Impact of Famine Exposure in Utero on Labor Market Behavior and Health Later in Life

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Abstract

Scholte et al. (2015) reported in a cross-sectional study that exposure to the Dutch famine in the third trimester of gestation reduces the individual employment rate later in life. We re-examined these findings in our longitudinal study that traced individuals from 18-63 years. To acknowledge the dynamic nature of labor market changes we focus on the impact of the famine on the timing of becoming disabled or retired. We account for selective fertility by restricting our analysis to those conceived before the famine. We also investigate the impact of the famine on health related outcomes, prescribed medications and health cost, later in life.

For the empirical analysis we linked the data on the military recruits linked to the Dutch mortality register (deaths trough 2014) and to monthly individual on the labor market status (1999-2014), annual income (2003-2014), prescribed medications (2006-2014), and, insured health costs (2009-2014).

In contrast to Scholte et al. (2015) we do not find any significant impact of the famine on labor market behavior. Neither do we find significant impact of the famine on income earned or health expenditures. We do find that exposure to the famine in the third trimester of gestation increases the probability of using medications for both Cardiovascular diseases and mental illness.

JEL classification: I10, J13, I14, I24.
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1 Introduction

An abundant economic literature has provided evidence that early life health, even as early as in utero, affects outcomes later in life, including employment and earnings (Currie, 2009, Almond and Currie, 2011). The literature offers several theories for a relationship between early life events and outcomes later in life. The *fetal-origins hypothesis* (Barker, 1995, Almond and Currie, 2011, Almond et al., 2017) suggests a direct link from in utero circumstances to adult health that may be independent of social class in adult life. *Life course models* Ben Shlomo and Kuh (2002) assume that health (socioeconomic) risks may accumulate over the life course. Recent studies for the UK (Case et al., 2005) and the US (Case and Paxson, 2008) show that good health during early childhood lead to a higher economic status later in life.

In recent years, there has been increasing awareness of the potential of using famine exposure to measure the long-run effects of in utero malnutrition. The Dutch Famine (Hunger Winter) of 1944-45 with civilian starvation caused by war conditions (Lumey et al., 2011), provides a unique opportunity to analyze the the effect of maternal decreased nutritional intake during pregnancy on later life outcomes. The famine struck unexpectedly in a society without prior food shortages, the affected population had major problems obtaining food elsewhere. The famine was well defined in time (7 months) and space (cities in the West). Population was ethnically homogeneous without prior differences in dietary patterns. The details of this famine has been well documented (Stein et al., 1975, Burger et al., 1948, Lumey and van Poppel, 1994). After the German surrender in May 1945 the famine was soon over.

In this paper, we study the long-term causal effects of the Dutch Famine on labor market and health behavior later in life. We focus on the timing of labor market changes and how it is influenced by exposure to the famine in utero. We will also investigate the impact of the famine on medication use and health costs when aged 60-65.

A few papers have investigated the impact of exposure to famine in utero on labor market outcomes later in life. Chen and Zhou (2007) estimated the impact of the great 1959-1961 famine in China on labor supply of the survivors They found that exposure to the famine reduced individual's working hours and income earned. Neelsen and Stratmann (2011) examined the long-run impact of the Greek 1941-9142 famine on human capital development. They found that exposure to this famine at young ages has impaired the educational attainment. Scholte et al. (2015) found that exposure to the Dutch famine in the third trimester of gestation reduces the individual employment rate later in life.

There are some important differences with their approach. In contrast to Scholte et al. (2015) we do not look at the impact of the famine on the employment probability, but at the impact of the famine on the hazard rate of becoming disabled or retired. Just as Scholte et al. (2015) we do not observe the labor market history up till the first observation of the labor market status in January 1999, when the affected individuals are around 55 years of age. The chance to work at this age is very much related to his labor market experience. The timing of disability and retirement is less influenced by labor market experience. Receiving disability benefits shows that somebody is not able to work due to health problems and early retirement is often also related to health problems. By only looking at the probability of occupying a particular state at a particular time neglects, as Scholte et al. (2015) do, the dynamic nature of labor market behavior. By using the hazard rate of a labor market transition to investigate the impact of famine exposure we acknowledge this dynamic nature. A hazard rate model also takes (right)censoring, the fact that some events may not have occurred before the end of the observation window, into account. The parametric hazard model we formulate also allows us to account for disability or (early) retirement before the first observation time, so called left-truncation.

Another issue ignored by Scholte et al. (2015) is selective fertility due to the famine. The fertility in the Netherlands dropped dramatically in the months after the famine (and spiked a year later). It is very likely that women with less access to food were less likely to become pregnant, which implies that the men

who were born have a more health/wealthy background. We do not observe the exact food intake, but by restricting our sample to those conceived before the famine struck in our final analysis we, effectively rule out selective fertility.

A final issue ignored by Scholte et al. (2015) is the possibility of selective survival. It has been shown that the famine increases mortality (Ekamper et al., 2014, 2015). Other factors that influence the labor market behavior may also affect mortality, e.g. education attained and intelligence. Neglecting such selective survival may bias the estimation. In our analyses we will account for possible selective survival.

