VOLUME 9 | ISSUE 3 | **JULY 2018**

ISSN 1757-9597

Longitudinal and Life Course Studies: International Journal

Published by

SLLS Society for Longitudinal and Life Course Studies

Inside this issue

LLCS EDITORIAL BOARD

IMPACT

The journal has been accepted (2018) into the **Social Sciences Citation Index**®, maintained by Clarivate Analytics, along with their Journal Citation Reports/Social Sciences Edition, and Current Contents®/Social and Behavioral Sciences. It had been in the Emerging Sources Citation Index since 2016, and will be eligible for rating with an Impact Factor in 2019. It is also indexed by Google and by the SCOPUS citation database since 2014.

SUBSCRIPTIONS

For immediate access to current issue and preceding 2016 and 2017 issues (Volumes 7 and 8): ***Annual*** Individual rate: £20 (discounted rate £55 for 3 years**)**; ***Annual*** Library rate: £200 to register any number of library readers; ***Annual* Society for Longitudinal and Life Course Studies (SLLS) Membership: *Individual* £65, Student £26, Corporate £300.** SLLS members have automatic free access to the journal among many other benefits. For further information about SLLS and details of how to become a member, go to https://www.slls.org.uk/membership All issues are accessible free of charge 12 months after publication.

Back issues

Printed back issues of the journal (to end of Volume 7) are available at a price of £10 (plus postage and packaging). If you would like to purchase a full set or individual copies, visit the SLLS Bookshop at https://www.slls.org.uk/llcsjournal Depending on distance, you will receive the order within two weeks of submitting your order. **Please note:** the reselling of personal registration subscriptions and hard copies of the journal is prohibited.

LLCS Journal can be accessed online at www.llcsjournal.org

Published by the Society for Longitudinal and Life Course Studies
info@slls.org.uk www.slls.org.uk

SLLS Society for Longitudinal and Life Course Studies

SAVE THE DATE!

SLLS International Conference

hosted by the

University of Potsdam, Germany

25th – 27th September 2019

(with post-conference workshops planned for the 24th September)

University of Potsdam

A Call for Papers will be distributed in Summer 2018.

The conference fee will be discounted for SLLS members so consider joining if you have not already done so! Visit the membership page of the Society website for full details and to join www.slls.org.uk/membership

More information will be posted on the SLLS conference website as it becomes available www.sllsconference.com

www.slls.org.uk

The Society for Longitudinal and Life Course Studies (SLLS) is a Registered UK Charity, No: 1144426

Editorial: From imputation to impact

Heather Joshi

The topics included in this issue range from the imputation of missing data in longitudinal surveys to demonstrating that their results make a difference in the public arena – both challenges to our research field the world over. Along the way through these pages, the papers include studies of various intergenerational transmissions of social advantages and disadvantages, and social predictors of the mental health of adults. As it happens, three Australian longitudinal datasets feature in these contributions, suggesting that the creation and analysis of longitudinal data resources is thriving 'down under'.

Missing data is a particular challenge in longitudinal research due to survey attrition. The first paper by Panteha Hayati Rezvan, Katherine Lee and Julie Simpson deals with a technique for multiple imputation of missing data where it cannot be assumed that such information is missing at random. The technique explained here, the delta adjustment method, uses expert assessments in the construction of a sensitivity analysis around the bias that may be generated when data are missing not at random. Although this approach is applicable more generally, the authors offer an illustration using data from the Longitudinal Study of Australian Children. They estimate a range of possible bias in the association of maternal distress when a child is age 4–5 with that child's Total Difficulties score four years later, on the Strengths and Difficulties Questionnaire, allowing for the possibility that both variables may be subject to non-random survey loss.

The second article, by Jack Lam and Francisco Perales investigates the stress process theory of adult mental health. It is based on 15 years of data from the Household, Income and Labour Dynamics in Australia (HILDA). Mental health appears to be protected by living in a couple, particularly if married, and particularly in the case where one of them suffers from chronic illness. The relationship applies to both genders. This points to policy implications including the need to recognise the vulnerability of unpartnered people with chronic illness to mental health problems.

The relationship of adult mental health to adverse life events, explored in the paper by Mandemakers and Kalmijn, uses data from the Netherlands Kinship Panel. The adverse events considered are partner loss (divorce/separation or death), death of a parent, unemployment and disability. The findings for partner loss echo patterns seen on the other side of the globe by Lam and Perales. This study adds to the picture by quantifying the cumulative and interactive impact of different sorts of troubles coming together.

The fourth paper, by Tiina Ristikari, Marko Merikukka and Mia Kristina Hakovirta, returns to the association of parents' adversity with offspring outcomes, in this case of young adults. This study is based on linked administrative records from the Finnish Cohort Study of people born in 1987. Adversity in childhood is represented by parents claiming social assistance for various timings and durations. Their offspring's outcomes to age 25 include educational failure, crime and teenage motherhood. Results confirm that early and persistent poverty has strong links with these problems in early adulthood.

By contrast, the paper by Francis Green, Samantha Parsons, Alice Sullivan and Richard Wiggins is concerned with the transmission of good fortune from one generation to the next. Taking data from the British Household Panel and the British 1970 Cohort, they investigate homogamy among people who have attended private schools. Women who have been to private schools are somewhat more likely than their state-educated counterparts to marry privately educated men. They are also more likely to have highly paid husbands. This contributes to the forces maintaining social immobility in Britain.

The Study Profile contributed by a team from Queensland University (Gita Mishra and nine colleagues) introduces the third Australian dataset this issue – the extension of the Australian Longitudinal Study of Women's Health to the second generation. MatCH (Mothers and their Children's Health) uses an internet or postal follow-up about the children's health, 'matched' to administrative sources about their education. As the data collection on the children was not completed until mid 2017, analysis has hardly begun, and its potential remains open.

We close with a piece based on David Bell's keynote talk at the 2017 SLLS Conference in Stirling. Based on his experience of setting up HAGIS (Healthy Ageing in Scotland), he discusses the requirement that long-term studies demonstrate their utility to the public and policy maker in the form of something called 'impact'. As was recently reaffirmed in the ESRC's review of UK longitudinal resources, there is an expectation by funders that investments in such resource justify their existence by demonstrating 'impact', beyond the academic sphere. Bell expresses some scepticism about establishing the long-term scientific benefits from short-term evidence. SLLS tries to help promote policy engagement of longitudinal research through the activities of its Policy Group and media outreach. Another step towards the elusive goal may be to give explicit credit to the data resources when we write up the results of research based upon them. They cannot be taken for granted.

Longitudinal and Life Course Studies 2018 Volume 9 Issue 3 Pp 259–278 ISSN 1757-9597

Sensitivity analysis within multiple imputation framework using delta-adjustment: Application to Longitudinal Study of Australian Children

Panteha Hayati Rezvan The University of California, Los Angeles, USA and The University of Melbourne, Australia

Katherine J. Lee Murdoch Children's Research Institute, Australia and The University of Melbourne, Australia

Julie A. Simpson The University of Melbourne, Australia
julieas@unimelb.edu.au

(Received February 2018 Revised June 2018) http://dx.doi.org/10.14301/llcs.v9i3.503

Abstract

Multiple imputation (MI) is a powerful statistical method for handling missing data. Standard implementations of MI are valid under the unverifiable assumption of missing at random (MAR), which is often implausible in practice. The delta-adjustment method, implemented within the MI framework, can be used to perform sensitivity analyses that assess the impact of departures from the MAR assumption on the final inference. This method requires specification of unknown sensitivity parameter(s) (termed as delta(s)).

We illustrate the application of the delta-adjustment method using data from the Longitudinal Study of Australian Children, where the epidemiological question is to estimate the association between exposure to maternal emotional distress at age 4–5 years and total (social, emotional, and behavioural) difficulties at age 8–9 years. We elicited the sensitivity parameters for the outcome (Y) and exposure (X) variables from a panel of experts. The elicited quantile judgements from each expert were converted into a suitable parametric probability distribution and combined using the linear pooling method. We then applied MI under MAR followed by sensitivity analyses under missing not at random (MNAR) using the delta-adjustment method. We present results from sensitivity analyses that used different percentile values of the pooled distributions for the delta parameters for Y and X, and demonstrate that twofold increases in the magnitude of the association between maternal distress and total difficulties are only observed for large departures from MAR.

Keywords

Missing data; multiple imputation; missing not at random; sensitivity analysis; delta-adjustment method; elicitation

Background

Missing data commonly occur in longitudinal studies with multiple waves of data collection over long periods of follow-up (Burton & Altman, 2004; Karahalios, Baglietto, Carlin, English, & Simpson, 2012; Sterne et al., 2009). The simplest and widely used approach for handling missing data in these studies is to omit participants with any missing observations from the statistical analysis (known as a complete case analysis), which can greatly reduce the sample size, resulting in loss of precision and statistical power (i.e. inefficiency). More importantly, complete case analyses may give rise to bias if participants with complete records do not represent the entire study sample (and if the statistical analyses do not adjust for predictors of missingness).

An alternative statistical approach for dealing with missing data is multiple imputation (MI) (Rubin, 1987), a flexible and sophisticated method, which has grown in popularity among researchers (Hayati Rezvan, Lee, & Simpson, 2015; Mackinnon, 2010; Manly & Wells, 2015). MI involves two stages. First, missing data are imputed by sampling multiple times (denoted *m*) from an imputation model based on the observed data to create multiple completed (observed and imputed values) datasets. Second, the completed datasets are analysed separately using the statistical method required for the target analysis, resulting in *m* sets of parameter estimates and associated variances. The estimates obtained from each completed dataset are then combined using special formulae, known as Rubin's rules (Rubin, 1987), to obtain one overall MI estimate and corresponding variance, which accounts for the within- and between- imputation variability. Unlike a complete case analysis, MI enables all participants to be included in the target analysis by replacing the missing data with plausible values, thereby, potentially improving efficiency and reducing the bias obtained from a complete case analysis (Little & Rubin, 2002; White & Carlin, 2010). Of note, this bias correction achieved with MI is only obtained if all of the variables associated with non-response are included in the imputation model.

The standard implementation of MI is typically valid under the assumption that data are missing at random (i.e. the probability of a value being missing in any variable depends on the observed data and is conditionally independent of any unobserved values (MAR)). However, in many practical settings, the plausibility of the MAR assumption is questionable, and it is more likely that the probability of data being missing depends on the unobserved values (i.e. data are missing not at random (MNAR)). In such cases, performing the standard MI procedure may not capture the true underlying missing data mechanism, and may lead to biased results (White & Carlin, 2010). Since it is not possible to verify whether the missingness depends on the missing data, it is desirable to assess the robustness of the MI results for the target analysis by conducting sensitivity analyses that explore the effects of plausible departures from the MAR assumption.

The necessity of performing such sensitivity analyses within the MI framework has been emphasised in a number of guidelines (Burzykowski et al., 2010; Little et al., 2012; Sterne et al., 2009; White, Horton, Carpenter, & Pocock, 2011) and reviews (Hayati Rezvan, Lee & Simpson, 2015; Mackinnon, 2010), and was also recommended in the 2010 report produced by the National Research Council (NRC) expert panel on handling missing data in clinical trials (National Research Council, 2010). In general, there are two frameworks for modelling data that are MNAR, both of which are based on different factorisations of the joint distribution of the data and the mechanism leading to missing data. These two broad classes of models are the selection model and pattern-mixture model (Diggle & Kenward, 1994; Hogan & Laird, 1997; Kenward & Molenberghs, 1999; Little, 1993; Little, 1995).

In the context of MI, there are a number of approaches that have been proposed in the statistical literature for performing sensitivity analyses to the MAR assumption under these two frameworks (Carpenter, Kenward, & White, 2007; Siddique, Harel, & Crespi, 2012; Siddique, Harel, Crespi, & Hedeker, 2014; van Buuren, Boshuizen, & Boshuizen, 1999). A selection-model-based weighting approach, which was proposed by Carpenter, Kenward and White (2007), involves re-weighting the parameter estimates obtained from analyses of the imputed values under the assumption of MAR in such a way that reflect the MNAR mechanism (i.e. a weighted version of Rubin's rules). This approach was proposed as an approximate method for performing sensitivity analyses following MI, for a single variable with missingness that is weakly MNAR (Carpenter et al., 2007; Carpenter, Rücker, & Schwarzer, 2011;

Carpenter & Kenward, 2013). The performance of the weighting approach was evaluated through a series of simulation experiments, where it was shown that the method does not recover unbiased estimates even when the number of imputations used is large (Hayati Rezvan, White, Lee, Carlin, & Simpson, 2015). Possible reasons for the failure of the method were explained in detail, and the method was not recommended for performing sensitivity analyses to assess the effects of departure from the MAR assumption.

An alternative approach to sensitivity analysis in the context of MI is the delta-adjustment method, which was proposed by Rubin (1987) within the pattern-mixture modelling framework, where the MAR imputed values are modified to reflect an assumed MNAR mechanism. Practical examples of this method in different settings are provided by a number of authors (Carpenter & Kenward, 2007; Carpenter & Kenward, 2013; Leacy, Floyd, Yates, & White, 2017; Liublinska & Rubin, 2014; Moreno-Betancur & Chavance, 2013; Ratitch, O'Kelly, & Tosiello, 2013; van Buuren et al., 1999; van Buuren, 2012). For continuous incomplete variables, the method often proceeds by adding some fixed constant to the imputed values obtained under MAR from a standard MI procedure. For categorical incomplete variables, the missing values are drawn from an imputation model assuming MNAR, which proceeds by adding offsets to the linear predictors of the variables with missing observations. The modified imputations are then analysed and the resulting estimates and variances for each completed dataset combined in the usual way (i.e. Rubin’s rules). “Multiple-model multiple imputation”, developed by Siddique et al. (Siddique et al., 2012; Siddique et al., 2014) within the pattern-mixture modelling framework, is an alternative approach which takes into account the uncertainty in the missingness mechanism by specifying a distribution for the value of the offset, so that imputations are generated from multiple imputation models with different offsets. The resulting estimates are combined using nested imputation combining rules (Shen, 2000) to obtain final estimates.

Despite recent emphasis on the importance of performing sensitivity analyses following MI to assess the influence of plausible departures from MAR, the approaches outlined earlier have not been widely adopted in practice, due to lack of explicit guidance for conducting such sensitivity analyses. The majority of pattern-mixture model-based methods have been implemented in the context of clinical trials, generally with missing data in the outcome variable only. Recently, a number of statistical packages (SAS 9.4, Proc MI (Yuan, 2014) and R, SensMice (Resseguier, 2010; Resseguier, Giorgi, & Paoletti, 2011; Resseguier, Verdoux, Giorgi, Clavel-Chapelon, & Paoletti, 2013) have been developed to impute missing values in multiple variables under MNAR using the delta-adjustment method (with the strong assumption that the MNAR mechanism is independent for multiple incomplete variables), however, they have not been widely used in practice.

One of the most important aspects of conducting sensitivity analyses to the MAR assumption using any of the above methods is selecting one or more sensitivity parameters that represent plausible departures from MAR. These parameters are generally unidentifiable values and must be specified by one or more experts who have relevant knowledge of the subject matter. For applications of the methods described above, in general, extreme values or a range of plausible values for the sensitivity parameters have been selected by the analyst instead of carefully elicited from content experts. Although there is a vast amount of literature on the different approaches for eliciting uncertain values from experts’ knowledge (Kadane & Wolfson, 1998; O’Hagan, 2006; White, 2015; White, Carpenter, Evans, & Schroter, 2007), there is limited research on eliciting unknown sensitivity parameters in practice for sensitivity analyses within the MI framework.

The outline for the rest of this paper is as follows. We begin with an overview of pattern-mixture models and explain the delta-adjustment method for performing sensitivity analyses within the MI framework. We also describe the process of obtaining prior information regarding the sensitivity parameters from subject-matter experts (i.e. elicitation). We then describe the Longitudinal Study of Australian Children (LSAC) and the case study that motivated this work. We address how we elicited the required information regarding the unknown values of sensitivity parameters from a panel of three experts, and then present results from the LSAC case study where the delta-adjustment method was implemented for imputing missing data assuming MNAR for the outcome and exposure of interest using the elicited sensitivity parameters. Finally, we conclude with a discussion of the delta-adjustment method.

Methods

Pattern-mixture models

In brief, within the pattern-mixture modelling framework, the incomplete data are modelled conditional on the missingness mechanism (i.e. the response pattern). Let Y be a partially observed variable, with Y^{obs} and Y^{mis} representing its observed and missing components, respectively. Suppose also that X is a fully observed variable and R_y is the usual indicator of missingness, taking value of 0 or 1, depending on whether Y is missing or observed. Under the pattern-mixture framework, the joint distribution of the complete data and the missing data mechanism, $f(Y, R_y | X)$, is factorised as:

$$f(Y^{obs}, Y^{mis}, R_y|X) = f(Y^{obs}, Y^{mis}|R_y, X)\, f(R_y|X) \quad (1)$$

i.e. the distribution of complete data (Y^{obs}, Y^{mis}) conditional on the missingness mechanism (R_y), and the marginal distribution of the missing data mechanism. Under this factorisation, there is a different joint distribution of the observed and missing data for each missing data pattern. Equation (1) can be further decomposed to:

$$f(Y^{obs}, Y^{mis}, R_y|X) = f(Y^{mis}|Y^{obs}, R_y, X)\, f(Y^{obs}|R_y, X)\, f(R_y|X) \quad (2)$$

which enables researchers to distinguish the conditional distribution of the missing data given the observed data from the distribution of the observed data. Under MAR, $f(Y^{mis}|Y^{obs}, R_y, X) = f(Y^{mis}|Y^{obs}, X)$, that is, $f(Y^{mis}|Y^{obs}, X, R_y = 1) = f(Y^{mis}|Y^{obs}, X, R_y = 0)$, implying that the imputed values are drawn from the posterior distribution of the observed data, $f(Y^{mis}|Y^{obs}, X, R_y = 1)$. However, under MNAR, $f(Y^{mis}|Y^{obs}, X, R_y = 1) \neq f(Y^{mis}|Y^{obs}, X, R_y = 0)$, indicating that the conditional distribution of the missing data given the observed data will differ with missingness patterns represented by R_y.

In order to fit these models, participants are initially divided into different groups based on their missing data pattern, and then the grouping variable is used to model the effect of missing data patterns on outcome(s). Using these models, the overall estimate is then obtained by averaging the outcome over the missing data patterns. A simple pattern-mixture model could be in the form of a linear function as below:

$$E(Y|R_y, X) = \varphi_0 + \varphi_1 X + \delta(1 - R_y) \quad (3)$$

where Y is a continuous variable with missing data, X is a fully observed covariate, and R_y is a missingness indicator of Y as described above. δ in equation (3) is known as the sensitivity parameter and quantifies the degree of departure from the MAR assumption, where $\delta = 0$ represents the MAR mechanism as there is no dependency between the missingness mechanism and the missing values in Y. This parameter shows the mean difference (shift) of the partially observed variable Y between the missing and observed data. If $\delta > 0$ (or $\delta < 0$), the mean of Y among non-respondents is δ units higher (or lower) than respondents, given a fixed value of X. If Y is a partially observed binary variable (0/1), then the pattern-mixture model could be in the form of the following logit function:

$$logit[Pr(Y = 1|R_y, X)] = \gamma_0 + \gamma_1 X + \delta(1 - R_y) \quad (4)$$

where δ represents the difference in the $\log_e$ odds of Y=1 between non-respondents and respondents. Of note, equations (3) and (4) represent models that allow for shifts of the intercepts, φ_0 and γ_0, respectively. In realistic scenarios, where the covariate X has different impacts on the outcome Y among different missing data patterns, it is more relevant that the pattern-mixture models allow for the association between X and Y to differ between non-respondents and respondents (i.e. additional sensitivity parameters are required to allow for shifts of the regression coefficients φ_1 and γ_1).

Implementation of the delta-adjustment method

For continuous variables with MNAR missingness, the imputation procedure using the delta-adjustment method proceeds as follows:

(i) Missing values in a partially observed variable are imputed using a standard MI procedure under MAR. Returning to the example explained earlier for an incomplete continuous variable, first the point estimates of φ_0, φ_1, and σ^2 are obtained by fitting the imputation model $Y = \varphi_0 + \varphi_1 X + \varepsilon$, where $\varepsilon \sim N(0, \sigma^2)$, to the observed data (i.e. $R_y = 1$) to characterise the joint posterior distribution of φ_0, φ_1 and σ^2. Then, imputed values are drawn from the posterior distribution of the parameters.

(ii) The imputed values are shifted by adding some fixed value, δ, which is obtained from content experts, to reflect the MNAR mechanism.

(iii) The completed (observed plus imputed values) datasets are analysed separately using standard statistical methods, and the resulting point estimates and standard errors are combined using Rubin's rules to give a single MNAR estimate.

The procedure for imputing MNAR missing data in categorical variables proceeds similarly but in step (i) an offset of δ is included in the univariate imputation model so that the missing values are drawn from an imputation model assuming MNAR, and thus step (ii) is omitted. In the example described previously for an incomplete binary variable, missing values in Y variable are imputed using the MNAR imputation model, $logit\ [Pr(Y=1|X)] = \gamma_0 + \gamma_1 X + \delta(1-R_y)$, instead of the MAR imputation model, $logit\ [Pr(Y=1|X)] = \gamma_0 + \gamma_1 X$.

Specification of the sensitivity parameters, δ

In practice the sensitivity parameters are unidentifiable values and cannot be estimated from the observed data since, by definition, they depend on the missing data. The only principled approach to determine plausible values of the unknown sensitivity parameters is to extract them from experts who have relevant knowledge about the subject matter. However, elicitation of the sensitivity parameters is often a challenging task in practice, and alternatively some investigators prefer to conduct a tipping-point analysis (Yan, Lee, & Li, 2009) in which sensitivity parameters are varied across a large range of values.

This section briefly describes the process of elicitation (O'Hagan, 2006), which, by definition, aims to formulate the expert's beliefs into a probability distribution for the parameter of interest, in this case the sensitivity parameter of the pattern-mixture model, δ. Briefly, as described by Garthwaite, Kadane and O'Hagan (2005), a good elicitation depends on the quality of the four important stages listed below.

Stage 1: Setting up the elicitation problem by identifying one or more unknown quantities for which expert judgement is required. It is common practice to elicit the opinion from a number of experts using a questionnaire and/or a face-to-face interview. Although, using a questionnaire has the advantage of the same structured questions being given to each expert, designing a questionnaire that will be clear to all experts is not a trivial task. Thus, for elicitation a face-to-face interview is considered the optimal approach (White, 2015).

Stage 2: Eliciting experts' opinion about the unknown quantities. Since the aim of the elicitation is to present experts' judgements in a form of a probability distribution, they are often asked to suggest values for suitable summary statistics of the associated parameters of that distribution. Usually these summaries are probabilities (e.g. single probabilities or quantiles), measures of location (e.g. mean, median or mode), measures of spread (e.g. usually variance or standard deviation), etc.

Stage 3: Fitting an appropriate probability distribution to those elicited summaries. In situations where elicitation is conducted for each expert individually, the resulting distributions must be combined into a single probability distribution (see O'Hagan (2006) and Garthwaite et al. (2005) for more details). Several methods have been proposed in the literature for combining distributions from multiple experts into a single distribution (O'Hagan, 2006). The simplest aggregation method is linear opinion pooling (Cooke, 1991; Genest & Zidek, 1986; Mcconway, 1981), which is a weighted average of each expert's distribution. Under this approach, the investigator must decide whether all experts should be weighted equally, or whether to assign larger weights to experts whose distributions are believed to be more accurate. There are number of software packages available to help elicitation in practice, which are mostly designed within the "Sheffield Elicitation Framework" (O'Hagan, 2013); examples include SHELF package (Oakley, 2017) in R software (R Development Core Team, 2005), Elicitator software (Kynn, 2005.), UncertWeb-The Elicitator (Bastin et al., 2013), and MATCH Uncertainty Elicitation Tool (Morris, Oakley, & Crowe, 2014).

Stage 4: Evaluating the adequacy of the elicitation process by providing feedback to the experts. Once the desirable summaries have been elicited from experts and an adequate probability distribution has been specified by the investigator, the experts should be informed about the implications of that fitted distribution (e.g. provided with visual feedback) and asked whether the estimated quantities adequately represent their opinions. In cases where experts believe that the fitted distribution does not express their opinions, repeating the procedure until the probability distribution accurately reflects their beliefs is required. Further, sensitivity analyses considering a number of alternative probability distributions can be conducted to explore the impacts of experts' uncertainty.

In the 'Results' section, where the application of the delta-adjustment method is presented using the LSAC case study, we explain how and what quantities we elicited for obtaining a probability distribution for the sensitivity parameters.

Motivation: The LSAC case study

The LSAC is a national longitudinal study of childhood development in Australia. Data have been collected on a range of important aspects of childhood development such as wellbeing (physical and mental health), education and schooling, and social, emotional, and cognitive functioning across childhood. Details of the LSAC dataset have been published elsewhere (Australian Institute of Family Studies, 2015). In brief, LSAC consists of two cohorts of children: the birth cohort, which includes 5107 infants aged 0–1 years, and the kindergarten cohort, which includes 4983 children aged 4–5 years. The primary objective of the LSAC case study presented in this paper, which only uses data from the kindergarten cohort, is to estimate the association between exposure to maternal emotional distress at age 4–5 years (i.e. pre–school children) and total (social, emotional, and behavioural) difficulties at age 8–9 years, controlling for potential confounders. This case study was motivated by previously published research (see Bayer et al. (2011)). Since our aim was to use the present case study to evaluate the delta-adjustment method rather than make any substantive claims about the LSAC data, we modified the analysis from that used in the original article (Bayer et al., 2011) in order to keep the target analysis and imputation models simple.

The outcome variable of interest is total social, emotional and behavioural difficulties of children aged 8–9 years as assessed by the total score on Strengths and Difficulties Questionnaire (SDQ) (Goodman, 1997) at wave 3 of the data collection. This was also measured at wave 1 (age 4–5 years). The SDQ total score is summed over four subscales relating to conduct, hyperactivity, peer, and emotional problems. Each of these subscales is averaged over five items, where 1 is the minimum score for an answer 'Not true' and 3 is the maximum score for an answer 'Certainly true'. This score is then rescaled to be an integer between 0 and 10, giving a total score that can range from 0 to 40. A lower SDQ total score corresponds to a better overall behaviour status. The SDQ total score in the present study ranged from 0 to 35 (25th, 50th, and 75th percentiles were 4, 6, and 10, respectively) and was moderately right-skewed. While a number of approaches have been proposed in the MI literature for handling non-normally distributed variables, including transforming the skewed variables prior to imputation or predictive mean matching, we imputed missing values of the SDQ total score on the raw scale since it was modelled on the raw scale in the analysis model (Lee & Carlin, 2017; von Hippel, 2013).

The primary exposure of interest is maternal emotional distress at age 4–5 years as measured by the Kessler-6 (K-6) (Kessler et al., 2010) depression scale at wave 1. In LSAC, this measure is calculated as the mean of six items assessing mother's anxiety and depression symptoms in the most recent four weeks. For each item, the minimum score of 1 represents 'All of the time', and the maximum score of 5 indicates 'None of the time', with higher averaged scores representing better mother's mental health status. The distribution of maternal emotional distress was left-skewed, with the bulk of the observations at the higher end of the scale (i.e. 4–5). For the purpose of illustrating the missing data problem in the current case study, we dichotomised the variable such that 1 represents a category with 'Probable serious mental illness' (i.e. average K-6 score less than 4), and 0 represents a category with 'No probable serious mental illness' (i.e. average K-6 score greater or equal to 4).

The target analysis model is a multivariable linear regression of SDQ total score at 8–9 years on maternal emotional distress at 4–5 years, controlling for ten potential confounders measured at wave 1. The confounders selected *a priori* were: SDQ total score (possible range 0–40), child physical functioning score based on Paediatric Quality of Life inventory (PEDS QL) (possible range 0–100) (Varni, 2006), mother's age (years), consistent parenting score (possible range 1–5), family financial hardship score (possible range 0–6), sex of child (male/female), whether child has a sibling in the home (yes/no), mother current cigarette smoker (yes/no), mother consumes >2 standard drinks of alcohol daily (yes/no), and mother completed high school (yes/no).

Table 1 presents the details of all the variables used in the statistical analysis along with the frequency of missing data. The percentage of missing observations in the dataset for a given variable ranged from 0% to 23.8% and the missing data pattern was non-monotonic. Out of the twelve variables included in the target analysis model, only sex of child and whether child has a sibling in the home were completely observed. While the SDQ total score at 8–9 years (outcome at wave 3) was the variable with the highest amount of missing observations (23.8%), data on maternal emotional distress at 4–5 years (exposure at wave 1) were also not available for 16.4% of the participants. Sixty-five percent of the sample (3244 participants) had complete data on the outcome and all of the covariates included in the target analysis model are listed in table 1. Among participants with observed SDQ total score at 8–9 years (i.e. 3798 out of 4331 participants at wave 3), the average total score was 7.5. Further, among mothers with observed scores for maternal emotional distress at 4–5 years, 20.9% had probable serious mental illness.

It is highly likely that the reason for missingness of SDQ total score is related to the underlying child behavioural status (i.e. MNAR). In a similar fashion, missing data for maternal emotional distress is of particular concern because the reason for a mother not completing certain survey questions or being unwilling to participate in a face-to-face interview is probably related to her underlying mental health status.

Table 1. Description of variables used in the LSAC case study analysis (n=4983).

Variable description	Grouping /range	Number missing (%)
Binary variables		
Sex of child	Male / Female	0(0)
Child has a sibling in the home	Yes / No	0(0)
Mother current cigarette smoker	Yes / No	852(17.1)
Mother alcohol consumption[†]	Yes / No	966(19.2)
Mother completed high school	Yes / No	44(0.9)
Mother emotional distress[‡*]	(<4) / (≥4)[‡]	819(16.4)
Continuous variables		
Mother's age	Years	39(0.8)
Consistent parenting score	[1 – 5]	81(1.6)
Family financial hardship score	[0 – 6]	14(0.3)
Child physical functioning score	[0 – 100]	785(15.8)
Child SDQ total score[*]	[0 – 40]	15(0.3)
Child SDQ total score[**]	[0 – 40]	1185(23.8)

[†] Mother consumes > 2 standard drinks of alcohol daily.
[‡] Probable serious mental illness / No probable serious mental illness.
[*] Measured at 4–5 years (wave 1).
[**] Measured at 8–9 years (wave 3).

Results

We initially carried out a complete case analysis to investigate the association between exposure to maternal emotional distress at 4–5 years and SDQ total score at 8–9 years, controlling for potential confounders. Performing a complete case analysis reduced the number of participants to 3244 from 4983 recruited individuals in the kindergarten cohort (i.e. 35% reduction). Further, the assessment of missing data suggests that a complete case analysis may lead to biased estimates as there are some characteristics that differed between children with incomplete (i.e. who had data available on at least one characteristic but missing observations on one or more other characteristics) and complete (i.e. who had data available on all twelve variables listed in table 1) observations (see table A1 in appendix A).

We implemented a standard MI procedure under the assumption of MAR, and then performed sensitivity analyses using the delta-adjustment method to assess the research question under plausible departures from MAR. We elicited the required values for the sensitivity parameters from content experts, and we obtained marginal distributions that reflect the uncertainty about the quantities of interest.

Eliciting expert opinion about two sensitivity parameters

As described earlier, for performing sensitivity analyses, we assumed that only the missing data in the SDQ total score (Y) and maternal emotional distress (X) follow a MNAR missingness mechanism. For simplicity we assumed that the two missingness mechanisms are independent (i.e. $R_x \perp\!\!\!\perp R_y$). This means that we elicited experts' opinions regarding the marginal distributions of the sensitivity parameters for the outcome and exposure separately (elicitation of a joint distribution for two sensitivity parameters is beyond the scope of this manuscript). In the context of the pattern-mixture modelling, the sensitivity parameters for the outcome and exposure variables correspond to the difference between LSAC non-respondents and respondents, which represent (1) the mean differences in the SDQ total score at 8–9 years (denoted by $\delta_{(y)} = \mu_{R_y(0)} - \mu_{R_y(1)}$, where the subscripts $R_y(0)$ and $R_y(1)$ correspond to R_y=0 and R_y=1, respectively), and (2) the shift in the log$_e$ odds of mothers with probable serious mental illness at 4–5 years (denoted by $\delta_{(x)} = log(\frac{\pi_{R_x(0)}/1-\pi_{R_x(0)}}{\pi_{R_x(1)}/1-\pi_{R_x(1)}})$, where $\pi_{R_x(0)}$ and $\pi_{R_x(1)}$ correspond to proportions of mothers with probable serious mental illness when R_x=0 and R_x=1, respectively). A summary of the elicitation process for these parameters is provided in the following sections.

Choice of experts and format of elicitation

Three experts, who had relevant knowledge about child health and development, were invited to form a panel for the elicitation task. The elicitation was implemented through a written questionnaire (see appendix B) that was explained and discussed face-to-face with each expert individually.

Elicitation task

Since the aim of elicitation is to present an expert's judgement on how those with missing data differ to those with observed data (i.e. the sensitivity parameter) in a form of a probability distribution, they are often asked to determine desirable summary statistics (e.g. probabilities, measure of location and/or measures of spread) for this difference. Investigators may assist experts in this regard by providing the distributions of the incomplete variables in the observed data based on their elicited summaries to determine whether the summaries provided reflect their opinion.

Following a brief description of the primary outcome and exposure of interest, the questionnaire asked experts' expectation on how individuals with missing data differ from those with observed data (i.e. sensitivity parameter) in a form of a probability distribution. In particular, they were asked to determine desirable summary statistics for this difference including the mean differences in the SDQ total score at 8–9 years, and the proportional change in mothers who were emotionally distressed at 4–5 years between LSAC participants with missing and observed data. Note that for the latter sensitivity parameter (i.e. shift in the proportions, $\pi_{R_x(0)} - \pi_{R_x(1)}$) this was not exactly the same as the sensitivity parameter used in the delta-adjustment method as an offset in the imputation model (i.e. shift in the log$_e$ odds- $\delta_{(x)} = log(\frac{\pi_{R_x(0)}/1-\pi_{R_x(0)}}{\pi_{R_x(1)}/1-\pi_{R_x(1)}})$). Elicitation of the shift in the proportions was used to ease interpretation and avoid any confusion in eliciting the shift in log$_e$

odds. The elicited proportional shift was then transformed into the $\log_e$ odds scale using the proportion of mothers with probable serious mental illness in the observed data (i.e. $\pi_{R_x(1)}$ =21%), and the corresponding $\pi_{R_x(0)}$ calculated from $\pi_{R_x(0)} - \pi_{R_x(1)}$.

The experts were asked to provide a small number of quantile judgements for each of the sensitivity parameters including a median value, upper and lower quartiles and minimum and maximum values (see table 2) for $\mu_{R_y(0)} - \mu_{R_y(1)}$ and $\pi_{R_x(0)} - \pi_{R_x(1)}$. To help the experts to provide these quantities, investigators provided the distributions of the incomplete outcome and exposure in the observed data based on their elicited summaries to determine whether the summaries provided reflect their opinion. In addition, an example of a hypothetical expert was given in the questionnaire with the graphical illustration of the expert's probability distribution function for $\mu_{R_y(0)} - \mu_{R_y(1)}$ and $\pi_{R_x(0)} - \pi_{R_x(1)}$ (see appendix B).

Feedback

Feedback was provided after eliciting the required summaries from each expert by asking them whether the elicited quantities adequately express each expert's opinion. For example, based on the information provided by expert 1 in table 2 (bottom), the following feedback was given.

"We know that the proportion of maternal emotional distress in the observed data is 21%. Based on the summaries that you provided, you think among those who are not observed, this proportion is going to be somewhere between 22% and 30%, and your best guess is 25%. Does that sound about right?"

If the experts believed that the chosen summaries did not represent their opinion, they were kindly asked to provide the summaries again.

Fitting a distribution and pooling experts' opinion

The elicited quantile judgements from each expert, corresponding to the points on the cumulative distribution functions (CDF), were converted into a suitable parametric probability distribution (PDF) for $\mu_{R_y(0)} - \mu_{R_y(1)}$ and $\pi_{R_x(0)} - \pi_{R_x(1)}$ using SHELF package (O'Hagan, 2013; Oakley, 2017) in R (R Development Core Team, 2005) (see appendix C for more details). The linear pooling method, explained earlier, with equal weight given to each expert, was adopted to combine the experts' individual PDFs into a single PDF (see figure 1).

Table 2. Elicitation of prior information for the distribution of the mean difference in the SDQ total score at 8–9 years ($\delta_{(y)} = \mu_{Ry(0)} - \mu_{Ry(1)}$), and the average change in the percentage of mothers who were emotionally distressed at 4–5 years ($\pi_{Rx(0)} - \pi_{Rx(1)}$), between LSAC non-respondents and respondents.

