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When financials get tough, life gets rough? Problematic debts and ill health

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Abstract

It is often suggested that problematic debts antecede health problems. In this paper, we investigate whether individuals obtaining problematic debts are more likely to use mental healthcare or social guidance and/or financial help, and whether they have higher mental healthcare costs. We use nationwide individual-level panel data from the Netherlands for the years 2011-2015. We employ a difference-in-differences approach with individual fixed effects and find that obtaining problematic debt is strongly associated with ill (mental) health. We find that average mental healthcare expenditures increased with approximately 200 euro in 2014 and 2015 for individuals who experienced problematic debts in 2013. The effect corresponds to an increase of 30% of individual mental healthcare expenditures because of problematic debts. Furthermore, the use of mental healthcare increased with 7% and the use of social guidance and/or financial assistance increased with 40% after getting into problematic debt. We therefore conclude that policies that prevent people from getting into debt may generate positive external effects by saving on expenditures on healthcare or social guidance and/or financial assistance.

JEL Codes: I01, I14, D14

Keywords: Problematic debts; mental healthcare; health expenditures; social guidance and financial assistance.

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1. Introduction

In many countries, problematic household debts have increased in recent years. For example, orders against borrowers that fail to make repayments on debts (the so-called country court judgements) grew by more than 110% between 2013 and 2018 in the United Kingdom (Narayan, 2020) and arrears in utility, mortgage or rent and hire purchase increased between 2014 and 2018 in Belgium and Denmark (amongst others) (Eurofound, 2020). It is expected that the economic crisis resulting from the corona pandemic may further accelerate the increase in problematic debts. For example, due to the corona crisis the number of Dutch households being in debt is expected to rise by almost 1 million in the short run and 41% of these new debts is expected not to be solved without the help from others (Fd, 2020). Being in debt does not only have consequences for the debtor, but also for creditors and employers because of productivity loss and sickness (NIBUD). For policymakers, creditors and employers interested in household debt prevention, it is therefore important to understand the relationship between problematic debts and other factors.

In this paper, we focus on the relationship between problematic individual debts and its effect on individual health. If problematic debts lead to a poor health status then policies that prevent people from getting into debt may generate positive external effects in the sense that they will prevent people from becoming ill and therefore may save on healthcare expenditures or social guidance and/or financial assistance. Indeed, several studies suggest that there is an association between individual health and problematic debts, arrears or financial strain (Fitch et al. 2011; Gathergood, 2012; Richardson et al. 2013; Turunen & Hiilamo, 2014; French & Vigne, 2019), but measuring a causal relationship is often hampered by methodological problems.

First, the vast majority of studies are cross-sectional (Richardson et al., 2013). Second, many studies rely on survey data, self-rated health and subjective measures of problematic debts which are prone to measurement biases (Richardson et al., 2013; Bridges & Disney, 2010). Third, most studies are not able to differentiate between normative and problematic debts (Fitch et al. 2011).¹

The question we study in this paper is whether obtaining problematic debts has a negative effect on individual health. We hypothesize that persons with problematic debts will have more mental psychosocial problems and/or difficulties with financial management. To quantify

¹ Not all debts are problematic. Problematic debts are those debts that people fail to repay or for which people default. Normative debts are managed debts which are paid without difficulty.

individual health, we measure the use and expenditures of mental healthcare of individuals, and their use of social guidance and/or financial assistance.

Our data allows us to overcome many of the methodological problems outlined above. First, we overcome the problems of cross-sectional data by using a panel data set which allows us to follow individuals over time. Our data over 2011-2015 enables us to measure the timing of ending up in a problematic debt situation: we observe the year when people are transferred from the private health insurer to the public National Administration Office (see below). We have data available for the full population, which allows us to compare the health status of individual before and after they end up in a problematic debt situation to the health status of individuals without problematic debts.

Second, we use standardized measures of health and observe these measures for the full population. We use nationwide administrative individual-level data on the use and expenditures of mental healthcare, and the use of social guidance and/or financial assistance.

Third, we obtained a population wide objective measure of a problematic debt: we focus on payment arrears for health insurance premiums in the Netherlands. In the Netherlands, there is an individual mandate for health insurance. Persons who do not pay their health insurance premium are transferred from the private health insurer to the public National Administration Office (CAK) (section 3). We use these transfers to measure problematic debts.

Using this measure of problematic debts is particularly interesting because failure to pay for the basic health insurance premium is one of the most common types of problematic debts in the Netherlands (TK, 2013). Because defaulting on health insurance premiums is shown to be positively related to having difficulties to pay mortgage/rent, electricity, gas and water and to pay off debts or payment arrangements (Jungmann & Werksma, 2012; Posthumus et al., 2019), it also serves as a proxy for having other problematic debts. The Dutch healthcare system provides an ideal case to test our hypotheses since all persons retain full access to healthcare, even if they fail to pay their premium. Therefore, defaulting on health insurance premiums does not obscure our measures of health, while providing a clean measure of a problematic debt. Hence, the institutional context that we study facilitates identification without running much risk of the bias that most other studies of the problematic debt-health relationship encounter.

Lastly, we also have detailed administrative data on a variety of other relevant variables. Although the measurement of the problematic debt-health relationship may suffer from endogeneity issues (section 5), detailed information on individual characteristics on, for example, the timing of negative life events (e.g. unemployment, divorce, being suspected of

committing a criminal offence, widowhood), other socio-economic variables (e.g. income, financial wealth, social benefits) and personal characteristics (e.g. gender, age, ethnicity) help to alleviate some of the selection biases that hamper earlier studies. We use these variables in addition to fixed effects to control for important differences in individual characteristics that may influence the problematic debt-health relationship.

We find that mental healthcare expenditures increased by about 30% for individuals experiencing problematic debts. Furthermore, the use of mental healthcare increased with 7% and the use of social guidance and/or financial assistance increased with 40% after getting into problematic debt. Overall, we thus find that obtaining problematic debt is associated with ill health.

The paper proceeds as follows. Section 2 summarizes previous literature on the problematic debt-health relationship. Section 3 describes the institutional context of our study. Section 4 outlines the data and Section 5 presents the methodology. The results are shown in Section 6 and Section 7 discusses and concludes on the results.

2. The relationship between problematic debts and health

A variety of socioeconomic and -demographic variables - including income, education, wealth, occupation, race and ethnicity are found to be associated with health. Although early research concluded that therefore there must be a broader underlying dimension of the social gradient (Adler et al. 1994), recent evidence shows that socioeconomic status (SES) and health relate to each other in diverse ways (Cutler et al. 2011). It is found that different measures of SES operate through different mechanisms (Currie 2009; Almond et al. 2018; Galama & Van Kippersluis, 2018). Besides, the causal pathway of the gradient is variable: some measures of SES are found to affect health, whereas other measures of health are found to impact SES.

The literature has posited several explanations for the association between SES and health (Adler & Ostrove, 1999; Hoffmann et al. 2018; Bhattacharya et al. 2013). The first is the social causation hypothesis which posits that SES influences health. The second is the health selection hypothesis which states that health influences SES. Third, some third variable, like major life events, family background or genetic endowment, may cause both worse health and SES (Zimmerman & Katon, 2005; Hoffmann et al. 2018). See Bhattacharya et al. (2013) for a breakdown of the several hypotheses.

