INSURING AGAINST HUNGER? LONG-TERM POLITICAL CONSEQUENCES OF EXPOSURE TO THE DUTCH FAMINE

Raluca L. Pahontu
LSE

Gerda Hooijer
UCL

David Rueda
University of Oxford

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Abstract

Does experiencing a shock alter one's voting behaviour? 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 affects insurance demand and voting behaviour later in life. Given similar socialization patterns, we find conglomerations of affected individuals more likely to support the Left 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 behaviour.

Keywords: political economy, voting, insurance, redistribution, famine, administrative data

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

1 Introduction

It is by now widely recognized that prenatal malnutrition or hunger in early life has long-lasting effects on the individual's health and labour market outcomes later in life. Affecting more than 900 million individuals around the world, undernutrition is particularly relevant in developing countries (Currie and Vogl, 2013; Martins et al., 2011; Fernald et al., 2008; Behrman et al., 2004; Behrman, 1993; Field et al., 2009). Yet, even in developed countries, children from disadvantaged backgrounds may face malnutrition or even hunger spells throughout their lives and benefit from programmes such as Food Stamps Programmes (Hoynes and Schanzenbach, 2009), Head Start (Garces et al., 2002; Walters, 2015) and 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 behaviour, especially in terms of electoral participation, insurance demand and voting preferences. If in-utero malnutrition has any long-term political consequences, this would be because of the persistent effects of malnutrition throughout an individual's life – the fetal origins hypothesis (Barker, 1990). This biological channel therefore affects health and non-health outcomes, lowering overall an individual's health and human capital stock. 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). Yet, voter behaviour in adulthood is endogenous to the accumulation of human and health capital, including it being 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 behaviour and views poses a major empirical challenge. Unpredicted and severe, we therefore consider the Dutch famine – which left more than 4.5 million people malnurished 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 market temporally and spatially, making it easier to distinguish between exposure in-utero and after birth. Additionally, quality administrative data allows us to identify the respondent's 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 a Cultural Revolution. By comparison, the end of the Dutch Famine coincided with the German surrender (Evans, 2009) and was in

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close proximity to the end of the war.

This study makes two main contributions. First, the determinants of redistribution preferences have seen a resurgence in academic interest in the last 20 years. While this influential political economy literature covers the diverse determinants of redistribution preferences (see, for example, Rueda and Stegmueller 2019, we know much less about whether these preferences do in fact affect political behavior. There is to be sure, a voluminous literature on voting itself. As we make clear below, this paper's arguments are generally aligned with a long and prominent literature on economic voting. We do not deny that non-economic factors significantly affect voting but agree with Tufte and many others that, to understand voting, we need to "think economics" (1978, 65). We are more specifically interested in exploring whether the demand for redistribution, and its relationship to income loss and risk, affect voting choice. Second, much of the literature on the economic basis for voting is besieged by endogeneity concerns. Income and risk influence preferences which then influence voting, but individuals with particular preferences select into education, occupation, income, region, etc. To our knowledge, this is the first study to provide causal estimates of in-utero exposure to malnutrition on individuals' political behaviour in attempt to address this endogeneity. Previous studies have established that malnutrition has serious and lasting negative consequences on individuals' health, education, and labor market outcomes. Malnutrition may affect both the likelihood of negative health and economic outcomes (risk exposure) and the importance of risk on policy preferences (risk-aversion). We therefore explore the importance of biological but also sociological mechanisms.

2 Famine, Insurance, Redistribution and Voting

Previous research has demonstrated that famines cast a long shadow. Individuals who were exposed to famine in utero are, for example, more likely to be overweight (Barker, 1990; Conti et al., 2019; Ravelli et al., 1976), shorter (Van den Berg et al., 2015; Portrait et al., 2017), 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 problems (Susser et al., 1998; Brown et al., 1995; Neugebauer et al., 1999).[1] Economists have also found that in-utero malnutrition leads to lower educational attainment and occupational status (Jürges, 2013; Neelsen and Stratmann, 2011), a higher likelihood of unemployment and working reduced hours (Scholte et al., 2015; Chen and Zhou, 2007), and spending a larger fraction of one's income on food (Kesternich et al., 2014).

Given these long-term adverse health and labor market effects, the starting point for our paper is to posit a relationship between in-utero exposure to famine and redistribu-

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

tion/insurance prerences. Why would those affected by the famine demand higher insurance? There is first a biological effect. We follow the literature arguing that in-utero exposure to famine reduces labour market potential by lowering an individual's human and health capital stock. There is evidence for the negative effects of malnutrition on health (Portrait et al., 2017; Neelsen and Stratmann, 2011; Lindeboom et al., 2010; Kesternich et al., 2015; Conti et al., 2019), on educational achievement (Greve et al., 2017), on income and wealth (Hamoudi and Sachs, 1999), and labour force participation (Scholte et al., 2015; Jürges, 2013; Neelsen and Stratmann, 2011; Chen and Zhou, 2007), as well as of shocks on risk aversion (Pahontu, 2020; Decker and Schmitz, 2016). The second mechanism through which famines may impact demand for redistribution is through the socialization of risk aversion. Several studies have documented that historically traumatic events have effects that persist over time and are transmitted from parents to children.[2]

The rest of our theoretical argument proceeds in two stages. First, we address the formation of preferences for redistribution, explaining why risk is an important determinant of demand for redistribution. Second, we detail the influence of redistribution preferences on voting choices. We argue that those who are supportive of redistribution will be more likely to vote for redistributive parties.

2.1   Risk and redistribution preferences

The first step in our argument involves the relationship between individual levels of income, risk and redistribution preferences. Political economy approaches that start from the assumption that an individual's position in the income distribution determines her preferences for redistribution are often inspired by the theoretical model proposed by Romer (1975) and developed by Meltzer and Richard (1981). To recapitulate very briefly, the RMR model assumes that the preferences of the median voter determine government policy and that the median voter seeks to maximize current income. If there are no deadweight costs to redistribution, all voters with incomes below the mean maximize their utility by imposing a 100% tax rate. Conversely, all voters with incomes above the mean prefer a tax rate of zero. When there are distortionary costs to taxation, the RMR model implies that, by increasing the distance between the median and the mean incomes, more inequality should be associated with more redistribution.

