Depressed Peers in Early Parenthood *

Anne Toft Hansen[1], Jonas Cuzulan Hirani[1], and Miriam Wüst[1,2,3]

[1]The Danish Center for Social Science Research - VIVE

[2]University of Copenhagen

[3]Center of Economic Behavior and Inequality - CEBI

Version: February 7, 2023.

Perliminary and uncomplete draft.

Abstract

We examine peer effects in mental health problems among new mothers. To study these spillovers, we exploit an institutional feature of the Danish public health service: Home visiting nurses assign consenting new mothers to 4-6 peers in what is known as "new mother groups". Those groups constitute informal fora for meetings among these externally assigned peers. Combining data on group assignment, pre-group meetings mental health screenings and family background, we show that mother group assignment is arbitrary conditional on a narrow set of well-defined characteristics observed by nurses. Exploiting the resulting variation in the mental health status of members across groups, we document that exposure to a depressed peer during early parenthood increases the mental health care use of other group members. Mothers exposed to a depressed peer are more likely to consult their general practitioner and to be hospitalized due to mental health issues. Results for the heterogeneity of these health impacts and results documenting labor market impacts for mothers support that social contagion of mental health is a driving mechanism for our findings.

*We thank the Danish municipalities that have generously shared their records from the home visiting program with us. The use of these municipal health data in our research project was approved by the Danish Patient Safety Authority (approval 3-3013-2507/1). We gratefully acknowledge financial support from Danish Council for Independent Research (grant 0218-00003B). Wüst is a member of the Center for Economic Behavior and Inequality (CEBI), which is financed by the Danish National Research Foundation, Grant DNRF134.
We thank Hans Henrik Sievertsen and seminar participants at CEBI (University of Copenhagen) for helpful comments.
Contact information. Hansen: ath@vive.dk, Hirani (corresponding author): jjh@vive.dk and Wüst: miriam.w@econ.ku.dk

1 Introduction

Mental health problems are a rising concern across settings and understanding their complex causes and consequences is of first order policy importance. This paper zooms in on the question on whether mental health problems spread within networks or peer groups. Specifically, we study the role of peers for the postnatal mental health of new mothers.

The postpartum period represents a crucial stage for women's mental health (Munk-Olsen and Agerbo, 2015; Saxbe et al., 2018). Postpartum depression is very common with prevalence rates ranging from 13-19 percent (O'hara and McCabe, 2013). It has been linked to longer-run maternal health problems and delayed child development (Halligan et al., 2007; Stein et al., 2008). In our paper we apply a causal design (arbitrary assignment of relevant peers) to contribute evidence on social contagion of mental health in this important and sensitive population (new mothers) during a crucial period (the postpartum period).

Our paper contributes to a larger literature on peer effects in mental health. Evidence from psychology and the medical science has documented strong correlations of mental health among peers (Kramer et al., 2014). To isolate the causal impact of exposure to peers with poor mental health, empirical research in economics has zoomed in on settings that provide plausibly exogenous variation in peer mental health, typically class and dorm room settings (Eisenberg et al., 2013). These studies tend to find weaker peer impacts in mental health.

We study peer effects among new mothers in Denmark, where we exploit an institutional feature of the universal home visiting program to isolate variation in peer mental health. In this program, all new families are offered up to five universal nurse visits to both monitor and screen infant and mother health, inform and guide parents, as well as refer families to additional treatments if necessary. Moreover, as part of the program, new mothers are offered assignment to a "new mother group", typically a group of five to seven new mothers.[1] The purpose of these mother groups is to put new mothers in touch with a relevant peer group

[1] As we detail in section 2, there are exceptions to this general rule for families with identified social or health needs. Municipalities typically offer specialized treatments for them.

available for daytime meetings and peer interactions—primarily while on parental leave and thus likely to be detached from other peer groups, such as co-workers. As we discuss in detail, municipal nurses form mother groups based on observable information on families, primarily the date of birth of the child, family address and child parity. Mothers are invited to join their mother group during family home visits. After initial assignment, mother group members autonomously decide about meeting schedules and activities. In other words, nurses do not participate in group meetings or decide on their meeting schedule or content.[2] As we discuss in detail, the relevant population for mother group assignment excludes families with ex ante identified severe risk factors (among them known maternal mental illnesses) and well-defined groups such as twin families (who are typically assigned in specialized groups). Thus, as we show using our population data, our analyses do not speak to impacts of peers at these margins but focus on the broad population of new parents who enter family life without identified need for specialized health or social treatments.

In our setting, new mothers are exposed to an externally assigned peer group, which in principle allows us to study the causal effect of exposure to a peer with maternal mental health issues–we refer to this peer as a depressed peer in the following. Our empirical strategy rests on two critical components: (1) conditionally random assignment into mother groups and (2) a measure of maternal mental health (to define "peer quality"), which is measured prior to peer interactions and thus circumvents the reflection problem that may flaw our analysis of peer effects. In our analyses, we show that group assignment (into treated and control groups), balances a broad range of baseline characteristics across mother groups (only some of which are observed to assigning nurses). Moreover, our measure of own maternal mental health does not predict treatment status of mothers (i.e., exposure to a peer in poor mental health). We measure "peer quality" using the score of a postnatal depression screening with the Edinburgh Postnatal Depression Scale (EPDS) around two months after the birth of their child. This screening typically happens prior to the first group interaction.

[2]Some municipalities offer nurse participation in initial mother group meetings. Unfortunately, we have no data documenting how consistently this takes place.

Combining data from administrative sources at Statistics Denmark and municipal nurse records for births in the 2010-2017 period, we examine the impact of exposure to a peer in poor mental health on mothers' own mental health. We find that mothers exposed to a depressed peer in their mother group have a higher uptake of mental health-related care. Specifically, a depressed peer increases the probability of a mental health care contact two years after childbirth by 10 percent. Decomposing this overall effect, we find a nine percent increase in GP mental health consultations and a 30 percent increase in mental health hospitalizations. We do not find any spillovers to fathers' mental health care usage.[3] Considering other types of health care, we find an increase in take-up of care in the universal home visiting program (both in need-based additional home visits and phone contacts with the family nurse).

The increase in mental health care usage among mothers exposed to a depressed peer may be due to two primary mechanisms: social contagion of mental health problems or a transfer of information on available health care services. Given that we measure mental health impacts as uptake of health care, we are concerned about this competing information channel: Mothers may have unchanged mental health but exposure to a depressed peer may lead them to adjust their health behaviors (higher uptake of care) due to updated information on treatment options in the Danish public health care system. To discriminate between these mechanisms, we study effects beyond mental health care usage and provide heterogeneity analyses. First, we find that exposure to a depressed peer negatively impacts maternal employment while increasing the share of mothers on sick leave **when their children are XX months old**. As we also find suggestive evidence for health impacts on children, we conclude that our results strongly point towards actual impacts on underlying maternal mental health and thus towards social contagion of mental health as a mechanism. Second, exploring heterogeneity of impacts across types of mothers and groups, we find the strongest peer effects for mothers, who most likely already possess information related to mental health care. Also this finding points to the importance of social contagion rather than a pure information channel.

[3]Fathers have on average less than half the number of contacts compared to mothers.

Importantly, in the absence of data on actual group meetings, all our analyses remain reduced form analyses of the effect of peer assignment. Critical for the interpretation and policy implications of our findings, we need to discuss the potential impact of endogenous group stability. May our results be due to selective group dissolution (if groups with a depressed member have a higher (lower) probability of not interacting)? We argue that if depressed peers drop out mother groups our estimates will underestimate mental health spillovers to other group members. If, however, there is selective group dissolution for groups with a depressed peer, a lack of overall peer interactions could be a mechanism explaining our findings of poorer mental health for treated mothers. While we have no data on actual group meetings (a feature that we share with many studies on peer effects), we show that observable group characteristics do not critically matter for our results. Examining heterogeneity across group SES concordance, we find peer effects for mental health in both SES concordant and mixed groups. This finding suggests that across different types of groups–that may vary in their propensity to dissolve–peer effects in mental health are relevant.

Our study makes three main contributions: First, we study an unexplored margin of peer effects in mental health. In doing so, we extend the literature on peer effects in mental health exploiting peer variation in college dorm rooms, class rooms, workplaces and neigborhoods (Sacerdote, 2001; Lavy and Schlosser, 2011; Dahl et al., 2014; Herbst and Mas, 2015; Getik and Meier, 2022; Giulietti et al., 2022; Eisenberg et al., 2013; Golberstein et al., 2016; Frijters et al., 2019; Zhang, 2019; Hasager and Jørgensen, 2021). Within this small literature, most papers use assignment into class rooms as identifying variation and find weak evidence of mental health peer effects in this setting. In general, papers that exploit natural experiments to estimate causal effects find much weaker mental health peer effects compared to an observational literature that likely suffers from selection issues (Joiner Jr and Katz, 1999; Fowler and Christakis, 2008; Hill et al., 2015). Our paper contributes new evidence on peer effects in mental health in terms of setting (mother groups) and thus study population (new mothers). Closely related to our study is a literature documenting other peer effects in the pre-

and postnatal period (Nicoletti et al., 2018; Welteke and Wrohlich, 2019; Cavapozzi et al., 2021; Cortés et al., 2022). Considering fellow mothers in family and workplace networks, these studies show a strong influence of peers for maternal labor market and parental leave decisions. Our findings are in line with the general conclusions of this work that peers matter in this crucial period of family formation. Our findings highlight the role of mental health and as a consequence also adds evidence on the economic and societal costs of mental health distress (Conti et al., 2006; Peng et al., 2016; Banerjee et al., 2017; Wang et al., 2022).

Second, our work sheds light on a modifiable cause for postnatal mental health issues: exposure to depressed peers. Existing research on maternal postparum mental health has identified several causes such as parental leave policies, external health shocks (e.g. the death of a relative) and father involvement (Aitken et al., 2015; Saxbe et al., 2018; Persson and Rossin-Slater, 2018; Maselko et al., 2019; Baranov et al., 2020; von Hinke et al., 2022).

Finally and as a third contribution, we provide further evidence on the impacts of the design of early investment policies, such as the Danish universal home visiting program for new families (Hirani and Wüst, 2022; Hirani et al., 2022). Our paper adds to this literature by extending our understanding of the determinants of postnatal mental health and by studying a key element in the Danish program (mother groups). Central policy implications of our paper are that mother group assignment and potentially active support of groups with mothers with mental health issues are worth considering to confront public health issues such as poor mental health in the postpartum period.

2 Background and Data

2.1 Universal Postnatal Care in Denmark

In Denmark, all new families have access to universal nurse home visits in a program administered in each municipality within the frame of national guidelines.[4] These guidelines

[4]For a general overview of the universal early-life health policies in Denmark see Wüst (2022).

recommend up to five nurse visits within the first year after childbirth.[5] The program is a composite treatment with the goal to monitor family health, support parental investments of various kinds, and refer families with needs to other specialists. We focus in our analyses on two central features in the nurse program: First, the program has a strong focus on postnatal maternal mental health. Second, nurses offer all new mothers to be part of a mother group, a group of peers to meet and interact with during at least the time of parental leave.

During the universal nurse visit in the third month of the child's life, nurses screen mothers (and increasingly fathers) for mental health issues. Municipalities have over time adopted a standardized test (rather than an informal nurse assessment) to evaluate postnatal mental health, the Edinburgh Postnatal Depression Scale (EPDS) (Cox and Holden, 2003; Smith-Nielsen et al., 2018). Nurses administer this internationally recognized questionnaire with 10 items in the family home. The final score of the EPDS ranges from 0-30 with a higher score indicating worse mental health.[6] The EPDS serves as our measure of postnatal maternal mental health upon entry in the mother groups. The main benefit of the screening score (instead of using mental health care usage) is that it is not demand-driven but a universal screening provided by nurses during a home visit. 90 percent of screenings take place during the third month of the child's life in conjunction with a nurse visit. The screening is the earliest signal we have on postnatal maternal mental health.

In case of a high screening result, nurses can take a set of possible actions: They can follow-up themselves at later universal visits, book an extra nurse visit, schedule phone conversations, or refer mothers to the family GP or other mental health professionals. Additionally, nurses can choose to administer a second test at a following visit. Nurse make their own assessment of additional offers to mothers with high screening scores. While the EPDS has a validated cut-off at score of 11, only very recently, have municipalities adopted this cut-off as a formal decision tool to support nurses (Smith-Nielsen et al., 2018).

[5] Additionally, the program includes extra services (such as targeted visits) for families with identified needs. These extra services are offered at the discretion of nurses.

[6] We present the full questionnaire in Appendix A.1.