Many studies focus on the SES-health relationship. Generally, it is found that mortality rates and rates of poorer self-assessed health are substantially higher in groups of lower socioeconomic status, although the magnitude of the inequalities between groups of higher and

lower socioeconomic status differs between countries (Mackenbach et al. 2008; 2015; 2017). It is unclear which causal mechanism (social causation, health selection or common background factors) is relatively more important. Goldman (2006) surveys the literature on the different hypotheses and concludes that the observed disparities in health are driven largely by social causation involving a complex set of causal processes, rather than by health selection. Recent studies find that both social causation and health selection are responsible for socioeconomic inequalities in health, but that their relative importance changes with age and health measures used (Hoffmann et al. 2018; Stauder, 2019).

The problematic debts-health literature is much sparser than the literature on the SES-health relationship. A few papers have systematically reviewed the literature on the relationship between personal unsecured debts and health (Fitch et al. 2011; Richardson et al. 2013; Turunen & Hiilamo, 2014). The reviewed studies differ in their definitions of indebtedness, measures of change, sample populations and methodological designs. The vast majority of studies are cross-sectional and studies using longitudinal data are generally less likely to have standardized measures of health (Richardson et al. 2013). Only three longitudinal studies controlled for mental health issues at baseline, or socio-economic variables (Fitch et al. 2011). Furthermore, many studies do not differentiate between types of debt, and whether the debt was problematic or normative (Fitch et al. 2011; Turunen & Hiilamo, 2014) or are, by design, not generalizable beyond specific populations such as university students, debt management clients, people on income support or older adults (Richardson et al. 2013).

Although the reviews conclude that, given the above mentioned reasons, causality is hard to establish, most studies point at a negative relationship between debt and health. The meta-analysis of pooled odds ratios by Richardson et al. (2013) shows a significant relationship between debt and mental disorder, depression, suicide completion or attempt, problem drinking, drug dependence, neurotic disorder and psychotic disorders. No relationship was found between debt and smoking. Turunen & Hiilamo (2014) find that unpaid financial obligations are related to poorer subjective health assessment and health-related behavior and that indebtedness has a serious and long-lasting impact on peoples' lives. In line with this finding, Fitch et al. (2011) conclude that indebtedness may not only contribute to the development of mental health problems, but also mediate relationships between poverty and mental disorder. Studies of physical health have shown a relationship between debt and poorer self-rated health, long-term illness or disability, chronic fatigue, back pain, higher levels of obesity and worse health and health related quality of life as measured by the SF-36 (Richardson et al., 2013). All three review studies conclude that more longitudinal research is

needed to establish potential mechanisms and mediators of the relationship and to determine causality.

3. Defaulting on health insurance premiums in the Netherlands

In the Netherlands, there is an individual mandate for health insurance, i.e. all citizens are obliged to buy standardized individual basic health insurance from a private insurer. The standardized basic benefits package covered by the Health Insurance Act (HIA) includes amongst others hospital care, GP services, prescription drugs and maternity care. In addition to basic health insurance, people are free to buy supplementary health insurance, which provides coverage for benefits that are not included in the basic benefits package. There is a fixed mandatory annual deductible², but individuals may choose to supplement this mandatory deductible with a voluntary deductible up to 500 euro. There are no additional co-payments, apart from a copayment for secondary mental healthcare which was temporarily introduced in 2012 and abolished shortly after.^{3,4}

According to the HIA, insureds who fail to pay their basic insurance premium for (more than) six consecutive months are defaulters. To prevent people from defaulting on their basic health insurance, Dutch health insurers are obliged to send out payment notices and additional letters as soon as the enrollee fails to pay for his health insurance. Enrollees are informed about the consequences for not recommencing their payments. Moreover, they are not allowed to switch to another health insurer as long as premiums are not paid. This is to prevent defaulters from shopping around and defaulting at another health insurer. The letters also offer payment arrangements and debt assistance. After several months of defaulting, the health insurer terminates any supplementary health insurance and voluntary deductible to limit the amount of debt that needs to be settled.

Insureds who fail to pay their basic insurance premium for (more than) six consecutive months are transferred from the private health insurer to the public National Administration Office (CAK). Defaulting does not affect access to care covered under the basic benefits package, but it does introduce a different payment regime. On behalf of the defaulter, CAK

² The mandatory deductible ranged from 170 euro in 2011 to 360 euro in 2015.

³ Primary mental healthcare is provided to patients with mild mental disorders in an ambulatory setting. Patients may use primary mental healthcare without a referral by a general practitioner. Secondary mental healthcare includes ambulatory curative mental healthcare and inpatient curative mental healthcare up to one year. A referral from a primary care physician is needed for patients to use secondary mental healthcare. Both primary and secondary mental healthcare are part of the standardized basic benefits package.

⁴ Because copayments may influence the decision to use/forego care as well as the individual's financial status, we perform tests to show robustness of our results to the introduction of copayments (see section 5.2).

will compensate the health insurer for lost premiums to ensure that the defaulter keeps access to the basic health insurance. CAK charges and collects an administrative premium from the defaulter for the basic insurance policy, which is higher than the nominal premium that health insurers charge. The defaulter also has to pay additional costs related to debt collection and interest.

Only when the defaulter pays off his debt at the health insurer and CAK, the defaulter will flow out of the CAK regime and his basic health insurance policy will be transferred back to the health insurer. Any remaining debts that accumulated in the first six months of defaulting before entering CAK and debts resulting from not paying the out-of-pocket payments related to the mandatory deductible have to be settled by the health insurer with the defaulter (without inference of CAK).

In the period 2011-2015, the annual number of defaulters registered on December 31 ranged between 298.000 to 326.000 (MinVWS, 2018). Failure to pay for the basic health insurance premium is one of the most common types of problematic debts in the Netherlands (TK, 2013; Posthumus et al., 2019). For the majority of defaulters the debt at the health insurer, which does not include the administrative premium of CAK, (well) exceeds 750 euro (MinVWS, 2018). The majority of defaulters remains under the CAK regime for over 24 months (MinVWS, 2018). Defaulting on health insurance premiums is shown to be positively related to having difficulties to pay mortgage/rent, electricity, gas and water and to pay off debts or payment arrangements (Jungmann & Werksma, 2012; Posthumus et al., 2019). Therefore defaulting on health insurance premiums also serves as a proxy for having other problematic debts.

4. Data

4.1. Data

We combine several nationwide individual-level administrative datasets from the Netherlands. We obtain annual data for 2011-2015 on individual healthcare expenditures from the Dutch data warehouse of insurers (VEKTIS) and link this to data for 2011-2014 on the use of social and financial assistance from the National Administration Office (CAK). Although studies show that there may be a relationship between debt and physical health (Richardson et al., 2013), the focus of most studies has been on mental health. Indeed, we hypothesize that persons with problematic debts will have more mental psychosocial problems and/or difficulties with financial management. Therefore, we focus on the next three variables to measure health. First, we have total annual individual mental healthcare expenditures in euro. This includes both

primary and secondary mental healthcare (footnote 3). Second, we use a binary variable to indicate whether an individual uses mental healthcare in a given year or not. Third, we focus on individual use of social guidance and/or financial assistance in a year. Someone is defined as an user when he/she receives assistance, individually or in a group, to enhance independence and self-reliance, for example by receiving help with financial matters. Our data covers guidance and assistance provided by long term care providers covered by the publicly funded Exceptional Medical Expenses Act (AWBZ).⁵ The AWBZ covered social guidance and/or financial assistance, personal care, nursing, treatment and accommodation.

