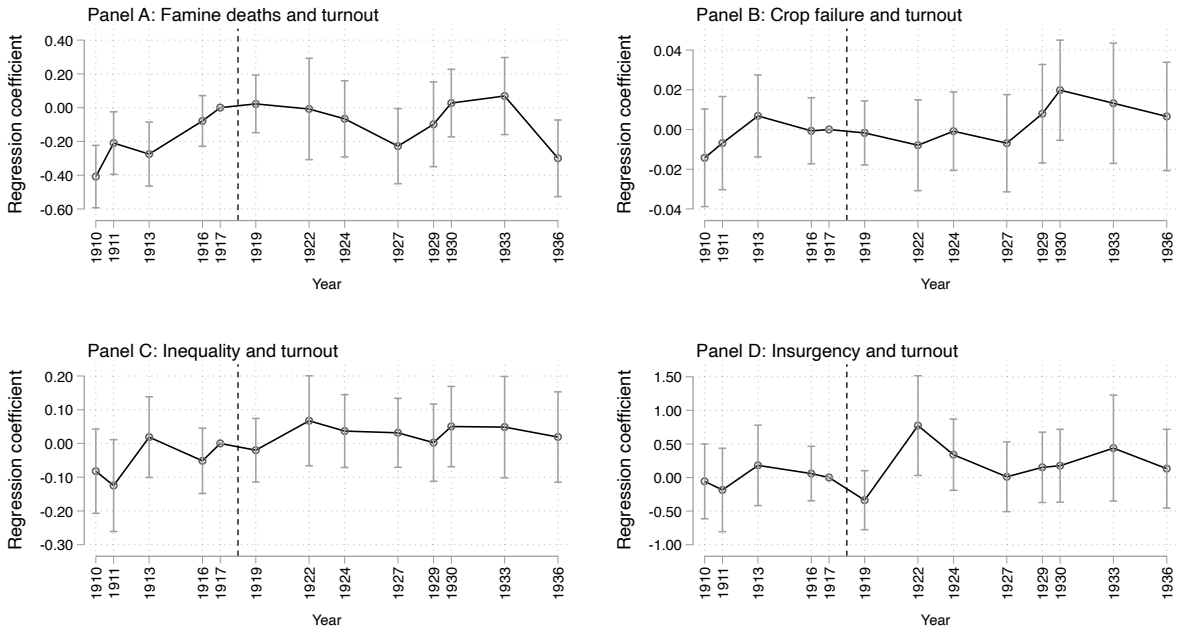

Figure D2. Event-study estimates: voter turnout.

Notes: The dependnet variable is voter turnout. The figures plot the raw differences from an event-study specification and their 95% confidence intervals that are constructed using standard errors clustered at the municipality level. We use 1917 as the base year. The vertical line marks the civil war year. All regressions control for municipality fixed effects, county-year fixed effects, and baseline controls interacted with year dummies.

D.2 Regression Results on Workers' Associations

We complement our analysis on workers' associations and their membership rates by showing that workers' association membership is positively associated with land inequality and labor coercion (Table D2). However, one exception to this is daily wage (columns 7 and 8) which does not seem to be correlated with the membership rate. Note also that some of the independent variables are measured after the dependent variable. Tables D3 and D4 show that we obtain qualitatively similar results if we look at the presence of local workers' associations.

Table D2. Land inequality, labor coercion, and workers' association membership in 1906.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Land Gini	0.267***	0.242***						
	(0.046)	(0.047)						
Tenant farm share			0.070***	0.086***				
			(0.015)	(0.019)				
Terminated lease share					0.272***	0.251***		
					(0.065)	(0.072)		
Daily wage							0.000	-0.004
							(0.006)	(0.006)
Conley SE	0.056	0.050	0.021	0.025	0.087	0.091	0.008	0.005
N	410	410	410	410	397	397	390	390
R^2	0.250	0.277	0.206	0.258	0.213	0.241	0.156	0.209
Outcome mean	0.055	0.055	0.055	0.055	0.053	0.053	0.057	0.057
Baseline controls	✓	✓	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓		✓

Notes: The dependent variable is the population share of workers' association members. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table D3. Famine, inequality, and workers' associations in 1906.

