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# In Sickness and in Health?
## Health Shocks and Relationship Breakdown: Empirical Evidence from Germany*

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**Abstract**

From an economic perspective, marriage and long-term partnership can be seen as a risk-pooling device. This informal insurance contract is, however, not fully enforceable. Each partner is free to leave when his or her support is needed in case of an adverse life event. An adverse health shock is a prominent example for such events. Since relationship breakdown itself is an extremely stressful experience, partnership may backfire as informal insurance against health risks, if health shocks increase the likelihood of relationship breakdown. We address this question empirically, using survey data from Germany. Results from various matching estimators indicate that adverse shocks to mental health substantially increase the probability of a couple splitting up over the following two years. In contrast, there is little effect of a sharp decrease in physical health on relationship stability. If at all, physical health shocks that hit both partners simultaneously stabilize a relationship.

*JEL codes*: I12, J12, D13.
*Keywords*: separation, partnership dissolution, health shock, MCS, PCS, matching.

*Address for correspondence: Harald Tauchmann, Professur für Gesundheitsökonomie, Findelgasse 7/9, 90402 Nürnberg, Germany. Email: harald.tauchmann@fau.de. Phone: +49 (0)911 5302 635. We would like to thank Bettina Siflinger and Eugenio Zucchelli as well as the participants of the 2018 dggö annual meeting at the University of Hamburg, the 2019 Essen Mental Health Economics Workshop, the 2019 annual meeting of the VfS Health Economics Study Group, and the participants of the Economics Research Seminars at the Universities of Göttingen, Bamberg, and Portsmouth for many valuable comments and suggestions. We gratefully acknowledge excellent research assistance from Franziska Valder and Irina Simankova.

# 1 Introduction

Many economists think of marriage and long-term relationships as risk-pooling instruments (e.g. Weiss, 1997; Dercon and Krishnan, 2000; Schmidt, 2008; Chiappori et al., 2018).[1] Income uncertainty is the most obvious risk against which long-term partnership may provide informal insurance. Yet, a relationship may also provide informal insurance against non- or just partially monetary adverse life events, such as career and social disappointments, loss of relatives and friends, and in particular negative health shocks. While the income security from marriage can be substituted by the purchase of formal insurance on the market, there is no formal insurance mechanism for emotional support after adverse life events.

Marriages and long term-relationships as implicit insurance contracts are – if at all – only partially enforceable. While withdrawing from financial obligations may not be possible in case of divorce, emotional support can be denied at any time. In particular, each partner is free to leave if one does not want to share the (non-monetary) burden of an adverse shock that hits the spouse. Ironically, partnership breakdown itself is a particularly stressful life event (Holmes and Rahe, 1967; Scully et al., 2000; Dolan et al., 2008). In consequence, if a negative health shock results in partnership breakdown, partnership does not only fail but may even backfire as informal insurance mechanism. In this paper, we show that the informal insurance that marriage provides against the emotional strain of an adverse life effect – here a health shock – works only for selected types of events and fails for others.

The statistical association of health and relationship – in particular marital – status is well established in the empirical literature (e.g. Schoenborn, 2004; Wilson and Oswald, 2005; Wood et al., 2009; Koball et al., 2010). Most relevant to our analysis, Kohn and Averett (2014b) and Lillard and Panis (1996) find that poor health is associated with a higher probability of divorce. In a related analysis that focuses on the interplay of mental health and marital transitions, Wade and Pevalin (2004) not only find divorce to be a predictor of poor mental health but also that divorce rates are higher among those who had poor mental health in the past. Johnson and Wu (2002) conduct an empirical analysis similar to Wade and Pevalin (2004) and disentangle different channels through which psychological distress and marital disruption are linked and find that selection out of marriage due to poor mental health may play a role in the link between psychological health and relationship breakdown.

The majority of papers however focuses on marriage – or cohabitation – as a determinant of health. Yet, in several contributions to this literature the reverse direction of causation still comes into play via selection into marriage being identified as affected by health.[2] That

[1] Further economic gains from marriage, such as economies of scale and efficiency gains through intra-household division of labor, are nevertheless emphasized even more in the economic literature, for instance in the seminal work of Becker (Becker, 1973, 1974; Becker et al., 1977).

[2] Another strand of the literature exclusively focuses on the role of health for selection into marriage (e.g. Mastekaasa, 1992; Manfredini et al., 2010; Lipowicz, 2014). The latter, for instance, links various health measures measured at pre-marriage age to later marital status. This generates strong indication for good health being a critical success factor at the marriage market. Yet, this is a different question than the one regarding the link between partnership stability and health shocks.

is, in a major share of this literature, effects of health on relationship status are primarily regarded as an obstacle to identifying the effect of prime interest that needs to be dealt with (Lillard and Panis, 1996; Brockmann and Klein, 2004; Averett et al., 2013; Kohn and Averett, 2014b,a; van den Berg and Gupta, 2015; Guner et al., 2018).

In this paper we contribute to the small literature that directly analyses the effect of poor health or negative health shocks on partnership stability.[3] An early explicit analysis on the topic comes from Merikangas (1984) who uses a rather small and intentionally selective sample of married individuals who suffer from depression and finds that the probability of later divorce is substantially higher if the spouse also suffers from mental disorders. This telling yet purely descriptive result may however not be informative about the effect of poor health in the general population. Using survey data from the US, Booth and Johnson (1994) find a negative association of self-reported health and self-reported marital quality and marital happiness, which they interpret as adverse effects of deteriorating health. Though they discuss several channels through which these variables might be linked in a non-causal way, the analysis does little to isolate the effect of interest besides controlling for lagged health and lagged outcome variable. In a descriptive study using the National Co-morbidity Survey from the US, Kessler et al. (1998) document a significant association of later divorce with several mental disorders. Based on longitudinal data from the Dutch city of Eindhoven, Joung et al. (1998) examine the association of self-reported health and several marital transitions between the states unmarried, married, divorced, and widowed. Only the transition from 'married' to 'divorced' is significantly correlated with health which can be interpreted as suggestive evidence for an effect of health on partnership stability. Pevalin and Ermisch (2004) use data on cohabiting but unmarried individuals from the British Household Panel and find that the risk of dissolution of a cohabiting union is positively associated with poor mental health in the previous year for men. The corresponding result for women is less clear but points in the same direction.[4] Negrusa and Negrusa (2014), to which in some respects our paper is most closely related, use longitudinal information on deployed US soldiers to establish a strong detrimental effect of post-traumatic stress disorder (PTSD) on marriage stability. Stressing that conditional on deployment developing PTSD is largely a matter of exogenous factors, they interpret this effect as causal. This argument is strengthened by instrumenting PTSD with, for instance, actual involvement in combats, which qualitatively does not change the key result. Negrusa and Negrusa (2014) do however not find an effect of general health on the probability of divorce. Referring

[3]Chiappori et al. (2018) is another recent contribution, to which our analysis is related, but is not part of this literature. This paper also focusses on (monetary and non-monetary) shocks as determinants of marital dissolution. However, Chiappori et al. (2018) do not think of non-monetary shocks as health shocks but rather as shocks to life-satisfaction net of – among others – health effects, and interpret them as shocks to a couple's matching quality. The roles of health and partnership stability are hence somehow reversed in our study as compared to Chiappori et al. (2018). While the latter aims on purging the effect of partnership quality from possibly confounding health effects, we – in some specifications, see section 4.2.2 – control for measures of partnership quality in order to isolate the effect of health shocks.

[4]The results regarding the effect of poor mental health on the probability to marry are however inconclusive. Though the empirical evidence is suggestive, it seems still questionable whether the estimated relative risks reflect a causal effect of mental health on partnership stability. Considering lagged rather than contemporaneous mental health as explanatory variable suggests that the direction of causation is from mental health to partnership stability. However, one still cannot rule out that mental health and relationship quality interactively deteriorate over time, ultimately resulting in a separation.

to earlier work (e.g. Charles and Stephens, 2004) that did not establish negative effects of disability on marriage stability, they hypothesize that shocks to mental and shocks to physical health may exert different effects on the probability of divorce.

Our paper contributes to the existing literature in several dimensions. Similar to Negrusa and Negrusa (2014), we separate the effect of a sharp worsening of health from the role the level of health plays for relationship stability. Yet, unlike Negrusa and Negrusa (2014), our analysis is not restricted to a very specific population. Similar to Pevalin and Ermisch (2004), we base our analysis on a population survey. More precisely, we use data from the German Socioeconomic Panel (SOEP). To our knowledge, the present paper is the first in this mainly US and UK dominated empirical literature to use data from Germany. Moreover, we do not analyze the effects of a specific health shock like developing PTSD or becoming permanently disabled but consider general health shocks. We nevertheless distinguish shocks on mental and on physical health and do not restrict the analysis to one dimension of health. Every other year, the SOEP includes the SF12 questionnaire and aggregates the results to a mental as well as a physical health index (the mental health component scale (MCS) and the physical health component scale (PCS) respectively). Additionally, we do not restrict our analysis to married couples but also consider partners that cohabit without being married and in some variants of our empirical model we also include homosexual couples.

We estimate effects of health shocks on partnership stability that are more relevant to the general population than those estimated for specific sub-populations and specific health events. As we cannot exploit purely exogenous sources of variation in general health, we rely on matching estimators to address the possible endogeneity of changes in mental and physical health. In addition, we bound the size of a possible endogeneity bias using the method suggested by Oster (2019). Our results show that mental health shocks increase the probability of relationship breakdown while physical health shocks may even stabilize partnerships. The remainder of the paper is organized as follows. In Section 2, we introduce the data and describe how the key variables are constructed. We discuss our empirical approach in Section 3 and present our estimation results in Section 4, followed by a concluding Section 5.

# 2 Data

## 2.1 Data Source

We use data from the German Socio-Economic Panel (SOEP), a large annual longitudinal household survey that started in 1984 (Goebel et al., 2019; SOEP, 2019) and can be regarded as the German counterpart to the British Household Panel (BHPS), which is used in several related studies (Wade and Pevalin, 2004; Pevalin and Ermisch, 2004; Kohn and Averett, 2014b,a). Even though the SOEP comprises rich, retrospective information about the partnership histories of the survey respondents that partly dates back into time long before the start of SOEP, we can only use the panel waves from 2002 on, after health information from the SF12 was included in the survey. Moreover, the SF12 questionnaire is part of the survey only every other year, we therefore use a biennial panel for our analysis.

## 2.2 Couples and Separations

In our empirical analysis, we consider the 'couple' the unit of observation. A couple is defined as two individuals in the SOEP who mutually identify themselves as partners. This effectively implies living together in one household as the SOEP is a household survey that collects information about all household members but not about individuals living in a different household, even if strong social ties exist.[5] One may regard excluding non-cohabiting couples from the analysis as rather restrictive. Yet, as we look at partnership from a risk pooling perspective, living together can be regarded as a suitable criterion for distinguishing romantic affairs from relationships in which sharing economic resources and life risks plays a significant role. Though the vast majority of such defined couples are married couples, the analysis is not confined to this group.

Since a couple consists of two partners, distinguishing two 'roles' within a couple is tempting, in particular if one is interested in heterogeneous effects of health shocks within a couple. A traditional way of defining two roles in a couple is to distinguish between the female (♀) and the male (♂) partner. This allows for addressing the question of whether it makes a difference if the male or if the female partner experiences a health shock. We choose this traditional model as reference. One drawback of this model is that it does not allow for considering homosexual couples. Moreover, with respect to partnership as risk-pooling instrument, the sex of the partners might be an ill-suited criterion for differentiating the partners. Hence, we also estimate an alternative model that considers the roles 'main breadwinner' (☉) and 'partner of main breadwinner' (☾). These roles may better capture economic – and possibly bargaining – power within a couple that may matter for how partners cope with adverse events that hit the couple. Unlike the traditional man-woman model, the alternative model in principal allows for switching roles within an existing couple. Moreover, the main breadwinner-partner model allows for considering homosexual couples. For roughly three in four couple-year observations, the main breadwinner is male.

Our final sample consists of seven biennial panel waves (2004–2016) and comprises 10 055 couples and 30 296 couple-year observations. Roughly one third of all couples enter the estimation sample in only one[6] wave, while 12 percent of them are present in each of the seven waves. It is possible that different couples are linked by individuals who have relationships with different partners in their lives. This conflicts with the idea that couples are independent observational units. We hence identify couple networks in the data, that is couples that are directly or indirectly linked through individuals they share and use these couple networks for clustering estimated standard errors. In the population some of these couple networks are presumably very large and connect even very distant individuals. The couple networks we identify in our data are however rather small. This is explained by the

[5]Very limited information is available even for some partners who do not live in a 'SOEP household'. Yet this information does not originate from a personal questionnaire and, in consequence, does not comprise the health information that is required in our analysis. One exception is the rare case of one partner leaving the household while not exiting the partnership. In this case, the partner is tracked by the SOEP constituting a 'SOEP couple' that lives in different households.

[6]This still requires that the members of a couple participate in the SOEP for several years; see Figure 1.

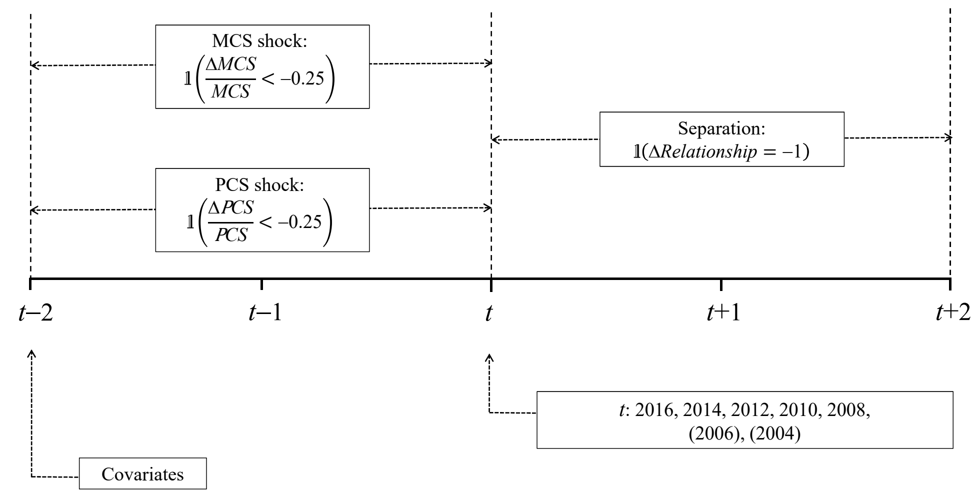


Figure 1: Time Line for Construction of Key Variables

SOEP being just a small sample from the population and by considering a relatively short period of time in our analysis. The number of couple networks (9 859) is not much smaller than the total number of couples, which implies that we observe most of the individuals in just one couple.