A second central feature of the nurse program is the initiation of mother groups for new mothers and their children. The goal of this offer is to help mothers establish a group of peers in similar circumstances during their time spent on parental leave and thus more or less detached from other peer groups (for example, at work). A subset of nurses administer group formation (i.e., not all assigned group members have the same family nurse). These nurses typically form groups based on the timing of birth, geographic proximity and parity. Other observables may facor into the nurse decision on group assignment and as we show in section 3, group assignment excludes families that have identified risk factors. Thus our analyses will be relevant for a general population of families entering the nurse program without identified social or health issues.

After group assignment, family nurses are informed and invite respective mothers during standard home visits. They only put consenting group members in touch with each other but do not participate in group activities. Once formed, groups autonomously decide when, where and how frequently to meet and what to discuss during meetings. Thus there is large variation in how specific mother groups function. Anecdotal evidence suggests that group intensity ranges from a few monthly meetings to long-lasting friendships. As we only observe group formation and not meetings, we study the impact of group assignment in a reduced form framework.[7]

In our empirical analyses, we exploit the centralized group assignment of mothers and the screening of mothers with the EPDS in the family home. In practice, a subset of municipal nurses regularly allocate incoming new mothers into groups (based on the described observables)–thus the actual mental health screening by the individual family's nurse is typically not a factor taken into account in this step (even though in extreme cases screening of severe issues can lead to additional treatments and thus drop out of the standard mother group system). While mothers with known severe issues (with respect to mental health or

[7]Parallel to the nurse program, families are also offered postnatal care including vaccinations and preventive health checks at the family GP. The recommended childhood vaccinations include three vaccines in the first year of children's life scheduled three, five and 12 months after birth. Preventive health checks are scheduled five weeks, five and 12 months after childbirth.

other areas of concern) do not enter this process and may be allocated in special groups, for the vast majority of mothers entering the pool of eligible group participants the assignment mechanism implies arbitrariness of group assignment. We discuss the assumption of exogenous group formation in greater detail in section 3 and provide suggestive evidence for its validity.

2.2 Data and Sample

All our analyse are based on data from families residing in the 61 out of the 98 Danish municipalities that have agreed to share nurse records for the 2010-2017 period. To measure maternal mental health issues and peer assignment in mother groups, we use data from this subset of municipalities because no national administrative data on nurse treatments (such as visits or screenings) exist.[8] We merge the nurse records obtained from the municipalities with the well-known national Danish administrative register data from Statistics Denmark. We use the unique Danish personal identifier for mothers, which is recorded in both the nurse records and the register data. As mothers can have have more than one child, we use the date of birth relative to the registration date of nurse visits to identify the relevant child of each mother in the administrative data.

The administrative data at Statistics Denmark include parental background characteristics such as age, education, employment and cohabitation status, as well as information recorded at birth such as birth weight, gestational age, parity and length of hospital stay. Also from the national register data, we obtain information on our main outcome measures: parents mental health care usage (GP mental health consultations, psychologists/psychiatrists sessions and hospitalization based on a mental health diagnosis). Finally, the data contain information on uptake of general GP care, hospitalizations, employment, sick leave and parental leave, which we use as secondary outcomes. The labor market data contain weekly indicators for labor market status. From these status data we create weekly indicators for

[8]For detailed description of the nurse data see (Hirani and Wüst, 2022) and (Wüst et al., 2020) (in Danish).

the mother being on either parental leave, sick leave, or in employment, respectively.

To arrive at our analysis sample, we apply the following sample restrictions: First, we only keep families from municipalities and years with a good coverage of the relevant nurse registrations in our data. This step is necessary because not all 61 municipalities record mental health screenings or mother group assignments for all years in our data.[9] Thus we only use data on families covered in an unbalanced sample of 31 municipalities and only during years with systematic registrations during our data period. These municipality-year cells together account for 25 percent of all birth in Denmark in those years (or 86,045 children).[10] Second, as not all mothers with a newborn join a mother group, mothers who are not assigned to a group are not in our analysis sample. We can, however, assess the nature of potential selection into mother groups (which can be due to maternal decisions or identified special needs and thus alternative treatments assigned by nurses). We analyze background characteristics of assigned vs not-assigned mothers and their families to do so. Third, we constrain our sample to only include mothers from mother groups with complete mental health registrations for all members (we relax this limitation in our robustness tests). Our final analysis sample includes mothers of children born between 2012 and 2017, assigned to a mother group where all group members are screened for postnatal mental health issues (N=17,187).[11] We use data on a total of 3,499 groups with an average size of 5.1 assigned mothers.

2.3 Descriptive Statistics

To understand potential selection into our analysis sample, we start by comparing mothers assigned to mother groups and mothers not assigned to (universal) groups in the 31 Danish

[9]There is no legal requirement for specific nurse registrations and municipalities have discretion on how to register their nurse activities. Thus among the 61 municipalities that share their data with us, there is variation in what types of registrations they use.

[10]For each municipality and year, we calculate the coverage rate of the nurse data by diving the number of mothers in the nurse data with the total number of new (resident) mothers. We include municipalities and years with a coverage rate above 60 percent.

[11]Appendix Table A1 compares the final sample to the population in the relevant municipalities and birth cohorts.

Tab. 1 Sample Selection

	Number of observations (1)
Children in relevant municipalities*years with data coverage	86,045
Children in munic*year with data coverage: In mother group	45,758
Children in munic*year with data coverage: In group where all members are screened	17,187

Notes: The table shows the number of children as the sample is sequentially restricted going from no restrictions in the first row to all imposed restrictions in the last row.

municipalities and relevant years between 2012 and 2017.[12] As illustrated in Appendix Table A2, mothers who are not assigned to a mother group are less likely to be of Danish origin, have a lower educational attainment and have lower incomes–in line with what we would expect as these observables are likely correlated with other (unobserved) measures of health and social disadvantage that triggers a specialized treatment instead of a mother group assignment. Thus the descriptives suggest some selection into mother groups and potential for threats to generalizability. When we move to all assigned mothers, Appendix Table A3 shows that mother groups with and without full screening coverage are very similar on many background characteristics. There are, however, also a few differences worth emphasizing: First, 56 percent of children in our analysis sample of mother groups with full screening coverage are first-born (compared to 45 percent mother groups without full coverage). This difference is likely due to nurses putting greater emphasis on first-time mothers both with respect to frequency of visits and screenings. Also at this step of the data selection process, we are more likely to omit non-Danish origin mothers from our analysis sample. Given that the screening tool has to be completed in Danish, language barriers likely explain this selection.

Finally, we assess whether mothers with mental health issues as indicated by the EPDS screening are more or less likely to enrol in mother groups than mothers without issues. Figure 1 plots the distribution of screening scores in our mother group sample overlayed with

[12]To be precise, mothers not assigned to mother groups can be members of "specialized" groups such as group interventions for families with health risks. Each municipality can decide on which specific offers to provide as a specialized treatment scheme in parallel to the universal offer for the general population of families.

the unrestricted distribution of scores. The two distribution are very similar bell-shaped distributions with a long right tail. The median score is four and the mean score is 4.9 in the unrestricted sample and 4.9 in our final sample while 5.9 percent scores above 10 in unrestricted sample compared to 5.7 percent in the final sample. A formal Komogorov-Smirnov test for equality between the two distribution can not be rejected and confirms that on average mothers in the final sample are not different to the unrestricted population in terms of their postnatal mental health as measured in the screening.

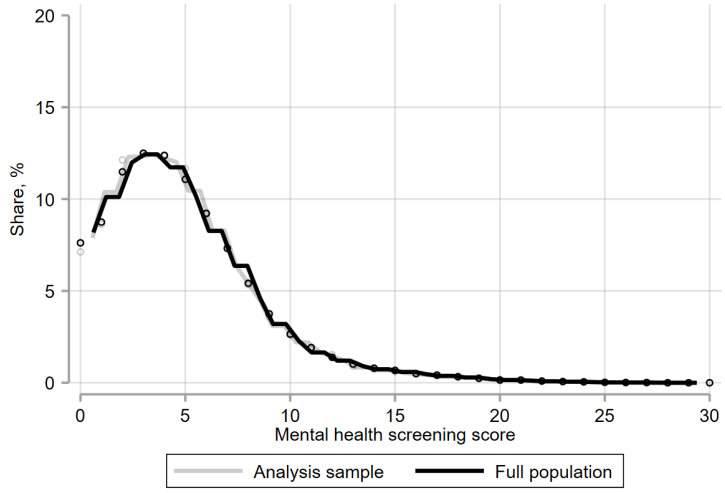


Fig. 1 Distribution of Mental Health Screening Scores (EPDS) among all Screened Children and in the Final Sample

Notes: The figure shows the distribution of EPDS screening scores in our final analysis sample and among all screened mothers, i.e. including those not restricted to the mother being assigned to a mother group. The dots show the share of the population within each score.

Table 2 presents summary statistics for our final analysis sample of mother groups by treatment status (i.e., mothers in group with at least one mother with an EPDS screening score above 10). The treatment and control group mothers are very similar in this mother group sample. The largest difference is again the share of first-born children (59 percent are first-born in the treated group vs. 55 percent in the control group). Empirically, first-time mothers are more likely to experience mental health issues and because first-time mothers are more likely to be grouped together, first-time mothers are also more likely treated. The table

also shows that screenings are made on average 63.5 days after childbirth in both treatment and control group.

In sum, while our analysis data consist of a sample with more affluent families than the general population and under-represents non-native Danes, it is a sample of relevance as especially first-time parents might be more susceptible to peer influence. Moreover, impacts of peers in this sample are relevant in a policy perspective across settings with similar universal offers for new parents.

Tab. 2 Summary Statistics: Characteristics of Treated and Control Individuals in the Main Analysis Sample.

	— Means —	
	No peers with depression (1)	Depressed peer (2)
First-time mothers	0.55	0.59
C-section	0.19	0.19
Child sex	0.49	0.48
Low birth weight	0.03	0.03
Preterm birth	0.04	0.04
Home birth	0.03	0.03
Income, mother	257,718	252,759
Income, father	352,089	342,050
Married	0.38	0.36
Cohabiting	0.84	0.83
Prim. school, mother	0.08	0.09
Prim. school, father	0.11	0.11
Higher educ, mother	0.32	0.31
Higher educ, father	0.19	0.19
Uni. degree, mother	0.25	0.24
Uni. degree, father	0.20	0.20
Danish, mother	0.91	0.92
Danish, father	0.87	0.86
Age, mother	30.86	30.69
Age, father	30.91	30.52
Inpatient mental health hosp.	0.02	0.03
Outpatient mental health hosp.	0.11	0.12
GP mental health consultation prior to birth	0.35	0.36
Psychologist/psychiatrist prior to birth	0.22	0.23
EPDS above 10	0.07	0.09
EPDS score	4.81	5.15
Age for screening (days)	63.50	63.53
Observations	12615	4572

Notes: The table shows means of prebirth characteristics for children with a depressed peer (scoring above 10 in the EPDS score) and children without a depressed peer. All children are part of the final analysis sample and are thus born in 2012-2017 in municipalities with nurse registration and registered in both a mother group and with a screening result. Birth characteristics (top six rows) are measured at birth while remaining parental characteristics are measured in the year prior to birth. The last three rows are measured after birth.

3 Empirical Strategy

To identify the role of peer mental health issues for shaping maternal mental health trajectories, we rely on variation in exposure to a depressed group member that is plausibly unrelated

to selection of individuals into peer groups. We exploit that typically (a subset of) nurses assign mothers to groups based on a narrow set of observable characteristics, predominately date of birth and parity of the child, as well as geography. The families' nurses hereafter invite mothers during standard home visits to join the groups.

We construct our treatment measure for individual i in group g as an indicator equal to one if at least one of i's peers j has a high score S_j on the postnatal mental health screening (i.e., EPDS score above 10),

$$T_{ig} = \mathbb{1}\sum_{j \neq i} S_j \leq 1. \tag{1}$$

Note that individual i's own screening result does not feature in the peer measure for i. This leave-out strategy rules out mechanical correlation between T_{ig} and future maternal mental health outcomes. Using T_{ig} as our measure of exposure, we estimate the effects of peer exposure in the following regression model:

$$y_{ig} = \alpha T_{ig} + \delta_m + \zeta_t + \beta Firstborn_i + \epsilon_{ig} \tag{2}$$

The coefficient α is the estimate of interest and represents the causal effect on outcome Y_{ig} of assignment to at least one peer with postnatal mental health issues. To control for systematic differences across time of birth and municipality, we include municipality and month $\times$ birth year fixed effects, δ_m and ζ_t. Furthermore, as most municipalities use child parity in their assignment process for mother groups, we include an indicator for having a first-born child in all regressions. Finally ϵ_{ig} is an error term, and we cluster standard errors at the mother group level.