While it is the case that the rich support redistribution less than the poor almost everywhere, the strength of this relationship is hardly consistent (Dion, 2010; Dion and Birchfield, 2010; Beramendi and Rehm, 2016). A reason for this is that, as mentioned above, the material self-interested factors affecting redistribution preferences should not be limited to a measure of present income. If material self-interest is defined inter-temporally, the more direct effects of contemporary relative income (as in Romer 1975 and Meltzer and

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[2]See Lupu and on the deportation of Crimean Tatars in 1944

Richard 1981) should be complemented by arguments about about social insurance and risk (as in Sinn, 1995; Moene and Wallerstein, 2003; Iversen and Soskice, 2001; Rehm, 2009; Mares, 2003), and about social mobility and life-cycle profiles (Rueda and Stegmueller, 2019; Alesina and Giuliano, 2011; Haider and Solon, 2006; Benabou and Ok, 2001). These approaches distinguish an insurance component of redistribution preferences that incorporates an intertemporal element in material self-interest. Individuals will insure against uncertain future income levels and will therefore favor social protection when they are exposed to an increased risk of job or wage loss. As these forms of social security (such as unemployment benefits or social assistance) are redistributive, redistribution preferences for individuals exposed to these risks will be high. Most insurance models of redistribution preferences integrate four elements: (i) the risk of job/wage loss; (ii) the likelihood of regaining employment; (iii) the degree of risk-aversion; and (iv) the presence of some policy that redistributes resources to those who experience the job/wage loss. Our main contribution in this paper is to argue that the exposure to famine (whether this exposure is biological or social) affects the risk of job/wage loss in (i) as well as possible the degree of risk-aversion in (iii).[3]

2.2 Redistribution preferences and vote choice

In the second stage of our argument, we argue for the relevance of redistribution preferences to voting. We therefore follow a well-established literature on the relationship between economic considerations and political behavior. As mentioned above, most political economy arguments start from the assumption that an individual's redistribution preferences affect her political choices (see Romer 1975 and Meltzer and Richard 1981). The literatures on economic voting and class voting are based on similar arguments. Like authors in the economic voting tradition (e.g., Duch and Stevenson 2008), our argument proposes that there is a relationship between an individual's economic interests and her likelihood to vote for a particular party. Like the traditional economic voting literature (Downs, 1957) we conceive of voters as instrumental rational actors. Individuals will vote following a comparison of what they gain or lose from the policies proposed by each party. In the words of Duch and Stevenson, we assume that "voters rationally derive expected utilities for competing political parties and that these determine their vote choice" (2008: 9). As in the pioneering work of Kramer (1971) and Fair (1978), we consider that economic well-being (and therefore redistribution and insurance) is a significant factor affecting a voter's utility function.

A substantial literature debates the issue of how economic considerations enter a citizen's vote choice function. Two main approaches can be distinguished, one emphasizing

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[3]While we provide some evidence below that there are both biological and social effects to the Dutch Famine, we cannot directly address questions about individual risk aversion with our data.

sanctioning and the other focusing on *selection*. The *sanctioning model* is characterized by the consideration that voters are narrowly retrospective and mostly motivated by punishing or rewarding incumbents (see the classic works of Kramer 1971, Key 1966 and Fiorina 1981). Focusing on moral hazard, i.e., the risk of rent-seeking by incumbents if not punished for bad economic outcomes, Barro (1973) and Ferejohn (1986) also belong within this tradition. The *selection/competency model* argues that voters gather more information to assess the likely economic outcomes associated with competing political alternatives. Downs (1957) and Stigler (1973) are classical examples of this approach but we would argue that this is also the understanding of voting underlying Meltzer and Richard (1981) and subsequent political economy treatments of redistribution and voting (Persson and Tabellini, 2000). While not incompatible with sanctioning, our argument more clearly implies a selection logic. We propose that individuals who are in favor of redistribution and insurance will identify the party more likely to promote equality and therefore be more likely to vote for it.

3   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 2.6 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 (Lumey et al., 2007; Dols and Van Arcken, 1946). 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 in Figure 1) came under threat. The Allied forces subsequently halted their liberation efforts in the Netherlands and as 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 the German occupiers 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).

During the 'Hunger Winter', as the Dutch Famine is also referred to, people tried to

Figure 1: Regions Affected by the Dutch Winter Hunger
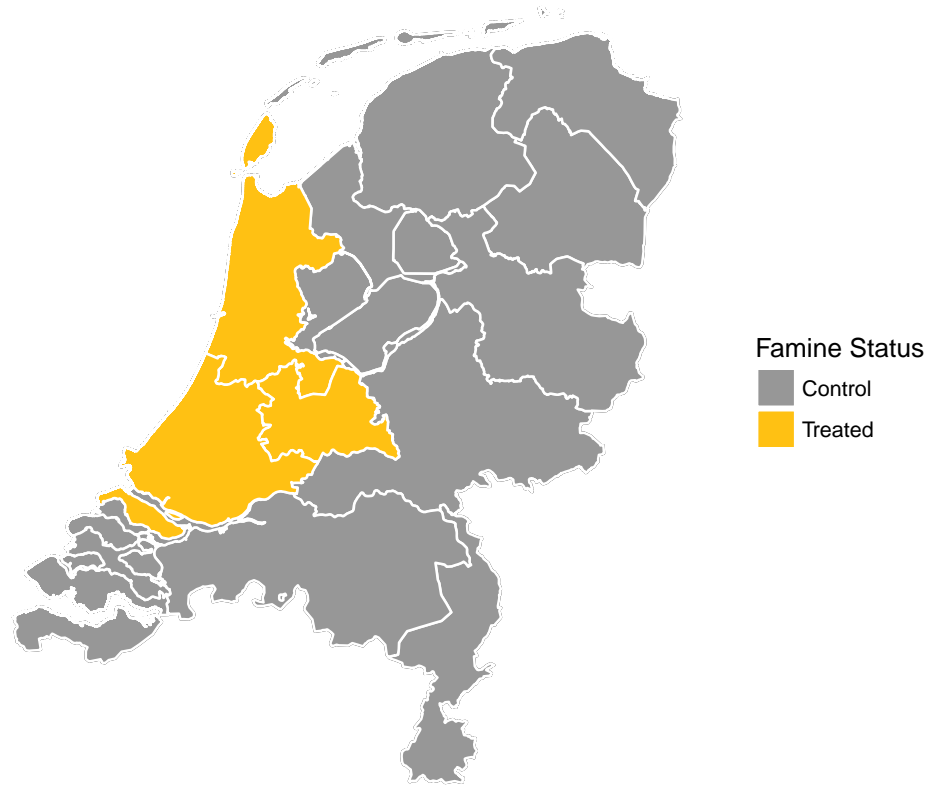
secure food in different ways. As before, they received food from centralized, government-operated food distribution systems. By November 1944, however, official rations had fallen below 1,000 dietary calories per day and by April 1945 they were down to 500 calories per day (Almond and Currie, 2011) – 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 also turned to the black market though rapidly rising food prices meant that this route was mostly available to the wealthy. Others, many of whom from the working and lower-middle class, undertook dangerous and physically demanding journeys to rural areas to trade with farmers their belongings for food (de Zwarte, 2018; Roseboom et al., 2011). In addition, feeding initiatives by civil society organizations, in particular churches, provided crucial supplemental food resources, initially to school age children and later also to adults (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).