Data on problematic debts are obtained from the National Administration Office (CAK). People who are registered at CAK are classified as defaulters, i.e. persons with problematic debts, as they did not manage to repay their debt or recommence their payments in the six months before entering the CAK regime. The dataset includes all defaulters registered at December 31 of each year, their (past) entry and (future) exit dates from CAK, and information on the debt amount at CAK. We do not observe debt amounts at the health insurer.

We use data from the Dutch population register on age, gender and ethnicity (1 for native; 0 immigrant); data from the tax authority to measure the individual's gross total household income (in quintiles), financial wealth⁶ (in quintiles) and whether an individual receives social benefits (1 for receiving sickness, unemployment or disability benefits; 0 otherwise). Moreover, we have data on whether an individual experienced one of the following negative life events in a given year (1 for yes; 0 for no): widowhood, losing a loved one, getting unemployed, being a victim of violence, divorcing, having to follow a public debt restructuring trajectory, or being suspected of committing a criminal offence.

4.2. Sample selection

Because in the Netherlands minors do not pay premiums for their health insurance policies and thus cannot default, we construct a panel and restrict our sample to persons aged 18 years or older at January 1, 2011. Because we observe that most defaulters for health insurance are (much) younger than non-defaulters and this may affect their financial situation, we limit our sample to those younger than 51 years old at January 1, 2011 for comparative reasons.⁷

⁵ Since 2010, there was a small income-related copayment for social guidance and/or financial assistance covered under the AWBZ (Bakx et al. 2020).

⁶ Financial wealth is defined as the household's assets corrected for the value of the first house and, if applicable, the mortgage.

⁷ 51 was chosen because it equals the average age of defaulters (38) plus the standard error (13) at January 1, 2013.

We define defaulters as individuals who are new to defaulting in 2013 (i.e. registered at CAK in 2013 but not registered in 2011 and 2012).⁸ Defaulters in 2013 can be registered as defaulters in 2014 and 2015 as well, but this is not necessarily the case. We define the group of non-defaulters as all people who never defaulted (i.e. not registered at CAK during the total period 2011-2015). This strategy allows us to study responses of new defaulters in the first two years after becoming a defaulter. We exclude the year 2013 from the analyses, as in this year persons become defaulters which obscures a before-and-after comparison.

Our final sample for the baseline specification for mental healthcare expenditures and the use of mental healthcare includes 26,522,242 observations, of which 85,461 are defaulters. 19,910,548 observations are included in the main specification for social guidance and/or financial assistance. 64,068 of these observations experienced problematic debts.

4.3. Descriptive statistics

We define defaulters as individuals who are new to defaulting in 2013. Table 1 shows the descriptive statistics of our independent and dependent variables for the pre-period (2011-2012) and the post-period (2014-2015). Annually, there are more than 6 million non-defaulters and slightly less than 21,000 new defaulters in our dataset.

The first three rows of table 1 display descriptive statistics on the three dependent variables.⁹ Average annual mental healthcare expenditures are significantly lower for non-defaulters than for defaulters (about 250 euro versus 631 euro in the pre-event period). Correspondingly, mental healthcare use is significantly lower for non-defaulters in the pre-event period (7.9% versus 12.6%). Non-defaulters also receive significantly less social guidance and/or financial assistance than defaulters in the pre-event period (0.7% versus 1.7%).

We also observe that the change in the three health variables from the pre-event period to the post-event period is larger for defaulters than non-defaulters. For example, average annual mental health expenditures are only slightly higher for non-defaulters in 2014-2015, but increase with more than 200 euro for defaulters. For mental healthcare use this pattern is less clear, which suggests that the growth in mental healthcare is along the intensive margin. For social guidance and/or financial assistance we again find a substantial increase for defaulters.

⁸ Newly registered at CAK implies that defaulters did not pay their premium six months before entering the CAK regime in a given year. Thus, new defaulters registered at CAK could be non-defaulters until June in that given year. To ensure that we truly only include individuals who are new to defaulting in 2013, we only include individuals that enter the CAK regime after June 2013.

⁹ We tested whether the variables are significantly different at the 1% level and report the results in-text. Tables are available upon request.

The second part of the table reports the descriptive statistics on independent variables. The average age in the pre-period is higher for non-defaulters, 35.7, than for defaulters, 32.7, indicating that the age distribution (for persons from 18-51 years old) is somewhat more skewed to younger ages for defaulters.¹⁰ Since we use a panel structure, the average age difference between the pre- and post-period is about 3 years. We also observe that on average defaulters are about 10% point more likely to be males than females, and are about 27% point more likely to be an immigrant than non-defaulters in both the pre- and post-period. Average financial wealth and gross household income increase over time for non-defaulters, which corresponds to a young working population and increased earnings. However, for defaulters the opposite is true. For defaulters average financial wealth decreases from 5,854 euro in 2011-2012 to 1,736 euro in 2014-2015, and average income from 42,364 euro to 41,358 euro. Figures 1 and 2 show frequency graphs for financial wealth and gross household income for the event and control groups at 1 January 2013. As expected, financial wealth and gross household income are substantially higher for non-defaulters compared to defaulters. Financial wealth is also negative for a large part of the defaulter population, which explains the remarkably large standard deviations for financial wealth as reported in table 1.

The variable ‘social benefits’, which indicates if a person receives sickness, unemployment or disability benefits, shows that defaulters are significantly more likely to receive social benefits in the pre-period than non-defaulters. Both defaulters and non-defaulters are significantly more likely to use social benefits in the post-period than in the pre-period. Finally, Table 1 shows that defaulters experience more often a negative life event in the pre-period than non-defaulters. Moreover, both groups are significantly less likely to have experienced a negative life event in the post-period than in the pre-period.

5. Empirical strategy

We employ a difference-in-differences (DID) analysis to study the relationship between obtaining problematic debts and healthcare use and expenditures and the use of social guidance and/or financial assistance. To this end we compare mental healthcare use and expenditures and the use of social guidance and/or financial assistance for defaulters (treatment group) and non-defaulters (control group) over time.

¹⁰ Table 1 reports descriptive statistics on the age restricted sample (see section 4.2).

5.1. Main analysis

We estimate the following fixed effects model by least squares:

$$H_{i,t} = \beta_1 D_{2012} + \beta_2 D_{2014} + \beta_3 D_{2015} + \beta_4 DF_i + \beta_5 D_{2014} * DF_i + \beta_6 D_{2015} * DF_i + \alpha_i + \epsilon_{i,t} \quad (1)$$

where H_{it} is either use or expenditures of mental healthcare or the use of social guidance and/or financial assistance. We include three year dummies D_{2012} , D_{2014} and D_{2015} with the year 2011 being the reference category. Treatment year 2013 is excluded from the analyses (section 4.2). DF_i is the treatment indicator, equal to one for the defaulters and zero for the non-defaulters. To study the treatment effect we include interaction terms between the treatment indicator and the post-period dummies. As a result, β_5 and β_6 are our coefficients of interest. Since many controls lack data for the year 2010, we do not add (lagged) control variables to the main regression but rather include individual fixed effects: α_i . Note that the fixed effects absorb the treatment indicator DF_i , which therefore drops out. Since we have a panel structure we cluster the errors $\epsilon_{i,t}$ at the individual level.

5.2. Robustness tests

To ensure that the results that we find by estimating equation (1) are not merely the result of confounding factors, we perform a number of robustness checks.