	(1)	(2)	(3)	(4)	(5)	(6)
Famine deaths per capita	1.260***	0.874*				
	(0.443)	(0.448)				
Crop failure			0.043	0.025		
			(0.045)	(0.045)		
Income Gini					1.152***	0.999***
					(0.214)	(0.215)
Conley SE	0.599	0.533	0.047	0.045	0.255	0.264
N	404	404	324	324	415	415
R^2	0.301	0.331	0.222	0.256	0.330	0.353
Outcome mean	0.738	0.738	0.815	0.815	0.740	0.740

Notes: The dependent variable is an indicator for a municipality having a local workers' association. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table D4. Land inequality, labor coercion, and workers' associations in 1906.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Land Gini	1.044***	1.008***						
	(0.227)	(0.225)						
Tenant farm share			0.560***	0.596***				
			(0.098)	(0.104)				
Terminated lease share					1.000***	0.848***		
					(0.273)	(0.264)		
Daily wage							-0.099***	-0.098***
							(0.035)	(0.036)
Conley SE	0.259	0.221	0.137	0.146	0.279	0.231	0.045	0.045
N	410	410	410	410	397	397	390	390
R^2	0.326	0.357	0.358	0.381	0.314	0.340	0.309	0.342
Outcome mean	0.737	0.737	0.737	0.737	0.730	0.730	0.754	0.754
Baseline controls	✓	✓	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓		✓

Notes: The dependent variable is an indicator for a municipality having a local workers' association Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

D.3 Regression Results on Strikes

In this section, we complement our analyses on the relationship between the famine, inequality, labor coercion, and strikes. We first look at the correlation between land inequality and labor coercion, and agricultural workers' strikes in Table D5. We see that land inequality and labor coercion correlate with strike activity as we would expect, although the regression results are mostly not statistically significant. Table D6 then shows the estimation results using the occurrence of agricultural workers' strikes as a dependent variable. The regression results do not suggest that there was a clear relationship between the famine and strike activity in 1917. However, there appears to be a positive correlation between economic inequality and strikes. The results on land inequality and labor coercion, and agricultural workers' strikes are reported in Table D6, and they are largely in line with our other results.

We have also reproced all the analyses using data on all strikes that occurred in 1917. First, Table D8 presents results that correspond to Panel C in Table 8. Second, we examine the correlation between strike days per capita and land inequality and labor coercion in Table D9. We see that there were more strike days per capita in locations that had higher land inequality, that had more tenant farmers, where more leases were terminated, or where wages were lower. While many of the regression coefficients reported in the table are not statistically significant, they always have the expected sign. Third, Tables D10 and D11 then look at an alternative outcome: the mere occurrence of a strike in a given municipality. Table D10 suggests that there is no clear relationship between the famine and strikes happening during the year 1917. However, columns (5) and (6) show a clear positive relationship between strike occurrence and economic inequality. This notion is mostly strengthened by the results that we report in Table D11. Strikes were more likely to happen in locations with a higher land inequality, more tenant famers, and more terminated tenant famer leases. However, the regression results for daily wages go to the opposite direction than what we would have expected.

Table D5. Land inequality, labor coercion, and agricultural workers' strike days per capita in 1917.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Land Gini	0.889	1.171						
	(0.628)	(1.125)						
Tenant farm share			0.754**	1.148*				
			(0.359)	(0.662)				
Terminated lease share					0.065	0.574		
					(0.800)	(0.429)		
Daily wage							-0.140	-0.144
							(0.155)	(0.139)
Conley SE	0.631	1.098	0.314	0.513	0.792	0.346	0.140	0.126
N	410	410	410	410	397	397	390	390
R^2	0.013	0.029	0.022	0.043	0.010	0.026	0.014	0.027
Outcome mean	0.160	0.160	0.160	0.160	0.141	0.141	0.168	0.168
Baseline controls	✓	✓	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓		✓

Notes: The dependent variable is agricultural workers' strike days per capita. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table D6. Famine, inequality, and agricultural workers' strikes in 1917.

	(1)	(2)	(3)	(4)	(5)	(6)
Famine deaths per capita	0.477	0.210				
	(0.360)	(0.383)				
Crop failure			-0.001	0.001		
			(0.039)	(0.042)		
Income Gini					0.436**	0.308*
					(0.175)	(0.176)
Conley SE	0.325	0.342	0.052	0.052	0.160	0.166
N	404	404	324	324	415	415
R^2	0.074	0.117	0.066	0.098	0.083	0.123
Outcome mean	0.114	0.114	0.117	0.117	0.111	0.111
Baseline controls	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓

Notes: The dependent variable is an indicator for a local agricultural workers' strike. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table D7. Land inequality, labor coercion, and agricultural workers' strike days per capita in 1917.

	(1)	(2)	(3)	(4)	(5)	(6)
Famine deaths per capita	0.268	-0.009				
	(0.331)	(0.361)				
Crop failure			-0.026	-0.027		
			(0.036)	(0.039)		
Income Gini					0.381**	0.251
					(0.170)	(0.168)
Conley SE	0.296	0.381	0.042	0.040	0.173	0.179
N	409	409	328	328	420	420
R^2	0.065	0.106	0.053	0.083	0.075	0.112
Outcome mean	0.104	0.104	0.104	0.104	0.104	0.104
Baseline controls	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓

Notes: The dependent variable is an indicator for a local strike. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table D8. Famine, inequality, and strike days per capita in 1917.