In our empirical analyses we use administrative data on Dutch men who were examined for military service in the Netherlands between 1961-1965 after completing their secondary schooling. The military records include a standardized recording of demographic and socioeconomic characteristics such as education, father's occupation, religion, family size, and birth order, along with a standardized psychometric test battery. These data were linked to the Dutch death register (till the end of 2014) and to register data. We have access to monthly information of the labor market status (main source of income, observed from 1999 till 2014), including (self)employment, unemployment benefits, social security benefits, disability benefits and pensions, to annual personal income to annual prescribed medications 2006-2014 and to annual health expenditures 2009-2014. The death register provides the age at death.

2 Background of the Famine and Data

Prior to World War II, food standards had been high in the Netherlands, both in terms of caloric value as well as composition of the diet. There were no notable disruptions in food availability during the last years of the occupation of the Netherlands, which started in May, 1940. In September 1944, parts of the South of the country were liberated, and the London-based Dutch Government in exile called out a railroad strike in the occupied parts of the Netherlands as an attempt to display authority over the inhabitants of the occupied parts. In response, the occupying forces moved away the means of transportation from the occupied parts, effectively initiating an embargo on food transports to the densely populated western part of the country, i.e. the provinces of North and South Holland and in combination with the early onset of the harsh winter of 1944/45, the freezing of waterways, and the generally bad state of transport infrastructure, this effectively closed off the western part of the country from any imports of food, fuel, medication etc. This triggered the Dutch hunger winter. Individuals had to live on rations as low as 500 kcal per day. For school children, average rations amounted to 664 kcal in the first quarter of 1945. The situation lasted until the end of the occupation which coincided with the end of World War II (early May 1945).

Data from a large sample from the nationwide Dutch Military Service Conscription Register for the years 1961—1965 and male birth cohorts born around the famine, 1944—1947, are analysed. All men, except those living in psychiatric institutions or in nursing institutes for the blind or for the deaf-mute, were called to a military service induction exam. The majority attended the conscription examination at age 18. We have information from the military examinations for 45,037 men. These individuals were originally sampled to study the relation between prenatal famine exposure and mortality Ekamper et al. (2014). For this reason all the 25,283 men born in the Western Netherlands between November 1944 and March 1946 were included in the subsample. The remaining linked data are composed of a random sample of 10,667 individuals who were born in the same cities but before November 1944 or after March 1946, and a random sample of 9,087 individuals who were born in a different part of the Netherlands in 1944 – 1947. Details of the data are described elsewhere, Ekamper et al. (2014), here we provide the main characteristics. These data were linked to the Dutch death register through to the end of 2014 using unique personal identification numbers. Follow-up status was incomplete (due to emigration and other right-censoring events) for 1,316 (2.9%) and entirely unknown for 2,625 (5.8%) men. The latter were removed from the data. These data allow us to follow a large group of men from age 18 until age 71

or until death. At the military examination a standardized recording of demographic and socioeconomic characteristics such as education, father's occupation, religion, family size, region of birth, and birth order is recorded.

These data were also linked to monthly register data on the labor market status (main source of income, for 1999-2014), annual income, medication use and insured health expenditures. We observe the annual personal gross income for the years 2003-2014. Prescribed medications are observed on annual basis in Anatomical Therapeutic Chemical (ATC) code and classified in three levels: the first indicates the anatomical main group, the second the therapeutic subgroup, and the third the pharmacological subgroup. We focus on six medications groups associated with (1) Diabetes; (2) Cardiovascular diseases (CVD); (3) blood pressure; (4) hyperlipidemia; (5) psycholeptics; and (6) respiratory diseases. We have annual data on eight different health expenditures: costs distinguished by: general practitioner, pharmacy (medication use), dental, hospital, paramedical, medical aids, medical transportation and, mental costs. We focus on the costs for general practitioner (GP), pharmacy (medications), hospital and mental health.

To account for selective fertility, we only include individuals who were already conceived at the start of the famine. In other words, we exclude from the analysis everyone born after July 1945, just like (Conti et al., 2019). We define three treatment groups: exposure starting in the first trimester (born May-July 1945), exposure starting in the second trimester (born February- April 1945), and exposure starting in the third trimester (born November 1944-January 1945). The control group includes those exposed only postnatally in the first months of life (born May-October 1944). We used the same selection criteria as (Conti et al., 2019) for the control cities.

Thus, we have selected our control group of cities through the following steps. Given that the famine has historically affected more the cities, we have first restricted our sample of interest to the 46 municipalities with a population greater than 25,000 inhabitants on January 1, 1940. We have further excluded from this group, : (a) 4 municipalities where the majority of the population was not living in the largest place in the municipality (i.e. to be classified as rural not urban from their population dispersion pattern); (b) 13 municipalities where the population underwent major changes in size since 1930 (either increased more than 50% over the decade or had a decrease in the population after the onset of the war).

- *Famine cities*: Amsterdam, Delft, The hague, Haarlem, Rotterdam and Utrecht.
- *Control-cities*: Alkmaar, Almelo, Amersfoort, Apeldoorn, Bergen op Zoom, Bussum, Deventer, Dordrecht, Eindhoven, Gouda, Groningen, Heerlen, Helmond, Hengelo,'s Hertogenbosch, Kerkrade, Maastricht, Nijmegen, Roosendaal, Tilburg, Zaanstad, Zwolle

Due the original sampling procedure of oversampling those men born in famine cities during the famine the number of men born in the control cities during this period is limited (only a few hundred).