Given the long-term adverse health and labor market effects of famines mentioned in the section above, the clear temporal and spatial demarcation (Scholte et al., 2015; Stein et al., 1975), and the availability of high-quality administrative data with which we

can identify individuals by their month, year, and place of birth, we leverage the Dutch Famine to study the effects of individual exposure to famine on demands for social insurance later in life. An additional practical advantage is that the Dutch Famine is less susceptible to confounders or cohort effects than other famines because its end coincided with the surrender of the Germans and the end of the war (Evans, 2009). By contrast, the Chinese Famine, for example, was followed by the Cultural Revolution.

One concern with our empirical strategy 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 (CBS 1947: 14).[4] 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). In addition, feeding initiatives by societal organizations targeted school age children and adults while pregnant women were only eligible if they were at least 30 percent underweight (Zwarte, 2019). The limited ability of pregnant women to avoid hunger also comes out of hospital records from prenatal appointments showing that most pregnant women were several kilograms lighter 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, Stein et al. 1975; Scholte et al. 2015), the urban areas in the densely populated West of the country are likely 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).

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[4]The main exception were the evacuations of roughly 40.000 severely malnourished children (or 9 percent 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)

4 Data and Measurement

To test our theoretical claims, we would ideally use individual-level data on 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.[5] Given these limitations, we rely on 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.[6] We include all available parliamentary elections held between 1998, the first election for which we have data on our main explanatory variable, and 2017, the most recent election.[7] Our data set thus includes 388 Dutch municipalities across seven national elections held in the past two decades.[8]

4.1 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.[9] 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.[10]

4.2 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

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

[6]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.

[7]The next election will be held on March 17, 2021.

[8]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.

[9]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.

[10]We describe the operationalization in the Section 6.6.

Netherlands on individuals' date and place of birth.[11] 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 than 40,000 inhabitants in 1944.[12] We then calculate the proportion of treated in 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 $\Delta\text{Treat}_m = \%\text{Treat}_{m,t} - \%\text{Treat}_{m,t-1}$.

4.3 Control Variables

We control for a set of time-varying municipal characteristics that may be correlated with changes in treatment and support for the Left: average housing values, the proportion of women, the proportion of foreign-born, and municipal spending.[13] These data are publicly available through Statistics Netherlands. To control for possible cohort effects, the main analysis also includes a measure of the proportion of individuals born during the famine in urban areas in the Northern, Eastern, and Southern parts of the Netherlands (i.e. the proportion of control population).[14] These individuals are in many respects similar to their counterparts in the West, but they were not exposed to the famine in-utero.

5 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.[15]

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

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[11]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).

[12]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 A2).

[13]Most economic variables, such as economic growth or unemployment rates, are not available at the municipal level for our period of analysis.

[14]Since the Allied forces liberated the South earlier, we also define the control population as those born in urban areas only in the North and East of the country as a robustness test. Table A1 confirms that the findings

Figure 2: Left Vote Share and Change in % Treated
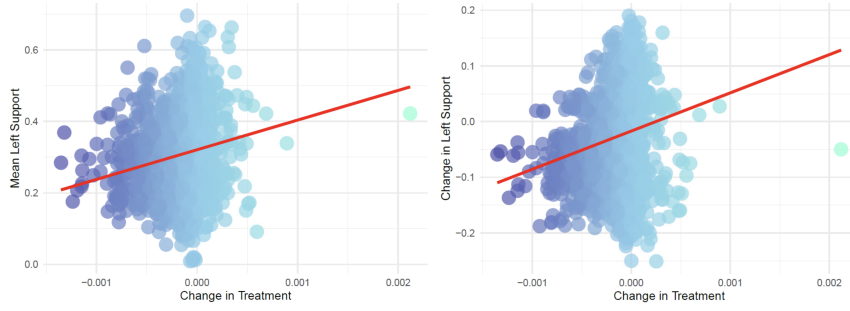
NOTE: On the y-axis, we report the pooled level of left support (left panel) and the change in left support (right panel). On the x-axis we report the change in treatment levels, calculated as % Treat$_t$ - % Treat$_{t-1}$.

Dutch municipalities by estimating the following linear regression model:

$$\text{Left Vote Share}_{mt} = \tau \Delta \text{Treated}_m + \mathbf{X}_{mt} + \lambda_t + \epsilon_{mt} \tag{1}$$

where Δ*Treated* indicates the election-to-election change in the proportion of the treated population in municipality m, $\mathbf{X}$ is a vector of time-varying controls (average housing values, proportion of women, proportion of foreign-born, municipal spending, and the proportion of control), and λ captures election specific effects.