	Minimum	Lower quartile	Median	Upper quartile	Maximum
$\mu_{R_y(0)} - \mu_{R_y(1)}$					
Hypothetical example	**-1**	**1**	**3**	**7**	**10**
Expert 1 response	0.5	0.75	1.3	2.25	2.5
Expert 2 response	-1	1	2.6	8	10.6
Expert 3 response	-1	1	3	6	9
$\pi_{R_x(0)} - \pi_{R_x(1)}$					
Hypothetical example	**-5%**	**3%**	**5%**	**9%**	**20%**
Expert 1 response	1%	2.5%	4%	7%	9%
Expert 2 response	-3%	5%	10%	15%	20%
Expert 3 response	0	2%	10%	18%	26%

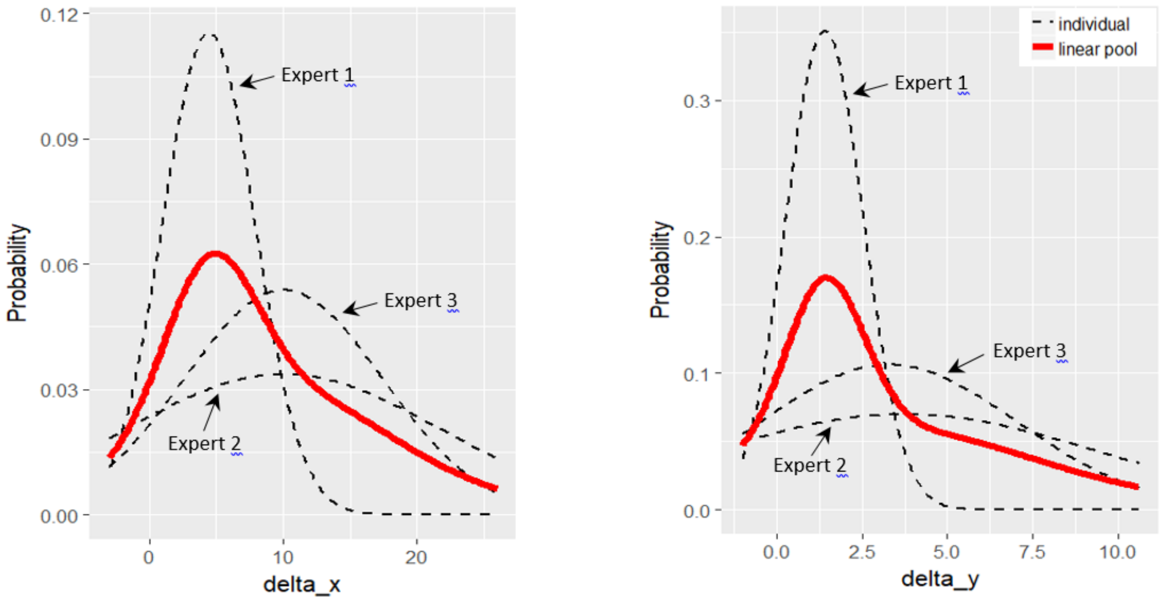


Figure 1. Graphical illustration of linear opinion pooling. Dashed lines represent experts' individual PDFs and solid lines correspond to the final pooled PDF. Left panel: elicited and pooled distributions for the average change in the proportion of maternal emotional distress at 4–5 years between non-respondents and respondents ($\pi_{Rx(0)}$- $\pi_{Rx(1)}$). Right panel: elicited and pooled distributions for the average differences of SDQ total score at 8–9 years between non-respondents and respondents ($\mu_{Ry(0)}$-$\mu_{Ry(1)}$).

Table 3. Percentiles of the pooled distributions for the sensitivity parameters of interest.

Sensitivity parameter	5th percentile	Lower quartile	Median	Upper quartile	95th percentile
$\mu_{R_y(0)} - \mu_{R_y(1)}$	-3.277	0.560	2.119	4.916	10.398
$\pi_{R_x(0)} - \pi_{R_x(1)}$	-0.042	0.028	0.069	0.130	0.239
$log(\frac{\pi_{R_x(0)}/1-\pi_{R_x(0)}}{\pi_{R_x(1)}/1-\pi_{R_x(1)}})^*$	-0.276	0.162	0.377	0.663	1.122

* $\pi_{R_x(1)}$ was set to 21%, the observed value in study sample.

It is apparent from the plots above that expert 1's distributions for both sensitivity parameters are quite different from the two other experts, and the pooled distributions are right-skewed. Consequently, instead of making random draws from the distributions for conducting sensitivity analyses, we used different percentile values of the pooled distributions (5th, 25th, 50th, 75th, and 95th percentiles) as an offset in separate pattern-mixture models. Table 3 presents the relevant percentiles of the pooled distributions for $\delta_{(y)} = \mu_{R_y(0)} - \mu_{R_y(1)}$, $\pi_{R_x(0)} - \pi_{R_x(1)}$, and $\delta_{pm(x)} = log(\frac{\pi_{R_x(0)}/1-\pi_{R_x(0)}}{\pi_{R_x(1)}/1-\pi_{R_x(1)}})$.

Multiple imputation under the assumption of MAR

MI by chained equations (MICE – also known as fully conditional specification (FCS) (van Buuren, 2007; van Buuren et al., 1999; van Buuren, 2015)), which is a widely used imputation approach for handling missing data in multiple incomplete variables with a general missingness pattern, was adopted using the user-written command *–ice–* (Royston & White, 2011) in Stata 15.0 (50 cycles and 100 imputations). The variables included in the MAR imputation models were the same as the variables in the target analysis model (see table 1) to ensure compatibility between the imputation and analysis models. In particular, the outcome variable SDQ total score at 8–9 years was included in the imputation model to prevent the association between the outcome and covariates being falsely weakened (Moons, Donders, Stijnen, & Harrell, 2006; Sterne et al., 2009; White, Royston, & Wood, 2011). Further, two auxiliary variables measured at wave 1, which were predictors of missingness in the exposure and/or outcome, were also included in the imputation models to make the MAR assumption more plausible and to reduce bias (Collins, Schafer, & Kam, 2001; White et al., 2011). These were mother's primary language was not English (yes/no –2.9% missingness) and whether child has two parents in the home (yes/no – 0 % missingness).

All the variables included in the imputation model were imputed except the completely observed variables, sex of child, whether child has a sibling in the home, and whether child has two parents in the home. The estimates of the model parameters which were obtained from separate analyses of each of the 100 completed datasets were combined to provide an overall MI estimate (MI_{MAR}). The estimates of the regression coefficients obtained from MI under MAR as well as complete case analysis are shown in table 4 with their corresponding standard errors (SE) and 95% confidence intervals (95% CI).

As expected, MI increased the precision of the regression estimates slightly in comparison to the complete case analysis. The differences between the estimates obtained from the complete case analysis and MI under MAR were more apparent for the variables with higher proportion of missing data (e.g. maternal emotional distress, mother current cigarette smoker, and mother alcohol consumption; 16–19%) as well as mother high school completion, family financial hardship score, and whether child has a sibling in the home. Both complete case analysis and MI under MAR provide estimates, CIs and p-value that collectively provide evidence for positive relationship between total difficulties at 8–9 years and maternal emotional distress at 4–5 years.

Sensitivity analyses to the MAR assumption using the pattern-mixture method

As mentioned previously, different percentiles of the pooled distributions for the two sensitivity parameters were used to illustrate the application of the delta-adjustment method for performing sensitivity analyses following MI in the LSAC case study. The sensitivity analysis was implemented using –*uvis*– command (i.e. univariate imputation sampling, which imputes missing observations in a single variable given a set of predictors) in Stata 15.0 (Royston, 2004; Royston, 2005). We repeatedly called this procedure to iteratively draw missing values from a specified set of univariate conditional distributions for each incomplete variable, where we included the elicited sensitivity parameters as offsets in univariate imputation models for the outcome and exposure. Although the –*uvis*– command is repeatedly called by the standard routine of "*mi impute chained*" or the user written –*ice*– commands to perform multivariate imputation, incorporating offsets in each univariate imputation model for both continuous and binary incomplete variables simultaneously was not readily available in these procedures.

The variables included in the MNAR imputation models were the same as the MAR imputation models, that is, the target analysis variables as well as the two auxiliary variables. The missing data in the outcome and exposure were imputed such that imputations were drawn from imputation models assuming MNAR. We used different percentile values of the pooled distributions for $\delta_{(y)}$ and $\delta_{(x)}$ derived previously, and included these values as an offset to the linear predictors of the univariate imputation models for imputing the outcome and exposure, respectively. Estimates of the regression coefficients (95% CIs) obtained from the sensitivity analyses (MI under MNAR- MI_{MNAR}) are presented in figure 2, table 4, and table D1 in appendix D.

Table 4. Linear regression analysis results from complete case analysis, MI under MAR(MI_{MAR}), and MI under MNAR (MI_{MAR}) using the pattern-mixture method for the LSAC case study; outcome variable is total social, emotional and behavioural difficulties of children as assessed by the total score on Strength and Difficulties Questionnaire (SDQ total score) measured at 8–9 years (wave 3).

Variables	Complete analysis		MI_{MAR}		MI_{MNAR}*	
	Coefficient (SE)	95% CI	Coefficient (SE)	95% CI	Coefficient (SE)	95% CI
Maternal emotional distress†	0.59 (0.20)	(0.20, 0.98)	0.67 (0.20)	(0.27, 1.06)	0.82 (0.20)	(0.43, 1.21)
Child SDQ total score†	0.50 (0.02)	(0.47, 0.53)	0.50 (0.02)	(0.47, 0.53)	0.50 (0.02)	(0.47, 0.53)
Mother's age	-0.02 (0.02)	(-0.05, 0.01)	-0.03 (0.01)	(-0.05, 0.00)	-0.04 (0.01)	(-0.07, -0.01)
Sex of child	1.12 (0.15)	(0.83, 1.41)	1.09 (0.14)	(0.82, 1.36)	1.11 (0.14)	(0.84, 1.38)
Child has a sibling in the home	-0.78 (0.24)	(-1.25, -0.31)	-0.82 (0.22)	(-1.25, -0.40)	-0.85 (0.23)	(-1.29, -0.41)
Mother completed high school	-0.49 (0.16)	(-0.80, -0.18)	-0.58 (0.15)	(-0.87, -0.30)	-0.75 (0.16)	(-1.05, -0.44)
Mother current cigarette smoker	0.34 (0.20)	(-0.05, 0.72)	0.30 (0.19)	(-0.08, 0.68)	0.38 (0.21)	(-0.03, 0.80)
Mother alcohol consumption‡	-0.32 (0.37)	(-1.04, 0.41)	-0.31 (0.37)	(-1.03, 0.41)	-0.23 (0.41)	(-1.02, 0.57)
Consistent parenting score	-0.12 (0.12)	(-0.36, 0.12)	-0.12 (0.12)	(-0.35, 0.11)	-0.27 (0.12)	(-0.51, -0.04)
Child physical functioning score	-0.03 (0.01)	(-0.05, -0.02)	-0.03 (0.01)	(-0.04, -0.02)	-0.03 (0.01)	(-0.04, -0.02)
Family financial hardship score	0.51 (0.09)	(0.33, 0.69)	0.39 (0.09)	(0.22, 0.56)	0.46 (0.09)	(0.30, 0.63)

*50th percentile value of the pooled distribution obtained from experts used as a sensitivity parameter in the MNAR analysis.
†Measured at 4–5 years (wave 1). ‡Mother consumes >2 standard drinks of alcohol daily

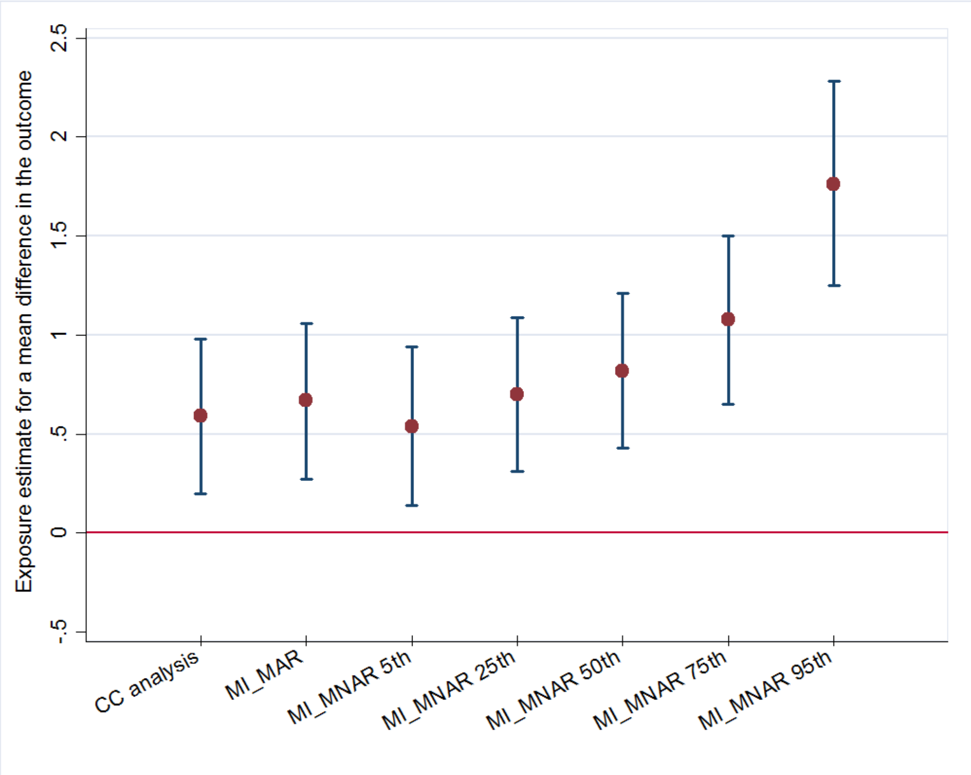


Figure 2. Estimates of the regression coefficient (95% CI) for the target analysis (i.e. the estimated adjusted mean difference in SDQ total score at 8–9 years associated with the exposure, maternal emotional distress at 4–5 years) obtained from a complete case analysis, MI under MAR, and MI under MNAR using different percentile values (5th, 25th, 50th, 75th, 95th percentile) of the pooled elicited distributions for the sensitivity parameters.

Table D1 in appendix D shows that the magnitude of regression estimates for the exposure and some of the confounder variables (e.g. mother current cigarette smoker and mother alcohol consumption) changes as the higher percentile values of the pooled distribution are used as a sensitivity parameter for both the exposure and outcome. This is not surprising since these variables have a high proportion of missing data and the missingness in these variables generally coincided with missingness in the exposure variable.

It is apparent from figure 2 that the MNAR estimates using the 5th and 25th percentiles of the pooled distributions for the sensitivity parameter are similar to the MAR estimate, since these percentile values represent minimal departures from MAR (see table 4).

Using the higher percentile values of the pooled distributions for the sensitivity parameter (i.e. 50th, 75th, and 95th) results in greater departures from MAR and therefore greater shifts in the MNAR compared with the MAR parameter estimates, as expected. Of note though, under all MNAR scenarios, the sensitivity analysis provides the same overall conclusion; that is, there is evidence that exposure to maternal emotional distress at 4–5 years is associated with the higher levels of SDQ total score at 8–9 years. Thus, we can conclude that the result obtained under the MAR analysis is robust (not sensitive) to plausible departures from the MAR assumption.

However, it is important to note that although the overall conclusion has not been changed under all MNAR scenarios, the magnitude of the association between exposure to maternal emotional distress at 4–5 years and SDQ total score at 8–9 years has increased from 0.54 (95% CI: 0.14, 0.94) using 5th percentile values of sensitivity parameters for both the exposure and outcome to 1.76 (95% CI: 1.25, 2.28) for the 95th percentile values, potentially a clinically important difference.

Discussion

Drop-out and non-response are problematic in LSAC and many other large-scale longitudinal studies with multiple follow-up waves of data collection. Since the complete cases in the LSAC case study appeared to differ from the incomplete cases on a number of characteristics, we adopted MI for handling missing data to avoid producing biased results. While there were small differences between the magnitude of the regression estimates obtained from the complete case analysis and MI, both of these methods provided evidence for the positive relationship between maternal emotional distress at 4–5 years and SDQ total score at 8–9 years. Further, the findings did not differ dramatically in terms of precision of the parameter estimates, which may be related to the small number of auxiliary variables that were included in the imputation models and/or small proportion of missing observations in the variables imputed.

We conducted sensitivity analyses using the delta-adjustment method to assess the potential impact of departures from MAR on the final inference with elicitation from content experts for specification of the distributions of the sensitivity parameters. The elicited distributions suggested that children with higher SDQ total score and children with mothers who were emotionally distressed were more likely to be non-respondents, the elicited mean difference in SDQ total score (at 8–9 years) would most likely be 2 units, and the most probable average shift in the proportion of emotionally distressed mothers (at 4–5 years) would be 6.9%. Our results show that exposure to maternal emotional distress at 4–5 years was associated with higher levels of SDQ total score at 8–9 years, and that the magnitude of this association increases two-fold only for the extreme values of the pooled distributions of the sensitivity parameters.

Limitations and future work

In their paper, van Buuren et al. (1999) illustrated the δ-adjustment method with missing data in the highly correlated variables, systolic and diastolic blood pressure, where the effect of applying the δ-adjustment on systolic blood pressure (i.e. adding an offset δ to the univariate imputation model) was carried over to the diastolic blood pressure. This problem is due to the feedback of δ-adjustment, where the offset is amplified by the iterative FCS algorithm in the presence of highly correlated incomplete variables in the imputation model (van Buuren & Groothuis-Oudshoorn, 2011; van Buuren, 2012). Such feedback could be problematic since the offset may not represent the actual departure from MAR, and may not correspond to the value of the sensitivity parameter that was elicited from the content expert. To avoid the issue of feedback, it was initially suggested to exclude the strongest predictors for the incomplete variables in the imputation model (van Buuren & Groothuis-Oudshoorn, 2011). A correction was also suggested to multiply δ by a damping factor (van Buuren, 2012); however, the performance of the δ-adjustment method has not been explored further, e.g. in scenarios with different types of incomplete variables using multiple offset values corresponding to each of the incomplete variables. Thus, further exploration of the performance of the delta-adjustment method under a range of realistic scenarios would be desirable to provide methodological insights and assist practical researchers to deal with incomplete data in complex settings, where the missing data mechanism is MNAR.

Our case study involved multiple incomplete variables, where it was suspected that the incomplete outcome and exposure of interest followed a MNAR mechanism, where the missingness in the outcome and exposure was independent (i.e. $R_x \perp\!\!\!\perp R_y$). For ease of exposition, the target analysis model was modified to include ten potential confounders with no interaction and non-linear terms. We also included two auxiliary variables, both of which contained a small proportion of missing data, in the imputation model to make the MAR assumption more plausible and reduce bias in implementing MI under MAR. Future research could be conducted to explore the performance of the delta-adjustment method through a number of complicated case studies using more complex imputation and analysis models. For example, exploration of missing data in more than two incomplete variables with MNAR missingness, with different variable types and when the independence assumption of missingness indicators does not hold for multiple variables with MNAR missing data, which would require elicitation of the joint distribution between incomplete variables.

In general, for performing a MNAR sensitivity analysis, when the interest lies in the association between an outcome Y and a covariate X, considering a scenario that allows the association between X and Y to differ between non-respondents and respondents may seem more relevant and realistic. Although such elicitation in practice may be challenging, it ensures a more comprehensive investigation of departures from the MAR assumption. Further research would be to evaluate the performance of the delta-adjustment method for estimating the association between Y and X, when this association differs between non-respondents and respondents for the outcome Y by each category of X (i.e. when the assumption of independence between the missingness indicators is relaxed). For this evaluation, there would be three sensitivity parameters: $\delta_{(x)}$ to account for MNAR missing data in X (i.e. a shift in $\log_e$ odds of X =1 for R_x=0 and R_x=1), and $\delta_{(y|x=1)}$ and $\delta_{(y|x=0)}$ to account for MNAR missing data in Y for X =1 and X =0, respectively (i.e. a shift in mean of Y for $R_{y|x=1}$=0 vs. $R_{y|x=1}$=1 when X =1, and a shift in mean of Y for $R_{y|x=0}$=0 vs. $R_{y|x=0}$=1 when X=0, respectively). These three sensitivity parameters would need to be elicited from experts in order to include in the imputation models to frame the MNAR missingness in Y and X. For the LSAC case study, this would mean eliciting additional prior information to allow for the difference between missing and observed values in SDQ total score at 8–9 years to vary between those with and without maternal emotional distress at 4–5 years.

A work undertaken by Leacy and White (2015) has illustrated the importance of considering the dependence between the missingness indicators for incomplete variables when modelling MNAR missingness. They have extended the δ-adjustment method for imputing missing data under MNAR to allow for the dependence between the variables by including indicators for the missingness in the incomplete variables in each of the univariate imputation models. Further exploration is desirable to examine the proposed method in more realistic situations when the imputation model involves multiple continuous variables, as well as a mixture of binary and continuous variables with missing data, and potentially time-dependent variables.

Additional research exploring the delta-adjustment method when different MNAR imputation models for different groups of participants are required is also important. White, Kalaitzaki, and Thompson (2011) incorporated interaction terms to allow the sensitivity parameters to differ between the trial arms for a continuous outcome measured at a single time-point. Moreno-Betancur and Chavance (2013) proposed a method within the pattern-mixture modelling framework for imputing a continuous time-dependent outcome and performed a sensitivity analysis, in a clinical trial setting, to assess the robustness of the MAR results, allowing the sensitivity parameter for the incomplete repeated measure continuous outcome (baseline plus up to five follow-up visits) to differ between treatment and control groups at the last visit. Further development of the delta-adjustment method is required to incorporate sensitivity parameters for different groups of participants for variables of different data types with missing data.

As described in detail earlier, we carefully elicited the required quantile judgements from a panel of experts to specify distributions for the unknown sensitivity parameters of interest. We initially contacted six experts and invited them to be part of the elicitation task; however, the final elicitation panel was limited to three experts who were available for face-to-face interview. We gathered their opinions via separate interviews with the use of a written questionnaire and aggregated the results into a single probability distribution rather than carrying out a group elicitation, which may be subject to biases (e.g. quantile judgments provided by one expert may influence others' decisions). Of note, it is theoretically possible for experts to select a value of the sensitivity parameter that would strengthen the findings. In our study, we asked experts to consider the raw data i.e. to elicit on the raw scale, and they were not aware of the effect their elicited values/distribution had on the analysis until the end of the elicitation process. The elicited summaries from expert 1 were quite different from the other two experts who had similar responses to the hypothetical examples in the questionnaire. Providing more hypothetical examples, which consider multiple scenarios with different quantile values, may minimise the potential to sway expert opinion.

We conducted sensitivity analyses over extreme values of the pooled distribution (i.e. 5th and 95th percentiles) to examine whether the study conclusion was sensitive to larger departures from MAR instead of randomly drawing the sensitivity parameters from their corresponding pooled distributions, since the incompatibility between the experts led to skewness in the pooled distribution. It may be of interest to examine how inferences might change across the range of values using the graphical tipping-point analysis (Liublinska & Rubin, 2014; Ratitch et al., 2013; Yan et al., 2009), where the impact of alternative assumptions regarding the missing data on the final study conclusions can be graphically assessed. Future research could also incorporate alternative approaches for summarising the results from sensitivity analyses such as a fully Bayesian methodology proposed by Scharfstein, Daniels, and Robins (2003), which allows researchers to draw a single conclusion by incorporating prior beliefs for sensitivity parameters, or uncertainty intervals developed by Vansteelandt, Goetghebeur, Kenward, and Molenberghs (2006), which accounts for both the uncertainty in the data and the uncertainty in the MNAR mechanism.

As noted earlier, the delta-adjustment method in the current research was implemented in Stata 15.0 using loops of the *–uvis–* commands; the standard routine of '*mi impute chained*' command did not offer the flexibility of including an offset in the univariate generalised linear models. For widespread uptake of the delta-adjustment method, development of a user-friendly package for conducting such sensitivity analyses in Stata and other commonly used statistical software is required. Furthermore, development of practical tools (e.g. visual representation), which allow for elicitation of multiple sensitivity parameters, and guidance on elicitation of sensitivity parameters from content experts within the MI framework is warranted.

Conclusions

Finally, the findings of this paper addressed some of the knowledge gaps in the literature by using a case study as a motivating example to illustrate the application of the delta-adjustment method in practice. Despite the limitations in this research, our investigation provides valuable insights into sensitivity analyses following MI using the delta-adjustment method when two incomplete variables follow the MNAR missingness mechanisms. Nevertheless, additional development of the method is desirable to assist the researcher for conducting sensitivity analyses in more complex studies in practice.

Acknowledgements

This work was supported by funding from the National Health and Medical Research Council: a Centre of Research Excellence grant, ID 1035261, awarded to the Victorian Centre of Biostatistics (ViCBiostat), Career Development Fellowship ID 1053609 (KJL), and Senior Research Fellowship ID 1104975 (JAS). PHR was funded by a University of Melbourne International Research Scholarship (MIRS). She is currently funded by National Institute of Mental Health (NIMH) [grant number: 1T32MH109205-01A1] and the UCLA Center for HIV Identification, Prevention, and Treatment Services (CHIPTS) grant (P30MH58107).

References

Australian Institute of Family Studies. (2015). *The Longitudinal Study of Australian Children data user guide*. Retrieved from http://www.growingupinaustralia.gov.au

Bastin, L., Cornford, D., Jones, R., Heuvelink, G. B. M., Pebesma, E., Stasch, C., . . . Williams, M. (2013). Managing uncertainty in integrated environmental modelling: The UncertWeb framework. *Environmental Modelling and Software*, *39*, 116–134. https://doi.org/10.1016/j.envsoft.2012.02.008

Bayer, J. K., Ukoumunne, O. C., Lucas, N., Wake, M., Scalzo, K., & Nicholson, J. M. (2011). Risk factors for childhood mental health symptoms: National Longitudinal Study of Australian Children. *Pediatrics*, *128*(4), 865–879. https://doi.org/10.1542/peds.2011-0491

Burton, A., & Altman, D. (2004). Missing covariate data within cancer prognostic studies: A review of current reporting and proposed guidelines. *British Journal of Cancer*, *91*(1), 4–8. https://doi.org/10.1038/sj.bjc.6601907

Burzykowski, T., Carpenter, J. R., Coens, C., Evans, D., France, L., Kenward, M. G., . . . Yu, L. (2010). Missing data: Discussion points from the PSI missing data expert group. *Pharmaceutical Statistics*, *9*(4), 288–297. https://doi.org/10.1002/pst.391

Carpenter, J. R., Kenward, M. G., & White, I. R. (2007). Sensitivity analysis after multiple imputation under missing at random: A weighting approach. *Statistical Methods in Medical Research*, *16*(3), 259–275.

Carpenter, J. R., & Kenward, M. G. (2007). *Missing data in randomised controlled trials: A practical guide*. Birmingham: Health Technology Assessment Methodology Programme. Accessed from http://researchonline.lshtm.ac.uk/4018500/

Carpenter, J. R., & Kenward, M. G. (2013). *Multiple imputation and its application* (1st ed.). Chichester: Wiley. https://doi.org/10.1002/9781119942283

Carpenter, J. R., Rücker, G., & Schwarzer, G. (2011). Assessing the sensitivity of meta-analysis to selection bias: A multiple imputation approach. *Biometrics*, *67*(3), 1066–1072. https://doi.org/10.1111/j.1541-0420.2010.01498.x

Collins, L. M., Schafer, J. L., & Kam, C. M. (2001). A comparison of inclusive and restrictive strategies in modern missing data procedures. *Psychological Methods*, *6*(4), 330–351. https://doi.org/10.1037/1082-989X.6.4.330

Cooke, R. (1991). *Experts in uncertainty: Opinion and subjective probability in science*. Oxford: Oxford University Press.

Diggle, P., & Kenward, M. G. (1994). Informative drop-out in longitudinal data analysis. *Journal of the Royal Statistical Society: Series C (Applied Statistics)*, *43*(1), 49–93. https://doi.org/10.2307/2986113

Garthwaite, P. H., Kadane, J. B., & O'Hagan, A. (2005). Statistical methods for eliciting probability distributions. *Journal of the American Statistical Association*, *100*(470), 680–700. https://doi.org/10.1198/016214505000000105

Genest, C., & Zidek, J. V. (1986). Combining probability distributions: A critique and an annotated bibliography. *Statistical Science*, *1*(1), 114–135. https://doi.org/10.1214/ss/1177013825

Goodman, R. (1997). The strengths and difficulties questionnaire: A research note. *Journal of Child Psychology and Psychiatry and Allied Disciplines*, *38*(5), 581–586. https://doi.org/10.1111/j.1469-7610.1997.tb01545.x

Hayati Rezvan, P., White, I. R., Lee, K. J., Carlin, J. B., & Simpson, J. A. (2015). Evaluation of a weighting approach for performing sensitivity analysis after multiple imputation. *BMC Medical Research Methodology*, *15*, 83. https://doi.org/10.1186/s12874-015-0074-2

Hayati Rezvan, P., Lee, K. J., & Simpson, J. A. (2015). The rise of multiple imputation: A review of the reporting and implementation of the method in medical research. *BMC Medical Research Methodology*, *15*, 30. https://doi.org/10.1186/s12874-015-0022-1

Hogan, J. W., & Laird, N. M. (1997). Model-based approaches to analysing incomplete longitudinal and failure time data. *Statistics in Medicine*, *16*(3), 259–272. https://doi.org/10.1002/(SICI)1097-0258(19970215)16:3<259::AID-SIM484>3.0.CO;2-S

Kadane, J. B., & Wolfson, L. J. (1998). Experiences in elicitation. *Journal of the Royal Statistical Society, Series D (the Statistician)*, *47*(1), 3–19. https://doi.org/10.1111/1467-9884.00113

Karahalios, A., Baglietto, L., Carlin, J. B., English, D. R., & Simpson, J. A. (2012). A review of the reporting and handling of missing data in cohort studies with repeated assessment of exposure measures. *BMC Medical Research Methodology*, *12*(1), 96–105. https://doi.org/10.1186/1471-2288-12-96

Kenward, M., & Molenberghs, G. (1999). Parametric models for incomplete continuous and categorical longitudinal data. *Statistical Methods in Medical Research*, *8*(1), 51–83. https://doi.org/10.1177/096228029900800105

Kessler, R. C., Green, J. G., Gruber, M. J., Sampson, N. A., Bromet, E., Cuitan, M., . . . Zaslavsky, A. M. (2010). Screening for serious mental illness in the general population with the K6 screening scale: Results from the WHO world mental health (WMH) survey initiative. *International Journal of Methods in Psychiatric Research*, *19 Suppl 1*, 4–22. https://doi.org/10.1002/mpr.310

Kynn, M. (2005.). *Eliciting expert knowledge for bayesian logistic regression in species habitat modelling*. (PhD thesis, Queensland University of Technology, Australia).

Leacy, F. P., Floyd, S., Yates, T. A., & White, I. R. (2017). Analyses of sensitivity to the missing-at-random assumption using multiple imputation with delta adjustment: Application to a tuberculosis/HIV prevalence survey with incomplete HIV-status data. *American Journal of Epidemiology*, *185*(4), 304–315. https://doi.org/10.1093/aje/kww107

Leacy, F. P., & White, I. R. (2015). *Multiple imputation for data that are missing not at random: Extending the fully conditional specification procedure* [Abstract]. (Joint Statistical Meeting) Seattle, United States. Accessed from https://ww2.amstat.org/meetings/jsm/2015/onlineprogram/AbstractDetails.cfm?abstractid=315410

Lee, K. J., & Carlin, J. B. (2017). Multiple imputation in the presence of non-normal data. *Statistics in Medicine*, *36*(4), 606–617. https://doi.org/10.1002/sim.7173

Little, R. J. A. (1993). Pattern-mixture models for multivariate incomplete data. *Journal of the American Statistical Association*, *88*(421), 125–134.

Little, R. J. A. (1995). Modeling the drop-out mechanism in repeated-measures studies. *Journal of the American Statistical Association*, *90*(431), 1112–1121. https://doi.org/10.1080/01621459.1995.10476615

Little, R. J. A., D'Agostino, R., Cohen, M. L., Dickersin, K., Emerson, S. S., Farrar, J. T., . . . Stern, H. (2012). The prevention and treatment of missing data in clinical trials. *New England Journal of Medicine*, *367*(14), 1355–1360. https://doi.org/10.1056/NEJMsr1203730

Little, R. J. A., & Rubin, D. B. (2002). Statistical analysis with missing data (2nd ed.). Hoboken, N.J.: Wiley.

Liublinska, V., & Rubin, D. B. (2014). Sensitivity analysis for a partially missing binary outcome in a two-arm randomized clinical trial. *Statistics in Medicine*, *33*(24), 4170–4185. https://doi.org/10.1002/sim.6197

Mackinnon, A. (2010). The use and reporting of multiple imputation in medical research – a review. *Journal of Internal Medicine*, *268*(6), 586–593. https://doi.org/10.1111/j.1365-2796.2010.02274.x

Manly, C. A., & Wells, R. S. (2015). Reporting the use of multiple imputation for missing data in higher education research. *Research in Higher Education*, *56*(4), 397–409. https://doi.org/10.1007/s11162-014-9344-9

Mcconway, K. J. (1981). Marginalization and linear opinion pools. *Journal of the American Statistical Association*, *76*(374), 410–414. https://doi.org/10.1080/01621459.1981.10477661

Moons, K. G. M., Donders, R. A. R. T., Stijnen, T., & Harrell, F. E. (2006). Using the outcome for imputation of missing predictor values was preferred. *Journal of Clinical Epidemiology*, *59*(10), 1092–1101. https://doi.org/10.1016/j.jclinepi.2006.01.009

Moreno-Betancur, M., & Chavance, M. (2013). Sensitivity analysis of incomplete longitudinal data departing from the missing at random assumption: Methodology and application in a clinical trial with drop-outs. *Statistical Methods in Medical Research*, *25*(4), 1471–1489. https://doi.org/10.1177/0962280213490014

Morris, D. E., Oakley, J. E., & Crowe, J. A. (2014). A web-based tool for eliciting probability distributions from experts. *Environmental Modelling and Software*, *52*, 1–4. https://doi.org/10.1016/j.envsoft.2013.10.010

National Research Council. (2010). *The prevention and treatment of missing data in clinical trials*. Washington, D.C.: National Academies Press. Accessed from https://www.cytel.com/hs-fs/hub/1670/file-2411099288-pdf/Pdf/MissingDataNationalAcademyof_Medicine.2010.pdf

O'Hagan, A. (2006). *Uncertain judgements: Eliciting experts' probabilities* (1st ed.). London; Hoboken, NJ : John Wiley & Sons. https://doi.org/10.1002/0470033312

O'Hagan, T. (2013). *SHELF: The Sheffield Elicitation framework*. Accessed from http://www.tonyohagan.co.uk/shelf

Oakley, J. (2017). *Tools to support the Sheffield elicitation framework (SHELF) 1.3.0*. Accessed from http://www.tonyohagan.co.uk/shelf

R Development Core Team. (2005). *R: A language and environment for statistical computing, reference index version 2.2.1*. Vienna, Austria: R Foundation for Statistical Computing.

Ratitch, B., O'Kelly, M., & Tosiello, R. (2013). Missing data in clinical trials: From clinical assumptions to statistical analysis using pattern mixture models. *Pharmaceutical Statistics*, *12*(6), 337–347. https://doi.org/10.1002/pst.1549

Resseguier, N. (2010). Package 'SensMice'. Accessed from http://download.lww.com/wolterskluwer_vitalstream_com/PermaLink/EDE/A/EDE_2010_12_08_PAOLETTI_200963_SDC1.pdf

Resseguier, N., Giorgi, R., & Paoletti, X. (2011). Sensitivity analysis when data are missing not-at-random. *Epidemiology* (Cambridge, Mass.), *22*(2), 282–283. https://doi.org/10.1097/EDE.0b013e318209dec7

Resseguier, N., Verdoux, H., Giorgi, R., Clavel-Chapelon, F., & Paoletti, X. (2013). Dealing with missing data in the center for epidemiologic studies depression self-report scale: A study based on the French E3N cohort. *BMC Medical Research Methodology*, *13*(1), 1–11. https://doi.org/10.1186/1471-2288-13-28

Royston, P. (2004). Multiple imputation of missing values. *Stata Journal*, *4*(3), 227–241.

Royston, P. (2005). Multiple imputation of missing values: Update. *Stata Journal*, *5*(2), 188–201.

Royston, P., & White, I. R. (2011). Multiple imputation by chained equations (MICE): Implementation in Stata. *Journal of Statistical Software*, *45*(4). https://doi.org/10.18637/jss.v045.i04

Rubin, D. B. (1987). *Multiple imputation for nonresponse in surveys* (1st ed.). New York, NY: Wiley. https://doi.org/10.1002/9780470316696

Scharfstein, D. O., Daniels, M. J., & Robins, J. M. (2003). Incorporating prior beliefs about selection bias into the analysis of randomized trials with missing outcomes. *Biostatistics* (Oxford, England), *4*(4), 495–512. https://doi.org/10.1093/biostatistics/4.4.495

Shen, Z. (2000). *Nested multiple imputations*. (PhD thesis, Harvard University, United States).

Siddique, J., Harel, O., & Crespi, C. M. (2012). Addressing missing data mechanism uncertainty using multiple-model multiple imputation: Application to a longitudinal clinical trial. *Annals of Applied Statistics*, *6*(4), 1814–1837. https://doi.org/10.1214/12-AOAS555

Siddique, J., Harel, O., Crespi, C. M., & Hedeker, D. (2014). Binary variable multiple–model multiple imputation to address missing data mechanism uncertainty: Application to a smoking cessation trial. *Statistics in Medicine*, *33*(17), 3013–3028. https://doi.org/10.1002/sim.6137

Sterne, J. A. C., White, I. R., Carlin, J. B., Spratt, M., Royston, P., Kenward, M. G., . . . Carpenter, J. R. (2009). Multiple imputation for missing data in epidemiological and clinical research: Potential and pitfalls. *British Medical Journal* (BMJ), *339*(7713), 157–160. https://doi.org/10.1136/bmj.b2393

van Buuren, S. (2007). Multiple imputation of discrete and continuous data by fully conditional specification. *Statistical Methods in Medical Research*, *16*(3), 219–242. https://doi.org/10.1177/0962280206074463

van Buuren, S. (2012). *Flexible imputation of missing data* (1st ed.). Hoboken, NJ: Taylor and Francis. https://doi.org/10.1201/b11826

van Buuren, S. (2015). Fully conditional specification. In G. Molenberghs, G. Fitzmaurice, M. G. Kenward, A. Tsiatis & G. Verbeke (Eds.). *Handbook of missing data methodology* (1st edn. ed., pp. 267–294). Chapman & Hall/CRC.

van Buuren, S., Boshuizen, H. C., & Boshuizen, H. C. (1999). Multiple imputation of missing blood pressure covariates in survival analysis. *Statistics in Medicine*, *18*(6), 681–694.

van Buuren, S., & Groothuis-Oudshoorn, K. (2011). Mice: Multivariate imputation by chained equations in R. *Journal of Statistical Software*, *45*(3). https://doi.org/10.18637/jss.v045.i03

Vansteelandt, S., Goetghebeur, E., Kenward, M. G., & Molenberghs, G. (2006). Ignorance and uncertainty regions as inferential tools in a sensitivity analysis. *Statistica Sinica*, *16*(3), 953–980.

