The Causal Effect of Job Loss on Health: The Danish Miracle ? *

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September 13, 2017

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Abstract

Unemployment can affect health both through the income shock and through non-pecuniary channels like the loss of self-esteem or the loss of a structured schedule. I investigate whether there is still a causal effect of job loss on health when the unemployment risk is well-insured by policy through generous unemployment insurance, active labor market policies and public health insurance with universal coverage. In a difference-in-difference design, I compare roughly 25,000 workers who are in an establishment that closed in Denmark between 2001 and 2006 to a control group of workers matched on lagged observables. I find that in such a setting job losses driven by establishment closures do not cause any significant effect on health, whether looking at mental health proxies such as antidepressant purchases, severe physical health outcomes that require inpatient care or mortality. I can rule out effects of the order of 2% for most health outcomes and 15% for mortality. My results taken together with prior literature suggest that it is possible, presumably through an adequate set of policies, to make the causal effect of job loss on health negligible.

*For invaluable guidance and support, I would like to thank my advisors Lawrence Katz, Philippe Aghion, Raj Chetty and David Cutler. For very helpful comments I thank Mitra Akhtari, Peter Ganong, Xavier Jaravel, Amanda Pallais, Emmanuel Saez, Gabriel Zucman and seminar participants at Harvard Labor Lunch and the IZA Workshop on the Social and Welfare Consequences of Unemployment. For help with accessing the data I thank Tore Olsen and Nete Munk Nielsen. I gratefully acknowledge support from an NBER doctoral fellowship.

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1 Introduction

Job loss can affect health both through the income shock and through non-pecuniary channels like the loss of self-esteem or the loss of a structured schedule. The seminal work in sociology on the unemployed community of Marienthal, a small town in Austria where the main factory closed in 1930 leaving many people unemployed for a long time, shows how desperate unemployed and their family can become and the many dimensions in life that can be affected, from standards of living to the loss of a sense of purpose or of a social identity (Lazarsfeld et al., 1933) [1]. Fortunately, since the Great Depression, developed countries have implemented some policies to alleviate the burden of unemployment, in particular unemployment insurance (UI). But job loss might also entail some non-pecuniary aspects against which policy cannot provide insurance.

This paper tries to investigate whether there remains a causal effect of job loss on health, and in particular mental health and substance abuse, in a setting where UI is generous, active labor market policies are available and health insurance is universal. The identification strategy relies on establishment closures, which lead to job losses that are arguably exogenous to employees' health. The context is that of Denmark after the implementation of flexicurity policies. The replacement rate of unemployment insurance is 90%, wih a cap, [2] and the maximum potential duration of unemployment benefits, though it has been gradually reduced, remains long: 4 years during the relevant period for this study[3]. Active labor market policies are in place throughout the period [4]. Moreover health insurance is publicly provided with universal coverage.

Using a difference-in-difference design and Danish administrative data, I compare the health of roughly 25,000 workers who experience an establishment closure to that of a control group matched on observables. I find that, on average in Denmark, job losses due to establishment closures that occurred between 2001 and 2006 did not cause large significant health problems.

I focus on people strongly attached to their job: my sample consists of men and women of age between 25 and 60 who have at least 5 years of tenure at their establishment. 25% of my treatment group goes through a period of unemployment in the year of the closure, as opposed to 4% in the control group. They are also more likely to leave the labor force. However, despite a long lasting effect on their wage earnings, the drop they experience in post-tax post-transfer household income is only of 6%. In terms of health, the treatment group is not significantly more likely to purchase antidepressants or other anti-anxiety drugs, which I use as proxy for mental health. I can rule out effects of the order of 2%. I do not observe either any change in their regular

[1] Interestingly for our purpose, for the vast majority, the unemployed studied in that work lost their job as part of a plant closure. Thus we can view their distress as at least not entirely driven by selection biases.

[2] In 2015 the cap was 4135 DKK per week which would be roughly equivalent to 628 USD (or 32,000 USD in annual terms)

[3] In 1994, it was set to 7 years, split between a passive period and an active one, where people had to participate in active labor market programs; from 2001 to 2010, which is the relevant period for this paper, it was 4 years; it is now 2 years.

[4] Active labor market policies started to be implemented by the Social democrats in 1993-1994. For more details see section 3.2.

health care consumption such as the number of visits to the General Practitioner nor any effect on severe physical health outcomes that require inpatient care at the hospital, for which I can rule out respectively effects of the order of 1 and 4%. Mortality is also not significantly affected. [5] The two exceptions for which I find a marginally significant effect are visits to the hospital for alcohol issues [6] as well as purchases of diabetes related drugs, but these results are not very precisely estimated, the baseline mean is very low and the effects might well be false positives due to multiple hypothesis testing.

This paper is related to several lines of research. The most closely related papers, which I discuss further after presenting my results, are those looking at the effect of mass layoffs or plant closures on some health outcomes. Table 1 provides a synthetic summary of the context, methods and results of these papers. Results from this literature are mixed: while some papers find strong effects, in particular on mortality for males (Sullivan and Von Wachter (2009), Eliason and Storrie (2009a), Browning and Heinesen (2012), Rege et al. (2009)), others find a relatively precise zero (Browning et al. (2006), Kuhn et al. (2009)). Part of the variety of the results comes from differences in the precise definition of the treatment and of the control groups as well as sample restrictions and outcomes of interest (for instance many papers focus on mortality for males and I do find a positive point estimate for mortality for males, though not significant) or on some methodological differences (whether or not one includes the deaths that occur in the year of displacement can make a difference). But another part presumably has to do with the fact that the effect of job loss on health depends a lot on the institutional context and it is hard to compare results across countries, or even within country across time.

[5] As I show, there is a strong significant difference in the death hazard of the treatment v. the control group in the year of displacement but some of these deaths could be the cause of the establishment closure rather than caused by the closure. This reverse causality concern seems all the more relevant that the difference is entirely driven by the smallest establishments. Thus, though I report results both with and without the deaths of year 0, my preferred estimates are, as in Sullivan and Von Wachter (2009), the ones that focus on deaths that occurred from year 1 onwards.

[6] This includes both visits to the emergency room for alcohol abuse and inpatient or outpatient care for alcohol dependence

Table 1: Literature review

Paper	*Context*	*Treatment*	*Control group*	*Health outcome(s)*	*Results*
Browning & al. 2006	Denmark 1986-1996	Mass-layoff and plant closure	All other establishments	Diseases of circulatory or digestive system	No effect
Browning & al. 2012	Denmark 1986-2002	Plant closure	Positive growth or less than 10% downsizing	Mortality	Significant effect for males
Eliason & Storrie 2007	Sweden 1987-1988	Plant closure	All other establishments	Mortality	Short run effect for males
Eliason & Storrie 2009	Sweden 1987-1988	Plant closure	All other establishments	Cardiovascular diagnoses Alcohol-related diagnoses	No effect Positive effect
Kuhn & al. 2009	Austria 1999-2001	Plant closure	Positive growth or less than 30% downsizing	Hospitalizations Doctor visits Mental health drugs	No effect No effect Small effect for males
Martikainen & al. 2007	Finland 1989 and 1994	Plant closure or 50% downsizing	All other establishments	Mortality	No causal effect
Rege & al. 2009	Norway 1995-2000	Downsizing by at least 60%	Positive growth establishments	Mortality	9%increase significant at 10%
Sullivan & von Wachter 2009	United States 1980-1986	Downsizing by at least 30%		Mortality	50-100% effect in year 1 10-15% afterwards

Compared to this literature, I contribute by looking at a very wide set of health outcomes with a long period of observation, which allows me to give a comprehensive picture. Moreover I am able to provide direct visual evidence that the treatment and control groups were on parallel trends in terms of health in the five years before the job loss shock. I interpret my results as showing that it is possible, presumably through an adequate set of policies, to make the causal effect of job loss on health very small, if not negligible.