The outcome we consider in the empirical analysis is whether or not an existing relationship breaks down. This binary variable is constructed according to our partnership definition. A couple splits up, if two partners who have mutually identified themselves as partners in the previous period no longer do this. This may or may not involve the formation of new couples. In some sense the outcome is whether or not an observational unit disappears from the sample and in consequence is not observed in the subsequent period. One may, for this reason, interpret the analysis as estimating a hazard model in discrete time.[7] Consistent with the availability of health information we stick to the biennial structure of the panel. That is the final panel wave that we use for conditioning on existing partnerships is 2016 while the corresponding outcome 'separation (within the next two years)' is constructed from the 2018 wave of the SOEP (see Figure 1 for an illustration how information from different panel waves is used for constructing the key variables). This definition of the outcome variable is prone to confusing separations with other reasons for a couple disappearing from the data. We hence identify couples in which one partner dies and do not regard this event as separation. Moreover, a separation requires that at least one partner is still observed in the SOEP. This guarantees that panel attrition is not erroneously counted as separation. The biennial panel structure hence ignores temporary

[7]Very few couples are observed to restore their partnership after having split up. The event 'separation' is hence quasi non-repeated.

separations, if the partners restore their relationship within two years relative to the year of reference.[8] Based on this definition, we observe 784 separations in the estimation sample, which corresponds to an average unconditional separation rate of 0.026, see Table 1, upper panel. This seems to be a rather small number compared to the divorce rate of 35 percent that is reported for Germany (Destatis and WZB, 2016, p. 50). Due to a relatively short observation period, one should be aware that the number of observed separations over the number of couples is not an appropriate estimate of the probability that a couple ever splits up. Indeed, considering all available panel waves we observe separations for roughly 20 percent of all couples, which is still a conservative estimate due to censoring and panel attrition.

## 2.3 Health and Health Shocks

The focus of the analysis is on the effect of adverse health shocks. While we want to distinguish between a sharp deterioration in mental and in physical health, we do not focus on a very specific health event to preserve the spirit of general health shocks. By including the SF12v2 questionnaire (Ware et al., 2005) in the survey, the SOEP provides a well suited basis for an empirical analysis of general mental and physical health. The standardized SF12 questionnaire comprises a list of twelve questions concerning physical, mental, and emotional well being.[9] By the means of an explorative factor analysis two factors, the MCS (mental component summary scale) and the PCS (physical component summary scale) are extracted from the information provided through the SF12. This procedure is carried out by the SOEP group and the PCS and MCS are provided as part of the SOEP data.[10] Figure A1 in the appendix depicts the distributions of MCS and PCS in the sample. The virtue of this approach to measuring health is that at the one hand it captures self-perceived subjective health rather than a specific diagnosis that may be of different importance to individual well being. On the other hand, it allows for clearly differentiating between mental and physical health. Moreover, MCS and PCS are well established health measures that are advocated as screening tools for quickly identifying health deficits (Salyers et al., 2000; Gill et al., 2007; Huo et al., 2018).

As pointed out, we are less interested in the levels of mental and physical health as determinants of partnership stability but how robust partnerships are to a sudden deterioration of health. In other words, we do not consider PCS and MCS as the key regressors in our analysis but changes in these variables. Figure A1 displays the sample distributions of relative and absolute changes in the MCS and in the PCS, respectively. Obviously, there is no universal answer to the question of how severe a deterioration in health needs to be in order to constitute a negative health shock. In the present analysis

[8]As a robustness check, we also considered the alternative outcome variable ‘separation within the next year’. In terms of the results, this did not make much difference. We still prefer the outcome ‘separation within the next two years’. Otherwise we would ignore separations that occur between 12 and 24 months after the point of reference. We only deviate from the biennial framework for couples that are observed to have separated after one year and then drop out from the SOEP. We regard this pattern as separation.

[9]It is a reduced variant of the SF36 questionnaire.

[10]See Andersen et al. (2007) for a detailed description of how PCS and MCS are generated on basis of the SOEP data.

we consider a loss in MCS and PCS by more than 25 percent as experiencing a mental and physical health shock respectively.[11] This threshold has been used earlier in the literature (Bünnings, 2017; Li et al., 2019) with respect to MCS and PCS, and also with respect to other health measures such as grip strength (Decker and Schmitz, 2016). Though this definition of a shock is arbitrary to some extent, it still captures the notion of an extraordinary adverse health event as just roughly one in twenty respondents exhibits such severe reductions in MCS or PCS. In Section 4.2 we show results for estimations using alternative definitions of health shocks. They are rather similar to those we get from using the health shock definition of reference. As the SOEP includes MCS and PCS only every other year, health shocks are necessarily defined on basis of a change over two years. Since health shocks may result in future separations but cannot cause relationship breakdown in the past, the two year interval on which a health shock is defined needs to proceed the two year interval in which a separation may happen. Figure 1 illustrates that for this reason several panel waves that span four years are required for constructing the key variables. This explains why the estimation sample is relatively small given an observation period of 16 years.

Each partner may suffer from a MCS or from a PCS shock and a couple might hence be hit by four different health shocks. Table 1, upper panel, provides descriptive statistics for these variables. The corresponding statistics for the model that differentiates between main breadwinner and partner are found in Table A1, upper panel in the Appendix. Health shocks are relatively rare events, each being observed for 5 to 6 percent of couple-year observations. While shocks to mental health are slightly more frequent among women, physical health shocks occur at similar rates for women and men. As the empirical analysis considers four 'treatments', there is much room to analyze various treatment interactions. We focus on one particular sort of interactions, joint health shocks. More specifically we are interested in the effect of both partners (♂♀ or ⊙☾ ) being hit by a shock of the same kind. Descriptive statistics for the corresponding interaction variables are presented in Table 1, upper panel. Although 'joint' health shocks that hit both partners within the same two-years interval are rare, according to the descriptive statistics they occur more frequently than one would expect if health shocks were uncorrelated across both partners. This in particular applies to MCS shocks. This correlation pattern suggests that these shocks might not be purely random. The key objective of our analysis is to identify the effect of health shocks on partnership stability, which should not be intermixed with the effect of the partners' health levels may have. Therefore, we condition on the levels of MCS and PCS of both partners prior to the (possible) occurrence of a health shock. The corresponding descriptives are also displayed in Table 1, upper panel. Since MCS and PCS are standardized variables[12] these statistics are of limited informational value, apart from women being in somewhat poorer – in particular mental – health as compared to their male partners.

[11] This measure does not distinguish between a permeant and a transitory deterioration of health. Indeed, either sort of shock appears to be present in the estimation sample. This rises the question, of whether any possible effect of health shocks on partnership stability might be driven by the group of permanently affected couples. Yet, this is not warranted by the data, since future (post-treatment) changes in health have virtually no predictive power for separations.

[12] PCS and MCS are both standardized to have a mean of 50 and a variance of 10 in the full sample. In order to align them with the remaining variables, we re-scaled them by the factor 0.01.

## 2.4 Covariates

If health shocks were purely random, one could identify their effects on partnership breakdown straightforwardly by just comparing separation rates without considering any further variables. However, as discussed in Section 1, the interrelation of health and relationship stability is complex. In particular confounding variables may exert effects on health as well as relationship stability. In order to isolate the effect of health shocks we condition on several covariates. All time-variant covariates are measured prior to a possible health shock, i.e. two years before the year of reference (see Figure 1). We condition on covariates that are observed on the individual level, i.e. they enter the empirical analysis once for each partner, as well as covariates that are observed on the couple level. The former are age, years of education, an indicator for being employed, and personal gross income [€1 000/month]. The latter is the sum of personal labor and pension income. As expected, male partners are on average older, slightly longer educated, more frequently employed and have a substantially higher personal income. Since the match between the partners is likely to matter for partnership stability and may also be linked to health, we construct variables that capture how different the partners are: the absolute difference in age, the absolute difference in years of education, and share in total labor and pension income that is earned by the male partner.[13] Conditioning variables that are measured at the couple level are indicators for living in the eastern part of Germany, being married, being homeowner, and a dummy indicating that at least one child under 17 lives in the household. The latter three are often regarded as important stabilizers of relationships. For couples with under aged kids we additionally condition on the number of children, the age of the youngest child, and whether the partners are jointly parents to at least one of the children.[14] Finally we condition on the previous duration of the partnership.[15] See Table 1, mid panel, for descriptive statistics for the estimation sample.[16] We also include a set of year dummies in order to neutralize any spurious correlation between relationship breakdown and health driven by some underlying temporal development.

An important confounder in our analysis is partnership quality. Usually a separation will be preceded by a period of poor relationship quality (cf. Chiappori et al., 2018). At the same time a low-quality marriage or partnership is less likely to generate health benefits (Wu and Hart, 2002) or may even result in declining (mental) health (Wickrama et al., 1997). Unfortunately, partnership quality cannot be observed directly. The closest proxy available in the SOEP is self-reported satisfaction with family life measured on a ten-point scale ranging from low (0) to high (10), a question that has not been included in the survey prior to 2006. In consequence, including this information as covariat reduces the

[13] For couples without labor or pension income from either partner, this variable is defined to take the value of 0.5 in order to indicate equal personal income.

[14] The reported value of 65 percent most likely underestimates the true share, since the SOEP does not allow for identifying the relationship of a child to the partner of the mother if he is not the head of the household.

[15] Unfortunately the reported partnership history is incomplete for numerous couples, for long-lasting partnerships in particular. This is the reason for also including a censoring dummy indicating that we could not track the relationship back to its start.

[16] In the model that considers the roles 'main breadwinner' and 'partner' and allows for considering homosexual couples a dummy for 'homosexual' and one for 'male homosexual' are also included. Descriptive statistics for this are found in Table A1 in the Appendix.

Table 1: Descriptive Statistics for Estimation Sample

| | Mean | S.D. | Med. | Min. | Max. |
|---|---|---|---|---|---|
| separation | 0.026 | 0.159 | 0 | 0 | 1 |
| MCS shock$_{♂}$ | 0.056 | 0.229 | 0 | 0 | 1 |
| MCS shock$_{♀}$ | 0.073 | 0.259 | 0 | 0 | 1 |
| PCS shock$_{♂}$ | 0.057 | 0.231 | 0 | 0 | 1 |
| PCS shock$_{♀}$ | 0.060 | 0.237 | 0 | 0 | 1 |
| MCS shock$_{♂♀}$ | 0.011 | 0.105 | 0 | 0 | 1 |
| PCS shock$_{♂♀}$ | 0.006 | 0.077 | 0 | 0 | 1 |
| MCS$_{t-2,♂}{}^{\flat}$ | 0.522 | 0.091 | 0.537 | 0.088 | 0.794 |
| MCS$_{t-2,♀}{}^{\flat}$ | 0.502 | 0.097 | 0.516 | 0.019 | 0.777 |
| PCS$_{t-2,♂}{}^{\flat}$ | 0.493 | 0.095 | 0.513 | 0.092 | 0.725 |
| PCS$_{t-2,♀}{}^{\flat}$ | 0.491 | 0.097 | 0.509 | 0.101 | 0.781 |
| age$_{♂}$ | 54.79 | 13.96 | 54 | 21 | 98 |
| age$_{♀}$ | 52.02 | 13.81 | 51 | 20 | 92 |
| abs. age difference | 3.801 | 3.622 | 3 | 0 | 37 |
| education$_{t-2,♂}$ | 12.69 | 2.825 | 11.5 | 7 | 18 |
| education$_{t-2,♀}$ | 12.23 | 2.609 | 11.5 | 7 | 18 |
| abs. educ. difference$_{t-2}$ | 1.686 | 1.91 | 1 | 0 | 11 |
| east$_{t-2}$ | 0.248 | 0.432 | 0 | 0 | 1 |
| home owner$_{t-2}$ | 0.611 | 0.487 | 1 | 0 | 1 |
| child in hh$_{t-2}$ | 0.352 | 0.478 | 0 | 0 | 1 |
| common child$_{t-2}{}^{\sharp}$ | 0.648 | 0.477 | 1 | 0 | 1 |
| # of children$_{t-2}{}^{\sharp}$ | 1.728 | 0.817 | 2 | 1 | 8 |
| min. age child$_{t-2}{}^{\sharp}$ | 7.628 | 4.776 | 7 | 0 | 16 |
| married$_{t-2}$ | 0.890 | 0.313 | 1 | 0 | 1 |
| partnership duration$_{t-2}$ | 8.893 | 7.412 | 6 | 0 | 30 |
| censored couple info$_{t-2}$ | 0.751 | 0.432 | 1 | 0 | 1 |
| employed$_{t-2,♂}$ | 0.706 | 0.456 | 1 | 0 | 1 |
| employed$_{t-2,♀}$ | 0.613 | 0.487 | 1 | 0 | 1 |
| employed$_{t-2,♂♀}$ | 0.537 | 0.499 | 1 | 0 | 1 |
| income$_{t-2,♂}$ | 2.897 | 2.639 | 2.4 | 0 | 116.2 |
| income$_{t-2,♀}$ | 1.193 | 1.389 | 0.82 | 0 | 45 |
| income share$_{t-2,♂}$ | 0.694 | 0.257 | 0.706 | 0 | 1 |
| satisfaction fam. life$_{t-2,♂}{}^{\dagger}$ | 8.294 | 1.525 | 8 | 0 | 10 |
| satisfaction fam. life$_{t-2,♀}{}^{\dagger}$ | 8.197 | 1.638 | 8 | 0 | 10 |
| abs. dif. satisf. fam. life$_{t-2}{}^{\dagger}$ | 0.970 | 1.194 | 1 | 0 | 10 |

**Notes:** Statistics based on 30 296 couple-year observations; model that differentiates between male and female partner ignoring homosexual couples; seven panel waves (biennially 2004–2016); $^{\dagger}$ not available for years earlier than 2008; $^{\sharp}$ conditional on children in household; $^{\flat}$ rescaled to the unit interval; see Table A1 for corresponding statistics for the alternative 'main breadwinner-partner' model and Table A2 for descriptive statistics regarding character traits. **Source**: Own calculations based on SOEP data.

estimation sample from seven to only five panel waves.[17] Unlike the remaining variables, the descriptive statistics reported for family-life satisfaction in Table 1, lower panel, hence refer to the years 2008 to 2016. Satisfaction with family life does not seem to differ much between men and women and is high on average. Yet, even very conflicting perceptions regarding the quality of a partnership occur in the sample as indicated by the max of the variable 'absolute difference in satisfaction with family life'. Furthermore, we condition our analysis on partner characteristics that are possibly related to partnership quality by considering measures of character traits ('big five', i.e. agreeableness, conscientiousness, neuroticism, extraversion, openness) for each partner as additional covariates. Since this

[17] In 2013 the SOEP also included a question regarding satisfaction with partnership. Yet this question was not part of the regular version of the questionnaire. This information is hence insufficient for our analysis.

further reduces the sample, we did this only as a robustness check. The corresponding descriptives reported in Table A2 (Appendix) are for this estimation sample.