Varni, W. J. (2006). The PedsQL(pediatric quality of life inventory). Retrieved from http://www.pedsql.org/

von Hippel, P. T. (2013). Should a normal imputation model be modified to impute skewed variables? *Sociological Methods & Research*, *42*(1), 105–138. https://doi.org/10.1177/0049124112464866

White, I. R. (2015). The elicitation and use of expert opinion. In G. Molenberghs, G. Fitzmaurice, M. G. Kenward, A. Tsiatis & G. Verbeke (Eds.). *Handbook of missing data methodology* (1st ed., pp. 471–490). Chapman & Hall/CRC.

White, I. R., Royston, P., & Wood, A. M. (2011). Multiple imputation using chained equations: Issues and guidance for practice. *Statistics in Medicine*, *30*(4), 377–399. https://doi.org/10.1002/sim.4067

White, I. R., & Carlin, J. B. (2010). Bias and efficiency of multiple imputation compared with complete-case analysis for missing covariate values. *Statistics in Medicine*, *29*(28), 2920–2931. https://doi.org/10.1002/sim.3944

White, I. R., Carpenter, J. R., Evans, S., & Schroter, S. (2007). Eliciting and using expert opinions about dropout bias in randomized controlled trials. *Clinical Trials*, *4*(2), 125–139. https://doi.org/10.1177/1740774507077849

White, I. R., Horton, N. J., Carpenter, J. R., & Pocock, S. J. (2011). Strategy for intention to treat analysis in randomised trials with missing outcome data. *British Medical Journal* (BMJ), *342*(7803), 910–912. https://doi.org/10.1136/bmj.d40

White, I. R., Kalaitzaki, E., & Thompson, S. G. (2011). Allowing for missing outcome data and incomplete uptake of randomised interventions, with application to an internet-based alcohol trial. *Statistics in Medicine*, *30*(27), 3192–3207. https://doi.org/10.1002/sim.4360

Yan, X., Lee, S., & Li, N. (2009). Missing data handling methods in medical device clinical trials. *Journal of Biopharmaceutical Statistics*, *19*(6), 1085–1098. https://doi.org/10.1080/10543400903243009

Yuan, Y. (2014). *Sensitivity analysis in multiple imputation for missing data*. SAS Global Forum. Accessed from http://support.sas.com/resources/papers/proceedings14/SAS270-2014.pdf

Supplementary material

Online appendices – see website.

Appendix A: Characteristics of children with and without missing data in the LSAC case study (DOCX)
Appendix B: Elicitation questionnaire (DOCX)
Appendix C: Elicitation using SHELF package (DOCX)
Appendix D: MNAR analysis results for the LSAC case study (DOCX)

Longitudinal and Life Course Studies 2018 Volume 9 Issue 3 Pp 279–298 ISSN 1757-9597

Chronic illness and mental strain: The protective role of partners with time since illness onset

Jack Lam The University of Queensland, Australian Research Council Centre of Excellence for Children and Families Over the Life Course and Institute for Social Science Research, Australia

Francisco Perales The University of Queensland, Australian Research Council Centre of Excellence for Children and Families Over the Life Course and Institute for Social Science Research, Australia

(Received December 2017 Revised March 2018) http://dx.doi.org/10.14301/llcs.v9i3.483

Abstract

Chronic conditions are associated with large personal, familial and social costs, and have deleterious effects on individuals' mental health. Drawing on the stress process model, we theorise and test how the presence of a partner moderates the extent to which living with a chronic condition affects mental health, and whether any protective effects change with time since illness onset, or differ between men and women. Our empirical analyses rely on nationally representative, panel data for Australia (n ~180,000 observations) and panel regression models. Being in a partnership, particularly in a marriage, is associated with better mental health amongst all individuals, but more so amongst the chronically ill. This advantage remains beyond the year of illness onset, and is of a comparable magnitude for men and women. These findings bear important implications for mental health in modern societies experiencing rapid population ageing, a rising prevalence of chronic illness, and declining marriage rates.

Keywords

Chronic illness; ageing; Australia

Introduction

Chronic conditions are associated with large monetary and social costs to individuals and their families, and constitute a sizeable burden to the health care system (AIHW, 2014; Essue, Kelly, Roberts, Leeder, & Jan, 2011). In addition, such conditions are becoming more prevalent, largely as a consequence of an ageing population. As people live longer, they have both increasing chances of ever being diagnosed with a chronic illness and of experiencing such illness over a prolonged period of time.[1] Compared to acute illnesses, chronic conditions are slow in their progression and often result in the emergence of new or increasingly pronounced functional limitations to which individuals must adjust (Dunlop, Lyons, Manheim, Song, & Chang, 2004). This is important, as undertaking large, recurrent and long-lasting life adjustments is a mentally straining process, and can have detrimental impacts on individuals' mental wellbeing (Holahan, Holahan, & Belk, 1984; Serido, Almeida, & Wethington, 2004). Consistent with this, a breadth of international research has documented robust links between living with a chronic illness and poor mental health (Hollingshaus & Utz, 2013; Polsky et al., 2005; Pudrovska 2010).

Within this context, it is important that we further our understanding of the factors that buffer the negative mental health consequences of living with a chronic illness. Previous research has made some inroads in this regard, providing evidence that social support and networks are important moderators of the relationship between chronic conditions and mental wellbeing (Schafer & Koltai, 2015). Partners are important sources of social support, and have the potential to buffer the impact of stressors associated with living with a chronic condition (Berkman & Glass, 2000; Cohen, 2004). In this paper, we extend previous studies of the protective effects of partnerships on health by incorporating a temporal component (Liu & Umberson, 2008; Rendall et al., 2011). Specifically, we consider whether and how any protective effects of having a partner on mental health evolve with time since illness onset. Because there are well-established gender differences in spousal caregiving, whereby wives typically provide more care than husbands (Noël-Miller, 2010), we also pay attention to potential gender differences.

Drawing on the stress process model, we develop a set of hypotheses and test these using nationally representative, panel data from the Household Income and Labour Dynamics in Australia (HILDA) Survey and random-effect panel regression models. Our findings indicate that being in a partnership, and particularly being in a marriage, is associated with comparatively better mental health amongst the chronically ill. This advantage remains beyond the year of illness onset, and is of a comparable magnitude for men and women. These findings bear important implications for mental health trends in modern societies in which the population is ageing, chronic illness is prevalent, and the incidence of marriage is in decline.

Theoretical background

The stress process and the dynamics of chronic illness and mental strain

Stress process theory and an associated body of evidence link individuals' social relations to their health outcomes (Pearlin, Menaghan, Lieberman, & Mullan, 1981; Pearlin, 1989). The stress process model pays particular attention to how 'stressors' (i.e. sources of stress) lead on to stress manifestations, of which poor mental health is a key one (Pearlin et al., 1981). Life events can be stressors, particularly if they are undesirable, uncontrollable or unscheduled, and if they lead on to life strains. The onset and maintenance of a chronic illness is a recognised stressor: the onset of a chronic condition is disruptive and uncontrollable, and it often leads to functional limitations on the self that produce prolonged strains. Consistent with this, a robust body of empirical evidence documents that individuals living with a chronic condition commonly report elevated levels of depressive symptoms (Hollingshaus & Utz, 2013; Polsky et al., 2005; Pudrovska 2010) and anxiety disorders (Clarke & Currie, 2009). Given this, we hypothesise that:

Hypothesis 1: Living with a chronic condition will be associated with poor mental health.

While this is not a new proposition, we revisit it in our panel data to (i) provide novel evidence that applies to the contemporary Australian context, and (ii) 'set the scene' to develop more innovative hypotheses that consider moderation by marital status and longitudinal dependencies. Concerning the latter, the stress process model also pays attention to different types of stressors depending on their temporal nature, distinguishing between 'eventful experiences' (i.e. the occurrence of discrete events that are disruptive to the self) and 'chronic strains' (i.e. the presence of relatively continuous problems) (Avison & Turner, 1988). The experience of chronic illness combines elements of both: while illness onset qualifies as an 'eventful experience', the physical pain and functional limitations associated with the condition qualify as 'chronic strains'. This suggests different scenarios concerning the association between chronic conditions and mental health and its possible changes with time since illness onset (De Ridder, Geenen, Kuijer, & van Middendorp, 2008; Kristofferzon, Lofmark, & Carlsson, 2003; Polsky et al., 2005). On the one hand, it is possible to expect a 'cumulative disadvantage' scenario: beyond the initial diagnosis, living with a chronic condition could become more stressful (De Ridder et al., 2008). The functional limitations associated with chronic conditions often change as the illness progresses, requiring not just initial but continuous adjustments in multiple life domains (Stanton, Revenson & Tennen, 2007). This is particularly so for degenerative chronic conditions, which entail more strenuous and costly adjustments over time (Cho, et al. 1998). As time since illness onset progresses, psychological distress amongst the chronically ill may also emerge due to protective resources being eroded, lost or depleted. For example, the costs of medication could produce financial constraints that in turn affect mental wellbeing. Altogether, this line of reasoning suggests that:

Hypothesis 2a: Chronic illnesses will have a less negative effect on mental health in the year of their onset than in subsequent years.

On the other hand, it is possible that the emotional consequences associated with the initial disruption produced by illness onset can take a particularly large emotional toll (Newman, Steed, & Mulligan, 2004). Learning that one has a chronic condition is a form of 'biographical disruption' affecting individuals' sense of self and personal identity (Bury, 1982), as they take on a new social role as a 'sick person' (Mechanic & Volkart, 1961) and become exposed to disease discourses that affect their emotions (Perry, 2011). For example, cancer diagnoses are often followed by external inquiries about what people had done to become sick, or whether they would adequately manage their illness (Willig, 2011). In this early stage after diagnosis, individuals may not yet have gathered the resources necessary to buffer the functional limitations associated with the health condition, or developed a capacity to mentally process and cope with the event. Pearlin's stress process model describes this phase as *"a period of readjustment during which the system struggles to re-establish a homeostasis"* and highlights how *"the struggle for readjustment can be wearing and exhausting, and under these conditions the organism becomes outstandingly vulnerable to stress and its physical and psychological consequences"* (Pearlin et al., 1981: 339). As time since illness onset progresses and resources that help buffer the negative consequences of the illness are obtained, the psychological wellbeing of chronically ill individuals could improve. Consistent with this, studies such as Polsky et al., (2005) find that depressive symptoms decrease with time since illness onset. Given this, and in contrast with Hypothesis 2a, we also hypothesise that:

Hypothesis 2b: Chronic illnesses will have a more negative effect on mental health in the year of their onset than in subsequent years.

The protective role of partners and time since illness onset

The stress process model highlights the importance of resources that can be invoked by individuals to protect themselves from the detrimental effects of stressors (Pearlin et al., 1981; Pearlin, 1989). Such resources include coping mechanisms (mastery and self-esteem) and social support. Partners are amongst the most integral relationships in adults' lives, and constitute a key source of social support and personal wellbeing (Coombs, 1991; House, Landis, & Umberson, 1988). Partnerships are particularly important as sources of social support against stressors because they are characterised by extensive relations and frequent interaction, and high levels of involvement and concern (Pearlin et al., 1981). Consistent with these theoretical propositions, an extensive body of evidence demonstrates the mental health benefits of partnerships and marriages, and the factors upon which this relationship is dependent (e.g. relationship duration and marital quality) (Frech & Williams, 2007; Williams, 2003). The positive health effects of having a partner often remain after accounting for the selection of healthy individuals into relationships (Guner, Kulikova & Llull, 2014; Murray, 2000; Rendall et al., 2011). In addition to its direct effects on mental health, being in an intimate relationship can also modify the severity of the detrimental consequences that negative life events and transitions can have on individuals' mental wellbeing. That is, partners can act as 'buffers'. For example, the negative mental health impacts of unemployment and job loss are felt less strongly by partnered individuals (Kasl & Jones, 2000). Generally, these protective effects are attributed to the emotional and financial support provided by one's partner, as well as their provision of health-related social control (August & Sorkin, 2010; Rendall et al., 2011; Umberson, 1992) and social integration and attachment (Umberson, 1987).

In the case of chronic illness and mental health, partners are an especially important source of support that may help alleviate the negative mental health effects of living with a chronic condition. On the one hand, partners can provide *intangible* types of support, such as emotional availability, being present when important information about the health condition is passed on, backing up important decisions (e.g. about treatment or employment participation), assist in managing medication use, and/or gathering information about how to best address functional limitations associated with the condition. On the other hand, partners can also provide more *tangible* types of support to meet the needs associated with the illness. This includes financial support to cover medication costs, help confronting reductions in labour income due to the illness, and assistance in daily activities (e.g. help walking, carrying things, or performing routine household tasks and housework). Individuals with chronic conditions receiving either kind of support may feel that they have the resources necessary to make successful life adjustments following the onset of a chronic illness, and suffer less psychological strain as a result (Berkman & Glass, 2000; Cohen & Wills, 1985; Helgeson & Cohen, 1996; Stanton, Revenson, & Tennen, 2007).

Some authors also report an additional buffer provided by marriage relative to *de facto* relationships. This is attributed to lower relationship commitment, satisfaction and expectations amongst individuals in *de facto* relationships compared to their married counterparts (Wiik, Bernhardt, & Noack, 2009; Wiik, Keizer, & Lappegård, 2012), which may result in a comparative advantage amongst married individuals in eliciting care and support from their partners (Noël-Miller, 2011). Based on these theoretical propositions, we hypothesise that:

Hypothesis 3: The negative mental health impacts of chronic conditions will be stronger amongst unpartnered than partnered individuals – particularly married individuals.

The role of a partner as a support source for people who live with a chronic condition may however change with time since illness onset (Noël-Miller, 2010). First, there is evidence that marital quality decreases with declines in spousal health (Booth & Johnson, 1994), and such changes may have implications for the willingness and ability of spouses to provide care and assistance. Second, partners who act as caregivers over a prolonged period of time may experience stress and burnout as a result (Adelman, Lyubov, Tmanova, Dion, & Lachs, 2014; Pitceathly and Maguire, 2003), which would also make them less likely to continue providing care, the same amount of care, or care of the same quality. Third, unpartnered individuals may adjust to living with a chronic condition through self-care, as well as drawing on friends and family for support, rendering the presence of a partner less salient as time since illness onset elapses. We thus develop a final research hypothesis:

Hypothesis 4: The protective effect of being partnered on the mental health of individuals living with a chronic condition will decline with time since illness onset.

Accounting for gender differences

The stress-buffering effects associated with having a partner may operate through emotional support (e.g. listening, showing affection, dealing with a partner's frustration) or instrumental support (e.g. help moving, cleaning oneself, preparing meals). However, there are important gender differences in the propensity to provide these supports, with women being more likely than men to do so (Moen, Robison & Fields, 1994). This is the product of the persistence of traditional gender ideologies and gender divisions of paid and unpaid labour (England, 2010), as well as of the normative association of 'emotion work' (i.e. intentional activities performed to improve another person's emotional wellbeing) with femininity (Thomeer, Reczek, & Umberson, 2015). Consistent with this, empirical research has demonstrated that women tend to receive less support from their partners when they are ill than men (Kristofferzon, Lofmark, & Carlsson, 2003; Noël-Miller, 2010). Furthermore, female partners are typically the primary source of support for male partners who are ill, but when female partners are ill they are more likely to receive outside help; for example, from adult children or formal care services (Katz, Kabeto, & Langa, 2000; Spitze & Ward, 2000). Women who live with chronic illnesses are also less likely than men to nominate their partner as their primary caregiver (Allen, Goldscheider, & Ciambrone, 1999). It is therefore possible that chronically ill women are less likely to receive support from their partners than chronically ill men, and we may thus expect smaller differences in mental health by marital status amongst women with chronic illness. This is consistent with findings indicating that, in elderly couples, marriage protects men but not women from depressive symptoms (Bierman, 2012).

Given the gender differences established in the literature, we will examine whether the expected associations outlined in our research hypotheses differ across men and women. In the next section, we introduce the data used to undertake our empirical analyses.

Dataset and variables

To examine the longitudinal associations between chronic conditions, marital status and mental health we leverage panel data from the

Household, Income and Labour Dynamics in Australia (HILDA) Survey. The HILDA Survey is a longitudinal survey comprising the period 2001–2016, and its sample is largely representative of the Australian population in 2001. In Wave 1 (2001), 7,682 households were selected for participation in the study following a complex, multi-stage sampling strategy. Information was then collected from *all* individuals age 15 and over living in the selected households using a mixture of self-complete questionnaires and computer-assisted face-to-face interviews. Since then, these individuals have been followed over time and data have been collected from them on an annual basis. New participants can join the HILDA Survey after the initial wave if they live in participating households and turn 15 years of age, or if they begin living with an existing sample member. If the new panel member marries or has a child with an existing sample member, he/she would also be tracked over time. The HILDA Survey has sample sizes ranging from 12,408 to 17,612 across its waves, and remarkably low attrition rates for international standards. For example, the attrition rate in its latest wave (Wave 15) was circa 5% (Summerfield et al., 2016). For more detailed information on the HILDA Survey, see Watson and Wooden (2012) and Summerfield et al. (2016).

The HILDA Survey is a unique dataset to test our research hypotheses because: (i) it contains repeated measurements of the mental health, chronic conditions, and marital status of the same individuals over time, which enables us to examine their longitudinal associations; (ii) it tracks individuals annually and over a prolonged observation window (15 years), which permits us to observe change in marital status and time since illness onset; (iii) it contains information from a large sample of individuals, which provides adequate statistical power; and (iv) it is nationally representative at baseline, which makes our results likely generalisable to the Australian population.

We use information from individuals who participated in waves 1 to 15 of the HILDA Survey, irrespective of whether or not these individuals reported having a chronic condition upon entering the panel. Our analytic sample includes all person-year observations without missing data on model variables. Where there was missing data in an independent or dependent variable, we dropped the person-year record containing the missing data, but retained other person-year records from the same respondent. Of 217,917 person-year observations in the HILDA Survey, 24,436 (or 11%) have missing data on model variables. Of these, 24,251 (or 99%) lack information on mental health, as this is asked within a self-complete questionnaire that incurs higher non-response than the face-to-face instrument. A bias analysis was undertaken by comparing the records dropped due to missing data and those retained for analysis. Given the very large sample size, differences in the sample means of the records dropped and retained were generally statistically significant at $p<0.05$ in t-tests (20 of 22 comparisons). However, differences in means were generally small in magnitude. Mean differences amounting to 10% or more of the variable's standard deviation indicated that, in the analytic sample, there was overrepresentation of individuals who were older, married, highly educated, non-Indigenous Australian or born in an English-speaking country and had two or more children, and underrepresentation of individuals who were younger, single, poorly educated, Indigenous Australian or born in a non-English-speaking country, and childless. In addition, derivation of a longitudinal measure of chronic conditions entails dropping information from individuals' first observation (n=12,917 observations; details below). Our final sample comprises 180,564 observations (96,138 for women and 84,426 for men) from 26,734 individuals (13,859 women and 12,877 men).

Our outcome of interests is respondents' mental health. To operationalise this, we use the Mental Health Inventory (MHI-5), which is a subscale of the Medical Outcomes Questionnaire Short-Form Health Survey (SF-36) (Ware & Sherbourne, 1992). The MHI-5 is a validated measure that is routinely used in the survey literature to capture overall mental health levels. It is an additive scale constructed by combining responses to five questions, which collectively tap the four major mental health dimensions – anxiety, depression, loss of emotional/behavioural control, and psychological wellbeing (Ware & Sherbourne, 1992). Questions ask how often in the past four weeks the respondent had (i) *"been a nervous person"*, (ii) *"felt so down in the dumps that nothing could cheer them up"*, (iii) *"felt calm and peaceful"*, (iv) *"felt down"* and (v) *"been a happy person"*. Possible responses are: (i) *"all of the time"*, (ii) *"most of the time"*, (ii) *"a good bit of the time"*, (iv) *"some of the time"*, (v) *"a little of the time"* and (vi) *"none of the time"*. As is typical in the literature, the resulting additive scale is then transformed so that it ranges from 0 (worst possible outcome) to 100 (best possible outcome). In the HILDA Survey, the MHI-5 questions are collected via a self-complete questionnaire, as to ensure that respondents' answers are not biased by the presence of an interviewer (e.g. through social desirability bias). In our sample, the average score in the MHI-5 is 74.2 (SD=17.2), 73 amongst women (SD=17.5) and 75.4 amongst men (SD=16.7).

The HILDA Survey includes information, updated annually, on whether respondents have a chronic condition. Specifically, as part of the face-to-face interview, HILDA Survey participants are asked the following yes/no question: *"Do you have any long-term health condition, impairment or disability (such as these) that restricts you in your everyday activities, and has lasted or is likely to last for 6 months or more, and cannot be corrected by medication or medical aids?"* Respondents are then presented with a showcard containing a list of 15 conditions, which are used as prompts. However, the question wording does not limit responses to these conditions. We first peruse this information to create a 'base' variable capturing the concurrent presence of a chronic condition. This is a dichotomous variable that takes the value zero if the respondent does not have a chronic condition (73%; n=131,277), and the value one if the respondent has a chronic condition (27%; n=49,287 observations). We then develop a more complex and insightful longitudinal measure of chronic conditions that splits time around illness onset into three stages. This takes the value zero if the respondent does *not* have a chronic condition (73%; n=131,277), the value one if the respondent has a chronic condition that emerged since the previous year (9%; n=16,998), and the value two if the respondent has a chronic condition that emerged more than one year before (18%; n=32,289). Individuals who previously reported having a chronic condition but no longer do so score zero in this variable. It is not possible to determine whether the chronic conditions observed in Wave 1 had their onset within the previous year, and so all observations from this survey wave are excluded from analysis.

The HILDA Survey contains time-varying information on individuals' marital status across all of its waves. We use this information to create a discrete variable separating each respondent observation into: (i) married (50%, n=89,477), (ii) in a *de facto* relationship (13%, n=24,675), (iii) divorced, separated or widowed (14%, n=24,635), and (iv) single (never married) (23%, n=41,777). This variable is used as a potential moderator of the relationship between the presence of a chronic condition and mental health in our regression models.

In our multivariate regression models we control for a set of factors known to be correlated with both the presence of a chronic condition and mental health, and which have been used in previous studies (Hollingshaus & Utz, 2013; Schnittker, 2005). These include respondent's age (in years, uncentered), number of children (none, one, two or more), highest educational qualification (below school Year 12, school Year 12, professional qualification, degree or higher), employment status (employed, unemployed, not in the labour force), household financial-year, disposable, regular income (adjusted to 2015 prices and expressed in AU$10,000s), and respondent's ethno-migrant background (Australian, not Indigenous; Australian, Indigenous; migrant, main English-speaking country; migrant, other country). Gender-specific means and standard deviations for all model variables are shown in table 1.

Table 1. Gender-specific sample means/percentages and standard deviations

	Mean or % (SD)		
	Women	Men	All
Outcome variable			
MHI-5 (transformed)	73.0 (17.5)	75.4 (16.7)	74.2 (17.2)
Key explanatory variable			
Has a chronic condition			
No	73%	73%	73%
Yes, on the year of onset	9%	9%	9%
Yes, on a subsequent year	18%	18%	18%
Marital status			
Married	48%	52%	50%
In a *de facto* relationship	13%	14%	13%
Divorced, separated or widowed	18%	9%	14%
Single (never married)	21%	25%	23%
Control variables			
Age (in years)	45.0 (18.6)	44.6 (18.4)	44.8 (18.5)
Number of children ever had			
Zero	31%	37%	34%
One	12%	11%	11%
Two or more	57%	52%	55%
Educational attainment			
Below Year 12	37%	30%	34%
Year 12	16%	14%	15%
Professional qualification	23%	36%	29%
Degree	24%	21%	22%
Employment status			
Employed	58%	70%	64%
Unemployed	3%	4%	4%
Not in the labour force	39%	26%	33%
Household income (in A$10,000s)	8.7 (6.7)	9.2 (6.8)	9.0 (6.8)
Ethno-migrant background			
Australian, not Indigenous	77%	76%	77%
Australian, Indigenous	2%	2%	2%
Migrant, main English-speaking country	9%	11%	10%
Migrant, other country	12%	11%	11%
N (observations)	96,138	84,426	180,564

Notes: HILDA Survey data, 2002–2015. Statistics are for person–year observations across all survey waves.

Estimation method and analytic approach

To examine the multivariate associations between chronic conditions, marital status and mental health, we estimate random-effect panel regression models that leverage the panel structure of the HILDA Survey data.[2] These random-effect models account for the nesting of observations within the same individuals, and their coefficients are estimated using a weighted average of the within-individual and between-individual variability in the panel data (Wooldridge 2010). Our base model, model 1, can be expressed as:

$$H_{it} = C_{it}\beta_1 + M_{it}\beta_2 + X_{it}\beta_3 + Z_i\beta_4 + u_i + e_{it} \quad (1)$$

where subscripts *i* and *t* denote individuals and time periods, respectively; *H* is a continuous variable capturing mental health; *C* is a categorical variable capturing the experience of a chronic condition that enters the model as a set of dummy variables; *M* is a categorical marital status variable that also enters the model as a set of dummy variables; *X* and *Z* are vectors of time-varying and time-constant explanatory variables, respectively; and the *βs* are the model coefficients to be estimated. There are two distinct error terms in this model: *u* is a person-specific error (i.e. a random intercept or random effect) capturing individual-specific heterogeneity, and is assumed to come from a normal distribution with a mean of zero and to be orthogonal to the observable variables, while *e* is the usual stochastic error term in regression.[3] The coefficients on the chronic condition and marital status dummy variables in this model are used to test hypotheses 1 and 2. The coefficients on the two dummy variables capturing time since illness onset and the three dummy variables denoting marital status that appear in the models are compared through Wald tests.

To test hypotheses 3 and 4 we estimate an additional model, model 2. This includes interaction terms between the measures of marital status and chronic conditions:

$$H_{it} = C_{it}\beta_1 + M_{it}\beta_2 + (C_{it} * M_{it})\beta_3 + X_{it}\beta_4 + Z_i\beta_5 + u_i + e_{it} \quad (2)$$

The estimated coefficients on the interactions between the chronic condition (*C*) and marital status (*M*) variables, i.e. β_3 in equation (2), are used to assess whether there is evidence of moderation (hypothesis 3). To test whether moderation differs by time since illness onset (hypothesis 4), we compare the interaction effects between marital status and the experience of a chronic condition on the year of onset, with the interaction effects between marital status and the experience of a chronic condition in subsequent years. Again, this is accomplished via Wald tests.

Both models are estimated separately for men and women because the correlates of mental health are known to differ by gender (Pincinelli & Wilkinson, 2000; van de Velde, Bracke & Levecque, 2010). We test for gender differences in the estimated effects using analogous models in which all covariates are interacted with gender (see e.g. Gordon, 2015: 324). In these models, the *p*-values on the interactions between gender and the covariates provide the requisite evidence to assess effect heterogeneity by gender.

Empirical evidence

Dynamic effects of chronic conditions on mental health

Results from model 1 in table 2 indicate that, when women have a chronic condition on the year of onset, they report worse mental health than when they do not have a condition (β=–4.00; *p*<0.001). The adverse effects of having a chronic condition are even stronger when women are observed at a later stage after illness onset (β=–5.44, p<0.001). Wald tests confirm that this difference is statistically significant (*p*<0.001). There is also analogous evidence of differences in mental health with time since illness onset in the male sample. Men's average mental health is 3.2 units (*p*<0.001) lower when they are in the year of onset of a chronic condition, compared to when they have no chronic conditions. For a chronic condition observed at a subsequent year, the drop in mental health is even more pronounced, of 4.9 units (*p*<0.001). Again, Wald tests confirm that this difference is statistically significant (*p*<0.001).

Altogether, these results are consistent with hypotheses 1 (*Living with a chronic condition will be associated with poor mental health*) and 2a (*Chronic illnesses will have a less negative effect on mental health in the year of their onset than in subsequent years*). For both men and women, living with a chronic condition is associated with decreases in mental health, and more so at later stages than at the year of onset. Concerning gender differences, Wald tests reveal that the negative impact on mental health of having a chronic condition on the year of onset is stronger for women than men ($p<0.001$). However, there are no gender differences in mental health for the experience of chronic conditions at subsequent stages ($p>0.1$).

The mental health premium of partnerships revisited

Results from model 1 are also used to establish whether or not the mental health premium associated with partnerships, particularly, marriages, identified in previous research is also apparent in our Australian sample. The estimated coefficients on the marital status dummy variables in model 1 indicate that women who are divorced, separated or widowed ($\beta=-2.71$; $p<0.001$), single ($\beta=-1.72$; $p<0.001$), or in a *de facto* relationship ($\beta=-0.94$; $p<0.001$) report poorer mental health than married women. Similarly, men who are divorced, separated or widowed ($\beta=-2.90$; $p<0.001$), single ($\beta=-1.65$; $p<0.001$) or in a *de facto* relationship ($\beta=-0.62$; $p<0.01$) report poorer mental health than married men. Results from Wald tests further reveal that men and women in *de facto* relationships report better mental health than their counterparts who are single or divorced, separated or widowed ($p<0.01$ to $p<0.001$ across all pairwise comparisons).

Therefore, model 1 provides evidence that is consistent with previous literature: for both men and women there is a mental health premium associated with being partnered, which is stronger for married individuals. Results of Wald tests comparing the predicted effects on the marital status dummy variables between men and women yielded no evidence of gender differences.

Marital status as a dynamic moderator of the chronic condition/mental health association

Model 2 is used to test our remaining hypotheses. The results for women provide evidence of moderation in the effects of our measure of chronic conditions on mental health by marital status and that, to some extent, the pattern of moderation varies with time since illness onset. Five of the six interaction terms are statistically significant, confirming that being married (the reference category) is an important moderator of the chronic illness/mental health association. However, Wald tests provide little evidence of moderation for *de facto* relationships, as few of these tests are statistically significant. To get an overall picture of the results, it is helpful to interpret the main and interaction effects in conjunction. When doing so, the mental health impacts of being in the onset year of a chronic condition (compared to not having a condition) are largest when women are single (–5.6 units), followed by when they are divorced, separated or widowed (–3.9 units), in a *de facto* relationship (–5 units), and married (–3.1 units). This ordering changes slightly when one considers the mental health impacts of being in a subsequent year of a chronic condition (compared to not having a condition). For this category, the largest mental health ‘penalties’ occur when women are single (–6.7 units), followed by when they are in a *de facto* relationship (–6 units), divorced, separated or widowed (–5.4 units), and married (–4.9 units).

There is also evidence of moderation by marital status amongst men. As for women, the pattern of results provides strong evidence of moderation for marriage, and only weak evidence for *de facto* relationships. Taking the main and interaction effects together, being in the onset year of a chronic condition takes the greatest toll on men’s mental health when they are single (–4.5 units), followed by when they are divorced, separated or widowed (–3.9 units), in a *de facto* relationship (–3.8 units), and married (–2.4 units). Chronic conditions observed after the year of onset have the largest negative effects on men’s mental health when they are single (–6.9 units), followed by when they are in a *de facto* relationship (–6.2 units), married (–4.4 units), and divorced, separated or widowed (–4.1 units). The results of model 2 are visually represented in figure 1 as marginal effects, with covariate values set at their means and random effects at zero.

The protection on mental health of being married (compared to being single) was stronger when individuals experienced chronic conditions than when they did not. While this marriage protection effect was apparent amongst women (–3.8 units) and men (–2.1 units) experiencing no such conditions, its magnitude was larger for women (–6.2 units) and men (–4.1 units) in the year of onset of a condition, and for women (–5.6 units) and men (–4.6 units) experiencing a condition beyond the year of onset.

Altogether, these results provide mixed evidence for our third hypothesis (*The negative mental health impacts of chronic conditions will be stronger*

Table 2. **Random-effect models of mental health**

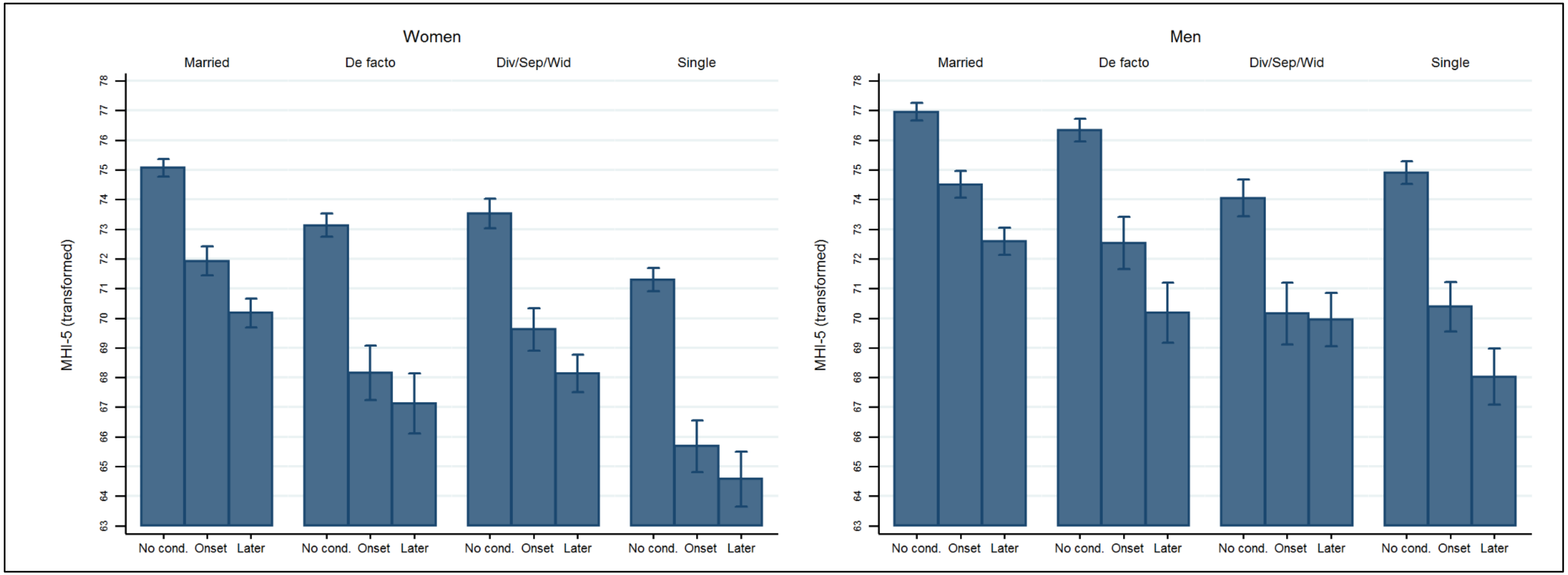
	Model 1			Model 2		
	Women	Men	Gender diff.	Women	Men	Gender diff.
Has a chronic condition *(ref. No)*						
On the year of onset	-4.00***	-3.21***	***	-3.14***	-2.44***	*
On a subsequent year	-5.44***	-4.94***	n.s.	-4.90***	-4.36***	n.s.
Marital status (*ref. Married*)						
In a *de facto* relationship	-0.94***	-0.62**	n.s.	-0.65**	-0.31	n.s.
Divorced, separated or widowed	-2.71***	-2.90***	n.s.	-2.57***	-2.90***	n.s.
Single (never married)	-1.72***	-1.65***	n.s.	-1.34***	-1.23***	n.s.
Marital status & chronic condition interactions						
Condition, onset * In a *de facto* relationship				-1.84***	-1.37**	n.s.
Condition, onset * Divorced, separated or widowed				-0.77(*)	-1.45**	n.s.
Condition, onset * Single (never married)				-2.48***	-2.07***	n.s.
Condition, no onset * In a *de facto* relationship				-1.12*	-1.80***	n.s.
Condition, no onset * Divorced, separated or widowed				-0.49	0.26	n.s.
Condition, no onset * Single (never married)				-1.83***	-2.51***	n.s.
Control variables						
Age (in years)	0.14***	0.08***	***	0.14***	0.08***	***
Number of children ever had (*ref. Zero*)						
One	0.14	-1.02***	***	0.19	-0.96***	***
Two or more	-1.05***	-1.44***	n.s.	-1.01***	-1.39***	n.s.
Educational attainment (*ref. Degree*)						
Below Year 12	-1.32***	0.87**	***	-1.36***	0.84**	***
Year 12	-1.06***	0.29	***	-1.10***	0.26	***
Professional qualification	-1.05***	0.09	**	-1.08***	0.08	**
Employment status (*ref. Employed*)						
Unemployed	-1.51***	-2.70***	**	-1.49***	-2.68***	**
Not in the labour force	-1.03***	-1.48***	*	-1.03***	-1.49***	*
Household income (in A$10,000s)	0.05***	0.08***	**	0.05***	0.08***	**

Table 2. Random-effect models of mental health (cont.)