The paper also relates to work on unemployment and subjective wellbeing (Winkelmann and Winkelmann, 1998). This literature has shown that unemployment is associated with lower subjective well-being. My paper adds to this literature by focusing on job losses that are arguably exogenous and by trying to capture more objective but also more severe health conditions. It is possible that job loss leads to lower satisfaction in my setting as well, but if it is not to the point that people start taking anti-depressants or to be in worse health, it is may not be of first order importance for policy to deal with it.

This paper also relates to work on income-health gradients, and in particular recent work by Cesarini et al. (2016) on the causal effect of wealth on health and child development in Sweden. They find that an exogenous increase in wealth (due to winning a lottery) has overall no effect on health, neither on mortality [7] nor on health care utilization. This provides evidence totally in line with what I find that, at least nowadays in Scandinavian countries, the cross-sectional association between health and economic variables is mostly driven by selection.

Finally the paper can relate to rising concerns in the US about addiction to painkillers and the increase in mortality from poisoning, suicide and alcohol related deaths highlighted by Case and Deaton (2015). Data from the International Narcotics Control Board show that Denmark also experienced rising trends in painkillers consumption. [8] Our data allows us to test whether job loss makes people more likely to develop addiction to such substances, to engage in excessive alcohol drinking or to commit suicide. Despite a strong association in the cross-section between unemployment and purchases of opiod painkillers, I do not find any effect on such purchases following an exogenous layoff. However I do find a marginally significant effect of establishment closures on alcohol issues (though not very precisely estimated and starting from a very low baseline mean).

The rest of the paper is organized as follows. Section 2 provides some background information about the Danish context and the data used, as well as the cross-sectional association between unemployment and health in Denmark, controlling for observables. Section 3 describes the empirical strategy to identify the causal effect of job loss on health, while Section 4 presents the results. Section 5 discusses the relationship with the literature while Section 6 concludes.

[7]In particular they are able to rule out effects on mortality one sixth as large as the cross-sectional gradient.

[8]In 2013, Denmark was fifth out of 76 countries in terms of oxycodone consumption but with amounts in mg/capita four times smaller than that of the US who were first; however Denmark was first in terms of hydromorphone consumption.

2 The Danish Data and Context

Denmark is an ideal setting to study the question at hand for several reasons. Because of its generous unemployment insurance, its active labor market policies and its universal public health insurance, it allows us to test whether there remains a causal effect of job loss on health when the unemployment risk is well insured by policies. The results have to be understood as a lower bound of the causal effect of job loss on health in general: the question of interest is to what extent this lower bound can be made zero or negligible. Moreover because of its single-payer health care system with universal coverage, administrative data on health provide a comprehensive and reliable picture of health care consumption, which would be very different in countries like the United States.

2.1 The Data

I use Danish administrative data from several registers covering the period 1996-2013 for the universe of Danes. The data on economic outcomes come from tax data and from matched employer-employee registers. Tax data give in particular annual wage earnings, annual amount of unemployment benefits, post-tax post-transfer income [9]. Employer-employee registers allow to compute, at the establishment level, closures and employment changes. I also use data from unemployment insurance funds from 1996 to 2007, which give UI claims at the weekly level.

The health data come from reimbursements of medical care or related purchases: they include doctor visits; hospital visits/admissions with precise diagnosis; and prescription drugs purchases. In addition I also use death records with the precise cause of death. Table A1 provides a summary of the main registers and variables used. One of the advantage of the data is the many health outcomes it allows to look at. To proxy for poor mental health or addiction, I look at purchases of antidepressants and related drugs as well as purchases of painkillers and at specific causes of deaths like suicide and deaths associated with excessive alcohol consumption (e.g. chronic liver disease). To proxy for severe health conditions, I look at diagnosis of disease of the circulatory system and cancer diagnosis, as well as any inpatient visit to the hospital (treating separately pregnancy related visits). I also look at regular health care consumption such as visits to various types of doctors. Of course health care consumption may fail to capture accurately the health status if people do not seek treatment but i) because health care is basically free there is no liquidity constraints for people to seek treatment and ii) by looking at mortality, I am sure to capture severe health issues that could have gone undiagnosed or untreated.

2.2 The Context

Over the period studied, the unemployment rate was on a declining trend from more than 10% in the early 1990s to 2% right before the crisis. Unemployment had started to rise in the 1970s and the first response to this increase in unemployment was to push old workers out of the labor

[9] All amounts in annual DKK are converted in 2015 DKK using the CPI table from Statistics Denmark http://www.dst.dk/en/Statistik/emner/forbrugerpriser/forbrugerprisindeks

force through early retirement schemes, while maintaining the standard of living of the unemployed through generous and long-lasting social transfers. In 1993, the Social Democrats came to power and started to implement what we now call a "flexicurity" system. [10] They reduced the potential duration of unemployment benefits, which used to be unlimited, to a maximum of seven years: four years of passive support followed by three years of activation. In parallel, contrary to other countries, spendings for active labor market policies were increased, with a significant share devoted to training.

Contrary to many countries, unemployment insurance is voluntary in Denmark. There are 36-state approved unemployment funds to which people must contribute if they want to benefit from unemployment insurance. Most people do insure themselves. 77% of the labor force is a member of such a UI fund and 95% if we consider people with at least 5 years of tenure at their establishment, which is the group I focus on in this paper. However, even if you are not insured, you are entitled to some welfare benefits from the municipalities if you become unemployed. The income measure I use is post-tax post-transfer income such that I abstract from where the redistribution is coming from[11].

2.3 Health and Unemployment in the Cross-Section

Before focusing on the causal estimates of layoff on health, I document the association in the cross-section between unemployment and a few health outcomes. I look at all new UI recipients in a given year 0, who are full-time unemployed and who are between 25 to 60 years old. By new recipients, I mean UI claimants who did not receive UI in the prior year -1. I perform event studies around the year of UI receipt, comparing them to a control group of people who did not receive UI in year 0 but who were fully insured (i.e. who would have received full-time UI had they been fired), and who were also non UI recipient in year -1. I match, without replacement, each UI recipient to a non UI recipient based on gender, exact age and some lagged characteristics of year -1: occupation (6 categories: manager, skilled employee, unskilled employee, blue-collar worker etc) marital status (dummy for being married), region of residence (5 categories), previous unemployment history (dummy for being never unemployed since 1980).

In Figure 1 I just plot, for some outcomes, the difference in means between the two groups over time, taking out individual fixed effects [12]. Patterns are similar if I don't include the individual

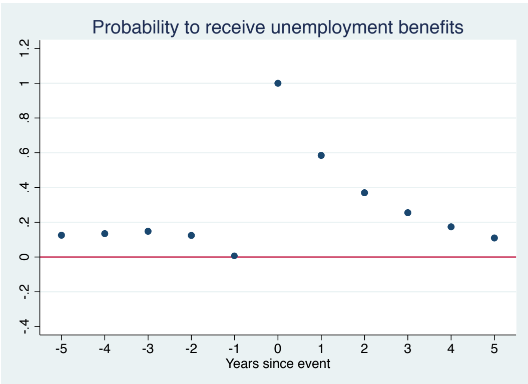
[10]A quick note on the history of the flexicurity system: In 1982 a center-right government came to power under the leadership of Poul Schluter and implemented some liberal reforms (massive liberalisation of capital markets, decentralisation and revamping of the public sector etc) but did not have enough political support to implement a big labor market reform in a time of high unemployment. However cuts in funding for local governments led municipal officials to experiment with active social policies as a way to simultaneously cope with fiscal constraints and enhance the productivity of marginal workers. Schluter ordered a report in 1991 to a tripartite commission, the Zeuthen Commission, which advocated deep structural changes to the Danish unemployment insurance system. In 1993, the Social Democrats returned to power and started to implement the recommendations of the Zeuthen Commission in which they had been involved.

