Insuring Against Hunger? The Long-Term Political Consequences of Exposure to the Dutch Famine

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ABSTRACT

Does experiencing a shock alter one's voting behavior? We explore how a specific shock to individuals' health and human capital accumulation — in-utero malnutrition — prompted by the sudden onset of the 1944/45 Dutch Famine affected insurance demand and voting behavior later in life. Given similar socialization patterns, we find conglomerations of affected individuals to be associated with higher support for Left parties more than 50 years after the exposure. Relying on rich administrative data and leveraging the Dutch Famine as a natural experiment, this paper represents an initial effort to investigate and confirm the long-term effects of shocks on political behavior.

> "Hunger makes raw beans taste sweet." (Dutch Proverb)

Introduction

It is by now widely recognized that prenatal malnutrition or hunger in early life has long-lasting effects on an individual's health and labor market outcomes later in life. Affecting more than 900 million individuals around the world, undernutrition is particularly relevant in developing countries (Behrman, 1993; Behrman *et al.*, 2004; Currie and Vogl, 2013; Fernald *et al.*, 2008; Field *et al.*, 2009; Martins *et al.*, 2011). Yet, even in developed countries, children from disadvantaged backgrounds may face malnutrition or even hunger spells throughout their lives and benefit from Food Stamps Programmes (Hoynes and Schanzenbach, 2009), Head Start (Garces *et al.*, 2002; Walters, 2015) or the Special Supplemental Nutrition Programme for Woman, Infants, and Children (Black *et al.*, 2004).

Although much work has been done to understand how malnutrition affects individuals' health, little is known about its impact on voter behavior, especially in terms of electoral participation, insurance demand, and voting preferences. The possibility that in-utero malnutrition has long-term political consequences seems justified given the persistent effects of malnutrition throughout an individual's life — the fetal origins hypothesis (Barker, 1990). This biological channel affects health and non-health outcomes, lowering also an individual's human capital stock. The existing literature in political economy identifies such voters as being more likely to support redistribution and demand social insurance to protect themselves against future income losses (Alesina and La Ferrara, 2005; Moene and Wallerstein, 2001; Rehm, 2009; Rueda and Stegmueller, 2019). Yet, voter behavior in adulthood is endogenous to the accumulation of human and health capital, which we propose is closely related to one's prenatal environment.

Since malnutrition is unlikely to be randomly distributed across individuals or countries, estimating its impact on individuals' political preferences and behavior poses a major empirical challenge. Unpredicted and severe, we therefore consider the Dutch Famine — which left more than 4.5 million people malnourished during the 1944/45 winter — a suitable case study to explore these questions. Compared to other famines (like those in Greece or China), studying the Dutch Famine is advantageous for a number of methodological reasons. As Scholte *et al.* (2015) note, the Dutch Famine is clearly located temporally and spatially, making it easier to distinguish between exposure in-utero and after birth. Additionally, quality administrative data allows us to identify respondents' month and place of birth, something not always possible in other famine-based studies. Finally, the analysis of the Dutch Famine is less susceptible to confounders or cohort effects as it may be the case with the Chinese Famine which was followed by the Cultural Revolution. By comparison, the end of the Dutch Famine coincided with the German surrender (Evans, 2009) and was in close proximity to the end of the war.

Consistent with expectations from insurance models of redistribution, we find that in municipalities where there are more individuals exposed to the famine prenatally, there is a greater demand in adulthood for higher social protection, captured through the support for Left wing parties. We find significant biological effects that add to the potential socialization effects of the famine, and our results are robust to a variety of data sources and alternative specifications of exposure to this health shock. We also find tentative support for our proposed mechanism that exposure to famine informs voting behavior through its impact on risk exposure and possibly the degree of risk aversion among those affected, and rule out potential alternative mechanisms related to geographic sorting, conception timing, or spending type. Taken together, the analysis provided in this paper supports the argument that in-utero exposure to malnutrition is a significant contributor to present-day political differences in communities of treated individuals.

Famine, Insurance, Redistribution, and Voting

Famines cast a long shadow. Whilst severe food deprivation negatively impacts any individual, irrespective of age, poor nutrition at a young age, and especially prenatally, is linked to a series of adverse effects later-in-life. In this paper we conceive of exposure to famine as risk, since it constitutes a shock to individuals' health and human capital that increases the probability of hospitalization, ill health, or unemployment.[1] Exposure to shocks can also impact individuals' risk aversion. We posit that, together, the probability of experiencing a bad event and an individual's tolerance to that risk are the foundation of a relationship between in-utero exposure to famine and voting behaviour later in life.

The rest of our theoretical argument proceeds as follows. First, we explain why the shock could lead to different levels of risk among treated individuals. Second, we link this variation in risk to vote choice.

Variation in Risk

We begin by addressing the connection between famine and risk, which constitutes the starting point of the causal chain we propose in this paper. We distinguish two ways in which variation in risk occurs, one as a direct result of exposure to the shock on individual characteristics, and an indirect one as a result of socialization.

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[1]Note that by risk we do not mean either complete uncertainty or completely known risk. We mean risk in the sense that individuals can assign a high or low probability of ill health or unemployment, even if one is not knowledgeable of the specific value of that probability (Baderin and Barnes, 2020).