Ethno-migrant background *(ref. Australian, not Ind.)*						
Australian, Indigenous	-4.47***	-1.90*	*	-4.39***	-1.84*	*
Migrant, main English-speaking country	0.24	0.59	n.s.	0.22	0.59	n.s.
Migrant, other country	-2.70***	-2.76***	n.s.	-2.74***	-2.79***	n.s.
Wald tests						
β(cond_onset)=β(cond_later)	***	***		***	***	
β(*de facto*)=β(divorced)	***	***		***	***	
β(*de facto*)=β(single)	***	***		**	***	
β(divorced)=β(single)	**	***		***	***	
β(cond_onset**de facto*)=β(cond_onset*divorced)				(*)	n.s.	
β(cond_onset**de facto*)=β(cond_onset*single)				n.s.	n.s.	
β(cond_onset*divorced)=β(cond_onset*single)				**	n.s.	
β(cond_later**de facto*)=β(cond_later*divorced)				n.s.	**	
β(cond_later**de facto*)=β(cond_later*single)				n.s.	n.s.	
β(cond_later*divorced)=β(cond_later*single)				*	***	
N (observations)	96,138	84,426		96,138	84,426	
N (individuals)	13,859	12,877		13,859	12,877	
R^2	0.097	0.083		0.097	0.084	

Notes: HILDA Survey data, 2002–2015. Outcome variable: MHI-5 (transformed to range from 0 to 100). Standard errors are clustered for the nesting of observations within households. Gender differences are tested via models in which all variables are interacted with gender. Significance levels: (*) p<0.1, * p<0.05, ** p<0.01, *** p<0.001, n.s. *p*>0.1.

Figure 1. Predicted mental health by gender, marital status and time since illness onset



Notes: HILDA Survey data, 2002–2015. Whiskers denote 95% confidence intervals. Based on results from models 3 (women) and 4 (men) in table 2. Values of the covariates are set at their means and random effects are set at zero.

amongst unpartnered than partnered individuals – particularly married individuals) and our fourth hypothesis (*The protective effect of being partnered on the mental health of individuals living with a chronic condition will decline with time since illness onset*). For both men and women, being married (and to a lesser extent being in a *de facto* relationship) buffers the negative mental health effects of living with a chronic condition. This is consistent with hypothesis 3. However, against the predictions in hypothesis 4, we find little evidence that the protective effect of partnerships on mental health changes as time since illness onset progresses.

Discussion and conclusion

In this paper, we have drawn on the stress process model to examine the longitudinal associations between marital status, chronic conditions and mental health. We innovated by testing the role of partnership as a factor moderating the negative association between chronic conditions and mental health in a longitudinal context and considering gender differences. Our empirical analyses relied on 15 years of panel data from a national sample, the Household, Income and Labour Dynamics in Australia Survey, and random-effect panel regression models.

We began our empirical analysis by testing a confirmatory hypothesis on the direct effects of chronic conditions on mental health. This posed that living with a chronic condition should be associated with poor mental health (hypothesis 1). Consistent with previous studies and in favour of this hypothesis, we found that in our sample chronic conditions were indeed related to poorer mental health for both men and women (Hollingshaus & Utz, 2013; Polsky et al., 2005; Pudrovska, 2010). Hypotheses 2a and 2b delved into the longitudinal dimensions of this association: hypothesis 2a suggested a 'cumulative disadvantage' scenario (chronic illnesses having *less* negative effects on mental health in the year of onset than in subsequent years), whereas hypothesis 2b suggested a 'biographical disruption' scenario (chronic illnesses having *more* negative effects on mental health in the year of onset than in subsequent years). While previous studies have provided mixed findings regarding the dynamics of chronic illness and mental health (De Ridder et al., 2008; van't Spijker, Trijsburg & Duivenvoorden, 1997), our findings are clearly consistent with a 'cumulative disadvantage' situation: for both men and women, respondents reported worse mental health levels beyond the year of the illness onset, *ceteris paribus*. Concerning effect magnitude, for chronic conditions experienced in the year of onset, the estimates amount to 23% (women) and 19% (men) of a standard deviation in self-reported mental health. The analogous figures for chronic conditions experienced in subsequent years are 31% (women) and 30% (men) of a standard deviation.

Our results were also telling in regards to how being partnered, and particularly being married, is associated with a mental health premium in Australia. We found evidence that, all else being equal, men and women in partnerships reported better mental health levels relative to men and women who were single, divorced, separated and widowed (Rendall et al., 2011). There were also statistically significant differences in mental health levels between marital and *de facto* unions, with men and women in *de facto* relationships reporting poorer mental health than their married counterparts. These differences in mental health by marital status were generally substantially significant. For example, the coefficient on being divorced, separated or widowed amounts to about 15% of the measure's standard deviation for women and 17% for men.

Next, we innovated by explicitly considering whether and how marital status acts as a moderator of the chronic conditions/mental health nexus (hypothesis 3), and whether the stress-buffering role of partners persists with time since illness onset (hypothesis 4). As predicted by the stress process model and consistent with hypothesis 3, we found that for both men and women the negative mental health impacts of chronic conditions were stronger amongst unpartnered than partnered individuals. As such, partners do seem to act as protective resources or 'stress buffers' in the face of chronic conditions. While this applied to both marital and *de facto* unions, the estimated protective effects were more pronounced amongst the former than the latter. Importantly, the marriage protection effect was stronger amongst women and men experiencing a chronic condition, irrespective of its stage, than amongst women and men with no conditions.

When considering temporal dynamics in the panel data, we found less support for hypothesis 4: for both men and women, the protective effect of partnerships against the negative mental health effects of chronic conditions remained stable with time since illness onset. This suggests that mental health advantages amongst the chronically ill conferred by their partners are durable. The magnitude of the interaction effects between marital status and chronic illnesses was not negligible. For example, the protective effect of being married relative to single in the year of illness onset amounts to between 14% (women) and 12% (men) of a standard deviation in the mental health measure. The analogous figures for subsequent years after illness onset were 10% (women) and 15% (men). The size of these interaction effects is thus comparable to or stronger than those of factors to which the literature has paid more attention. For example, it is similar to the estimated effects of employment status and greater than those of educational qualifications and the number of children. Altogether, these findings highlight the importance of considering partners and the support they confer as a continuing resource that has the potential to ease the deleterious effects of having a chronic condition on mental health. Such protective effects have significant ramifications, as mental health is a crucial factor promoting adherence to medical recommendations amongst individuals with a chronic condition and has implications for disease management, recovery and mortality (DiMatteo, Lepper, & Croghan, 2000; Carney, Freeland, Miller, & Jaffe, 2002).

Given known differences in the emotional and instrumental resources that men and women bring into relationships, we tested for gender differences. We found some evidence of gender differences in the negative impact on mental health of having a chronic condition on the year of onset, which was stronger for women than men. However, this difference faded at subsequent years. There was however only weak evidence of differences by gender in the direct effect of being in a partnership on mental health, or in the protective effects of partnerships on the mental health of the chronically ill. Concerning the latter, the protective effects of marriage relative to singlehood and *de facto* relationships seemed to become stronger in the years after illness onset amongst men, but weaker amongst women. This pattern of results is consistent with reports of gender differences in spousal caregiving, with women being more likely than men to provide care and to provide greater amounts of care (Moen, Robison, & Fields, 1994; Goldzweig et al., 2009; Katz et al., 2000; Noël-Miller, 2010; Spitze & Ward, 2000). However, in our data, these differences were neither statistically nor substantially significant. We therefore conclude that the relationships of interest were generally consistent for men and women in our Australian sample.

Despite our several contributions to the literature, there are limitations to our study that point towards avenues for future refinement. First, as with others before us (see e.g. Hollinghaus & Utz, 2013; Patten, 1999; Schnittker, 2005), we could only use a coarse, binary measure of whether or not individuals have a chronic condition. This did not distinguish the number of conditions, their nature, or the severity of the symptoms. Hence, our results are a broad starting point, and may mask heterogeneity in how marital status and mental health relate to each other across different types and experiences of chronic conditions. Second, while we are amongst the first to study the relationships of interest longitudinally, data shortcomings forced us to simplify time since illness onset into two components: (i) the year of onset and (ii) subsequent years. This is chiefly due to a lack of information on the specific year in which a chronic condition had its onset for individuals who entered the panel with a pre-existing chronic condition. As a result, we could not discern more finely grained trends in mental health without losing a large portion of our analytic sample. Third, while our results are informative of the main and interactive effects of chronic conditions and marital status on mental health, they are silent about the mechanisms driving these associations. We interpret our findings as suggesting that partnerships (and especially marriages) are an important source of emotional, instrumental and/or financial support for the chronically ill. Subsequent studies may delve further into the types of support that partners provide, and which of these are more crucial to the mental health of their significant others. Qualitative studies may be particularly well-suited for this task. Fourth, the results reported here are associations, and may not represent causal relationships. In particular, it is possible that some of the protective effects of partnerships/marriages are due to selection into these states. For example, individuals who enjoy better physical and mental health may be more likely to find and retain a partner. However, recent research finds that the positive health effects of having a partner often remain after accounting for such selection (Blekesuane, 2008; Guner, Kulikova, & Llull, 2014), suggesting that selection is unlikely to single-handedly explain our results.

Despite these limitations, our findings have significant implications for health policy and practice. The pattern of results found for partnerships (particularly marriages) indicates that being diagnosed and living with a chronic illness has more detrimental effects on the mental health of people who are unpartnered. The protective effect of partnerships was also found to be durable. These are policy-relevant findings, as they point towards the need for institutional intervention to ensure that the mental health levels of unpartnered individuals remain stable over the duration of their illness. Provision of external support to these individuals to compensate for the absence of a partner should be considered. Interventions might include redefining laws and policies that disadvantage unpartnered individuals (e.g. insurance policies), implementation of support groups and information sessions where individuals learn about how to manage their illness, or direct provision of care or economic transfers to cover its cost by government (Helgeson & Cohen, 1996; Meyer & Mark, 1995).

While the magnitude of the individual effects on the protective effects of partnerships in our findings is moderate, the population accumulation of such effects constitutes a public health problem. The associated economic and social costs at the national level are also likely to grow in the future: the population in advanced economies such as Australia is ageing, chronic illness is prevalent, and the incidence of partnerships (particularly marriages) is in decline (Australian Bureau of Statistics, 2015; Jain, 2007; Kennedy & Ruggles, 2014). Collectively, these patterns suggest that, while population ageing may lead to more people living with chronic illness and for longer spells of time, the benefits conferred by having a partner in buffering the mental strain of a chronic condition will apply to a shrinking portion of the population. Addressing these issues is thus an important challenge for social and public health policy, and will require more scholarly attention.

Acknowledgements

This paper uses unit record data from the Household, Income, and Labour Dynamics in Australia (HILDA) Survey. The HILDA Project was initiated and is funded by the Australian Government Department of Social Services (DSS) and is managed by the Melbourne Institute of Applied Economic and Social Research (Melbourne Institute). The findings and views reported in this paper, however, are those of the authors and should not be attributed to either DSS or the Melbourne Institute. This research was supported by the Australian Research Council Centre of Excellence for Children and Families over the Life Course (project number CE140100027). The Centre is administered by the Institute for Social Science Research at The University of Queensland, with nodes at The University of Western Australia, The University of Melbourne and The University of Sydney.

References

Adelman, R. D., Lyubov L., Tmanova, D. D., Dion, S., & Lachs, M. (2014). Caregiver Burden: A Clinical Review. *JAMA*, *311*(10), 1052–1060. https://doi.org/10.1001/jama.2014.304

Allen, S. M., Goldscheider, F., & Ciambrone, D. A. (1999). Gender roles, marital intimacy, and nomination of spouse as primary caregiver. *The Gerontologist*, *39*(2), 150–158. https://doi.org/10.1093/geront/39.2.150

August, K. J. & Sorkin, D. H. (2010). Marital Status and Gender Differences in Managing a Chronic Illness: The Function of Health-Related Social Control. *Social Science and Medicine*, *71*(10), 1831–1838. https://doi.org/10.1016/j.socscimed.2010.08.022

Australian Bureau of Statistics (ABS). (2015). Marriages and Divorces, Australia, 2014 (cat. no. 331.0). Canberra: ABS.

Australian Institute of Health and Welfare (AIHW). (2014). Chapter 4. Chronic Disease—Australia's Biggest Health Challenge (Australia's health series no.14, Cat.No.AUS178). Canberra: AIHW.

Avison, W. R. & Turner, R. J. (1988). Stressful Life Events and Depressive Symptoms: Disaggregating the Effects of Acute Stressors and Chronic Strains. *Journal of Health and Social Behavior*, *29*(3), 253–264. https://doi.org/10.2307/2137036

Berkman, L. F. & Glass, T. (2000). Social Integration, Social Networks, Social Support, and Health. *Social Epidemiology*, *1*, 137–173.

Bierman, A. (2012). Functional Limitations and Psychological Distress: Marital Status as Moderator. *Society and Mental Health*, *2*(1), 35–52. https://doi.org/10.1177/2156869312442884

Blekesuane, M. (2008). Partnership Transitions and Mental Distress: Investigating Temporal Order. *Journal of Marriage and Family*, *70*(4), 879–890. https://doi.org/10.1111/j.1741-3737.2008.00533.x

Booth, A. & Johnson, D. R. (1994). Declining Health and Marital Quality. *Journal of Marriage and Family*, *56*(1), 218–223. https://doi.org/10.2307/352716

Bury, M. (1982). Chronic Illness as Biographical Disruption. *Sociology of Health and Illness*, *4*(2), 167–82. https://doi.org/10.1111/1467-9566.ep11339939

Carney, R. M., Freedland, K., Miller, G., & Jaffe, A. (2002). Depression as a Risk Factor for Cardiac Mortality and Morbidity: A Review of Potential Mechanisms. *Journal of Psychosomatic Research*, *53*, 897–902. https://doi.org/10.1016/S0022-3999(02)00311-2

Cho, C.-Y., Alessi, C., Cho, M., Aronow, H., Stuck, A., Rubenstein, L., & Beck, J. (1998). The Association between Chronic Illness and Functional Change among Participants in a Comprehensive Geriatric Assessment Program. *Journal of the American Geriatrics Society*, *46*(6), 677–682. https://doi.org/10.1111/j.1532-5415.1998.tb03800.x

Clarke, D. M. & Currie, K. C. (2009). Depression, Anxiety and their Relationship with Chronic Diseases: A Review of the Epidemiology, Risk and Treatment Evidence. *The Medical Journal of Australia*, *190*(7), 54–60.

Cohen, S. (2004). Social Relationships and Health. *American Psychologist*, *59*(8), 676–684. https://doi.org/10.1037/0003-066X.59.8.676

Cohen, S. & Wills, T. A. (1985). Stress, Social Support, and the Buffering Hypothesis. *Psychological Bulletin*, *98*(2), 310–357.

Coombs, R. H. (1991). Marital Status and Personal Well-Being: A Literature Review. *Family Relations*, *40*(1), 97–102. https://doi.org/10.2307/585665

De Ridder, D., Geenen, R., Kuijer, R., & van Middendorp, H. (2008). Psychological Adjustment to Chronic Disease. *The Lancet*, *372*(9634), 246–55. https://doi.org/10.1016/S0140-6736(08)61078-8

DiMatteo, M. R., Lepper, H., & Croghan, T. W. (2000). Depression is a Risk Factor for Noncompliance with Medical Treatment: Meta-Analysis of The Effects of Anxiety and Depression on Patient Adherence. *Archives of Internal Medicine*, *160*(14), 2101–7. https://doi.org/10.1001/archinte.160.14.2101

Dunlop, D. D., Lyons, J. S., Manheim, L., Song, J. & Chang, R. W. (2004). Arthritis and Heart Disease as Risk Factors for Major Depression: The Role of Functional Limitation. *Medical Care*, *42*(6), 502–511. https://doi.org/10.1097/01.mlr.0000127997.51128.81

England, P. (2010). The Gender Revolution: Uneven and Stalled. *Gender & Society*, *24*(2), 149–166. https://doi.org/10.1177/0891243210361475

Essue, B., Kelly, P., Roberts, M., Leeder, S., & Jan, S. (2011). We Can't Afford My Chronic Illness! The Out-Of-Pocket Burden Associated with Managing Chronic Obstructive Pulmonary Disease in Western Sydney, Australia. *Journal of Health Services Research & Policy*, *16*(4), 226–231. https://doi.org/10.1258/jhsrp.2011.010159

Frech, A., & Williams, K. (2007). Depression and the Psychological Benefits of Entering Marriage. *Journal of Health and Social Behavior*, *48*(2), 149–163. https://doi.org/10.1177/002214650704800204

Goldzweig, G., Hubert, A., Walach, N., Bremmer, B., Perry, S., Andritsch, E., & Baider, L. (2009). Gender and Psychological Distress among Middle- and Older-Aged Colorectal Cancer Patients and their Spouses: An Unexpected Outcome. *Critical Reviews in Oncology/Hematology*, *70*(1), 71–82. https://doi.org/10.1016/j.critrevonc.2008.07.014

Gordon, R. (2015). *Regression Analysis for the Social Sciences (2nd Edition)*. New York: Routledge.

Guner, N., Kulikova, Y., & Llull, J. (2014). *Does Marriage Make You Healthier?* (IZA Discussion Paper No. 8633). Bonn: IZA.

Helgeson, V. S. & Cohen, S. (1996). Social Support and Adjustment to Cancer: Reconciling Descriptive Correlational, and Intervention Research. *Healthy Psychology*, *15*(2), 135–48. https://doi.org/10.1037/0278-6133.15.2.135

Holahan, C. K., Holahan, C., & Belk, S. S. (1984). Adjustment in Aging: The Roles of Life Stress, Hassles and Self-Efficacy. *Health Psychology*, *3*(4), 315–328. https://doi.org/10.1037/0278-6133.3.4.315

Hollingshaus, M. S. & Utz, R. (2013). Depressive Symptoms Following the Diagnosis of Major Chronic Illness. *Society and Mental Health*, *3*(1), 22–39. https://doi.org/10.1177/2156869312464788

House, J, S., Landis, K., & Umberson, D. (1988). Social Relationships and Health. *Science*, *241*(4865), 540–5. https://doi.org/10.1126/science.3399889

Jain, S. (2007). *Lifetime Marriage and Divorce Trends*. (Australian Social Trends, 2007). Canberra: Australian Bureau of Statistics.

Kasl, S. V. & Jones, B. A. (2000). The Impact of Job Loss and Retirement on Health. In: L. F. Berkman & I. Kawachi (eds.). *Social Epidemiology*. New York: Oxford University Press, *pp.* 118–136.

Katz, S. J., Kabeto, M., & Langa, K. M. (2000). Gender Disparities in the Receipt of Home Care for Elderly People with Disability in the United States. *JAMA*, *284*, 3022–3027. https://doi.org/10.1001/jama.284.23.3022

Kennedy, S. & Ruggles, S. (2014). Breaking up is hard to count: The rise of divorce in the United States, 1980–2010. *Demography*, *51*(2), 587–598. https://doi.org/10.1007/s13524-013-0270-9
Kristofferzon, M.-L., Lofmark, R., & Carlsson, M. (2003). Myocardial Infarction: Gender Differences in Coping and Social Support. *Journal of Advanced Nursing*, *44*(4), 360–374. https://doi.org/10.1046/j.0309-2402.2003.02815.x
Liu, H. & Umberson, D. (2008). The times they are changin': Marital status and health differentials from 1972 to 2003. *Journal of Health and Social Behavior*, *49*, 239–253. https://doi.org/10.1177/002214650804900301
Mechanic, D. & Volkart, E. H. (1961). Stress, Illness Behavior, and the Sick Role. *American Sociological Review*, *26*(1), 51–58. https://doi.org/10.2307/2090512
Meyer, T. J. & Mark, M. M. (1995). Effects of psychosocial interventions with adult cancer patients: A meta-analysis of randomized experiments. *Health Psychology*, *14*(2), 101–108. https://doi.org/10.1037/0278-6133.14.2.101
Moen, P., Robison, J., & Fields, V. (1994). Women's Work and Caregiving Roles: A Life Course Approach. *The Journals of Gerontology. Series B, Psychological Sciences and Social Sciences*, *49*(4), S176–S186. https://doi.org/10.1093/geronj/49.4.S176
Murray, J. E. (2000). Marital protection and marital selection: Evidence from a historical-prospective sample of American men. *Demography*, *37*, 511–521. https://doi.org/10.1353/dem.2000.0010
Newman, S., Steed, L., & Mulligan, K. (2004). Self-Management Interventions for Chronic Illness. *The Lancet*, *364*(9444), 1523–37. https://doi.org/10.1016/S0140-6736(04)17277-2
Noël-Miller, C. (2010). Longitudinal Changes in Disabled Husbands' and Wives' Receipt of Care. *The Gerontologist, 50*(5), 681–693. https://doi.org/10.1093/geront/gnq028
Noël-Miller, C. (2011). Partner Caregiving in Older Cohabiting Couples. *The Journals of Gerontology. Series B, Psychological Sciences and Social Sciences*, *66B*(3), 341–53. https://doi.org/10.1093/geronb/gbr027
Patten, S. B. (1999). Long-Term Medical Conditions and Major Depression in the Canadian Population. *Canadian Journal of Psychiatry*, *44*(2), 151–157. https://doi.org/10.1177/070674379904400205
Pearlin, L. I. (1989). The Sociological Study of Stress. *Journal of Health and Social Behavior*, *30*(3), 241–256. https://doi.org/10.2307/2136956
Pearlin, L. I., Menaghan, E. G., Lieberman, M. A., & Mullan, J. T. (1981). The Stress Process. *Journal of Health and Social Behavior*, *22*(4), 337–356. https://doi.org/10.2307/2136676
Perry, B. L. (2011). The Labeling Paradox: Stigma, the Sick Role, and the Social Networks in Mental Illness. *Journal of Health and Social Behavior*, *52*(4), 460–477. https://doi.org/10.1177/0022146511408913
Pincinelli, M., & Wilkinson, G. (2000). Gender differences in depression: Critical review. *British Journal of Psychiatry*, *177*(6), 486–492. https://doi.org/10.1192/bjp.177.6.486
Pitceathly, C. & Maguire, P. (2003). The Psychological Impact of Cancer on Patients' Partners and Other Key Relatives: A Review. *European Journal of Cancer*, *39*(11), 1517–1524. https://doi.org/10.1016/S0959-8049(03)00309-5
Polsky, D., Doshi, J., Marcus, S., Oslin, D., Rothbard, A., Thomas, N., & Thompson, C. L. (2005). Long-Term Risk for Depressive Symptoms after a Medical Diagnosis. *Archives of Internal Medicine*, *165*(11), 1260–66. https://doi.org/10.1001/archinte.165.11.1260
Pudrovska, T. (2010). Why Is Cancer More Depressing for Men than Women among Older White Adults? *Social Forces*, *89*(2), 535–558. https://doi.org/10.1353/sof.2010.0102
Rendall, M., Weden, M. M., Favreault, M. M., & Waldron, H. (2011). The Protective Effect of Marriage for Survival: A Review and Update. *Demography, 48*(2), 481–506. https://doi.org/10.1007/s13524-011-0032-5

Schafer, M. H. & Koltai, J. (2015). Cancer Diagnosis and Mental Health among Older White Adults: Moderating Role for Social Networks. *Society and Mental Health*, *5*(3), 282–202. https://doi.org/10.1177/2156869315577631
Schnittker, J. (2005). Chronic Illness and Depressive Symptoms in Late Life. *Social Science & Medicine*, *60*, 13–23. https://doi.org/10.1016/j.socscimed.2004.04.020
Serido, J., Almeida, D. M., & Wethington, E. (2004). Chronic Stressors and Daily Hassles: Unique and Interactive Relationships with Psychological Distress. *Journal of Health and Social Behavio*r, *45*(1), 17–33. https://doi.org/10.1177/002214650404500102
Spitze, G. & Ward, R. R. (2000). Gender, Marriage, and Expectations for Personal Care. *Research on Aging*, *22*(5), 451–469. https://doi.org/10.1177/0164027500225001
Stanton, A. L., Revenson, T. A., & Tennen, H. (2007). Health Psychology: Psychological Adjustment to Chronic Disease. *Annual Review of Psychology*, *58*, 565–92. https://doi.org/10.1146/annurev.psych.58.110405.085615
Summerfield, M., Freidin, S., Hahn, M., La, N, Li, N., Macalalad, N., O'Shea, M., Watson, N., Wilkins, R. & Wooden, M. (2016). *HILDA User Manual – Release 15*. Melbourne Institute of Applied Economic and Social Research, University of Melbourne.
Thomeer, M. B., Reczek, C., & Umberson, D. (2015). Gendered Emotion Work around Physical Health Problems in Mid- and Later-life Marriages. *Journal of Aging Studies*; *32*, 12–22. https://doi.org/10.1016/j.jaging.2014.12.001
Umberson, D. (1987). Family Status and Health Behaviors: Social Control as a Dimension of Social Integration. *Journal of Health and Social Behavior*, *28*(3), 306–319. https://doi.org/10.2307/2136848
Umberson, D. (1992). Gender, marital status, and the social control of health behaviour. *Journal of Health and Social Behavior*, *34*, 907–917.
van de Velde, S., Bracke, P., & Levecque, K. (2010). Gender differences in depression in 23 European countries. Cross-national variation in the gender gap in depression. *Social Science & Medicine*, *71*(2), 305–313. https://doi.org/10.1016/j.socscimed.2010.03.035
van't Spijker, A., Trijsburg, R. W., & Duivenvoorden, H. J. (1997). Psychological Sequelae of Cancer Diagnosis: A Meta-Analytical Review of 58 Studies after 1980. *Psychosomatic Medicine*, *59*(3), 280–293. https://doi.org/10.1097/00006842-199705000-00011
Ware, J. E., & Sherbourne, C. D. (1992). The MOS 36-Item Short-Form Health Survey (SF-36):I. Conceptual Framework and Item Selection. *Medical Care*, *30*(6), 473–483. https://doi.org/10.1097/00005650-199206000-00002
Wiik, K. A., Bernhardt, E., & Noack, T. (2009). A Study of Commitment and Relationship Quality in Sweden and Norway. *Journal of Marriage and Family*, *71*(3), 465–477. https://doi.org/10.1111/j.1741-3737.2009.00613.x
Wiik, K. A., Keizer, R., & Lappegård, T. (2012). Relationship Quality in Marital and Cohabiting Unions across Europe. *Journal of Marriage and Family*, *74*(3), 389–398. https://doi.org/10.1111/j.1741-3737.2012.00967.x
Williams, K. (2003). Has the future of marriage arrived? A contemporary examination of gender, marriage, and psychological well-being. *Journal of Health and Social Behavior*, *44*(4), 470–487. https://doi.org/10.2307/1519794
Willig, C. (2011). Cancer Diagnosis as Discursive Capture: Phenomenological Repercussions of Being Positioned within Dominant Constructions of Cancer. *Social Science & Medicine*, *73*, 897–903. https://doi.org/10.1016/j.socscimed.2011.02.028
Watson, N., & Wooden, M. (2012). The HILDA Survey: A Case Study in the Design and Development of a Successful Household Panel Survey. *Longitudinal and Life Course Studies*, *3*(3), 369–381

Endnotes

1. For example, 86% of people aged 65 years and older in the US and 78% of people in the same age group in Australia have at least one chronic condition (AIHW 2014; Ward, Schiller, and Goodman 2014).
2. We opt for random-effect instead of fixed-effect models because low rates of within individual change in marital status and the experience of chronic conditions make the latter inefficient. Sensitivity analyses conducted using fixed-effect models yield a very similar pattern of results, and are available from the authors upon request.
3. The standard errors in all regression models are adjusted for the nesting of individuals within households.

From bad to worse? Effects of multiple adverse life course transitions on mental health

Jornt J. Mandemakers Utrecht University, the Netherlands
j.j.mandemakers@uu.nl
Matthijs Kalmijn University of Amsterdam, the Netherlands

(Received November 2017 Revised May 2018) http://dx.doi.org/10.14301/llcs.v9i3.484

Abstract

This paper examines whether the simultaneous occurrence of two or more adverse life course transitions has a stronger effect on mental health compared to the effects of the sum of each. The focus is on four life course transitions (partner loss (divorce/separation or death), death of a parent, unemployment, disability) and the data come from a large four-wave longitudinal dataset in the Netherlands (*N* = 4,192 respondents). There is clear evidence that negative life course transitions tend to cluster. Of the four transitions, partner loss and disability onset have the largest negative impact on mental health but unemployment also has a clear effect. There is not only additive but also interactive accumulation during the life course: one adverse event has a more negative impact on mental health when it occurs simultaneously with another. This provides evidence on the link between 'turbulent times' in the life course and negative mental health trajectories. We did not find evidence that effects of adverse transitions depend on education.

Keywords

Life course transition; mental health; mhi-5; partner separation; unemployment; disability; parental death; accumulation; the Netherlands

Introduction

There is ample research into the effect of life course transitions on wellbeing and mental health. Research on the domains of work, health, and family indicate that several life course transitions can reduce mental health. Partner loss, in the family domain, is well known to impair people's mental health (Amato, 2000; Amato & Cheadle, 2005). The loss of close family members, such as the death of a parent, may also hurt wellbeing and mental health (Ballas & Dorling, 2007; Marks, Jun & Song, 2007; Umberson, 1995). In the work domain, the loss of a job and the experience of unemployment are important transitions that affect mental health (Dooley, Fielding & Levi, 1996; Kasl & Jones, 2000; Korpi, 2001; Paul & Moser, 2009). Furthermore, physical health is important for people's wellbeing; declines in physical health are associated with marked declines in wellbeing (Kelley-Moore & Ferraro, 2005; Lucas, 2007; Oswald & Powdthavee, 2008; Turner & Noh, 1988).

The main approach taken by research on mental health is to consider life course transitions one at a time. Studies typically focus on one domain and one transition in one paper or one model. Life courses in different domains are interconnected, however. A change in one domain of life may lead to or coincide with a change in another domain. For example, the relationship history and employment career are strongly intertwined; a common finding is that union dissolution heightens the risk of unemployment (Charles & Stephens, 2004; Covizzi, 2008; Kalmijn, 2005) and vice versa (Hansen, 2005).

Furthermore, parental death is related to marital problems (Umberson, 1995) and because marital problems increase the risk of divorce (Amato & Rogers, 1997), parental death may be associated with partner loss. Interconnectedness also exists in other domains. For example health transitions and transitions into and out of unemployment are clearly associated (García Gómez & López Nicolás, 2006; Van de Mheen, Stronks, Schrijvers & Mackenbach, 1999; Schuring, Burdoff, Kunst & Mackenbach, 2007; Wagenaar et al., 2012) but disability and divorce are associated as well (Kalmijn, 2005; Wilson & Waddoups, 2002). The interconnectedness of the various life domains and the synchronicity of transitions imply that some people will experience multiple adverse transitions in a short period of time (e.g., partner loss and unemployment). Such 'turbulent' periods may have especially profound negative consequences for mental health. Although studies of particular life course transitions sometimes include information on 'other' domains of life, this information is mostly used as a control variable.

A second approach to mental health, which is more often applied in public health research and some psychological studies, is to consider a range of 'events' simultaneously by constructing cumulative measures of life events, typically retrospectively assessed (Kessler, 1997; Lantz, House, Mero & Williams, 2005; Turner, Wheaton & Lloyd, 1995). Research along such lines shows that people who experience several negative life events have lower mental health and a heightened risk of depression (Turner & Avison, 1992; Turner et al., 1995). These findings suggest that 'turbulent' periods may be especially harmful to wellbeing and mental health: the more adverse life course transitions people experienced, the lower their wellbeing. The 'life event' approach has the merit that life course transitions are no longer viewed in isolation. A disadvantage, however, is that rather different events such as loss of a partner and parental death, are treated as equivalent: the scales for life events are simple counts of the number of events that occurred. Another limitation is that lifetime events are sometimes mixed with more recent events to create measures of total lifetime stress (e.g., Reynolds & Turner, 2008), which complicates the interpretation of the findings. Finally, in the life events approach a number of strains and events (e.g. financial problems, partner infidelity) that are associated with adverse life course transitions (e.g., job loss and partner loss) are treated as life events as well (see for example Turner et al., 1995). This practice may inflate the number of life events in the scale.

In studying how multiple life course transitions affect mental health, there are two theoretical approaches possible. One hypothesis is that the occurrence of multiple adverse transitions leads to worse outcomes than what each transition on its own would imply. This reasoning is based on an interactive influence of the various transitions: the experience of one adverse event becomes more painful when another transition occurs at the same time. The basis for this idea lies in the notion of resources. Adverse life course transitions tend to decrease people's social and economic resources. For example, following the death of a parent it may be harder to deal with partner loss as one can no longer turn to a parent for help and the loss of a parent may be harder to face without a partner. Similarly, it may be more difficult to deal with unemployment in combination with separation as there is no longer a partner at home who can provide emotional support, and former colleagues may be less likely to be a source of support in dealing with the separation. Similar negative patterns are imaginable for the other combinations of life course transitions, such as disability and partner loss or disability and job loss. A related idea is that a reduction in mental health makes people more vulnerable in an emotional sense because it reduces the psychological (coping) resources that people normally have (Thoits, 1982). Because adverse life course transitions reduce people's resources, the experience of another adverse life course transition will be particularly problematic and reduce mental health more than one would expect if that transition had occurred on its own.

An alternative hypothesis is based on an additive influence of the various transitions: each transition reduces mental health and if people experience multiple transitions, the total decline in mental health is the sum of the individual effects. Two main reasons can be given for such a simple additive pattern of effects: support mobilisation and floor effects. First, people who experience multiple life course transitions in a short span of time may be more likely to ask and receive support compared to people who experience just one transition. When people receive more support, this may counterbalance the negative interactive effects of experiencing several adverse transitions. If support is mobilised, it is often given for more than one problem at a time, which may reduce the interactive effects of multiple transitions. Second, there may be a floor effect operating. A first transition affects mental health in a negative way. For the next transition, when life is already in turmoil, another change may still have damaging potential but it will not have an extra negative effect and could even have a weaker effect if the initial effect is already severe. This possible floor effect may counterbalance the interactive effects of psychological vulnerability and decreased resources.

Which scenario is true – interactive or additive – has important implications for how inequality accumulates across the life course. If the additive scenario is valid, people who experience one event are more likely to experience another event and this leads to increasing inequality in mental health across the life course. This is important for inequality but only to the extent that adverse transitions tend to cluster in persons. If the interactive scenario is valid, inequality during the life course is increased for two reasons: adverse transitions cluster *and* they have interactive effects on life chances. Clearly, the latter scenario implies a more serious accumulation of inequality during the life course than the former. So far, the literature has been unclear about the nature of accumulation and the typical analysis has not clearly tested interaction effects vis-à-vis additive effects of life course transitions.

Another approach to accumulation has been to examine to what extent the effects of certain life course transitions depend on socioeconomic conditions; the differential vulnerability perspective (McLeod & Kessler, 1990). Examples of this perspective are studies on how the effects of divorce on children's mental health depend on parental education (Bernardi & Radl, 2014; Mandemakers & Kalmijn, 2014) and studies of how employment effects on health differ depending on a person's prior occupational position (Mandemakers & Monden, 2013). A common hypothesis is that lower strata – due to more limited economic, cultural, and psychological resources – are more vulnerable for the impact of adverse events. For partner loss, disability, and (to a lesser extent) unemployment, such effects have been demonstrated, especially for education (Clark, Georgellis & Sanfey, 2001 in Germany; Mandemakers & Monden, 2010, 2013, Mandemakers, Monden & Kalmijn, 2010 in the UK; and Strully, 2009 in the USA). Whether such interactions also apply to the effects of multiple life course transitions, i.e., to simultaneous adverse events, is not known.

In this paper, we study accumulation by examining the impact of multiple adverse life course transitions on mental health. We improve on the "one transition at a time" and the "life events" literatures by focusing on four major life course transitions simultaneously. We examine partner loss (divorce/separation or death of a partner), the death of a parent, unemployment, and the experience of a serious deterioration in health (called 'disability' in this paper). These are not only adverse life events but they also change the roles that people occupy (e.g. from worker to non-worker, from healthy person to sick person) and are therefore considered life course transitions as well (Wheaton, 1990). We use large-scale longitudinal data from the Netherlands to examine life course effects and we systematically compare additive and interactive dynamic models to examine their combined impact. We also examine if effects of (combined) transitions depend on a person's level of education.

Data, measurement, and design

Data

This study uses data from four waves of the Netherlands Kinship Panel Study (the waves were carried out in 2002–2004, 2006–2007, 2010–2011, and 2014–2015, the mean interval between waves was 3.5 years). Respondents were drawn from a random sample of private addresses in the Netherlands (N = 8,161). The overall response rate of the primary respondents was 45%, which is comparable to other family surveys in the Netherlands (Dykstra et al., 2005). Response rates of follow-up waves were 74% for the second, 72% for the third, and 65% for the fourth wave (de Hoon, Dykstra, Komter, Liefbroer & Mulder, 2015). We limited the analyses to respondents who were observed for at least two subsequent waves and to waves of respondents when they were at most 60 because unemployment may be different for people approaching retirement age and pensioners cannot become unemployed. In addition, as people age, health declines and the death of a parent may be the expected course of transitions. This does not mean that such transitions are no longer problematic but they will have a rather different nature earlier in the life course. This left us with 4,543 respondents and 13,791 respondent-wave combinations. We used listwise deletion of missing values. The final analytic sample encompasses 4,192 respondents who were observed for at least two subsequent waves and 12,770 respondent-wave combinations. At baseline, the people that were not included were more likely to be men, had a slightly lower educational level and were somewhat more likely to have a physical disability, not to have a job and to be single. There were no differences with regard to baseline mental health, whether parents were alive, and the number of children.