## 3 Estimation Procedures

The key challenge for the empirical analysis is to disentangle the effects under scrutiny from the impacts of confounding factors and possible reverse causality. As discussed in Section 1, the latter is a particularly severe concern since the empirical literature provides ample evidence for relationship status and relationship transition affecting health. Yet, we are still confident that our analysis does not generate spurious results due to reverse causality for two reasons: (i) as discussed above, health shocks and separations are chronologically defined such that the former cannot be caused by the latter; (ii) we focus on extraordinary changes in health, while controlling for its past level and – in some specifications – the past level of family-life quality, and hence avoid capturing the effects of an underlying, interactive deterioration of health and relationship quality that ultimately results in relationship dissolution. The considered substantial relative changes in PCS and MCS are, for these reasons, likely to capture some exogenous health events.

For addressing possibly remaining non-randomness of health shocks, we primarily rely on matching and closely related inverse probability weighting (IPW) that is conditioning on observables.[18] Both procedures estimate counterfactual outcomes by weighting the observed data. That is, the mean outcome under no treatment (no health shock) which is not observed for treated observations (couples hit by a health shock) is estimated as a weighted average of the outcomes observed for the control group (couples not hit by a health shock), and vice versa. If the treatment is purely random, no weighting is required since asymptotically the mean outcome under treatment and no treatment is the same for either group. If the treatment is however non-random and the groups differ systematically, the idea is to give more weight to atypical and less weight to typical observations in either group. Intuitively speaking, by selectively 'adding' and 'removing' observations from each group one makes them more similar and in turn, more comparable. One popular class of such estimators is based on the propensity score (PS; Rosenbaum and Rubin, 1983). The PS is the probability of the observed treatment status. IPW (Wooldridge, 2007) uses the propensity score to estimate the counterfactual separation rate of those couples, who have actually experienced a health shock as the weighted average separation rate of those who have not experienced a health shock using the inverse of the estimated probability of not experiencing a health shock as weights. The propensity score IPW estimator is extreme in the sense that every couple from the group without health shock – even those who are very different from any couple that is hit by a shock – enters the estimated counterfactual, though its weight might be very small. PS matching uses a different weighting scheme in which only those couples from the control group receive a weight different from zero,

[18]Ideally we could use an instrument for health shocks in our analysis. It is however hard to think of any event that strongly affects health but has no direct impact on partnership. Accidents, which are occasionally used as instrument (e.g. Doyle, 2005), may for instance change the partners willingness to take risks in every day live and affect partnership quality through this channel.

whose propensity score is very similar to the propensity score of at least one couple in the treatment group. For nearest neighbor matching, which can be regarded as the most intuitive approach, only couples who are the most similar to a counterpart in the treatment group receive non-zero weight. In other words, the counterfactual outcome of each couple in the treatment group is estimated as the observed outcome of its nearest neighbor in the control group.[19] Though asymptotically equivalent, IPW and nearest neighbor matching differ in their small sample properties. By considering many observations for estimating the counterfactual but allowing even very poor matches to enter the weighted mean, IPW reduces sampling error to the expense of accepting a larger finite sample bias. In contrast, nearest neighbor matching is quite picky in what is accepted as a good match. By this it reduces the bias while inflating the variance. All other PS matching estimators can be regarded as approaches that balance variance against bias in a different way.

In this application we only apply inverse probability weighting and nearest neighbor matching, as the two extreme approaches to deal with the trade-off between variance and finite sample bias. Besides basic inverse probability weighting we also use inverse probability weighted regression adjustment (Cattaneo, 2010). That is, instead of comparing weighted averages of observed outcomes, we compare weighted predicted outcomes that are generated in a preceding regression analysis. This approach has the so called double robustness property (e.g. Bang and Robins, 2005). That means for consistent estimation, either the outcome model – i.e. the regression model to generate the predicted outcomes – needs to be correctly specified and in consequence does not suffer from endogeneity bias, or the weighting succeeds in generating quasi randomness of treatment.

In the present analysis we estimate the PS using binary logit models.[20] In doing this, the counterfactual to any health shock, irrespective of whether the female, or the male partner, or both partners are hit, is that no health shock in the considered health domain hits the couple. For all IPW and matching analyses we report the average treatment effect (ATE). Technically, that means the observed and the estimated counterfactual outcomes for all couples, not only those who experience a health shock, enter the comparison of separation rates. Economically, the ATE measures how much a health shock in expectation increases the risk of separation for a couple randomly drawn from the population. In addition to those covariates discussed in section 2.4 we also match on the respective other type of health shock. That means in comparing couples who are hit by a MCS shock with those who are untroubled by a serious decline in mental health, we condition on the PCS shock status. Interaction terms are not considered in the matching procedures.

As an alternative – not matching or IPW but linear regression based – approach to addressing possibly remaining non-randomness of health shocks, we correct the estimated effects for possible unobserved confounder driven bias using the method suggested by Oster (2019). The intuition behind this approach is that – under certain assumptions – the sensitivity of the estimated effects to considering (further) covariates is informative for the magnitude of possible omitted variables bias; see section 4.2.3.

[19]We use the term 'nearest neighbor matching' in its narrow sense. That is the respective very best matching partner is exclusively used for estimating the counterfactual, but not a weighted average of several good matches.

[20]See Tables A7 and A8 for the estimated coefficients of the binary logit models.

Table 2: Descriptive Linear Regression without Controls

| | **without interact.** | | **with interact.** | |
|---|---|---|---|---|
| | Est. Coef. | S.E. | Est. Coef. | S.E. |
| MCS shock$_{♂}$ | 0.025$^{***}$ | (0.006) | 0.026$^{***}$ | (0.006) |
| MCS shock$_{♀}$ | 0.025$^{***}$ | (0.005) | 0.025$^{***}$ | (0.005) |
| MCS shock$_{♂♀}$ | | | −0.003 | (0.016) |
| PCS shock$_{♂}$ | −0.000 | (0.004) | 0.001 | (0.004) |
| PCS shock$_{♀}$ | −0.006 | (0.003) | −0.005 | (0.004) |
| PCS shock$_{♂♀}$ | | | −0.011 | (0.010) |
| *MCS shock*$_{♂♀}^{‡}$ | 0.050$^{***}$ | (0.007) | 0.048$^{***}$ | (0.014) |
| *PCS shock*$_{♂♀}^{‡}$ | −0.006 | (0.005) | −0.015$^{*}$ | (0.008) |
| $N$ | 30 296 | | 30 296 | |
| $R$-squared | 0.003 | | 0.003 | |

**Notes:** $^{***}$ $p$-value < 0.01; $^{**}$ $p$-value < 0.05; $^{*}$ $p$-value < 0.1; clustered standard errors; ‡ sum of regression coefficients; see Table A3 for the results from corresponding regressions with controls. **Source**: Own calculations based on SOEP data.

# 4 Results

As a first step, we estimate a simple descriptive regression of separation on health shocks not considering any covariates; see Table 2. We distinguish between two specifications, one without and one with health shock interactions. In both specifications, the separation rate is much higher, if one of the partners has experienced a substantial worsening of mental health in the past, compared to couples that did not experience any health shock. This difference is not only statistically significant but also of relevant magnitude as it is roughly as big as the unconditional sample separation rate. The estimated coefficients do not differ much between shocks hitting the male or the female partner. When we look at physical health shocks, the pattern is quite different. For physical health shocks, the coefficients are very small and statistically insignificant throughout. The asymmetry between MCS and PCS shocks carries over to the coefficient of the interaction terms (see Table 2, right columns). While interactions do not seem to matter for mental health shocks, we get a relatively large and negative coefficient for the PCS shock interaction. In terms of the effect of a joint PCS shock (sum of coefficients), it even passes the threshold of (weak) statistical significance. That is, couples which are jointly hit by a physical health shock are less likely to split up compared to couples who stay healthy. Though these results[21] have much intuitive appeal, they are purely descriptive and may as well capture the effect of confounding factors. As a next step we therefore employ different matching estimators discussed above.

## 4.1 Matching Analyses

Before we turn to the results from our matching estimations discussed in Section 3, we check how successful these estimators are in balancing the groups of couples hit by a health shock and the group of those who remained untroubled by such shocks. For inverse

[21] Including covariates in the regression analysis has just little impact on the estimated coefficients; see Table A3. While the point estimates get slightly smaller and the effect of a joint PCS shock turns statistically insignificant, the overall pattern remains largely unchanged.

Table 3: Mean Absolute Standardized Percentage Bias (MASPB)

| | Unmatched | Matched | |
|---|---|---|---|
| | | IPW♣ | NN Matching♡ |
| MCS shock$_{♂}$ | 6.495 | 1.351 | 1.662 |
| MCS shock$_{♀}$ | 6.947 | 1.293 | 1.782 |
| MCS shock$_{♂♀}$ | 12.968 | 2.838 | 4.328 |
| PCS shock$_{♂}$ | 10.052 | 1.999 | 2.424 |
| PCS shock$_{♀}$ | 8.634 | 1.510 | 1.398 |
| PCS shock$_{♂♀}$ | 21.051 | 7.889 | 6.257 |

**Notes:** Six panel waves (biennially 2004–2016). ♣ Inverse probability weighting; ♡ Propensity score nearest neighbor matching. **Source**: Own calculations based on SOEP data.

probability weighting and propensity score nearest neighbor matching Table 3 displays the mean absolute standardized percentage bias (MASPB, Rosenbaum and Rubin, 1985) for the matched and the unmatched estimation sample.[22] Inverse probability weighting does a good job in balancing 'treatment' and 'control' group, irrespective of whether a PCS or a MCS shock is considered. For health shocks that hit either the male or the female partner the MASPB is much smaller than the rule of thumb threshold of 5 percent (Caliendo and Kopeinig, 2008) and is also much smaller than its counterpart for the unmatched sample.[23] In other words, after inverse probability weighting, both groups are reasonably similar in terms of the observed covariates and by far more alike compared to the unweighted samples. This in essence also applies to nearest-neighbor propensity score matching, although the MASPB – with a single exception – is somewhat bigger. Things are different for health shocks that hit both partners. There the matching is clearly less successful in aligning treatment and control group. This in particular applies to joint physical health shocks for which both IPW and nearest neighbor matching fail in reducing the MASPB to a value smaller than five. The small and presumably rather special group of couples who experience a joint physical health shock, even after matching remains rather different from the control group though matching reduces the deviation substantially. Effects estimated for a joint (physical) health shock hence have to be interpreted with some caution.

Effects from the different matching estimators are displayed in Table 4. They are quite similar to the results from our unconditional descriptive analysis. A MCS shock that hits one partner increases the risk of a separation by 2 to 3 percentage points. Only nearest-neighbor matching yields a larger, yet rather noisily estimated, effect for the male partner being hit. Across all matching procedures, an MCS shock seems to have a somewhat stronger impact on partnership stability if it hits the male partner. Compared to the expected dissolution rate of 2.3 percent estimated for the control group, 2 to 3 percentage points is a rather substantial effect size. PCS shocks have much smaller effects throughout and are statistically insignificant. The only exception to the latter (NN matching, PCS shock to female partner) even suggests a stabilizing effect. With respect to the signs of the insignificant coefficients, we see the same pattern as for the unconditional analysis.

[22]Since regression adjustment does not make a difference with respect to matching as such, we do not distinguish between the variants with and without regression adjustment.

[23]We also tried the Mahalanobis distance to determine matching partners. However, this alternative approach did clearly worse in balancing treatment and control compared the propensity score matching. We hence stick to the latter.

Table 4: ATE Estimates – Reference Specification

| | **IPW**$^{♣}$ | | **IPW Reg. Adj.**$^{♠}$ | | **NN Matching**$^{♡}$ | |
|---|---|---|---|---|---|---|
| | Est. Eff. | S.E. | Est. Eff. | S.E. | Est. Eff. | S.E. |
| MCS shock$_{♂}$ | $0.030^{***}$ | (0.007) | $0.028^{***}$ | (0.007) | $0.039^{***}$ | (0.011) |
| MCS shock$_{♀}$ | $0.022^{***}$ | (0.006) | $0.021^{***}$ | (0.005) | $0.023^{***}$ | (0.006) |
| MCS shock$_{♂♀}$ | $0.041^{***}$ | (0.016) | $0.049^{***}$ | (0.015) | $0.036^{*}$ | (0.019) |
| PCS shock$_{♂}$ | 0.001 | (0.005) | 0.003 | (0.005) | −0.001 | (0.005) |
| PCS shock$_{♀}$ | −0.006 | (0.004) | −0.006 | (0.004) | $-0.008^{**}$ | (0.004) |
| PCS shock$_{♂♀}$ | $-0.018^{***}$ | (0.006) | −0.010 | (0.010) | $-0.017^{**}$ | (0.008) |

**Notes:** $^{***}$ $p$-value < 0.01; $^{**}$ $p$-value < 0.05; $^{*}$ $p$-value < 0.1; clustered standard errors; $^{♣}$ Propensity score weighting; $^{♠}$ Propensity score weighting with regression adjustment; $^{♡}$ Propensity score nearest neighbor matching; seven panel waves (biennially 2004–2016). **Source**: Own calculations based on SOEP data.

The results for joint health shocks also exhibit a pattern similar to the OLS results. That is, if both partners experience a strong deterioration in mental health a separation gets more likely, yet the effects of shocks that hit just one partner seem not to cumulate linearly. Due to the small number of joint health shocks in the data, the effects are however noisily estimated and their magnitude has to be interpreted with some caution. For a joint physical health shock we find the same stabilizing effect that we found in the unconditional analysis, which – except for the empirical model using regression adjustment – is statistically significant.