To explore potential mechanisms for the impact of depressed peer assignment on the outcomes of mothers, we study response heterogeneity across different types of mothers and

groups. We run both split-sample analyses and interacted analyses on the full sample.[13]

The identifying variation in our analyses comes from differences in assignment to a peer with poor mental health across individuals in different mother groups resident in the the same municipality and giving birth to their child in the same month of a given year. This variation has to be uncorrelated with observed and unobserved characteristics of mothers not controlled for in our analyses. We argue that two central aspects of our institutional setting make this assumption plausible: First, while group assignment is centrally performed by a subset of nurses in the municipality on a first-come-first-served basis, the mental health screening is performed by the family nurse in the family home. Thus for all mothers eligible for assignment only a very limited set of observables are available to the nurse deciding on group allocation. Following assignment, mothers accept or decline the invitation to participate in a group prior to knowing anybody from the group. Second, we use the EPDS screening result to measure "peer quality". This measure is independent among group members and typically generated *prior* to groups' meetings. Thus, it helps us address the reflection problem, i.e. that we cannot conclude on individuals' behavior based on group average behavior (Manski, 1993; Bramoullé et al., 2020).

We informally assess the arbitrariness of group assignment by showing that we cannot predict our treatment using a large set of background characteristics as explanatory variables. We regress the treatment indicator on these characteristics, as well as time and municipality FE and an indicator for a first-born child to test if observed characteristics predict the presence of a depressed peer.[14] Figure 2 shows parameter estimates alongside confidence

[13]Specifically, we start by estimating separate regressions for samples defined by mother or group characteristics. Additionally, and to judge on significance of potential differences across subgroups, we run interacted versions of equation (2) with a subgroup indicator χ_i, as in

$$y_{ig} = \alpha T_{ig} + \omega\chi_i + \Omega\chi_i \times T_{ig} + \delta_m + \zeta_t + \beta Firstborn_i + \epsilon_{ig}. \tag{3}$$

Ω captures the differential impact of the treatment (a depressed peer) on subgroups that could both defined by individual characteristics (e.g. low SES mother) and group characteristics (e.g. group of individual i consists only of first-time mothers).

[14]Specifically we run the following regression: $T_{ig} = \beta' X_{ig} + \delta_m + \zeta_t + \beta Firstborn_i + \epsilon_{ig}$ and test the hypothesis that $\beta' = 0$.

intervals and a F-test for the joint significance of the individual mother, birth and family characteristics. All coefficients (beyond the first-born indicator) are insignificant and close to zero. Our F-test cannot reject that all coefficients are jointly zero. Hence, the test supports our assumption that assignment into mother groups is conditionally random. The first-born indicator is highly significant which shows that this characteristic is a factor in group assignment and important for us to condition on. In Appendix Figure A1, we perform a related test where we estimate the difference in screening score across the distribution (i.e. we use the screening score as outcome) between treatment and control group. We find that the treatment and control groups are balanced across the scale of the EPDS screening tool.

[tbc: survey evidence among DK nurses on nature of group assignment]

Finally, a natural question arising in our context is: Does the mental health of a mother in poor condition improve or worsen if she is assigned a mother group with peers in "good" mental health? Our data and empirical framework is not well-suited to answer this question. If we focus on the impact of groups on depressed mothers, we only have a very small sample of focal mothers. Moreover, it is unclear how to define a happy peer group. The EPDS screening tool screens for poor mental health (depression) and not for happiness. Thus, it does not measure well-being but the risk of depression. We believe the data at hand suits our primary research question best, and that results from a reverse exercise should be interpreted as suggestive at best.[15]

[15]In this more suggestive analysis, we have attempted to define an alternative treatment as the average screening score among mothers' peers. While we chose the average screening score among peers of the depressed mother to proxy the happiness of the group, this approach may be problematic as an average score likely masks very different peer compositions across groups. We used this average score as a measure of group happiness and ran analyses for the subgroup of mothers with a screening score above 10 (N=1,282), always controlling non-parametrically for these mothers' own screening score. Appendix Figure A3 presents the results for the impact of the average screening score among peers on the health of depressed mothers. We do not find any systematic evidence that the average screening score of peers influences mental health care usage of high scoring mothers as all coefficients are insignificant. In general, these results suffer from power issues and the evidence is suggestive at best.

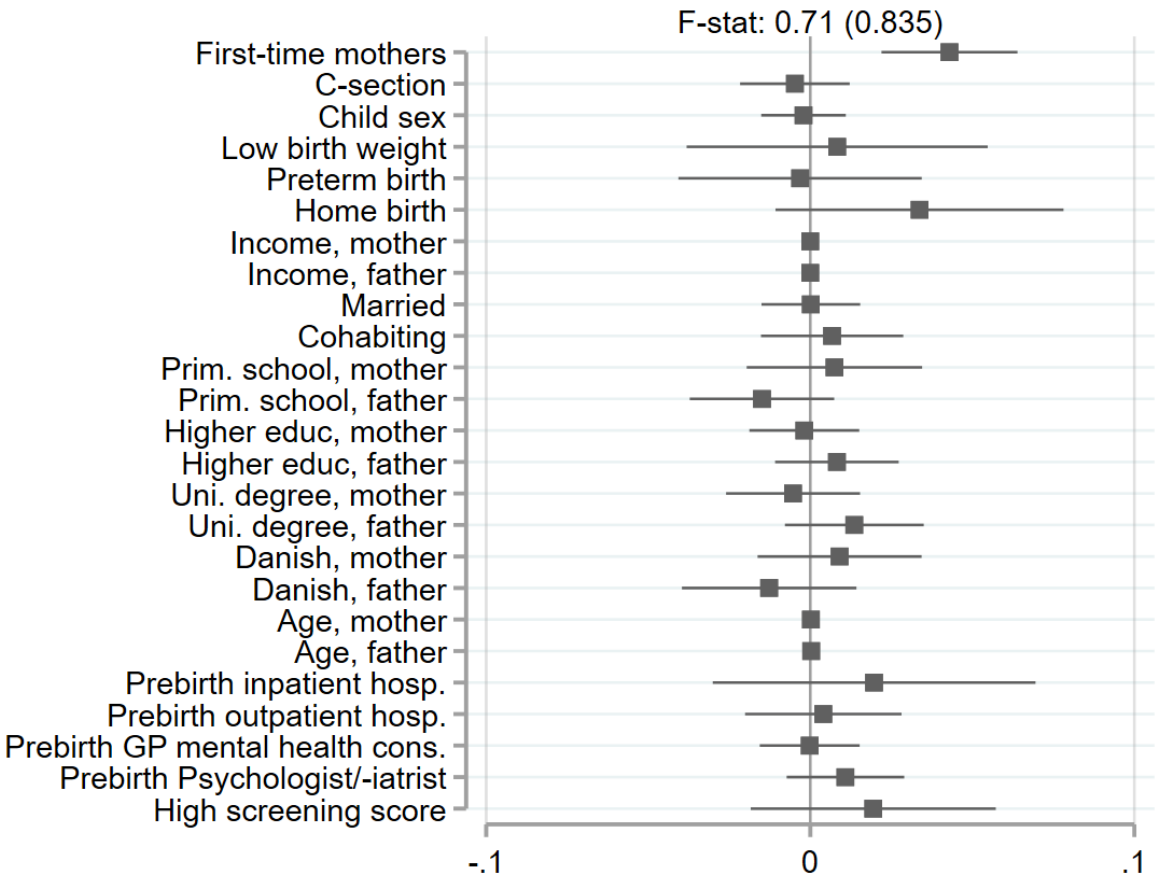


Fig. 2 Predicting Group Assignment

Notes: The figure shows coefficients and 95 percent confidence intervals from a regression with the treatment variable (indicator for exposure to a depressed peer) as outcome and all listed characteristics as control variables while including month-of-birth and municipality fixed effects. Standard errors are clustered at mother group level. The F-test tests for the joint significance of all listed predetermined characteristics other than the first-born indicator. The number in parenthesis is the associated p-value.

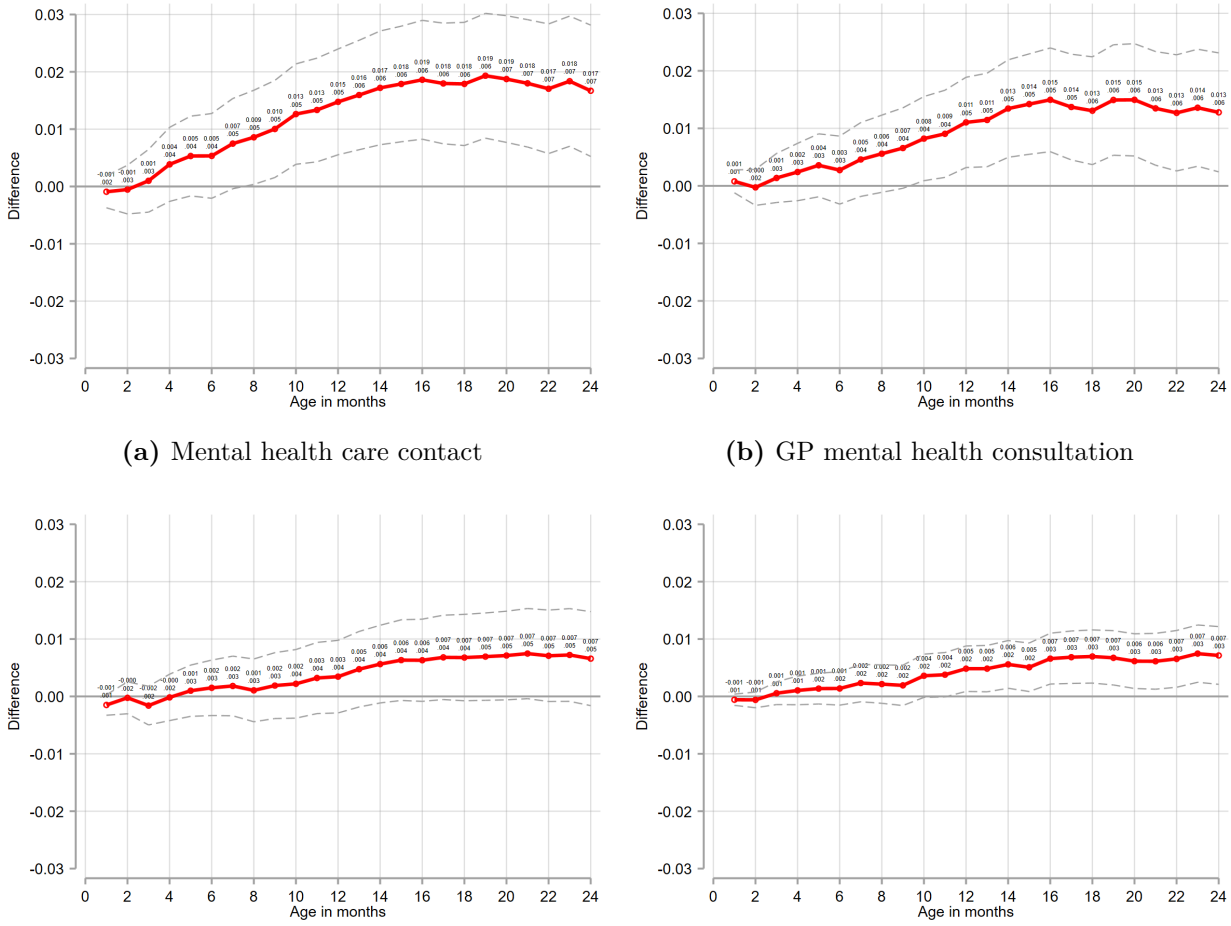
4 Results

4.1 Main Results

Figure 3 presents estimates for the impact of being assigned to at least one peer with mental health issues on maternal mental health during the first two years of the child's life. We start by measuring maternal mental health outcomes as the accumulated uptake of any mental health care at a given age (panel (a)). The subsequent panels (b)-(d) break down this indicator into separate measures for having at least one mental health consultation at the family GP, having at least one session at a psychologists or psychiatrists, and having at least one mental health hospitalization by the given age.

We find a significant impact of exposure to a depressed peer on mental health care usage: assignment to a depressed peer in a mother's group increases the probability of a mental health care contact with 1.7 percentage points (corresponding to a 11.3 percent increase evaluated at the mean for unexposed mothers) two years after childbirth. Considering dynamics, those are similar across the underlying types of mental health care: the differences in mental health of mothers across treatment and control groups diverges from four month after birth, and are significant at a 10 percent level after eight months. The divergence in mental health is not reverted, i.e. remains stable after 14 months. The overall mental health effect comes from increases in both GP mental health consultations, psychologist/psychiatrist sessions and hospitalizations. In terms of percentage differences, the increase in hospitalizations is large (33.3 percent at age two). Given the severity of this outcome, it also indicates actual mental health impacts rather than impacts on demand for care due to information channels (about treatment options).