Measurement

We measured mental health with the five-item mental health screening test MHI-5 (Berwick et al., 1991). This test consists of five questions about how the respondent felt in the past four weeks: (a) how often the respondent was tense, (b) how often the respondent was feeling so down that nothing could cheer him/her up, (c) how often the respondent was calm and peaceful, (d) how often the respondent felt miserable and depressed, (e) how often the respondent felt happy. Answers range from 1 “all the time” to 6 “never”. The two positively worded items (c and e) were reversed. The scale is the average of the items and the resulting scale was standardised (mean = 0, s.d. = 1), which facilitates interpretation of the estimated coefficients in the linear regression as effect sizes. The reliability of this scale is good (alpha is .85). The correlation between waves is about .55. Cuijpers, Smits, Donker, ten Have, and de Graaf (2009) show that the MHI-5 is a good predictor of mood disorders (major depression, dysthymia), but less so of anxiety disorders. As such, the MHI-5 is often used as a measure of depressed mood and psychological mental health (e.g. Giver, Faber, Hannerz, Strøyer & Rugulies, 2010).

Respondents were categorised in distinct life course trajectories for each domain, based on their status at each wave. We focus on the changes in status between waves (*t* to *t*+1). The analyses focus on the four transitions of interest (partner loss, death of a parent, unemployment and disability onset) and include other transitions in relevant domains (e.g. partnership formation in the partnership domain) and age as control variables.

Respondents were asked for their relationship status at each wave. We consider partnership as either cohabitation or marriage. We distinguish the following three relationship transitions between two subsequent waves (*t* to *t+1*): (1) from being single to having a partner; (2) from having a partner to being single; (3) from having a partner to having a *different* partner (re-partnered). Partner loss (category 2 and 3) consists of people who divorce or separate and includes people whose partner died. Death of a partner is uncommon (21 out of 342 partner losses). The reference group consists of people who remained with the same partner and those who remained single from one wave to the next.

We considered whether a parental death (mother or father or both) occurred in between subsequent waves (*t* to *t+1*). No parental death observed between waves is the reference.

The main current economic activity was used to classify respondents into three groups: employed (includes pregnancy leave), unemployed (unemployed and those on a disability benefit), and inactive (ranging from homemakers, students to pensioners). Receiving disability benefits was included in the unemployment category; disability itself was treated as a separate transition (for further discussion, see below). It has been suggested that a substantial part of enrolment into disability benefits in the Netherlands is hidden unemployment (Koning & van Vuuren, 2010). We distinguish the following four employment transitions between waves (*t* to *t+1*): (1) employment/inactive to unemployment; (2) unemployment to employment/inactive; (3) employment to inactive; (4) inactive to employment. The reference is if there was no change between waves (remained employed, unemployed or inactive).

Two questions in each wave were posed regarding disability. Note that the term disability is not used here in the sense of disability from work, but in the sense of having a long-standing illness or a disability that limits people in their daily life. Hence, people do not necessarily have to stop working if they have a disability. We first asked respondents if they had such an illness or disability and if so, to what extent it limited them in their life. Based on these questions we examined the following two transitions between waves (*t* to *t+1*): (1) no disability or a disability with light limitations to a disability with severe limitations; (2) disability with severe limitations to no disability or to a disability with light limitations. The reference is if there was no change between waves (remain without disability/light limitations, remain with severe limitations).

Education was categorised in 11 levels that were recoded into three distinct educational groups: primary/lower secondary level (only primary education or lower and lower secondary level); higher secondary level/vocational; tertiary and higher). These three levels reflect the main social divide by educational level in the Netherlands. This coding was done to make our tests more parsimonious and was based on previous work in the field of social mobility research (De Graaf & Ganzeboom, 1993). We used the reported level of education in the first of each set of subsequent pairs of waves, so it can vary within individuals over the course of the study. Table 1 reports the descriptive statistics for the dependent and independent variables.

Table 1. Descriptive statistics (*N*=8,578)

	mean	sd
change score MHI-5 (std.)	.02	.97
change score age (std.)	3.60	.53
educational level		
primary/lower secondary (ref.)	.21	
secondary/vocational	.36	
tertiary	.43	
parenthood transitions		
no new child (ref.)	.87	
got new child	.13	
no change in parenthood status (ref.)	.94	
became parent first time	.06	
partnership transitions		
no change in partnership (ref.)	.90	
entered partnership	.06	
became single	.03	
repartnered with new partner	.01	
parental transitions		
no change in parental vital status (ref.)	.88	
one or both parents died	.12	
employment transitions		
no change in employment status (ref.)	.83	
entered unemployment	.04	
left unemployment	.03	
employment -> inactive	.04	
inactive -> employment	.06	
health transitions		
no change in health (ref.)	.94	
became disabled	.03	
regained health	.03	
simultaneous transitions	.02	

Design

In our first set of models, we estimate the effect of simultaneous adverse transitions on mental health (table 3). We model the level of mental health using first difference regression (change score models). First differences regression predicts changes in mental health by transitions between two subsequent waves. All time constant (un)observed individual differences are thereby differenced out, such as pre-existing differences in mental health and other relevant confounders. This is akin to fixed effect regression except that we here only contrast a current wave with a previous wave (hence *first* difference). Fixed-effect regression cannot be used for our purposes because we want to differentiate positive (e.g. becoming married) from adverse transitions (e.g. partner loss). Fixed-effect models equate positive and negative changes, which make sense for studying a change in a continuous variable such as income (increase/decrease), but not necessarily for events/transitions in the life course such as in relationship status (marriage versus divorce). Similar to fixed-effects regression the estimates of our models rely on within-individual variation.

Note that respondents participated for at least two and at most four waves (N = 12,770 respondent-wave combinations, which covered 7.4 years of a respondent's life on average), so we observed between one and three pairs of subsequent waves per individual. The observations in our analysis consist of the pooled 8,578 instances of subsequent pairs of waves for 4,192 respondents. Standard errors were adjusted for clustering of multiple observations in individuals in all models.

We included the main effects of the transitions between waves and added an additional variable indicating whether or not a respondent experienced more than one transition into a negative state. The 'simultaneous event' variable is central to our paper: it estimates to what extent there is an extra effect on mental health on top of the effects of each separate transition. 174 respondents experienced more than one adverse event between two sequential waves, of which three experienced it twice. So, we observed 177 simultaneous adverse transitions in total. This is sufficient to estimate the effect of simultaneous transitions. We estimate that we would need at least 130 observations in the 'experimental group' of simultaneous transitions to detect an effect size of d = .25 at alpha 5%. Unfortunately, there were not enough transitions for each specific event combination (e.g. for simultaneous unemployment entry and partner loss N = 20) to allow further delineation into specific combinations of transitions (see table 2 for the number of transitions per combination).

In our last set of analyses (table 4), we examined whether education moderates the impact of the (combined) transitions we study. Specifically, we repeat the primary analysis (table 3) for each of three educational groups. We thus present three sets of two models: the base model and the model including the simultaneous transitions indicator.

Table 2. Simultaneous transition breakdown (*N*=8,578)

Combinations of simultaneous transitions	N^a	Odds ratio	Adjusted odds ratio[b]
Any simultaneous transition	177	-	-
Partner loss * Unemployment	20	1.68*	1.72*
Partner loss * Disability	11	1.01	.91
Partner loss * Death of a parent	35	.83	.88
Unemployment * Disability	46	5.94***	5.88***
Unemployment * Death of a parent	39	1.02	.86
Disability * Death of a parent	50	1.63**	1.51**

[a] *N* of transition combinations does not add up to 177 because some respondents experience more than two transitions simultaneously;
[b] adjusted for wave, age, sex and the other transitions;
* p<0.05; ** p<0.01; *** p<0.001, two-tailed

Results

Occurrence of adverse transitions

We observed a total of 177 simultaneous transitions, which is only 0.2% of the 8,578 observations. This shows that it is uncommon to experience multiple adverse transitions and that one needs very large samples to study these issues. Table 2 shows the odds ratios of experiencing the six possible simultaneous combinations of the four delineated adverse transitions. Several combinations of transitions had odds ratios that differed significantly from 1. People who become unemployed are more likely to lose a partner (72% more likely after adjusting for covariates) and, conversely, partner loss increases the odds of unemployment. It is clear that becoming unemployed and becoming disabled are associated; one is almost six times more likely to observe the onset of disability if someone becomes unemployed and vice versa. Also parental death and the onset of a disability seem to be related, as the odds ratio is about 1.6.

First difference regression

We now turn to the question of accumulation of adverse transitions on mental health. Table 3 shows the results of two first difference regression models. Model 1 is the base model of mental health regressed on transitions in the life course domains and a set of control transitions. As we use first difference models the results refer to *changes* in levels of mental health within individuals following changes in their lives (akin to fixed-effects regression). The reference group consists of the observations where we did not observe an event in between the waves. The coefficients of the dummy variables can be interpreted as effect sizes because the dependent variable is standardised. We first turn to the controls and then to the transitions.

Table 3. First difference regression of MHI-5.

	(1) base		(2) simultaneous transitions	
	b	se	b	se
change score age (std.)	-.024	(.020)	-.024	(.020)
parenthood transitions				
got new child	-.081*	(.040)	-.080*	(.040)
became parent first time	.009	(.058)	.009	(.057)
partnership transitions				
entered partnership	.129**	(.045)	.130**	(.045)
became single	-.226***	(.058)	-.182**	(.060)
repartnered with new partner	.236	(.130)	.266*	(.130)
parental transitions				
one or both parents died	-.050	(.032)	-.025	(.033)
employment transitions				
entered unemployment	-.191***	(.056)	-.130*	(.060)
left unemployment	.146*	(.065)	.145*	(.065)
employment -> inactive	.102	(.055)	.102	(.055)
inactive -> employment	.024	(.046)	.025	(.046)
health transitions				
became disabled	-.348***	(.060)	-.272***	(.066)
regained health	.273***	(.060)	.273***	(.060)
simultaneous transitions			-.253**	(.093)
constant	.126	(.073)	.121	(.072)
N observations	8578		8578	
N individuals	4192		4192	

* p<0.05; ** p<0.01; *** p<0.001, two-tailed

Mental health does not change as people age, but note that the sample is limited to people below 60. Becoming a parent for the first time was not associated with a change in mental health, but the number of children was negatively associated with mental health.

Changes in the main life course domains will be discussed below. In the family domain, we find that people who experience partner loss (either through divorce/separation or death) experience a decrease in mental health. Becoming single has an effect size of -.226, which is modest. Re-partnering mitigates the negative effect of partner loss, as people who lost a partner between waves and who re-partner do not experience a decrease in mental health. The predicted change is even positive, although not significantly different from zero. People who enter a partnership experience a significant but small increase in mental health (.129). People who experience the loss of a parent (one or both) do not suffer the expected decrease in mental health. The effect (-.050) is in the expected direction but too small to be significantly different from zero. Transitions in the employment career also affect mental health. Those who become unemployed between waves have, as expected, a significantly lower level of mental health (-.191). Transitions out of unemployment lead to an increase in mental health but transitions in and out of the labour market do not affect it. Disabilities are quite important for mental health, as the following findings show. People who experience the onset of a disability have a decrease in mental health (-.348). People who were no longer limited by a disability experience an increase in mental health (.273). Note that models differentiating in the level of change in health over time show that the health shock is contingent on the level of disablement (not shown, available upon request).

Model 1 implies people who experience several adverse transitions accumulate adversity that reduces their mental health. It does not answer our main question of whether accumulation is additive or interactive. To answer that question, model 2 adds an indicator that measures whether people had experienced two or more adverse transitions between subsequent waves ('simultaneous transitions'). This effect tests our hypothesis of interactive effects. There appear to be amplifying interactive effects as the effect of this indicator is significant and negative (-.253). The main effects of the adverse transitions remain significant once we allow for interactive effects, although the size of these effects is reduced. This is especially the case for unemployment (-.191 to -.130, a reduction by almost a third), which suggests that part of the unemployment effect is related to experiencing simultaneous adversity in other domains of life. Only the positive effect of repartnering slightly increases between models 1 and 2 and is now significantly different from zero (increased from .236 to .266). Note that the reduction in the main effects of these adverse transitions (model 2 versus 1) cannot make up for the additional decrease in mental health due to experiencing simultaneous adverse transitions (-.253 versus a decrease from -.191 to -.130 for the unemployment effect), so these findings provide support for the idea that adversity accumulates. Further note that sample size restrictions did not permit us to investigate which combinations of transitions were most detrimental to mental health.

Finally, we evaluate the possibility that having a higher education benefits people by making the impact of adverse transitions weaker. We tested for moderation by stratifying the main analysis into three educational groups and tested whether the coefficients were statistically equivalent or not (table 4, model 1). Education did not moderate the impact of adverse transitions in the Netherlands. No significant interactions effects were found for partner loss, parental death, unemployment, and for disability. Only one effect was significantly different; the lowest educated seem to benefit from repartnering following partner loss as their mental health is predicted to increase by about 1 standard deviation. However, there were only 11 people in the lowest educational group who experienced this so we do not attach too much value to this finding. We note that there is some evidence that becoming single has a stronger negative effect for people with less education, whereas unemployment and disability have a more negative impact for the higher educated. These differences are tentative since they are not statistically significant.

Table 4. First difference regression of MHI-5 by education.

	base			simultaneous events		
	primary/ lower secondary	secondary/ vocational	tertiary	primary/ lower secondary	secondary/ vocational	tertiary
	b/se	b/se	b/se	b/se	b/se	b/se
change score age (std.)	-.004	-.036	-.020	-.004	-.035	-.020
	(.048)	(.034)	(.028)	(.048)	(.034)	(.028)
parenthood transitions						
got new child	-.083	-.094	-.068	-.084	-.096	-.067
	(.116)	(.069)	(.052)	(.116)	(.069)	(.052)
became parent first time	-.014	.021	.017	-.015	.022	.017
	(.199)	(.100)	(.071)	(.199)	(.099)	(.071)
partnership transitions						
entered partnership	.374**	.115	.080	.375**	.121	.080
	(.133)	(.075)	(.060)	(.133)	(.075)	(.060)
became single	-.262	-.236*	-.206*	-.284	-.112	-.193*
	(.135)	(.095)	(.088)	(.145)	(.098)	(.089)
repartnered with new partner	**1.048****	**.086**	**.038**	**1.032****	**.193**	**.045**
	(.366)	(.203)	(.184)	(.368)	(.204)	(.184)
parental transitions						
one or both parents died	-.082	-.027	-.055	-.093	.038	-.046
	(.072)	(.056)	(.046)	(.076)	(.057)	(.047)
employment transitions						
entered unemployment	-.184	-.099	-.317***	**-.205**	**.078**	**-.290****
	(.114)	(.093)	(.091)	(.124)	(.101)	(.096)
left unemployment	.348**	.012	.050	.349**	-.003	.052
	(.129)	(.112)	(.103)	(.130)	(.112)	(.103)
employment -> inactive	.102	.062	.142	.102	.064	.142
	(.117)	(.093)	(.087)	(.117)	(.093)	(.087)
inactive -> employment	.108	-.011	-.013	.107	-.010	-.013
	(.107)	(.068)	(.078)	(.107)	(.068)	(.078)
health transitions						
became disabled	-.160	-.449***	-.437***	-.187	-.271**	-.396***
	(.123)	(.095)	(.101)	(.138)	(.103)	(.113)
regained health	.152	.399***	.250*	.153	.399***	.252*
	(.111)	(.104)	(.104)	(.111)	(.104)	(.104)
simultaneous transitions				**.080**	**-.709*****	**-.126**
				(.187)	(.158)	(.156)
constant	.077	.179	.097	.080	.164	.096
	(.172)	(.125)	(.102)	(.172)	(.124)	(.102)
N observations	1836	3067	3675	1836	3067	3675
N individuals	1167	1707	1836	1167	1707	1836

* p<0.05; ** p<0.01; *** p<0.001, two-tailed;

Bold coefficients differ across educational groups at the p<0.05 level (Wald tests of equality of coefficients in pooled analyses with interactions by educational level).

Note that the total individual sample is larger than that of table 3 (4,710 versus 4,192) because we used a time-varying indicator of educational level and so individuals can belong to several educational categories.

We did find a significant interaction of education and the impact of experiencing simultaneous transitions (table 4, model 2). Contrary to our expectation, however, the experience of simultaneous adverse transitions appears to be especially harmful for the intermediate educational group. The highest educational group is also negatively affected but less strongly, whereas there is no effect for the lowest educated. This may reflect on the one hand that resources protect the highest educated from suffering more when experiencing simultaneous transitions but, on the other hand, possible floor effects that do the same for the lowest educated groups. Once we allow for simultaneous effects, some of the two-way interactions become significant. Especially, the highest educated appear to suffer more from unemployment. This suggests that the income and status 'fall' after unemployment can be greater for those with high-status jobs.

Conclusion and discussion

In this study, we examined the question of whether experiencing several adverse life course transitions in a relatively short period of time has interactive (accumulating) detrimental effects for mental health or whether the effects are merely additive. Empirically, we tackled this question using a large four-wave panel study in the Netherlands with about three to four years between subsequent waves. We examined transitions with potential negative consequences for mental health in three important life course domains: partner loss and the death of a parent in the family domain; becoming unemployed in the work domain; and the onset of disability in the health domain.

We found that each of the delineated adverse transitions had its predicted negative consequence for mental health, except for the death of a parent. The onset of disability had the largest effect, followed by the loss of a partner and unemployment. The experience of combined adverse transitions in the span of time between two subsequent waves of the study led to an extra negative effect on mental health, on top of the main effects of the component transitions. More specifically, the accumulative effect of two simultaneous effects was worse than would be expected given the sum of these effects. In other words, there seems to be interactive and not simply additive accumulation of misery during the life course.

Theoretically, we interpret this finding in terms of resources and vulnerability. We believe that simultaneous adverse transitions can have interactive effects because people are psychologically more vulnerable to the effects of a second adverse transition after just going through one. In addition, the first adverse event may decrease people's social and economic resources, which may make them more vulnerable to future adverse transitions. We also developed alternative hypotheses about floor effects and support mobilisation, but these apparently do not play a role or at least are not strong enough to overturn the interaction effect. Our finding that the accumulation effect is weaker for the higher educated – who will be more likely to mobilise professional help – could point in this direction, however.

Conceptually, we moved beyond the "one transition at a time" and the "life events" literatures by examining four transitions in different domains of life. Even in the relatively long span of time between subsequent waves, three to four years, the effects of simultaneous transitions on mental health accumulate. It seems that, especially when unemployment was added to the mix, the effects worsened. Although we do not have information on the order of events, we suppose that the effects accumulate because the first adverse transition that occurs erodes people's ability to cope with subsequent adverse events. Unemployment erodes financial resources and may put a strain on social resources so that a subsequent event, such as the death of a parent, comes as a much bigger blow. It is also possible that a first adverse transition sets in motion an adverse chain of events, for instance unemployment leading to divorce. Unfortunately, we were not able to investigate such processes given the relatively long time interval between waves in this study.

It is also possible that people experience simultaneous adversity (e.g. unemployment and partner loss) as a result of a mental health breakdown or because of other external events that affect both mental health and the chance of negative transitions. Observational studies are not able to rule out such alternative narratives but the clear existence of accumulation in the life course (be it cause and/or effect) points to the need for future studies of single life course transitions, such as job loss, to carefully consider the role of transitions in other domains of life, as they may have exacerbating effects and/or may be part of adverse chain of transitions. Longitudinal studies using shorter time intervals (preferably yearly) than we had available would be especially suited for this task.

References

Amato, P. R. (2000). The consequences of divorce for adults and children. *J Marriage Fam, 62*, 1269–1287. https://doi.org/10.1111/j.1741-3737.2000.01269.x

Amato, P. R. & Cheadle, J. (2005). The long reach of divorce: Divorce and child well-being across three generations. *J Marriage Fam, 67*, 191–206. https://doi.org/10.1111/j.0022-2445.2005.00014.x

Amato, P. R. & Rogers, S. J. (1997). A Longitudinal Study of Marital Problems and Subsequent Divorce. *J Marriage Fam, 59*, 612. https://doi.org/10.2307/353949

Ballas, D. & Dorling, D. (2007). Measuring the impact of major life events upon happiness *Int J Epidemiology, 36*, 1244–1252. https://doi.org/10.1093/ije/dym182

Bernardi, F. & Radl, J. (2014). The long-term consequences of parental divorce for children's educational attainment. *Demographic Research, 30*, 1653–1680. https://doi.org/10.4054/DemRes.2014.30.61

Berwick, D. M., Murphy, J. M., Goldman, P. A., Ware, J. E., Barsky, A. J. & Weinstein, M. C. (1991). Performance of a Five-Item Mental Health Screening Test. *Medical Care, 29*, 169–176. https://doi.org/10.1097/00005650-199102000-00008

Charles, K. K. & Stephens, M. (2004). Job displacement, disability, and divorce. *J Labor Econ, 22*, 489–523. https://doi.org/10.1086/381258

Clark, A. E., Georgellis, Y. & Sanfey, P. (2001). Scarring: The Psychological Impact of Past Unemployment. *Economica, 68*, 221–241. Clark, A. E., Georgellis, Y. & Sanfey, P. (2001). Scarring: The Psychological Impact

Covizzi, I. (2008). Does Union Dissolution Lead to Unemployment? A Longitudinal Study of Health and Risk of Unemployment for Women and Men Undergoing Separation. *Eur Sociol Rev, 24*, 347–361. https://doi.org/10.1093/esr/jcn006

Cuijpers, P., Smits, N. Donker, T., ten Have, M. & de Graaf, R. (2009). Screening for mood and anxiety disorders with the five-item, the three-item, and the two-item Mental Health Inventory. *Psychiatry Research, 168*, 250. https://doi.org/10.1016/j.psychres.2008.05.012

De Graaf. P. & Ganzeboom, H. B. G. (1993). Family background and educational attainment in the Netherlands for the 1891-1960 birth cohorts. In Y. Shavit & H-P. Blossfeld (Eds.), *Persistent inequality: Changing educational attainment in thirteen countries* (pp 75–99). Boulder, Co.: Westview Press.

Dooley, D., Fielding, J. & Levi, L. (1996). Health and Unemployment. *Annu Rev Publ Health, 17*, 449–465. https://doi.org/10.1146/annurev.pu.17.050196.002313

Dykstra, P.A., Kalmijn, M., Knijn, T. C. M., Komter, A. E., Liefbroer, A. C. & Mulder, C. H. (2005). *Codebook of the NKPS, a multi-actor, multi-method panel study on solidarity in family relationships, Wave 1.* The Hague: NIDI.

García Gómez, P. & López Nicolás, A. (2006). Health shocks, employment and income in the Spanish labour market. *Health Economics, 15*, 997–1009. https://doi.org/10.1002/hec.1151

Giver, H., Faber, A., Hannerz, H., Strøyer, J. & Rugulies, R. (2010). Psychological well-being as a predictor of dropout among recently qualified Danish eldercare workers. *Scand J Public Healt, 38*, 239–245. https://doi.org/10.1177/1403494809348939

Hansen, H-T. (2005). Unemployment and Marital Dissolution: A Panel Data Study of Norway. *Eur Sociol Rev, 21*, 135–148. https://doi.org/10.1093/esr/jci009

de Hoon, S., Dykstra, P. A., Komter, A. E., Liefbroer, A. C. & Mulder, C. H. (2015). *Codebook of the NKPS, a multi-actor, multi-method panel study on solidarity in family relationships, Wave 4.* The Hague: NIDI.

Kalmijn, M. (2005). The Effects of Divorce on Men's Employment and Social Security Histories. *Eur J Popul, 21*, 347. https://doi.org/10.1007/s10680-005-0200-7

Kasl, S. V. & Jones, B. A. (2000). The impact of job loss and retirement on health. In L. F. Berkman & I. Kawachi (Eds.), *Social Epidemiology*, pp118–136. New York, NY: OUP.

Kelley-Moore, J. A. & Ferraro, K. F. (2005). A 3-D Model of Health Decline: Disease, Disability and Depression among Black and White Older Adults. *J Health Soc Behav, 46*, 376–391. https://doi.org/10.1177/002214650504600405

Kessler, R. C. (1997). The effects of stressful life events on depression. *Annu Rev Psychol, 48*, 191–214. https://doi.org/10.1146/annurev.psych.48.1.191

Koning, P. W. C. & van Vuuren, D. J. (2010). Disability insurance and unemployment insurance as substitute pathways. *Applied Economics, 42*, 575–588. https://doi.org/10.1080/00036840701704436

Korpi, T. (2001). Accumulating disadvantage: Longitudinal analyses of unemployment and physical health in representative samples of the Swedish population. *Eur Sociol Rev, 17*, 255–273. https://doi.org/10.1093/esr/17.3.255

Lantz, P. M., House, J. S., Mero, R. P. & Williams, D. R. (2005). Stress, Life Events, and Socioeconomic Disparities in Health: Results from the Americans' Changing Lives Study. *J Health Soc Behav, 46*, 274–288. https://doi.org/10.1177/002214650504600305

Lucas, R. E. (2007). Long-Term Disability is Associated with Lasting Changes in Subjective Well-Being : Evidence from Two Nationally Representative Longitudinal Studies. *J Pers Soc Psychol, 92*, 717–730. https://doi.org/10.1037/0022-3514.92.4.717

Mandemakers, J. J. & Kalmijn, M. (2014). Do mother's and father's education condition the impact of parental divorce on child well-being? *Soc Sci Res, 44*, 187–199. https://doi.org/10.1016/j.ssresearch.2013.12.003

Mandemakers, J. J. & Monden, C. W. S. (2010). Does education buffer the impact of disability on psychological distress? *Soc Sci Med, 71*, 288–297. https://doi.org/10.1016/j.socscimed.2010.04.004

Mandemakers, J. J. & Monden, C. W. S. (2013). Does the effect of job loss on psychological distress differ by educational level? *Work, Employ Soc,* 27, 73–93. https://doi.org/10.1177/0950017012460312

Mandemakers, J. J., Monden, C.W.S. & Kalmijn, M. (2010). Are the Effects of Divorce on Psychological Distress Modified by Family Background? *Adv Life Course Res,* 15:27-40. https://doi.org/10.1016/j.alcr.2010.08.002

Marks, N. F., Jun, J. & Song, J. (2007). Death of Parents and Adult Psychological and Physical Well-Being: A Prospective U.S. National Study. *J Fam Issues, 28*, 1611–1638. https://doi.org/10.1177/0192513X07302728

McLeod, J. D. & Kessler, R. C. (1990). Socioeconomic status differences in vulnerability to undesirable life events. *J Health Soc Behav, 31*, 162–172. https://doi.org/10.2307/2137170

Oswald, A. J. & Powdthavee, N. (2008). Does Happiness Adapt? A Longitudinal Study of Disability with Implications for Economists and Judges. *J Public Econ, 92*, 1061–1077. https://doi.org/10.1016/j.jpubeco.2008.01.002

Paul, K. I. & Moser, K. (2009). Unemployment impairs mental health: Meta-analyses. *J Voc Behav, 74*, 264. https://doi.org/10.1016/j.jvb.2009.01.001

Reynolds, J. R. & Turner, R. J. (2008). Major Life Events: Their Personal Meaning, Resolution, and Mental Health Significance. *J Health Soc Behav,* 49, 223–237. https://doi.org/10.1177/002214650804900208

Schuring, M., Burdoff, L., Kunst, A. & Mackenbach, J. (2007). The effects of ill health on entering and maintaining paid employment: evidence in European countries. *J Epidem Comm Health, 61*, 577–604. https://doi.org/10.1136/jech.2006.047456

Strully, K. W. (2009). Job Loss and Health in the U.S. Labor Market. *Demography, 46*, 221–246. https://doi.org/10.1353/dem.0.0050

Thoits, P. A. (1982). Life Stress, Social Support, and Psychological Vulnerability: Epidemiological Considerations. *J Community Psych*, *10*, 341–62. https://doi.org/10.1002/1520-6629(198210)10:4<341::AID-JCOP2290100406>3.0.CO;2-J

Turner, R. J. & Avison, W. R. (1992). Innovations in the Measurement of Life Stress – Crisis Theory and the Significance of Event Resolution. *J Health Soc Behav, 33*, 36–50. https://doi.org/10.2307/2136856

Turner, R. J. & Noh, S. (1988). Physical Disability and Depression: A Longitudinal Analysis. *J Health Soc Behav, 29*, 23–37. https://doi.org/10.2307/2137178

Turner, R. J., Wheaton, B. & Lloyd, D. A. (1995). The Epidemiology of Social Stress. *Am Soc Rev, 60*, 104–125. https://doi.org/10.2307/2096348

Umberson, D. (1995). Marriage as Support or Strain? Marital Quality Following the Death of a Parent. *J Marriage Fam, 57*, 709. https://doi.org/10.2307/353925

Van de Mheen, H., Stronks, K., Schrijvers, C. T. & Mackenbach, J. P. (1999). The influence of adult ill health on occupational class mobility and mobility out of and into employment in the Netherlands. *Soc Sci Med, 49*, 509. https://doi.org/10.1016/S0277-9536(99)00140-9

Wagenaar, A. F., Kompier, M. A. J., Houtman, I. L. D., van den Bossche, S. N. J. & Taris, T.W. (2012). Employment Contracts and Health Selection Unhealthy Employees Out and Healthy Employees In? *J Occup Environ Med, 54*, 1192–1200. https://doi.org/10.1097/JOM.0b013e3182717633

Wheaton, B. (1990). Life transitions, role histories and mental health. *Am Soc Rev, 55*, 209–223. https://doi.org/10.2307/2095627

Wilson, S. E. & Waddoups, S. L. (2002). Good Marriages Gone Bad: Health Mismatches as a Cause of Later-Life Marital Dissolution. *Pop Res Policy Rev, 21*, 505–533. https://doi.org/10.1023/A:1022990517611

The significance of timing and duration of social assistance receipt during childhood on early adult outcomes

Tiina Ristikari National Institute of Health and Welfare, Finland
tiina.ristikari@thl.fi
Marko Merikukka National Institute of Health and Welfare, Finland
Mia Hakovirta University of Turku, Finland

(Received July 2017 Revised June 2018) http://dx.doi.org/10.14301/llcs.v9i3.471

Abstract

The experience of economic disadvantage during childhood is a major predictor of a variety of negative outcomes during early adulthood. This study provides evidence on the significance of timing of social assistance receipt during childhood on children's later adjustment problems. The study uses data from the 1987 Finnish Birth Cohort (FBC), which covered all children born in Finland in 1987 (N=59476) and followed them until the age of 25. The data were gathered from Finnish registers that cover health and sociodemographic data for cohort members and their parents. Altogether 11,062 female (38.1%) and 11,537 male (37.9%) cohort members had parents who had received social assistance. Social assistance receipt during childhood increased the risk for all measured adjustment problems: early school leaving (OR 2.37), conviction (OR 1.87), teenage pregnancy (OR 1.89) and mental disorders (OR 1.68) even when adjusting for several social background variables. Economic disadvantage during early childhood (0–2 years) was found to associate with highest risk; all measured adjustment problems compared to exposure to poverty later in childhood. The study concludes that early childhood is a period in which children acquire cognitive and social competencies that form the basis for future wellbeing. Our analysis, based on a total nation-wide birth cohort, indicates that economic disadvantage in early childhood poses the most significant risk for later adjustment problems.

Keywords

Social assistance; early adult outcome; birth cohort; registers study

Introduction

The negative impact of childhood economic disadvantage on children's later wellbeing has been documented extensively over past decades (e.g., Hansen, 2008; Holzer, Schanzenbach, Duncan, & Ludwig, 2007; Ilmakunnas, 2018; Mayer, 1997; McLoyd, 1998; Saraceno, 2002). When comparing children from more affluent families to children living in low-income families, earlier studies have found that children growing up in poverty are more likely, for example, to have health, behavioural, and emotional problems; to drop out of school; to have low academic achievement; and a risk of an intergenerational cycle of welfare receipt (e.g. Duncan & Brooks-Gunn, 1999; Duncan, Ziol-Guest, & Kalil, 2010; Kauppinen et al., 2014; Nielsen, Juon, & Ensminger, 2004). Parental income has been shown to be a more significant determinant of children's wellbeing and achievement than parental mental or physical health, maternal schooling, and family structure, for example (Duncan & Brooks-Gunn, 2000). The linkages between low income and negative outcomes are particularly strong for children who are trapped in poverty for a long time (Wagmiller, Lennon, Kuang, Alberti, & Aber, 2006).

Much less is known about how the timing of economic disadvantage associates with early adult outcomes and very few studies have investigated how family income at different stages of child life impacts early adult wellbeing (see Kalil, Duncan, & Ziol-Guest, 2016).

Background

Studies using both survey and register data from several cultural contexts and welfare state regimes have documented the detrimental short- and long-term impact of childhood poverty (for a systematic review of mental health outcomes, see Reiss, 2013). For example, several British cohort studies have documented the short- and long-term consequences of poverty on child development, including the most recent Millennium Cohort (Dickerson & Popli, 2016). Najman and colleagues (2009) in Australia showed, using a population-based prospective birth cohort study, that being exposed to poverty at four different measurement points was associated with the worst outcomes in terms of cognitive development. Each additional exposure to poverty between birth and age 14 was associated with an additional decline in the score on Raven's Standard Progressive Matrices by 2.19 units. Duncan and colleagues (Duncan, Telle, Ziol-Guest, & Kalil, 2010), using data from the US, measured poverty across several distinct periods of childhood, distinguishing family income in early childhood from income in middle childhood and adolescence. They found that, compared with children whose families had incomes of at least twice the poverty line during their early childhood, poor children completed two fewer years of schooling, earned less than half the salaries of their more affluent counterparts, worked fewer hours, and received more food stamps as adults. Males who grew up in poverty were found to be arrested more often and females' poverty in early childhood was associated with teenage pregnancy. Gender-specific effects of poverty have been reported in a number of studies, but in her systematic review, Reiss (2013) concludes that, in terms of mental health outcomes, no consistent patterns can be identified. Gibb, Fergusson and Horwood (2012) reported from New Zealand that declining childhood family income was associated with a range of negative outcomes in adulthood, including lower educational achievement, poorer economic circumstances, higher rates of criminal offending, mental health problems and teenage pregnancy. After covariate adjustment, childhood family income remained significantly associated with educational achievement and economic circumstances, but was no longer significantly associated with mental health, offending and teenage pregnancy outcomes. Other research has also acknowledged the differential effect of childhood poverty on cognitive ability and emotional adjustment. For example, in their early work, Brooks-Gunn and Duncan (1997) showed that childhood poverty had the largest impact on children's ability and achievement and a smaller impact on emotional development.

A recent study from Norway showed that low income experienced in early childhood correlates with the chances of later-life economic success (Duncan, Telle, Ziol-Guest, & Kalil, 2010). In Sweden, children in families receiving long-term social assistance showed considerably less satisfactory future prospects regarding health-related outcomes, educational attainment and social assistance receipt in young adulthood compared with the rest of the population, and also in comparison with other low-income individuals (Weitoft, Hjern, Batljan, & Vinnerljung, 2008). Similar findings have also been made in non-Western countries. For example, Pakpahan, Suryadarm and Suryahadi (2009) showed that in Indonesia, children exposed to recurrent poverty in childhood were 31% more likely to be poor adults than their more affluent childhood counterparts.

Theoretical models explaining the effect of poverty on later outcomes

The most widely accepted explanation for the detrimental effect of childhood poverty on adult wellbeing assumes that low income, poverty and material deprivation lead to a series of barriers that limit the development and life course opportunities of those reared in low-income families. Low family income and welfare receipt has been argued to influence children's future success and achievements through parents' material and emotional endowments and investment with their children (Becker, 1991). Broadly speaking, this model suggests that child development is produced as a result of genetic endowments and the values and preferences parents pass on to their children, together with different types of resource, such as time. Recent evidence from the United States has shown that the key driver behind diverging destinies of rich and poor children in terms of cognitive and non-cognitive skills is the behaviour, and in particular the engagement, of parents with their children at home (Mayer, Kalil, Oreopoulos, & Gallegos, 2015). Several US studies have shown that more affluent parents engage more often and for longer periods of time in behaviours such as reading that stimulate the cognitive and non-cognitive development of their children, than their less affluent peers do, leading to sizable differences in their children's outcomes (Cunha, Heckman, Lochner, & Masterov, 2006; Heckman & Masterov, 2007).

Another theoretical model explains the impact of poverty through stress experienced within the family or surrounding environment (Elder, 1974; Kessler & Cleary, 1980; McLeod & Kessler, 1990). The distress caused by having to negotiate everyday challenges in the context of economic scarcity is seen to impact child development. Stress may act as a mediator explaining the association between childhood poverty and negative outcomes in adulthood (Winning, Glymour, McCormick, Gilsanz, & Kubzansky, 2016). The impact of stress as a result of low income is also often exacerbated by co-occurring disruptions in family functioning (Solantaus, Leinonen, & Punamäki, 2004). The stress model has recently been developed further by researchers in behavioural economics who have demonstrated that periods of economic scarcity lead to reductions in our cognitive resources that impact our ability to regulate our behaviour (Spears, 2011). Poverty, in essence, reduces our cognitive bandwidth, affecting our ability to make good choices for the future at the expense of more immediate benefits (Schilbach, Schofield, & Mullainathan, 2016).

A third theoretical model explaining the negative impact of poverty on child development is one that considers selection effects (Mayer, 1997). When trying to isolate the effect of poverty on child development, researchers must consider the often co-occurring socioeconomic effects impacting child outcomes. Poor parents are often less educated and have lower socioeconomic status (SES) than their more affluent counterparts. Also, poor households are more often headed by a single breadwinner and more likely to experience mental health problems, or an accumulation of these challenges, which may have more than an additive negative impact on child development (Ben-Shlomo & Kuh, 2002; Crystal & Shea, 1990). Also the influence of often unmeasured effects, such as parental motivation or mental health, may have significant impact on child outcomes that non-experimental studies are unable to identify.