Table 1 presents the results of four models that assess whether higher density of the treated population explains Left support. Our main specification (models (1) and (2) as in equation 1) is complemented by the inclusion of ξ_m, a municipality specific effect, in models (3) and (4). Moreover, models 2 and 4 extend the baseline specification by including the vector of time-varying covariates $\mathbf{X}$ described in equation 1. The models report standardized coefficient estimates for τ. Our estimates confirm that a higher 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 to 0.017 percentage points, in models (1) and (2), and by 0.01 to 0.013 in the fixed effects estimation, in models (3) and (4).

Interpreting these coefficients substantively is not straightforward as the number of treated individuals per municipality is small (Figure A3). If individual-level data were available, we would expect 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 clarify that Left support varies by 16% within the period of observation (between 22% and 38%), meaning that the treatment effect explains as much as 0.017 of

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are similar.

[15] In the Appendix, we document a similar relationship when we distinguish between Western and Eastern municipalities (Figure A2). We also plot the pooled changes in treatment (Figure A1) and their geographical distribution (Figure A3).

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this variation (in model (2)). By contrast, the number of women within a municipality, a characteristic frequently associated with higher Left support, and of similar variance as our treatment, 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. Perhaps more importantly, these results also suggest not only that malnutrition may have an impact on 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.

Table 1: Famine Treatment Effect on Left Vote Share

|	(1)	(2)	(3)	(4)
Δ Treat (SD)	0.008**	0.017***		
	(0.003)	(0.003)		
Δ Control (SD)	-0.010***	-0.007***		
	(0.003)	(0.003)		
% Treat (SD)			0.010**	0.013***
			(0.004)	(0.004)
% Control (SD)			-0.003	-0.003
			(0.006)	(0.006)
Log Home Price		-0.060***		0.005
		(0.016)		(0.003)
Log Spending		-0.061***		-0.034**
		(0.015)		(0.017)
% Female Population		0.011**		0.001
		(0.005)		(0.003)
% Foreign Population		0.006***		0.001
		(0.001)		(0.001)
Constant	0.247***	0.942***	0.369***	0.745**
	(0.004)	(0.339)	(0.002)	(0.294)
Mun FE	No	No	Yes	Yes
Election FE	Yes	Yes	Yes	Yes
Observations	2,281	2,281	2,687	2,687
R-squared	0.331	0.446	0.865	0.866
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. Controls include the log of average home prices, municipality spending, proportion of women and foreign population. Standard errors are clustered at municipality level. *** p<0.01, ** p<0.05, * p<0.1

Even though focused on in-utero famine, however, these results 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 choose to instill in their offspring certain behaviours (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 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 then 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. To explore this, 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.[16]

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 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.

Figure 3: Left Support in West Pre Famine
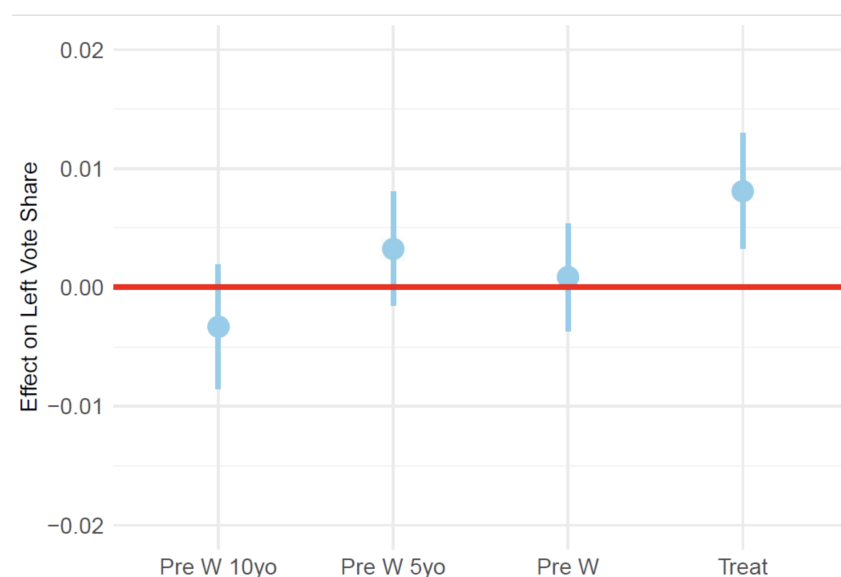
To explore the possibility that younger cohorts born in the famine areas nevertheless experienced a social effect (parents instilling risk aversion to children born after the famine), we compare the in-utero exposed cohort with younger ones. As before, we shift the original treatment's duration (15 months) 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 across time, such that those exposed in-utero have a statistically significantly (at the 90% level) higher support for the Left than cohorts

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

born 10 years after the Liberation within the previous famine areas.

Figure 4: Left Support in West Post Famine
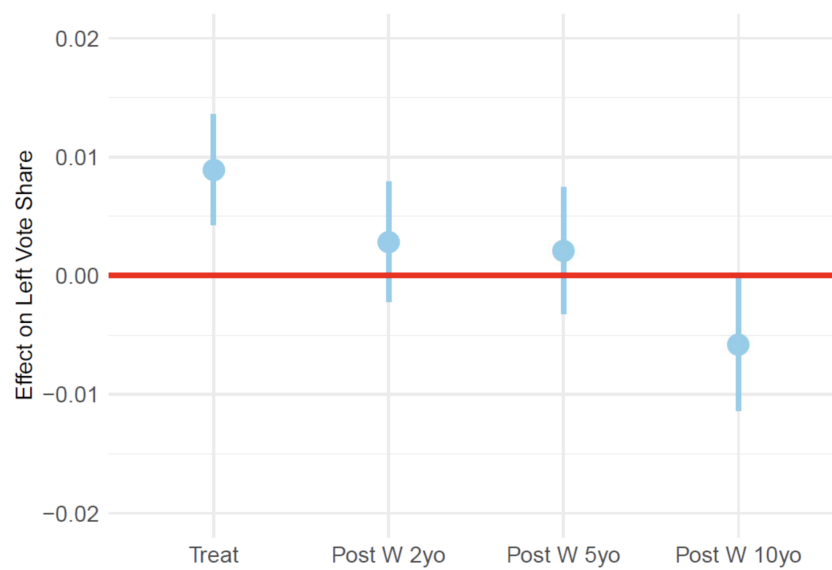
Together, these results show the resilience of the shock for those exposed in-utero to the famine, who are most likely to support the Left compared to both younger and older cohorts born in the same areas in the West. At the same time, the results suggest an accompanying socialization-based effect which, although it dissipates across time, mostly affects those born immediately after liberation.