[11]In addition, although UI funds are privately organized, 90% of their resources come from the state

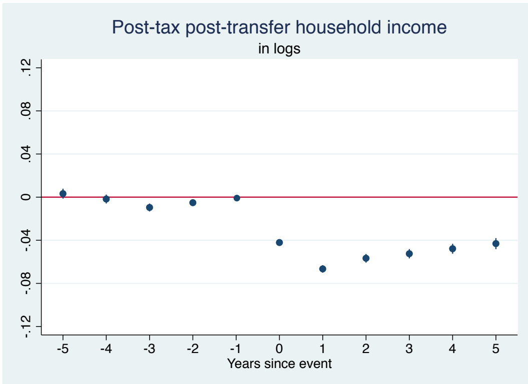
[12]I normalize to zero for both groups the value in year -5, except for UI receipt where I use year - 1 as benchmark

Figure 1: Unemployment and health in the cross-section

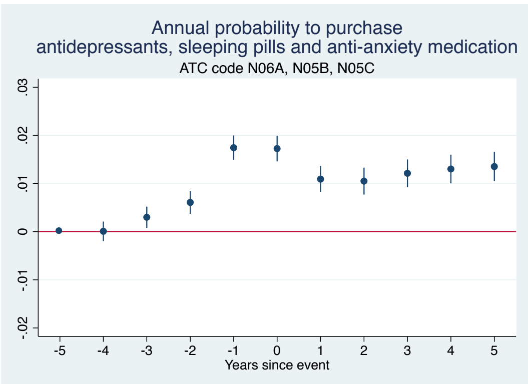
(a) Unemployment benefits



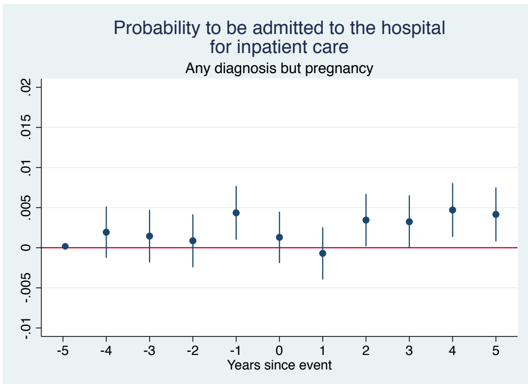
(b) Household disposable income



(c) Mental health



(d) Severe physical health issues



fixed-effects [13]. UI recipients experience a 110,000 (2015) DKK drop in annual wage earnings in year 1, which is equivalent to a 16,500 USD or a 30% drop; earnings then recover but are still 10 % lower (40,000 (2015) DKK) five years out than they were prior to unemployment. In terms of post-tax post-transfer income, the drop, though long-lasting as well, is smaller at impact: around 10% for individual income and 7% for household income. On the figure, I only show the results for household disposable income as this seems to me the most relevant income measure for thinking about health. Figure 1 b also shows that there is no strong sign of declining income prior to unemployment in year 0 and this is also true for wage earnings. This stands in contrast with what happens for many health outcomes. In particular, UI recipients are significantly more likely to have purchased antidepressants, sleeping pills or anti-anxiety medication already 3 years before their UI spell as can be seen on Figure 1 c. The same kind of pattern holds for opioid painkillers. Regarding severe physical health issues, there is a significant increase in the probability to have received inpatient care at the hospital in the year just before UI receipt (see Figure 1 d). This is driven by diseases of the circulatory system. The results shown in Figure 1 are for UI recipients of 2002 but results are very similar if I look at any other year in our sample frame. As seen on Figure 1 a, UI recipients in a given year are more likely to have received UI in some prior years so this could explain why some health issues show up prior to the event year. But even if I restrict the pool of new UI recipients and the control group to people who have never been unemployed since 1980 [14], the patterns just described stay the same (results available upon request).

Table A2 shows that UI recipients are also much more likely to die: on average their 5 years and 10 years mortality rate are roughly 30% higher than that of the control group (columns 1 and 2). Again this is also true if I restrict attention to people with no prior history of unemployment: the effect is actually even bigger (column 3). And the effect is stronger for males (column 4 versus 5). Results are similar whether using a linear probability model or logit regressions. Table A3 shows that there is no significant increase in the probability to die of cancer: the overall average effect is thus mostly driven by deaths from external causes and from diseases of the circulatory system. There is a 100% increase in the 5 years suicide rate and a 150% increase in the 5 years probability to die from an alcohol-related cause. Of course this increased mortality can be the result i) of the drop in income experienced by UI recipients, ii) of a causal effect of unemployment on mortality and/or iii) of the selection of less healthy people into unemployment. More generally the health issues associated with unemployment that I documented in this subsection calls for a careful research design in order to correct for selection into unemployment.

since UI receipt is by construction 0 for both groups in year - 1

[13] Results are also similar whether or not I add additional controls such as an immigrant dummy, very detailed education (over 1,000 categories), time varying detailed marital status, number of years of employment in year -1 (as measured by number of years of social security contribution) and number of years of UI fund membership in year -1

[14] This date is chosen because that's when the data on labor market status start to be collected

3 Identifying the Causal Effect of Job Loss on Health

I use establishment closures as a way to isolate job losses that are exogenous to the employee's pre-existing health. Recent very interesting work by Hilger (2016) argues that people who work at plants that close tend to be selected compared to people who work at plants that do not close. However, as Hilger (2016) says himself, this is worrying when studying the effect of job loss on an outcome that cannot be observed in the pre-period such as mortality or children's adult outcomes because there is then no way to check whether treatment and control groups are on parallel trends in the pre-period. On the contrary my health panel data on a wide range of outcomes allows me to provide direct graphical evidence in support of the common trend assumption and to be thus confident that there is no selection in terms of health into our treatment group.

3.1 Identifying Establishment Closures

Establishment closures. I use a workplace level employment register to identify establishment closures. The data contain a variable which directly codes the status of the workplace in the following year, and in particular whether it closes. This variable also allows me to drop, from both treatment and control groups, establishments that either split or merged. Moreover, for each workplace, the data give for each year of operation, the current workplace identifier but also the workplace identifier in the preceding and in the following year, which enables me to make sure that I do not code as closure a change in workplace identifier. I keep for the treatment group all the establishments which closed between 2001 and 2006 [15], and who had at least 5 employees five years before closure (this excludes 65% of closures). I chose this cutoff to strike a balance between the importance of maximizing power and the need to discard too small establishments for which the closure could be driven by the health of one individual. Results are robust, though less precisely estimated, to using a higher threshold such as for instance 10 employees.

In total, I have 8,233 treated establishments, approximately 1,300 per year, which represents 2.5% of the pool of control group establishments. For this control group, I keep workplaces which operate any time between 2001 and 2006 and who had between 5 and 500 employees five years prior (the 500 cutoff comes from the fact that the biggest establishment which closes in my data had 447 employees 5 years before closure).