An abundant literature documents that individuals exposed to famine in-utero are more likely to be overweight (Barker, 1990; Conti *et al.*, 2019; Ravelli *et al.*, 1976), shorter (Portrait *et al.*, 2017; Van den Berg *et al.*, 2015), have a lower life expectancy (Lindeboom *et al.*, 2010) and suffer from coronary heart disease, disturbed blood coagulation, and diabetes (Barker, 1990; Roseboom *et al.*, 2006), as well as mental health problems (Brown *et al.*, 1995; Neugebauer *et al.*, 1999; Susser *et al.*, 1998).[2] Lower levels of health capital not only intensify the risk of ill health in adulthood, but also affect employment and labor market outcomes. Exposure to famine has been shown to impact an individual's ability to work by lowering human capital, by decreasing returns to education or increasing the cost of school attendance, both of which are factors that affect wages and labor supply later-in-life (Meng and Qian, 2009). Economists document lower educational attainment and occupational status (Jürges, 2013; Neelsen and Stratmann, 2011), lower income and wealth (Hamoudi and Sachs, 1999), and a higher likelihood of unemployment and reduced work hours (Chen and Zhou, 2007; Scholte *et al.*, 2015) among those exposed to famine early in life. In brief, this suggests a *direct* effect of famine exposure on individuals' probability of ill health or unemployment.

Apart from increased risk of unemployment or ill health, famine exposure could also impact individuals' tolerance to risk, commonly denoted as risk aversion. The degree of risk aversion may be influenced by famines in at least two ways. First, exposure to the famine may *directly* impact risk aversion by altering the development of such preferences (Paulsen *et al.*, 2012; Zhang *et al.*, 2014). This suggests differences in risk attitudes could originate in prenatal exposure to shocks and already vary at birth. Evidence of a similar long-term direct effect on risk aversion is found among children whose risk aversion increases if exposed to war (Kim and Lee, 2014).[3]

Second, variation in risk aversion could *indirectly* result from exposure to the famine through socialization. In this paper, by socialization we mean the process by which children may acquire political orientations by social learning in the family and other contexts.[4] While most analysts would agree that the transmission of political values through socialization matters, there is no consensus in the literature about a number of important questions (like the age at which socialization is most relevant or the importance of socio-economic childhood experiences in the process of socialization). Our approach

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[2]For a review on the long-term health consequences, see Conti *et al.* (2020).

[3]Individuals are also found to be more risk averse if they experience job loss (Hetschko and Preuss, 2020) or a health shock (Decker and Schmitz, 2016).

[4]See, for example, Gidengil *et al.* (2016) for an argument about socialization effects on turnout.

is most similar to recent efforts to identify the effects of material deprivation in childhood on political socialization.[5]

Our main goal is to separate the potential effects of socialization from those of in-utero health. Growing evidence suggests that children's risk aversion is associated strongly with parents' risk aversion and emphasizes the importance of upbringing and the intergenerational transmission of values in understanding the origins of these preferences (Albanese *et al.*, 2016; Hryshko *et al.*, 2011).[6] This socialization mechanism would be consistent with the famine fundamentally affecting the treated parents' risk preferences in response to this shock, and, in turn, altering the type of values they pass on to their children. While a significant literature on the historical legacies of traumatic events has emphasized the importance of the intergenerational transmission of values (Charnysh and Finkel, 2017; Dinas *et al.*, 2019; Homola *et al.*, 2020; Rozenas and Zhukov, 2019), we propose to distinguish between biological and sociological effects. By comparing cohorts affected by famine in-utero to those born immediately before and after in the same locations, we explore directly the effects of the social and biological factors associated to historical traumas. We should emphasize that our approach only allows us to estimate the aggregation of these two effects. As we explain in more detail below, our results add the effect of in-utero exposure to the socialization effect at various ages less prone to be biologically affected by malnutrition. In essence we assume socialization to be similar around the specific in-utero timing and estimate the additional biological effect over the socialization one. We show that the significance of the effect dissipates for cohorts born increasingly earlier or later than the famine.

Vote Choice

An extensive literature documents that economic interests, in particular in response to shocks, affect an individual's vote choice. For example, individuals exposed to unemployment increase their demand for insurance (Iversen and Soskice, 2001; Mares, 2003; Rehm, 2009), which is associated with support for redistributive parties (Rueda and Stegmueller, 2019). These approaches emphasize an insurance logic in which individuals want to insure against uncertain future income levels and favor social protection when they are exposed to job or wage loss. While this literature tends to focus on labor market risk, we argue this logic generalizes to other classes of risk, and therefore

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[5]See particularly Jungkunz and Marx (2023), who focus on the long-term consequences of unequal socialization patterns and find that material conditions in youth and childhood contribute to rather stable trajectories of political involvement.

[6]Several studies have documented that historically traumatic events have effects that persist over time and are transmitted from parents to children. See, for example, Balcells (2012) on the Spanish Civil War and Franco's dictatorship or Lupu and Peisakhin (2017) on the deportation of Crimean Tatars in 1944.

expect a positive relationship between health risk and support for the welfare state and Left parties. Indeed, Rapeli *et al.* (2020) and Pacheco and Fletcher (2015) show that individuals exposed to ill-health could be more likely to identify with or vote for the Left. Apart from the risk of unemployment or ill health, support for social spending and the Left is documented to increase with an individual's risk aversion (Gärtner *et al.*, 2017; Pahontu, 2020).

While the connection between redistributive preferences and voting for redistributive parties may seem common place, our analysis represents an effort to identify this relationship more convincingly. Most of the literature proposing this relationship relies on labor market markers that are potentially endogenous to preferences (individuals selecting occupations or skills, for example, that reflect their risk aversion). Our emphasis on in-utero famine allows us to sidestep questions about the endogeneity of treatment to pre-existing insurance preferences.