First, we add time variant individual characteristics as controls (income, financial wealth and negative life events) to reduce the probability of omitted variable bias.¹¹ As explained in section 3, in 2012, adults had to pay a copayment for secondary mental healthcare. Since poorer people may respond more strongly to a copayment and defaulters have less financial wealth and income (Table 1), the introduction of the copayment may affect our results. However, although we lack precise estimates of the copayment paid, including the exact copayments would result in a bad control problem because copayments influence the decision to use/forego care as well as the individual's financial status. Therefore, to control for financial space and differences in wealth and income, we control for lagged financial wealth and gross household income and add a binary dummy showing whether a person received social benefits in $t-1$ or not.¹² Because earlier studies indicate that third variables, like major life events, may influence

¹¹ Since equation (1) includes individual fixed effects, we leave out time invariant personal characteristics such as age, gender and ethnicity.

¹² Given the relationship between wealth/income and defaulting and health, any measures of wealth and income may be bad controls, which is why we added these results in our robustness analyses and not in our main analysis.

the problematic debt-health relationship (Zimmerman & Katon, 2005 and Hoffmann et al., 2018), we also add negative life events as controls to equation (1). In these analyses, we need to restrict the samples to data from 2012-2015 because we include them as lagged variables.

Second, table 1 reported large standard deviations for financial wealth and figure 1 showed that average financial wealth was negative at January 2013 for a large part of the defaulting population. This suggests large differences between defaulters and non-defaulters. To create more comparable treatment and control groups, we restricted our sample to individuals for whom financial wealth was lower or equal to 3,460 euro on 1 January 2013 (i.e. the average financial wealth of defaulters on 1 January 2013) and again estimate equation (1). To check for robustness of our results to other differences between treatment and control groups, we also estimate equation (1) on a restricted sample of individuals with household income lower than or equal to the average household income of defaulters in 2012 (i.e. 42,655 euro) (robustness test 3) and individuals who experienced a negative life event in 2012 (robustness test 4).¹³

5.3. Assumptions

5.3.1. Common trends

The difference-in-differences strategy identifies the effect of problematic debts on health provided the health of defaulters and non-defaulters would have followed common trends if the defaulters would not have defaulted. Estimation of a model similar to (1) using also data from the pre-event period and allowing the year effects to differ between defaulters and non-defaulters approximates whether the common trend assumption holds. We run the regression below on the final sample as outlined in Section 4.2:

$$H_{it} = \beta_1 D_{2011} + \beta_2 D_{2014} + \beta_3 D_{2015} + \beta_4 DF_i + \beta_5 D_{2011} * DF_i + \beta_6 D_{2014} * DF_i + \beta_7 D_{2015} * DF_i + \alpha_i + \epsilon_{it} \quad (2)$$

where H_{it} represents either expenditures or use of mental healthcare and the use of social guidance and/or financial assistance for person i at time t . We include year dummies (D_{2011} - D_{2015}) with 2012 being the reference category. Treatment year 2013 is excluded from the analyses. DF_i is the treatment indicator for person i , equal to one for defaulters and zero for

¹³ We do not employ matching techniques. Not only would matching decrease our sample size substantially, matching in combination with DID could introduce regression to the mean bias (Daw and Hatfield, 2018).

non-defaulters. We include interactions between the year dummies and the treatment indicator and leave out the interaction with the last pre-treatment period (2012). To control for time-invariant personal characteristics, we add individual fixed effects α_i . The random error term ϵ_{it} is clustered at the individual level. For the common trend assumption to hold, β_5 should be equal to zero.

5.3.2. Exogenous event

To claim a causal relationship, the event under study must be exogenous. While this assumption may hold in individual cases, in general, the event of encountering problematic debts may not be fully exogenous. In fact, studies have shown that the causal relationship between problematic debts and health may, inherently, be reverse (Gathergood, 2012). In addition to reverse causality bias, unobservable attributes may influence both problematic debts as well as the individual's health.

The empirical strategy adopted minimizes the threats to identification from these two potential sources of endogeneity. Individual fixed effects deal with any time invariant correlated unobservables, while the time varying patient characteristics control for potential differential changes in observable characteristics across the treatment and control groups. The only remaining source of bias then stems from unobservable individual characteristics with differential effects across treatment and control groups. Although we may not be able to completely alleviate concerns for omitted variable bias, our data allow us to include the most relevant priors with a differential effect (section 5.2). We furthermore improve the comparability of the treatment and control group by studying comparable subsets of the population (section 5.2). Selection issues are minimized because the composition of treatment and control groups is held constant by construction. In our study, defaulters are individuals who are new to defaulting in 2013; that is, they are not registered at CAK in 2011 and 2012. In the control group are only those who never defaulted (i.e. not registered at CAK during the total study period). Hence, no individuals can switch from the treatment to the control group or vice versa (section 4.2). Therefore, we are less concerned about time varying information on problematic debts.

6. Results

6.1. Common trends assumption

Figures 3-5 show the development of use and expenditures of mental healthcare over the period 2011 to 2015 and use of social guidance and/or financial assistance over the period 2011 to

2014. Again we observe that the relative change in the average annual mental health expenditures is higher than the relative change in mental healthcare use, which suggests that the growth in mental healthcare is along the intensive margin. The figures furthermore show the need for panel data as mental healthcare use and expenditures, and the use of social guidance and/or financial assistance are already higher for defaulters in the pre-period. For all three specifications, estimation of model (2) reveals no evidence of differential trends in the period immediately preceding the event (Table 2).

6.2. Main specification

We present our main regression results in column 1 of Table 3 for mental healthcare expenditures, Table 4 for mental healthcare use and Table 5 for the use of social guidance and/or financial assistance.

The estimates for the interaction terms show that mental healthcare expenditures increase with approximately 200 euro in 2014 and 2015 (p -value < 0.01) after getting into problematic debts in 2013 (Table 3).¹⁴ Because average mental health expenditures in the pre-period were about 631.0 euro (Table 1), expenditures increase over the study period by about 30% because of problematic debts.

Moreover, we find that defaulters are 0.7 percentage points more likely to use mental healthcare in 2014 (p -value < 0.01), and 1.0 percentage points more likely to use mental healthcare in 2015 (p -value < 0.01) (Table 4). Average mental healthcare use was about 12.6% in the pre-event period (Table 1). Hence, the increase in mental healthcare use after becoming a defaulter is about 7%.

Finally, the use of social guidance and/or financial assistance increases with 0.73 percentage points in 2014 (p -value < 0.01), which corresponds with an increase of about 40% if we compare it to the pre-event period rate (i.e. 1.72% -- Table 1).

6.3. Robustness tests

Columns 2-5 in Tables 3, 4, 5 show the results of the robustness tests for mental healthcare expenditures, mental healthcare use and the use of social guidance and/or financial assistance

¹⁴ For all specifications, we performed a z-test, $z = |\hat{\beta}_4 - \hat{\beta}_5| / \sqrt{se(\hat{\beta}_4)^2 + se(\hat{\beta}_5)^2}$, to test whether in equation (1) the treatment effect β_4 in 2014 is different from the treatment effect β_5 in 2015. At the 5% level, we cannot reject the hypothesis that β_4 and β_5 are equal in all specifications. Results are available upon request.

respectively.¹⁵ Column 3 in each of the tables reports the results for adding time variant individual characteristics as controls to equation (1). Column 3 of Table 3 shows that the interaction terms are similar in sign, magnitude and significance level as the baseline results. Hence, defaulters have higher mental healthcare costs in the years after they encounter problematic debts. The interaction terms are smaller in size but have a similar sign as the main analysis for mental healthcare use (Column 2 Table 4). The result for financial and/or social assistance is comparable to the baseline result (Column 2 Table 5).