Closure year. Following prior work (Eliason and Storrie (2009a) and Browning and Heinesen (2012)) I consider the closure event year to be either the last year of operation of the establishment or one of the 2 preceding ones depending on which of these 3 years saw the biggest reduction in the workforce in absolute value, and making sure that this reduction represents at least 30% of the workforce and at least 3 people. For 70% of establishments the closure year is the last year of operation (for 22% it's the year before and for 8% 2 years before[16]). Given this definition, I also

[15] These years are chosen in order to have at least 5 years of observation in the pre- and post-periods.

[16] The numbers are respectively 58%, 31% and 11% if we do in terms of share of people rather than share of establishments.

have in my treatment group some people who were not laid off in the event year (but who will ultimately lose their job at most 2 years after). They represent 10% of the treatment group. My estimates are thus to be interpreted as Intention-To-Treat estimates. Results are similar if I always consider as closure year the last year of operation but the sample size is almost halved.

To be precise, the employment registers are updated every year at the end of November. Thus when I say event year 2002, I mean that the closure/downsizing/job loss occurred some time between December 1st 2001 and November 30th 2002 and I call 2002 the year 0.

3.2 Matched Sampling Procedure to Identify the Comparison Group

Treatment group. The treatment group is composed of everyone of age 25 to 60 whose primary job is at a treated establishment during the closure year and who has at least 5 years of tenure at this establishment. The tenure restriction allows me to focus on people who are losing a stable job, which is presumably a big shock. I also looked at what happens when I relax this assumption and results are qualitatively similar for instance when including everyone who had at least 2 years of tenure. If an individual experiences two establishment closures over the period 2001-2006, I keep the first occurrence.

Pool of potential controls. The control group consists of all workers at control establishments who satisfy the same age and tenure restrictions as the treatment group.

Matching procedure to select the control group. I use exact matching instead of propensity score techniques following Azoulay et al. (2010), Jaravel et al. (2016) and Jaeger (2016). I perform the matching year by year without replacement. Potential controls can appear in multiple years. For each year, all treated individuals and potential controls are assigned to a strata based on lagged characteristics, five years before. The characteristics I use for these strata are gender, exact age, tenure quintiles, establishment size quintiles, and occupation (6 categories: managers, skilled employees, unskilled employees, blue collar workers). Then for each treated individual, I select randomly a control within the relevant strata. Once a control has been assigned to a treated individual, he is dropped from the pool of potential controls for the subsequent years.

3.3 Summary Statistics

Table 2 presents the summary statistics of the sample in year -1. The difference-in-difference strategy allows for differences in average levels of outcomes variables between treatment and control group and only requires a common trend assumption. However, the summary statistics enable us to assess to what extent the matching procedure has created a balanced comparison group and provides some information to get a sense of the population on which we are estimating the effect of job loss on health. Note that the similarity between the treatment and the control group is not a mechanical effect of the matching since the matching was done on variables in $t-5$.

Table 2: Summary statistics in year -1

	Treatment	Control
Male	0.59	0.59
	(0.49)	(0.49)
Age	45.16	45.16
	(9.13)	(9.13)
Married	0.66	0.69
	(0.47)	(0.46)
Tenure at estab. (in years)	10.71	10.72
	(5.43)	(5.44)
# of years of employment	20.02	20.02
	(4.73)	(4.77)
# of years of UI fund membership	17.77	17.32
	(6.71)	(7.05)
Gross wage (in 2015 DKK)	368,329	365,984
	(185,468)	(158,251)
	≈ 55,249 USD	54,897 USD
Post-tax post transfer household	412,057	421,580
income (in 2015 DKK)	(275,618)	(337,789)
	≈ 61,809 USD	63,237 USD
Estab. size	45,62	54,21
	(67,42)	(79,66)
# visits to G.P.	2.55	2.50
	(3.21)	(3.11)
Takes antidepressants, anti-	0.09	0.09
anxiety medication or sleeping pills	(0.29)	(0.29)
Takes painkillers	0.19	0.18
	(2.00)	(1.83)
Received inpatient care	0.07	0.07
	(0.25)	(0.25)
# of observations	24,234	24,234

3.4 Estimating Equations and Identification

Figures 2 through 4 show the results of event studies, comparing the within individual variation over time for the treatment group and the control group. More precisely, they plot the α_j coefficients from the following specification:

$$Y_{i,t} = \sum_{j=-5}^{6} \alpha_j (DistYear_t = j) \times Treatment_i + \sum_{j=-5}^{6} \beta_j (DistYear_t = j) + Indiv_i + \epsilon_{i,t} \quad (1)$$

where $Y_{i,t}$ is the economic or health outcomes of indiv. i in year t; $Treatment_i$ is a dummy for being in an establishment that closes; $DistYear_t = j$ are dummies equal to 1 if Year t is j years apart from the event year; and $Indiv_i$ are individual fixed-effects. Standard errors are clustered at the individual level. We normalize the α_{-5} coefficient to 0. The common trend identification assumption, which can be very easily checked visually, is that for each $j < 0$ and for each health outcome of interest, the α_js are not statistically significantly different from zero. The effect of job loss on health is then given by the α_js for all $j > 0$.
For the health outcomes for which the event studies look totally flat both before and after the shock, I pool together all the years in the pre- and post-period respectively in order to assess the magnitude of the zero effect with more precision. That is I report the α coefficient from the following difference-in-difference equation:

$$Y_{i,t} = \alpha After_t \times Treatment_i + \beta After_t + \gamma T_t + Indiv_i + \epsilon_{i,t} \quad (2)$$

Results are similar if I replace the individual fixed effects by a $Treatment_i$ dummy.
Mortality specifications. For mortality, we estimate both linear probability models and logit specifications. For linear probability models, I report the α coefficient from equations of the form

$$p_{i,t+1,t+5} = \alpha Treatment_{i,t} + \delta X_i + T_t + \epsilon_{i,t} \quad (3)$$

where $p_{i,t}$ is the probability that individual i dies between year $t + 1$ and $t + 5$ (or sometimes t+10, either of any cause or by cause of death), where t is the event year or placebo event year of individual i. X_i are individual controls and T_t are Year fixed effects. I also show graphically the death probability of both the treatment and the control group, by year, from the year of the event to 6 years out.
For logit specifications, I report the α coefficient from the following equation

$$Ln(\frac{p_{i,t+1,t+5}}{1 - p_{i,t+1,t+5}}) = \alpha Treatment_i + \delta X_{i,t} + T_t + \epsilon_{i,t} \quad (4)$$

Where notations are the same as for equation (3). α gives the increase in the log-odds of death associated with experiencing an establishment closure, holding constant the other variables in the model. Because mortality rates are typically quite small, α can be interpreted as the percentage increase in the 5 years mortality rate.