The Dutch Famine

> "The hunger was so great, that the central kitchen very often served food approved for human consumption, but which animals would refuse"
>
> — (Van Der Zee, 1998, p. 70)

We focus on the Dutch Famine, a large and exogenous shock affecting close to 4.5 million people in the West of the Netherlands in the final months of the Second World War. Although the Netherlands was under German occupation since May 1940, the supply of food and nutritional intake were generally adequate during most of the war (Dols and Van Arcken, 1946; Lumey *et al.*, 2007). It was not until the Allied forces lost the Battle of Arnhem in September 1944, after liberating Southern Netherlands, and the exiled Dutch government called out a national railroad strike in response that the food supply of the Western part of the country (i.e., the provinces North Holland, South Holland, and Utrecht as shown in Figure 1) came under threat. The Allied forces subsequently halted their liberation efforts in the Netherlands. But food could no longer be transported by train from the agricultural North and East to the urbanized West due to the strike (which would last until May 1945) and to the fact that the German occupiers had retaliated by blocking all transport via waterways. The situation became increasingly dire for the urban population in the West. Even though the German occupiers lifted the shipping embargo in November 1944, the combination of the railroad strike, an extremely harsh winter which resulted in frozen rivers, and fuel shortages led to starvation in Western Netherlands (Stein *et al.*, 1975).
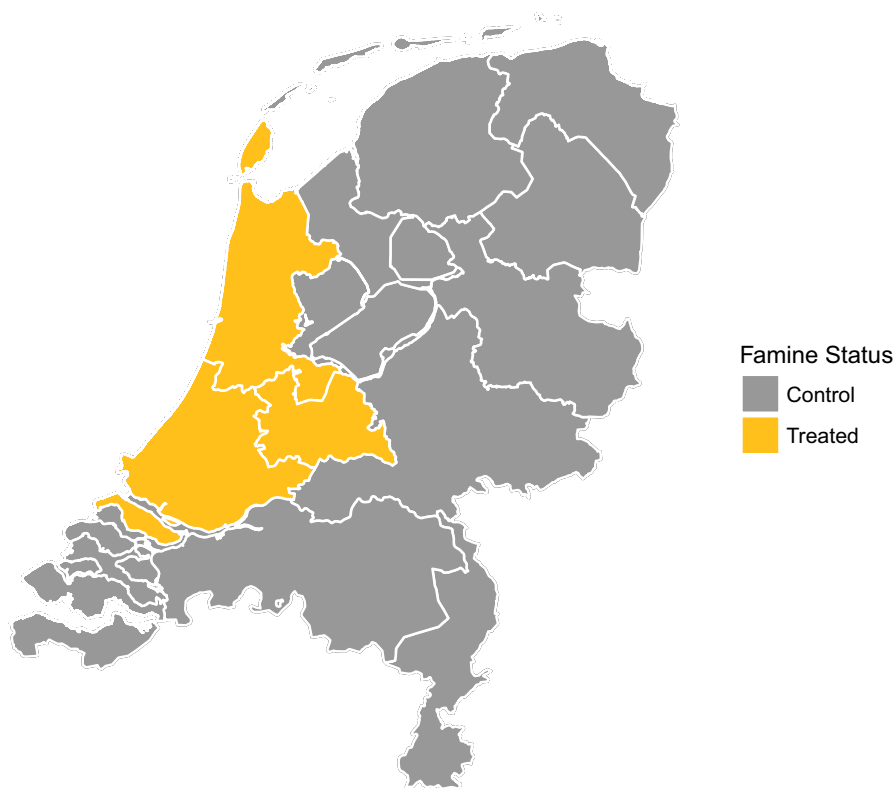
Figure 1: Regions affected by the Dutch winter hunger.

During the 'Hunger Winter,' as the Dutch Famine is also referred to, people tried to secure food in different ways. As before, they received food from centralized, government-operated food distribution systems. By November 1944, however, official rations had fallen below 1,000 dietary calories per day and by April 1945 they were down to 500 calories per day (Almond and Currie, 2011) — a dramatic decrease considering adults need about 2,000 calories a day. "It is just too much to die on, but certainly too little to keep you alive" (Van Der Zee, 1998). To complement the declining rations, people relied on different coping strategies. Detailed historical research shows that the wealthy were more likely to buy food on the urban black market, though their wealth may not have cushioned them for long given soaring prices and the price discrimination employed by sellers. Others, mostly from the working and lower-middle classes, relied more on their social networks, engaged in illegal activities to obtain food, and were more likely to undertake dangerous

and physically demanding food expeditions to rural areas, where prices were more within their reach (De Zwarte, 2020; de Zwarte, 2018; Roseboom *et al.*, 2011). All segments of the population were thus affected by the famine, though to varying degrees. Importantly, data from military induction records show that families from higher social classes were more likely to conceive during the famine than families from lower social classes (Stein *et al.*, 1975). If we find that those affected by in-utero malnutrition are more likely to vote for left-wing parties, the results could therefore be interpreted as a lower bound.

In addition to the government food rationing system, civil society played an important role in mitigating the exposure to the famine. In December 1944, a collaboration of churches, the Interdenominational Bureau for Emergency Nutrition (IKB), became the only organization officially allowed to provide food support alongside the rationing system. This organization distributed food in all affected areas based on medical need, and not religious denomination. Importantly, they focused on school-aged children (and later infants and toddlers) and provided them additional meals in a community setting to prevent this food from being redistributed within the household. While they also served adults in the last months of the famine, the eligibility criteria were very strict and even pregnant women would only receive extra food if they were more than 30% underweight (de Zwarte, 2018). Nevertheless, the large and formerly well-fed population in Western Netherlands suffered from acute under-nutrition and an estimated 25,000 people died from hunger (Zwarte, 2019).

One concern emerging from the empirical strategy developed in this paper could be that pregnant women in urban areas in the West may not have been exposed to the famine. This could have been the case if they moved away from these urban areas on a large scale or if they had access to more food than assumed despite the famine. The former seems unlikely given the German occupation and the railroad strike which further limited inter-regional mobility (Centraal Bureau voor de Statistiek (CBS), 1947, p. 14).[7] Although we cannot observe private food distribution within households, there is also little evidence that pregnant women were not affected by the famine. While they were officially entitled to higher food rations from the state, these could not be provided at the height of the famine (Roseboom *et al.*, 2011). Feeding initiatives by societal organizations also did not target pregnant women and their unborn children. In fact, "this choice seemed based on the incorrect assumption that prenatal care was not as vital as infant relief, resulting in impairments in adult health for these as-of-yet-unborn babies" (De Zwarte, 2020, p. 214). The limited ability of pregnant women to avoid hunger also

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[7]The main exception were the evacuations of roughly 40,000 severely malnourished children (or 9% of the urban child population), who, with the help of churches and societal organizations, were sent to the rural North or East to improve their health (de Zwarte, 2018; Zwarte, 2019, p. 302).

comes out of hospital records from prenatal appointments showing that most pregnant women were several kilograms thinner by the end of their pregnancy compared to the start of it.