In the majority of estimated results, the control variables have the expected sign. The coefficient for financial wealth is negative indicating that wealthier individuals have lower mental healthcare expenditures and are less likely to use mental healthcare or social guidance and/or financial assistance. The occurrence of a negative life event and receiving social benefits has, as expected, an upward effect on all three outcomes variables. The only somewhat counterintuitive results are the estimators for income in Tables 3 and 4. The coefficients for income are positive for quintiles 2-5 in the regression for mental healthcare expenditures and use, suggesting that individuals in higher income quintiles have higher mental healthcare expenditures and are more likely to use mental health care. These positive effects disappear when excluding the individual fixed effects, which suggests that there is a correlation between the fixed effects and income in these estimations.¹⁶ Inclusion of the control variables does not change the sign, magnitude or significance of our coefficients of interest.

Column 4 of Tables 4, 5 and 6 reports the results of estimating equation (1) on a subsample of individuals with comparable financial wealth. The coefficients of interest are consistent with the results from the main analyses for both mental healthcare costs and the use of social guidance and/or financial assistance. Only the use of mental healthcare is somewhat larger but has the same sign (column 4 Table 4).

Each table presents a test for the robustness of our results for the difference in income in column 5. It shows the results of estimating equation (1) on a subsample of individuals with comparable income distributions. We again find that defaulters use more care in the years after becoming a defaulter. The coefficients of the interaction terms are now larger than in the baseline analyses: mental healthcare costs are approximately 300 euro higher, defaulters are

¹⁵ We tested the common trends assumption for each robustness test. The interaction term between de default indicator and 2011 is never statistically significant at the 10% level. The results are available upon request.

¹⁶ The coefficients of the other control variables are insensitive to exclusion of the individual fixed effects. The only exception is ‘social benefits’, which has a positive effect on mental healthcare expenditures when individual fixed effects are excluded, while having a negative effect when the fixed effects are in the model. The results are available on request.

1.8%-points more likely to use mental healthcare than non-defaulters, and defaulters are 0.9%-points more likely to use social guidance and/or financial assistance.

Lastly, the samples are restricted to individuals experiencing a negative life event in 2012 (column 6). For individuals who experienced a negative life event in 2012, the results are similar as in the main analysis. Although in Table 3 the magnitude of the estimated coefficients of the interaction terms can differ substantially, we cannot reject the hypothesis that they are similar.¹⁷

7. Discussion and conclusion

Many studies focus on the social gradient in health. The problematic debts-health literature is much sparser. In this paper, we focused on the relationship between problematic debts and health status. The question we studied is whether obtaining problematic debts has a negative effect on health. Although several studies suggest that there is an association between health and problematic debts, many of these studies are hampered by methodological problems and the relationship is not well understood (Fitch et al. 2011; Gathergood, 2012; Richardson et al. 2013; Turunen & Hiilamo, 2014; French & Vigne, 2019).

We find that persons who get into problematic debts have higher mental healthcare expenditures and are more likely to use mental healthcare and social guidance and/or financial assistance in the following years. This finding corresponds with the finding of previous studies and, more specifically, with the findings of the only other study on the relationship between indebtedness and health in the Netherlands (Rijnsoever et al., 2012). Rijnsoever et al. (2012) rely on survey data and find that unhealthy lifestyles, psychosocial problems and self-reported overall worse health were more common among a sample with high self-reported debts in 2008.

We contribute to the literature in several ways. First, most studies are not able to differentiate between normative and problematic debts (Fitch et al., 2011). Our measure of problematic debts comes from an administrative population-wide dataset and does not obscure our measures of health. Second, the vast majority of studies are cross-sectional (Richardson et al., 2013). We, in contrast, use a panel data set which allows us to study individuals before and after they end up in a problematic debt situation and to compare the change in their health status to that of individuals without problematic debts. Third, many studies rely on survey data and self-rated health (Richardson et al., 2013). In this study, we use more standardized measures of health and observe these measures for the full population. Fourth, we have detailed

¹⁷ See footnote 14.

administrative data on a variety of other relevant variables and used these variables in addition to fixed effects to control for a wide range of individual and household characteristics that may influence the problematic debt-health relationship.

We find that average mental healthcare expenditures increased with approximately 200 euro in 2014 and 2015 for individuals who got into problematic debts in 2013. The effect of indebtedness on healthcare expenditures thus extends beyond the first year of getting into financial strain. The effect corresponds to an increase of 30% of mental healthcare expenditures because of problematic debts. Moreover, we find that defaulters were 0.7 percentage points more likely to use mental healthcare in 2014, and 1.0 percentage points more likely to use mental healthcare in 2015. This corresponds to an increase in mental healthcare use after becoming a defaulter of about 7%. The higher relative change in average annual mental health expenditures than in mental healthcare use, suggests that the growth in mental healthcare is along the intensive margin. Finally, the use of social guidance and/or financial assistance increases with 0.73 percentage points in 2014, which corresponds to an increase of about 40% in comparison to the baseline of 1.72%. The common trends assumption holds and the endogeneity issues that hamper causal identification of the problematic debt-health relationship are minimized. Both conditioning on observables as well as estimating the model on different subsets of the population does not markedly change the estimates. Sign and significance are insensitive to different specifications. Magnitude of the effect somewhat differs, but never worryingly: we consistently find, in all specifications, that the effect of getting into problematic debt on health is substantial.

We were concerned that the introduction of the copayment for secondary mental healthcare in 2012 would obscure our estimations. We hypothesized that poorer people may respond more strongly to a copayment and we therefore included measures of income, financial wealth and receiving social benefits to the estimation and tested the sensitivity of the results by estimating equation (1) on a subsample of individuals with comparable income and financial wealth distributions. The robustness tests reveal no sensitivity for differences in financial opportunities. If there were to be an effect of the copayment that we are not able to capture in our estimations, our results are lower bound estimators: the actual effects of problematic debt on health are then even more severe than we find.

Alternatively, instead of deterioration of health, our results may also be explained by increased awareness of health problems because of getting into problematic debts. Ending up in a situation of problematic debts intensifies contacts with insurance companies, CAK, municipalities, organizations for social guidance and/or financial assistance, etc. Although

these contact may predominantly be financially motivated, they may also increase awareness of health problems that were previously unrecognized or untreated. We do not find any evidence for this alternative explanation: our findings suggest that the increase in healthcare use is along the intensive margin, suggesting that most defaulters already used health care before getting into problematic debt.

Overall, our results suggest that, in the Dutch healthcare system, even though people may experience financial difficulties, and may not be able to pay for the basic health insurance premium, they remain to have full access to healthcare. This characteristic is in stark contrast to other healthcare systems, where people with problematic debts may not be able to afford their health insurance premiums and illnesses might go untreated (Frank et al., 2009). It is exactly this institutional feature that makes the Dutch healthcare system an ideal case to test our hypothesis. Our measures of health, which are only reliable proxies for health status if financial access is equal, are unaffected by our measure of financial debt. Of course, theoretically, people might forgo care if they believe they are no longer eligible for healthcare when defaulting. However, we have no indication that this misunderstanding occurs. Dutch health insurers are obliged to send out payment notices and informational letters as soon as enrollees fail to pay for their health insurance which inform defaulters about the process and their insurance status. Also, when defaulters are transferred to CAK, they are informed about their rights (and obligations). Again, however, even if having problematic debts has a differential effect on healthcare use and expenditures, our estimates are lower bounds of the actual effects of indebtedness.