Mothers may also need/demand more assistance from the home visiting nurses in response to being exposed to a depressed peer. Figure 4 studies usage of services within the nurse visiting program. We consider the number of accumulated universal visits (part of the standard offer), in-need visits (related to a specific issue in the family) and phone contacts between



(a) Mental health care contact

(b) GP mental health consultation

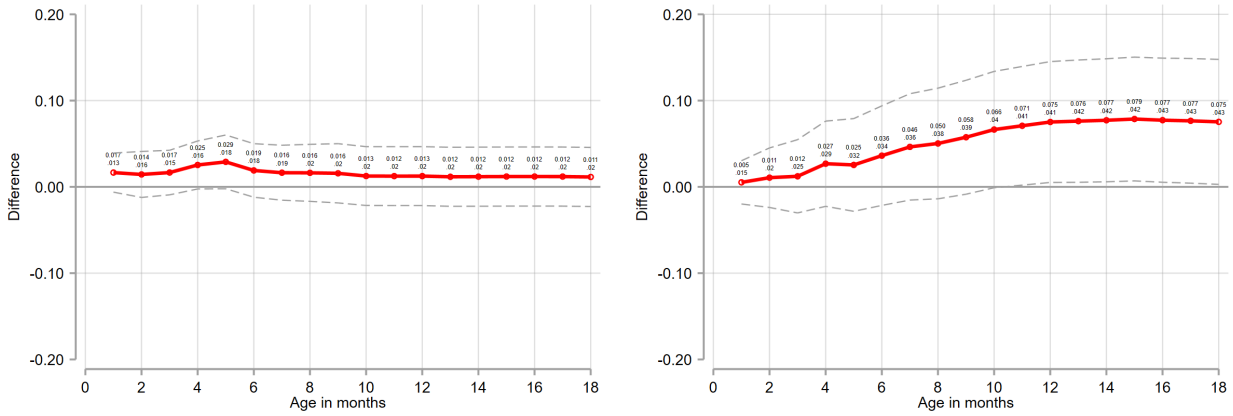
(c) Psychologist/psychiatrist consultation

(d) Mental health hospitalization

Fig. 3 Effects of Exposure to a Depressed Peer on Maternal Mental Health Care Usage

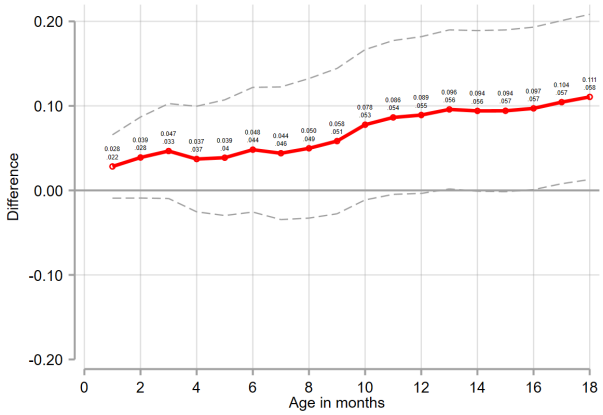
Notes: The figure shows coefficients and 90 percent confidence intervals from separate regressions. Outcomes are accumulated health care takeup measures up to a given age. We plot the estimates for the impact of the of the treatment variable (indicator for exposure to a depressed peer). The regressions include a first-born indicator and month-of-birth and municipality fixed effects. Standard errors are clustered at mother group level.

families and nurses from birth to 18 months after birth. The presence of a depressed peer increases the use of in-need nurse visits and phone contacts but not universal visits granted, as we would expect (universal visits are granted to all families irrespective of needs and thus can serve as a natural placebo outcome here). 18 months after childbirth exposed mothers have received 0.08 more in-need visits compared to mothers not exposed to a depressed peer, corresponding to a 4.5 percent increase compared to the control group mean of 1.65 in-need visits. The dynamics show that the extra in-need visits are given between six and 12 months after birth. For phone contacts we estimate a 0.11 increase in the number of phone contacts for families with a depressed peer in the mother group, corresponding to a 4.1 percent increase relative the control group mean. Thus, mothers exposed to a depressed peer require or demand more help and guidance from the home visiting nurses.



(a) Universal nurse visits

(b) In-need nurse visits



(c) Phone contacts

Fig. 4 Effects of Exposure to a Depressed Peer on Nurse Care

Notes: The figure shows coefficients as dots from separate regressions with outcomes given by the panel title measured monthly throughout the first two years after childbirth. The estimates are the coefficients in front of the treatment variable (indicator for exposure to a depressed peer). The regressions include a first-born indicator and month-of-birth and municipality fixed effects. Standard errors are clustered at mother group level. Dashed lines are 90 percent confidence intervals.

Mental health issues in mothers have the potential to impact fathers as well. Appendix Table A4 shows that there do not appear to be spillovers to fathers' mental health of peer exposure of focal mothers. In general, fathers have a lower (but not well-understood) prevalence of post-birth mental health issues.[16]

4.2 Robustness

Our main analysis is based on variation in peer mental health across groups. We assess the robustness of our conclusions to choices related to both alterations of treatment variable definition and sample restrictions. For brevity, we present results of our robustness checks for the maternal mental health care usage two years after birth.

First, we use the average screening score of all peers in a mother's group as the treatment variable (as opposed to an indicator for having at least one peer with a screening score above the validated cut-off). This definition utilizes the continuity of the screening but does not attach specific weight to a peer crossing the validated cut-off. Appendix Table A8 shows the results. While the size estimates are difficult to compare to the main results, the main patterns of the results are robust. The point estimate indicates that being in a group with an average one point higher increases the probability of having a mental health care contact at age two with 0.4 percentage points. The conclusions are also robust when we define the treatment variable as the share of peers with a screening score above the cut-off (presented in Appendix Table A9). When using these two treatment variable definitions having a psychologist/psychiatrist session becomes significant at the 10 percent level (insignificant in the main specification).

Second, we relax the requirement of complete screening coverage and also include mother groups that are only "partially screened" with the EPDS prior to group meetings. This step introduces noise in the regressions as we do not observe the mental health status of unscreened members of the group (and we therefore cannot factor in potential mental health

[16]In our sample 7.5 percent of fathers have a mental health care contact after the birth of their child compared to 15 percent of mothers.

issues of unscreened members when defining our treatment measure). Thus, in these analyses we expect to find attenuated results. We vary the required percent of screened mothers in the groups from zero to 100 percent for each of our four outcomes on maternal mental health care usage (see Appendix Figure A2). Qualitatively our conclusions are robust across the different samples but the samples that restrict to groups with 90 percent of screened mothers or above produce the largest absolute estimates but also the largest standard errors.

As complementary robustness checks, we drop any mental health contacts in the first six month after childbirth to rule out that contacts prior to peer exposure drive our results (Appendix Table A10). Moreover, we add a rich set of covariates including both birth outcomes and parental background characteristics (Appendix Table A11), drop groups with a depressed peer who is screened later than three months after birth (Appendix Table A11) to avoid the reflection problem, and drop the high scoring mothers themselves (Appendix Table A13). As shown in the Appendix material, the estimates are relatively stable to these modifications. As an example, the estimates increase in size when we drop contacts in the first six months after childbirth, strengthening our trust in our main conclusions since the effects on mental health are strongest when the groups most likely have interacted.

In a final robustness test we perform a random placebo exercise. We randomize group composition within birth month and municipality. Thus for each pool of children born in the same month and in the same municipality, we randomize mother group assignment. We repeat this exercise 1000 times and for each iteration we re-estimate the effect of exposure to a depressed peer. Appendix Figure A4 shows the resulting distribution of placebo peer effects along with the true effect (red vertical lines) for our four main mental health care outcomes. For the outcomes with significant effects in our main analyses the true effects are extreme observations in the distribution of placebo effects. This is in line with our main conclusions and shows that the true group composition is crucial to generate our estimated impacts.

5 Mechanism: Social Contagion or Transfer of Information?

Our finding that mothers exposed to a peer with mental health issues also have a higher tendency to consult a GP or have a hospital admission in relation to mental health may be due to two main causes: (1) social contagion of mental health (exposed mothers have worse mental health due to social interactions) and/or (2) transfer of information related to mental health treatment opportunities.

Our main finding of a significant peer effect on mental health hospitalizations–a rather severe and rare outcome–indicates actual social contagion of mental health rather than stable mental health but an increased probability of seeking a treatment. Transfer of information as main mechanism would likely have resulted in fewer hospitalizations as mothers would likely be better off as they would seek (milder) treatments earlier. To further examine the potential underlying mechanisms, we study complementary family outcomes and perform a range of heterogeneity analyses.

5.1 Return to Labor Market and Child Health

We consider mothers' parental leave, employment and sick leave as relevant labor market outcomes responsive to mental health status. If social contagion is the main mechanisms, we expect exposure to a depressed peer to have a negative impact on transitions back to employment after parental leave and a positive impact on transitions to sick leave. For parental leave duration we think that impacts could go both ways.

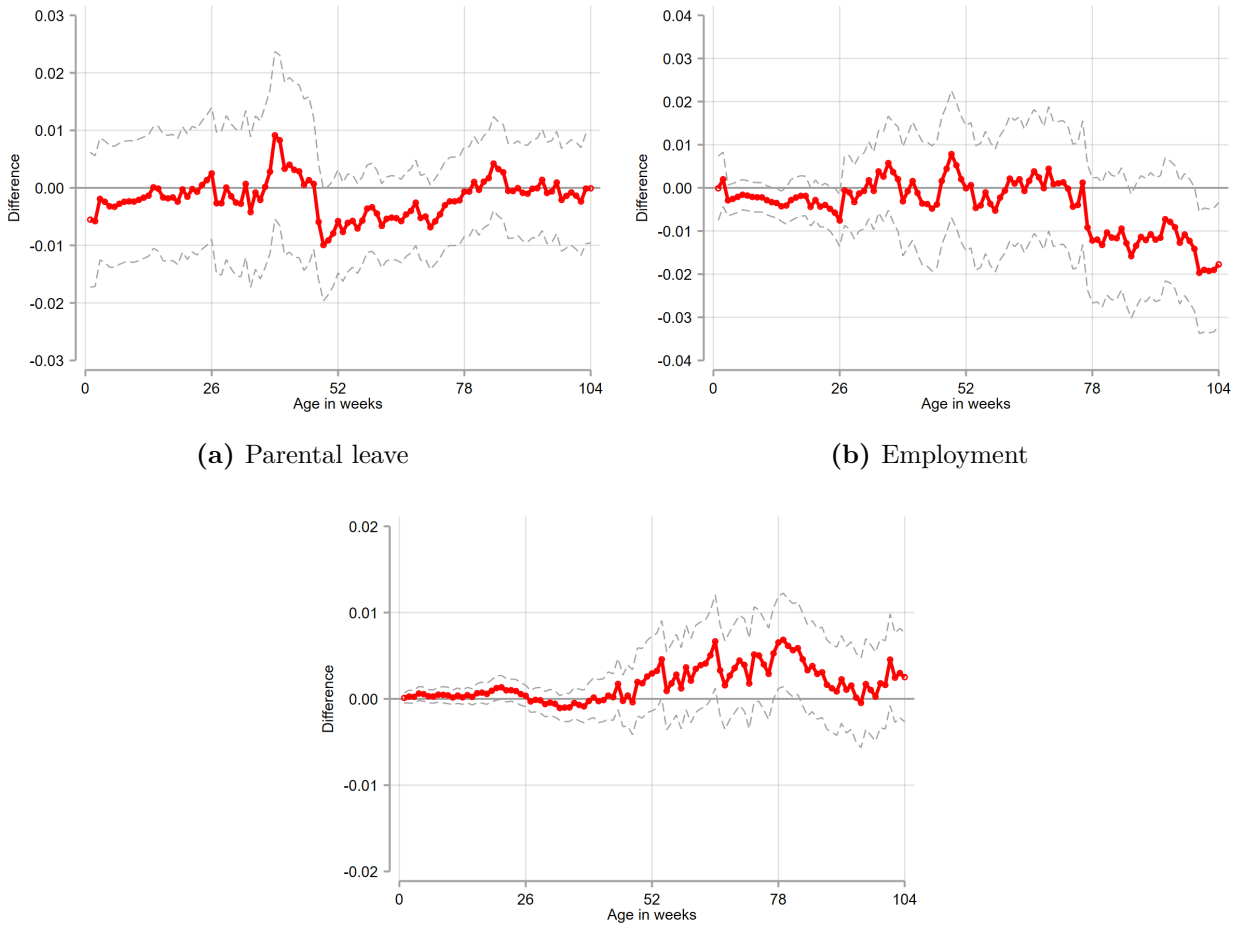
Figure 5 shows our results for weekly (status) indicators for being in each of the three labor market states. For parental leave, we do not find any clear or significant indications that having a depressed peer have an impact during the first two years after birth. Panel (b) shows a negative impact on employment of exposure to a depressed peer in the period 1.5-2 years after birth. Specifically, we estimate a one to two percentage points difference in employment

probability in that period corresponding to a three percent decrease in employment two years after childbirth evaluated at the control group mean (66 percent). In panel (c) the estimates on sick leave indicate that having a depressed peer increases the probability of being on sick leave with 0.5 percentage points around 1-1.5 years after birth. Since three percent of mothers are on sick leave 1.5 years after birth this impact corresponds to 17 percent impact. The impact on sick leave coincides with the increase in mental health care usage shown in Figure 3 while the negative impact on employment suggests long-term effects of exposure to a depressed peer. These labor market effects strongly suggest social contagion of mental health as the main mechanism.