3 Labor market status

During the last four decades exiting from the labor force before the official retirement age of 65 has been quite common in the Netherlands. Up till the 1980s disability and unemployment were the main labor force exit routes for older workers, especially during periods of high unemployment (De Vos et al., 2012, Henkens, 1998). Receiving a disability benefit was both socially more acceptable and financially more attractive than receiving an unemployment benefit. Though several system reforms since the mid-1980s decreased the generosity of the disability (and unemployment) benefits, disability remained an attractive labor force exit route for a long time (De Vos et al., 2012). The first early retirement schemes in the Netherlands were introduced in 1976, mainly in reaction to rising unemployment, and gradually implemented by the various industrial sectors, covering around 75 per cent of the labor force end-1990s

(De Vos et al., 2012, Henkens, 1998). Around 1980 about 60 per cent of the men aged 55-64 years were still working, 25 per cent in disability, and 9 per cent in early retirement. During the 1980s early retirement schemes started to become the most important exit route for older workers, partly caused by an overhaul of the system of social security in 1987 which made it more difficult for companies to lay off their older workers through disability or unemployment (De Vroome and Blomsma, 1991). But for those not entitled to early retirement, disability and unemployment – though less generous – remained early labor force exit routes (De Vos et al., 2012). Since 2000 some effective system reforms have been implemented to reduce the increasing costs of early retirement schemes and stimulate older workers to retire at later age; particularly the termination of tax exemption for early retirement contributions since 2006 (De Vos et al., 2012). The average (early) retirement age for men increased from 60 years in 2000-2006 to 61 in 2007 and since then gradually increased to 63.5 in 2015. Between 2000 and 2015 the percentage of the men aged 55-64 in early retirement and disability both declined from around 20 to 10 percent, the percentage still working increased from 42 to 62 per cent.

Men from birth cohorts 1944-1947 reached age 60 years in the period 2004-2007 and official retirement age 65 years in the period 2009-2012. Men from birth cohort 1947 were subject to the early retirement system reform of 2006.

We start with a simple analysis of the labor market status at 1999 and how it is affected by the famine. Table 1 reports the observed differences in labor market status. Note that the percentages add up to more than 100%, as people may be both working and receiving benefits. Note that the limited number of men born in the control cities is due to sampling procedures (Ekamper et al., 2014).

Table 1: Labor market status in 1999 by famine exposure period: famine cities vs control

	working	disabled	unemployed	retired
	Famine cities, $N = 13,082$			
1st trimester	82.39%	16.45%	7.78%	7.38%
2nd trimester	83.89%	15.84%	7.66%	6.60%
3rd trimester	83.94%	14.74%	7.04%	6.72%
	control cities, $N = 394$			
1st trimester	79.66%	8.47%	10.17%	6.78%
2nd trimester	72.34%	24.47%	10.64%	6.38%
3rd trimester	70.77%	21.54%	12.31%	10.77%
	difference			
1st trimester	2.73%	7.98%	−2.39%	0.60%
2nd trimester	11.40%	−8.22%	−2.25%	−0.30%
3rd trimester	13.17%	−6.79%	−5.26%	−4.05%

We estimate linear probability models with a DID structure to estimate the impact of the famine. The results of this standard DID analysis in Table 2 do not show any significant impact of the famine on the labor market status in 1999.

In contrast to the approach taken by Scholte, we do not look at the impact of the famine on the employment probability further, but focus on the impact of the famine on the timing of becoming disabled or retired. This is essential as the probability of occupying a particular employment state at a particular time ignores the dynamic nature of labor market changes. Figure 1 depicts the development of labor market status by age. At age 52 more than 80% of the men are working, 9% is disabled, 5% is unemployed and 1% has already retired.

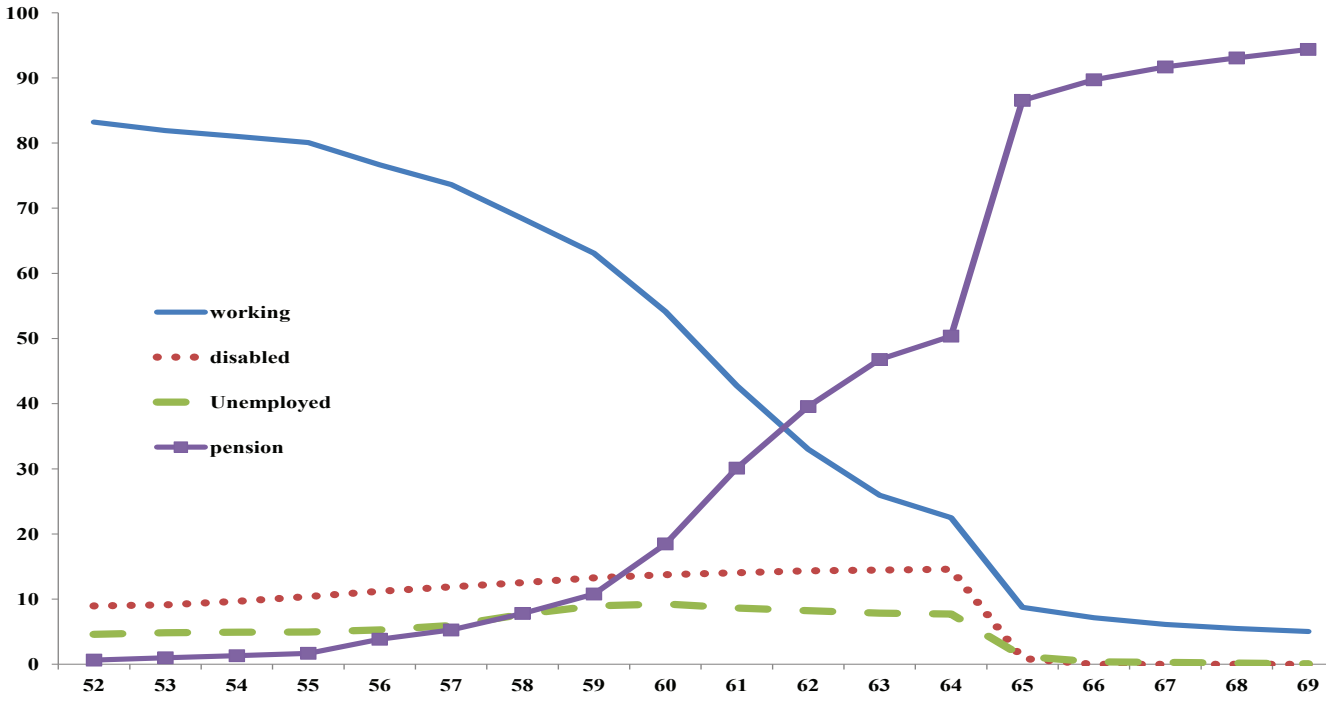
We therefore investigate the timing of a labor market change, into disability or (early) retirement. These two labor market states are clearly influenced by health. We do not consider (un)employment entry