6 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, the possibility that a Rotterdam birth effect dominates our estimation 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 defence or law and order spending).

6.1 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.

6.2 Exclusion of Rotterdam

Rotterdam was disproportionately affected by the famine compared to other urban areas in Western Netherlands. In Table 3, we explore the extent to which our results merely reflect a Rotterdam birth effect rather than a general famine effect. We proceed by including a very restrictive definition of treatment, in which all those born in Rotterdam within the famine period are excluded from the analysis. The results show the resilience of a positive treatment effect on Left support in municipalities with higher treated population (even though the effect does not reach statistical significance in the model without controls).

Table 2: Left Support Among Treated Conceived Before the Onset of the Famine

	(1)	(2)
Δ Treat (SD)	0.007**	0.016***
	(0.003)	(0.003)
Δ Control (SD)	-0.009***	-0.006**
	(0.003)	(0.002)
Controls	No	Yes
Observations	2,281	2,281
R-squared	0.327	0.441

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. Controls include the log of average home prices, municipality spending, proportion of women, foreign population. Standard errors are clustered at municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table 3: Left Support among Treated Excluding Rotterdam

	(1)	(2)
Δ Treat (SD)	0.005	0.014***
	(0.003)	(0.003)
Δ Control (SD)	-0.010***	-0.008***
	(0.003)	(0.003)
Controls	No	Yes
Observations	2,281	2,281
R-squared	0.328	0.437

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. Controls include the log of average home prices, municipality spending, proportion of women, foreign population. Standard errors are clustered at municipality level. *** p<0.01, ** p<0.05, * p<0.1

6.3   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 life. In Table 4, 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.

Table 4: Famine Effect on Left Vote Share By Gender

	(1)	(2)	(3)	(4)
Δ Treat Male (SD)	0.006**	0.015***		
	(0.003)	(0.002)		
Δ Control Male (SD)	-0.010***	-0.008***		
	(0.003)	(0.002)		
Δ Treat Female (SD)			0.007***	0.014***
			(0.003)	(0.002)
Δ Control Female (SD)			-0.007**	-0.005*
			(0.003)	(0.003)
Controls	No	Yes	No	Yes
Observations	2,281	2,281	2,281	2,281
R-squared	0.329	0.440	0.325	0.435

NOTE: Outcome is vote share (rescaled between 0 and 1) obtained by 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. Controls include the log of average home prices, municipality spending, proportion of women, foreign population. Standard errors are clustered at municipality level. *** p<0.01, ** p<0.05, * p<0.1

6.4 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 public goods provisions (e.g. better parks). 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 5 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.

6.5 Sorting

Our argument implies that decisions regarding the location of treated individuals across municipalities are not positively related to pre-existing municipality characteristics that favour the Left. Table 6 shows that about 4% of treated individuals change residence over our period of observation. Among them, about two thirds move within the West. Is their decision to relocate related to the characteristics of the new residence? Of particular concern are 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

Table 5: Residence Change Effect on Treated Density and Left Support

	(1) % Treat	(2) Left Vote Share
% Movers (SD)	-0.051***	0.012***
	(0.013)	(0.002)
Controls	Yes	Yes
Mun FE	Yes	Yes
Time Fe	Yes	Yes
Observations	2,687	2,687
R-squared	0.432	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. Controls include the log of average home prices, municipality spending, proportion of women and foreign population. Standard errors are clustered at municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Characteristics Treated Movers

Status	Proportion	
Municipality Non Mover	96.2%	
Municipality Mover	3.8%	
	Within West	Within East
Non Mover	78.07%	21.93%
Mover	64.54%	35.46%

with these characteristics. If anything, a higher density of treated movers is negatively correlated with municipality spending levels, and not at all correlated with certain characteristics, such as average home prices.

6.6 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 by virtue of its ideology and its commitments to historically meaningful groups of voters. The existence of stable ideological and historical connections between parties and some social groups "not only creates easily identifiable choices for citizens, it also makes it easier for parties to seek out their probable supporters and mobilize them at election time" (Powell, 1982, 116). To the extent that party labels are used as information shortcuts by voters to capture a party's redistributive/insurance position, this is an attractive strategy. Labels, ideology and history, however, are not enough. Elections need to be contested and they inevitably revolve around issues, like redistribution and insurance,

Figure 5: Treated Movers and Municipality Characteristics
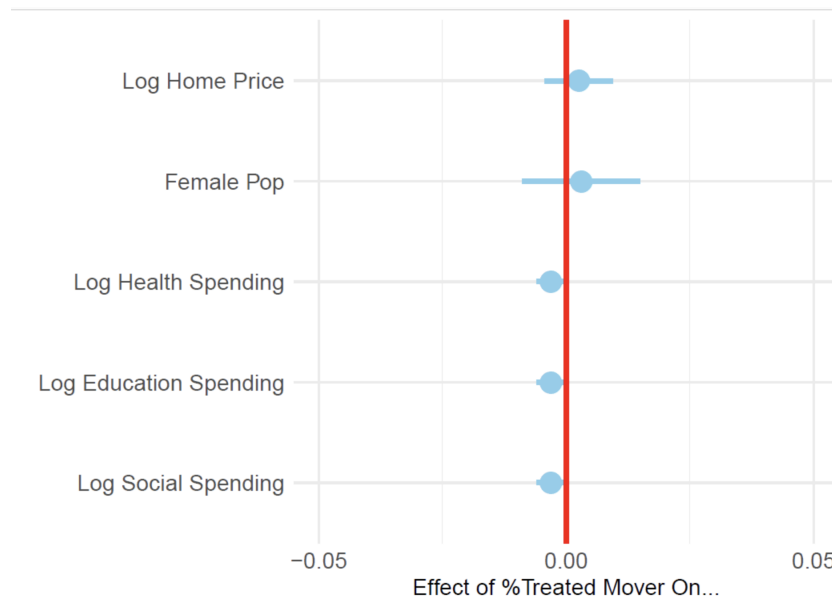
that give political meaning to partisan attachments and social divisions (Dalton, 2002, 195). Moreover, in our analysis, simply classifying parties based on their label might not constitute an accurate operationalization of the concept of redistributive/insurance voting, since election-specific factors 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.[17] 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.[18] We focus our analysis on welfare and health spending proposals as our redistributive/pro-insurance policies.