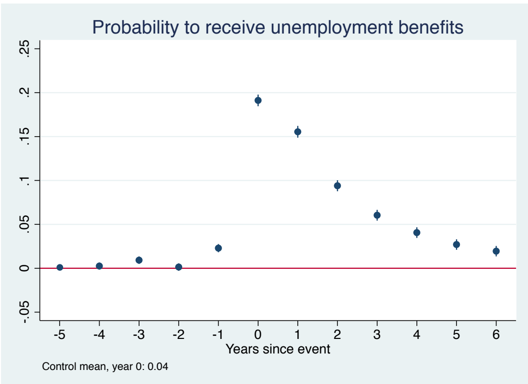
4 The Effect of Establishment Closures on Economic and Health Outcomes in Denmark in the 2000s

4.1 Effect on Economic Outcomes

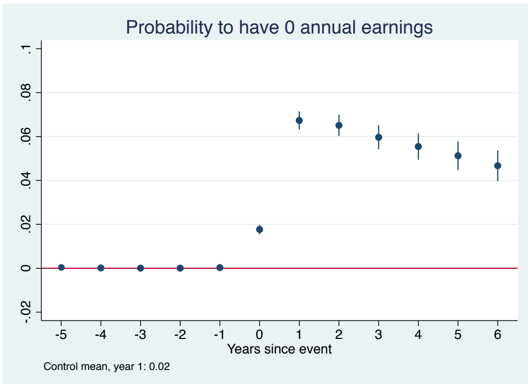
Figure 2(a) shows that workers in the treatment group are much more likely to go through an unemployment spell than those in the control group. 4% of the control group receives unemployment benefits at some point during year 0 as opposed to 24% in the treatment group. Conditional on receiving UI in year 0, their unemployment duration is slightly longer: they receive benefits during 18 weeks in year 0, relative to 15 weeks for the control group. [17] The treatment group is also more likely to have zero annual earnings, which can be seen as a proxy for dropping out of the labor force. This effect is more long-lasting than the unemployment effect, as can be seen on Figure 2(b): the treatment group is on average 7 percentage points more likely to have zero annual earnings in the year following the event, relative to a control mean of 2%, and still 5 percentage points more likely 5 years out.

Figure 2: Effect of establishment closures on economic outcomes

(a) Unemployment



(b) Zero annual wage earnings



(c) Annual wage earnings

(d) Post-tax post-transfer income

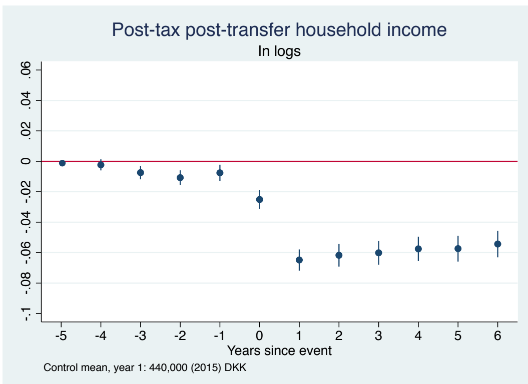


Figure 2(c) and (d) then show the effect of plant closure on earnings and income. Workers who

[17] And in total, still conditional on receiving UI in year 0, the treatment group receives 51 weeks of benefits over a period of 92 weeks, whereas the control group receives 45 weeks of benefits over a 97 weeks period.

experience an establishment closure suffer from a long-lasting drop in annual earnings: on average, 60,000 (2015) DKK in the year following displacement; and their earnings are still 40,000 (2015) DKK lower than that of the control group five years out. That's roughly equivalent to a 9,000 USD or 17 % drop in year 1 and a 6,000 USD or 12% drop in year 5. In terms of household post-tax post-transfer income, which is arguably the relevant economic outcome when thinking about health, the effect is smaller. Workers in the plant closing group experienced a 6% income drop (with the average disposable household income for the control group in the post-period being 450,000 (2015) DKK or 67,500 USD). Most of the insurance against the earnings drop comes from the transfer and tax system, not the spousal adjustment. Indeed if we look at individual, instead of household, disposable income, we observe a drop of 8%. [18]

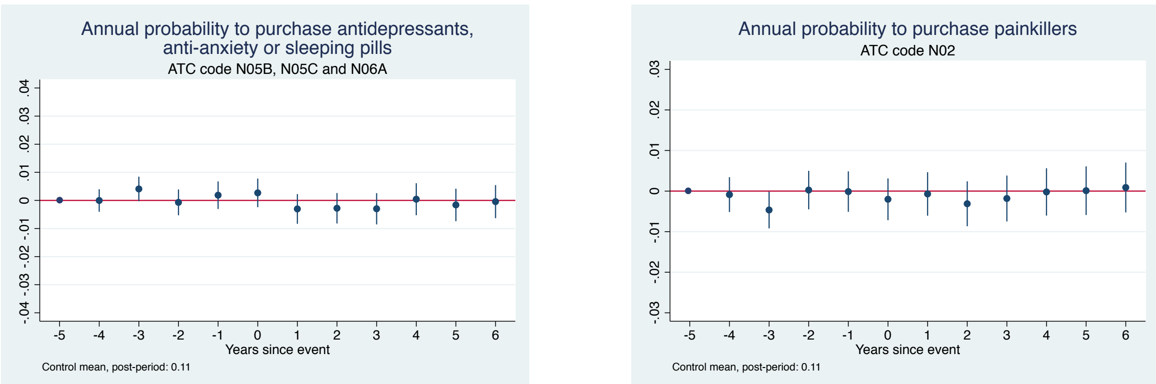
4.2 Effect on Health Outcomes

Figures 3 through 5 and Table 3 through 5 show the effect of establishment closures on some health outcomes. The event studies provide graphical evidence that there is no selection in terms of health in our treatment since there is no significant differences in the pre-period between the treatment and the control group. The regressions help assess the magnitude of the effects by pooling together all the years in the pre- and post-period respectively.

4.2.1 Effect on Mental Health

Figure 3: Effect of establishment closures on prescription drugs purchases

(a) Antidepressants and related drugs (b) Painkillers

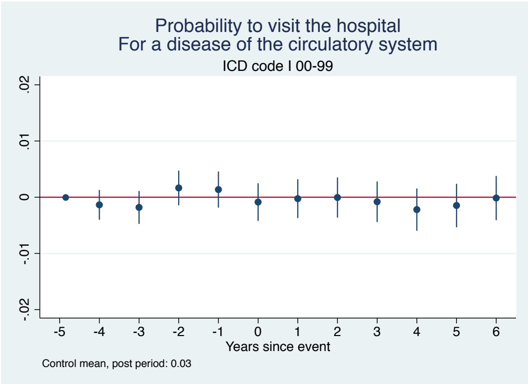


The main outcome of mental health that I consider is the purchase of antidepressants, anti-anxiety medication and sleeping pills. Figure 3 shows that exogenous job losses do not cause any change in the probability to purchase any of these drugs at some point during the year. There does not seem either to be any significant effect on purchases of opioid painkillers. The figure displays the results for the extensive margins but patterns are similar if one looks at the intensive margin,

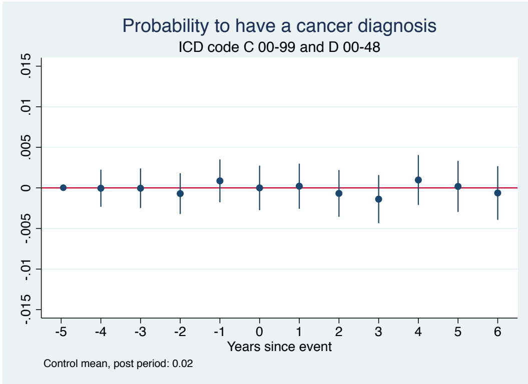
[18]I use a specification in levels for earnings and in logs for disposable income because for earnings, as opposed to income, there are many zeros in the post-event period.