Another concern that could be raised is the comparability of individuals who were exposed before birth to the famine in Western Netherlands and those individuals born in the same period who were unaffected by the famine because they lived in Northern, Eastern, or Southern Netherlands. Although we follow previous studies by limiting the comparison to individuals born in urban areas (see, for example, Scholte *et al.*, 2015; Stein *et al.*, 1975), the urban areas in the densely populated West of the country are likely to be different from the urban areas in the more sparsely populated rest of the country. They are, for example, more populous as the four largest cities in the country (Amsterdam, Rotterdam, The Hague, and Utrecht) are all located in the West and their access to agricultural land is more restricted than in non-Western cities. Further on in this paper, we will therefore also leverage comparisons of different cohorts born in urban areas in Western Netherlands (i.e., individuals who shared the same regional context but differed in their exposure to the famine before birth).

Data and Measurement

The proposed causal chain links exposure to famine to variation in risk and then vote choice. To test our theoretical claims, then, we would ideally use individual-level data connecting famine exposure to political preferences or voting behavior. Unfortunately, such data are not available. Existing survey data, such as the Dutch Parliamentary Election Studies (DPES) or the Longitudinal Internet studies for the Social Sciences (LISS), would result in very small sample sizes once we limit our focus to respondents born during the Dutch Famine. Moreover, the Netherlands does not permit keeping records of individual-level voting behavior in elections.[8] Given these limitations, we test a reduced-form version of the expected relationship, that between famine exposure and contemporaneous vote choice, and rely instead on municipality level electoral data from the Dutch Electoral Council, which covers the number of votes received by political parties competing in national elections measured at the municipal level.[9] We include all available parliamentary elections held between 1998, the first election for which we have data on our main explanatory

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[8]This is enshrined in the constitution which states that "Elections shall be by secret ballot" (Article 53, paragraph 2, Dutch Constitution).

[9]Note that there are no electoral reasons for sorting because these national elections are about national policies, including social policies, and the Netherlands is a single electoral district.

variable, and 2017, the most recent election we have municipal data for.[10] Our data set thus includes 388 Dutch municipalities across seven national elections held in the past two decades.[11] The use of all available municipalities is possible as these are areas where individuals contemporaneously reside and cast their votes in, rather than the specific urban areas historically affected by the famine. We use the latter information to trace individuals as we explain below.

Demand for Social Insurance. Our dependent variable, demand for social insurance, is measured using aggregate data on the vote share of Left-wing parties in each national election. We take this as a percentage of all valid votes and exclude blank votes. Since voting data is not available by age cohort, this can be considered a hard test for our argument. It is likely that the effects we find would be stronger when comparing treated to untreated with regards to the voting behavior of their own cohort, and not to the effects on the general support of Left parties (as we do in this paper). These parties have been traditionally and historically associated with promoting redistribution and social insurance. Left-wing parties include the Labor Party, the Socialist Party, and the Green Party. We also use alternative definitions by classifying parties based on their positions on social insurance and income redistribution, using data from the Comparative Manifesto Project, and on their proposed spending commitments, using data from the CPB Netherlands Bureau for Economic Policy Analysis.[12]

Population Exposed In-Utero to the Dutch Famine. The famine mostly affected urban areas in the Western part of the Netherlands between October 1944 and May 1945 (Stein *et al.*, 1975). To identify which individuals were exposed to the famine before birth, we rely on rich administrative data from Statistics Netherlands on individuals' date and place of birth.[13] We assume that individuals were exposed to the famine in-utero if they were born between November 1944 and January 1946 in urban areas in the West, defined as having more

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[10]We cannot include the election held on March 17, 2021.

[11]These municipalities had on average 44,000 inhabitants. Since municipal reforms occur almost annually in the Netherlands, we adjust all variables to reflect the boundaries in January 1, 2017.

[12]We describe this operationalization in the section "Alternative Mechanisms: Left Party Definition".

[13]These administrative records contain information about individuals who legally resided in the Netherlands in 1995 or later. This means that we cannot analyze the behavior of individuals who died (or emigrated) before 1995. As the already deceased individuals arguably had the strongest reasons to demand more social protection, our results could be interpreted as lower-bound effects. Since day of birth is not released due to privacy reasons, we rely on month and year of birth (see, also Scholte *et al.*, 2015).

than 40,000 inhabitants in 1944.[14] We then calculate the proportion of such treated individuals in contemporaneous municipality m at time t as follows:

$$\%\text{Treat}_{m,t} = \frac{\text{sum(Treat)}_{m,t}}{\text{Adult Population}_{m,t}}$$

Based on this, we are able to calculate election-to-election changes in the treatment population within each municipality as $\Delta\text{Treat}_m = \%\text{Treat}_{m,t} - \%\text{Treat}_{m,t-1}$.

We also define a control cohort comprised of the proportion of individuals born during the famine in the urban areas in the Northern, Eastern, and Southern parts of the Netherlands.[15] These are individuals similar to their counterparts in the West, but who were not exposed in-utero to the famine.