All in all the results yield a coherent picture. A deterioration of mental health strongly increases the risk of separation. It does not seem to make much difference whether the male or the female partner suffers from this health shock. In contrast, negative shocks on physical health seem to be largely immaterial for partnership stability. If at all, a partnership is less likely to be terminated if both partners jointly experience a deterioration of physical health. The different matching estimators and the simple unconditional comparison of means do not differ much in terms of estimated effects. This can be regarded as indication for relationship breakdown not being linked to – at least observed – determinants of health shocks.[24]

## 4.2 Alternative Models and Robustness Checks

In order to get more confidence in the results discussed above, we test the robustness of our estimates by varying the empirical model in three dimensions: (i) we vary the definition of a health shock, (ii) we address possible endogeneity due to unobserved confounders by considering additional covariates and by applying the method of Oster (2019) to quantify the size of the possible bias, (iii) we vary the selection of the estimation sample and, related to that, consider alternative intra-family roles than the traditional 'male and female partner model'. Due to the superior matching performance, very similar results for the different matching approaches and for the sake of simplicity we solely present the results of the alternative models and robustness checks for the inverse probability weighting without regression adjustment.[25] The method of Oster (2019) is based on the linear regression.

[24]Adding further covariates to the descriptive linear regression model (Table 2) does not change the overall pattern of results (see Table A3).

[25]Results for propensity score weighting with regression adjustment and propensity score nearest neighbor matching are similar and available upon request.

#### 4.2.1 Alternative Health Shock Definitions

Firstly we address the issue of our – to some extent – arbitrary health shock definition as a relative decrease in MCS and PCS, respectively, of more than 25 percent. We now systematically vary the threshold value. Figure 2 depicts the estimated effects on the separation hazard considering different required minimum reductions in MCS (upper panel) and PCS (lower panel), respectively, between 0 and 50 percent. Starting with physical health, shocks on the PCS of either only the male or only the female partner exhibit very small and statistically insignificant effects virtually over the entire range of the considered threshold values. In contrast, for a joint PCS shock we see a significant stabilizing effect for any threshold that exceeds 16 percent. For any threshold value that exceeds 32 percent – except for those close to 50 percent – Figure 2 indicates an almost constant effect of a joint PCS shock with an associated rather narrow confidence band. This is an artifact of quasi complete separation. That is if threshold values $\geq 32.5$ percent are considered, no separation is observed in the group of couples hit by a joined health shock defined this way. In consequence the probability of such shock is estimated to be zero and the estimated effect is just the relative separation frequency in the control group multiplied by minus one. This explains the rather small standard error. However, a significant effect is also found for threshold values smaller than 32 percent that does not generate quasi complete separation. That is, the stabilizing effect found for joint physical health shock is not an artifact of quasi complete separation.

Turning to shocks to mental health, not surprisingly, the estimated effects get relatively small if even very minor deteriorations in health are counted as health shocks. Yet, the estimated effects stay significant a the 10 percent level until a threshold of zero percent is considered, that is if any worsening of the MCS is regarded as a shock. If one regards only very severe relative losses in MCS and PCS as health shocks, the estimates get very noisy as indicated by the rather wide confidence intervals. This is easily explained by health shocks, joint ones in particular, then becoming extremely rare events; see Figure A2 in the Appendix. However, most importantly, if one considers more reasonable health shock definitions – relative reductions between 15 and 35 percent for instance – the estimates turn out to be fairly robust and do not strongly deviate from those we got when considering the 25 percent threshold.

Next, we vary the definition of a health shock in another dimension, by considering a minimum absolute change rather than a relative one as criterion. Tables 5 (matching) and A6 (linear regressions, Appendix) display results for a loss of 13 units (original scale) of MCS and PCS, respectively, constituting a health shock. This value was chosen to make health shocks roughly as frequent as in the reference specification.[26] Defining health shocks in terms of absolute changes shifts the occurrence of such shocks from individuals in poor health to individuals in good health. The correlations between the corresponding indicators are high (0.840, 0.842, 0.831, and 0.801) but clearly smaller than one. A disadvantage of the alternative health shock definition is that health shocks for those at the very bottom of

[26]MCS shock are slightly more and PCS shocks are slightly less frequent compared to the reference specification (see Table A5 in the Appendix and the corresponding entries in Table 1).

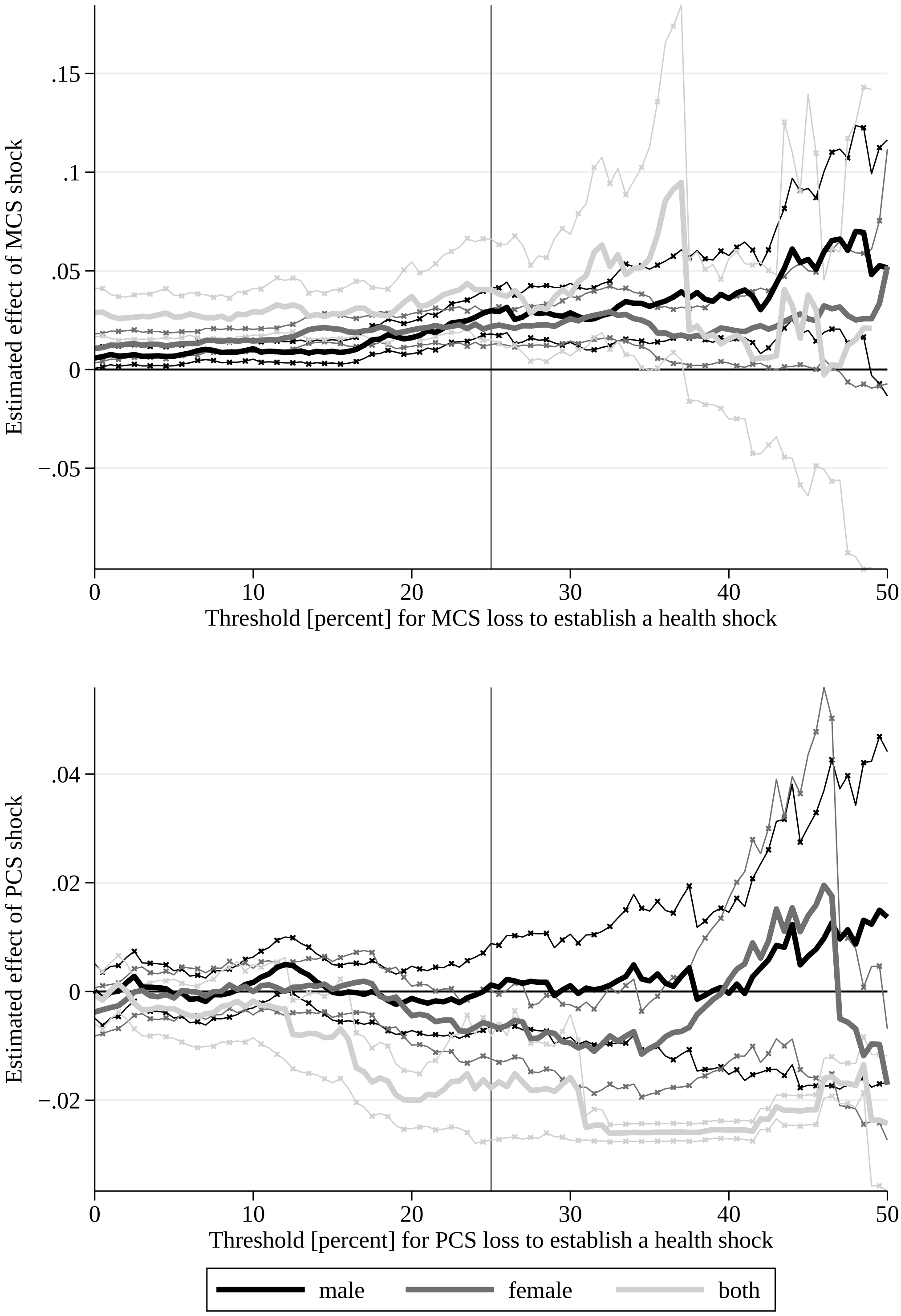


Figure 2: Estimated Effects of MCS (upper panel) and PCS (lower panel) shocks on separation hazard for different threshold values (percentage loss in MCS and PCS, respectively); inverse probability weighting estimates; threshold varied in steps of 0.5 percentage points; x-marked lines mark 90 percent intervals of confidence; solid vertical line marks reference threshold of 25 percent. **Source**: Own calculations based on SOEP data.

Table 5: ATE Estimates – Absolute Health Shock

| | Est. Eff. | S.E. |
|---|---|---|
| MCS shock$_{♂}$ | 0.026$^{***}$ | (0.009) |
| MCS shock$_{♀}$ | 0.024$^{**}$ | (0.010) |
| MCS shock$_{♂♀}$ | 0.041$^{**}$ | (0.016) |
| PCS shock$_{♂}$ | $-0.004$ | (0.005) |
| PCS shock$_{♀}$ | $-0.008^{*}$ | (0.005) |
| PCS shock$_{♂♀}$ | $-0.018^{***}$ | (0.006) |

**Notes:** $^{***}$ *p*-value < 0.01; $^{**}$ *p*-value < 0.05; $^{*}$ *p*-value < 0.1; clustered standard errors in parentheses; seven panel waves (biennially 2004–2016); propensity score weighting; see Table 4 for the reference specification.
**Source**: Own calculations based on SOEP data.

the distribution are virtually ruled out. This is why we regard our baseline specification as clearly preferable. Nevertheless, the health shock definition based on absolute changes in MCS and PCS hardly alter the estimated effects.

### 4.2.2 Additional Covariates

All so far discussed analyses do not condition on pretreatment satisfaction with family life or partner characteristics that are possibly related to partnership quality (Guven et al., 2012). Table 6 displays the key coefficients of three additional inverse probability weighting specifications, where we condition on the big five personality traits of each partner (column 1), satisfaction with family life (column 2) and both additional sets of covariates (column 3).

Controlling for satisfaction with family life and the big five personality traits does not change the overall pattern of results from their counterpart displayed in Table 4. Only some small deviations occur. The effect of the shock on the male partner's MCS turns somewhat smaller in all three specifications, but stays highly significant. The adverse effect of a joint MCS shock is estimated slightly bigger if family life satisfaction is controlled (columns 2 and 3). There is a weak indication that physical health shocks to the female partner positively affect relationship stability (columns 1). All in all, conditioning on satisfaction with family live – as a proxy for relationship quality – and partner characteristics that are possibly related to partnership quality does not put the key result of adverse effects of MCS shocks into question. Although these results do not rule out effects of PCS shocks, they seem to be of much smaller relevance than shocks to mental health. Moreover, our results still suggest that there is a stabilizing effect of joint physical health shocks.[27]

Income losses that may accompany health shocks – or might be triggered by them – are another concern. In other words, if health shocks and monetary shocks go frequently hand-in-hand, interpreting our earlier results in terms of genuine health effects might be misleading. To address this concern we augment the list of conditioning variables by the (contemporaneous) income change of either partner. We do this in three different ways: (i) we define an adverse income shock in the very same way as we define an adverse health

[27] In a linear regression the coefficients attached to the additional control 'satisfaction with family life' are highly significant, negative and symmetric for the male and the female partner. Not surprisingly, relationships of partners who are happy with his or her family are less likely to be terminated. Including the partner-interaction of the satisfaction variable in this regression yields a quite telling pattern of coefficients. The coefficients of individual satisfaction in absolute terms get much bigger while the interaction term is positiv and highly significant. This can be interpreted such that the risk of separation already substantially increases if one partner is unhappy.

Table 6: ATE Estimates – Additional Covariates

| | Big Five† | | Fam. Satisfaction‡ | | Big Five & Fam. Satis.‡ | |
|---|---|---|---|---|---|---|
| | Est. Eff. | S.E. | Est. Eff. | S.E. | Est. Eff. | S.E. |
| MCS shock$_{♂}$ | 0.023*** | (0.007) | 0.025*** | (0.008) | 0.023*** | (0.008) |
| MCS shock$_{♀}$ | 0.018*** | (0.006) | 0.022*** | (0.007) | 0.021*** | (0.008) |
| MCS shock$_{♂♀}$ | 0.037** | (0.017) | 0.053** | (0.021) | 0.056** | (0.023) |
| PCS shock$_{♂}$ | 0.001 | (0.005) | 0.005 | (0.006) | 0.006 | (0.007) |
| PCS shock$_{♀}$ | −0.007* | (0.004) | −0.002 | (0.005) | −0.003 | (0.005) |
| PCS shock$_{♂♀}$ | −0.021*** | (0.002) | −0.016*** | (0.006) | −0.021*** | (0.002) |
| Big Five | ✓ | | | | ✓ | |
| Satisfaction with fam. life | | | ✓ | | ✓ | |

**Notes:** *** $p$-value < 0.01; ** $p$-value < 0.05; * $p$-value < 0.1; clustered standard errors; †seven panel waves (biennially 2004–2016); ‡five panel waves (biennially 2008–2016); propensity score weighting; see Table 4 for the reference specification. **Source**: Own calculations based on SOEP data.

Table 7: ATE Estimates – Additional Covariates II

| | Adverse Income Shock | | | | Income Change | |
|---|---|---|---|---|---|---|
| | Est. Eff. | S.E. | Est. Eff. | S.E. | Est. Eff. | S.E. |
| MCS shock$_{♂}$ | 0.029*** | (0.007) | 0.029*** | (0.007) | 0.033*** | (0.009) |
| MCS shock$_{♀}$ | 0.021*** | (0.006) | 0.021*** | (0.006) | 0.022*** | (0.006) |
| MCS shock$_{♂♀}$ | 0.042** | (0.016) | 0.041** | (0.016) | 0.040** | (0.015) |
| PCS shock$_{♂}$ | 0.000 | (0.005) | 0.000 | (0.005) | −0.005 | (0.007) |
| PCS shock$_{♀}$ | −0.007* | (0.004) | −0.007* | (0.004) | −0.006* | (0.004) |
| PCS shock$_{♂♀}$ | −0.018*** | (0.006) | −0.018*** | (0.006) | −0.018*** | (0.006) |
| $\mathbb{1}\,(\Delta\text{income}/\text{income} < -1/4)$ | ✓ | | | | | |
| $\mathbb{1}\,(\Delta\text{income}/\text{income} < -1/2)$ | | | ✓ | | | |
| $\Delta$income | | | | | ✓ | |

**Notes:** *** $p$-value < 0.01; ** $p$-value < 0.05; * $p$-value < 0.1; clustered standard errors; seven panel waves (biennially 2004–2016); propensity score weighting; see Table 4 for the reference specification. **Source**: Own calculations based on SOEP data.

shock, i.e. as a reduction in income by more than 25 percent, and include two income-shock indicators as additional covariates; since this results in a relatively large share of couples hit by an income shock, we (ii) use a reduction of more than 50 percent as alternative, stricter criterion; (iii) we condition on the raw change in personal income. Table 7 indicates that in neither case conditioning on contemporaneous income changes has a significant impact on the estimated effects. This also applies to the results from the simple linear regression and corresponds to the descriptive evidence that health and income shocks are just weakly correlated in the estimation sample. This robustness check hence suggests that the estimated effects do not capture income effects and that income is not the prime channel through which health shocks impact on partnership stability.