Although this paper further clarifies the health-problematic debts relationship, interesting avenues for future research remain. For example, in this study, we used negative life events as control variables. It would be interesting to further our knowledge by studying whether negative life events could trigger problematic debts. Also, because the causal relationship between problematic debts and health may, inherently, be reverse (Gathergood, 2012), and our findings suggest an increase in healthcare use along the intensive margin, it would be interesting to study the effect of health on the propensity to acquire problematic debts.

For policymakers, it is important to understand the relationship between problematic debts, health and health expenditures. Problematic debts constitute an enormous loss not only to debtors and creditors, but also to society as a whole. If problematic debts lead to a poor health status then policies that prevent people from getting into debt may generate positive external effects (e.g. on productivity) and may save on expenditures on healthcare or social guidance and/or financial assistance. Our results suggest that problematic debts may have a substantial

negative effect on health and preventing people from getting into problematic debts may therefore well be an effective way to save on healthcare expenditures and improve population health.

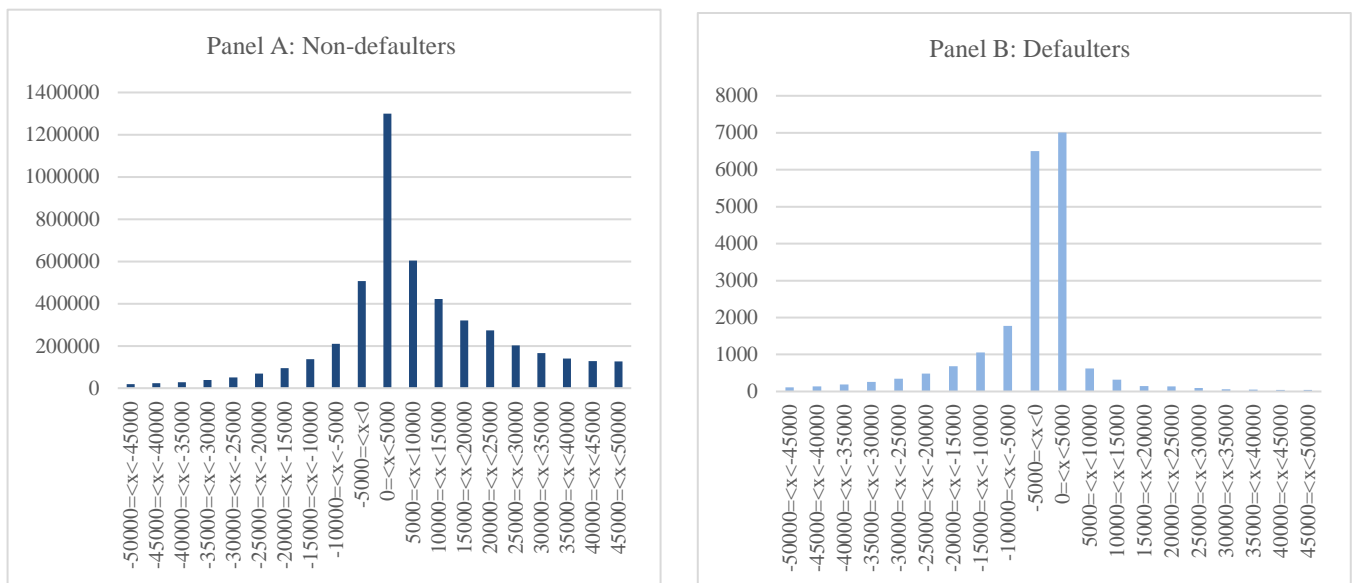
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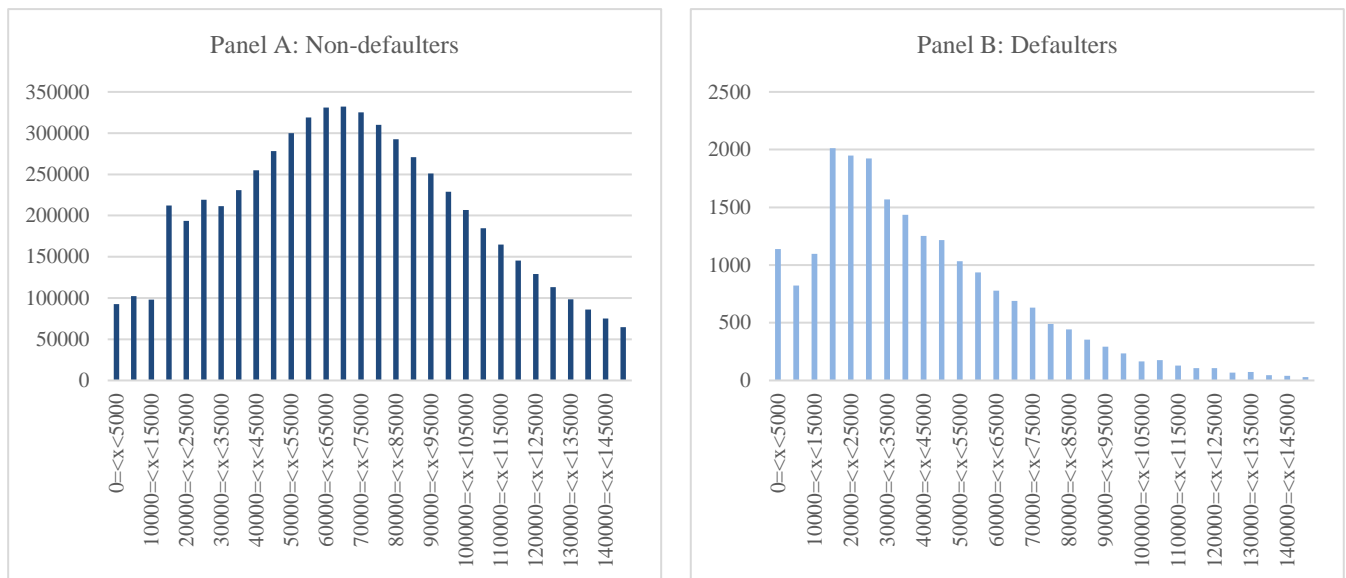
FIGURES AND TABLES

Figure 1. FREQUENCY GRAPHS FINANCIAL WEALTH FOR NON-DEFAULTERS AND DEFAULTERS (1 JANUARY 2013)



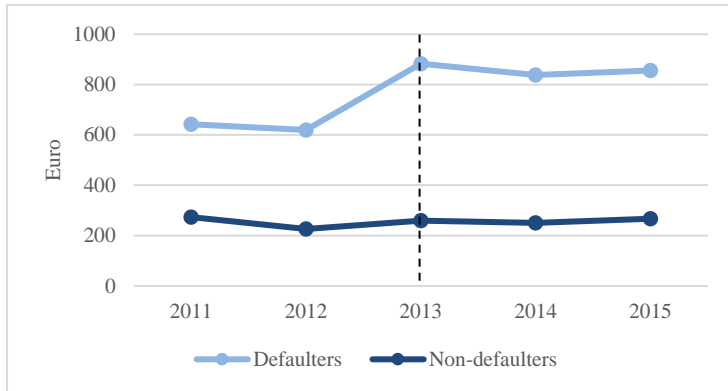
Notes: The figures show frequency graphs of financial wealth (capped at -50,000 and +50,000). Defaulters are those individuals who defaulted in 2013. Non-defaulters are those individuals who did not default during the entire sample period. Observations selected by criteria described in Section 4.2. In total, there were 4,859,971 observations for non-defaulters and 20,075 observations for defaulters.