Appendix Table A5 shows that having a depressed peer in the mother group increases the probability of a child hospitalization in the second year of life with 0.02 percentage point (5.4 percent relative to the control group mean). This result supports an underlying maternal mental health effect (i.e., social contagion of mental health) because we would not expect impacts on child health if the information channel was the main mechanism. Appendix Table A6 turns to the non-mental health of the mothers measured in a similar manner as for the children. Here we find no differences in usage from being in mother group with a depressed peer. Appendix Table A7 considers cohabitation and fertility. Here we find a 0.013 percentage point (corresponding to 4.5 percent) lower probability of having had another child three years after birth of the focal child of depressed peer exposure but no effects on the likelihood of cohabitation. The negative fertility response also pulls mostly in the direction of social contagion of mental health.

5.2 Heterogeneity

To further assess the relevance of both social contagion and the information channel, we perform a series of heterogeneity tests across different characteristics of the exposed mothers. The idea is to divide parents based on their experience in terms of mental health issues and care. If mothers who are likely to have knowledge of the mental health care sector also



(a) Parental leave

(b) Employment

(c) Sick leave

Fig. 5 Effects of Exposure to a Depressed Peer on Parental Leave, Employment and Sick Leave of Mothers; Weekly Status Measures

Notes: The figure shows coefficients as dots from separate regressions with outcomes given by the panel title measured weekly throughout the first two years after childbirth. The estimates are the coefficients estimated for our treatment variable (indicator for exposure to a depressed peer). The regressions include a first-born indicator and month-of-birth and municipality fixed effects. Standard errors are clustered at mother group level. Dashed lines are 90 percent confidence intervals.

increase usage from exposure to a depressed peer the channel is most likely not a transfer of information (as they already possess it) but rather contagion of mental health.

Figure 6 considers heterogeneity across mothers' experience in terms of both parenting and mental health issues. First, we split the sample into two groups determined by mothers having a personal history with the mental health care sector (at least one contact to GPs, psychiatrists/psychologist or hospitals in relation to mental health from 2005 to the year of their childbirth). Here we only see a significant peer effect (2.2 percentage point increase in the probability of having a mental health care contact at age two) for mothers with previous contacts. Although the difference in effects is not significant, this finding suggests the main mechanism is social contagion of mental health as opposed to information as experienced mothers would already possess some information from previous interactions with the mental health care sector.

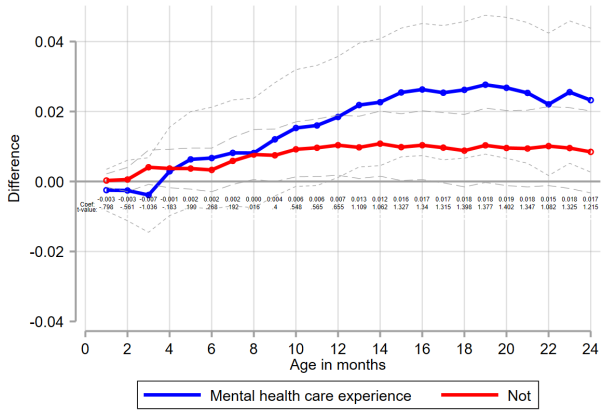
Second, we split our sample into first-time parents and mothers with previous experience from parenthood (having children already). Here we observe very similar effects from having a depressed peer. If social contagion of information was the main mechanism we would have expected first-time parents to drive the result. Third, we split our sample based on mothers age at birth. We find significant differences in the effects towards the end of the second year of life indicating that only mothers above 25 years at the time of childbirth have more mental health care contacts when exposed to a depressed peer. In line with the other results, this finding indicates social contagion of mental as more experienced mothers appear to be those affected. Finally, we split by measures of mental health of the exposed mothers. We divide the sample of exposed mothers based on their own screening score: a group with relatively better mental health (score below five) and slightly worse mental health (score between five and 10). Those with scores below five are not affected by the presence of a depressed peer as those mothers with the worse mental health upon group formation drive the results (the difference in mental health care uptake is significantly different from each other). This result suggests that mothers with risk factors (among them own mental health challenges) are

impacted more by the presence of a depressed peer.

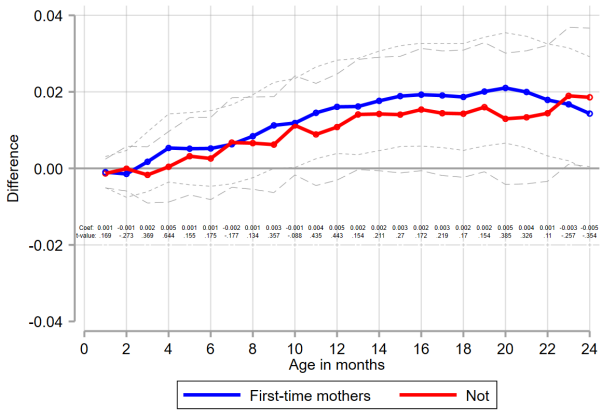
Figure 7 turns to heterogeneity across groups rather than individual characteristics. In panel (a), we split groups by the presence of a trained medical doctor, psychologist or nurse. Having a peer with this type of health expertise could impact the influence of the depressed peer in two ways. First, a medically-trained peer could counter the detrimental effects of having a depressed peer.[17] Second, doctors, psychologist and nurses might provide information (similar to that of a depressed peer) and again reduce the impact of having a depressed peer. Thus both channels contribute similarly to the estimate and do not allow us to determine the mechanism underlining the peer effect. The results show that the effect of having a depressed peers is driven by exposed mothers in groups with no medical or psychological expertise. Some municipalities create groups for first-time mothers only. In panel (b) we estimate the peer effect for groups consisting only of first-time mothers and mixed parity and only higher parity groups separately. We only estimate a significant effect for exposed mothers in mixed and higher parity groups and not in groups with first-time mothers. Thus the most experienced groups drive the overall effects which suggests social contagion of mental health as the most probable mechanism.

In panel (c) we split groups by SES concordance. Specifically, we use maternal university degree prior to birth as a measure of SES and split mother groups into two categories: 1) groups where all or none have university degrees (SES concordant groups) and 2) groups with mixed SES status. At age two the overall effect is driven by mothers in both types of groups. However the timing of uptake differs across SES concordance. In SES concordant groups the effect on mental health care materializes in year one while in mixed SES groups the effect mothers exposed to a depressed peer leads to increased mental health care contact in year two. In panel (d), we split by whether at least 50 percent of mother group members have had a mental health care contact prior to birth. In the groups with less mental health care history, we do not observe any effect on mental health care uptake from having a depressed

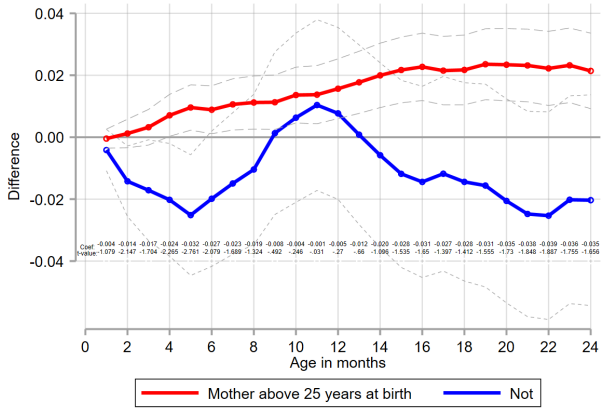
[17] This channel (the positive impact on health and health care uptake) within family networks have recently been shown in Chen et al. (2022)



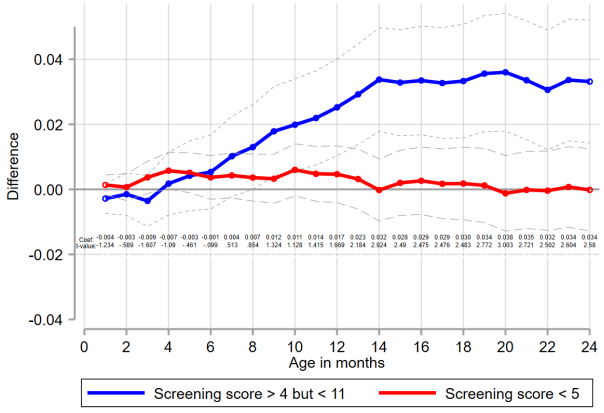
(a) Prior contact with mental health care system



(b) First-time mother



(c) Age of mother



(d) Mental health of exposed mothers

Fig. 6 Heterogeneity: Experience of mothers in terms parenthood, mental health issues and in general

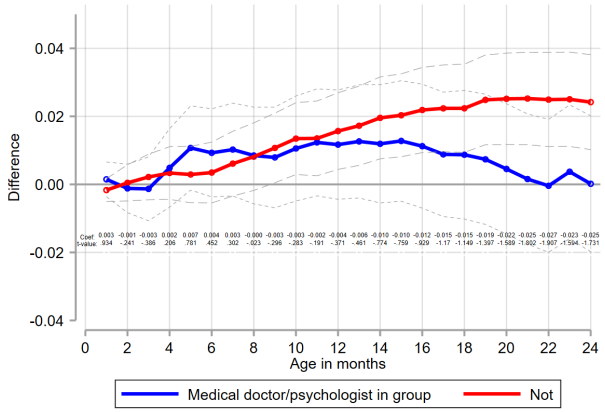
Notes: The figure shows coefficients as dots from separate regressions with outcomes given by the panel title measured monthly throughout the first two years after childbirth. The estimates are the coefficients in front of the treatment variable (indicator for exposure to a depressed peer). The regressions include a first-born indicator and month-of-birth and municipality fixed effects. Standard errors are clustered at mother group level. Shaded lines are 90 percent confidence intervals. The numbers in the graphs are coefficients and t-statistics from the interaction model in equation (3) and tests for whether the subgroup differences are significantly different from each other.

peer. As the groups, where many members have a mental health care history already (and thus possess information related to mental health care), drive the overall effects transfer of information is likely not the main mechanisms.

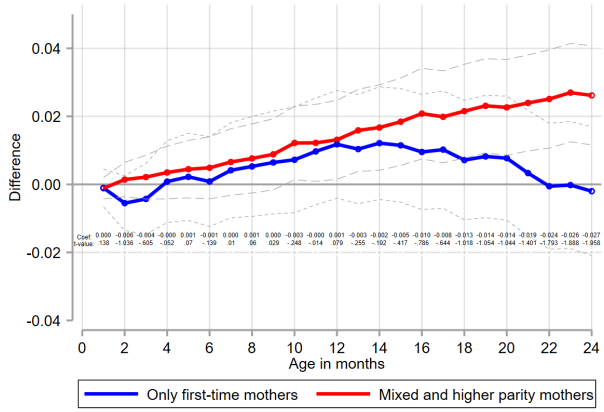
As a final analysis we look at treatment intensity in terms of the screening score of the depressed peer. Social contagion of mental health would imply that more depressed peers (higher screening score of the depressed peer) lead to higher impact on mental health care usage for exposed mothers while the information channel could potentially imply the reverse as the most depressed peers could be less inclined to share due to mental exhaustion. We test this in a regression where we augment equation (2) with the (mean) screening score of the depressed peer(s). Presented in Appendix Table A14, the results show that the severity of the depressed peers mental health issues matters for the impact of depressed peer exposure. Interpreting the estimated coefficient, we find that an increase of one in the screening score of the depressed peer increases the impact on the probability of mental health care contact at age two with 0.005 percentage points. For instance, going from having a depressed peer with a screening score at 11 to 20 increases the impact from 0.003 to 0.048 percentage points.[18]

The presented evidence is mostly consistent with social contagion of mental health as the main mechanism. We find that the presence of a depressed peer increases sick leave and child hospitalizations in the second year of life while decreasing the probability of employment. If information transfer and unaffected (possibly improved) maternal mental health was mechanism, worse child health and labor market outcomes would not be expected. Further, we found experienced mothers to be affected relatively more compared to lesser experienced mothers. Assuming that experienced mothers have more information on mental health care, the fact that they drive the overall effect suggests social contagion of mental health. Additionally, we considered group composition heterogeneity. Here we show that groups with more mental health care history (and consequently more information) drive the overall effect

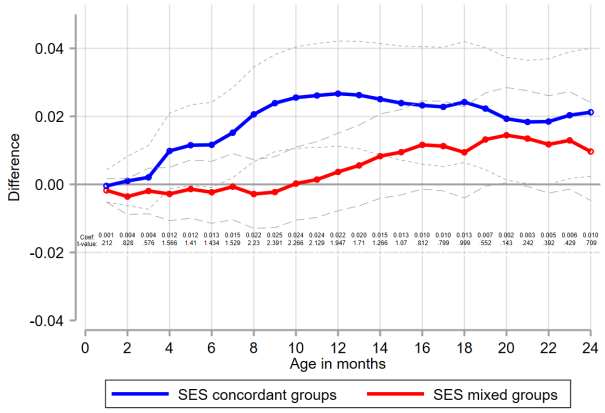
[18]We have also considered the differential impact of having two or more depressed peers compared to one depressed peer. We do not find evidence of differential impacts. One obstacle in this analysis is that only 3.8 percent (N=654) of mothers in the sample have more than one depressed peer compared to 23 percent with one depressed peer.