### 4.2.3 Quantifying the Bias due to Unobserved Confounders

The robustness of the estimated effects to including (further) covariates is a reassuring result. Under certain assumptions this can also be regarded as indication for the estimated effects not being driven by unobserved confounders (e.g. Altonji et al., 2005, 2008; Oster, 2019). In oder to formalize this line of argument, we follow Oster (2019) and calculate (omitted variables) bias adjusted estimates of the health shock effects. In pointing out her contribution to this literature, Oster (2019) stresses that the stability of the estimated effects to including (further) covariates is per se of little informational values, but needs to be related to the gain in explanatory power in order to learn more about the size of a possible bias that originates from unobserved confounders. This is what her suggested procedure effectively does. Since Oster (2019) considers a linear regression model, we base this robustness check on the linear model rather than the matching estimators. In our application of Oster's method, we hence exploit the deviation in the estimated coefficients and the $R$-squared value in Table A3 from the corresponding entries in Table 2 (right panel each); also see Table A4 in the Appendix which provides this information at a glance.

Evidently, quantifying a bias that originates from something unobserved requires assumptions about unknown quantities. More specifically, applying Oster's method to correct for such bias requires the choice of two parameter values: (i) the importance of unobserved covariates for selection into treatment relative to the observed ones (denoted $\delta$ in Oster, 2019), and (ii) the share of variation in the outcome that could be explained if all covariates were observed (denoted $R_{\max}$ in Oster, 2019). For $\delta$, unity may be regarded as a natural threshold value. That is if an estimated effect 'survives' even if one assumes that unobservables are as important as observables for selection into treatment, one may regard the result as robust (cf. Oster, 2019). For the choice[28] of $R_{\max}$ Oster (2019) suggests – on basis of examining randomized studies that by design do not to suffer from asymptotic omitted variables bias – 1.3 times the $R$-squared (denoted $\tilde{R}$ in Oster, 2019) the regression of the outcome on the observed covariates and the treatment(s) yields.[29]

Figure 3 depicts the estimated bias-adjusted effects on the separation hazard, as a function of $\delta$. The vertical line indicates the benchmark value $\delta = 1$. The OLS regression with controls and shock-interactions serves as reference; cf. Table A3, column 3. In other words, for $\delta = 0$ the bias-corrected estimator just reproduces coefficients of the reference regression. This has much intuitive appeal since assuming $\delta = 0$ means that unobserved confounders are immaterial for selection into treatment, more specific to our application they are immaterial for the occurrence of a health shock. The displayed confidence intervals are based on a clustered bootstrap and calculated under the assumption of asymptotic normality. The assumed value of $R_{\max}$ is throughout the suggested one of $1.3 \times \tilde{R}$.

The graphs below indicate that the estimated effects are astonishingly robust to different assumptions regarding the relative importance of unobserved covariates for selection into treatment. If equal importance, that is $\delta = 1$, is assumed the bias-corrected estimates hardly deviate from their uncorrected counterparts. Yet, the effects of MCS shocks do also clearly survive if the unobserved covariates is assumed to be even five times more important as predictors of a health shock than the included controls. This not only holds in terms of the point estimates, but even if sampling variability it taken into account. By all standards,

[28]The most conservative choice obviously is $R_{\max} = 1$ (cf. Altonji et al., 2005). Oster (2019), however, argues that this value is a way too conservative choice because one would typically not regard the outcome to be deterministic, even if all systematic determinants were observed. Oster's argument seems to be particularly relevant to our analysis. Since we use a linear probability model, the outcome 'separation' is regarded as genuinely random and, for this reason, cannot be predicted perfectly.

[29]A simplified expression for the adjustment that is applied to the estimated effect, which however holds exactly only under some restrictive assumptions, is very telling about which quantities determine its size: $-\delta(\mathring{\beta} - \tilde{\beta})(R_{\max} - \tilde{R})(\tilde{R} - \mathring{R})^{-1}$ (Oster, 2019, p. 193). $\tilde{\beta}$ denotes the estimated effect in the regression with controls. $\mathring{\beta}$ denotes its counterpart from the unconditional regression. $\tilde{R}$ and $\mathring{R}$ denote the corresponding $R$-squared values.

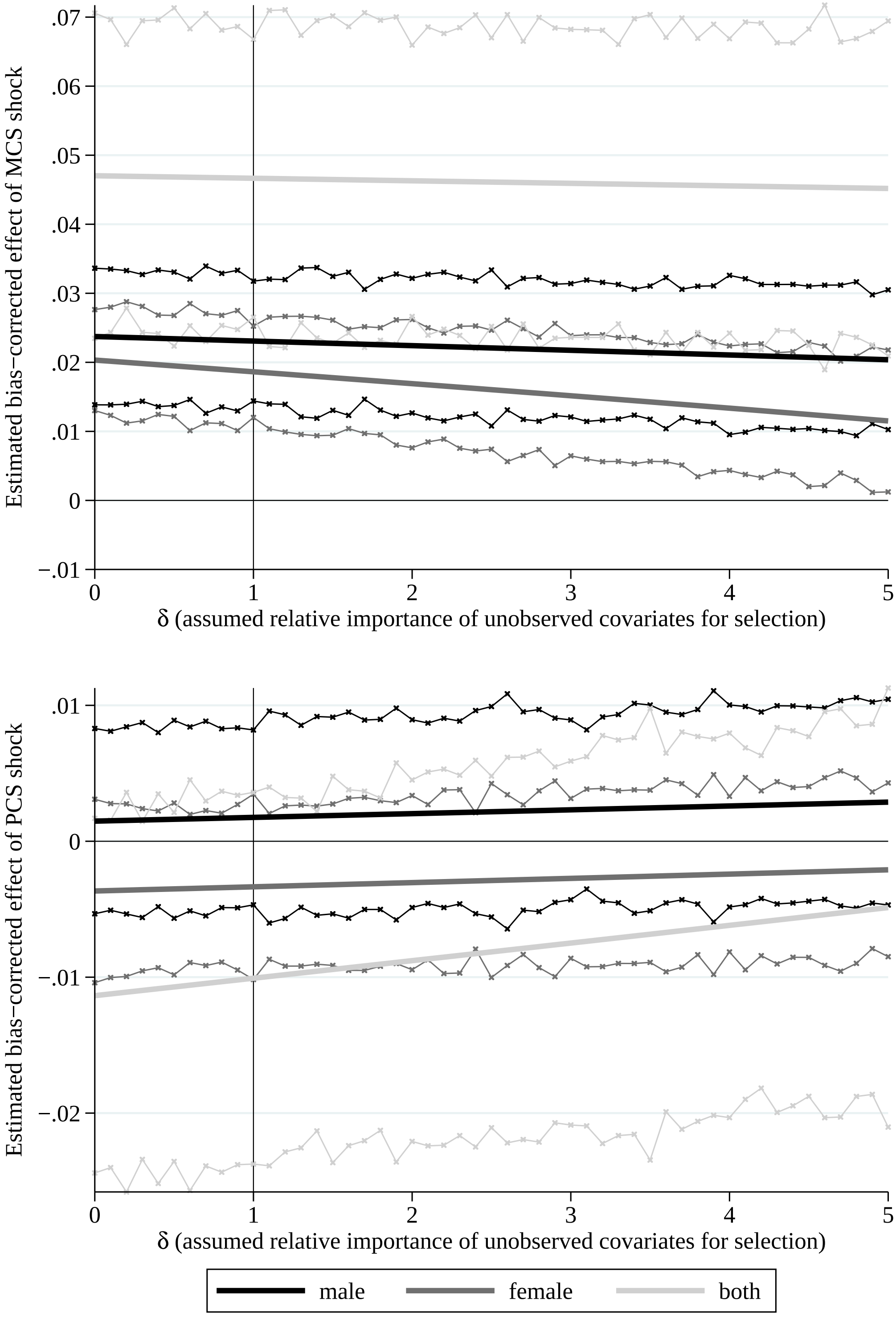


Figure 3: Estimated bias adjusted (Oster, 2019) effects of MCS (upper panel) and PCS (lower panel) shocks on separation hazard for different values of $\delta$ (assumed relative importance of unobserved covariates for selection); $\delta$ varied in steps of 0.1; OLS with shock-interactions (cf. Table A3) x-marked lines mark 90 percent intervals of confidence; solid vertical line marks equal importance of observed and unobserved covariates for selection into treatment; $R_{\max} = 1.3 \times \tilde{R}$. **Source**: Own calculations based on SOEP data using `psacalc` (Oster, 2013).

Table 8: ATE Estimates – Unmarried and Old Couples Excluded

| | **Married** | | **Age ≤ 85** | | **Age ≤ 75** | | **Age ≤ 65** | |
|---|---|---|---|---|---|---|---|---|
| | Est. Eff. | S.E. | Est. Eff. | S.E. | Est. Eff. | S.E. | Est. Eff. | S.E. |
| MCS shock$_{♂}$ | $0.031^{***}$ | (0.007) | $0.030^{***}$ | (0.007) | $0.033^{***}$ | (0.008) | $0.038^{***}$ | (0.009) |
| MCS shock$_{♀}$ | $0.024^{***}$ | (0.006) | $0.021^{***}$ | (0.005) | $0.022^{***}$ | (0.006) | $0.025^{***}$ | (0.007) |
| MCS shock$_{♂♀}$ | $0.036^{**}$ | (0.015) | $0.041^{***}$ | (0.016) | $0.049^{***}$ | (0.018) | $0.049^{**}$ | (0.019) |
| PCS shock$_{♂}$ | $-0.002$ | (0.004) | 0.001 | (0.005) | 0.000 | (0.005) | 0.000 | (0.006) |
| PCS shock$_{♀}$ | $-0.005$ | (0.004) | $-0.007^{*}$ | (0.004) | $-0.006$ | (0.004) | $-0.007$ | (0.005) |
| PCS shock$_{♂♀}$ | $-0.020^{***}$ | (0.001) | $-0.017^{***}$ | (0.006) | $-0.021^{***}$ | (0.005) | $-0.025^{***}$ | (0.006) |

**Notes:** $^{***}$ $p$-value < 0.01; $^{**}$ $p$-value < 0.05; $^{*}$ $p$-value < 0.1; clustered standard errors; seven panel waves (biennially 2004–2016); propensity score weighting; see Table 4 for the reference specification. **Source**: Own calculations based on SOEP data.

$\delta = 5$ has to be regarded as a very conservative choice.[30] According to Oster's method, it thus appears very unlikely that the estimated health-shock effects are just artifacts of unobserved confounders.

### 4.2.4 Sample and Roles within Partnership

A frequent question in the relevant literature (e.g. Pevalin and Ermisch, 2004; Kohn and Averett, 2014b) is whether marriage makes a difference in the interplay of partnership and health. In our analysis we address this issue by re-estimating the inverse probability weighting model only considering married couples. In a similar way we address the concern that institutionalization of partners in need of nursing care may generate technical separations that cannot be regarded as relationship break down. We hence restrict the estimation sample to relatively young couples (older partner's age ≤ 85, ≤ 75, and ≤ 65), for which moving to a nursing home is less likely than for couples with at least one partner of very advanced age. Table 8 displays the estimated coefficients for these models. Excluding unmarried and old couples from the estimation sample appears to make little difference. If at all, focussing on relatively young couples (≤ 65) yields somewhat stronger adverse effects of MCS shocks as compared to considering couples of all age groups.

Next, we use another sample restriction to once again address the issue of unobserved heterogeneity that may render health shocks endogenous. More specifically, we only consider couples that are observed to ever experience a health shock of the considered type. In other words, the counterfactual separation rate for treated couples is estimated as a weighted average among couples that are not hit by the shock in the previous period but at some different point in time. The intuition behind this sample restriction is that time invariante confounders, such as innate health, are neutralized as factors for selection into treatment by conditioning on ever being treated. Restricting the sample this way has a noticeable effect on the estimated effects; see Table 9. Compared to the specification of reference, all point estimates get bigger and more detrimental. That is the results from

[30] Robustness with respect to the choice of $R_{\text{max}}$ is a somewhat different matter. Given the small $R$-squared value of only 0.029 in the reference regression, it is no surprise that assuming unity – or a value close to it – for $R_{\text{max}}$ makes the procedure indicate enormously large possible biases; cf. footnote 29. Nevertheless, for equal selection ($\delta = 1$) the procedure allows for choosing scaling-factors that substantially exceed the suggested one of 1.3, without making the procedure indicate huge biases. More specifically, the maximum value that yields a ratio $\frac{\text{bias-corrected est. effect}}{\text{uncorrected est. effect}}$ within the $[0.5, 2]$ interval is 5.9, 2.7, and 17.8 for the MCS-shock effects, and 2.5, 2.7, and 2.3 for the PCS-shock effects.

Table 9: ATE Estimates – Only Couples Ever Hit by respective Health Shock

| | Est. Eff. | S.E. |
|---|---|---|
| MCS shock$_{♂}$ | 0.042*** | (0.007) |
| MCS shock$_{♀}$ | 0.034*** | (0.006) |
| MCS shock$_{♂♀}$ | 0.042*** | (0.011) |
| PCS shock$_{♂}$ | 0.015*** | (0.005) |
| PCS shock$_{♀}$ | 0.008** | (0.004) |
| PCS shock$_{♂♀}$ | 0.000 | (0.006) |

**Notes:** *** *p*-value < 0.01; ** *p*-value < 0.05; * *p*-value < 0.1; clustered standard errors; seven panel waves (biennially 2004–2016); propensity score weighting; see Table 4 for the reference specification. **Source**: Own calculations based on SOEP data.

this empirical model (i) suggest even stronger adverse effects of mental health shocks on partnership stability and (ii) challenge the earlier result that physical health shocks are immaterial for the separation hazard. A possible explanation is that couples with unobserved characteristics that makes health shocks more likely are also more vulnerable to these shocks.[31] Nevertheless, the earlier pattern of shocks to mental health being by far more detrimental than shocks to physical health survives. This also holds for the finding that a jointly experienced physical health shock is the least destructive type of shock. Moreover, with a single exception, the 95 percent confidence intervals still overlap with their counterparts from the reference specification. Taking sampling variability into account, the effects estimated for the restricted sample are hence not as different from the results of reference as the point estimates may suggest.