Figure 2. FREQUENCY GRAPHS GROSS HOUSEHOLD INCOME (OVER 2012) FOR NON-DEFAULTERS AND DEFAULTERS



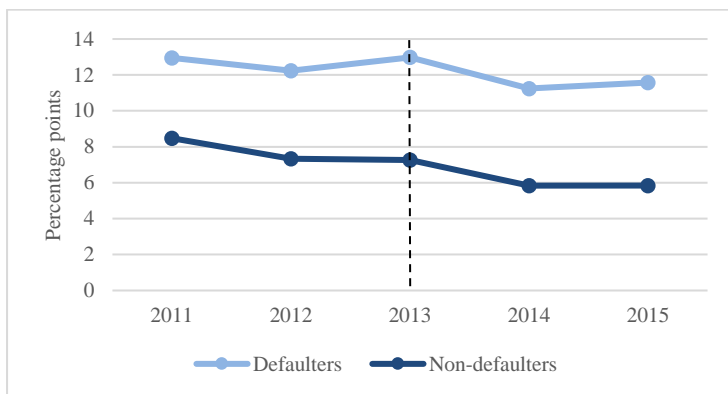
Notes: The figures show frequency graphs of gross household income (capped at +150,000). Defaulters are those individuals who defaulted in 2013. Non-defaulters are those individuals who did not default during the entire sample period. Observations selected by criteria described in Section 4.2. In total, there were 6,121,823 observations for defaulters and 21,228 observations for non-defaulters.

Figure 3. AVERAGE MENTAL HEALTHCARE EXPENDITURES FOR DEFAULTERS AND NON-DEFAULTERS OVER TIME



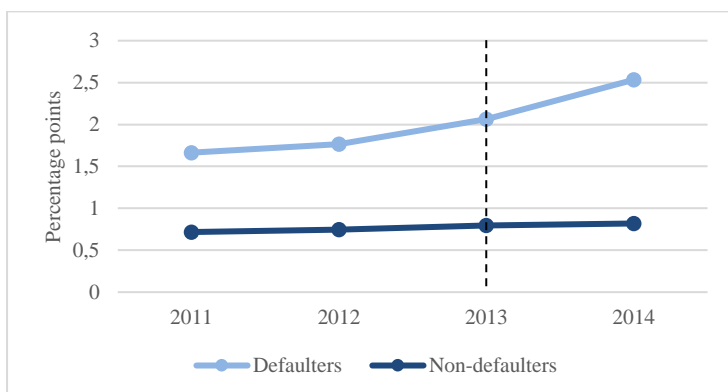
Notes: Defaulters are those individuals who defaulted in 2013. Non-defaulters are those individuals who did not default during the entire sample period. Observations selected by criteria described in Section 4.2. In total, there were 33,087,082 observations for non-defaulters and 106,854 observations for defaulters.

Figure 4. USE OF MENTAL HEALTHCARE FOR DEFAULTERS AND NON-DEFAULTERS OVER TIME



Notes: Defaulters are those individuals who defaulted in 2013. Non-defaulters are those individuals who did not default during the entire sample period. Observations selected by criteria described in Section 4.2. In total, there were 33,087,082 observations for non-defaulters and 106,854 observations for defaulters.

Figure 5. USE OF PERSONAL AND FINANCIAL ASSISTANCE FOR DEFAULTERS AND NON-DEFAULTERS OVER TIME



Notes: Defaulters are those individuals who defaulted in 2013. Non-defaulters are those individuals who did not default during the entire sample period. Observations selected by criteria described in Section 4.2. In total, there were 26,466,781 observations for non-defaulters and 85,461 observations for defaulters.

Table 1. DESCRIPTIVE STATISTICS

	Non-defaulters		Defaulters	
	2011-2012	2014-2015	2011-2012	2014-2015
Mental healthcare costs (average costs per year in euro)	250.0 (3520)	258.8 (2838)	631.0 (5717)	847.2 (6378)
Mental healthcare use (in %)	7.90 (26.98)	5.84 (23.46)	12.59 (33.17)	11.41 (31.79)
Social guidance and/or financial assistance (in %)	0.73 (8.53)	0.82 (9.01)	1.72 (12.98)	2.53 (15.71)
Age	35.7 (9.6)	38.7 (9.6)	32.7 (9.5)	35.8 (9.5)
Gender (1 = male; 0 = female)	0.494 (0.50)	0.494 (0.50)	0.597 (0.49)	0.597 (0.49)
Ethnicity (1 = immigrant; 0 = native)	0.211 (0.41)	0.211 (0.41)	0.484 (0.50)	0.483 (0.50)
Financial wealth lagged (in euro)	81,539 (443,478)	88,263 (488006)	5,854 (92556)	1,736 (75125)
Gross household income lagged (in euro)	74,116 (50,865)	83,342 (61570)	42,364 (32079)	41,358 (32157)
Use disability benefits lagged (1= use; 0 = non-use)	0.058 (0.23)	0.062 (0.24)	0.093 (0.29)	0.108 (0.31)
Experienced negative life event in year t (1= yes; 0 = no)	0.108 (0.31)	0.099 (0.30)	0.151 (0.36)	0.140 (0.35)
Experienced negative life event in prior year (1=yes; 0=no)	0.084 (0.28)	0.142 (0.35)	0.116 (0.32)	0.214 (0.41)

Notes: This table reports the simple annual averages and standard deviations for the pre-event period (2011-2012) and post-event period (2014-2015) for the treatment group (defaulters) and the comparison group (non-defaulters). Standard errors in parentheses. Observations selected by criteria described Section 4.2. The number of observations varies across variables. For the dependent variables, we have 13,226,179 observations for non-defaulters in the pre-period and 13,240,602 observations (mental healthcare costs and use) and 6,620,301 observations (use of social guidance and/or financial assistance) in the post-period. For defaulters we have 42,675 observations for the dependent variables in the pre-period and 42,786 (mental healthcare costs and use) and 21,393 (use of social guidance and/or financial assistance) in the post-period. The number of observations may slightly differ for the control variables. Only for the variables on experiencing a negative life event, we have much fewer observations (about 15% less observations for defaulters and 30% less for non-defaulters).

Table 2. TEST FOR COMMON TRENDS ASSUMPTION

	1	2	3
	Mental healthcare expenditures	Use of mental healthcare	Use of social guidance and/or financial assistance
D ₂₀₁₁	47.069*** (1.962)	0.012*** (0.000)	-0.029*** (0.003)
D ₂₀₁₄	24.423*** (1.961)	-0.015*** (0.000)	0.073*** (0.003)
D ₂₀₁₅	40.428*** (1.986)	-0.015*** (0.000)	
D ₂₀₁₁ *DF	-22.267 (56.284)	-0.004 (0.003)	-0.067 (0.088)
D ₂₀₁₄ *DF	196.617*** (64.136)	0.005* (0.003)	0.700*** (0.123)
D ₂₀₁₅ *DF	197.916*** (64.672)	0.008*** (0.003)	
Observations	26,552,242	26,552,242	19,910,548
R-squared	0.410	0.497	0.799
Individual FE	YES	YES	YES

Notes: Table reports estimates from equation (2). The variables ‘use of mental healthcare’ and ‘use of social guidance and/or financial assistance’ are equal to 100 if someone uses care and zero otherwise. Standard errors are reported between brackets and are clustered at the individual level. For the use of social guidance and/or financial assistance we lack observations for the year 2015. Observations selected by criteria described in Section 4.2. *** Significant at the 1 percent level. * Significant at the 10 percent level.