Significance of timing of poverty

Despite these earlier findings, it is not clear how significant the timing of childhood poverty is on early adult outcomes. Economic disadvantage in early childhood has been argued to be more detrimental to child development than disadvantage experienced during adolescence, because early childhood is the period in which children acquire cognitive and social competencies that form the basis of future wellbeing and academic success (Brooks-Gunn & Duncan, 1997; Farkas & Beron, 2004; Kalil, Ryan, & Corey, 2012; Schoon et al., 2002). Also, in early childhood brain development is rapid and the brain may be more vulnerable to environmental stressors associated with poverty than it is later in life (Knudsen, Heckman, Cameron, & Shonkoff, 2006). On the other hand, some evidence suggests that low income in adolescence rather than early childhood is associated with negative outcomes in young adulthood, because older children in low-income families may be more aware of their economically limited circumstances and their restricted opportunities for success, and as a result may reduce their aspirations and effort (Sobolewski & Amato, 2005). In a chapter of the recent *Handbook of the Life Course*, Kalil and colleagues review the evidence regarding the short- and long-term impact of poverty and conclude that very little is known about the significance of the timing of the poverty (Kalil et al., 2016).

In our study we ask how does the timing of childhood economic disadvantage impact children's later adjustment and mental health. We operationalise economic disadvantage as parental receipt of social assistance and measure it at five different age periods: 0–2, 3–6, 7–12, 13–16, and 17+. As previous research has conclusively indicated that long-term and persistent poverty has significantly more detrimental effects on child outcomes than transient or short-term poverty, we investigate the significance of timing among those families experiencing poverty for an extended period, more than four months.

The principal focus of the analysis is to examine whether early childhood poverty is more harmful for child development than economic disadvantage in later childhood. We measure child outcomes using several objective indicators of adjustment problems, as conceptualised by Schoon and Bartley (2008). Specifically, we measure early school leaving, teenage pregnancy, criminality, and mental health disorders before age 25. A major advantage of this study is that our data contain reliable measures of both social assistance receipt and correlated aspects of parental socioeconomic status, and indicators of parental mental health, and it is possible to estimate the separate contributions of each.

Social assistance in Finland

Social assistance is a means-tested last-resort form of income protection in Finland. It is granted only if the applicant has no other source of income, or the income is inadequate to meet the individual's or family's basic needs (Kuivalainen, 2013). Other benefits, such as child allowance and unemployment benefits reduce the level of social assistance. Only if the level of primary benefits and other sources of income are not adequate to cover the basic necessities such as food, shelter and medicine will social assistance be granted. Social assistance is meant to be a temporary benefit that fills gaps in income. Recipients of social assistance are often unemployed, single parenting or living alone, experiencing health problems and less educated than the Finnish populations on average (Kuivalainen, 2013).

Social assistance in Finland is considered a good indicator of poverty for several reasons. One reason is that in order to received social assistance one has to actively apply for it, which implies that the applicant has a subjective evaluation of need for the assistance. Another reason is that receiving social assistance requires a public authority to determine that a person's primary sources of income are inadequate to meet basic needs (Kangas & Ritakallio, 2008). Altogether, 7.3% of the Finnish population received social assistance in 2015 and people increasingly received social assistance for longer periods of time (Official statistics of Finland, 2016). In Finland, students may qualify for social assistance if they do not find summer jobs and as a result, receive social assistance for two to three months. In the 1990s, Finland experienced a major economic recession and unemployment rose dramatically from 3.5 to 18.9%. Children in the 1987 FBC were about four years of age when the recession began and approximately 13 when the recession finally ended. We tried to estimate potential selection among social assistance recipients in different timing groups using all our parental covariates, but were not able to identify any major sociodemographic or health differences that could explain differences in the child outcomes.

Data and methods

The 1987 Finnish Birth Cohort. The 1987 Finnish Birth Cohort is a register-based dataset that includes all 59,476 children born in the year 1987 in Finland and followed during the years 1987 to 2012. The register data were combined using cohort members' and their parents' personal identification numbers (Paananen, Ristikari, & Gissler, 2014). The institutional ethical review board at the National Institute for Health and Welfare (THL) gave a positive statement for the whole study (March 26, 2009, § 28/2009), and all register authorities gave permission to use their data.

Variables

Social assistance receipt

The main independent variable measures the incidence of social assistance receipt. The information of parental social assistance receipts were obtained from National Institute for Health and Welfare's registers. Parental social assistance was registered for either the biological mother, biological father or for both parents during the follow-up of 1987 to 2008 (child aged between 0 and 21). Parental social assistance was coded in three different ways: i) receipt (received or not); ii) duration (0 months, 1–3 months, and 4 months or more); and iii) duration x age of child at first instance of social assistance receipt (0 months, 1–3 months, 4 months or more, when child was on age 0–2/3–6/7–12/13–16/17 or more).

Control variables

In order to estimate the impact of social assistance receipt on early adulthood outcomes, we included a number of social background variables. As Mayer (1997) has argued, previous research has often been unable to control for the following family characteristics that may account for much of the observed association between social assistance receipt and child outcomes.

Parental education

Information on the highest educational achievements was received from Statistics Finland

and was categorised in the variable "parents' highest education" as a binary variable, early school leaving vs. higher education. We categorised educational attainment by UNESCO's International Standard Classification of Education (ISCED), where early school leaving was set as having not completed ISCED level three education by the year 2008. In other words, our definition for early school leaving includes those who have not completed compulsory education which ends at age 16, those who have completed compulsory education at best and those who have entered secondary education (non-compulsory after age 16) but did not complete a programme of study.

Parental SES

The data on the socioeconomic status (SES) of the cohort members' biological parents were collected from the Finnish Central Population Register on June 10, 2009, and it included parents' most recent occupations: classified as upper white-collar workers, lower white-collar workers, blue-collar workers, or 'others', including entrepreneurs, students, housewives and farmers. We considered the SES of both parents and chose the higher of the two. Residents of Finland are asked to report to the Central Population Register their occupation, and not all residents adhere to the regulation. Of the cohort member's parents, 1.9% have a missing value, which we have categorised as "missing".

Other than nuclear family

Data on cohort members' biological parents' mutual marriages and divorces and dates of death during the follow-up (1987–2008) were received from the Finnish Central Population Register. Data on mother's marital status at the time of cohort member's birth were collected from the Medical Birth Register (MBR), kept by THL. The variable 'other than nuclear family' was coded as a binary variable, yes/no, where 'yes' consisted of families where the mother of the cohort member was either not married at the time of the birth, either parent had died during the follow-up, or the cohort member's parents had divorced during the follow-up.

Teenage pregnancy, cohort member's mother

The Medical Birth Register (MBR) includes the information on the year and month of birth of the mother of the cohort member. Based on this information, the age of the mother at the time of giving birth to the cohort member was calculated. If the mother was less than 20 years at the time of the cohort member's birth, we gave value 1, and value 0 for mothers who were older than 20.

Parental psychiatric in/outpatient care

The variable "parental psychiatric in/outpatient" care was coded as a binary variable, yes/no, based on whether either parent had been treated in either inpatient (1987–2008) or outpatient (1998–2008) psychiatric care according to the data from the hospital discharge register kept by the National Institute for Health and Welfare, Finland.

To assess developmental outcomes during young adulthood, four outcome variables were measured: 1) early school leaving; 2) criminal record; 3) mental health disorders; and 4) teenage pregnancy. We used listwise deletion for missing values, except for parental SES where we coded for missing as a category. Table 1 shows the frequency distribution of all the measured variables by gender. Nine per cent of parents had received social assistance for short periods, less than three months, and about 29% more than four months. Four months was the median duration of social assistance receipt (not shown in table).

Cohort members' early adult outcomes

Early school leaving

Information on the highest educational achievements until 2012 was received from Statistics Finland. We measured educational attainment by UNESCO's International Standard Classification of Education (ISCED), and considered early school leaving as having not completed ISCED level three education—or secondary education—by the end of year 2012, when cohort members turned 25. In other words, our definition for early school leaving includes those who have not completed compulsory education, those who have completed compulsory education at best, and those who have entered secondary education but did not complete a programme of study.

Criminal record

The Finnish Legal Register Centre has a national central register of criminal records. The variable "criminal record" was created (binary, yes/no) from the information on criminal offending for which a court had imposed a conviction between 1987 and 2012.

Table 1. Composition of the 1987 Finnish Birth Cohort for females and males

	Females		Males	
	n	%	N	%
Number of births	29,041		30,435	
Variable				
Cohort member outcomes:				
Early school leaving	4,688	16.1	6,296	20.7
Criminal record	890	3.1	4,238	13.9
Mental health disorder	3,699	12.7	2,816	9.3
Teenage pregnancy	2,834	9.8	-	-
Parent indicators:				
Parent's highest education (early school leaving vs higher)	2,009	6.9	2,119	7.0
Parent's highest SES				
Upper	9,014	31.0	9,456	31.1
Lower	11,811	40.7	12,401	40.7
Workers	6,683	23.0	6,977	22.9
Other	985	3.4	1,030	3.4
Unknown	548	1.9	571	1.9
Other than nuclear family	13,359	46.0	13,573	44.6
Teenage pregnancy, cohort member’s mother	933	3.2	974	3.2
Parental psychiatric in/outpatient care	6,781	76.7	6,309	20.7
Parental social assistance receipt	11,062	38.1	11,537	37.9
Parental social assistance, duration				
0 months	17,979	61.9	18,898	62.1
1–3 months	2,560	8.8	2,765	9.1
4 months or more	8,502	29.3	8,772	28.8
Parental social assistance, duration x age				
4 months or more, child on age 0–2	4,158	14.3	4,304	14.1
4 months or more, child on age 3–6	2,517	8.7	2,600	8.5
4 months or more, child on age 7–12	1,343	4.6	1,393	4.6
4 months or more, child on age 13–16	334	1.2	304	1.0
4 months or more, child on age 17 or more	150	0.5	171	0.6

Mental health disorder

Cohort member's mental health was studied by creating a variable 'mental health disorder', which was coded as a binary variable, yes/no, based on whether according to the Finnish Hospital Discharge Register (HDR), the cohort member had been given a mental or behavioural disorders diagnosis between 1987 and 2012 (ICD-9: 290–319, ICD-10: F00–99) based on International Classification of Diseases 9th Revision (ICD-9) or ICD-10 codes.

Teenage pregnancy

Based on this information from the MBR, the age of the female cohort member at the time of giving birth for the first time was calculated, and we gave a value of one for those who have given birth prior to age 20.

Statistical methods

Frequency distributions of the parental social assistance variables and control variables with percentages by child outcomes were calculated for females and males, and are shown in table 2. In model 1, using logistic regression, we estimated bivariate associations between parental social assistance receipt and all four dependent variables separately for females and males (table 3), replicating earlier findings about the association of childhood poverty on negative early adult outcomes.

In model 2, we categorised parental social assistance receipt for two different durations (1–3 months and more than 4 months) and adjusted with other related family sociodemographic variables (education, SES, non-nuclear family, teenage pregnancy) and psychiatric care.

In model 3, we estimated the odds ratios for all the outcome variables within five separate age categories (0–2, 3–6, 7–12, 13–16, 17+) for those who had received social assistance for at least four months, to estimate the *impact of timing* of social assistance receipt, while adjusting for the same parent-related covariate as in model 2. The results of model 3 are shown in table 5. All statistical analyses were performed using IBM SPSS Statistics, version 22.

Results

Almost 38% of parents received social assistance during the first 21 years of the cohorts' lives; 11,062 female (38.1%) and 11,537 male (37.9%) cohort members had a parent who had received social assistance. Furthermore, the outcome measures were closely associated with parental social assistance receipt. As shown in table 2, over 60% of the cohort members who left school early had a parent who had received social assistance (66% for females and 60.8 for males). Of the female cohort members who had a criminal record, 68.8% had a parent who had received social assistance (58.3% for males). Also, of the male cohort members with a mental health disorders, 58.3% had a parent who had received social assistance (53.8% for females).

Table 3 also shows the unadjusted odds ratios from the logistic regression models estimating the association between parental social assistance receipt and cohort members' early adult outcomes. Parental social assistance receipt increased the risk for all the studied child outcomes significantly. Parental social assistance increased the risk for early school leaving three to four times (OR 4.00 for females, 3.30 for males), and criminal records almost four times for females (3.73) and almost two and a half times for males (2.47). The OR for mental disorders for females was 2.09, and for males 2.51. Furthermore, parental social assistance receipt increased the risk for teenage pregnancy by 2.85. Females' ORs were larger than those of males, except for mental disorders.

Table 4 shows the adjusted ORs for two categories of parental social assistance receipt duration (1–3 months and 4+ months) by gender (model 2). The impact of the length of social assistance receipt is clear. Our results show that the highest risks for all negative outcomes were found when parental social assistance receipt had lengthened (4+ months), even though both of the different length categories were statistically significant. When parental social assistance receipt had lasted for 4 months or longer, the OR for early school leaving was 3.07 for females and 2.63 for males, the OR for criminal record was 2.45 for females and 1.98 for males, for mental health disorder the OR was 1.77 for females and 1.90 for males, and the OR for teenage pregnancy for females was 2.36. According to our results, childhood poverty associated more negatively with female outcomes. When childhood poverty was more transient, gender differences in outcomes were almost inexistent. A low level of parent educational attainment was also associated with an increased risk for all negative outcomes, with females showing larger ORs for early school leaving (female 2.04 and male 1.99) and criminal record

Table 2. Composition of dependent variables for females and males

	Early school leaving				Criminal record				Mental health disorder				Teenage pregnancy	
	Females		Males		Females		Males		Females		Males		Females	
Variable (parental indicators)	n	%	n	%	n	%	n	%	n	%	n	%	n	%
Number of births	4,688		6,296		890		4,238		3,699		2,816		2,834	
Parent's highest education (early school leaving vs highest)	709	15.1	862	13.7	158	17.8	530	12.5	374	10.1	299	10.6	374	13.0
Parent's highest SES														
Upper	841	17.9	1,367	21.7	145	16.3	848	20.0	1,037	28.0	772	27.4	486	17.0
Lower	1,866	39.8	2,482	39.4	338	38.0	1,745	41.2	1,437	38.8	1,044	37.1	1,166	40.7
Workers	1,645	35.1	3,041	32.4	337	37.9	1,404	33.1	1,015	27.4	841	29.9	1,023	35.7
Other	223	4.8	270	4.3	49	5.5	163	3.8	143	3.9	117	4.2	131	4.6
Unknown	113	2.4	136	2.2	21	2.4	78	1.8	67	1.8	42	1.5	61	2.1
Other than nuclear family	3,152	67.2	4,002	63.6	632	71.0	2,631	62.1	2,269	61.3	1,749	62.1	1,863	65.0
Teenage pregnancy	346	7.4	418	6.6	79	8.9	292	6.9	197	5.3	180	6.4	237	8.3
Parental psychiatric in/outpatient care	1,365	29.1	1,670	26.5	282	31.7	1,133	26.7	1,167	31.5	976	34.7	831	29.0
Parental social assistance, receipt	3,095	66.0	3,826	60.8	612	68.8	2,469	58.3	1,991	53.8	1,642	58.3	1,789	62.4
Parental social assistance, duration														
0 months	1,593	34.0	2,470	39.2	278	31.2	1,769	41.7	1,708	46.2	1,174	41.7	1,078	37.6
1–3 months	438	9.3	639	10.1	70	7.9	417	9.8	328	8.9	245	8.7	270	9.4
4 months or more	2,657	56.7	3,187	50.6	542	60.9	2,052	48.4	1,663	45.0	1,397	49.6	1,519	53.0
Parental social assistance, duration x age														
4 months or more, child on age 0–2	1,587	33.9	1,816	28.8	354	39.8	1,191	28.1	914	24.7	781	27.7	910	31.7
4 months or more, child on age 3–6	636	13.6	840	13.3	118	13.3	549	13.0	426	11.5	379	13.5	376	13.1
4 months or more, child on age 7–12	324	6.9	417	6.6	43	4.8	239	5.6	240	6.5	178	6.3	179	6.2
4 months or more, child on age 13–16	72	1.5	74	1.2	19	2.1	47	1.1	59	1.6	35	1.2	39	1.4
4 months or more, child on age 17 or more	38	0.8	40	0.6	8	0.9	26	0.6	24	0.6	24	0.9	15	0.5

Table 3. Model 1 – unadjusted odds ratios with 95% confidence intervals by dependent variables for females and males

	Early school leaving				Criminal record				Mental health disorder				Teenage pregnancy	
	Females		Males		Females		Males		Females		Males		Females	
Variable	OR	95% CI	OR	95 %CI	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Parental social assistance, receipt	4.00	3.74–4.27	3.30	3.12–3.50	3.73	3.23–4.31	2.47	2.47–2.82	2.09	1.95–2.24	2.51	2.32–2.71	2.85	2.64–3.08

OR, odds ratio; CI, confidence interval; * indicates statistical significance (<0.05)

Table 4. Model 2 with two categories for length of parental social assistance – adjusted odds ratios with 95% confidence intervals by dependent variables for females and males

	Early school leaving				Criminal record				Mental health disorder				Teenage pregnancy	
	Females		Males		Females		Males		Females		Males		Females	
Variables	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Parental social assistance, duration (0 months ref)	1.00		1.00		1.00		1.00		1.00		1.00		1.00	
1–3 months	1.63*	1.41–1.88	1.73*	1.55–1.93	1.38*	1.11–1.72	1.36*	1.23–1.52	1.26*	1.13–1.41	1.27*	1.13–1.43	1.55*	1.36–1.77
4 months or more	3.07*	2.79–3.38	2.63*	2.43–2.84	2.45*	2.12–2.83	1.98*	1.84–2.13	1.77*	1.64–1.91	1.90*	1.76–2.06	2.36*	2.16–2.59
Parent's highest education (early school leaving vs higher)	2.04*	1.82–2.29	1.99*	1.79–2.20	1.73*	1.47–2.03	1.48*	1.33–1.64	1.22*	1.10–1.37	1.26*	1.13–1.42	1.32*	1.17–1.48
Parent's highest SES (upper as ref.)	1.00		1.00		1.00		1.00		1.00		1.00		1.00	
Lower	1.22*	1.10–1.36	1.21*	1.11–1.31	1.43*	1.21–1.69	1.36*	1.25–1.47	0.92*	0.85–0.99	0.94	0.87–1.02	1.58*	1.42–1.75
Workers	1.65*	1.47–1.85	1.61*	1.46–1.76	1.91*	1.60–2.27	1.65*	1.52–1.80	0.99	0.90–1.07	1.08	0.98–1.18	2.08*	1.86–2.32
Other	1.59*	1.31–1.94	1.41*	1.18–1.67	1.86*	1.39–2.48	1.29*	1.09–1.52	0.99	0.84–1.17	1.01	0.85–1.21	1.87*	1.54–2.28
Unknown	1.80*	1.38–2.35	1.57*	1.25–1.98	1.54*	1.01–2.36	1.27*	1.01–1.59	0.95	0.76–1.19	0.93	0.73–1.19	1.82*	1.40–2.37
Other than nuclear family	1.64*	1.50–1.79	1.73*	1.61–1.86	1.61*	1.40–1.85	1.51*	1.41–1.62	1.39*	1.30–1.49	1.33*	1.24–1.43	1.32*	1.22–1.44
Teenage pregnancy	1.39*	1.18–1.62	1.36*	1.17–1.56	1.55*	1.26–1.91	1.47*	1.28–1.69	1.19*	1.02–1.38	1.21*	1.03–1.41	1.74*	1.50–2.02
Parental psychiatric in/outpatient care	1.00	0.91–1.09	1.02	0.95–1.10	1.08	0.95–1.22	1.10*	1.01–1.17	1.32*	1.23–1.41	1.58*	1.47–1.70	1.03	0.95–1.12

OR, odds ratio; CI, confidence interval, * indicates statistical significance (< 0.05)

Table 5. Model 3 with length and age of parental social assistance – adjusted odds ratios with 95% confidence intervals by dependent variables for females and males

	Early school leaving				Criminal record				Mental health disorder				Teenage pregnanc	
	Females		Males		Females		Males		Females		Males		Females	
Variable	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI	OR	95% CI
Parental social assistance, duration x age (0 months ref.)	1.00		1.00		1.00		1.00		1.00		1.00		1.00	
1–3 months	1.65*	1.43–1.90	1.74*	1.56–1.94	1.41*	1.13–1.75	1.37*	1.23–1.53	1.27*	1.14–1.42	1.28*	1.14–1.44	1.56*	1.37–1.78
4 months or more, child on age 0–2	3.90*	3.50– 4.35	3.19*	2.91–3.50	3.27*	2.78–3.84	2.38*	2.18–2.60	2.07*	1.89–2.27	2.13*	1.93–2.35	2.92*	2.62–3.25
4 months or more, child on age 3–6	2.51*	2.21– 2.86	2.33*	2.09–2.59	1.99*	1.63–2.42	1.86*	1.67–2.06	1.54*	1.39–1.72	1.82*	1.63–2.03	2.05*	1.81–2.33
4 months or more, child on age 7–12	2.41*	2.05–2.84	2.24*	1.95–2.57	1.39*	1.05–1.85	1.54*	1.34–1.77	1.62*	1.41–1.85	1.64*	1.42–1.89	1.93*	1.64–2.26
4 months or more, child on age 13–16	2.31*	1.71–3.13	1.43*	1.06–1.93	2.38*	1.56–3.62	1.12	0.83–1.50	1.43*	1.10–1.86	1.3	0.96–1.75	1.62*	1.18–2.22
4 months or more, child on age 17 or more	3.03*	2.02–4.55	1.64*	1.12–2.41	1.77.	0.89–3.52	1.32	0.91–1.92	1.37	0.93–2.02	1.94*	1.36–2.77	1.3	0.79–2.15
Parent's highest education (early school leaving vs higher)	1.95*	1.73– 2.18	1.93*	1.74–2.14	1.62*	1.38–1.91	1.43*	1.29–1.59	1.19*	1.06–1.33	1.24*	1.11–1.39	1.26*	1.12–1.42
Parent's highest SES (Upper as ref.)	1.00		1.00		1.00		1.00		1.00		1.00		1.00	
Lower	1.22*	1.09–1.35	1.20*	1.11–1.31	1.42*	1.20–1.68	1.35*	1.25–1.46	0.92*	0.85–0.99	0.94	0.86–1.01	1.57*	1.42–1.74
Workers	1.56*	1.42–1.79	1.57*	1.43–1.73	1.82*	1.53–2.17	1.63*	1.49–1.77	0.97	0.89–1.06	1.07	0.97–1.17	2.02*	1.81–2.26
Other	1.55*	1.27–1.90	1.37*	1.16–1.63	1.80*	1.35–2.41	1.26*	1.06–1.49	0.98	0.83–1.15	1	0.84–1.20	1.84*	1.51–2.24
Unknown	1.75*	1.34–2.28	1.54*	1.22–1.93	1.48	0.97–2.27	1.24	0.98–1.56	0.94	0.74–1.18	0.92	0.72–1.18	1.78*	1.36–2.32
Other than nuclear family	1.62*	1.48–1.77	1.72*	1.60–1.85	1.58*	1.38–1.82	1.51*	1.41–1.61	1.39*	1.30–1.49	1.33*	1.23–1.43	1.31*	1.20–1.43
Teenage pregnancy	1.26*	1.07–1.47	1.25*	1.08–1.44	1.37*	1.11–1.70	1.36*	1.18–1.56	1.11	0.95–1.30	1.15	0.98–1.34	1.59*	1.36–1.85
Parental psychiatric in/outpatient care	0.98	0.90–1.07	1.02	0.94–1.10	1.06	0.93–1.20	1.09*	1.01–1.17	1.31*	1.22–1.40	1.58*	1.47–1.70	1.02	0.94–1.11

OR, odds ratio; CI, confidence interval, * indicates statistical significance (< 0.05) (female 1.73 and male 1.48). Family instability associated most significantly with early school leaving among males (OR 1.73) and parental psychiatric care with male mental health disorders (OR 1.58). Gender differences in the covariate associations with the outcome measures were largely smaller than those associated with parental social assistance receipt.

When evaluating the impact of long-term parental social assistance receipt at different ages of the birth cohort members' lives, the results indicate the highest risk when the cohort member was less than two years of age (table 5, model 3). For all the early adult outcomes studied, the highest odds ratios were found in the age category 0–2 years of age (early school leaving females: OR 3.90, males: OR 3.19; criminal record females: OR 3.27, males: OR 2.38; mental health disorders females: OR 2.07, males: OR 2.13; and teenage pregnancy: OR 2.92). In the age category of more than 17 years of age, the only significant outcome for females was early school leaving, and for males, early school leaving and mental health disorders. For the criminal record outcome, the second highest period of risk for females was in the age category 13–16 years of age (OR 2.38) and for males, 3–6 years of age (OR 1.86). For the mental health disorders outcome, the second highest period of risk for females was in the age category 7–12 years of age (OR 1.62), and for males, more than 17 years of age (OR 1.94). For the adolescent years, each successive age category had a slightly lower risk than the previous one. In terms of gender differences, all age categories for females associated with higher odds ratios than for males for early school leaving and criminal record. For the mental health disorder outcome, gender differences were mainly absent with the exception of age category 17+ where males had an OR 1.94 and females a non-statistically significant OR 1.37.

Discussion

Decades of research have documented the negative impact of childhood poverty on later wellbeing. This paper examined the association between parents' social assistance receipt and early adult outcomes using register data from Finland. We were especially interested in how the timing of social assistance receipt associated with early adult outcomes.

Results of our study show that parental social assistance receipt during childhood increases the risk of all measured negative adjustment outcomes: early school leaving, criminality, mental health, and wellbeing problems in early adulthood. Most detrimental effects of social assistance receipt were found when parents received social assistance for a long period, more than four months compared to more transient one- to three-month periods. These results are consistent with previous results that long-term social assistance receipt is more harmful than short-term for child development (Duncan & Yeung, 1995). Furthermore, according to the results, the impact of social assistance receipt was highest when the child was less than two years of age when the family received social assistance for the first time. Our finding is supported also by the recent study by Kalil and colleagues (2016), which suggested that poverty during the first five years of life poses the largest risk for children's development. Our results suggest that in fact, the first two years of life may be most crucial. The present study builds on the key conclusion that social assistance receipt in early childhood appears to matter more for shaping later development than economic conditions during adolescence. Early childhood is a developmental period that may be especially sensitive to environmental conditions affected by family income and early courses of development may reach well into adulthood.

We also found some gender differences. Our results indicate that females are more at risk from socioeconomic factors, while males are more affected by family instability and ill health. Previous research regarding gender differences has been inconclusive; as such, our results add to the evidence that gender differences may in fact be significant. Our results also support earlier research (Brooks-Gunn & Duncan, 1997) that argues poverty is more harmful for cognitive development than emotional outcomes. We show that at all ages in childhood, poverty associates most negatively with early school leaving and less severely with criminality, mental health, and teenage pregnancy.

The present study has both strengths and limitations. A major advantage of this total birth cohort study with 25-year follow-up is that it can simultaneously identify the children's age period during which they were exposed to poverty for the first time and the duration of that exposure, and adjust for several previously identified risk factors. The data contain reliable measures of both social assistance receipt and correlated aspects of parental socioeconomic status and it is possible to estimate the separate contributions of each. Due to data limitations, previous research has often not been able to include all the necessary control variables, in particular family SES and parental mental health (Mayer, 1997). Furthermore, many survey studies related to social assistance receipt often have difficulties reaching exactly those who are at most risk, as willingness to take part in research is related to SES and, as a result, often those with low SES are under-represented in surveys. The data used here are based on Finnish national registers, which provide virtually complete datasets and thus are not biased by selective attrition.

The limitations of the study include the fact that the findings reflect the experiences of a single cohort of young people studied in a particular social context and over a specific historical period. Finnish children born in 1987 grew up during the recession of the 1990s, when large numbers of families faced unemployment and financial difficulties. Although we could not identify any significant differences in the sociodemographic distribution of the families experiencing poverty for the first time at different ages of the cohort members, it may be that we suffer from an omitted variable bias, and in fact, some other factors explain the associations we have identified. The results of our study should be replicated with other cohorts, born and raised during different economic cycles. Another limitation of our study is that some of the parent indicators may coincide with the child outcomes, which poses a risk of reverse causality. Future studies with updated register linkages that allow for completely separate follow-up windows for parental indicators and child outcomes should test for this potential limitation.

Despite these limitations, the study findings provide clear evidence of linkages between social assistance receipt during childhood and early adult outcomes, and suggest that the earlier years of a child’s life are most sensitive to the environmental effects such as those associated with poverty.

Funding

The authors thank the Alli Paasikivi Foundation, the Emil Aaltonen Foundation, the Academy of Finland (grant number 288960 and PSYCHORTS consortium), and the National Institute for Health and Welfare for the financial support for the research.

References

Becker, G. S. (1991). *A treatise on the family*. Cambridge, Mass: Harvard University Press.

Ben-Shlomo, Y., & Kuh, D. (2002). A life course approach to chronic disease epidemiology: conceptual models, empirical challenges and interdisciplinary perspectives. International Journal of Epidemiology, *31*(2), 285–293. https://doi.org/10.1093/ije/31.2.285

Brooks-Gunn, J., & Duncan, G. J. (1997). The effects of poverty on children. *The Future of Children*, *7*(2,) 55–71. https://doi.org/10.2307/1602387

Crystal, S., & Shea, D. (1990). Cumulative advantage, cumulative disadvantage, and inequality among elderly people. *The Gerontologist*, *30*(4), 437–443. https://doi.org/10.1093/geront/30.4.437

Cunha, F., Heckman, J. J., Lochner, L., & Masterov, D. V. (2006). Interpreting the evidence on life cycle skill formation. *Handbook of the Economics of Education*, *1*, 697–812. https://doi.org/10.1016/S1574-0692(06)01012-9

Dickerson, A., & Popli, G. K. (2016). Persistent poverty and children's cognitive development: evidence from the UK Millennium Cohort Study. *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, *179*(2), 535–558. https://doi.org/10.1111/rssa.12128

Duncan, G. J., & Yeung, W. J. J. (1995). Extent and consequences of welfare dependence among America's children. *Children and Youth Services Review*, *17*(1–2), 157–182. https://doi.org/10.1016/0190-7409(95)00007-Y

Duncan, G. J., & Brooks-Gunn, J. (Eds.). (1999). *Consequences of growing up poor*. New York, NY: Russell Sage Foundation.

Duncan, G. J., & Brooks-Gunn, J. (2000). Family poverty, welfare reform, and child development. *Child development*, *71*(1), 188–196. https://doi.org/10.1111/1467-8624.00133

Duncan, G. J., Ziol-Guest, K. M., & Kalil, A. (2010). Early-childhood poverty and adult attainment, behavior, and health. *Child Development*, *81*(1), 306–325. https://doi.org/10.1111/j.1467-8624.2009.01396.x

Duncan, G., Telle, K., Ziol-Guest, K. & Kalil, A. (2010). Economic deprivation on early childhood and adult attainment: comparative evidence from Norwegian registry data and the US panel study of income dynamics. In Smeeding, T. M., Erikson, R., & Jäntti, M. (Eds.) *Persistence, privilege, and parenting. The comparative study of intergenerational mobility.* (pp.209–234). New York, NY: Russell Sage Foundation.

Elder Jr, G. H. (1974). *Children of the Great Depression*: Social Change in Life Experience. Chicago, IL: University of Chicago Press.

Farkas, G., & Beron, K. (2004). The detailed age trajectory of oral vocabulary knowledge: Differences by class and race. *Social Science Research*, *33*(3), 464–497. https://doi.org/10.1016/j.ssresearch.2003.08.001

Gibb, S. J., Fergusson, D. M., & Horwood, L. J. (2012). Childhood family income and life outcomes in adulthood: findings from a 30-year longitudinal study in New Zealand. *Social Science & Medicine*, *74*(12), 1979–1986. https://doi.org/10.1016/j.socscimed.2012.02.028

Hansen, H. T. (2008). The dynamics of social assistance recipiency: Empirical evidence from Norway. *European Sociological Review*, *25*(2), 215–231. https://doi.org/10.1093/esr/jcn040

Heckman, J. J., & Masterov, D. V. (2007). The productivity argument for investing in young children. *Applied Economic Perspectives and Policy*, *29*(3), 446–493. https://doi.org/10.3386/w13016

Holzer, H., Schanzenbach, D. W., Duncan, G. J., & Ludwig, J. (2007). The economic costs of poverty in the United States: Subsequent effects of children growing up poor. Discussion Paper no. 1327-07. Institute for Research on Poverty. Retrieved from https://irp.wisc.edu/publications/dps/pdfs/dp132707.pdf

Ilmakunnas, I. (2018). Risk and vulnerability in social assistance receipt of young adults in Finland. *International Journal of Social Welfare*, *27*(1), 5–16. https://doi.org/10.1111/ijsw.12273

Kalil, A., Duncan, G. J., & Ziol-Guest, K. M. (2016). Early childhood poverty: Short and long-run consequences over the lifespan. In *Handbook of the life course* (pp. 341–354). Cham: Springer. https://doi.org/10.1007/978-3-319-20880-0_15

Kalil, A., Ryan, R., & Corey, M. (2012). Diverging destinies: Maternal education and the developmental gradient in time with children. *Demography*, *49*(4), 1361–1383. https://doi.org/10.1007/s13524-012-0129-5

Kangas, O., & Ritakallio, V.-M. (2008). [Köyhyyden mittaustavat, sosiaaliturvan riittävyys ja köyhyyden yleisyys Suomessa]. Sosiaali- ja terveysturvan selosteita. No. 61. Helsinki.

Knudsen, E. I., Heckman, J. J., Cameron, J. L., & Shonkoff, J. P. (2006). Economic, neurobiological, and behavioral perspectives on building America’s future workforce. *Proceedings of the National Academy of Sciences*, *103*(27), 10155–10162. https://doi.org/10.1073/pnas.0600888103

Kauppinen, T. M., Angelin, A., Lorentzen, T., Bäckman, O., Salonen, T., Moisio, P., & Dahl, E. (2014). Social background and life-course risks as determinants of social assistance receipt among young adults in Sweden, Norway and Finland. *Journal of European Social Policy*, *24*(3), 273–288. https://doi.org/10.1177/0958928714525818

Kessler, R. C., & Cleary, P. D. (1980). Social class and psychological distress. *American Sociological Review*, *45*(3), 463–478. https://doi.org/10.2307/2095178

Kuivalainen, S. (2013). Social assistance in the 2010s. A study on social assistance clients and granting practices [Toimeentulotuki 2010-luvulla – Tutkimus toimeentulotuen asiakkuudesta ja myöntämiskäytännöistä]. National Institute for Health and Welfare (THL). Report 9/2013. Helsinki.

Mayer, S. E. (1997). *What money can't buy: Family income and children's life chances*. Cambridge, Mass.:Harvard University Press.

Mayer, S. E., Kalil, A., Oreopoulos, P., & Gallegos, S. (2015). *Using behavioral insights to increase parental engagement: The parents and children together (PACT) intervention* (No. w21602). National Bureau of Economic Research. https://doi.org/10.3386/w21602

McLeod, J. D., & Kessler, R. C. (1990). Socioeconomic status differences in vulnerability to undesirable life events. *Journal of Health and Social Behavior*, *31*(2), 162–172. https://doi.org/10.2307/2137170

McLoyd, V. C. (1998). Socioeconomic disadvantage and child development. *American Psychologist*, *53*(2), 185. https://doi.org/10.1037/0003-066X.53.2.185

Najman, J. M., Hayatbakhsh, M. R., Heron, M. A., Bor, W., O'Callaghan, M. J., & Williams, G. M. (2009). The impact of episodic and chronic poverty on child cognitive development. *The Journal of Pediatrics*, *154*(2), 284–289.Nielsen, M. J., Juon, H. S., & Ensminger, M. (2004). Preventing long-term welfare receipt: the theoretical relationship between health and poverty over the early life course. *Social Science & Medicine*, *59*(11), 2285–2301. https://doi.org/10.1016/j.jpeds.2008.08.052

Official Statistics of Finland, Social Protection (2016). Social assistance 2015 [e-publication). ISSN 1798-0887. Helsinki: National Institute for Health and Welfare, Finland [referred 12.6.2018]. Access method: https://www.julkari.fi/bitstream/handle/10024/131612/Tr19_16.pdf?sequence=4

Paananen R. & Ristikari T. & Gissler M. (2014) Young people's well-being in Finland in the light of the 1987 Finnish birth cohort. National Institute for Health and Welfare, THL 2014.

Pakpahan, Y. M., Suryadarma, D., & Suryahadi, A. (2009). Destined for Destitution: Intergenerational Poverty Persistence in Indonesia. Chronic Poverty Research Centre Working Paper No. 134. https://doi.org/10.2139/ssrn.1531697

Reiss, F. (2013). Socioeconomic inequalities and mental health problems in children and adolescents: a systematic review. *Social science & medicine*, *90*, 24–31. https://doi.org/10.1016/j.socscimed.2013.04.026

Saraceno, C. (Ed.). (2002). *Social assistance dynamics in Europe: national and local poverty regimes*. Bristol, UK: Policy Press. https://doi.org/10.2307/j.ctt1t89hvw

Schoon, I., Bynner, J., Joshi, H., Parsons, S., Wiggins, R. D., & Sacker, A. (2002). The influence of context, timing, and duration of risk experiences for the passage from childhood to midadulthood. *Child Development*, *73*(5), 1486–1504. https://doi.org/10.1111/1467-8624.00485

Schoon, I., & Bartley, M. (2008). The role of human capability and resilience. *Psychologist*, *21*(1), 24–27.