Table 2: Impact of famine exposure on labor market status in 1999

	working	disabled	unemployed	retired
1st trimester	0.12%	12.09%$^{+}$	−1.75%	0.60%
2nd trimester	6.82%	−3.85%	−4.84%	−0.30%
3rd trimester	9.72%	−2.77%	−5.52%	−4.05%

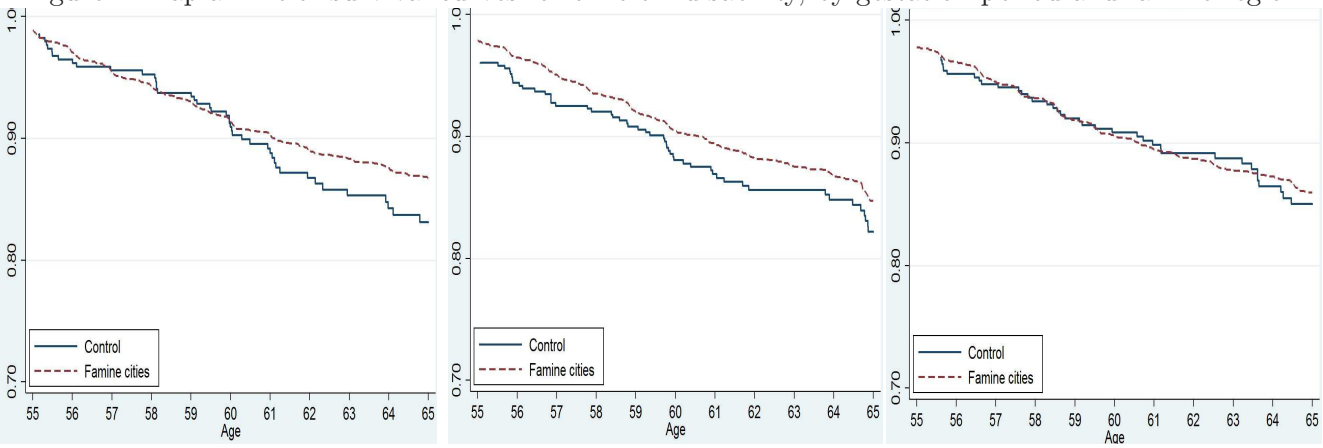
[a] Included controls: Province of birth dummies, number of older brothers, religion dummies, father's occupation (6 cat.), trend and trend squared

Figure 1: Development of labor market status by age



and departure as these labor market dynamics depend heavily on the labor market history from the start at the labor market and we only start observing these men when they are 52 (born in 1947) or 55 (born in 1944). Figure 2 and 3 depict the Kaplan-Meier survival curves by famine region and famine exposure period.

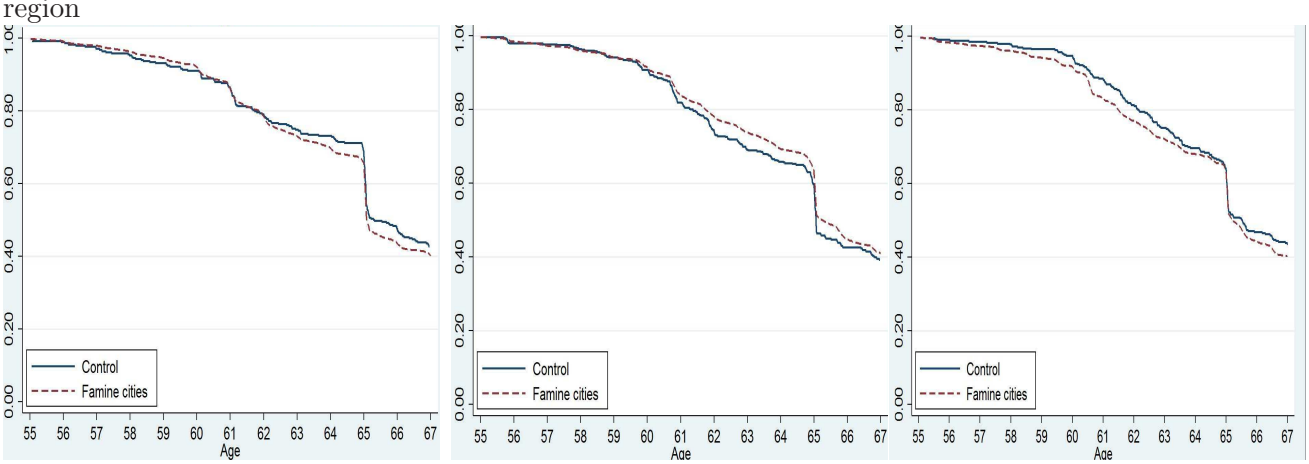
Figure 2: Kaplan-Meier survival curves for time till disability, by gestation period and famine region