	(1)	(2)	(3)	(4)	(5)	(6)
Famine deaths per capita	2.160*	2.169*				
	(1.212)	(1.297)				
Crop failure			0.218	0.228		
			(0.255)	(0.224)		
Income Gini					1.144***	1.330**
					(0.442)	(0.661)
Conley SE	0.720	1.040	0.243	0.203	0.420	0.596
N	409	409	328	328	420	420
R^2	0.020	0.033	0.015	0.032	0.016	0.032
Outcome mean	0.242	0.242	0.274	0.274	0.254	0.254
Baseline controls	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓

Notes: The dependent variable is strike days per capita. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table D9. Land inequality, labor coercion, and strike days per capita in 1917.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Land Gini	1.276**	1.571						
	(0.642)	(1.134)						
Tenant farm share			0.600	1.120*				
			(0.370)	(0.668)				
Terminated lease share					0.345	0.971*		
					(0.904)	(0.550)		
Daily wage							-0.099	-0.140
							(0.158)	(0.142)
Conley SE	0.667	1.138	0.323	0.522	0.987	0.504	0.145	0.130
N	414	414	414	414	401	401	393	393
R^2	0.016	0.032	0.019	0.042	0.012	0.029	0.013	0.028
Outcome mean	0.256	0.256	0.256	0.256	0.239	0.239	0.269	0.269
Baseline controls	✓	✓	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓		✓

Notes: The dependent variable is strike days per capita. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table D10. Famine, inequality, and strikes in 1917.

	(1)	(2)	(3)	(4)	(5)	(6)
Famine deaths per capita	1.046**	0.500				
	(0.505)	(0.519)				
Crop failure			-0.006	-0.021		
			(0.059)	(0.059)		
Income Gini					1.044***	0.858***
					(0.260)	(0.277)
Conley SE	0.397	0.326	0.074	0.063	0.227	0.220
First stage F	409	409	328	328	420	420
N	0.094	0.181	0.079	0.182	0.120	0.202
R^2	0.298	0.298	0.323	0.323	0.300	0.300
Outcome mean	✓	✓	✓	✓	✓	✓
Baseline controls		✓		✓		✓

Notes: The dependent variable is an indicator for a local strike. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table D11. Land inequality, labor coercion, and strikes in 1917.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Land Gini	0.997***	0.606**						
	(0.291)	(0.307)						
Tenant farm share			0.127	0.112				
			(0.109)	(0.123)				
Terminated lease share					1.213***	0.922**		
					(0.429)	(0.428)		
Daily wage							0.107**	0.090**
							(0.043)	(0.045)
Conley SE	0.335	0.331	0.128	0.144	0.422	0.442	0.041	0.037
N	414	414	414	414	401	401	393	393
R^2	0.113	0.197	0.090	0.190	0.116	0.205	0.095	0.190
Outcome mean	0.295	0.295	0.295	0.295	0.289	0.289	0.310	0.310
Baseline controls	✓	✓	✓	✓	✓	✓	✓	✓
County FE		✓		✓		✓		✓

Notes: The dependent variable is an indicator for a local strike. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

D.4 Regression Results on Famine and Emigration

While weather shocks and famines in other parts of Europe catalyzed emigration, we do not find any evidence that more people would have emigrated from Finnish municipalities that were more exposed to the famine (Table D12). To investigate this possibility, we use annual emigration statistics to measure the number of emigrants from each municipality between the years 1893 and 1914. While the regression coefficients are positive throughout the table, they are small in magnitude. The regression coefficients only show hints of statistical significance when we include county fixed effects in column 2. The coefficients of the crop failure indicator are not statistically significant. Thus, we find it unlikely that the famine would have induced a massive flow of emigration which in turn could have induced an increase in workers' bargaining power.

Table D12. Famine and emigration.

	(1)	(2)	(3)	(4)
Famine deaths per capita	0.064	0.124**		
	(0.073)	(0.060)		
Crop failure			0.006	0.005
			(0.009)	(0.007)
Conley SE	0.111	0.072	0.010	0.008
N	401	401	323	323
R^2	0.576	0.732	0.598	0.725
Outcome mean	0.089	0.089	0.080	0.080
Controls	✓	✓	✓	✓
County FE		✓		✓

Notes: The dependent variable is the emigration rate. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

D.5 Figures and Tables on Industrialization

To demonstrate that Finland was largely an agrarian society still in the early 1900s, Figure D3 shows the share of workers across main sectors and the share of value added by sector over time. It demonstrates two key features of the Finnish economy. Almost 70 percent of households were employed in agriculture, and almost half of the value added was genereted in the agricultural sector. The rise in manufacturing was modest before the Civil War. Thus, if industrialization was the driver of the income inequality, it should have had a massive impact on the economy very early on in the industrialization process.