We proceed by classifying a party as redistributive/pro-insurance if it occupies a policy position above the country-election specific redistribution policy mean (when it proposes more redistribution than the hypothetical average party) or median (when it proposes more redistribution than the hypothetical median party).[19] 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 Appendix C are consistent with, even mildly stronger than, the ones in our main specification. Overall, the results of this exercise favour the interpretation that an insurance-related mechanism is behind our finding that the proportion

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

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

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

of treated individuals influences Left support.

6.7   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 defence, because of the possible connection between war experiences and military 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. Table 7 presents results on defence spending (models (1)-(4)) and law and order (models (5) to (8)). As the table makes clear, parties supporting these policies do not have higher levels of electoral success in municipalities with higher percentages of treated populations.

Table 7: Spending Support on Unrelated Policy Areas

	(1) Defense	(2) Defense	(3) Defense	(4) Defense	(5) Order	(6) Order	(7) Order	(8) Order
Δ Treat (SD)	0.005	-0.007***	0.003	-0.010***	-0.000	-0.002	-0.000	-0.002
	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)
Δ Control (SD)	0.011***	0.007***	0.010***	0.006***	0.004***	0.003**	0.004***	0.003**
	(0.003)	(0.002)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Measure	Average	Average	Median	Median	Average	Average	Median	Median
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1,904	1,904	1,904	1,904	1,904	1,904	1,904	1,904
R-squared	0.604	0.685	0.544	0.649	0.823	0.829	0.823	0.829

NOTE: Outcome is vote share (rescaled between 0 and 1) obtained by all parties classified as being above the mean or median based on their proposed budget spending on defense and order. 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, except 2003 which was a snap election and for which data is not available. Controls include the log of average home prices, municipality spending, proportion of women, foreign population. Standard errors are clustered at municipality level. *** p<0.01, ** p<0.05, * p<0.1

Conclusion

We will not restate in this conclusion the theoretical arguments and empirical findings provided in the previous sections. Suffice it to say that our main finding is that famine exposure increases (aggregate) support for the Left. We believe this paper to make a contribution to the existing literature by establish a link between risk and support for the Left (and not just for the influence on risk on redistribution preferences) and by identifying the *long-term* effects of exposure to shocks (when studied, the effect of shocks have been shown to be short lived by Margalit 2013). We complement other work exploring the long-term political effects of historical events (Homola et al., 2020; Rozenas and Zhukov, 2019; Charnysh and Finkel, 2017; Dinas et al., 2019). And we address a missing link in the economics literature between malnutrition's effect on labour market or health outcomes and insurance demand (Scholte et al., 2015; Conti et al., 2019).

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A   Treatment Distribution

Figure A1: Variation in Treatment Proportion
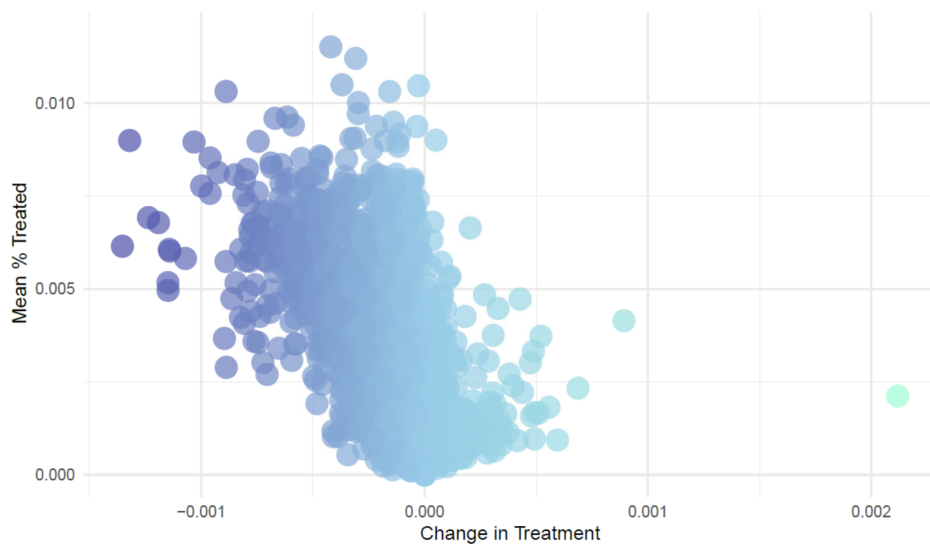
NOTE: Between any 2 elections, about 66% of municipalities experience a *decrease* in the proportion of treated, about 0.3% experience no change and the remaining 33% experience an *increase*.