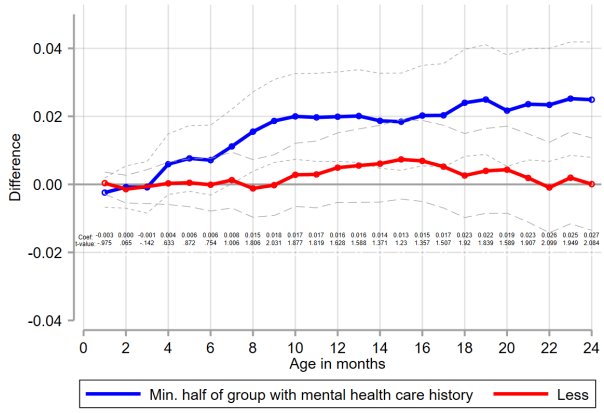
(a) Doctor/psychologist/nurse in group



(b) Only first-time mothers in group



(c) SES concordance



(d) Mental health care history in group

Fig. 7 Heterogeneity: Group composition

Notes: The figure shows coefficients as dots from separate regressions with outcomes given by the panel title measured monthly throughout the first two years after childbirth. The estimates are the coefficients in front of the treatment variable (indicator for exposure to a depressed peer). The regressions include a first-born indicator and month-of-birth and municipality fixed effects. Standard errors are clustered at mother group level. Shaded lines are 90 percent confidence intervals. The numbers in the graphs are coefficients and t-statistics from the interaction model in equation (3) and tests for whether the subgroup differences are significantly different from each other.

in line with social contagion of mental health. Lastly, the effect increases in the severity of the depressed peers' mental health issues.

6 Conclusion

The postpartum period is a crucial period, not only for the health of the newborn child but also maternal health and family outcomes more broadly. Especially the mental health of new mothers is sensitive to the dramatic and irreversible life changes associated with child birth. In this paper, we study how the mental health of peers in early parenthood affect maternal mental health. We combine data from universally offered mental health screenings and mother groups with administrative data on health care usage outcomes to estimate the effects of having a depressed peer in the assigned peer group.

We find that a depressed peer increases mental health contacts in the years following childbirth. Most strikingly, exposed mothers are more than 30 percent more likely hospitalized the first two years after childbirth on mental health grounds. Dynamically, the impact is centered eight to 16 months after birth. Mental health consultations at the GP start to increase six months after childbirth while the impact on hospitalizations occur later at 10 months after birth. We perform a series of robustness checks varying both the sample inclusion criteria, treatment definition, measurement of outcomes, and regression specifications. Our results and conclusions remain robust throughout.

Two mechanisms can explain the impact of a depressed peer on maternal mental health care usage: social contagion of mental health or transfer of information related to treatment opportunities. We explore the role of these two mechanisms by studying labor market and child health outcomes and heterogeneity across characteristics of the exposed mother and group composition. As we find evidence that a depressed peer reduces employment and increases sick leave, as well as that mothers and groups most likely to already possess information on mental health care drive the results, we conclude that social contagion of mental

health likely constitutes a main mechanism.

References

Aitken, Z., C. C. Garrett, B. Hewitt, L. Keogh, J. S. Hocking, and A. M. Kavanagh (2015). The maternal health outcomes of paid maternity leave: A systematic review. *Social Science & Medicine 130*, 32–41.

Almond, D., J. Currie, and V. Duque (2018). Childhood circumstances and adult outcomes: Act ii. *Journal of Economic Literature 56*(4), 1360–1446.

Banerjee, S., P. Chatterji, and K. Lahiri (2017). Effects of psychiatric disorders on labor market outcomes: a latent variable approach using multiple clinical indicators. *Health economics 26*(2), 184–205.

Baranov, V., S. Bhalotra, P. Biroli, and J. Maselko (2020). Maternal depression, women's empowerment, and parental investment: evidence from a randomized controlled trial. *American economic review 110*(3), 824–59.

Bramoullé, Y., H. Djebbari, and B. Fortin (2020). Peer effects in networks: A survey. *Annual Review of Economics 12*, 603–629.

Cavapozzi, D., M. Francesconi, and C. Nicoletti (2021). The impact of gender role norms on mothers' labor supply. *Journal of Economic Behavior & Organization 186*, 113–134.

Chen, Y., P. Persson, and M. Polyakova (2022). The roots of health inequality and the value of intrafamily expertise. *American Economic Journal: Applied Economics 14*(3), 185–223.

Conti, R. M., E. R. Berndt, and R. G. Frank (2006). Early retirement and public disability insurance applications: Exploring the impact of depression.

Cortés, P., G. Koşar, J. Pan, and B. Zafar (2022, October). Should mothers work? how perceptions of the social norm affect individual attitudes toward work in the u.s. Working Paper 30606, National Bureau of Economic Research.

Cox, J. and J. Holden (2003). *Perinatal mental health: A guide to the Edinburgh Postnatal Depression Scale (EPDS).* Royal College of Psychiatrists.

Dahl, G. B., K. V. Løken, and M. Mogstad (2014). Peer effects in program participation. *American Economic Review 104*(7), 2049–74.

Eisenberg, D., E. Golberstein, J. L. Whitlock, and M. F. Downs (2013). Social contagion of mental health: evidence from college roommates. *Health Economics 22*(8), 965–986.

Fowler, J. H. and N. A. Christakis (2008). Dynamic spread of happiness in a large social network: longitudinal analysis over 20 years in the framingham heart study. *Bmj 337*.

Frijters, P., A. Islam, and D. Pakrashi (2019). Heterogeneity in peer effects in random dormitory assignment in a developing country. *Journal of Economic Behavior & Organization 163*, 117–134.

Getik, D. and A. N. Meier (2022). Peer gender and mental health. *Journal of Economic Behavior & Organization 197*, 643–659.

Giulietti, C., M. Vlassopoulos, and Y. Zenou (2022). Peers, gender, and long-term depression. *European Economic Review 144*, 104084.

Golberstein, E., D. Eisenberg, and M. F. Downs (2016). Spillover effects in health service use: Evidence from mental health care using first-year college housing assignments. *Health economics 25*(1), 40–55.

Halligan, S. L., L. Murray, C. Martins, and P. J. Cooper (2007). Maternal depression and psychiatric outcomes in adolescent offspring: a 13-year longitudinal study. *Journal of affective disorders 97*(1-3), 145–154.

Hasager, L. and M. Jørgensen (2021). Sick of your poor neighborhood?

Herbst, D. and A. Mas (2015). Peer effects on worker output in the laboratory generalize to the field. *Science 350*(6260), 545–549.

Hill, E. M., F. E. Griffiths, and T. House (2015). Spreading of healthy mood in adolescent social networks. *Proceedings of the Royal Society B: Biological Sciences 282*(1813), 20151180.

Hirani, J. C., H. H. Sievertsen, and M. Wüst (2022). Beyond treatment exposure–the impact of the timing of early interventions on child and maternal health. *Journal of Human Resources*, 0421–11583R2.

Hirani, J. C. and M. Wüst (2022). Nurses and infant vaccination coverage. *Journal of Economic Behavior & Organization 196*, 402–428.

Joiner Jr, T. E. and J. Katz (1999). Contagion of depressive symptoms and mood: Meta-analytic review and explanations from cognitive, behavioral, and interpersonal viewpoints. *Clinical Psychology: Science and Practice 6*(2), 149.

Kramer, A. D., J. E. Guillory, and J. T. Hancock (2014). Experimental evidence of massive-scale emotional contagion through social networks. *Proceedings of the National Academy of Sciences 111*(24), 8788–8790.

Lavy, V. and A. Schlosser (2011). Mechanisms and impacts of gender peer effects at school. *American Economic Journal: Applied Economics 3*(2), 1–33.

Levy, D. A. and P. R. Nail (1993). Contagion: a theoretical and empirical review and reconceptualization. *Genetic, social, and general psychology monographs*.

Manski, C. F. (1993). Identification of endogenous social effects: The reflection problem. *The review of economic studies 60*(3), 531–542.

Maselko, J., A. K. Hagaman, L. M. Bates, S. Bhalotra, P. Biroli, J. A. Gallis, K. O'Donnell, S. Sikander, E. L. Turner, and A. Rahman (2019). Father involvement in the first year of life: Associations with maternal mental health and child development outcomes in rural pakistan. *Social Science & Medicine 237*, 112421.

Munk-Olsen, T. and E. Agerbo (2015). Does childbirth cause psychiatric disorders? a population-based study paralleling a natural experiment. *Epidemiology (Cambridge, Mass.) 26*(1), 79.

Nicoletti, C., K. G. Salvanes, and E. Tominey (2018). The family peer effect on mothers' labor supply. *American Economic Journal: Applied Economics 10*(3), 206–34.

O'hara, M. W. and J. E. McCabe (2013). Postpartum depression: current status and future directions. *Annual review of clinical psychology 9*, 379–407.

Peng, L., C. D. Meyerhoefer, and S. H. Zuvekas (2016). The short-term effect of depressive symptoms on labor market outcomes. *Health Economics 25*(10), 1223–1238.

Persson, P. and M. Rossin-Slater (2018). Family ruptures, stress, and the mental health of the next generation. *American economic review 108*(4-5), 1214–52.

Sacerdote, B. (2001). Peer effects with random assignment: Results for dartmouth roommates. *The Quarterly journal of economics 116*(2), 681–704.

Saxbe, D., M. Rossin-Slater, and D. Goldenberg (2018). The transition to parenthood as a critical window for adult health. *American Psychologist 73*(9), 1190.

Smith-Nielsen, J., S. Matthey, T. Lange, and M. S. Væver (2018). Validation of the edinburgh postnatal depression scale against both dsm-5 and icd-10 diagnostic criteria for depression. *BMC psychiatry 18*(1), 1–12.

Stein, A., L.-E. Malmberg, K. Sylva, J. Barnes, P. Leach, and F. team (2008). The influence of maternal depression, caregiving, and socioeconomic status in the post-natal year on children's language development. *Child: care, health and development 34*(5), 603–612.

von Hinke, S., N. Rice, and E. Tominey (2022). Mental health around pregnancy and child development from early childhood to adolescence. *Labour Economics 78*, 102245.

Wang, B., R. G. Frank, and S. A. Glied (2022, December). Lasting scars: The impact of depression in early adulthood on subsequent labor market outcomes. Working Paper 30776, National Bureau of Economic Research.

Welteke, C. and K. Wrohlich (2019). Peer effects in parental leave decisions. *Labour Economics 57*, 146–163.

Wüst, M. (2022). Universal early-life health policies in the nordic countries. *Journal of Economic Perspectives 36*(2), 175–98.

Wüst, M., J. L.-J. Hirani, A. T. Hansen, and L. H. Andersen (2020). Sundhedsplejerskeindsatsen i danmark: Perspektiver for anvendelse af kommunale journaldata i forskning om sundhedspleje. Technical report, The Danish Center for Social Science Research – VIVE.

Zhang, A. (2019). Peer effects on mental health: Evidence from random assignment into classrooms. *Available at SSRN 3685374*.