Finally we examine results for analyses in which we distinguish the roles 'main breadwinner' and 'partner', rather than male and female partner. The estimated role specific health shock coefficients may partly capture different channels through which health affects partnership. We define the 'main breadwinner' as the partner with the higher personal income. If there is no difference in income we use the information about who acts as 'household head' as secondary and the partners' age as tertiary criterion. We interpret the 'main breadwinner' to be the (economically) stronger partner, even though this may not apply to all couples. Disengaging the analysis from the traditional man-woman model allows for including both hetero- and homosexual couples. Yet, due to the relatively small number of homosexual couples that are identified in the SOEP, this does not make a major difference in terms of the estimation sample.[32] Table 10 shows the estimated coefficients for the inverse probability weighting for heterosexuals (column 1) and hetero- as well as homosexuals (column 2). Neither altering the roles within partnerships nor including homosexual couples leads to a deviation from the familiar overall pattern of estimated coefficients. The estimated heterogeneity in the effects of MCS shocks is of even smaller magnitude as in the model of reference and stays statistically insignificant. In terms of the point estimates, the risk of separation seems to be slightly smaller if the economically weaker partner is hit.

[31] In the sub-samples of couples ever hit by a specific health shock the (unconditional) separation rates are substantially lower than the corresponding rate in the overall sample. So one may speculate that individuals who are aware of high health risks in better times hesitate to forgo the implicit insurance a partnership may provide. Their partnerships prove, however, to be no more stable when the adverse event actually occurs.

[32] Roughly 0.3 percent of the observed couples are all female and less than 0.2 percent are all male. This is for various reasons unlikely to be a meaningful estimate of the share of homosexuals in the German population.

Table 10: ATE Estimates – Main Breadwinner (⊙) and Partner (☾ )

| | Heterosexual Couples | | Homosexuals included | |
|---|---|---|---|---|
| | Est. Eff. | S.E. | Est. Eff. | S.E. |
| MCS shock$_{\odot}$ | 0.027*** | (0.007) | 0.027*** | (0.007) |
| MCS shock$_{☾}$ | 0.024*** | (0.006) | 0.024*** | (0.006) |
| MCS shock$_{\odot☾}$ | 0.044*** | (0.016) | 0.045*** | (0.016) |
| PCS shock$_{\odot}$ | −0.002 | (0.004) | −0.002 | (0.004) |
| PCS shock$_{☾}$ | −0.003 | (0.004) | −0.003 | (0.004) |
| PCS shock$_{\odot☾}$ | −0.015* | (0.008) | −0.015** | (0.008) |

**Notes:** *** $p$-value < 0.01; ** $p$-value < 0.05; * $p$-value < 0.1; clustered standard errors; seven panel waves (biennially 2004–2016); propensity score weighting; see Table 4 for the reference specification. **Source**: Own calculations based on SOEP data.

Taken together the results form various estimations using different samples and estimation techniques and applying a bias correction method show a rather robust overall picture: Mental health shocks are detrimental to partnership stability. If one partner experiences a sharp decline in mental health over two years, the hazard for splitting up over the next two years roughly doubles. If both partners are hit by such a shock things are even worse. In terms of the point estimates, we see some heterogeneity with respect to the gender of the partner who is subject to the health shock and with respect to the partners relative economic position. More specifically, relationship breakdown seems to be less likely if the female or the economically weaker partner – roles that coincide for the majority of partnerships in the sample – experiences a loss of mental health. Our key finding challenges the notion of long term partnership as an effective informal insurance against mental health risks. It rather seems that the emotional support one may have expected to find in a relationship in case of such hardship is likely to be denied at the time, when it is needed most. Our results regarding physical health shocks are very different. There, we see very little evidence that a sharp deterioration in physical health increases the separation hazard. Although we cannot firmly rule out such an effect, it is almost certainly much smaller than the impact of a shock to mental health. Quite to the contrary, our results suggest that jointly experiencing a deterioration of physical health bonds partners closer together. In consequence, while long term partnership may fail as informal insurance against mental health shocks it seems to work substantially better as insurance against physical health risks.

Our analysis nevertheless has some limitations. First of all, identification is not based on a specific exogenous source of variation in health. Although we are confident that we have reduced the role endogeneity bias may play for the results, we cannot claim that we have completely eliminated it. The matching estimators do a reasonably good job in aligning treatment and control in terms of observables but unobservables may still jointly affect mental health and partnership stability. However, our results are robust to including satisfaction with family life as covariate. As a proxy for partnership quality one may regard this variable as key confounder, which if omitted generates a spurious correlation between (mental) health and partnership stability. Yet, this seems not to apply to our analysis. Moreover, formalizing the idea of robustness to including covariates is informative about the size of possible endogeneity bias, suggests that the estimated effects are most likely not driven by confounding unobservables.

## 5 Discussion and Conclusion

Using household level data from Germany and applying different matching estimators, we estimated the effects of shocks to physical and mental health on relationship stability. Our results reveal a robust pattern: a sudden and severe deterioration of mental health has a corrupting impact on relationships. The risk of separation over two years is roughly doubled by a mental health shock, irrespective of whether the male or the female partner is hit. We see some, yet statistically insignificant, asymmetry in this effect with respect to gender or with respect to the intra-family economic position of the partner who is hit by this shock. The results are quite different for shocks to physical health. There we do not find a destabilizing impact on marriage or long-term relationships. The data rather suggest that jointly experiencing a severe deterioration in physical health makes couples stay together.

Our results question whether marriage and long-term relationships are an effective informal insurance instruments against (non-monetary) consequences of sickness. How relationships react to health shocks appears to depend on the type of health shock and to a lesser extend on the economic position within the partnership. Mental health problems appear to be a risk for which finding informal insurance in a relationship is difficult. One may speculate that the external effects of mental illness to the healthy partner are so strong that he or she decides to end a relationship, even if he or she is altruistic to the suffering partner. An alternative way of interpreting the strong adverse effect of a shock to mental health is that the gains from partnership are complements to mental health. That is, emotional support might not be found in a partnership when suffering from mental illness, even if it is not denied by the healthy partner. If the former explanation is more relevant, the asymmetry between shocks to mental and to physical health points to the latter being less stressful to the not directly affected partner or that physical illness is more likely to activate altruism. In any case, relationships seem to provide much better informal insurance against physical as compared to mental health risks.

Partnership as a genuinely private matter should not be subject to policy interventions. We nevertheless regard the empirical evidence yielded in this analysis as relevant for health policy makers. Our results suggest that mental illness generates substantial indirect costs through inducing relationship breakdown, which itself reduces the well being of two partners.

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# A Appendix

Table A1: Descriptive Statistics – Main Breadwinner-Partner Model

| | Mean | S.D. | Med. | Min. | Max. |
|---|---|---|---|---|---|
| separation | 0.026 | 0.159 | 0 | 0 | 1 |
| MCS shock$_{\odot}$ | 0.057 | 0.232 | 0 | 0 | 1 |
| MCS shock$_{\leftmoon}$ | 0.071 | 0.257 | 0 | 0 | 1 |
| PCS shock$_{\odot}$ | 0.053 | 0.224 | 0 | 0 | 1 |
| PCS shock$_{\leftmoon}$ | 0.063 | 0.244 | 0 | 0 | 1 |
| MCS shock$_{\odot\leftmoon}$ | 0.011 | 0.105 | 0 | 0 | 1 |
| PCS shock$_{\odot\leftmoon}$ | 0.006 | 0.077 | 0 | 0 | 1 |
| $\text{MCS}_{t-2,\odot}{}^{\flat}$ | 0.519 | 0.091 | 0.534 | 0.101 | 0.794 |
| $\text{MCS}_{t-2,\leftmoon}{}^{\flat}$ | 0.504 | 0.097 | 0.517 | 0.019 | 0.777 |
| $\text{PCS}_{t-2,\odot}{}^{\flat}$ | 0.495 | 0.094 | 0.515 | 0.092 | 0.725 |
| $\text{PCS}_{t-2,\leftmoon}{}^{\flat}$ | 0.489 | 0.098 | 0.508 | 0.101 | 0.781 |
| age$_{\odot}$ | 54.22 | 13.97 | 53 | 20 | 98 |
| age$_{\leftmoon}$ | 52.53 | 13.89 | 52 | 20 | 97 |
| abs. age difference | 3.810 | 3.634 | 3 | 0 | 37 |
| education$_{t-2,\odot}$ | 12.780 | 2.852 | 11.5 | 7 | 18 |
| education$_{t-2,\leftmoon}$ | 12.140 | 2.566 | 11.5 | 7 | 18 |
| abs. educ. difference$_{t-2}$ | 1.689 | 1.912 | 1 | 0 | 11 |
| east$_{t-2}$ | 0.248 | 0.432 | 0 | 0 | 1 |
| home owner$_{t-2}$ | 0.610 | 0.488 | 1 | 0 | 1 |
| child in hh$_{t-2}$ | 0.351 | 0.477 | 0 | 0 | 1 |
| common child$_{t-2}{}^{\sharp}$ | 0.648 | 0.478 | 1 | 0 | 1 |
| # of children$_{t-2}{}^{\sharp}$ | 1.727 | 0.817 | 2 | 1 | 8 |
| min. age child$_{t-2}{}^{\sharp}$ | 7.628 | 4.778 | 7 | 0 | 16 |
| married$_{t-2}$ | 0.888 | 0.316 | 1 | 0 | 1 |
| partnership duration$_{t-2}$ | 8.875 | 7.407 | 6 | 0 | 30 |
| censored couple info$_{t-2}$ | 0.750 | 0.433 | 1 | 0 | 1 |
| employed$_{t-2,\odot}$ | 0.730 | 0.444 | 1 | 0 | 1 |
| employed$_{t-2,\leftmoon}$ | 0.589 | 0.492 | 1 | 0 | 1 |
| employed$_{t-2,\odot\leftmoon}$ | 0.537 | 0.499 | 1 | 0 | 1 |
| income$_{t-2,\odot}$ | 2.990 | 2.637 | 2.500 | 0 | 116.2 |
| income$_{t-2,\leftmoon}$ | 1.105 | 1.271 | 0.776 | 0 | 42 |
| income share$_{t-2,\odot}$ | 0.725 | 0.230 | 0.725 | 0 | 1 |
| homosexual | 0.004 | 0.062 | 0 | 0 | 1 |
| homosexual$_{\male}$ | 0.001 | 0.038 | 0 | 0 | 1 |

**Notes:** Statistics based on 30 412 couple-year observations; model that differentiates main breadwinner and partner of main breadwinner including homosexual couples; seven panel waves (biennially 2004–2016); $\odot$ main breadwinner; $\leftmoon$ partner of main breadwinner; $^{\sharp}$ conditional on children in household; $^{\flat}$ rescaled to the unit interval. **Source**: Own calculations based on SOEP data.

Table A2: Descriptive Statistics for Estimation Sample – Character Traits (Big Five)

| | **Mean** | **S.D.** | **Med.** | **Min.** | **Max.** |
|---|---|---|---|---|---|
| conscientiousness$_{♂}$ ‡ | 47.657 | 9.977 | 48.258 | −3.445 | 76.697 |
| conscientiousness$_{♀}$ ‡ | 51.990 | 9.302 | 53.011 | 12.782 | 75.341 |
| neuroticism$_{♂}$ ‡ | 51.252 | 9.385 | 52.606 | −9.873 | 72.145 |
| neuroticism$_{♀}$ ‡ | 51.053 | 9.006 | 52.499 | 2.572 | 71.812 |
| extraversion$_{♂}$ ‡ | 47.806 | 9.520 | 47.197 | 23.042 | 79.122 |
| extraversion$_{♀}$ ‡ | 52.305 | 9.937 | 51.910 | 26.732 | 80.557 |
| agreeableness$_{♂}$ ‡ | 48.883 | 9.889 | 48.317 | 17.623 | 80.131 |
| agreeableness$_{♀}$ ‡ | 50.717 | 9.818 | 50.122 | 19.318 | 79.893 |
| openness$_{♂}$ ‡ | 49.878 | 9.832 | 49.980 | 11.619 | 83.374 |
| openness$_{♀}$ ‡ | 50.107 | 9.799 | 50.226 | 12.496 | 84.575 |