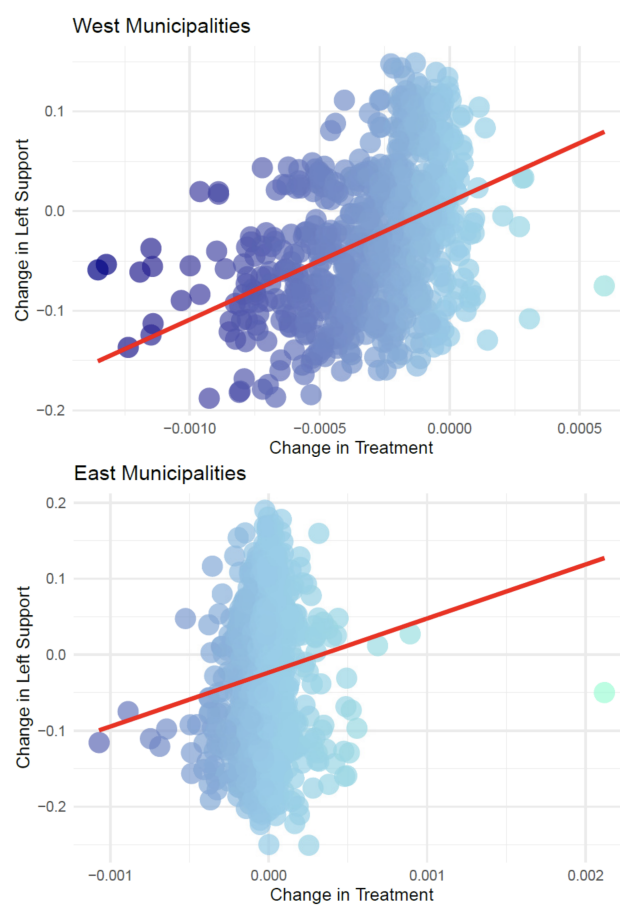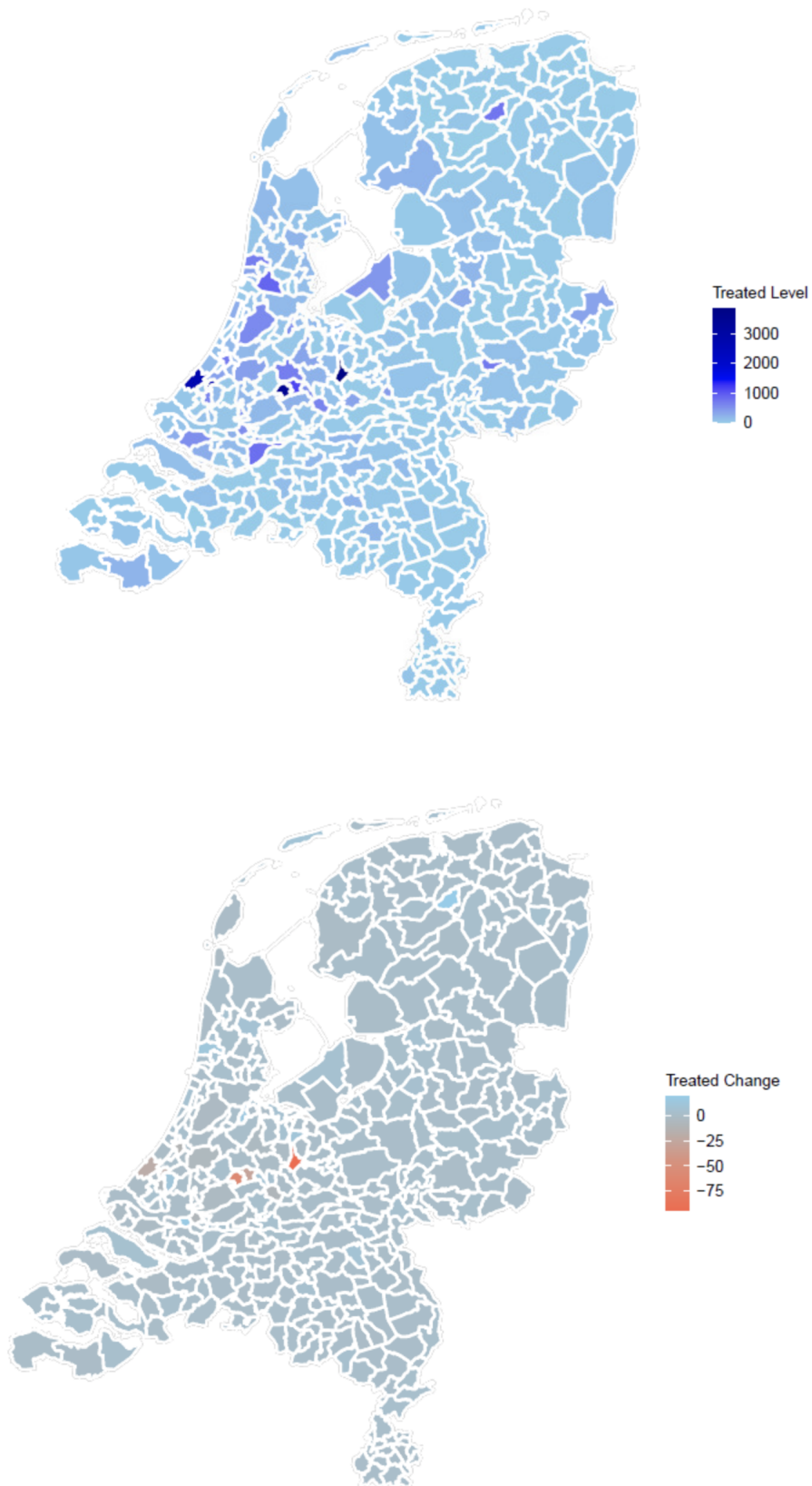Figure A2: Left Vote Share and Δ% Treated Within West and East

Figure A3: Average **Levels** and **Change** of Treated

B   Alternative Treatment and Control Definition

Table A1: Famine Treatment Effect on Left Vote Share Excluding the South

	(1)	(2)	(3)	(4)
Δ Treat (SD)	0.008**	0.018***		
	(0.003)	(0.003)		
Δ Control (SD)	-0.013***	-0.010***		
	(0.003)	(0.003)		
% Treat (SD)			0.010**	0.014***
			(0.004)	(0.004)
% Control (SD)			-0.002	-0.002
			(0.007)	(0.007)
Controls	No	Yes	No	Yes
Mun FE	No	No	Yes	Yes
Election FE	Yes	Yes	Yes	Yes
Observations	2,281	2,281	2,687	2,687
R-squared	0.336	0.451	0.865	0.866
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, excluding the South of the Netherlands which was liberated first. All elections between 1998 and 2017 are included. Controls include the log of average home prices, proportion of women, foreign population, municipality spending. Standard errors are clustered at municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table A2: Famine Treatment Effect on the Left Vote Share (Alternative Urban Definition)