Results

Our theoretical argument and empirical approach imply that in-utero malnutrition affects individuals' insurance demand later in life. Therefore, we expect municipalities with higher density of treated population to exhibit higher support for the Left. We explore this relationship first in the raw data. Figure 2 documents a strong, positive relationship between changes in the treated population within a municipality and the associated levels (left panel) or changes (right panel) in support for the Left.[16]

We proceed by formally exploiting the effect of the temporal and geographical variation in the in-utero exposure to the Dutch Famine on the electoral success for the Left across Dutch municipalities by estimating the following linear regression model:

$$\text{Left Vote Share}_{mt} = \tau\Delta\textit{Treated}_m + \lambda_t + \epsilon_{mt} \qquad (1)$$

where $\Delta\textit{Treated}$ indicates the election-to-election change in the proportion of the treated population in municipality m, and λ captures election specific effects. Table 1 presents the results of three models that assess whether higher density

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[14]This leads to the inclusion of 29 urban areas across the country. Following Stein *et al.* (1975) and Scholte *et al.* (2015), we exclude from the analysis Arnhem and Nijmegen. The results are similar if we lower the threshold to 30,000 inhabitants (see Table B.1 in the Online Appendix).

[15]Since the Allied forces liberated the South earlier, in a robustness test, we also define the control population as those born in urban areas only in the North and East of the country. Table B.1 confirms that the findings are similar.

[16]In the Online Appendix, we document a similar relationship when we distinguish between Western and Eastern municipalities (Figure A.2). We also plot the pooled changes in treatment (Figure A.1) and their geographical distribution (Figure A.3).
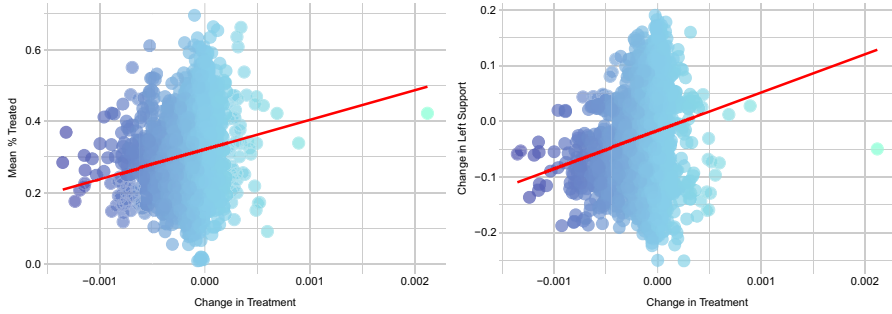
Figure 2: Left vote share and change in % treated.

Note: On the y-axis, we report the pooled level of left support (left panel) and the change in left support (right panel). On the x-axis we report the change in treatment levels, calculated as % $\text{Treat}_t - \%\ \text{Treat}_{t-1}$.

Table 1: Famine treatment effect on left vote share.

	(1)	(2)	(3)
ΔTreat (SD)	0.008**	0.010***	0.011***
	(0.003)	(0.003)	(0.003)
ΔControl born E (SD)	−0.010***		
	(0.003)		
Δ Control born W −10y (SD)		−0.002	
		(0.003)	
Δ Control born W +10y (SD)			−0.004
			(0.003)
Observations	2,328	2,328	2,328
R-squared	0.329	0.321	0.321

Note: Outcome is total vote share (rescaled between 0 and 1) obtained by all left wing parties. Treatment is defined based on the urban definition of more than 40,000 inhabitants during the famine. All elections between 1998 and 2017 are included and models include election fixed effects. Standard errors are clustered at municipality level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

of the treated population explains Left support.[17] Our main specification is model (1), as in Equation (1). It reports standardized coefficient estimates for τ. Our estimates confirm that a change in the density of individuals exposed in-utero to famine increases support for the Left within municipalities. On average, a one standard deviation increase in the proportion of treated individuals increases support for the Left by 0.008. Perhaps more importantly, these results also suggest not only that malnutrition may have an impact on

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[17]We also report an alternative specification including municipality and year fixed effects in Table B.3.

Left support but also that this effect is persistent across time — more than 50 years after exposure. By contrast, the effect on Left support of the control group (the individuals born at the same time, but in the Eastern side of the Netherlands unaffected by the famine), reveals no systematic relationship or a negative one. This suggests that our main result is unlikely to capture a cohort effect, and supports the insurance-related logic of the famine we propose in this paper.

Interpreting these coefficients substantively is not straightforward as the number of treated individuals per municipality is small (Figure A.3). If individual-level data were available, we would have expected treatment status to positively influence an individual's probability of voting Left. However, given that we rely on aggregate data, it would be unreasonable to expect increases or decreases in the small percentage of treated population within a municipality to disproportionally influence aggregate Left vote shares. That said, to put this effect into context, we provide an illustration in Table B.2. To do this, we estimate a model identical to (1) in Equation (1), but adding a vector of time-varying controls: average housing values, proportion of women, proportion of foreign-born, municipal spending, and the proportion of control. In our sample, Left support varies by 16% within the period of observation (between 22% and 38%). This means that the treatment effect explains as much as 0.017 in Table 1's model (2) in the Online Appendix. By contrast, the number of women within a municipality (also a variable in model 2 in the Online Appendix), a characteristic frequently associated with higher Left support and with similar variance as the treatment in our sample, explains 0.01 of the variation in Left support. This implies that our treatment effect is roughly 59% higher than that of the female population, which we consider quite sizeable.

Even though focused on in-utero famine, however, the results in model (1) do not address the possibility that both being born *and* growing up in the West rather than the East has a different effect on the likelihood of supporting the Left. At the individual level, exposure to traumatic events may last a long time for the victim (Cameron and Shah, 2015). Therefore, parents may instill in their offspring certain behaviors (e.g., risk aversion) that would be consistent with support for the Left. At the aggregate level, those born in the West could be socialized differently than those born in the East as a consequence of the famine, such that the former would be more supportive of the Left, irrespective of their actual exposure to the treatment. This implies that the results shown in model (1) of Table 1 aggregate two effects. First, the biological shock of the in-utero exposure to the famine on the offspring's health and human capital. But also a socialization effect captured through the offspring's experiences growing up. We are able to tease out these social effects by comparing cohorts exposed to the famine not just in-utero, but at various ages, less prone to be biologically affected by malnutrition. We explore this in two ways.