Figure 4: Effect of establishment closures on hospital visits and diagnoses

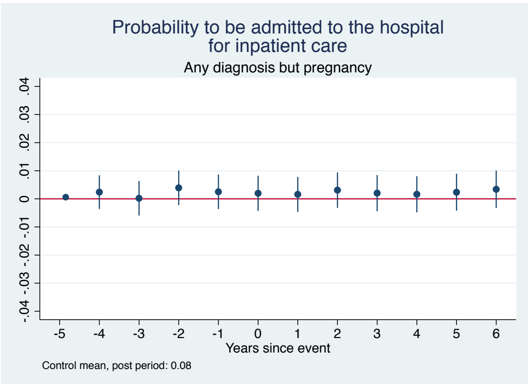
(a) Heart attacks



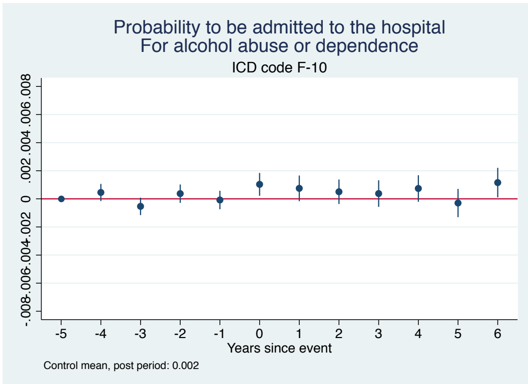
(b) Cancer diagnosis



(c) Inpatient care



(d) Alcohol-related issues



that is the number of days per year under treatment. In terms of magnitude, for instance for antidepressants (column 1 of Table 3), I can rule out increases of 0.1 percentage point relative to a control mean of 6%.

Antidepressants and related drugs are prescribed either by psychiatrists or by General Practitioners. I do not see any effect on visits to psychiatrists. I do not see any effect either on visits to General Practitioners (see Figure 5 a). Table 5 shows that I can rule out increase of 0.03 in the number of annual visits to a G.P., relative to a control mean of 2.8.

As can be seen on Figure 4 d, there is one outcome related to mental health on which there seems to be an effect: it is the probability to visit the hospital for an alcohol-related issue. This pools together both visits to the emergency room for alcohol abuse and inpatient or outpatient care related to alcohol addiction. This effect is entirely driven by males. However starting from such a low baseline mean, it is not sure that this result is very meaningful, it could just be a false positive.

4.2.2 Effect on Doctors' Visits

The Danish health insurance system reimburses fully all doctors' visits, except for dentists, physiotherapists and psychologists for which there are co-payments. This can explain the patterns I find in doctors visits. Indeed, as already mentioned, I do not find any significant change in the number

Figure 5: Effect of establishment closures on doctors' visits

(a) General Practitioners

(b) Physiotherapists (for which there is a co-payment)

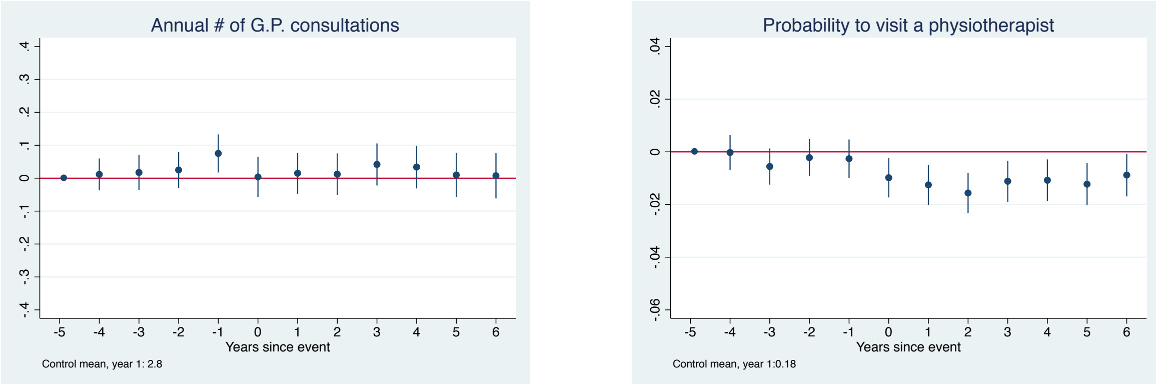


Table 3: Prescription drugs

VARIABLES	(1) Anti-depressants	(2) Opioid painkillers	(3) Anti-addiction	(4) Diabetes related
Displaced x After	-0.000457	-0.000963	0.000403	0.00221***
	(0.000820)	(0.000966)	(0.000396)	(0.000432)
Control Mean After	0.06	0.05	0.01	0.03
Obs.	578,496	578,496	578,496	578,496
R-squared	0.012	0.006	0.003	0.016
# of Indiv.	48,468	48,468	48,468	48,468

Note: this table reports the coefficient α from equation (2). The dependent variables are dummies for whether one purchased any antidepressants (ATC code N06A, column 1), painkillers (ATC code N02A, column 2), anti-addiction (ATC code N07B, column 3) or diabetes-related drugs (ATC code A01, column 4) during the year.

of visits to the General Practitioner, but there is a significant, though small, decrease in the annual probability to go to a physiotherapist (a one percentage point decrease, relative to a 17%control mean, see Figure 5 b and Table 5) and in the annual probability to see a dentist (half a percentage point decrease, relative to a 70%control mean). There is no significant effect on the probability to visit a psychologist.

Table 4: Hospital diagnosis

VARIABLES	(1) Any inpatient care	(2) Cardio-vascular issues	(3) Cancer	(4) Alcohol related issues
Displaced x After	0.000487 (0.00133)	-0.000797 (0.000802)	-0.000206 (0.000634)	0.000567*** (0.000206)
Control Mean After	0.077	0.034	0.021	0.002
Obs.	578,496	578,496	578,496	578,496
R-squared	0.000	0.002	0.003	0.000
# of Indiv.	48,468	48,468	48,468	48,468

Note: this table reports the coefficient α from equation (2). The dependent variables are dummy variables for whether one received during the year i) any inpatient care (column 1); ii) any diagnosis of a disease of the circulatory system (col. 2); iii) any cancer diagnosis (col. 3); iv) any alcohol-related diagnosis (col. 4).

Table 5: Doctors visits

VARIABLES	(1) G.P.	(2) Psycho-logist	(3) Physio-therapist	(4) Dentist
	# visits/year	Any visit in the year		
Displaced x After	-0.00829 (0.0128)	2.75e-05 (0.000390)	-0.00947*** (0.00158)	-0.00737*** (0.00154)
Control Mean After	2.78	0.01	0.17	0.78
Obs.	578,496	578,496	578,496	578,496
R-squared	0.007	0.000	0.002	0.002
# of Indiv.	48,468	48,468	48,468	48,468

Note: this table reports the coefficient α from equation (2).

4.2.3 Effect on Severe Physical Health Outcomes

Figure 4(a) shows that the treatment group is not significantly more likely to be hospitalized for a disease of the circulatory system, such as a heart attack, than the control group. It is not more likely either to be diagnosed with cancer, or to be hospitalized for any kind of disease that requires inpatient care (Figure 4(b) and (c)) . As Table 4 shows, I can rule out effects of the order of 3 to 4% for all these outcomes.

4.3 Effect on Mortality

As documented in previous work, I also find that the death hazard is higher for the treatment group than for the control group in the year of displacement (see Figure 6 a which just plots the raw death probability for the two groups, year by year). However because the employment data is at the annual level, I cannot be sure that the death occurred after the closure and it can very well be on the contrary that the establishment closure was at least in part driven by this death. This concern seems all the more relevant that this difference in death hazard is driven almost entirely by smaller establishments (see Figure 6(b) and (c) which just reproduce Figure 6(a) but splitting the sample at the median in terms of establishment size, that is 15 employees 5 years before the event year)[19]. This why my preferred estimates of the causal effect of job loss on mortality are the ones that do not include year 0 in the analysis. Yet I report results both ways, in particular for comparison with prior work.