3rd **trimester** 2nd **trimester** 1st **trimester**

Notes. 3rd trimester, born November 1944- January 1945; 2nd trimester, born February 1945 - April 1945; 1st trimester, born May 1945 - July 1945.

Figure 3: Kaplan-Meier survival curves for time till (early) retirement, by gestation period and famine region



3rd **trimester** 2nd **trimester** 1st **trimester**

Notes. 3rd trimester, born November 1944- January 1945; 2nd trimester, born February 1945 - April 1945; 1st trimester, born May 1945 - July 1945.

3.1 Methodology

We seek to find the impact of famine exposure in utero on the timing of disability and retirement. We will employ survival models for the timing (the age of entry) of these labor market transitions. Our two main outcome measures are the disability- and retirement hazard, $\lambda(t)$. We model these hazards using a proportional hazard model:

$$\lambda(t|X) = \lambda_0(t)\exp(\beta X + \alpha F + \gamma T + \theta F \cdot T) \tag{1}$$

The baseline hazard (the age-dependence) is either Gompertz, exponentially growing, for the (early) retirement hazard (after age 50) or a piecewise constant baseline hazard on seven intervals (younger than 39 as reference, 39-40, 40-41, 41-42, 42-43, 43-46 and older than 46) for the disability hazard (after age 18).

To account for possible secular trends and differences between the famine cities and control cities in The Netherlands we use a Differences-in-differences (DID)-approach for the famine exposure by using time-dummies T (using three different exposure periods), a place indicator, F (exposed cities in the West) and the interaction of these two $F \cdot T$ to identify the impact of the famine on the hazard rate. The hazard rates are inherently non-linear and the standard DID does not hold. Puhani (2012) derived a non-linear DID can be obtained by

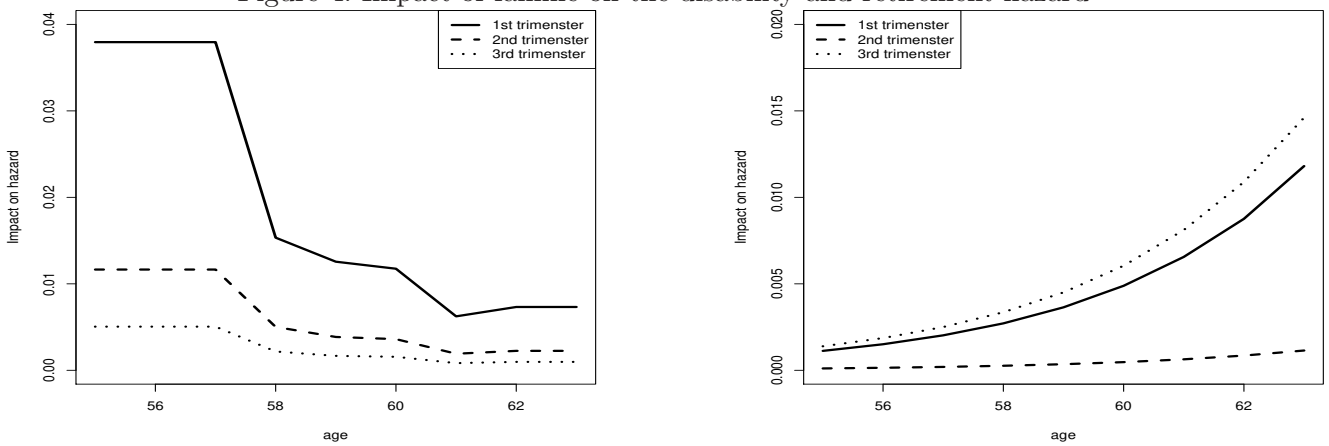
$$\text{DID}(t) = \lambda_0(t)\exp(\alpha + \gamma + \theta + \beta X) - \lambda_0(t)\exp(\alpha + \gamma + \beta X) \tag{2}$$

Note that the effect depends on the age, t, as the baseline hazard is changing with age and on the value of the control variables X. We will calculate the DID(t) for the reference individual, i.e. $X = 0$.

Some of the men have already entered disability (9%) or retirement (1%) at the first observation time, 1-1-1999. Such left-truncation can easily be handled in (parametric) hazard rate models, by adding the probability of such an event to the likelihood. Using the parametric model we have an explicit formula for the probability of having such an event, $F(t_{99}) = 1 - \exp\big(-\int_0^{t_{99}} \lambda_L(s|X, v)\, ds\big)$.