First, we switch the control groups in models (2) and (3) in Table 1. Model (2) compares the density of the treated in a municipality to that of those born also in the West, but 10 years before. Model (3) does the same, but the comparison is now between the density of the treated in a municipality to that of those born in the West 10 years before after the famine.[18] As was the case in model (1), our estimates confirm that a higher density of individuals exposed in-utero to famine increases support for the Left within municipalities while the effects of the control group (the density of individuals born 10 years before or after the famine in the West) reveal no systematic relationship.

Second, to test different ranges of time, we shift the original treatment's duration (15 months) to cohorts born before the famine, aged 2, 5, and 10 at the time of its onset.[19] Focusing now just on individuals born in the West, Figure 3 compares the density of treated individuals (in-utero exposed to the famine) to that of older cohorts. The results show a statistically significant (at the 95% level) higher support for the Left associated to the number of
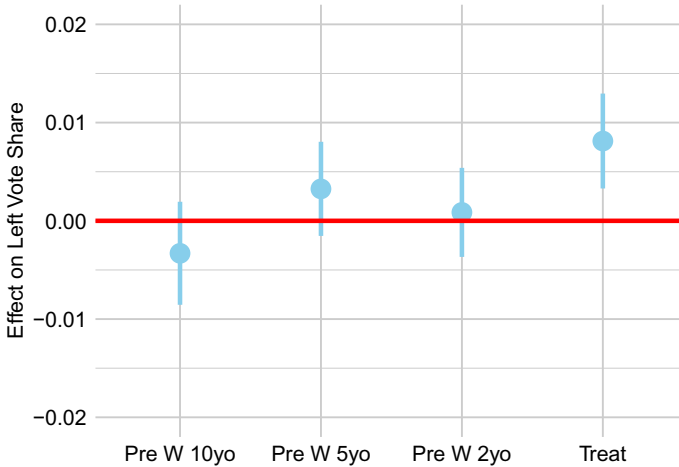

Figure 3: Left support in West pre-famine.

Note: The dependent variable represents the Left Vote share and varies between 0 and 1. The models report the effects of the proportion of individuals born in the West 10, 5, 2 years prior to and during the famine on the outcome. Estimates are based on linear models, and presented along with their 95% confidence interval based on clustered robust standard errors, and a red horizontal line at zero.

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[18]More specifically, we shift the original treatment's duration (15 months) to cohorts born 10 years before and after the famine.

[19]For example, children aged 2 at the onset of the famine are those born between November 1942 and September 1941 (a 15-months interval).

exposed in-utero compared to older cohorts born in the same areas. There may be a socializing effect, the figure shows the significance of being born in the West dissipating as cohorts are born increasingly earlier than the famine, but it is weak compared to the long-lasting biological effects of the in-utero treatment.

To explore the possibility that younger cohorts born in the famine areas nevertheless experienced a social effect (parents instilling risk aversion to children born after the famine), we compare the in-utero exposed cohort with younger ones. As before, we shift the original treatment's duration (15 months) to cohorts born in the West 2, 5, and 10 years after the Liberation of the Netherlands, when food intake reverted back to normal. Figure 4 shows that Left support once again dissipates as cohorts are born after the famine, such that those exposed in-utero have a statistically significantly (at 95% level of confidence) higher support for the Left than cohorts born 10 years after the Liberation within the previous famine areas.

Together, these results show the resilience of the shock for those exposed in-utero to the famine, who are most likely to support the Left compared to both younger and older cohorts born in the same areas in the West. At the same time, the results suggest an accompanying socialization-based effect
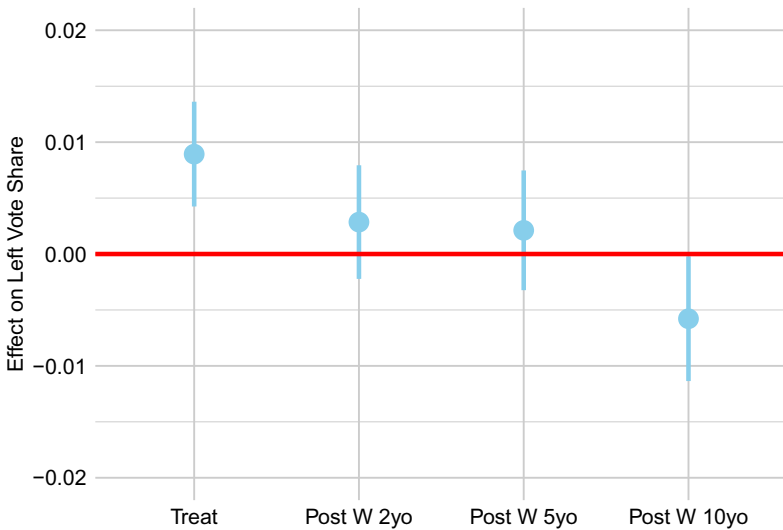

Figure 4: Left support in West post-famine.