A Appendix - For Online Publication

A.1 EPDS questionnaire

In the last seven days:

1. I have been able to laugh and see the funny side of things

   ☐ As much as I always could

   ☐ Not quite so much now

   ☐ Definitely not so much now

   ☐ Not at all

2. I have looked forward with enjoyment to things

   ☐ As much as I ever did

   ☐ Rather less than I used to

   ☐ Definitely less than I used to

   ☐ Hardly at all

3. I have blamed myself unnecessarily when things went wrong

   ☐ Yes, most of the time

   ☐ Yes, some of the time

   ☐ Not very often

   ☐ No, never

4. I have been anxious or worried for no good reason

   ☐ No, not at all

☐ Hardly ever

☐ Yes, sometimes

☐ Yes, very often

5. I have felt scared or panicky for no very good reason

☐ Yes, quite a lot

☐ Yes, sometimes

☐ No, not much

☐ No, not at all

6. Things have been getting on top of me

☐ Yes, most of the time I haven't been able to cope at all

☐ Yes, sometimes I haven't been coping as well as usual

☐ No, most of the time I have coped quite well

☐ No, I have been coping as well as ever

7. I have been so unhappy that I have had difficulty sleeping

☐ Yes, most of the time

☐ Yes, sometimes

☐ Not very often

☐ No, not at all

8. I have felt sad or miserable

☐ Yes, most of the time

☐ Yes, quite often

☐ Not very often

☐ No, not at all

9. I have been so unhappy that I have been crying

☐ Yes, most of the time

☐ Yes, quite often

☐ Only occasionally

☐ No, never

10. The thought of harming myself has occurred to me

☐ Yes, quite often

☐ Sometimes

☐ Hardly ever

☐ Never

Tab. A1 Summary Statistics: Comparison of Population and Final Analysis Sample

	— Means —	
	Population excl. final sample	Final sample
	(1)	(2)
First-time mothers	0.45	0.56
C-section	0.22	0.19
Child sex	0.49	0.49
Low birth weight	0.05	0.03
Preterm birth	0.07	0.04
Home birth	0.02	0.03
Income, mother	218924	256399
Income, father	323265	349419
Married	0.42	0.37
Cohabiting	0.80	0.84
Prim. school, mother	0.15	0.09
Prim. school, father	0.16	0.11
Higher educ, mother	0.26	0.32
Higher educ, father	0.16	0.19
Uni. degree, mother	0.21	0.25
Uni. degree, father	0.17	0.20
Danish, mother	0.78	0.92
Danish, father	0.73	0.86
Age, mother	30.54	30.81
Age, father	30.30	30.80
Inpatient mental health hosp.	0.03	0.02
Outpatient mental health hosp.	0.12	0.11
GP mental health cons. prior to birth	0.34	0.35
Psychologist/psychiatrist prior to birth	0.20	0.22
Observations	68858	17187

Notes: The table shows means of prebirth characteristics for children in the population in the relevant municipalities and birth cohorts excluding children in the analysis sample compared to the analysis sample. Birth characteristics (top six rows) are measured at birth while remaining parental characteristics are measured in the year prior to birth.

Tab. A2 Summary Statistics: Comparison of Families by Group Participation

	— Means —	
	Not in group (1)	In group (2)
First-time mothers	0.44	0.49
C-section	0.22	0.21
Child sex	0.49	0.49
Low birth weight	0.06	0.04
Preterm birth	0.07	0.05
Home birth	0.02	0.03
Income, mother	190487	256285
Income, father	296939	354728
Married	0.42	0.40
Cohabiting	0.77	0.83
Prim. school, mother	0.21	0.09
Prim. school, father	0.19	0.11
Higher educ, mother	0.22	0.31
Higher educ, father	0.13	0.19
Uni. degree, mother	0.17	0.26
Uni. degree, father	0.14	0.21
Danish, mother	0.72	0.89
Danish, father	0.66	0.84
Age, mother	29.96	31.13
Age, father	29.76	30.93
Inpatient mental health hosp.	0.03	0.02
Outpatient mental health hosp.	0.12	0.11
GP mental health cons. prior to birth	0.34	0.34
Psychologist/psychiatrist prior to birth	0.19	0.21
Observations	39069	46976

Notes: The table shows means of prebirth characteristics for children in mother groups where not all members are screened and in groups where all members are screened. Birth characteristics (top six rows) are measured at birth while remaining parental characteristics are measured in the year prior to birth.

Tab. A3 Summary Statistics: Comparison of Groups where all Mothers not are Screened and Groups where all Mothers are Screened

	— Means —	
	In group not all screened	In group all screened
	(1)	(2)
First-time mothers	0.45	0.56
C-section	0.22	0.19
Child sex	0.49	0.49
Low birth weight	0.05	0.03
Preterm birth	0.06	0.04
Home birth	0.03	0.03
Income, mother	256.76	256.40
Income, father	358.82	349.42
Married	0.42	0.37
Cohabiting	0.83	0.84
Prim. school, mother	0.09	0.09
Prim. school, father	0.10	0.11
Higher educ, mother	0.30	0.32
Higher educ, father	0.19	0.19
Uni. degree, mother	0.27	0.25
Uni. degree, father	0.21	0.20
Danish, mother	0.87	0.92
Danish, father	0.82	0.86
Age, mother	31.33	30.81
Age, father	31.01	30.80
Inpatient mental health hosp.	0.02	0.02
Outpatient mental health hosp.	0.11	0.11
GP mental health cons. prior to birth	0.34	0.35
Psychologist/psychiatrist prior to birth	0.21	0.22
Observations	28571	17187

Notes: The table shows means of prebirth characteristics for children in mother groups where not all members are screened and in groups where all members are screened. Birth characteristics (top six rows) are measured at birth while remaining parental characteristics are measured in the year prior to birth.

Tab. A4 Effects of Exposure to a Peer with Mental Health Issues on Paternal Mental Health Care Usage

	(1) Mental health contact age 2	(2) GP mental health consultation age 2	(3) Psychologist psychiatrist age 2	(4) Mental health hospitalization age 2
Partner has depressed peer	0.002	-0.001	0.002	0.000
	(0.005)	(0.004)	(0.003)	(0.002)
Control group mean	0.075	0.062	0.030	0.013
Obs.	17187	17187	17187	17187

Notes: See notes to Table A5. Standard errors are clustered at mother group level. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level.

Tab. A5 Effects of Exposure to a Depressed Peer on Child GP Care and Hospitalizations

	GP expenses		Hospitalizations	
	6-12 months	2nd year	6-12 months	2nd year
	(1)	(2)	(3)	(4)
Depressed peer	22.736	22.538	0.006	0.020**
	(23.213)	(30.404)	(0.008)	(0.010)
Control group mean	1098.434	2552.768	0.258	0.372
Obs.	17187	17187	17187	13045

Notes: Each column shows coefficient from separate regressions with outcomes given by the column header. For second-year hospitalizations we drop the 2017 cohort as we only have hospital admission data until 2018. The estimates are the coefficients in front of the treatment variable (indicator for exposure to a depressed peer). The regressions include a first-born indicator and month-of-birth and municipality fixed effects. Standard errors are clustered at mother group level. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level.

Tab. A6 Effects of Exposure to a Depressed Peer on Maternal GP Care and Hospitalizations

	GP expenses		Hospitalizations	
	6-12 months	2nd year	6-12 months	2nd year
	(1)	(2)	(3)	(4)
Depressed peer	11.513	34.233	0.003	-0.008
	(44.490)	(49.607)	(0.007)	(0.010)
Control group mean	2781.982	2505.190	0.212	0.395
Obs.	17187	17187	17187	13045

Notes: See notes to Table A5. Standard errors are clustered at mother group level. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level.

Tab. A7 Effects of Exposure to a Peer with Mental Health Issues on Fertility and Cohabitation

	(1)	(2)	(3)	(4)
	— 2 years after —		— 3 years after —	
	Gave birth	Cohabiting	Gave birth	Cohabiting
Depressed peer	0.002	0.001	-0.013*	-0.001
	(0.005)	(0.006)	(0.007)	(0.007)
Control group mean	0.088	0.856	0.290	0.834
Obs.	17187	17187	17187	17187

Notes: See notes to Table A5. Standard errors are clustered at mother group level. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level.

Tab. A8 Robustness check: Average Screening Score of Peers as Treatment Variable

	(1) Mental health contact age 2	(2) GP mental health consultation age 2	(3) Psychologist psychiatrist age 2	(4) Mental health hospitalization age 2
Average screening score of peers	0.004***	0.002*	0.002*	0.001**
	(0.002)	(0.001)	(0.001)	(0.001)
Control group mean	0.167	0.129	0.070	0.023
Obs.	17187	17187	17187	17187

Notes: See notes to Table A5. Compared to the main specification we use the average screening score of peers as treatment variable instead of an indicator for a peer having a score above 10 (being in the clinically validated depression region of the screening device). Standard errors are clustered at mother group level. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level.

Tab. A9 Robustness check: Share of Peers with High Screening Scores as Treatment Variable

	(1) Mental health contact age 2	(2) GP mental health consultation age 2	(3) Psychologist psychiatrist age 2	(4) Mental health hospitalization age 2
Depressed peer	0.050**	0.034*	0.029*	0.017*
	(0.021)	(0.020)	(0.016)	(0.010)
Control group mean	0.163	0.126	0.069	0.023
Obs.	17187	17187	17187	17187

Notes: See notes to Table A5. Compared to the main specification we the share of peers with scores above as treatment variable instead of an indicator for a peer having a score above 10 (being in the clinically validated depression region of the screening device). Standard errors are clustered at mother group level. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level.

Tab. A10 Robustness check: Drop Contacts in the First Six Months after Birth

	(1) Mental health contact age 2	(2) GP mental health consultation age 2	(3) Psychologist psychiatrist age 2	(4) Mental health hospitalization age 2
Depressed peer	0.018***	0.013**	0.008	0.009***
	(0.007)	(0.006)	(0.005)	(0.003)
Control group mean	0.161	0.123	0.068	0.021
Obs.	17187	17187	17187	17187

Notes: See notes to Table A5. Compared to the main specification we accumulate having a mental health contact in the first two years after childbirth excluding any contact in the first six months. Standard errors are clustered at mother group level. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level.

Tab. A11 Robustness check: Control for predetermined birth and parental characteristics

	(1) Mental health contact age 2	(2) GP mental health consultation age 2	(3) Psychologist psychiatrist age 2	(4) Mental health hospitalization age 2
Depressed peer	0.013**	0.011*	0.005	0.006**
	(0.006)	(0.006)	(0.005)	(0.003)
Control group mean	0.167	0.129	0.070	0.023
Obs.	17187	17187	17187	17187

Notes: See notes to Table A5. Compared to the main specification we control for a set of predetermined birth and parental characteristics. Standard errors are clustered at mother group level. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level.

Tab. A12 Robustness check: Drop Groups where the Depressed Peer is Screened Later than Three Months after Birth

	(1) Mental health contact age 2	(2) GP mental health consultation age 2	(3) Psychologist psychiatrist age 2	(4) Mental health hospitalization age 2
Depressed peer	0.016**	0.012**	0.007	0.006**
	(0.007)	(0.006)	(0.005)	(0.003)
Control group mean	0.167	0.129	0.070	0.023
Obs.	16905	16905	16905	16905

Notes: See notes to Table A5. Compared to the main specification we drop groups where the depressed peer was screened later than 3 months after birth. Standard errors are clustered at mother group level. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level.

Tab. A13 Robustness check: Drop Individuals with Screening Score above 10

	(1) Mental health contact age 2	(2) GP mental health consultation age 2	(3) Psychologist psychiatrist age 2	(4) Mental health hospitalization age 2
Depressed peer	0.015**	0.010*	0.005	0.006**
	(0.006)	(0.006)	(0.004)	(0.003)
Control group mean	0.147	0.113	0.060	0.018
Obs.	15904	15904	15904	15904

Notes: See notes to Table A5. Compared to the main sample we drop mothers with a high screening score. Standard errors are clustered at mother group level. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level.

Tab. A14 Treatment Intensity: Effects of Exposure to a Depressed Peer Maternal Mental Health Care Usage

	(1) Mental health contact year 2	(2) GP mental health consultation year 2	(3) Psychologist psychiatrist year 2	(4) Mental health hospitalization year 2
Depressed peer	-0.052*	-0.017	-0.030	-0.018
	(0.030)	(0.026)	(0.021)	(0.014)
Screening score of depressed peer	0.005**	0.002	0.003*	0.002*
	(0.002)	(0.002)	(0.001)	(0.001)
Control group mean	0.167	0.129	0.070	0.023
Obs.	17187	17187	17187	17187

Notes: See notes to Table A5. Compared to the main specification we accumulate having a mental health contact in the first two years after childbirth separately. Standard errors are clustered at mother group level. * significance at the 10 pct level; ** significance at the 5 pct level; *** significance at the 1 pct level.