Figure 4 presents the estimated impact of the famine for the three exposure trimesters for disability and retirement. We find positive (but insignificant) impact of the famine on the hazard of becoming disabled and (early) retirement.

Figure 4: Impact of famine on the disability and retirement hazard



Disability **Retirement**

4 Income

We have annual data 2004-2014 on personal gross income (and the main source of income, see Section 3). We only use data on men who are observed on 1-1-2003. Table 3 shows the mean and median of the income by famine region and famine period. We do not observe a significant difference in the income in the raw data.

Based on these data we estimate a log-linear model for the (positive, almost all) income in year 2003 (the first observation year for income). Using Puhani (2012) again we derive a non-linear DID for the exponentiated log-income with normal distributed errors, $\ln y \sim \mathcal{N}(x'\beta, \sigma^2)$.

$$\text{DID} = \exp(\alpha + \gamma + \theta + \beta X + \tfrac{1}{2}\sigma^2) - \exp(\alpha + \gamma + \beta X + \tfrac{1}{2}\sigma^2) \tag{3}$$

The last column of Table 3 presents the estimated DID impact of the famine on annual income in 2009.

Table 3: Annual income in 2003 by famine region and period

	Famine cities, $N = 13,081$		Control cities, $N = 394$		difference		DID[a]
1st trimester	49817	42748	55665	44704	−5848	−1956	−8524
2nd trimester	50379	43793	49384	38125	995	5668	423
3rd trimester	51825	44317	42385	36464	9440	7853	7560

[a] Included controls: Province of birth dummies, number of older brothers, religion dummies, father's occupation (6 cat.), trend and trend squared

5 Prescribed medication

We have data on annual medication prescription (distinguishing medication by the 4-level Anatomical Therapeutic Chemical Classification System, ATC-code) 2006-2013. We start with estimating a probit model for using one of the six important medications (diabetes, blood pressure, CVD, lipid, Psycholeptics or respiratory diseases) in 2006. Using Puhani (2012) again we derive a non-linear DID for (probit) probability of medication use:

$$\text{DID} = \Phi\big(\alpha + \gamma + \theta + x'\beta\big) - \Phi\big(\alpha + \gamma + x'\beta_1\big) \tag{4}$$

The raw data suggest many significant differences between the famine and control cities, see Table 4 .

Table 4: Difference in percentage use of prescribed medication between famine and control cities

	2006	2007	2008	2009	2010	2011	2012	2013
				Diabetes medication				
1st trimester	3.55**	4.07**	4.82**	4.12**	4.75**	5.43**	5.95**	4.18**
2nd trimester	−4.81**	−8.36**	−9.77**	−9.53**	−10.04**	−11.36**	−8.42**	−8.94**
3rd trimester	1.02	−1.22	−0.58	0.93	1.66	2.17	−0.32	−0.24
				CVD medication				
1st trimester	3.94+	−1.73	−1.92	1.23	−4.82+	1.52	−0.34	−0.08
2nd trimester	3.04	−4.16+	−0.52	−1.54	−5.28**	−4.57**	−4.42+	−4.72**
3rd trimester	1.19	0.18	4.14+	4.15+	2.88	5.83**	7.72**	8.37**
				Blood pressure medication				
1st trimester	−1.58	−5.07**	−5.30**	−5.38**	−3.42**	−2.83**	0.82	−3.79**
2nd trimester	−3.35**	−1.86	−6.74**	−3.33**	−2.32**	−1.28+	−1.50+	−1.65+
3rd trimester	−1.65	−0.54	2.10+	−3.63**	−2.29**	−2.55**	−0.86	−1.08
				Lipid medication				
1st trimester	5.39**	7.33**	2.89	7.14**	5.59**	7.81**	3.92+	3.33
2nd trimester	−3.54+	−6.40**	−4.79**	−3.27+	−6.28**	−7.11**	−7.57**	−11.88**
3rd trimester	−4.34**	−5.56**	−3.19	−2.49	−0.32	1.85	−1.74	−0.56
				Psycholeptics medication				
1st trimester	3.52+	−1.98	−2.36	0.81	−3.63	2.55	0.83	−0.94
2nd trimester	4.37**	−3.87+	−1.29	−0.09	−4.74**	−2.93	−3.86+	−3.89+
3rd trimester	1.90	0.56	3.54+	3.47	2.11	6.45**	7.11**	7.57**
				Medication for respiratory diseases				
1st trimester	1.73	−2.83+	−1.43	−1.72	0.13	0.14	−0.52	−2.00
2nd trimester	1.46	4.53**	0.40	1.15	5.89**	−2.37+	2.39+	1.38
3rd trimester	−5.15**	−0.57	−1.56	−4.02**	−2.01	−1.06	−5.28**	−3.76**
Famine cities $N =$	12,450	12,235	12,005	11,772	11,505	11,146	10,690	9,936
Control cities $N =$	378	375	370	364	355	340	327	301

$^{+}p < 0.05, ^{**}p < 0.01$.

However, the estimated DID impact of famine on medication use in Table 5 is only significant for CVD and mental illness (Psycholeptics medication) if exposed to the famine in the third trimester of gestation. For both medication categories the famine increases the use of these medications later in life.