Note: The dependent variable represents the Left Vote share and varies between 0 and 1. The models report, in turn, the effects of the proportion of individuals born in the West during and 2, 5, and 10 years after the famine on the outcome. Estimates are based on linear models, and presented along with their 95% confidence interval based on clustered robust standard errors, and a red horizontal line at zero.

which, although it dissipates across time, mostly affects those born immediately after Liberation.[20]

Robustness Tests

While the previous section illustrates the strength of the main relationship hypothesized in this paper, we dedicate this section to testing the robustness of our results. We do this in two distinct ways. First, we explore potential threats to the identification of the in-utero treatment. We focus on the timing of conception and the issue of embryo survival. Second, we analyze alternative explanations. Our theoretical expectation is that exposure to the famine has lasting consequences on individuals' human and health capital, as well as upbringing. We have identified municipalities with higher density of treated individuals to be more supportive of the Left. In what follows, we test the robustness of our results to two types of challenges: (i) the presence of time-varying factors that may coincide with the location of the treated population (or indeed reflect geographical sorting of the treated population) and (ii) alternative mechanisms to insurance (which we check by providing alternative definitions of Left parties based on manifesto and budget proposals, and support for spending on alternative insurance-unrelated policy areas such as law and order spending).

Timing of Conception

The characteristics of parents deciding to conceive after the onset of the famine may be systematically different from those of parents who decided to postpone conception. This would imply that the offspring's in-utero malnutrition may not be exogenous to parents pre-existing characteristics — a factor that may influence not only treatment status but may also be associated with the offspring's Left support later in life. To account for this possibility, we provide an alternative treatment definition, which includes only those individuals that were conceived *before* the famine started (i.e. born between 1 November 1944 and 1 August 1945 in urban areas in Western Netherlands). Following the same specification as in equation 1, Table 2 estimates the effect of higher density of treated individuals (conceived before the famine) on Left support. The results show a similar effect to that identified in Table 1.

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[20]Our results also speak to the far-reaching consequences of malnutrition noted in related outcomes (Aksoy and Gambetta, 2022; Campante and Yanagizawa-Drott, 2015).

Table 2: Left support among treated conceived before the onset of the famine and gender.

	(1)	(2)	(3)
ΔTreat conception (SD)	0.007**		
	(0.003)		
ΔControl conception (SD)	−0.009***		
	(0.003)		
ΔTreat male (SD)		0.006**	
		(0.003)	
ΔControl male (SD)		−0.010***	
		(0.003)	
ΔTreat female (SD)			0.008***
			(0.003)
ΔControl female (SD)			−0.007**
			(0.003)
Observations	2,328	2,328	2,328
R-squared	0.326	0.328	0.323

Note: Outcome is total vote share (rescaled between 0 and 1) obtained by all left wing parties. Treatment is defined based on the urban definition of more than 40,000 inhabitants during the famine. All elections between 1998 and 2017 are included and models include election fixed effects. Standard errors are clustered at municipality level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Embryo Survival

Male embryos are more likely to detach in-utero, so the ones surviving (i.e., the ones we observe) are the strongest, possibly meaning those individuals least likely to demand insurance later in life. In Table 2, we explore whether our main results exhibit heterogeneity by gender. The table shows the effect of the proportion of treated male/female out of the male/female population on Left support. The results show no gender-specific effect. Finding no significant differences between the proportion of females and males who are treated is consistent therefore with the idea that the treated, regardless of the embryo's chance of survival, are more likely to support the Left due to insurance motivations.

Movers

Time-varying confounders could threaten the validity of our estimates insofar as they are associated with treated density and Left support. Many economic factors are quite stable within municipalities or attract in a similar fashion treated and control individuals, so we are less concerned about some characteristics like the quality of general public goods provision (we return to the

Table 3: Residence change effect on treated density and left support.

| | (1) | (2) |
	% Treat	Left Vote Share
% Movers (SD)	-0.069^{***}	0.012^{***}
	(0.014)	(0.002)
Mun FE	Yes	Yes
Time FE	Yes	Yes
Observations	2,716	2,716
R-squared	0.334	0.869
Number of munid	388	388

Note: Outcome is total vote share (rescaled between 0 and 1) obtained by all left wing parties. Treatment is defined based on the urban definition of more than 40,000 inhabitants during the famine. All elections between 1998 and 2017 are included and models include election fixed effects. Standard errors are clustered at municipality level. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

issue of sorting into municipalities with specific policy characteristics below). Characteristics that attract only the treated but not the control (who have the same age, but were born in the East), however, would lead us to incorrectly attribute the effect to the famine and not to population movements. We start by focusing directly on movers, which we define as individuals changing their residence between any two elections. First, we note that there is no change in our results if we include the proportion of movers as an additional variable in our main estimation. Secondly, we show in Table 3 that the proportion of movers is positively associated with Left support, but negatively correlated with % treated. This makes it unlikely for the famine effect to capture population movements.

Sorting

Our argument implies that decisions regarding the location of treated individuals across municipalities are not positively related to pre-existing municipality characteristics that favor the Left. Table 4 shows that about 4% of treated individuals change residence over our period of observation. Among them, about two-thirds move within the West. Of particular concern is whether their decision to relocate is related to municipality characteristics that are correlated with Left support. For example, treated individuals may move to municipalities that devote a larger proportion of their budget to social spending, such as education or health, or that have more affordable housing. We explore these possibilities in Figure 5.

The results suggest that relocation decisions of treated individuals are not correlated with these characteristics. If anything, a higher density of treated

Table 4: Characteristics treated movers.

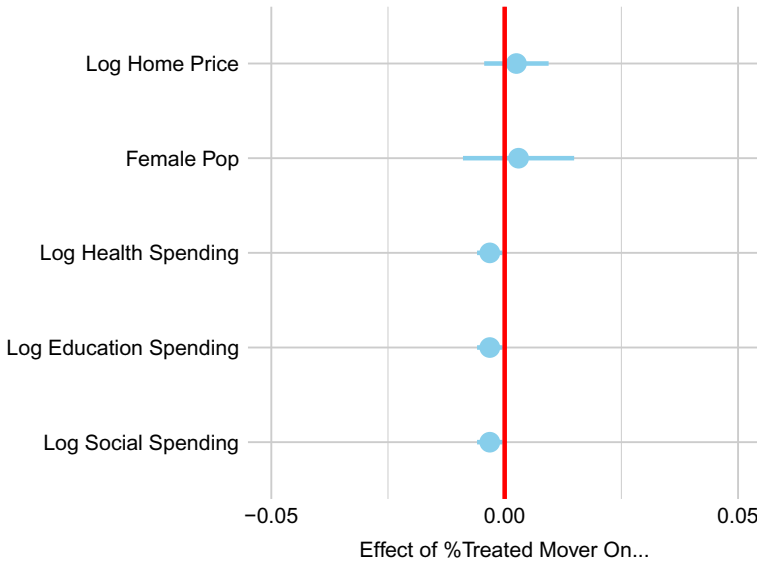| Status	Proportion	
Municipality nonmover	96.2%	
Municipality mover	3.8%	
	Within West	Within East
Nonmover	78.07%	21.93%
Mover	64.54%	35.46%

Figure 5: Treated movers and municipality characteristics.