**Notes:** ‡ not for years earlier than 2008. **Source**: Own calculations based on SOEP data.

Table A3: Linear Regression with Controls explaining Separation

| | **without interact.** | | **with interact.** | |
|---|---|---|---|---|
| | Est. Coef. | S.E. | Est. Coef. | S.E. |
| MCS shock$_{♂}$ | 0.024*** | 0.005 | 0.024*** | 0.006 |
| MCS shock$_{♀}$ | 0.021*** | 0.005 | 0.020*** | 0.005 |
| MCS shock$_{♂♀}$ | | | 0.003 | 0.016 |
| PCS shock$_{♂}$ | 0.001 | 0.004 | 0.001 | 0.004 |
| PCS shock$_{♀}$ | −0.005 | 0.005 | −0.004 | 0.004 |
| PCS shock$_{♂♀}$ | | | −0.009 | 0.010 |
| *MCS shock*$_{♂♀}$‡ | 0.045*** | 0.007 | 0.047*** | 0.014 |
| *PCS shock*$_{♂♀}$‡ | −0.004 | 0.005 | −0.011 | 0.008 |
| PCS$_{t-2,♂}$ | 0.010 | 0.012 | 0.010 | 0.012 |
| MCS$_{t-2,♂}$ | −0.040*** | 0.012 | −0.040*** | 0.012 |
| PCS$_{t-2,♀}$ | −0.012 | 0.011 | −0.012 | 0.011 |
| MCS$_{t-2,♀}$ | −0.015 | 0.011 | −0.015 | 0.011 |
| age$_{♂}$ | −0.001* | 0.000 | −0.001* | 0.000 |
| age$_{♀}$ | 0.000 | 0.000 | 0.000 | 0.000 |
| abs. age difference | 0.002*** | 0.001 | 0.002*** | 0.001 |
| education$_{t-2,♂}$ | 0.000 | 0.000 | 0.000 | 0.000 |
| education$_{t-2,♀}$ | −0.001** | 0.000 | −0.001** | 0.000 |
| abs. educ. difference$_{t-2}$ | 0.000 | 0.000 | 0.000 | 0.000 |
| east$_{t-2}$ | −0.005** | 0.002 | −0.005** | 0.002 |
| home owner$_{t-2}$ | −0.008*** | 0.002 | −0.008*** | 0.002 |
| child in hh$_{t-2}$ | 0.003 | 0.008 | 0.003 | 0.008 |
| common child$_{t-2}$♯ | −0.011*** | 0.004 | −0.011*** | 0.004 |
| # of children$_{t-2}$♯ | 0.002 | 0.002 | 0.002 | 0.002 |
| min. age child$_{t-2}$♯ | 0.001** | 0.000 | 0.001** | 0.000 |
| married$_{t-2}$ | −0.033*** | 0.005 | −0.033*** | 0.005 |
| partnership duration$_{t-2}$ | 0.000*** | 0.000 | 0.000*** | 0.000 |
| censored couple info$_{t-2}$ | −0.010*** | 0.003 | −0.010*** | 0.003 |
| employed$_{t-2,♂}$ | −0.010*** | 0.003 | −0.010*** | 0.003 |
| employed$_{t-2,♀}$ | −0.010*** | 0.004 | −0.010** | 0.004 |
| employed$_{t-2,♂♀}$ | 0.006 | 0.004 | 0.006 | 0.004 |
| income$_{t-2,♂}$ | 0.000 | 0.000 | 0.000 | 0.000 |
| income$_{t-2,♀}$ | 0.000 | 0.001 | 0.000 | 0.001 |
| income share$_{t-2,♂}$ | −0.022*** | 0.006 | −0.022*** | 0.006 |
| year 2006 | −0.004 | 0.003 | −0.004 | 0.003 |
| year 2008 | −0.004 | 0.003 | −0.004 | 0.003 |
| year 2010 | 0.000 | 0.004 | 0.000 | 0.004 |
| year 2012 | 0.000 | 0.004 | 0.000 | 0.004 |
| year 2014 | −0.006* | 0.003 | −0.006* | 0.003 |
| year 2016 | −0.003 | 0.003 | −0.003 | 0.003 |
| *N* | 30 296 | | 30 296 | |
| *R*-squared | 0.029 | | 0.029 | |

**Notes:** *** $p$-value < 0.01; ** $p$-value < 0.05; * $p$-value < 0.1; clustered standard errors; ‡sum of regression coefficients; seven panel waves (biennially 2004–2016); see Table 2 for the results from corresponding regressions without controls. **Source**: Own calculations based on SOEP data.

Table A4: Unconditional, Conditional, and Bias-Corrected estimated Effects (linear reg.)

| | **without controls** | | **with controls** | | **bias corrected**$^{\sharp}$ | |
|---|---|---|---|---|---|---|
| | Est. Effect | S.E. | Est. Effect | S.E. | Est. Effect | S.E.$^{\dagger}$ |
| MCS shock$_{♂}$ | 0.026*** | 0.006 | 0.024*** | 0.006 | 0.023*** | 0.005 |
| MCS shock$_{♀}$ | 0.025*** | 0.005 | 0.020*** | 0.005 | 0.019*** | 0.004 |
| *MCS shock*$_{♂♀}^{\ddagger}$ | 0.048*** | 0.014 | 0.047*** | 0.014 | 0.047*** | 0.012 |
| PCS shock$_{♂}$ | 0.001 | 0.004 | 0.001 | 0.004 | 0.002 | 0.004 |
| PCS shock$_{♀}$ | −0.005 | 0.004 | −0.004 | 0.004 | −0.003 | 0.004 |
| *PCS shock*$_{♂♀}^{\ddagger}$ | −0.015* | 0.008 | −0.011 | 0.008 | −0.010 | 0.008 |
| $N$ | 30 296 | | 30 296 | | – | |
| $R$-squared | 0.003 | | 0.029 | | – | |
| observed covariates | | | ✓ | | – | |

**Notes:** $^{\sharp}$Oster's (2019) method using $\delta = 1$ (equal selection) and $R_{\max} = 1.3\tilde{R} = 0.037$; *** $p$-value < 0.01; ** $p$-value < 0.05; * $p$-value < 0.1; clustered standard errors; $^{\dagger}$bootstrapped; $^{\ddagger}$sum of regression coefficients; seven panel waves (biennially 2004–2016); see Table 2 and Table A3 for comprehensive regression results. **Source**: Own calculations based on SOEP data using `psacalc` (Oster, 2013).

Table A5: Desc. Stats. – Health Shocks def. in terms of Abs. Changes in MCS and PCS

| | **Mean** | **S.D.** | **Med.** | **Min.** | **Max.** |
|---|---|---|---|---|---|
| MCS shock$_{♂}$ | 0.063 | 0.243 | 0 | 0 | 1 |
| MCS shock$_{♀}$ | 0.075 | 0.263 | 0 | 0 | 1 |
| PCS shock$_{♂}$ | 0.052 | 0.222 | 0 | 0 | 1 |
| PCS shock$_{♀}$ | 0.058 | 0.234 | 0 | 0 | 1 |
| MCS shock$_{♂♀}$ | 0.013 | 0.111 | 0 | 0 | 1 |
| PCS shock$_{♂♀}$ | 0.005 | 0.073 | 0 | 0 | 1 |

**Notes:** Statistics based on 30 296 couple-year observations that are used in OLS estimations; model that differentiates between male and female partner ignoring homosexual couples; seven panel waves (biennially 2004–2016); **health shocks** defined as $\Delta$MCS and $\Delta$PCS, respectively, smaller than $-0.13$; see Table 1, upper panel, for corresponding statistics for health shock definition of reference. **Source**: Own calculations based on SOEP data.

Table A6: Linear Regression explaining Separation – Absolute Health Shocks

| | **without interact.** | | **with interact.** | |
|---|---|---|---|---|
| | Est. Coef. | S.E. | Est. Coef. | S.E. |
| MCS shock$_{♂}$ | 0.020$^{***}$ | 0.005 | 0.017$^{***}$ | 0.005 |
| MCS shock$_{♀}$ | 0.019$^{***}$ | 0.005 | 0.016$^{***}$ | 0.005 |
| MCS shock$_{♂♀}$ | | | −0.004 | 0.004 |
| PCS shock$_{♂}$ | −0.003 | 0.004 | −0.007$^{*}$ | 0.004 |
| PCS shock$_{♀}$ | −0.006$^{*}$ | 0.004 | 0.019 | 0.015 |
| PCS shock$_{♂♀}$ | | | 0.006 | 0.013 |
| *MCS shock*$_{♂♀}^{‡}$ | 0.039$^{***}$ | 0.006 | 0.052$^{***}$ | 0.013 |
| *PCS shock*$_{♂♀}^{‡}$ | −0.009$^{*}$ | 0.005 | −0.004 | 0.012 |
| PCS$_{t-2,♂}$ | 0.011 | 0.012 | 0.011 | 0.012 |
| MCS$_{t-2,♂}$ | −0.045$^{***}$ | 0.012 | −0.045$^{***}$ | 0.012 |
| PCS$_{t-2,♀}$ | −0.011 | 0.012 | −0.011 | 0.012 |
| MCS$_{t-2,♀}$ | −0.020$^{*}$ | 0.011 | −0.020$^{*}$ | 0.011 |
| age$_{♂}$ | −0.001$^{*}$ | 0.000 | −0.001$^{**}$ | 0.000 |
| age$_{♀}$ | 0.000 | 0.000 | 0.000 | 0.000 |
| abs. age difference | 0.002$^{***}$ | 0.001 | 0.002$^{***}$ | 0.001 |
| education$_{t-2,♂}$ | 0.000 | 0.000 | 0.000 | 0.000 |
| education$_{t-2,♀}$ | −0.001$^{**}$ | 0.000 | −0.001$^{**}$ | 0.000 |
| abs. educ. difference$_{t-2}$ | 0.000 | 0.000 | 0.000 | 0.000 |
| east$_{t-2}$ | −0.005$^{**}$ | 0.002 | −0.005$^{**}$ | 0.002 |
| home owner$_{t-2}$ | −0.008$^{***}$ | 0.002 | −0.008$^{***}$ | 0.002 |
| child in hh$_{t-2}$ | 0.002 | 0.008 | 0.003 | 0.008 |
| common child$_{t-2}^{♯}$ | −0.011$^{***}$ | 0.004 | −0.011$^{***}$ | 0.004 |
| # of children$_{t-2}^{♯}$ | 0.002 | 0.002 | 0.002 | 0.002 |
| min. age child$_{t-2}^{♯}$ | 0.001$^{**}$ | 0.000 | 0.001$^{**}$ | 0.000 |
| married$_{t-2}$ | −0.033$^{***}$ | 0.005 | −0.033$^{***}$ | 0.005 |
| partnership duration$_{t-2}$ | 0.000$^{***}$ | 0.000 | 0.000$^{***}$ | 0.000 |
| censored couple info$_{t-2}$ | −0.010$^{***}$ | 0.003 | −0.010$^{***}$ | 0.003 |
| employed$_{t-2,♂}$ | −0.010$^{***}$ | 0.003 | −0.010$^{***}$ | 0.003 |
| employed$_{t-2,♀}$ | −0.010$^{**}$ | 0.004 | −0.010$^{**}$ | 0.004 |
| employed$_{t-2,♂♀}$ | 0.006 | 0.004 | 0.006 | 0.004 |
| income$_{t-2,♂}$ | 0.000 | 0.000 | 0.000 | 0.000 |
| income$_{t-2,♀}$ | 0.000 | 0.001 | 0.000 | 0.001 |
| income share$_{t-2,♂}$ | −0.022$^{***}$ | 0.006 | −0.022$^{***}$ | 0.006 |
| year 2006 | −0.004 | 0.003 | −0.004 | 0.003 |
| year 2008 | −0.004 | 0.003 | −0.005 | 0.003 |
| year 2010 | 0.000 | 0.004 | 0.000 | 0.004 |
| year 2012 | 0.000 | 0.004 | 0.000 | 0.004 |
| year 2014 | −0.006$^{*}$ | 0.003 | −0.006$^{*}$ | 0.003 |
| year 2016 | −0.003 | 0.003 | −0.003 | 0.003 |
| $N$ | 30 296 | | 30 296 | |
| $R$-squared | 0.028 | | 0.028 | |

**Notes:** $^{***}$ $p$-value < 0.01; $^{**}$ $p$-value < 0.05; $^{*}$ $p$-value < 0.1; clustered standard errors; $^{‡}$sum of regression coefficients; seven panel waves (biennially 2004–2014). **Source**: Own calculations based on SOEP data.

Table A7: PS Estimation – Logit Regressions explainig Mental Health Shocks

| | **MCS Shock Male** | | **MCS Shock Female** | | **MCS Shock Both** | |
|---|---|---|---|---|---|---|
| | Est. Coef. | S.E. | Est. Coef. | S.E. | Est. Coef. | S.E. |
| PCS shock$_{♂}$ | 0.266$^{**}$ | 0.118 | 0.055 | 0.106 | 0.345$^{*}$ | 0.208 |
| PCS shock$_{♀}$ | 0.003 | 0.116 | −0.133 | 0.115 | 0.113 | 0.221 |
| PCS$_{t-2,♂}$ | −4.431$^{***}$ | 0.317 | −0.970$^{***}$ | 0.309 | −3.787$^{***}$ | 0.625 |
| MCS$_{t-2,♂}$ | 3.679$^{***}$ | 0.382 | −2.490$^{***}$ | 0.283 | 1.443$^{**}$ | 0.718 |
| PCS$_{t-2,♀}$ | 0.201 | 0.348 | −3.763$^{***}$ | 0.278 | −2.808$^{***}$ | 0.595 |
| MCS$_{t-2,♀}$ | −2.273$^{***}$ | 0.305 | 3.999$^{***}$ | 0.317 | 2.886$^{***}$ | 0.709 |
| age$_{♂}$ | −0.030$^{***}$ | 0.009 | −0.023$^{***}$ | 0.007 | −0.019 | 0.016 |
| age$_{♀}$ | 0.015$^{*}$ | 0.008 | 0.000 | 0.007 | −0.001 | 0.016 |
| abs. age difference | 0.014 | 0.010 | 0.024$^{***}$ | 0.009 | 0.019 | 0.019 |
| education$_{t-2,♂}$ | −0.021 | 0.015 | −0.001 | 0.013 | 0.005 | 0.031 |
| education$_{t-2,♀}$ | 0.012 | 0.015 | −0.029$^{**}$ | 0.013 | −0.022 | 0.032 |
| abs. educ. difference$_{t-2}$ | −0.001 | 0.017 | 0.013 | 0.015 | 0.019 | 0.035 |
| east$_{t-2}$ | −0.126$^{*}$ | 0.073 | −0.009 | 0.062 | −0.089 | 0.147 |
| home owner$_{t-2}$ | −0.123$^{**}$ | 0.062 | −0.046 | 0.053 | −0.278$^{**}$ | 0.120 |
| child in hh$_{t-2}$ | −0.035 | 0.183 | 0.154 | 0.153 | −0.157 | 0.365 |
| common child$_{t-2}$ | 0.132 | 0.103 | −0.005 | 0.084 | 0.146 | 0.215 |
| # of children$_{t-2}$ | 0.016 | 0.062 | 0.003 | 0.050 | 0.158 | 0.106 |
| min. age child$_{t-2}$ | 0.006 | 0.011 | −0.009 | 0.010 | −0.015 | 0.023 |
| married$_{t-2}$ | 0.008 | 0.104 | −0.137 | 0.085 | 0.037 | 0.208 |
| partnership duration$_{t-2}$ | −0.003 | 0.004 | −0.007$^{*}$ | 0.004 | 0.003 | 0.009 |
| censored couple info$_{t-2}$ | 0.007 | 0.081 | −0.001 | 0.070 | −0.193 | 0.160 |
| employed$_{t-2,♂}$ | 0.157 | 0.112 | −0.083 | 0.100 | −0.087 | 0.197 |
| employed$_{t-2,♀}$ | 0.093 | 0.130 | −0.031 | 0.118 | −0.185 | 0.239 |
| employed$_{t-2,♂♀}$ | −0.179 | 0.146 | 0.068 | 0.132 | 0.061 | 0.273 |
| income$_{t-2,♂}$ | −0.020 | 0.021 | −0.012 | 0.012 | 0.009 | 0.024 |
| income$_{t-2,♀}$ | 0.017 | 0.030 | 0.028 | 0.020 | −0.130$^{*}$ | 0.077 |
| income share$_{t-2,♂}$ | −0.048 | 0.158 | 0.164 | 0.134 | −0.281 | 0.312 |
| year 2006 | 0.205$^{**}$ | 0.097 | 0.166$^{**}$ | 0.083 | 0.150 | 0.183 |
| year 2008 | 0.052 | 0.101 | −0.052 | 0.086 | −0.200 | 0.199 |
| year 2010 | 0.141 | 0.106 | 0.128 | 0.089 | 0.528$^{***}$ | 0.180 |
| year 2012 | 0.117 | 0.112 | 0.082 | 0.097 | 0.246 | 0.208 |
| year 2014 | 0.005 | 0.108 | −0.091 | 0.095 | −0.633$^{**}$ | 0.248 |
| year 2016 | −0.087 | 0.103 | −0.108 | 0.087 | −0.658$^{***}$ | 0.228 |

**Notes:** $^{***}$ $p$-value < 0.01; $^{**}$ $p$-value < 0.05; $^{*}$ $p$-value < 0.1; clustered standard errors; seven panel waves (biennially 2004–2014). **Source**: Own calculations based on SOEP data.