Table 5: Impact of famine on the probability of prescribed medication (DID)

	2006	2007	2008	2009	2010	2011	2012	2013
				Diabetes medication				
1st trimester	1.50	1.95	1.52	1.05	1.37	1.94	2.28	1.14
2nd trimester	−1.02	−3.58	−3.53	−4.05	−4.29	−3.18	−3.10	−3.48
3rd trimester	1.09	0.37	0.29	0.60	1.08	1.88	1.10	0.55
				CVD medication				
1st trimester	9.24	4.78	5.90	9.91	5.85	12.63	9.65	13.23
2nd trimester	9.23	3.98	8.48	8.41	5.38	7.14	5.22	8.26
3rd trimester	6.65	5.61	10.78	12.10^{+}	11.57^{+}	15.84^{+}	16.14^{+}	19.79**
				Blood pressure medication				
1st trimester	−0.53	−7.36	−6.51	−0.88	−1.42	−8.43	−0.77	−0.91
2nd trimester	−1.16	−3.54	−8.63	−0.48	−1.01	−5.49	−5.07	−0.34
3rd trimester	−0.97	−3.36	2.81	−0.83	−1.03	−9.25	−4.30	−0.30
				Lipid medication				
1st trimester	6.47	6.01	5.78	8.91	10.67	11.05	5.48	6.63
2nd trimester	0.59	−2.93	−0.23	−0.35	0.30	−2.21	−6.90	−6.70
3rd trimester	0.62	−2.81	1.08	0.91	6.09	6.50	−0.33	3.67
				Psycholeptics medication				
1st trimester	8.07	5.22	5.64	10.01	6.59	13.70	11.02	11.84
2nd trimester	9.35^{+}	4.74	7.78	10.00	5.55	8.52	5.80	8.33
3rd trimester	6.57	6.89	10.37	12.05^{+}	10.72	16.39^{+}	15.68^{+}	18.34**
				Medication for respiratory diseases				
1st trimester	2.83	−2.11	−5.00	−1.50	−1.02	−2.40	−1.12	−2.43
2nd trimester	−0.03	4.32	−2.49	0.01	6.94	−10.13	1.45	0.24
3rd trimester	−10.74	−1.16	−7.78	−6.54	−6.56	−7.72	−11.50	−8.03

[a] Included controls: Province of birth dummies, number of older brothers, religion dummies, father's occupation (6 cat.), trend and trend squared.
$^{+}p < 0.05$,$^{**}p < 0.01$.

6 Insured health expenditures

Most economic analyses of health costs have adopted the two-part model (Mullahy, 1998, Smith et al., 2017). A two-part models treats the probability of positive costs and the expected costs conditional on positive costs separately. It assumes that the probability of positive costs is governed by a binary probability model like a probit or logit and a conditional regression model for the positive costs. Health cost data are usually heavily skewed and exhibit excess kurtosis. Empirical applications of the two-part model for health costs have often used logarithmic transformation to account for this. For the probability of positive health costs we use probit models, with $\Pr(Y > 0|x) = \Phi(x'\beta_1)$ and for the health costs log-linear regression with normal distributed errors, $\ln y|y > 0 \sim \mathcal{N}\big(x'\beta_2, \sigma^2\big)$. The expected health costs based on such a model is

$$\mathrm{E}[y|x] = \Phi(x'\beta_1)\exp\big(x'\beta_2 + \tfrac{1}{2}\sigma^2\big)$$

From this and using (Puhani, 2012) again we derive the DID of the famine as:

$$\mathrm{DID} = \Phi\big(\alpha_1+\gamma_1+\theta_1+x'\beta_1\big)\exp\big(\alpha_2+\gamma_2+\theta_2+\beta_2 X+\tfrac{1}{2}\sigma^2\big) - \Phi\big(\alpha_1+\gamma_1+x'\beta_1\big)\exp\big(\alpha_2+\gamma_2+\beta_2 X+\tfrac{1}{2}\sigma^2\big) \quad (5)$$

We have data on annual insured health expenditures 2009-2013 and we only focus on expenditures on general practitioner (GP), hospital, medication costs and mental costs. Table 6 reports for each expenditure category the percentage individuals with positive cost and the mean costs for individuals with positive costs, by famine region and famine exposure period.

Table 6: Insured health expenditures by famine region and period, 2009

	% costs > 0			mean conditional on costs > 0		
	Famine cities	control	difference	Famine cities	control	difference
			GP			
1st trimester	98.66%	99.99%	−1.34%**	145	147	−2
2nd trimester	99.24%	98.72%	0.53%	148	159	−10
3rd trimester	99.07%	95.16%	3.91%**	145	146	−1
$N =$	11,547	350		11,433	346	
			Hospital			
1st trimester	74.20%	71.70%	2.50%	2603	1761	842
2nd trimester	72.69%	75.64%	−2.95%	2831	3318	−488
3rd trimester	72.43%	70.97%	1.46%	2970	3205	−235
$N =$	11,547	350		8,454	267	
			Medications			
1st trimester	83.91%	83.02%	0.89%	730	527	203
2nd trimester	83.60%	87.18%	−3.58%**	722	731	−8
3rd trimester	82.12%	79.03%	3.09%+	756	955	−181
$N =$	11,547	350		9,609	294	
			Mental			
1st trimester	2.74%	1.89%	0.85%	3304	155	3149
2nd trimester	3.75%	3.85%	−0.10%	8737	2876	5861
3rd trimester	3.09%	1.61%	1.48%**	6732	1481	5251
$N =$	11,547	350		367	14	

$^{+}p < 0.05, ^{**}p < 0.01$.