Note: The dependent variable represents, in turn, the log of home price, the proportion of female population, the log of health, education, and social spending. The models report the effect of the proportion of treated individuals that move municipalities on each of these outcomes. Estimates are based on linear models, and presented along with their 95% confidence interval based on clustered robust standard errors, and a vertical horizontal line at zero.

movers is negatively correlated with municipality spending levels, and not at all correlated with certain characteristics, such as average home prices.

Alternative Mechanisms: Left Party Definition

The outcome of interest in this paper is support for the Left, which we have so far defined based on party labels. In our main results, a 'Left' party is

defined as concerned about redistribution/insurance based on its ideology and historical connections to some meaningful groups of voters. While party labels can be used as information shortcuts by voters to capture a party's redistributive/insurance position, simply classifying parties based on their label might not accurately measure the concept of redistributive/insurance voting if election-specific factors influence parties' positions on these issues.

To ensure that the effect we capture is related to insurance-related spending, we provide alternative definitions of 'Left' support. First, we use data from the Comparative Manifesto Project (CMP) (Budge *et al.*, 2001) and its 2016 update (Volkens *et al.*, 2016). This allows us to define party positions based on three dimensions: redistribution, welfare, and insurance.[21] Second, we use data from CPB Netherlands Bureau for Economic Policy Analysis which, for each policy domain, estimates the net effect of proposed measures in billion euros compared to the status quo.[22] We focus our analysis on welfare and health spending proposals as our redistributive/pro-insurance policies.

Like Rueda and Stegmueller (2019), we classify a party as redistributive/ pro-insurance if it proposes more redistribution than the hypothetical average (or median) party in a given country–election year.[23] The advantage of this procedure is that it allows some parties to be defined as pro-redistributive in elections when they propose policies higher than the mean/median proposals and as anti-redistributive when they propose less. The results reported in Online Appendix C are consistent with, even mildly stronger than, the ones in our main specification. Overall, the results of this exercise favor the interpretation that an insurance-related mechanism is behind our finding that the proportion of treated individuals influences Left support.

Placebo: Support for Broader Spending

Individuals exposed to the famine may prefer higher spending in all domains, not just those likely to affect them — such as social or health policies. Alternatively, treated individuals may prefer the government to devote a larger share of its budget on law and order, because of the possible connection between war experiences and protection. We explore the relationship between treatment density and support for policy areas where, based on the insurance mechanism, we would not expect an effect. Exploring the effect on a broader set of spending domains therefore constitutes a placebo test for the proposed mechanism. Table C.2 presents the results. As models (5) and (6) make clear,

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[21]We explain the measurement of these dimensions in Online Appendix C.

[22]This measure is described in greater detail in Online Appendix C.

[23]Note that we drop small parties with less than 3% of votes in an election for the mean calculation.

parties supporting these policies do not have higher levels of electoral success in municipalities with higher percentages of treated populations.

Conclusion

Whenever they have been studied, the effect of shocks appear to be short lived (see, for example, Margalit, 2013). This paper demonstrates that famines cast a long shadow over insurance demand and identifies long-term effects of exposure to shocks on individuals' political behavior later in life. Our findings show that communities with higher density of individuals exposed in-utero to the Dutch Famine are more likely to support Left parties more than 50 years later. This suggests that present-day differences in political behavior can partly be traced back to traumatic historical events. We provide tentative evidence that the mechanism linking an early life shock to voting behavior during adulthood works through individuals' support for social insurance prompted by their risk profiles. We note significant spillovers among those born just after Liberation, which, however, dissipate as cohorts are born later. While it is likely that malnutrition has impacted these individuals' health and human capital as well, albeit to a lesser extent than those exposed in-utero, a sociological effect (exposed parents instilling risk aversion in their children) may exist, even if weaker.

We advance the literature on the determinants of redistribution preferences in at least three crucial ways. First, despite the substantive recent interest in the formation of redistribution preferences (Rueda and Stegmueller, 2019), we know much less about whether these preferences do in fact affect political behavior. We contribute to this literature by theorizing the link between the effect of an early-life shock to voting behavior later in life through its effect on redistribution demand. Second, we provide causal estimates of in-utero exposure to malnutrition on individuals' political behavior in an effort to address the endogeneity of risk on preferences and behavior. While a large literature finds that risk (related to occupation, education or expected income) affects preferences, we explore factors early in life that may affect the choices (into occupations, education, etc.) that then will affect political outcomes. Finally, we explore the importance of biological but also sociological mechanisms likely to drive this behavior. In line with a growing literature on the intergenerational transmission of values, our results are consistent with a possible upbringing effect within famine areas (complementary to the biological one) that inflicts a higher degree of risk aversion within those communities. Our work therefore complements the literature on the historical legacies of traumatic events (Charnysh and Finkel, 2017; Dinas *et al.*, 2019; Homola *et al.*, 2020; Rozenas and Zhukov, 2019). While prior work mostly focuses on inter-generational transmission of values and behaviors, we focus on those

directly affected by these events and study their political behavior later in life.

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