We also do not find any evidence that the famine would have been associated with industrialization in Finland. Table D13 looks at the share of population employed in manufacturing after the famine, and Table D14 examines manufacturing growth between the years 1880 and 1910. The regression coefficients are small and statistically insignificant throughout the table. Thus, it is unlikely that industrialization would be driving our results in the way that in Kuznets argued. This could have been the case, for instance, if the famine would have stimulated migration of cheap rural labor force to cities.

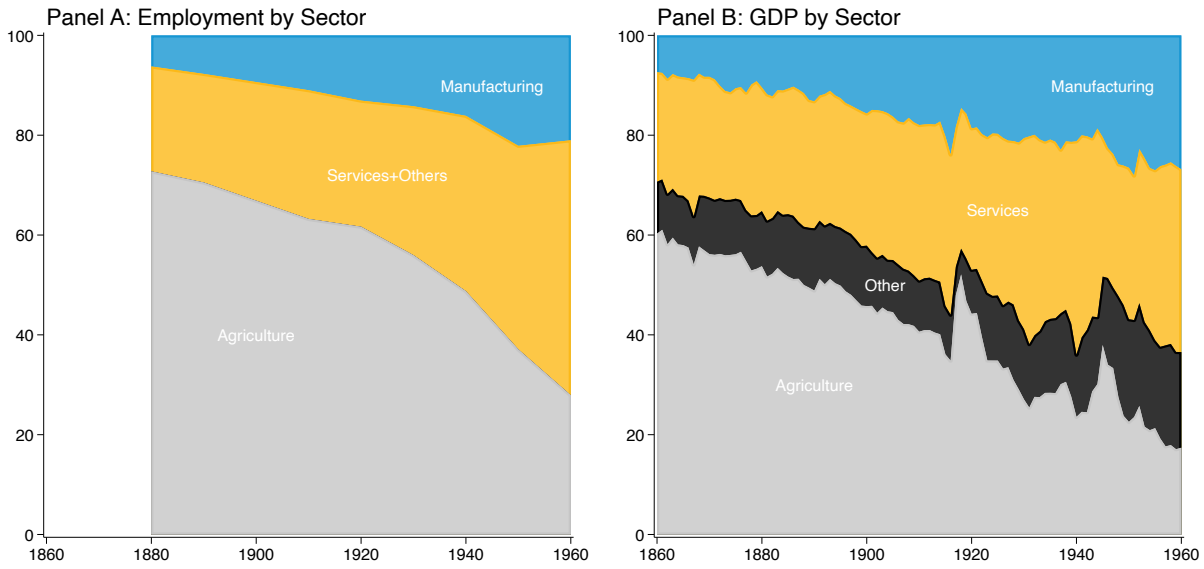

Figure D3. Structure of the Finnish economy, 1860-1960.

Table D13. Famine and manufacturing employment in 1880.

	(1)	(2)	(3)	(4)
Famine deaths per capita	-0.045	-0.056		
	(0.044)	(0.046)		
Crop failure			0.000	-0.000
			(0.005)	(0.005)
Conley SE	0.055	0.048	0.006	0.005
N	326	326	275	275
R^2	0.158	0.194	0.176	0.219
Outcome mean	0.041	0.041	0.042	0.042
Controls	✓	✓	✓	✓
County FE		✓		✓

Notes: The dependent variable is the share of population employed in manufacturing. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.

Table D14. Famine and manufacturing growth, 1880-1910.

	(1)	(2)	(3)	(4)
Famine deaths per capita	-0.063	0.012		
	(0.075)	(0.083)		
Crop failure			-0.009	-0.002
			(0.008)	(0.008)
Conley SE	0.087	0.100	0.008	0.008
N	326	326	275	275
R^2	0.023	0.081	0.026	0.101
Outcome mean	0.026	0.026	0.026	0.026
Controls	✓	✓	✓	✓
County FE		✓		✓

Notes: The dependent variable is the change in the share of population employed in manufacturing between the years 1880-1910. Robust standard errors are reported in parentheses. Conley standard errors allow for spatial autocorrelation among municipalities that are within 50 kilometers of each other. ***, ** and * denote statistical significance at 1%, 5% and 10% level, respectively.