In the raw data we find a few significant difference in the probability of positive health costs between the famine and control cities. However, the results from our DID analysis using a two-part model, shown in Table 7, do not indicate any significant impact of the famine on health costs.

Table 7: Impact of famine on insured health expenditures

	Expenditure category			
	GP	Hospital	Medications	Mental
1st trimester	1.8	255.6	115.4	73.0
2nd trimester	−8.6	−182.8	51.7	66.9
3rd trimester	−1.4	−221.2	−61.9	88.4

[a] Included controls: Province of birth dummies, number of older brothers, religion dummies, father's occupation (6 cat.), trend and trend squared.
$^{+}p < 0.05, ^{**}p < 0.01$.

7 Conclusion (preliminary)

In contrast to Scholte et al. (2015) we do not find any significant impact of the famine on labor market behavior. Neither do we find significant impact of the famine on income earned or health expenditures. We do find that exposure to the famine in the third trimester of gestation increases the probability of using medications for both Cardiovascular diseases and mental illness.

We can think of a couple of reasons why we cannot confirm the negative employment effects of the Dutch famine found by Scholte et al. (2015). First, we account for is selective fertility due to the famine. The fertility in the Netherlands dropped dramatically in the months after the famine. It is very likely that women with less access to food were less likely to become pregnant, which implies that the men who were born have a more health/wealthy background. Second, we use a different set of control cities than Scholte et al. (2015). The only use Breda, Eindhoven Enschede, Groningen, Heerlen, Hengelo, Leeuwarden, Maastricht, Tilburg and Zwolle as control cities. Unfortunately, the number of individuals born in the control cities in our sample is rather small (only a few hundred after fertility selection). This might be another reason that we do not find any significant famine effects on the employment. Scholte et al. (2015) have a much larger data set (especially in the number of individuals born in control cities), also including women. However, they do not have access to information collected at the military examination, which contains valuable background information, like father's occupation, birth order and religion.

A more substantive difference with Scholte et al. (2015) is that we view the labor market status as a dynamic process instead of a static outcome. We focus on the timing of disability and retirement. We do not consider the timing of employment and unemployment transitions, because these labor market transitions are known to be influenced by the whole labor market history (since entry to the labor market, after finishing education) and, unfortunately, we only start observing the labor market status in 1999, when the men born around the famine are around 55 years old. The employment status in 1999, that Scholte et al. (2015) use as outcome variable, is a snapshot of an evolving process of labour market transitions and therefore ignores important aspects of this process. The results may be different if they had used another year to measure the employment status. They mention that later years are confounded by early retirement. That is one of the reasons why for focus on the retirement process. We also think that retirement and disability are more related to health than employment and therefore more likely to be affected by the famine (if the famine has an health impact).

We are the first to investigate the impact of the Dutch famine on medication use and health expenditures, both health related outcomes. The reason we only find limited significant results might be driven by the limited number of individuals in the control cities (especially for mental medication use and costs). Ekamper et al. (2015) have investigated the impact of the famine on cause-specific mortality and they do not find an increase in death due to cardiovascular diseases. The increased probability of

cardiovascular medication due to the famine we find seems to contradict this. The increased death due to external causes (including suicide) due to the famine Ekamper et al. (2015) have found is in line with our finding of an increased probability of psycholeptics prescription.

A limitation of our study is the rather small number of individuals in the control cities, due to the original sampling procedures. Another limitation is that we do not have information for women as they were never called for conscript military service in the Netherlands. It is, therefore, not possible to compare sex-specific outcomes using our data.

Work in progress[1]

For the labor market analyses we will account for (possible) selective survival till the military examination and till 1-1-1999, the first observation of the labor market status, using either an IPW method with weights based on the probability to reach these times or a Copula approach in which the selective survival is jointly modeled (with correlated unobserved heterogeneity) together with the labor market hazards.

For the income analysis we will robustness by restricting the analysis to labor income only, accounting for selective employment. We will also account for the (possible) selective survival till the military examination and till 1-1-2003, the first observation of the annual income, using either an IPW method with weights based on the probability to reach these times or a Copula approach in which the selective survival is jointly modeled (with correlated unobserved heterogeneity) together with the income equation.

For the prescribed medications analyses we are accounting for selective survival till the military examination and till 1-1-2006, the first observation of prescribed medications, using either an IPW method with weights based on the probability to reach these times or a Copula approach in which the selective survival is jointly modeled (with correlated unobserved heterogeneity) together with medication use.

For the health costs analyses we are accounting for selective survival till the military examination and till 1-1-2009, the first observation of the health expenditures, using either an IPW method with weights based on the probability to reach these times or a Copula approach in which the selective survival is jointly modeled (with correlated unobserved heterogeneity) together with the two-part model for health costs.

We will also investigate whether extending the sample period, to include pre- and post famine period (i.e. ignoring selective fertility) affects the estimated impact of the famine of the different outcomes, using a triple difference (difference-in-difference-in-differences) approach. Finally, we will check wether extending the included control cities affects the results.

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[1]The updated findings will be distributed prior to the conference meeting in Boston

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