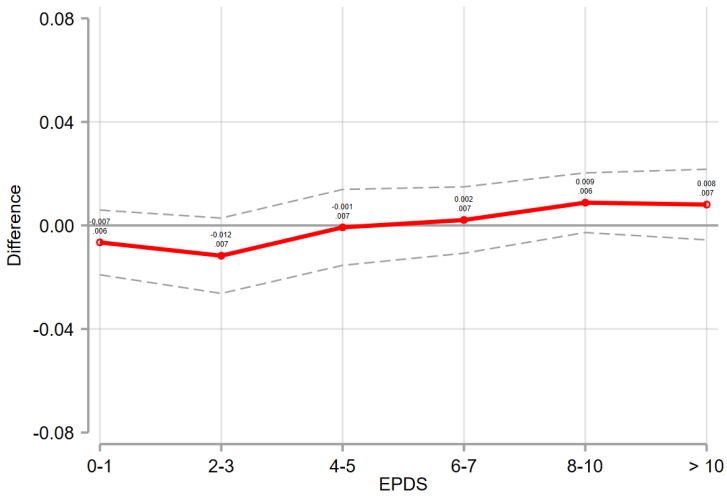


Fig. A1 EPDS Score Balance across Treatment Status

Notes: The figure shows coefficients as dots from separate regressions. The outcomes are indicators for having EPDS screening scores in the intervals given by the x-axis. The estimates are the coefficients in front of the treatment variable (indicator for exposure to a depressed peer). The regressions include a first-born indicator and month-of-birth and municipality fixed effects. Standard errors are clustered at mother group level. Dashed lines are 90 percent confidence intervals.

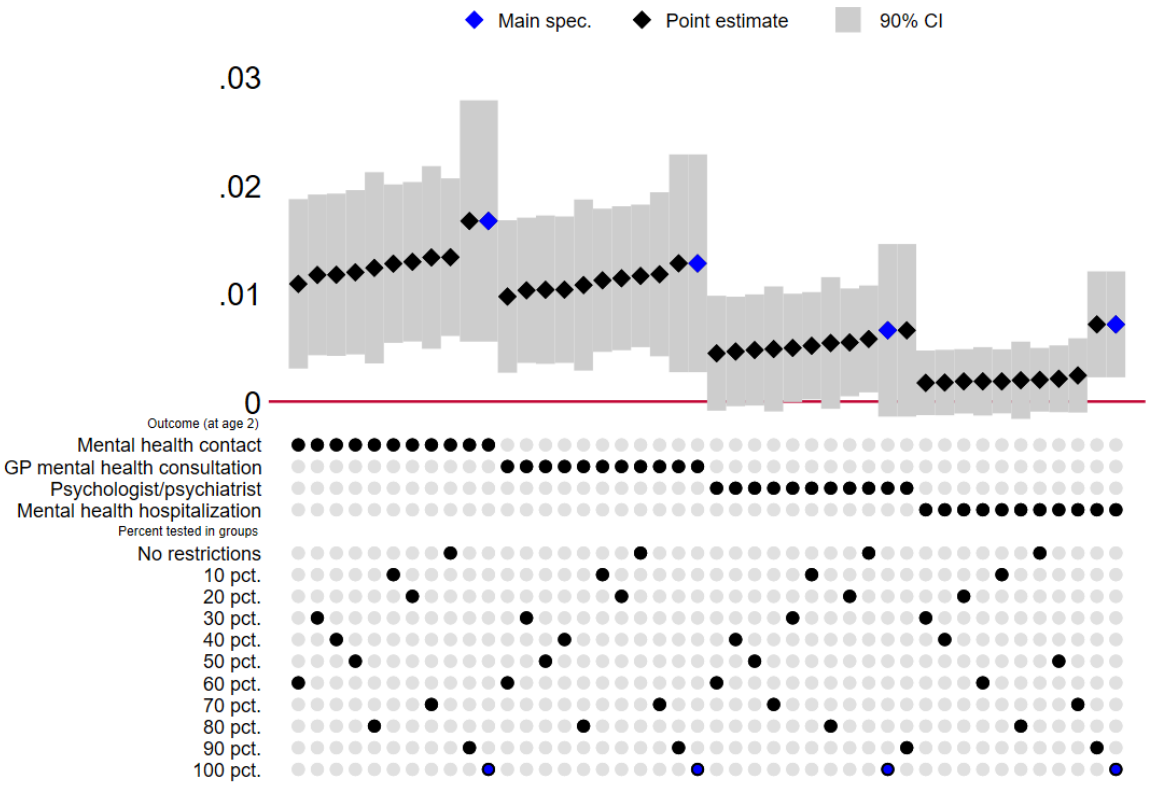
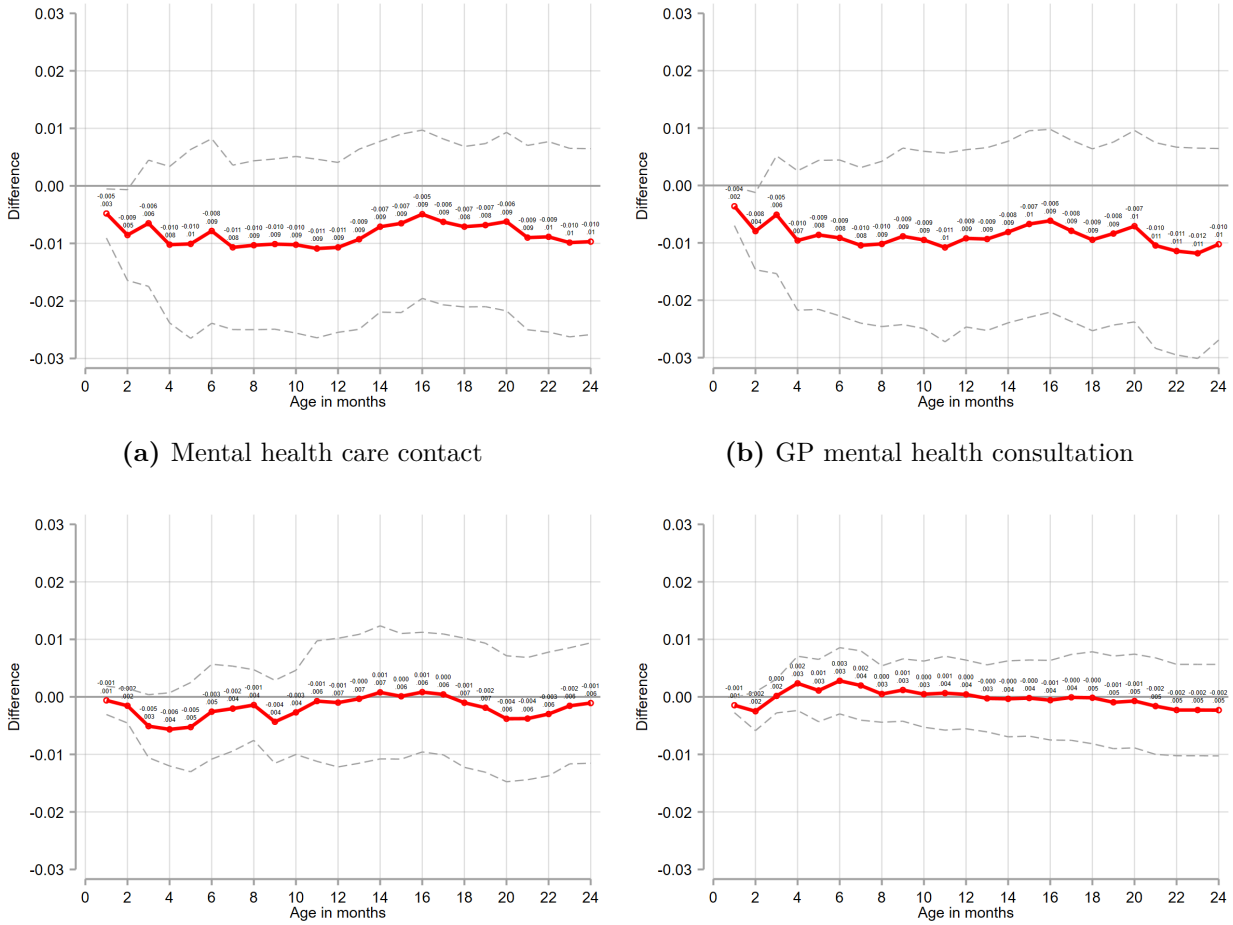


Fig. A2 Robustness: Percentage of group screened for mental health issues

Notes: Each dot represents coefficients from a specific specification to estimate the effect of having a depressed peer on mental health care usage. We vary the restriction on the percentage of groups members being screened for postnatal mental health issues. In the main results we require 100 percent of members to have been screened. Blue dots represent the main specifications. The regressions include a first-born indicator and month-of-birth and municipality fixed effects. Standard errors are clustered at mother group level. The grey bars indicate 90% confidence intervals.



(a) Mental health care contact

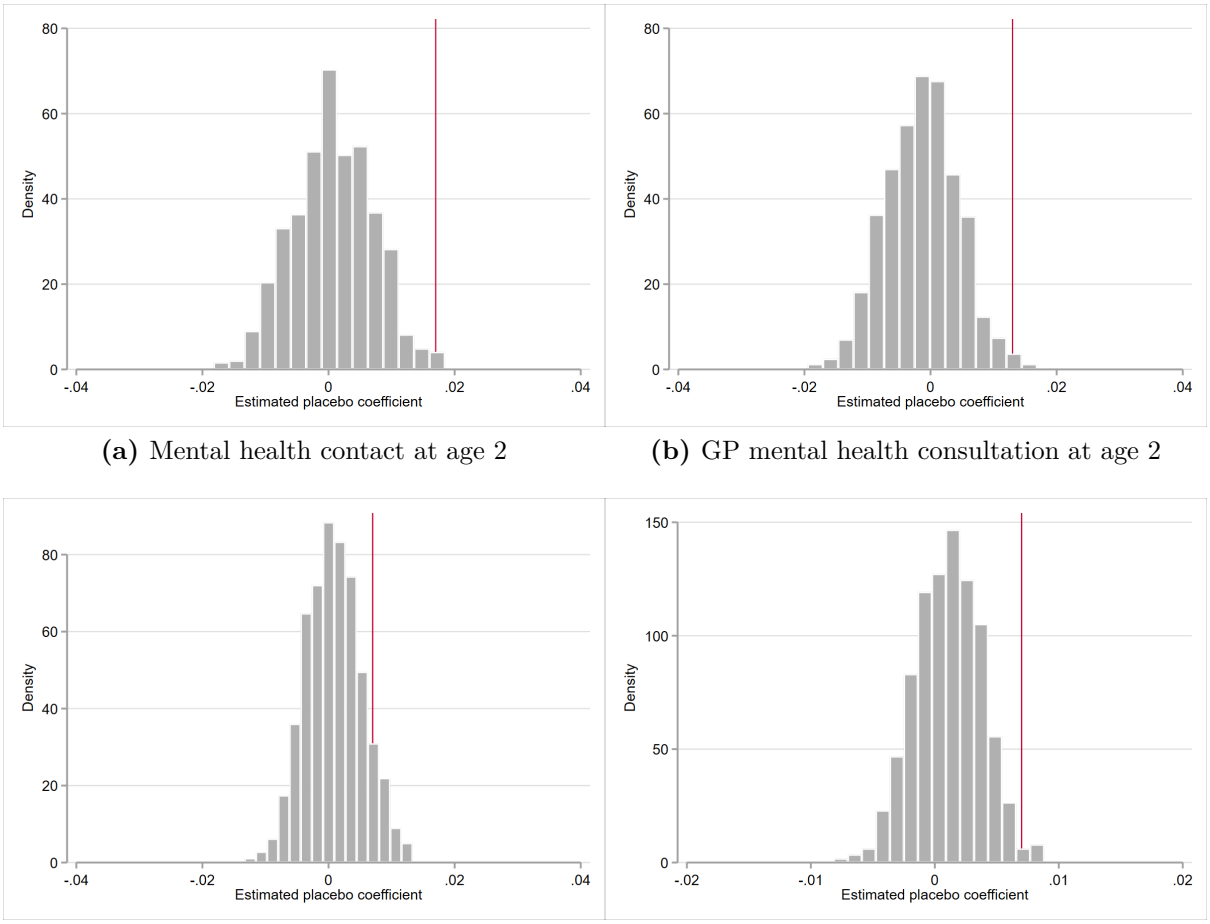
(b) GP mental health consultation

(c) Psychologist/psychiatrist consultation

(d) Mental health hospitalization

Fig. A3 Effects of Average Screening Scores of Peers on Maternal Mental Health Care Usage on High Scoring Mothers

Notes: The figure shows coefficients as dots from separate regressions with outcomes given by the panel title measured monthly throughout the first two years after childbirth. The estimates are the coefficients in front of the treatment variable (average screening score of peers). The regressions include a first-born indicator and month-of-birth and municipality fixed effects. Standard errors are clustered at mother group level. Dashed lines are 90 percent confidence intervals.



(a) Mental health contact at age 2

(b) GP mental health consultation at age 2

(c) Psychologist/psychiatrist at age 2

(d) Mental health hospitalization at age 2

Fig. A4 Random Placebo Tests

Notes: We randomize group assignment within month-of-birth and municipality for children in the sample and estimate the effect of having a depressed peer. We run this procedure 10000 times. The figures plot the distribution of the resulting placebo estimates along with the true effects of having a depressed peer (black vertical lines). The regressions include a first-born indicator and month-of-birth and municipality fixed effects.