Schilbach, F., Schofield, H., & Mullainathan, S. (2016). The psychological lives of the poor. *American Economic Review*, *106*(5), 435–440. https://doi.org/10.1257/aer.p20161101

Sobolewski, J. M., & Amato, P. R. (2005). Economic hardship in the family of origin and children's psychological well-being in adulthood. *Journal of Marriage and Family*, *67*(1), 141–156. https://doi.org/10.1111/j.0022-2445.2005.00011.x

Solantaus, T., Leinonen, J., & Punamäki, R. L. (2004). Children's mental health in times of economic recession: replication and extension of the family economic stress model in Finland. *Developmental Psychology*, *40*(3), 412–429. https://doi.org/10.1037/0012-1649.40.3.412

Spears, D. (2011). Economic decision-making in poverty depletes behavioral control. *The BE Journal of Economic Analysis & Policy*, *11*(1), 1935-1682. https://doi.org/10.2202/1935-1682.2973

Wagmiller Jr, R. L., Lennon, M. C., Kuang, L., Alberti, P. M., & Aber, J. L. (2006). The dynamics of economic disadvantage and children's life chances. *American Sociological Review*, *71*(5), 847–866. https://doi.org/10.1177/000312240607100507

Weitoft, G. R., Hjern, A., Batljan, I., & Vinnerljung, B. (2008). Health and social outcomes among children in low-income families and families receiving social assistance—a Swedish national cohort study. *Social Science & Medicine*, *66*(1), 14–30. https://doi.org/10.1016/j.socscimed.2007.07.031

Winning, A., Glymour, M. M., McCormick, M. C., Gilsanz, P., & Kubzansky, L. D. (2016). Childhood psychological distress as a mediator in the relationship between early-life social disadvantage and adult cardiometabolic risk: Evidence from the 1958 British birth cohort. *Psychosomatic Medicine*, *78*(9), 1019–1030. https://doi.org/10.1097/PSY.0000000000000409

Longitudinal and Life Course Studies 2018 Volume 9 Issue 3 Pp 327–350 ISSN 1757-9597

Do private school girls marry rich?

Francis Green UCL Institute of Education, UK
Francis.green@ucl.ac.uk
Golo Henseke UCL Institute of Education, UK
Samantha Parsons UCL Institute of Education, UK
Alice Sullivan UCL Institute of Education, UK
Richard Wiggins UCL Institute of Education, UK

(Received January 2018 Revised March 2018) http://dx.doi.org/10.14301/llcs.v9i3.496

Abstract

This paper considers for the first time whether there is school-type homogamy, and whether for women there are significant advantages from private schooling as a consequence of school-type homogamy. Its focus is Britain, where a private education is associated with substantial labour market advantages and where access is socially exclusive. We find that privately educated women are 7 percentage points more likely than observably similar state-educated women to marry privately educated men. Privately educated married women have husbands who earn 15% higher pay, according to the BHPS-UKHLS panel (20% at age 42, according to the British Cohort Study). Causation is not established and considerable caution would be needed if interpreting these associations as reflecting causal effects from private schools. The findings nevertheless raise anew the issue of the negative association between Britain's private schooling and social mobility.

Keywords

Private school; pay; social mobility; homogamy; marriage

Introduction

Historically, education in Britain for affluent families' girls in private schools focused on domestic economy and "accomplishments", rather than intellectual pursuits, in a socially segregated setting, ready for a suitable marriage within their class (Gathorne-Hardy, 1977). In the last part of the 19th century a number of pioneering schools emerged with a more academic curriculum, forerunners of a convergence between the orientations of boys and girls schools in the last half-century, as women raised their labour force participation. Nevertheless, the idea that a private education might be a springboard (intended or not) for securing marriage to a high-earnings husband retains its place in some public discourse even in modern times.[1] This expectation draws some credence from the extensive evidence of homogamy by education and by social class in modern societies (see the review by Schwartz, 2013). Homogamy within social class is, of course, nothing new. The last half-century, however, has seen a transformation in the importance of education for economic and social success (Goldin & Katz, 2008). Since in Britain the dividing line between state and private education is especially sharp, it can be suggested that homogamy associated with education level would additionally take the form of homogamy associated with school-type (interpreted here as private or state). Yet hitherto no studies have investigated whether privately educated people are disproportionately likely to marry others who are privately educated, or whether there is any return to private schooling via marriage.

This paper examines whether there is homogamy within school-type in England, and whether for women there are significant returns to private schooling through marriage. Private education is associated with substantial labour market advantages in Britain, including greater access to high-status and influential jobs and a large pay premium (Dearden, Ferri, & Meghir, 2002; Green, Machin, Murphy, & Zhu, 2012; McKnight, 2015; Green, Henseke, & Vignoles, 2017; Green, Parsons, Sullivan, & Wiggins, 2018). Unsurprisingly, the schools' pupil populations are drawn from high-income families, in many of which one or both parents have themselves been privately educated (Dearden, Ryan & Sibieta, 2011; Green, Anders, Henderson, & Henseke 2017). The high labour market returns and the exclusivity together lead, it is held, to a constraint on social mobility and a buttress for high levels of inequality in Britain. Any additional return to private schooling via marriage to a higher-earning spouse would enhance such effects, while at the same time altering the calculus of parents' school choice.

The institutional separation between the private and state sectors is marked by a distinct and looser form of regulation in the private sector. British private schools can admit whomever they choose, charge whatever fees they like, determine their own budgets within market constraints, and manage and govern independently. They are subject only to their own voluntary regulatory body and, more pertinently, the pressures of the public national examination system. While many are engaged in minor relationships (such as the sharing of facilities) with neighbourhood state schools, their relationships are mainly with other private schools. The private schools have their own sectoral institutions (at the pinnacle of which is the Independent Schools Council). Unlike in the majority of other country's private sectors, Britain's private schools now receive virtually no direct public subsidies. There used to be some subsidy through the Assisted Places Scheme that ran from 1981 till 1997 (Power, Whitty, & Wisby, 2006), and historically local government would subsidise boarding school places in private schools for children in special circumstances. But such injections of funding have been virtually absent in the 21st century.[2] The private schools do benefit, however, from some tax concessions, which, for example, lower their local business taxes by 80%; however, these subsidies are relatively minor, amounting to only a few percent of their turnover. There are 82% of private schools with the status of charities, which among other advantages allows them to accept donations favourably; 16% are for-profit institutions.

This absence of substantive government funding is one reason why the private cost is especially high – in 2017 the average annual fee before any extras was £13,818 for day school and £32,259 for boarding school. Since the 1980s the schools have continuously elevated the fees in a competition over facilities and small class sizes. The 2017 fees were approximately three times the 1980 fees in real terms, and the teacher-pupil ratio has become twice as high in the private sector as in the state sector. To compete with state education, private schools in Britain have increased their emphasis on academic and cultural achievements (Turner, 2015; Peel, 2015;), not least so their pupils could gain access to high-ranked universities (Boliver, 2013). The private school sector in Britain arrived, at the start of this century, in good financial and academic health, still largely serving the increasingly affluent professional and managerial classes. The proportion of school children in private school has remained relatively steady, at between 6 and 7% for both sexes since the 1980s, despite the fee rises. Moreover, participation increases with age, up to approximately 17% at upper secondary level, which occurs in part because many state school pupils have left school at 16 (even though they may stay in education). There is a disproportionate concentration of private schools in the South East and in London. About one in four schools take boarders, and one in five are single sex, unlike in the state sector where single-sex education is rare (2% of schools) and boarding even rarer (less than 1%). With few exceptions, the elite sector of schooling is in the private sector, and the privately educated, in addition to enjoying the earnings premiums noted above, which they receive as a result of their high education levels and post-education labour market advantages, continue to dominate public life in Britain with vastly disproportionate representation in influential positions (Kirby, 2016).

In this context, it is important to understand better the links between private schooling and marriage outcomes in the modern era. The argument we develop below may be concisely stated as follows. Private schooling for women may lead to an economic return via the marriage market in two ways. First, owing to the high quality of private schooling in Britain, combined with educational homogamy, privately schooled women are more likely to marry a well-educated husband who in turn will therefore have a better job and higher earnings. Second, we hypothesise that there is also a tendency for 'school-type homogamy', in which privately-educated women are more likely (than with a random draw) to marry privately educated men; since privately educated men earn a premium beyond their education premium in Britain's labour market, this also yields a gain for the women who marry them.

While much of the argument would apply also to men, our focus here is on the marriage returns from private schooling for women, as manifested in their husband's labour market outcomes. We do this in part to reflect the historical role of girls' education, noted above, as a path to a good marriage rather than academic achievement. In addition, the own-wage returns to education, to private schooling and to marriage are all known to be gender specific (e.g. Silles, 2007; Green et al., 2012; Antonovics & Town, 2004).

To examine these marriage market returns from private schooling for females, we required suitable longitudinal data covering both partners. Accordingly, we use the British Household Panel Study combined with its successor the UK Household Longitudinal Study (BHPS-UKHLS); we also use data drawn from the 2012 wave of the longitudinal British Cohort Study when all cohort members were aged 42. After setting out our theoretical expectations in detail in the next section, we follow with a description of the data. The subsequent section presents findings concerning school-type homogamy. We investigate whether there is, as expected, an above-random chance that the husbands of privately educated women had also been privately educated, even after controlling for the women's social background. In Results, we investigate the association between women's private schooling and their husband's earnings, followed by our conclusions.

School-type homogamy and the marriage market return to private education

In this section, we outline how school-type homogamy might arise in Britain, partly as a by-product of the well-known phenomenon of homogamy by education level. This argument then paves the way for an analysis of the benefits of a private education for women, in terms of the earnings of future marriage partners.

The tendency for people to marry or partner with others of similar educational status is well established (Elder Jr, 1969; Uunk, Ganzeboom & Róbert, 1996; Blackwell, 1998; Kalmijn, 1998; Reynolds, Baker & Pedersen, 2000; Blossfeld & Timm, 2003; Schwartz & Mare, 2005; Schwartz, 2013; Greenwood, Guner, Kocharkov & Santos, 2014) and has been implicated in growing resource inequalities between households (Blossfeld & Buchholz, 2009). Homogamy is argued to result from matched preferences, and from competition in the marriage market for spouses with valuable traits. Blossfeld (2009) adds that the education system acts as a marriage market, with expanding post-school education providing contact opportunities for equally educated men and women at a transitional time when partner search is often high. Educational homogamy contributes significantly to both economic inequality and low inter-generational social mobility in Britain (Ermisch, Francesconi & Siedler, 2006). The trends in educational homogamy across countries are mixed (Blossfeld, 2009). There is evidence that educational homogamy declined in Britain between 1973 and 1986/7, but rebounded upwards between 1986/7 and 1994/5 (Halpin & Chan, 2003). In the United States, by contrast, there is a lack of consensus as to whether educational homogamy has risen or remained stable (Siow, 2015; Gihleb & Lang, 2016, though any change seems not, in itself, to have had a large effect on inequality (Schwartz, 2013; Breen & Andersen, 2012; Greenwood et al., 2014). Increases have been attributed to the changing socio-demographic composition of the population, changes in marriage rates, and more generally to a societal modernisation trend whereby achieved characteristics have become more important, relative to social and ethnic background, for partner selection; rising inequality is another factor.

Educational homogamy has implications for matching in terms of school type, because private schooling in Britain yields a substantial return in terms of educational achievement. Studies have shown that private schooling delivers (on average) superior performance in nationally validated exams, and in access to universities, especially high-ranking universities which select on academic achievement (Dearden et al., 2002; Bukodi & Goldthorpe, 2011; Gregg & Macmillan, 2010; Goodman, Gregg & Washbrook, 2011; Crawford, Goodman & Joyce, 2010; Jerrim, Parker, Chmielewski & Anders, 2015; Sullivan, Parsons, Wiggins, Heath & Green, 2014).[3] A consequence of this private schools' quality advantage is that educational homogamy is expected to be partially reflected in 'school-type homogamy': an above-random tendency for privately educated women to marry privately education men.

We also conjecture that school-type homogamy may arise directly, as a consequence of the shared values and social networks inculcated through private education, and from the spatial and social clustering of the privately educated. These factors can apply at multiple stages – at school and thereafter in universities, workplaces or other social networks.

British private secondary schools were traditionally single-sex, and the development of long-lasting romantic relationships was accordingly limited during school years. Nevertheless, even though many girls' schools came to fully embrace academic learning objectives, these schools were often largely concerned with preparing girls for an advantageous marriage (Gathorne-Hardy, 1977; Blandford, 1977). By the 1980s, the majority of the (traditionally elite, male) HMC (Headmasters' Conference) private schools admitted girls as well as boys (Walford, 1983). This change was motivated largely by economic pressures on the private schools, but Walford also cites views that girls would act as a civilising influence, and help the boys to learn how to interact with the opposite sex. There is only small-scale qualitative evidence for relationship development within or among private schools and its link to the reproduction of privileged status (Maxwell & Aggleton, 2014a, 2014b). For those who do not pursue their education further, post-school networks through family and work are likely to be disproportionately within school type, owing to shared values and friendship networks developed within and surrounding the schools (Ashley, Duberley, Sommerlad & Scholarios, 2015; Cookson & Persell, 1985).

For those who proceed to university, however, it could be expected that social networks broaden beyond the cultural range of potential partners encountered by the privately educated in their school days. Most of the higher-educated adult population in Britain have been to university at a time when no or low fees were payable, and many would have been in receipt of maintenance grants. Thus universities have been substantially more socially inclusive than private schools, and we would expect that school-type homogamy would be less frequent among those who proceed to university after school than among those who do not go to university. Nevertheless, a much higher proportion of private school leavers than of state school leavers go to university, and these privately educated students are concentrated in high-ranking universities (Boliver, 2013). The concentration is yet greater in certain subjects (arts and humanities), which is notable given recent Europe-wide evidence of field-of-study assortative mating (Bičáková & Jurajda, 2016). This spatial and disciplinary concentration of privately-educated people at a time of life when a notable part of mate selection takes place could therefore limit the extent to which going to college diminishes the probability of intra school-type mating.

While university is an important site of mate selection, selection and marriage also occur later in the context of work or other domains. Yet since private schooling also promotes access to scarce good jobs, and since value-sharing is thought to contribute to that advantage (Ashley et al., 2015), the social and spatial proximity of like-for-like school types can continue after the end of full-time education.

Our hypothesis, therefore, is that school-type homogamy arises both indirectly as the by-product of educational homogamy and private school educational quality, and directly since private schooling augments the extent to which values and social networks are shared. Formally, we conceive the high educational quality of private schooling for either females or males in the form of a latent quality index, τ_i^* which positively affects the quality of schooling of either wife or husband (i=w,h) and hence the achieved education level (S_i), according to a standard educational production function $S_i = f(\tau_i^*, x_i)$, with $\partial S_i / \partial \tau_i^* = f_{\tau_i^*} > 0,\ i = w, h$ with x a vector of all other factors affecting educational achievement. Here, for ease of exposition, we treat S_i as a continuous indicator, though this is operationalised conventionally below as discrete education levels, and we later operationalise τ_i^* with a dummy indicator for private schooling. Then, the hypothesis can be expressed in terms of the relationship between the husband's and wife's school qualities resulting from the marriage match, with both an indirect term linked to education-level homogamy ($\partial S_h / \partial S_w > 0$) and a direct term:

$$\partial \tau_h^* / \partial \tau_w^* = \alpha\, \partial S_h / \partial S_w + \beta(S) > 0 \qquad (1)$$

Here, α is capturing the extent to which educational homogamy is reflected earlier in the school-type (because of private school quality), while $\beta(S) > 0$ is a parameter capturing the extent to which a rise in school quality of the wife is directly associated, as a consequence of shared values and networks, with an increased school quality of the husband. As suggested above, social networks will be broadened for those who attend university, despite some degree of private school concentration within universities; hence we expect that $\beta^{/} < 0$.

In the light of this hypothesis of school-type homogamy, there are two arguments as to why there could be a premium for women in Britain from private schooling, manifested in the earnings of their husbands. First, their private schooling delivers higher educational achievement, which as a result of educational homogamy implies that their husbands have higher educational achievement than those of state-educated women, which yield them higher wages. Second, with school-type homogamy their husbands are more likely to be privately educated, and this yields an additional boost to their earnings. Studies variously reveal a large pay premium for private schooling in Britain, or a substantially greater chance of being in the top earnings decile, or of attending a high-status occupation, or of avoiding downward social mobility (Dolton & Vignoles, 2000; Naylor, Smith & McKnight, 2002; Dearden et al., 2002; Crawford & Vignoles, 2014; Macmillan, Tyler & Vignoles, 2015; Crawford & van der Erve, 2015; Green et al., 2012; Green et al., 2017a; Green et al., 2018). For women, much or all of this return is accounted for by their educational attainment, but for men there remains a direct pay premium, over and above that which can be explained by their educational credentials.

Formally we assume, as is conventional, that men will participate in the labour force and will receive an income (Y_h) according to a human capital earnings function, commensurate with their education achievement and private schooling:

$$Y_h = g(S_h, \tau_h^*) > 0 \text{ with } g_{Sh} > 0, g_{\tau_h^*} > 0 \qquad (2)$$

Combining this human capital earnings function with the hypothesised school-type homogamy (1), along with education level homogamy and the quality of private education, it is thus hypothesised that a woman's private education will be associated with having a more-educated, more likely privately-educated, and higher-earning husband:

$$\partial Y_h / \partial \tau_w^* = g_{S_h} f_{\tau_w^*}\, \partial S_h / \partial S_w + g_{\tau_h^*}\left[\alpha\, \partial S_h / \partial S_w + \beta(S)\right] > 0 \qquad (3)$$

This expression shows that a positive return to marriage for women's private education decision, in the form of the husband's income, stems from two elements. One element derives from a combination of the woman's educational benefits from the private schooling ($f_{\tau_w^*}$), the positive homogamy by education level ($\partial S_h / \partial S_w$) and the earnings benefits of that extra education (g_{S_h}). The second element stems from the "direct pay premium" ($g_{\tau_h^*}$) in combination with school-type homogamy, both direct (β) and indirect via education-level homogamy ($\partial S_h / \partial S_w$). Without any direct pay premium for men's private schooling, the benefit would be purely a by-product of the education level homogamy.

Consistent with the hypothesis of school-type positive homogamy, Evans and Tilley (2011) report, using the British Social Attitudes Survey, that privately educated people are more likely to marry other privately educated people. Similarly, Power, Sims and Whitty (2013) mention that among those who had gone through the Assisted Places Scheme

– a government-funded programme (1981–1997) for low-income pupils to attend private secondary schools – one in four of those cohabiting had privately-educated partners. Yet beyond these simple descriptive findings, there have been no studies, to our knowledge, of school-type homogamy as a sub-category of education homogamy, either in Britain or in any other country that resembles Britain through having relatively exclusive private schooling alongside a state system. In the context of the especially elite private school system in Britain, this represents an important gap in our understanding of the value of a private school investment and of the origins of low social mobility.

Nor have any studies, to our knowledge, examined the association between school-type and returns through marriage. However, two tangential studies focusing on university type are relevant to note at this point, each from countries that have an elite private education sector, namely Chile and the US. Kaufmann, Messner and Solis (2015) report that going to a higher-ranking university in Chile in the early 2000s meant that women were likely to marry someone who had scored more highly on the university admissions test. Meanwhile, Arum, Roksa and Budig (2008) report that, for a cohort of older graduates in the United States, going to a more elite university had raised the probability of marrying socially privileged spouses. For women, the pay-off was to marry husbands with higher income.

Our main aim is thus to investigate two hypotheses:

H1: There is predicted to be homogamy by school-type in Britain. We expect to find this, both with the raw data on school type and after conditioning on women's education and social background. If confirmed, does the homogamy diminish in magnitude, as predicted, among those with a university-level education?

H2: For married women, there is predicted to be an association between whether they have attended private schooling and their husbands' earnings.

Data

To address these questions, we required British data on households, covering both marriage partners' earnings and school-types, educational achievement data, and adequate controls for social background. Accordingly we combined data from the British Household Panel Survey (BHPS) and Understanding Society (the UK Household Longitudinal Study: UKHLS). BHPS was an annual panel study of each adult member of a representative sample of households in the UK between 1991 and 2008. UKHLS, from its second wave on, incorporated the BHPS sample, thus enabling us to track the original BHPS sample members and their current circumstances up until the latest UKHLS wave.[4]

In line with the majority of papers examining homogamy, our primary focus is on married, rather than cohabiting couples. This decision is motivated by indications that cohabiting but unmarried matches are much less durable (though some are a prelude to marriage) (Ermisch & Francesconi, 2000) and do not always involve full income-sharing between partners (Winkler, 1997; Hiekel, Liefbroer, & Poortman, 2014). Nevertheless, we test whether our findings are sensitive to this choice.

By selecting observations from women who were between 24 and 59 years old and their husbands, our sample is restricted to women below normal retirement age, almost all of whom had finished their initial cycle of full-time education. This leaves us with an unbalanced panel of approximately 72,000 person-year observations of 8,167 women, of which close to 43,000 observations were within marriages spanning the whole of Britain from 1991 to 2013. Respondents were asked once, when they joined the survey (or, in the case of the children in the households of panel members, when they were rising 16), to 'look at this card and tell me what type of school (you are attending/you attended last)'. We coded a binary 0/1 indicator variable to distinguish state-educated and privately-educated respondents according to whether they had last attended a private (that is, fee-paying) school; 6% stated that they had attended private school. This indicator is less than perfect, in that it does not capture the number of years that the panel members may have spent at private school; nevertheless, the last school attended should still be relevant for capturing school-type associations with values, networks, post-school activities and partner-selection.

For the analyses of husband's earnings, we also show findings derived from the age 42 wave of the British Cohort Study (BCS), a survey of people born in Britain in a single week in 1970 (Elliott & Shepherd, 2006). On one hand these additional findings provide a robustness check on the broad conclusions from the BHPS-UKHLS analyses, albeit using weekly, rather than hourly, pay. On the other hand the BCS contains high-quality, rich social background data derived from the childhood waves (at birth and at ages 5, 10 and 16), which enable us to control for an especially rich set of potentially confounding variables. We include, first, indicators of socio-economic background during childhood: parent's social class at cohort member's birth (highest of father or mother), parents' highest education level when the child was age 5, housing tenure type (at age 5), persons per room ratio (at age 5), number of days read to in a reference week at age 5 (a measure of cultural capital), mother's age at birth, receipt of free school meals at school at age 10 (a commonly used indicator of social deprivation) and banded family income at age 10. Second, we control for cohort members' early cognitive developments using indicators collected at ages 5 and 10 (Parsons, 2014). Third, we control for three personal characteristics typically associated with later development: birth order, weight at birth, and whether ever breast-fed. Finally, we include two standard demographic controls: ethnicity, and current region of residence.[5] The BCS school-type indicator we use is private school attendance at age 16. For this we used information from the 1986 Head Teacher's Questionnaire, but where this variable was missing we used the 1986 Schools Census; where information from 1986 was unavailable, we deployed an indicator derived from a retrospective question in the age 42 survey.[6] We excluded those from Special Needs schools (whether private or state). In BHPS-UKLHS the participation indicator is, for most respondents, a retrospective measure.

There are, however, issues of attrition and item non-response that needed addressing.[7] The age 16 survey wave remains broadly representative for important demographic characteristics (gender, parents' ages of leaving full-time education, biological parenthood, ethnicity and country) (Mostafa & Wiggins, 2015). Nevertheless, to avoid a substantive loss of information and representativeness that would arise by looking only at cases with full information on all waves, following (Schafer, 1997; Little & Rubin, 2002; Carpenter & Kenward, 2013) we applied multiple imputation to handle any item missingness for those cases with complete observations on husband's earnings and school type.

Further variable details and descriptive statistics from both data sets are given in the appendix.

School-type homogamy

We first investigate school type homogamy, beginning with a simple description of marriage frequencies and partners' school-type, using BHPS-UKHLS – see table 1. Among those that were married, there is strong indicative evidence for school type homogamy: while 13% of privately educated women were married to privately educated husbands, this was the case for only 3% of state school educated women.

One way to summarise the presence of homogamy is the local log odds ratio that compares the frequency of people in homogamous couples to that of people in non-homogamous couples (for each pair of school types) (Siow, 2015). Let μ_{τ_w,τ_h} be the absolute frequency of marriage between women of school type $\tau_w \in [0,1]$ with men of school type τ_h. If there is positive homogamy, the index m = $\ln\left([\mu_{1,1} \cdot \mu_{0,0}][\mu_{0,1} \cdot \mu_{1,0}]^{-1}\right)$ will be positive. This metric has been interpreted as the 'force of attraction' (Bičáková & Jurajda, 2016) and is related to the 'surplus' from marriage in

Table 1: Marital status and partner's school type by women's school type.
Pooled BHPS-UKHLS 1991-2013; row percentages. N= 72,037.

Women's School Type	Not married	Married Partner's School Type Private	 State
Private	33.9	13.3	52.9
State	37.9	3.1	59.0
Total	37.7	3.7	58.6

economics studies of partner choice (Choo & Siow, 2006; Menzel, 2015). With our data we find that m = 1.58 for school-type homogamy, and significantly above zero (p=0.000). This index compares with a value of m = 2.32 for homogamy by education-level (graduate/non-graduate).

Since private schooling is socially exclusive, given its high price for most pupils, school-type homogamy is likely to reflect, in part, the social closure brought about by social background. To what extent, then, is the school-type of the husband associated with a woman's private school attendance *per se*, after controlling for her social class background, as implied by the hypothesis of equation (2)? To examine this question, we restrict our attention to married women and estimate four probit models, the results of which are summarised in table 2.[8] In each the private school dummy is interacted with age groups, to allow for different effects at varying life course stages, and the table presents the average marginal effects of private school attendance on the probability of being married to a privately educated husband.

In the first column, only age group and ethnicity are controlled for, and the estimated coefficient approximately restates what was revealed in the descriptive table above, showing a large tendency for school-type homogamy. There is a 14 percentage point higher chance of being married to a private-school-educated husband for a woman who is privately educated than for a woman who is state educated.

In column 2, we also include as controls the women's social background. Compared with column (1) the estimated degree of school-type homogamy is substantially reduced by about a half. As expected, part of the preference for private school educated spouses can be attributed to the social background that usually determines private school participation. Yet even after accounting for social background, attending private school still raises the probability of being married to a privately educated husband by 7 percentage points. This is our best estimate describing school-type positive homogamy among observably similar women. However, we caution that, if interpreted as a causal effect, it could be biased if there are elements in the individuals' background not captured by parental social class and parental education, which affect both choice of school type and the likelihood of partnering with a privately-educated husband.

The hypothesis of equation (1) is that school-type homogamy potentially derives from two elements, one being a by-product of education-level homogamy, the other being an additional direct element. The education-level element here is binary: whether or not the women obtained higher education. To what extent, then, is the homogamy just a reflection of achieving higher education? Column (3) adds to the model a dummy for women's attainment of higher education. As can be seen, while higher education in itself increases the chances of being married to a privately educated husband, private-school-educated women were still 6 percentage points more likely to be married to a man of the same school type. Thus the school-type homogamy cannot be explained by the woman's educational level and social background. We take this as indicative of a positive average value for β, reflecting the direct element of the tendency for school-type homogamy.

But is this direct element less strong, as expected, among those who do go on to higher education, than among those who leave education at the secondary stage? Column (4) introduces an interaction term between higher education and private schooling. Among those who do not proceed to higher education, the chances of being married to a privately educated husband are 11 percentage points higher for privately educated women, as compared with state-educated women. Among those who do proceed to higher education, however, the chances are not significantly associated with private school status. Thus, while educational homogamy is a route through which school-type homogamy takes place (private schools raise entry to higher education, which itself raises the probability of school-type homogamy), nevertheless within that higher level of education school-type homogamy is attenuated, as expected, reflecting the wider social and cultural environment of university life and beyond. In this sense, the experience of higher education involves a reduction of social boundaries.

Females' private schooling and their husband's earnings

Given that there is education-level homogamy, that private schools in Britain are of high quality as measured by their pupils' educational achievements, and that in addition there is school-type homogamy in Britain, do privately-educated married women benefit from a premium on their husbands' earnings over those of state-educated women, as hypothesised in equation (4)?

From this point on, we are able to address our key questions using both sets of data. Using BHPS-UKHLS, table 3a shows the descriptive pattern of husbands' real gross monthly earnings, according to the school-types of each partner. As shown in the final column, women's private school attendance is associated with higher-earning husbands. The spouse of the average state-educated married women earned around £2,794 per month compared to £3,686 among women who attended private school – a raw premium of 32%. It is also of note that, among married women who attended private school, those in homogamous unions had on average husbands with the greatest gross monthly labour income (£3,834). Using BCS, table 3b shows that, at age 42, the raw weekly husband's earnings gap by school type was substantially higher at 62%.

Table 2: School-Type Homogamy: Probably of marriage to a privately educated spouse
Pooled BHPS-UKHLS 1991-2015. Average marginal effects

	(1)	(2)	(3)	(4)
Private School	0.140^{***}	0.0737^{**}	0.0618^{**}	
	(0.031)	(0.024)	(0.023)	
Higher Education			0.0502^{**}	0.0579^{**}
			(0.017)	(0.019)
Private School (within graduates)				-0.00613
				(0.044)
Private School (within non-graduates)				0.107^{**}
				(0.036)
Demographics (R)	X	X	X	X
Social Background (R)		X	X	X
N	42,957	42,957	42,957	42,957

Notes: Pooled probit estimator using the cross-sectional survey weights with robust standard errors in the unbalanced panel of married women aged 24–59 years. Dependent variable is 1 if partner has attended a private secondary school and zero otherwise. All models include a set of period dummies and a London dummy to account for differences across time and regions. Demographics comprise age-groups and ethnicity; social background is captured by highest parental socio-economic group and level of educational attainment. Women's highest level of educational achievement is captured by an indicator for higher tertiary educational attainment. Private school effect averaged over age-groups. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$

Table 3a: Husbands' real gross monthly earnings among married women in GBP (CPI 2015=100)

Pooled BHPS-UKHLS 1991-2015, N=29,103

Women's school type	Married Partner's school type Private	 State	 Mean
Private	3,833.85	3,621.02	3,685.52
State	3,314.53	2,763.44	2,794.19
Mean	3,408.26	2,806.07	2,844.68

Table 3b: Husbands' weekly earnings among married women in GBP

BCS-70, 2012, N=2,036

Women's school type	Mean spousal earnings
Private	930.16
State	575.75
Mean	635.22

We address the questions formally in tables 4a and 4b. Our basic estimating model operationalises equation (3) by treating private school status as a 0/1 dummy categorical variable (τ_w):

$$Y_h = a_0 + a_1\tau_w + a_2\sum_l \phi^l Z_w^l + \varepsilon_h \text{, with } E(\varepsilon_h) = 0 \quad (4)$$

The Z_w^l are observables capturing demographic characteristics (age, ethnicity, region), social class background, prior cognitive skills (BCS only), and birth characteristics (BCS only), which could be correlated with husbands' labour market outcomes as well as with women's own school-type, as described above. The first model (in both 4a and 4b) gives the estimates for a_1 with just the demographic characteristics included. Then the second model shows our core estimates of equation (4), including the whole range of other controls, giving the associations of husbands' earnings with women's school-type for observably similar women. We also include a third model that adds in indicators of the husband's social class, school type and education achievement, in order to see whether the private school premium we are investigating can be crudely accounted for by those characteristics.

To investigate outcomes other than just the mean linear effect on hourly earnings, we also investigate whether there are associations with husbands' earnings being in the top decile of the distribution, as well as the opposite association with husbands' earnings being in the bottom half of the distribution (thereby helping to protect against downward social mobility). We also examine the association of women's private education with their husband's attainment of a high occupation status (a professional or managerial job).

Estimator choice depends on the scale of the dependent variable and follows standard practices. For binary outcomes we employ a probit estimator. In all models shown, the tables report average marginal effects (AME). Since private school attendance is a time-constant individual characteristic in BHPS-UKHLS, we rely on cross-sectional estimators. To account for the repeated observations within individuals over time in the panel, potential heteroscedasticity, and changes in composition due to sample attrition, we use the supplied cross-section survey weights and a robust variance-covariance matrix in the estimations. With the BCS data, given that multiple imputation has been used for some variables, as recommended by Carpenter and Kenward (2013) we present estimates derived from 20 alternative imputation outcomes, using the Stata multiple estimation routines, with robust standard errors.

In this set-up there are inevitable limitations and caveats that would have to be borne in mind if the estimates were to be interpreted as implying causal effects of private schooling and if they were to be used to compute women's choice-relevant investment returns from private schooling via marriage. The main complication arises from the two-sided nature of the matching processes. In our data, we are able to observe neither all the feasible matching alternatives nor all the relevant factors that might inform the marriage decision. Thus unobservables on both sides may confound the estimates if they are related to the participation in private secondary education. We return to these caveats in the concluding discussion below.

Column (1) shows a substantial association with husbands' outcomes after controlling just for demographic characteristics. Married women who had attended private schools were matched with men who were considerably more successful in the labour market than the husbands of state-school educated women. In the case of the BHPS-UKHLS data, the husbands of these women received on average 26.0% (=exp(0.231)-1) higher hourly earnings. In the case of the BCS, the privately educated age-42 cohort members' husbands received 37.9% (=exp(0.327)-1) higher weekly earnings. With both data sets, the husbands of the privately educated women were significantly more likely to be in the top (>=90%) earnings decile, more likely to work in high status, managerial or professional occupations, and less likely to earn incomes below the median.

Some of these effects may be attributable in part to respondents' social background. Private-school-educated women are more likely to come from well-off families (Dearden et al., 2011), increasing the opportunity to access marriage networks of potential high earners. Hence we condition on respondents' social background, to give the estimates from equation (5). While the controls reduce the estimates substantially – see the average marginal effects in column (2) compared with those in column (1) – the remaining effects are still quantitatively significant. The conditional husbands' pay premium is 15% for hourly earnings in the BHPS-UKHLS, and 20% in BCS.

Similarly, the husbands of privately educated women remained more likely to be situated in the top earnings decile and to work in high-status occupations. Spouses of privately educated women were 10 percentage points more likely to be in the top pay decile as the husbands of state-educated women (8 points for BCS at age 42), and again 10 percentage points more likely to work in a high-status occupation (BHPS-UKHLS) though this effect is insignificant in the BCS data. In the BCS data (but not with the BHPS-UKHLS) there is also a weakly statistically significant protective effect of private school attendance against below-median earnings husbands, lowering that probability by 8 percentage points.

Column (3) in each table adds controls for husband's characteristics and respondent's post-secondary educational attainment (graduate/non-graduate). These reduce the average marginal effects considerably and in some cases the effect is not statistically significant, indicating that these characteristics mediate much of the association with husbands' outcomes. However, in two cases (top earnings for the BHPS data, log earnings for the BCS) the association remains weakly statistically significant. In these cases the estimates imply that private-educated women are matched with partners that earn more than their observed characteristics suggest on average. We conclude there are some unobserved husband characteristics associated both with their pay and with their wives' school type; in the case of the BCS this could include the husband's school-type which is unobserved.

So far our sample has excluded cohabiting but unmarried couples from the analyses. We do not have information about intra-household resource sharing in our data sets, but it could be expected that resources are shared at least partially among a substantial proportion of cohabiting, unmarried couples. For these, similar arguments about school-type homogamy and education-level homogamy apply. We therefore re-ran our analyses including all cohabiting couples, whether married or not. We found the same pattern of results with only small changes in the estimated coefficients. As a further test of robustness, we took account of differences within the state sector of education, distinguishing between grammar schools, which are academically selective schools available in a minority of regions of Britain, and all other state schools that are not permitted to select on academic merit. Including a separate dummy variable for grammar school attendance made only small differences to the estimated associations of private school attendance with subsequent marriage to a privately educated man, and with the husbands' labour market outcomes. Since a few women at the age of 24 may still have been in full-time education such as in postgraduate research programmes, we also run a robustness check with a BHPS/UKHLS sample limited to the age bracket 30–59 years. This has no effect on the estimated patterns.[9]

Table 4a: Husbands' labour market outcomes, pooled BHPS/UKHLS 1991–2015

	(1)	(2)	(3)
	(I) Log real hourly pay (log points)		
Private School	0.228***	0.142**	0.0667
	(0.045)	(0.045)	(0.042)
N	28,660	28,660	28,660
	(II) Real hourly earnings in 90% decile of distribution (AME)		
Private School	0.161***	0.0999**	0.0548**
	(0.034)	(0.031)	(0.024)
N	28,961	28,961	28,961
	(III) Real hourly earnings in the bottom half of distribution (AME)		
Private School	-0.0771**	-0.0168	0.0358
	(0.029)	(0.033)	(0.033)
N	28,961	28,961	28,961
	(IV) High Status Occupation (AME)		
Private School	0.184***	0.0930**	0.0287
	(0.032)	(0.034)	(0.030)
N	35,209	35,209	35,209
Demographics (R)	X	X	X
Social Background (R)		X	X
Higher Education (R)			X
Demographic, Social Background, Higher Education (H)			X

Notes: Pooled estimations using cross-sectional survey weights and a heteroscedasticity and autocorrelation robust variance-covariance matrix. Unbalanced panel of married women aged 24–59 years. Dependent variables: (I) husbands' usual log real gross hourly earnings; (II) dummy if husbands' real gross hourly earnings were in 90% decile; (III) dummy if husbands' real hourly earnings were below the grand median of the hourly earnings distribution; (IV) dummy if husband holds a high status occupation (SOC1990 or SOC2000, major groups 1&2). Including a set of period dummies for survey period and region of residence. For social background and education, see notes to table 2. Model 3 adds a range of husbands' observed characteristics (5-year age dummies, ethnicity, parental socio-economic group, parental level of education, private school attendance, and tertiary attainment). Standard errors in parentheses. Private school effect averaged over age-groups. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$

Table 4b: Husbands' labour market outcomes, BCS-70, age 42

	(1)	(2)	(3)
		(I) Log weekly earnings	
Private School	0.327***	0.184**	0.125*
	(0.068)	(0.073)	(0.069)
N	2,028	2,028	2,028
		(II) Weekly earnings in 90% Decile. AME	
Private School	0.172***	0.0728**	0.0423
	(0.039)	(0.036)	(0.033)
N	2,028	2,028	2,028
		(III) Weekly earnings below the median. AME	
Private School	-0.137***	-0.0790*	-0.0523
	(0.035)	(0.043)	(0.045)
N	2,028	2,028	2,028
		(IV) High Status Occupation. AME	
Private School	0.102**	0.0200	-0.0206
	(0.032)	(0.027)	(0.022)
N	2,509	2,509	2,509
Demographics	X	X	X
Childhood cognitive attainment, social background (R)		X	X
Higher education (R)			X
Partner's age, level of education (H)			X

Notes: Mean differences in spousal labour market outcomes by female private school attendance. (I) Effect on log weekly earnings. (II)/ (III) differences in the likelihood of spousal income in the 90th percentile/ below the median. Cut-points derived from BCS. (IV) female private school effects on likelihood to match with spouse in higher managerial and professional occupations (based on NS-SEC groupings). Column (1) reports differences adjusted by demographic characteristics (ethnicity, region of residence). Column (2) adds controls for socio-economic background during childhood (social class at birth, parental educational attainment, housing tenure, persons per room ratio, number of days read to in reference week, free school meal receipt, family income band), information on early cognitive developments at age 5 and 10, and controls for mother's age at birth, birth order, weight at birth and whether ever breast-fed). To address missingness, estimations based on multiple imputations. Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$

Conclusion

In this paper we have presented evidence that there is a substantive degree of school-type homogamy in Britain, whereby a privately educated woman, compared with a state-educated woman with otherwise similar social background, is 7 percentage points more likely to marry a privately educated man. Our explanation is that homogamy by school-type partly reflects education-level homogamy, owing to the high quality of private schooling in Britain, but that it also follows directly from shared values and networks inculcated in private schools. We found that homogamy by school-type is especially strong among non-graduates, and absent among graduates, testimony to the widening social environment associated with university life and beyond.