Table 6 reports the coefficient of a dummy variable for being in the treatment group on the 5 years mortality rate, using either a linear probability model or a logistic regression framework (see equations 3 and 4 in section 3), controlling for many observables listed at the end of the table. Column 1 gives the effect on the overall mortality rate, whereas the next columns looks at the effect by cause of death. Panel A reports results when looking at the 5 years mortality rate from year 1 to year 5, and panel B from year 0 to year 4. In both cases I do not find any significant effect on mortality, yet the point estimate is bigger when I include year 0. When I don't, I can rule out increase of the order of 15% of the overall 5 years mortality rate (0.25 percentage point relative to a 1.5% mean). Given the data, the longer horizon I can look at is the 10 years mortality rate and the results are very similar. The fact that the point estimate is positive comes from deaths from external causes, for which there is a strong but only marginally significant effect. When including year 0, the point estimate of the effect on the 5 years mortality rate is higher and this is entirely driven by deaths from circulatory diseases.

Table 7 shows that when looking by subgroups, I do not find any significant effect but the point estimate for males is higher, especially so for younger males [20]

[19]The difference in year 0 is also driven by males. There is no significant differences for females

[20]The study of Sullivan and Von Wachter (2009) in the US focuses on males and their effect is driven by the less than 40 years old.

Table 6: 5 years mortality rate (Deaths/100) - By cause of death

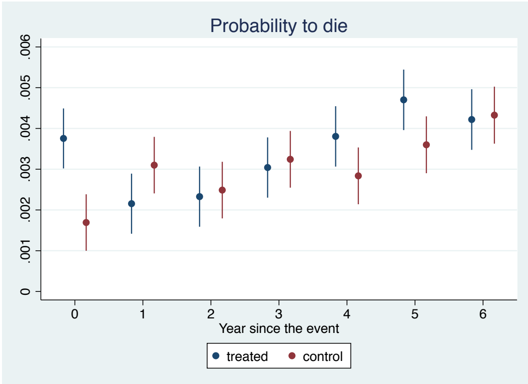
	(1) All causes	(2) Circulatory disease	(3) Cancer	(4) External cause	(5) Alcohol related
			Panel A: Not including Year 0		
Linear proba.	0.0367	0.00608	-0.0690	0.0560*	0.0227
model	(0.112)	(0.0515)	(0.0791)	(0.0301)	(0.0293)
Control Mean	1.51	0.31	0.79	0.08	0.09
Logit	0.0211	0.0127	-0.0957	0.533*	0.231
model	(0.0745)	(0.162)	(0.106)	(0.289)	(0.288)
			Panel B: Including Year 0		
Linear proba.	0.142	0.0340	-0.00248	0.0567*	0.0264
model	(0.107)	(0.0488)	(0.0770)	(0.0307)	(0.0272)
Control Mean	1.33	0.27	0.72	0.08	0.07
Logit	0.0979	0.110	-0.00860	0.537*	0.309
model	(0.0781)	(0.172)	(0.108)	(0.283)	(0.313)
Observations	48,467	48,467	48,467	48,467	48,467

Standard errors in parentheses - *** p<0.01, ** p<0.05, * p<0.1

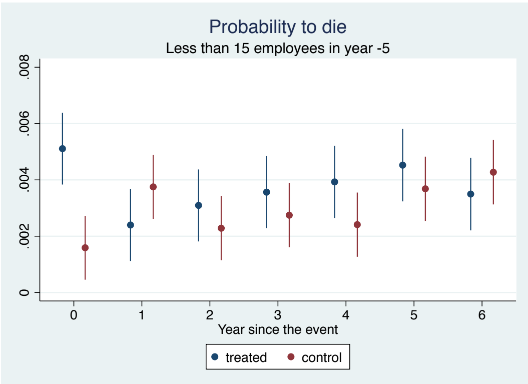
Controls include log of mean earnings in the 5 years prior, log of average household disposable income in the 5 years prior, age dummies, gender, a dummy for being foreign born, and some controls defined in year -1: occupation, tenure dummies, number of weeks of unemployment since 1980, number of years of employment, number of years of unemployment insurance membership, a dummy for being married or having a partner, region of residence, a dummy for living in Kopenhagen.

Figure 6: Effect of establishment closures on death hazards

(a) Overall



(b) Small establishments



(c) Big establishments

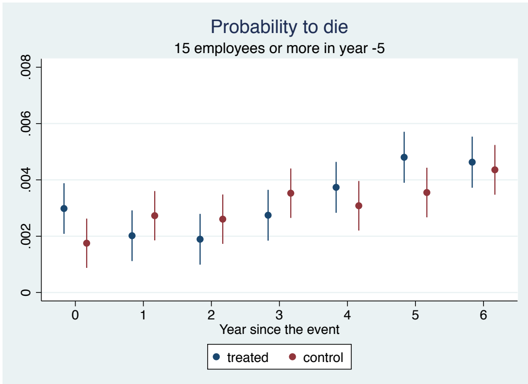


Table 7: 5 years mortality rate (Deaths/100) - By subsample

	(1)	(2)	(3)	(4)	(5)	(6)
	Males	Females	Males	Males	Males	Males
			Age $\leq$ 47	Age $>$ 47	Tenure$<$9	Tenure$\geq$9
Linear proba.	0.146	-0.121	0.177	0.0808	0.233	0.0767
model	(0.157)	(0.153)	(0.134)	(0.300)	(0.214)	(0.227)
Control Mean	1.71	1.23	0.58	2.99	1.36	1.99
Observations	28,633	19,834	15,200	13,433	12,905	15,728

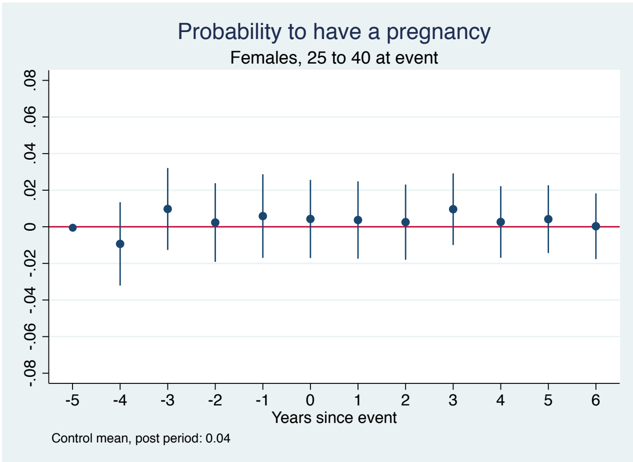
Standard errors in parentheses - *** p<0.01, ** p<0.05, * p<0.1

Same controls as for Table 6

4.4 Effect on Fertility Decisions

Figure 7 shows that, when restricting attention to females in child-bearing age, the treatment group is neither more nor less likely to become pregnant in any of the five years following the establishment closure than the control group.

Figure 7: Effect of establishment closures on fertility decisions



5 Discussion

As already mentioned in the introduction, it is hard to compare results across countries. For instance Sullivan and Von Wachter (2009) show that in the US mass layoffs have a significant and strong adverse effect on mortality. They find that Pennsylvanian workers who were laid off as part of a mass-layoff during the early 1980s recession had a 50-100% higher likelihood of dying in the year following displacement. 20 years out, mass-layoffs still induce a 10-15% increase in the death hazard. But the back of the envelope calculation they provide shows that the magnitude of their effect is consistent with a pure income channel explanation. In their setting, displaced workers experience a 50% drop in annual earnings in the year following displacement and their earnings are still 15-20% lower than the control group 5 years out. In contrast, in my setting, the earnings drop is much smaller at impact (15%), although long-lasting as well, and more importantly the drop in post-tax post-transfer household income is even smaller: 6%. With such a small drop, if we compute an upper bound of a potential causal effect on health using health-income gradients, we find very small figures that lie within my confidence interval.