Table 3. ESTIMATED EFFECTS OF PROBLEMATIC DEBTS ON MENTAL HEALTHCARE EXPENDITURES

	Baseline	Baseline with controls	Low wealth	Low income	Negative life event
D ₂₀₁₂	-47.00*** (1.96)		-72.91*** (4.29)	-124.72*** (7.41)	-37.46*** (8.13)
D ₂₀₁₄	-22.61*** (1.71)	24.10*** (2.86)	-42.08*** (3.65)	-116.99*** (6.05)	-56.90*** (7.32)
D ₂₀₁₅	-6.61*** (1.74)	44.52*** (2.91)	-22.29*** (3.68)	-90.15*** (6.12)	-50.02*** (7.15)
D ₂₀₁₄ *DF	207.79*** (56.01)	233.97*** (82.94)	217.77*** (60.76)	308.73*** (85.03)	100.92 (153.79)
D ₂₀₁₅ *DF	209.09*** (55.81)	222.15*** (82.46)	208.64*** (61.59)	297.89*** (82.88)	420.91** (190.12)
Gross household income (L) (quintile 2) (ref. category: low income)		40.25*** (7.88)			
Gross household income (L) (quintile 3)		52.61*** (8.63)			
Gross household income (L) (quintile 4)		51.55*** (8.30)			
Gross household income (L) (quintile 5)		48.96*** (8.55)			
Financial wealth (L) (quintile 2) (ref. category: low wealth)		-27.47*** (6.71)			
Financial wealth (L) (quintile 3)		-36.27*** (6.73)			
Financial wealth (L) (quintile 4)		-30.21*** (6.87)			
Financial wealth (L) (quintile 5)		-30.72*** (7.02)			
Negative life event (1 = yes; 0 = no)		55.42*** (5.11)			
Negative life event (L) (1 = yes; 0 = no)		29.99*** (4.52)			
Social benefits (L) (1=yes; 0=no)		-321.14*** (30.33)			
Individual FE	YES	YES	YES	YES	YES
Observations	26,552,242	13,325,221	9,360,256	6,010,733	2,340,200
R-squared	0.410	0.479	0.412	0.409	0.428

Notes: Table reports estimates from equation (1). (L) refers to lagged. All specifications include individual fixed effects. Standard errors are reported between brackets and are clustered at the individual level. For the use of social guidance and/or financial assistance we lack observations for the year 2015. Observations selected by criteria described in Section 4.2 or Section 5.2. *** Significant at the 1 percent level. ** Significant at the 5 percent level.

Table 4. ESTIMATED EFFECTS OF PROBLEMATIC DEBTS ON MENTAL HEALTHCARE USE

	Baseline	Baseline with controls	Low wealth	Low income	Negative life event
D ₂₀₁₂	-1.149*** (0.013)		-1.573*** (0.024)	-2.193*** (0.032)	-1.120*** (0.050)
D ₂₀₁₄	-2.631*** (0.014)	-1.551*** (0.018)	-3.115*** (0.026)	-4.034*** (0.035)	-3.836*** (0.053)
D ₂₀₁₅	-2.635*** (0.014)	-1.476*** (0.018)	-3.062*** (0.027)	-4.035*** (0.036)	-3.957*** (0.053)
D ₂₀₁₄ *DF	0.717*** (0.266)	0.499 (0.363)	1.022*** (0.287)	1.783*** (0.362)	0.904 (0.753)
D ₂₀₁₅ *DF	1.048*** (0.277)	0.805** (0.374)	1.268*** (0.300)	1.808*** (0.376)	0.838 (0.776)
Gross household income (L) (quintile 2) (ref. category: low income)		0.258*** (0.049)			
Gross household income (L) (quintile 3)		0.525*** (0.052)			
Gross household income (L) (quintile 4)		0.712*** (0.054)			
Gross household income (L) (quintile 5)		0.778*** (0.057)			
Financial wealth (L) (quintile 2) (ref. category: low wealth)		-0.195*** (0.038)			
Financial wealth (L) (quintile 3)		-0.220*** (0.042)			
Financial wealth (L) (quintile 4)		-0.230*** (0.047)			
Financial wealth (L) (quintile 5)		-0.312*** (0.054)			
Negative life event (1 = yes; 0 = no)		0.935*** (0.030)			
Negative life event (L) (1 = yes; 0 = no)		0.229*** (0.027)			
Social benefits (L) (1=yes; 0=no)		2.222*** (0.133)			
Individual FE	YES	YES	YES	YES	YES
Observations	26,552,242	13,325,221	9,360,256	6,010,733	2,340,200
R-squared	0.497	0.557	0.516	0.556	0.501

Notes: Table reports estimates from equation (1). The dependent variable is equal to 100 if someone uses mental health care and zero otherwise. (L) refers to lagged. All specifications include individual fixed effects. Standard errors are reported between brackets and are clustered at the individual level. For the use of social guidance and/or financial assistance we lack observations for the year 2015. Observations selected by criteria described in Section 4.2 or Section 5.2. *** Significant at the 1 percent level. ** Significant at the 5 percent level.

Table 5. ESTIMATED EFFECTS OF PROBLEMATIC DEBTS ON SOCIAL GUIDANCE AND/OR FINANCIAL ASSISTANCE

	Baseline	Baseline with controls	Low wealth	Low income	Negative life event
D ₂₀₁₂	0.030*** (0.003)		0.084*** (0.006)	0.084*** (0.009)	0.087*** (0.010)
D ₂₀₁₄	0.102*** (0.004)	0.094*** (0.005)	0.207*** (0.008)	0.271*** (0.014)	0.208*** (0.015)
D ₂₀₁₄ *DF	0.734*** (0.121)	0.764*** (0.169)	0.671*** (0.135)	0.867*** (0.187)	1.365*** (0.367)
Gross household income (L) (quintile 2) (ref. category: low income)		-0.080*** (0.024)			
Gross household income (L) (quintile 3)		-0.067*** (0.024)			
Gross household income (L) (quintile 4)		-0.072*** (0.024)			
Gross household income (L) (quintile 5)		-0.065*** (0.024)			
Financial wealth (L) (quintile 2) (ref. category: low wealth)		0.004 (0.018)			
Financial wealth (L) (quintile 3)		-0.045*** (0.017)			
Financial wealth (L) (quintile 4)		-0.035** (0.017)			
Financial wealth (L) (quintile 5)		-0.019 (0.018)			
Negative life event (1 = yes; 0 = no)		0.038*** (0.011)			
Negative life event (L) (1 = yes; 0 = no)		0.034*** (0.012)			
Social benefits (L) (1=yes; 0=no)		0.847*** (0.078)			
Individual FE	YES	YES	YES	YES	YES
Observations	19,910,548	8,880,121	7,018,705	4,505,857	1,755,150
R-squared	0.799	0.833	0.792	0.819	0.783

Notes: Table reports estimates from equation (1). The dependent variable is equal to 100 if someone uses social guidance and/or financial assistance and zero otherwise. (L) refers to lagged. All specifications include individual fixed effects. Standard errors are reported between brackets and are clustered at the individual level. For the use of social guidance and/or financial assistance we lack observations for the year 2015. Observations selected by criteria described in Section 4.2 or Section 5.2. *** Significant at the 1 percent level. ** Significant at the 5 percent level.