	(1)	(2)	(3)	(4)
Δ Treat (SD)	0.008**	0.017***		
	(0.003)	(0.003)		
Δ Control (SD)	-0.010***	-0.006**		
	(0.003)	(0.003)		
% Treat (SD)			0.009**	0.013***
			(0.004)	(0.004)
% Control (SD)			0.003	0.003
			(0.006)	(0.006)
Controls	No	Yes	No	Yes
Mun FE	No	No	Yes	Yes
Election FE	Yes	Yes	Yes	Yes
Observations	2,281	2,281	2,687	2,687
R-squared	0.331	0.444	0.865	0.866
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 30,000 inhabitants during the famine. All elections between 1998 and 2017 are included. Controls include the log of average home prices, municipality spending, proportion of women and foreign population. Standard errors are clustered at municipality level. *** p<0.01, ** p<0.05, * p<0.1

C   Alternative Left Definition

To measure the redistribution dimension we use the economic tax-spending dimension (Benoit and Laver, 2006, 2007). Positive statements include those referring to market regulation, economic planning, protectionism, controlled economy, nationalization, welfare, education, and labor groups. Negative statements refer to freemarket economy, incentives, (against) protectionism, economic orthodoxy, and (against) welfare. For the welfare dimension, we rely on welfare state expansion and welfare state limitation statements. Finally, for the insurance dimensions, we rely on positive incentives, negative protectionism, market regulation, economic planning, controlled economy, welfare state expansion and welfare state limitation, labour groups positive, and labour groups negative.

Table A3: Famine Effect on Left Support (CMP Redistributive)

	(1)	(2)	(3)	(4)
Δ Treat (SD)	0.013***	0.020***	0.009***	0.015***
	(0.003)	(0.002)	(0.003)	(0.002)
Δ Control (SD)	-0.006**	-0.004*	-0.008***	-0.006**
	(0.003)	(0.002)	(0.003)	(0.003)
Measure	Average	Average	Median	Median
Controls	No	Yes	No	Yes
Observations	2,281	2,281	2,281	2,281
R-squared	0.670	0.716	0.643	0.689

NOTE: Outcome is vote share (rescaled between 0 and 1) obtained by all parties classified as being above the mean or the median based on their position on redistribution in the Comparative Manifesto Project. 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. Controls include the log of average home prices, municipality spending, proportion of women, foreign population. Standard errors are clustered at municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A4: Famine Treatment Effect on Left Vote Share (CMP Pro-Welfare)

	(1)	(2)	(3)	(4)
Δ Treat (SD)	0.011***	0.015***	0.012***	0.018***
	(0.002)	(0.002)	(0.003)	(0.002)
Δ Control (SD)	-0.004*	-0.003	-0.005*	-0.003
	(0.002)	(0.002)	(0.003)	(0.002)
Measure	Average	Average	Median	Median
Controls	No	Yes	No	Yes
Observations	2,281	2,281	2,281	2,281
R-squared	0.630	0.676	0.406	0.496

NOTE: Outcome is vote share (rescaled between 0 and 1) obtained by all parties classified as being above the mean or the median based on their position on pro-welfare in the Comparative Manifesto Project. 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. Controls include the log of average home prices, municipality spending, proportion of women, foreign population. Standard errors are clustered at municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A5: Famine Effect on Left Support (CMP Insurance)

	(1)	(2)	(3)	(4)
Δ Treat (SD)	0.017***	0.016***	0.011***	0.018***
	(0.002)	(0.002)	(0.003)	(0.002)
Δ Control (SD)	-0.005**	-0.004**	-0.006**	-0.004*
	(0.002)	(0.002)	(0.003)	(0.002)
Measure	Average	Average	Median	Meidan
Controls	No	Yes	No	Yes
Observations	2,281	2,281	2,281	2,281
R-squared	0.775	0.788	0.397	0.495

NOTE: Outcome is vote share (rescaled between 0 and 1) obtained by all parties classified as being above the mean or the median based on their position on insurance in the Comparative Manifesto Project. 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. Controls include the log of average home prices, municipality spending, proportion of women, foreign population. Standard errors are clustered at municipality level. *** p<0.01, ** p<0.05, * p<0.1

CPB analyzes the economic impacts of the election manifestos of the political parties, at their request. It aims to provide voters a more objective manner to compare parties. These projections play a key role in elections and most established parties (large and small) submit their manifesto for assessment. The main exception is the far right party: they submitted in 2 out of the 4 elections they competed in. Note that we have no data for 2003 because the CPB could not analyze the manifestos for these snap elections.

Table A6: Famine Effect on Party Support (Budget Health Spending)

	(1)	(2)	(3)	(4)
Δ Treat (SD)	0.006***	0.011***	0.020***	0.021***
	(0.002)	(0.002)	(0.003)	(0.002)
Δ Control (SD)	-0.006***	-0.005***	-0.006**	-0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
Measure	Average	Average	Median	Median
Controls	No	Yes	No	Yes
Observations	1,904	1,904	1,904	1,904
R-squared	0.631	0.669	0.720	0.751

NOTE: Outcome is vote share (rescaled between 0 and 1) obtained by all parties classified as being above the mean (models (1) and (2)) or the median (models (3) and (4)) based on their proposed budget spending on health care. 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, except 2003 which was a snap election and for which data is not available. Controls include the log of average home prices, municipality spending, proportion of women, foreign population. Standard errors are clustered at municipality level. *** p<0.01, ** p<0.05, * p<0.1

Table A7: Famine Treatment Effect on Party Support (Budget Welfare Spending)

	(1)	(2)	(3)	(4)
Δ Treat (SD)	0.014***	0.012***	0.017***	0.014***
	(0.002)	(0.001)	(0.002)	(0.002)
Δ Control (SD)	-0.004***	-0.004***	-0.004*	-0.004**
	(0.002)	(0.001)	(0.002)	(0.002)
Measure	Average	Average	Median	Median
Controls	No	Yes	No	Yes
Observations	1,904	1,904	1,904	1,904
R-squared	0.898	0.903	0.810	0.821

NOTE: Outcome is vote share (rescaled between 0 and 1) obtained by all parties classified as being above the mean (models (1) and (2)) or the median (models (3) and (4)) based on their proposed budget spending on welfare. 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, except 2003 which was a snap election and for which data is not available. Controls include the log of average home prices, municipality spending, proportion of women, foreign population. Standard errors are clustered at municipality level. *** $p<0.01$, ** $p<0.05$, * $p<0.1$