However, there are several papers that look at one outcome in particular in Scandinavian or European countries. Consistent with what I find, some papers find zero or negligible effects. In particular Browning et al. (2006), who also study Denmark, find no effect of displacement on the 4 years probability of being hospitalized for potentially stress-related diseases of the circulatory system and digestive system: e.g. high blood pressure, heart disease, gastric catarrh and gastric ulcers. Kuhn et al. (2009), who look at the short-run effect of plant closures on health costs in Austria, find that overall expenditures on medical treatments are not significantly affected. The only exception is expenses for mental health drugs which increase for males but the magnitude is very small. Other papers report significant adverse effects of job loss on health. For instance Eliason and Storrie (2009b) report a positive effect of establishment closures in Sweden in the 1980s on alcohol-related hospitalizations. This is actually not inconsistent with what I find, although I tend to downplay the meaningfulness of this outcome given its extremely low baseline mean.

However my results can seem to stand somewhat in contrast with Browning and Heinesen (2012) and Eliason and Storrie (2009a) who find a positive effect of plant closures on males mortality, respectively in Denmark (in 1986-2002) and Sweden [21]. First, I also find a bigger point estimate for males but I can rule out the magnitudes that they report as average effects. Second, Browning and Heinesen (2012) include the year of displacement in their analysis. For them, the treatment group is 79% more likely to die in the year of displacement than the control group, which is also what I find. But I tend to disregard deaths that occur in the year of displacement as they might suffer from reverse causality. Indeed this higher likelihood of dying holds across causes of death, including cancer, which does not make much sense as an immediate consequence of job loss. However Browning and Heinesen (2012) also find a significant 35% higher death probability when looking at a 4 years horizon (including the year of displacement), whereas, even if I include the year of displacement and restrict to males, I can rule out effects of 35% (but our confidence intervals, with these same restrictions, overlap, mine is from -0.2% to 35%, whereas theirs is from 20% to 50%). Yet Browning and Heinesen (2012) are looking at closures that occurred between 1986 and 2002. One possible reconciliation between my magnitudes and theirs is that the late 1980s and 1990s was a different period than the 2000s I'm looking at and indeed at that time active labor market policies and the so-called "flexi-curity" system were not yet implemented.

6 Conclusion

This paper documents the causal effect of job loss on a wide variety of health outcomes for Danish people strongly attached to their job. I find that overall job losses driven by establishment closures in Denmark between 2001 and 2006 for people with at least 5 years of tenure at their establishment did not cause significant large effect on health. Not only do I observe no change in health care consumption, except for doctors visits for which there is a co payment, but I also find no significant increase in the death probability, as was the case for the Pennsylvanian workers during the 1982

[21] Rege et al. (2009) also report a positive effect of job loss on mortality. They are are primarily interested in the effect of mass layoffs on disability pension utilization in Norway, for which they find a large effect. However they also report an estimate of the effect on mortality for both genders pooled together. They find that workers in plants that downsized by 60% between 1995 and 2000, compared to workers in plants which experienced positive growth, have a 9% higher probability to have died by 2001, significant at the 10% level, which is not inconsistent with what I find though I tend to think that these estimates could be partly biased by reverse causality issues given the timing / horizon definition.

recession studied by Sullivan and Von Wachter (2009). I interpret my results as showing that, presumably with an adequate set of policies, it is possible to make the causal effect of job loss very small.

I see several important directions for future research. The main one would be to perform a similar comprehensive analysis on other countries than the Scandinavian ones, in particular the United States in more recent years. Of course this all relies on how to get access to relevant data.

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Appendix

Table A1: Dataset and variable definitions

Title	Unit of observation / Time frame	Variables used
Employer-employee registers	Workplace * Year	# of employees
Workplace level (*IDAS*)	1990-2012	Workplace status in the following year
Employer-employee registers	Individual * Job * Year	Primary job, tenure, occupation
Individual level (*IDAS*)	1990-2012	# of years of employment/ UI fund membership
Tax data	Individual * Year	Wage earnings, post-tax post transfer income
	1996-2012	Unemployment income
Unemployment insurance	Weekly claims 1996-2007	# of weeks of unemployment
Death records	Death event	Causes of death:
		Cardiovascular issue: ICD-10 code I00-I99
	1996-2013	Cancer: ICD-10 code C00-D48
		External cause: V01-Y89
		Alcohol-related: F10, K29, K70-K74, K85-K86, X45, X65
		Suicide: ICD-10 X60-84; Y87
Prescription drugs	Individual * Type of drugs	Annual probability to purchase or
(*Laegemiddelstatistikregisteret*)	* Day of purchase	# of days/year under treatment for
		i) Antidepressants, sleeping pills and anti-
	1996-2013	anxiety medication: ATC code N06A, N05B, N05C
		ii) Opioid painkillers:ATC code N02A
		iii) Diabetes-related drugs: A10
Doctor visits	Individual * Type of visit	Annual probability to visit a psychiatrist
(*Syegesikringsregistret*)	* Week of visit	a psychologist, a physiotherapist etc
	1996-2013	Annual # of visits to General Practioner
Hospital admissions	Individual * Diagnosis	Annual probability to receive inpatient care
(*LandsPatientregister*)	* Entry date	Annual probability to visit the emergency room
		Annual probability to be diagnosed with i) disease
	1996-2013	of the circulatory system; ii) cancer

Table A2: 5 years mortality rate (Deaths/100) of unemployed v. non-unemployed

	(1)	(2)	(3)	(4)	(5)
Sample	No restriction		Never unemployed prior to year t	Males	Females
Mortality rate	5 years	10 years	5 years	5 years	5 years
UI recipient	0.310***	0.713***	0.544***	0.435***	0.177***
in year t	(0.0518)	(0.0809)	(0.135)	(0.0810)	(0.0614)
Control Mean	0.95	2.46	0.98	1.24	0.64
Observations	187,636	187,636	30,833	88,566	99,070
R-squared	0.019	0.042	0.031	0.017	0.024

Note: The coefficients shown are those of a dummy for being a UI recipient in year t (where t=2002) in a linear probability model. Dependent variables are the probability to die between year t+1 and year t+5 (or t+10 for column 2). The sample consists of all UI recipients in 2002 with no UI in the year prior and of a control group matched on observables (see main text for more details). Additional controls include an immigrant dummy, very detailed education (over 1,000 categories), time varying detailed marital status, number of years of employment in year -1 (as measured by number of years of social security contribution) and number of years of UI fund membership in year -1). Levels of significance: *** 1%, ** 5%, * 10%.

Table A3: 5 years mortality rate (Deaths/100) of unemployed v. non-unemployed - By cause of death

	(1)	(2)	(3)	(4)	(5)
	Cancer	Circulatory disease	External cause	Alcohol related	Suicide
UI recipient	0.036	0.086***	0.067***	0.091***	0.049***
in year t	(0.0349)	(0.0224)	(0.0181)	(0.0164)	(0.0130)
Control Mean	0.48	0.16	0.10	0.06	0.05
Observations	187,636	187,636	187,636	187,636	187,636
R-squared	0.014	0.009	0.004	0.008	0.004

Note: Same sample and specification as for Table A2, except that the dependent variable is the probability to die between year t+1 and year t+5 of a specific cause of death, which varies by column.