Table A8: PS Estimation – Logit Regressions explaining Physical Health Shocks

| | **PCS Shock Male** | | **PCS Shock Female** | | **PCS Shock Both** | |
|---|---|---|---|---|---|---|
| | Est. Coef. | S.E. | Est. Coef. | S.E. | Est. Coef. | S.E. |
| MCS shock$_{♂}$ | 0.335*** | 0.110 | 0.090 | 0.109 | 0.031 | 0.319 |
| MCS shock$_{♀}$ | 0.051 | 0.102 | −0.112 | 0.111 | 0.240 | 0.273 |
| PCS$_{t-2,♂}$ | 3.726*** | 0.371 | −1.002*** | 0.315 | 1.886* | 1.030 |
| MCS$_{t-2,♂}$ | −2.285*** | 0.317 | 0.837*** | 0.323 | −1.128 | 0.967 |
| PCS$_{t-2,♀}$ | −0.841*** | 0.324 | 3.350*** | 0.344 | 5.603*** | 0.962 |
| MCS$_{t-2,♀}$ | 0.222 | 0.327 | −2.099*** | 0.288 | −1.611* | 0.846 |
| age$_{♂}$ | 0.029*** | 0.008 | −0.010 | 0.007 | 0.043* | 0.025 |
| age$_{♀}$ | 0.000 | 0.008 | 0.025*** | 0.007 | 0.000 | 0.023 |
| abs. age difference | 0.010 | 0.010 | 0.020** | 0.009 | 0.002 | 0.029 |
| education$_{t-2,♂}$ | −0.086*** | 0.014 | −0.021 | 0.015 | −0.152*** | 0.046 |
| education$_{t-2,♀}$ | −0.003 | 0.015 | −0.034** | 0.015 | −0.106** | 0.046 |
| abs. educ. difference$_{t-2}$ | 0.006 | 0.017 | 0.009 | 0.017 | 0.075 | 0.053 |
| east$_{t-2}$ | −0.044 | 0.067 | −0.187*** | 0.068 | −0.248 | 0.205 |
| home owner$_{t-2}$ | −0.130** | 0.059 | −0.111* | 0.058 | −0.111 | 0.162 |
| child in hh$_{t-2}$ | 0.098 | 0.195 | −0.369** | 0.183 | −0.032 | 0.664 |
| common child$_{t-2}$ | 0.158 | 0.106 | −0.176* | 0.099 | −0.506 | 0.347 |
| # of children$_{t-2}$ | −0.038 | 0.061 | 0.123** | 0.057 | 0.181 | 0.187 |
| min. age child$_{t-2}$ | −0.014 | 0.012 | 0.010 | 0.011 | −0.013 | 0.043 |
| married$_{t-2}$ | −0.096 | 0.103 | 0.066 | 0.096 | −0.263 | 0.308 |
| partnership duration$_{t-2}$ | −0.010** | 0.004 | −0.010** | 0.004 | −0.008 | 0.010 |
| censored couple info$_{t-2}$ | 0.079 | 0.084 | −0.157** | 0.077 | 0.209 | 0.267 |
| employed$_{t-2,♂}$ | −0.015 | 0.104 | −0.095 | 0.099 | −0.557* | 0.299 |
| employed$_{t-2,♀}$ | 0.031 | 0.113 | −0.258** | 0.117 | −1.228*** | 0.368 |
| employed$_{t-2,♂♀}$ | −0.039 | 0.133 | 0.167 | 0.130 | 1.110*** | 0.425 |
| income$_{t-2,♂}$ | −0.050** | 0.022 | −0.029* | 0.017 | 0.028 | 0.019 |
| income$_{t-2,♀}$ | 0.000 | 0.027 | −0.044 | 0.032 | −0.228* | 0.126 |
| income share$_{t-2,♂}$ | −0.215 | 0.163 | 0.058 | 0.161 | −1.147*** | 0.382 |
| year 2006 | 0.015 | 0.093 | 0.041 | 0.094 | 0.797*** | 0.269 |
| year 2008 | −0.087 | 0.094 | 0.078 | 0.093 | 0.452 | 0.275 |
| year 2010 | −0.009 | 0.100 | 0.103 | 0.099 | 0.409 | 0.308 |
| year 2012 | −0.116 | 0.109 | 0.066 | 0.105 | 0.185 | 0.344 |
| year 2014 | −0.054 | 0.098 | 0.146 | 0.097 | 0.169 | 0.320 |
| year 2016 | 0.070 | 0.092 | 0.291*** | 0.090 | 0.302 | 0.299 |

**Notes:** *** $p$-value < 0.01; ** $p$-value < 0.05; * $p$-value < 0.1; clustered standard errors; seven panel waves (biennially 2004–2016). **Source**: Own calculations based on SOEP data.

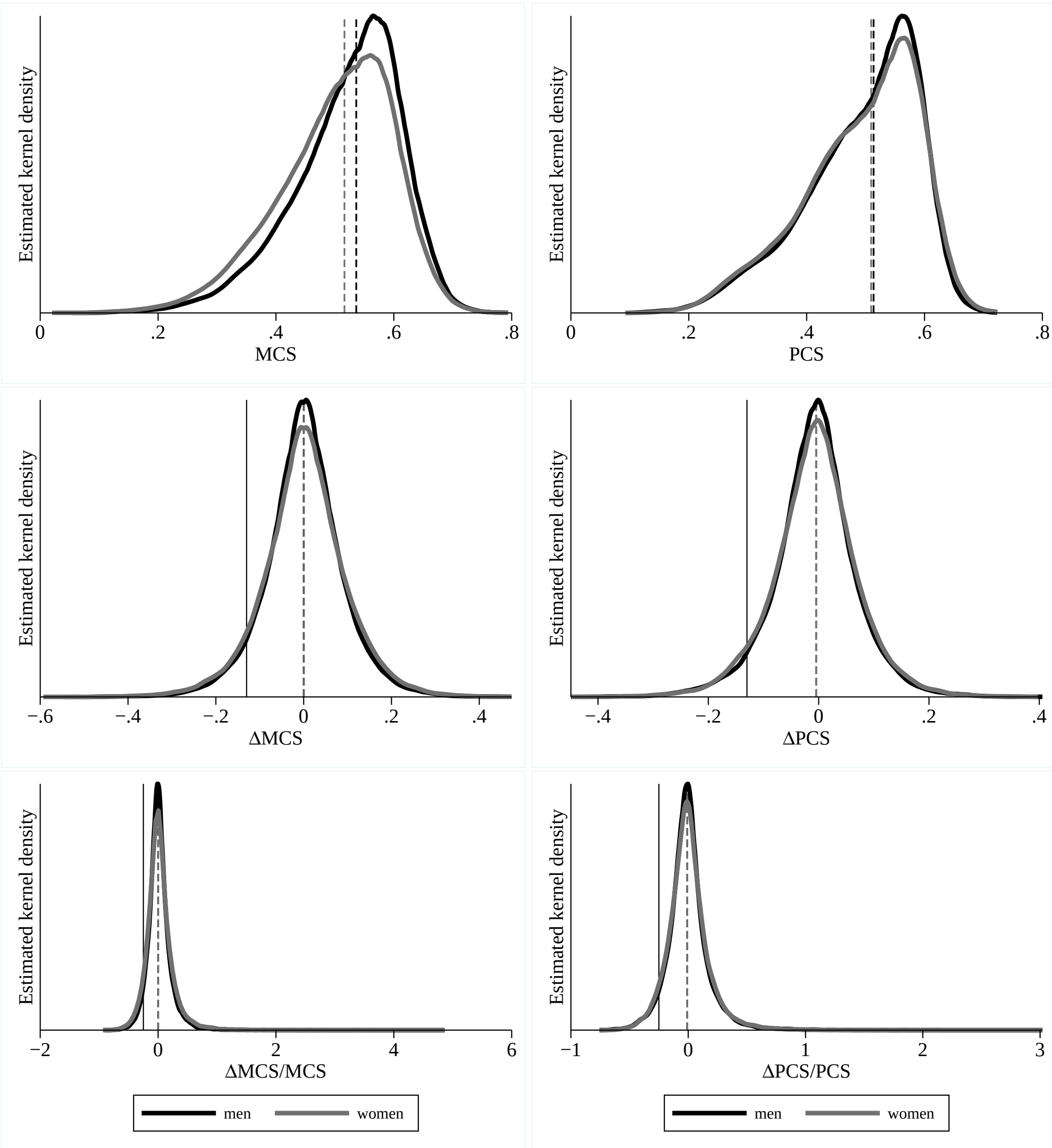


Figure A1: Estimated kernel densities of the level (first row), the absolute change (second row), and the relative change (third row) of MCS (left column) and PCS (right column). **Notes**: Based on estimation sample for the fixed effects model; dashed lines mark the respective median; solid vertical lines mark health-shock thresholds (−0.25 for relative and −0.13 for absolute change). **Source**: Own calculations based on SOEP data.

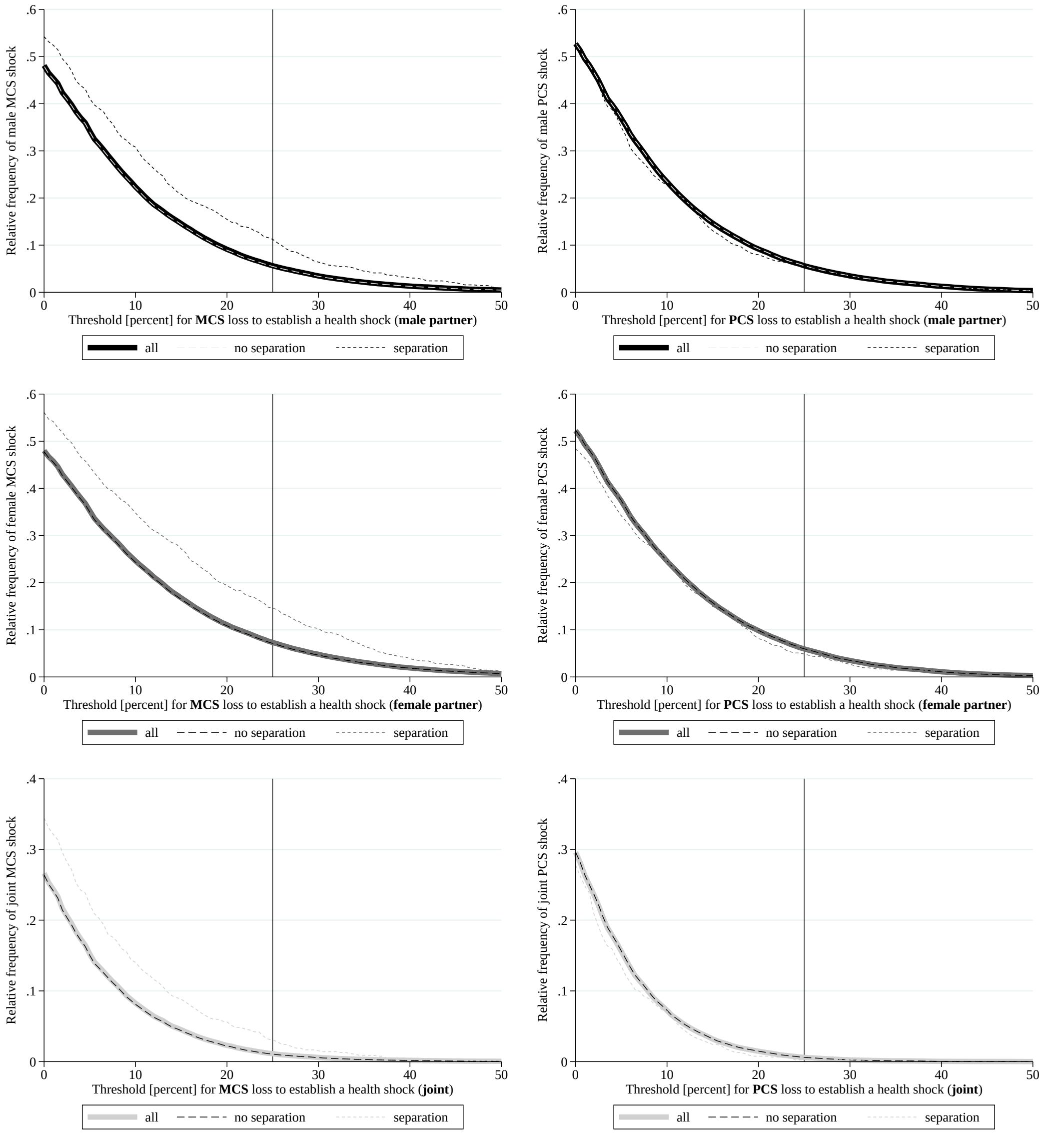


Figure A2: Relative frequency of health shocks as function of threshold to establish a health shock; MCS shock (left) and PCS shock (right); shock that hits male partner (top row), shock that hits female partner (mid row), joint health shock (bottom row). **Notes**: Dashed lines indicate relative frequencies conditional on whether or not the couple separates in the subsequent two-years period; the vertical solid line marks the preferred threshold of 25 percent. **Source**: Own calculations based on SOEP data.