We have also found that the husbands of privately educated women have an estimated 15% greater hourly pay using BHPS-UKHLS, and 20% greater weekly pay using age 42 BCS data, than the husbands of state-educated women. The husbands of the privately-educated were much more likely to work in high status occupations, more likely to be earning in the 90th percentile, and less likely to be earning below the median, than the husbands of the state educated.

A number of limitations to the analysis should be noted. We have reasonable controls for social background and educational credentials, including especially good ones in the BCS data. However, some other factors that are unobserved, such as parental attitudes, could be positively related both to private school choice and the matching outcomes investigated. If so, any causal effect of private school attendance would be lower than our estimates of the conditional association suggest. Measurement error of school-type, on the other hand, might suggest an underestimate of its effects. Even though school-type when leaving school is accurately recorded in retrospect by the vast majority of cases, we do not have information on the length of time in private school, or on differences in private school quality. Finally, another issue is that surveys such as BHPS/UKHLS and BCS do not adequately track those going abroad. There are relatively few of these, but especially in recent years, when some elite schools have become the training ground for some high earners in a globally integrated world, trimming from the sample those who leave the country might alter the estimated association with private school attendance.[10]

Nevertheless, the finding of strong relationships between females' private schooling and their husbands' earnings raises anew the issue of the negative association between private schooling and social mobility. This link is already implied by the labour market returns enjoyed by both men and women, and the exclusivity in access implied by the relatively high fees charged in most schools. The findings in this paper reinforce this link: school-type homogamy, and associated educational homogamy, combine to help retain economic and social advantages within the family. In asking whether private school girls marry rich, a natural extension of this line of research would be to examine the accumulated wealth of partners, and the consequent links with household income. Future research can also be extended to the marriage returns for males. One would expect to find similar effects in other countries or regions where private education is both sharply separated from the state in governance and funding, and associated with high earnings. Other sharp institutional dividing lines in education with potential implications for adult social and economic outcomes, such as between religious and secular schools, may also provide fruitful areas for research on homogamy linked to school type, though in such cases the school typology might reflect a different institutional dichotomy, more salient for different countries or contexts.

Our findings also may have implications for school choice by parents, seemingly raising the returns to private education. One potential further extension to the analysis of school-type homogamy would be to allow for the prospective marriage market return to affect the choice of private education and, further, the level of investment. Chiappori, Iyiguin and Weiss (2009) developed a model of equilibrium education and marriage choice in the presence of educational homogamy underpinned by complementary preferences. In empirical work Ge (2011) and Lafortune (2013) each report effects of changing marriage prospects on college educational investments in the US. Yet the possibility that choice of school-type in Britain is also affected by marriage prospects is a further reason for caution in interpreting our findings as unbiased estimates of causal effects.

Acknowledgements

This work was funded by the ESRC-funded LLAKES Centre for Research on Learning and Life Chances with an additional contribution to LLAKES from the ESRC/UKCES Strategic Partnership (ES/J019135/1). The research was also supported through a separate ESRC grant "Schooling and Unequal Outcomes in Youth and Adulthood" (ES/K007238/1).

References

Antonovics, K. & Town, R. (2004). "Are All the Good Men Married? Uncovering the Sources of the Marital Wage Premium." *The American Economic Review*, *94*(2), Papers and Proceedings: 317–321.

Arum, R., Roksa, J. & Budig, M. J. (2008). "The romance of college attendance: Higher education stratification and mate selection." *Research in Social Stratification and Mobility*, *26*, 107–121. https://doi.org/10.1016/j.rssm.2008.02.003

Ashley, L., Duberley, J., Sommerlad, H., & Scholarios, D. (2015). *A qualitative evaluation of non-educational barriers to the elite professions*. London: Social Mobility and Child Poverty Commission.

Bičáková, A., & Jurajda, Š. (2016). "Field-of-Study Homogamy." *IZA Discussion Paper Series*, (9844), 1–34.

Blackwell, D. L. (1998). "Marital homogamy in the United States: The influence of individual and paternal education." *Social Science Research*, *27*(2): 159–188. https://doi.org/10.1006/ssre.1998.0618

Blandford, L. (1977). "The making of a lady." In G. Macdonald-Fraser (ed.), *The World of the Public School*. London: Weidenfeld & Nicolson.

Blossfeld, H.-P. (2009). "Educational Assortative Marriage in Comparative Perspective." *Annual Review of Sociology*, *35*(5), 513–530. https://doi.org/10.1146/annurev-soc-070308-115913

Blossfeld, H.-P. & Buchholz, S. (2009). "Increasing resource inequality among families in modern societies: The mechanisms of growing educational homogamy, changes in the division of work in the family and the decline of the male breadwinner model." *Journal of Comparative Family Studies*, *40*(4), 603–616.

Blossfeld, H.-P. & Timm, A. (2003). *Who marries whom?: educational systems as marriage markets in modern societies*. Springer Science & Business Media. https://doi.org/10.1007/978-94-007-1065-8

Boliver, V. (2013). "How fair is access to more prestigious UK universities?" *The British Journal of Sociology*, *64* (2), 344 – 364. https://doi.org/10.1111/1468-4446.12021

Breen, R. & Andersen, S. H. (2012). "Educational Assortative Mating and Income Inequality in Denmark." *Demography*, *49*(3), 867–887. https://doi.org/10.1007/s13524-012-0111-2

Bukodi, E. & Goldthorpe, J. H. (2011). "Class Origins, Education and Occupational Attainment in Britain: Secular Trends or Cohort-Specific Effects?" *European Societies*, *13*(3), 347–75. https://doi.org/10.1080/14616696.2011.568259

Carpenter, J. & Kenward, M. G. (2013). Chapter 2 in *Multiple Imputation and its application.* Chichester: Wiley & Sons Ltd Statistics in Practice. https://doi.org/10.1002/9781119942283

Chiappori, P.-A., Iyiguin, M., & Weiss, Y. (2009). "Investment in Schooling and the Marriage Market." *American Economic Review*, *99* (5), 1689–1713. https://doi.org/10.1257/aer.99.5.1689

Choo, E., & Siow, A. (2006). Who Marries Whom and Why. *Journal of Political Economy*, *114*(1), 175–201. https://doi.org/10.1086/498585

Cookson, Jr., P. W., & Persell, C. H. (1985). Comparative Education Review, Vol. 29, No. 3 (Aug, 1985), 283–298. https://doi.org/10.1086/446523

Crawford, C., & van der Erve, L. (2015). Does Higher Education Level the Playing Field? Socio-Economic Differences in Graduate Earnings. *Education Sciences*, *5*(4), 380–412. https://doi.org/10.3390/educsci5040380

Crawford, C., & Vignoles, A. (2014). "Heterogeneity in graduate earnings by socio-economic background." *IFS Working Paper* (W14/30): 1–20.

Crawford, C., Goodman, A., & Joyce, R. (2010), *Explaining the Socio-Economic Gradient in Child Outcomes: The Intergenerational Transmission of Cognitive Skills*, London: Institute for Fiscal Studies.

Dearden, L., Ferri, J., & Meghir, C. (2002). "The effect of school quality on educational attainment and wages." *Review of Economics and Statistics*, *84*(1), 1–20. https://doi.org/10.1162/003465302317331883

Dearden, L., Ryan, C., & Sibieta, L. (2011). "What Determines Private School Choice? A Comparison between the United Kingdom and Australia." *Australian Economic Review*, 44(3), 308–320. https://doi.org/10.1111/j.1467-8462.2011.00650.x

Dolton, P. & Vignoles, A. (2000). The incidence and effects of overeducation in the UK graduate labour market. *Economics of education review*, *19*(2), 179–198. https://doi.org/10.1016/S0272-7757(97)00036-8

Elder Jr, G. H. (1969). "Appearance and education in marriage mobility." *American Sociological Review*, *34*(4), 519–533. https://doi.org/10.2307/2091961

Elliott, J., & Shepherd, P. (2006). "Cohort profile: 1970 British birth cohort (BCS70)", *International Journal of Epidemiology*, *35*, 836–843. https://doi.org/10.1093/ije/dyl174

Ermisch, J., & Francesconi, M. (2000). "Cohabitation in Great Britain: not for long, but here to stay." *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, *163*(2), 153–171. https://doi.org/10.1111/1467-985X.00163

Ermisch, J., Francesconi, M., & Siedler, T. (2006). "Intergenerational Mobility and Marital Sorting." *The Economic Journal*, *116*(513), 659–679. https://doi.org/10.1111/j.1468-0297.2006.01105.x

Evans, G. & Tilley, J. (2011). "Private schools and public divisions: the influence of fee-paying education on social attitudes". In A. Park, E. Clery, J. Curtice, M. Philips and D. Utting (eds), *British Social Attitudes 28.* London: Sage, 37–52.

Gathorne-Hardy, J. (1977). *The Public School Phenomenon, 597–1977.* London: Hodder and Stoughton.

Ge, S. (2011). Women's College Decisions: How Much Does Marriage Matter? *Journal of Labor Economics*, *29*(4), 773–818. https://doi.org/10.1086/660774

Gihleb, R., & Lang, K. 2016. *Educational Homogamy and Assortative Mating Have Not Increased*. NBER, Working Paper No. 22927.

Goldin, C., & Katz, L. F. (2008). *The Race Between Education and Technology*. Cambridge, Mass.: Harvard University Press.

Goodman, A., Gregg, P., & Washbrook, E. (2011), 'Children's educational attainment and the aspirations, attitudes and behaviours of parents and children through childhood in the UK', *Longitudinal and Life Course Studies*, *2*(1), 1–18.

Green, F., Machin, S., Murphy, R., & Zhu, Y. (2012). "The Changing Economic Advantage from Private Schools." *Economica*, *79*, 658–679.

Green, F., Henseke, G. & Vignoles, A. (2017) "Private schooling and labour market outcomes". *British Educational Research Journal*. 43(1), 7–28. https://doi.org/10.1002/berj.3256

Green, F., Parsons, S., Sullivan, A., & Wiggins, R. (2018). **"**Dreaming Big? Self-Evaluations, Aspirations, Valued Social Networks, and the Private School Earnings Premium in the UK". *Cambridge Journal of Economics*.42 (3), 757-778. https://doi.org/10.1093/cje/bex023

Green, F., Anders, J., Henderson, M., & Henseke, G. (2017). *Who Chooses Private Schooling in Britain and Why?* Centre for Research on Learning and Life Chances (LLAKES), Research Paper 62. London.

Greenwood, J., Guner, N., Kocharkov, G. & Santos, C. (2014). 'Marry Your Like: Assortative Mating and Income Inequality.' *American Economic Review*, Papers and Proceedings (May), 1–10. https://doi.org/10.3386/w19829

Gregg, P. & Macmillan, L. (2010), 'Family income, education and cognitive ability in the next generation: exploring income gradients in education and test scores for current cohorts of youth'. *Longitudinal and Life Course Studies*, *1*(3), 259–80.

Halpin, B. & Chan, T. W. (2003). "Educational homogamy in Ireland and Britain: trends and patterns." *The British Journal of Sociology*, *54*(4), 473–495. https://doi.org/10.1080/0007131032000143546

Hiekel, N., Liefbroer, A. C., & Poortman, A. R. (2014). Income pooling strategies among cohabiting and married couples: A comparative perspective. *Demographic Research*, *30*, 1527. https://doi.org/10.4054/DemRes.2014.30.55

Jerrim, J., Parker, P. D., Chmielewski, A. K. & Anders, J. (2015 online). "Private Schooling, Educational Transitions, and Early Labour Market Outcomes: Evidence from Three Anglophone Countries." *European Sociological Review*.

Kalmijn, M. (1998). "Intermarriage and homogamy: Causes, patterns, trends." *Annual Review of Sociology*, *24*(1), 395–421. https://doi.org/10.1146/annurev.soc.24.1.395

Kaufmann, K. M., Messner, M., & Solis, A. (2015). Elite Higher Education, the Marriage Market and the Intergenerational Transmission of Human Capital. Mimeo, European University Institute, Department of Economics Seminar, https://www.eui.eu/Documents/DepartmentsCentres/Economics/Seminarsevents/Kaufmann.pdf.

Kirby, P. (2016). *Leading People 2016*. London: The Sutton Trust.

Lafortune, J. (2013). "Making Yourself Attractive: Pre-Marital Investments and the Returns to Education in the Marriage Market." *American Economic Journal: Applied Economics*, *5*(2), 151–178. https://doi.org/10.1257/app.5.2.151

Little, R. J. & Rubin, D. B. (2002). *Statistical analysis with missing data*. Chichester: John Wiley & Sons. https://doi.org/10.1002/9781119013563

Macmillan, L., Tyler, C., & Vignoles, A. (2015). "Who Gets the Top Jobs? The Role of Family Background and Networks in Recent Graduates' Access to High-status Professions." *Journal of Social Policy*, *44*(3), 487–515. https://doi.org/10.1017/S0047279414000634

Maxwell, C. & Aggleton, P. (2014a). "Agentic practice and privileging orientations among privately educated young women". *The Sociological Review*, *62*(4), 800–820. https://doi.org/10.1111/1467-954X.12164

Maxwell, C. & Aggleton, P. (2014b) "The reproduction of privilege: Young women, the family and private education". *International Studies in Sociology of Education*, *24*(2), 189–209. https://doi.org/10.1080/09620214.2014.919091

McKnight, A. (2015). *Downward mobility, opportunity hoarding and the 'glass floor'*. London: Social Mobility and Child Poverty Commission.

Menzel, K. (2015). Large Matching Markets as Two-Sided Demand Systems. *Econometrica*, *83*(3), 897–941. https://doi.org/10.3982/ECTA12299

Mostafa, T. & Wiggins, D. (2015). "The impact of attrition and non-response in birth cohort studies: a need to incorporate missingness strategies." *Longitudinal and Life Course Studies*, *6*(2), 131–146. https://doi.org/10.14301/llcs.v6i2.312

Naylor, R., Smith, J., & McKnight, A. (2002). "Why is there a graduate earnings premium for students from independent schools." *Bulletin of Economic Research*, *54*(4), 315–339. https://doi.org/10.1111/1467-8586.00155

Parsons, S. (2014). *Childhood cognition in the 1970 British Cohort Study* (CLS Working Papers).

Peel, M. (2015). *The New Meritocracy: A History of UK Independent Schools 1979–2015*. London: Elliott and Thompson.

Plewis, I., Calderwood, L., Hawkes, D., & Nathan, G. (2004). *Changes in the NCDS and BCS70 Populations And Samples Over Time*. Centre for Longitudinal Studies Technical Report. London: Institute of Education.

Power, S., Whitty, G., & Wisby, E. (2006). *The Education and Career Trajectories of Assisted Place Holders.* London: The Sutton Trust.

Power, S., Sims, S., & Whitty, G. (2013). *Lasting Benefits*. London: The Sutton Trust.

Reynolds, C. A., Baker, L. A., & Pedersen, N. L., (2000). "Multivariate models of mixed assortment: phenotypic assortment and social homogamy for education and fluid ability." *Behavior Genetics*, *30*(6), 455–476. https://doi.org/10.1023/A:1010250818089

Schafer, J. L. (1997). *Analysis of Incomplete Multivariate Data*. London: Chapman and Hall. https://doi.org/10.1201/9781439821862

Schwartz, C. R. (2013). 'Trends and Variation in Assortative Mating: Causes and Consequences.' *Annual Review of Sociology*, *39*, 451–70. https://doi.org/10.1146/annurev-soc-071312-145544

Schwartz, C. R., & Mare, R. D. (2005). "Trends in educational assortative marriage from 1940 to 2003." *Demography*, *42*(4), 621–646. https://doi.org/10.1353/dem.2005.0036

Silles, M. A. (2007). "The returns to education for the United Kingdom." *Journal of Applied Economics*, *10*(2), 391–413.

Siow, A. (2015). Testing Becker's theory of positive assortative matching. *Journal of Labor Economics*, *33*(2), 409–441. https://doi.org/10.1086/678496

Sullivan, A., Parsons, S., Wiggins, R., Heath, A., & Green, F. (2014). "Social origins, school type and higher education destinations." *Oxford Review of Education*, *40*(6), 739–763. https://doi.org/10.1080/03054985.2014.979015

Turner, D. (2015). *The Old Boys. The Decline and Rise of the Public School*. London: Yale University Press. https://doi.org/10.12987/yale/9780300189926.001.0001

Uunk, W. J., Ganzeboom, H. B., & Róbert, P. (1996). "Bivariate and multivariate scaled association models. An application to homogamy of social origin and education in Hungary between 1930 and 1979." *Quality & Quantity*, *30*(3), 323–343.

Walford, G. (1983). "Girls in boys' public schools: A prelude to further research." *British Journal of Sociology of Education*, *4*(1), 39–54. https://doi.org/10.1080/0142569830040103

Winkler, A. E. (1997). Economic decision-making by cohabitors: Findings regarding income pooling. *Applied Economics*, *29*(8), 1079–1090. https://doi.org/10.1080/000368497326471

Endnotes

1. The following citation from Mail Online, 12/2/2014, illustrates this point under the headline: "I spend a fortune to send my girl to private school – so she'll marry rich and never work". The citation proceeds: "my husband ... and I place great importance on her learning. Indeed, we hope she will go on to study at Oxford University, (... which is...) the ideal place for her to find a husband with the right background and career prospects to make enough money so Matilda can become a stay at-home mother." See also "Private schools continue to divide 'them and us' Britain", The Week, 2/11/2011; "Kate Middleton's family and the Upper Middleton", Evening Standard, 7/5/2015.
2. A new scheme to enable boarding education for disadvantaged students on the edge of the care system has recently launched on a small scale; see https://www.gov.uk/government/news/more-help-for-vulnerable-children-to-attend-top-boarding-schools
3. The source of the superior quality of private schooling in terms of educational outcomes is not fully understood, but is held to lie in some combination of their superior resources, autonomous governance and beneficial peer effects from privileged and supportive social background of the pupils. The resource gap is especially large in Britain, with fees alone being of the order of three times the unit expenditure on state school pupils.
4. https://www.iser.essex.ac.uk/bhps/; https://www.understandingsociety.ac.uk/ Since the specific question on private school attendance is not included in UKHLS, we are unable to incorporate its larger sample.
5. These key indicators could be supplemented by other controls available in the data at various waves, though typically there will be considerable multiple collinearity among the indicators.
6. Inevitably, recall data might be liable to some recall error. Nevertheless where multiple reports of participation were available, the retrospective data proved to be reliable in the large majority of cases: in fewer than 1% of cases did the age 42 recall data differ from the contemporary information sources (Green et al., 2018).
7. Plewis, Calderwood, Hawkes and Nathan (2004) provide an analysis of all samples up to age 30.
8. Generating a similar pattern of conclusions we also tried an alternative approach, embedding the decision in a multinomial model of marriage and husband's school types, comparing for women the option of remaining unmarried with heterogamous marriage or homogamous marriage. Menzel (2015) shows that for large marriage markets the conditional choice problem can be approximated by a logit model.
9. The findings for both these sensitivity analyses are available on request.
10. Overall, 2% of the BCS age 16 sample had ever emigrated by age 42, including 3% of those at private secondary school.

Appendix

This appendix presents details of variable descriptions and tables of descriptive statistics of all variables from both data sets used in our analyses.

Table A1 Variable descriptions

Variable	Description
	Demographics
Age	Categorical variable. Cutpoints differ between females and males to account for the persistent age difference in marriages • Respondents: 24–29, 30–34, 35–39, 40–44, 45–49, 50–54, 55–59. • Partner: <27, 27–31, 32–36, 37–41, 42–46, 47–51, 52–56, 57–61, 62+
Ethnicity	Binary indicator to distinguish between whites and non-white respondents (self-reported)
	Social Background
Paternal education (at age 14)	Categorical variable that distinguishes between - Neither parent had formal qualifications - At least one parent with some qualifications/further education quals - At least one parent with a university/higher degree - No valid data for either parent
Paternal socio-economic group class (at age 14)	Categorical variable that groups parents into 5 classes based on the highest reported socio-economic group: - High: large managers, large employers, self-employed and employed professionals - Intermediate: small managers and intermediate level supervisors, intermediate level non-manual workers - Small employers: small employers and own account workers (non-farm and farm) - Services: junior non-manual, personal services, armed forces - Manual: foreman manual, (semi-)skilled and unskilled manual and agricultural workers - Unemployed/Inactive: neither parent in paid work - Other/ missing: no information
	Educational Attainment
Educational attainment	Categorical variable to distinguish between higher tertiary attainment and below: Higher tertiary (=1) or lower (=0)

Table A2: Descriptive statistics, married women, BHPS/UKHLS

	mean	sd
Log monthly household income	8.223	0.603
Not in work	0.269	0.444
Private school	0.062	0.241
Tertiary attainment	0.138	0.345
24–29	0.081	0.273
30–34	0.131	0.337
35–39	0.157	0.364
40–44	0.166	0.372
45–49	0.160	0.367
50–54	0.156	0.363
55–59	0.148	0.355
Non-white	0.037	0.188
London	0.236	0.425
Parental SEG		
Neither parent had formal qualifications	0.323	0.468
some qualifications/ further education	0.426	0.494
university/ higher degree	0.073	0.259
Missing	0.179	0.383
large managers, large employers, employed professionals	0.165	0.371
small managers and intermediate level supervisors	0.125	0.331
small employers and own account workers	0.131	0.337
junior non-manual, personal services	0.175	0.380
foreman manual, (semi-)skilled and unskilled manual	0.319	0.466
neither parent in paid work	0.043	0.202
no information	0.042	0.201
Highest parental qualification		

no formal qualifications	0.323	0.468
some qualifications/ further education	0.426	0.494
university/ higher degree	0.073	0.259
Missing	0.179	0.383
N	44035	

Table A3: Descriptive statistics, husbands, BHPS/UKHLS

	mean	sd
Log hourly wages	2.641	0.608
Earnings in 9th decile	0.139	0.346
Earnings below median	0.351	0.477
SOC MG1/MG2	0.351	0.477
Private school	0.060	0.238
Higher education	0.179	0.383
27–31	0.015	0.122
32–36	0.084	0.277
37–41	0.150	0.358
42–46	0.178	0.382
47–51	0.177	0.382
52–56	0.161	0.368
57–61	0.132	0.338
62+	0.103	0.304
Non-white	0.035	0.183
Parental SEG		
large managers, large employers, employed professionals	0.162	0.368
small managers and intermediate level supervisors	0.125	0.331
small employers and own account workers	0.124	0.330
junior non-manual, personal services	0.171	0.377

foreman manual, (semi-)skilled and unskilled manual	0.330	0.470
neither parent in paid work	0.037	0.188
no information	0.051	0.221
Highest parental qualification		
no formal qualifications	0.332	0.471
some qualifications/ further education	0.428	0.495
university/ higher degree	0.064	0.245
Missing	0.175	0.380
N	29347	

Table A4 Descriptive statistics for the married women in the British Cohort Study at age 42

	mean	sd
Annual household income >£55.9k	0.237	0.425
Not in work	0.181	0.385
Log Husband's weekly pay	6.167	0.676
Husband's weekly pay in 9th decile	0.166	0.373
Husband's weekly pay below median	0.313	0.464
Husband's occupation MG1/ MG2	0.138	0.345
Private school	0.069	0.253
Higher education	0.333	0.622
At Birth		
V unskilled	0.043	0.203
IV partly-skilled	0.146	0.353
III manual	0.431	0.495
III non manual	0.152	0.359
II managerial and Technical	0.162	0.368
I professional	0.062	0.242
Other (not in work/ other)	0.003	0.056

Birthweight (in g)	0.055	0.229
livebaby	1.989	1.197
Mother's age at birth	26.089	5.248
Age 5		
Breast Feeding	0.407	0.491
Housing tenure (own)	0.650	0.477
Housing tenure (social rent)	0.255	0.436
Housing tenure (private rent)	0.050	0.218
Housing tenure (other)	0.045	0.207
Persons per room	1.012	0.250
English Picture Vocabulary Test [max 56)	35.600	10.044
Schonell Reading Test	1.986	4.385
Copying Designs Test	4.962	1.900
Number of Days Read to in Last Week	4.709	2.484
European UK	0.973	0.161
European Other	0.008	0.091
Other	0.018	0.134
Age 10		
banded family income (1)	0.051	0.220
banded family income (2)	0.264	0.441
banded family income (3)	0.350	0.477
banded family income (4)	0.185	0.388
banded family income (5)	0.080	0.272
banded family income (6)	0.070	0.255
Free school meal recipient	0.117	0.321
Friendly Maths Test	45.978	10.925
Edinburgh Reading Test	44.008	11.239

BAS Word Definitions	10.517	4.842
Age 42		
Number Of Children in HH	1.840	1.013
North	0.060	0.238
Yorks and Humberside	0.091	0.287
East Midlands	0.072	0.259
East Anglia	0.045	0.208
South East	0.289	0.453
South West	0.105	0.307
West Midlands	0.089	0.284
North West	0.107	0.309
Wales	0.058	0.234
Scotland	0.084	0.278
Partner		
Age	44.244	4.558
Age left ft-education (<18)	0.612	0.487
Age left ft-education (18/19)	0.152	0.359
Age left ft-education (20/21)	0.087	0.282
Age left ft-education (22/29)	0.135	0.342
Age left ft-education (30+)	0.013	0.115
N	2529	

STUDY PROFILE
MatCH (Mothers and their Children's Health) Profile: Offspring of the 1973–78 Cohort of the Australian Longitudinal Study on Women's Health

Gita D Mishra The University of Queensland, Australia
G.mishra@uq.edu.au
Katrina Moss The University of Queensland, Australia
Colleen Loos The University of Queensland, Australia
Annette J Dobson The University of Queensland, Australia
Peter S W Davies The University of Queensland, Australia
Deborah Loxton The University of Newcastle, Australia
Kylie D Hesketh Deakin University, Australia
Ilona Koupil Stockholm University and Karolinska Institutet, Sweden
Carol Bower Telethon Kids Institute, Australia
Peter Sly The University of Queensland, Australia
Leigh Tooth The University of Queensland, Australia

(Received December 2017 Revised March 2018) http://dx.doi.org/10.14301/llcs.v9i3.491

Abstract

MatCH (Mothers and their Children's Health) is a nationwide Australian study to investigate the links between the history of health, wellbeing and living conditions of mothers and the health and development of their children. MatCH builds on the Australian Longitudinal Study on Women's Health (ALSWH), which began in 1996 and has surveyed more than 58,000 women in four nationally representative age cohorts. MatCH focuses on the three youngest offspring of the cohort of ALSWH participants randomly sampled from all women in Australia born in 1973–78 (N=5780 children of N=3039 mothers). These women, who had completed up to seven postal or online surveys since 1996, were invited in 2016–17 to complete surveys about the health and development of their three youngest children aged under 13. The mothers reported on their children's health conditions and symptoms, diet, anthropometric measures, childcare, screen time, physical activity, temperament, behaviour, language development, motor development and health service utilisation, as well as household and environmental factors. These data are being linked with each child's records from official sources including the Australian Early Development Census (collected at age five to six), the National Assessment Program – Literacy and Numeracy (collected at age eight, 10, 12 and 14) and other external datasets. MatCH will combine 20 years of maternal data with all the information on her children, taking into account the family setting. MatCH offers an unprecedented opportunity to advance our understanding of the relationship between maternal health and wellbeing and child health and development.

Keywords

Maternal health; child health; health service use; intergenerational effects; social determinants; environmental factors

Introduction

Cohort studies are essential to understanding the evolution of health problems across the life course and for health policy planning. However, to fully understand human health and wellbeing, and to plan appropriate prevention policies, we need to understand the intergenerational influences that shape health behaviour and disease onset. The Mothers and their Children's Health study (MatCH; http://www.alswh.org.au/match-about) is designed to investigate the extent to which the history of maternal health and wellbeing, along with characteristics of the family environment, lead to disparities in child health, development and health service use, and how this varies between the three youngest offspring (for example, by sex, birth order, number of siblings).

MatCH builds on the long-running Australian Longitudinal Study on Women's Health (ALSWH) (Dobson et al., 2015; C. Lee et al., 2005; Loxton et al., 2017). Beginning in 1996, ALSWH has surveyed more than 58,000 women in four age cohorts (born in 1921–26, 1946–51, 1973–78 and 1989–95). MatCH combines the rich history of existing maternal data from the 1973–78 cohort (collected seven times between 1996 and 2015) with new data (collected in 2016–17) from the mother about her three youngest children aged under 13. These data are being linked to external data from the Australian Early Development Census (AEDC) (collected during the first year of school when children are aged 5–6), and the National Assessment Program – Literacy and Numeracy (NAPLAN) (collected at ages eight, 10, 12 and 14) and other sources.

The objectives of MatCH are to examine the associations between child health, development and health service use and: 1) maternal sexual and reproductive health; 2) maternal socioeconomic factors and health-related behaviours; 3) history of maternal and family characteristics; 4) family environment; 5) physical home environment (including environmental exposures); and 6) access to health services, including distance from major towns or cities.

Women's sexual health, age at first birth and use of assisted reproductive technology have implications for their own overall health as well as their children's health at birth and beyond (Mishra et al., 2013). Sexually transmitted infections affect almost one in five Australian women (Australia. Department of Health and Ageing, 2010) and can lead to pelvic inflammatory disease, ectopic pregnancy, infertility, pre-term delivery and low birthweight offspring (Aral, 2001). An increasing number of women are having children later in life, increasing the risk of pregnancy complications and poor birth outcomes. Findings from the ALSWH show that by the age of 36, one in five women report infertility (Herbert, Lucke, & Dobson, 2012). Use of assisted reproductive technology by sub-fertile and infertile women has substantial costs, emotionally, physically and financially, and is also associated with an increased risk of pregnancy complications and adverse birth outcomes such as prematurity and low birthweight offspring (Camarano et al., 2012; Filicori et al., 2005; Qin, Liu, Sheng, Wang, & Gao, 2016). MatCH will investigate the extent to which maternal history of poor sexual and reproductive health is associated with poor child outcomes, and whether this effect varies among the three youngest siblings.

Maternal socioeconomic position is a strong determinant of intergenerational effects, from birth outcomes to early child health and development and adult health and wellbeing (Morris et al., 2017). Women in the most disadvantaged areas of Australia have a 50% higher fertility rate, earlier first birth and a higher percentage of low birthweight offspring compared to women in areas with least disadvantage (Australia. Department of Health and Ageing, 2010) (with the continuum of disadvantage–advantage defined by geographical distance to health services provided by large towns or cities). Findings from the ALSWH show that the most disadvantaged young women are twice as likely to smoke, be obese and to have low levels of physical activity compared to the most advantaged women (Lawlor, Tooth, Lee, & Dobson, 2005). Children from families of low socioeconomic position are at increased risk of poor outcomes such as injury, chronic health conditions, psychiatric disturbance, poor cognitive development, poor academic engagement, and maladaptive social functioning (Bradley & Corwyn, 2002; Chen, Matthews, & Boyce, 2002; Propper, Rigg, & Burgess, 2007; Schoon, Sacker, & Bartley, 2003). Compared to children in the least disadvantaged areas, children in the most disadvantaged areas are less likely to be exclusively breastfed to four months of age (Australian Institute of Health and Welfare, 2012). They have more screen time, have a higher intake of energy-dense drinks and snacks, and lower consumption of fruit and vegetables (Cameron et al., 2012), and are almost twice as likely to be overweight or obese (Australian Institute of Health and Welfare, 2012). To identify the potential for early family-specific interventions, MatCH will investigate the extent to which maternal socioeconomic factors (education level, employment status and income) and maternal lifestyle factors (diet, physical activity, body weight, alcohol intake and illicit drug use) are associated with child birth outcomes, health, development and health service use, and whether these effects vary between the three youngest offspring. It will also investigate whether maternal and family characteristics have varying influences on the diet and physical activity of children in the family, and whether this in turn influences child health and development.

The family environment is the most influential social context in early childhood. Family structure, the defining characteristic of the family environment, has become less stable, with family dissolution and divorce increasingly commonplace (Australian Institute of Health and Welfare, 2012). Changes in family structure heighten the risk of poor child mental health and wellbeing (Sawyer et al., 2012; Vimpani, Patton, & Hayes, 2004), and children in one-parent families can be more vulnerable (Pearce, Lewis, & Law, 2013). For example, in 2014, 25% of one-parent families reported ever experiencing homelessness and 56% could not access healthcare due to cost (Australian Bureau of Statistics, 2015). Furthermore, ALSWH data revealed that sole mothers had significant mental health issues, explained in part by financial stress (Loxton, Mooney, & Young, 2006). Parent–child interactions play a key role in determining child health and wellbeing and can have long-term consequences on child development. Positive and responsive parenting reduces the risk of child behaviour problems and enhances child health and educational outcomes, placing children on a positive developmental trajectory through to adolescence (Maggi, Irwin, Siddiqi, & Hertzman, 2010). While positive and negative parenting practices are largely independent of socioeconomic position (Maggi et al., 2010), family socioeconomic position is closely linked with family stressors (Australian Institute of Health and Welfare, 2012) and also predicts brain development in early childhood (Luby et al., 2013). Positive parenting can build children's coping resources and buffer the impact of family stressors such as financial strain (Maggi et al., 2010). MatCH will investigate the extent to which family environment (household composition, social support, child care, maternal stress, parenting, ability to manage on income, job insecurity, and poor partner relationship including the presence of domestic violence) affect outcomes among children in the family.

Environmental exposures in the home can have long-term influences on health, as infants and young children spend the majority of their time indoors. There is increasing evidence that adverse environmental exposures may play a substantial role in the initiation and/or progression of diseases, including respiratory diseases (e.g. asthma), neuro-behavioural disorders, mild mental disability, obesity, Type 2 diabetes and childhood cancer (Grant, Carpenter, Sly, & Sly, 2013). Exposure to harmful substances in the environment during vulnerable developmental stages can have lifelong consequences. For example, exposure to persistent organic pollutants early in life can influence metabolism in a way that has been hypothesised to promote obesity (D. H. Lee et al., 2010). MatCH will investigate the extent to which the home environment (housing type, over-crowding, tobacco smoke, particulate matter and household chemicals) affects the health and development of children.

In Australia, distance from major towns and cities can affect people's health and their access to health services (Australian Institute of Health and Welfare, 2010; Schofield, Shrestha, & Callander, 2012), with one in three Australians aged over 15 years residing in outer regional and remote areas reporting difficulty accessing health care (Australian Bureau of Statistics, 2015). These issues can be further compounded by greater socioeconomic disadvantage in rural and remote areas (Australian Bureau of Statistics, 2015). Improving primary health care in these areas is a central tenet of Australian national health policy (Australian Department of Health and Ageing, 2011). Compared to those who live in major cities, children who live in remote areas are 1.4 times more likely to have been born with low birthweight, are twice as likely to be developmentally vulnerable in their first year of school, and have twice the rate of hospital admission due to injury (Australian Institute of Health and Welfare, 2012). MatCH will strengthen the evidence base for family health and health service use needs by investigating the extent to which living in rural and remote areas of Australia is associated with child outcomes, particularly health service use, and whether these effects vary among offspring.

Study design

Selection of mothers

In 1996, three random samples of women born between 1973–78, 1946–51 and 1921–26 were invited to participate in the ALSWH. The women were selected from the database of the Health Insurance Commission (now Medicare Australia). Medicare is Australia's universal health insurance scheme that covers all Australian citizens and permanent residents regardless of age or income. Those living in rural and remote areas were sampled at twice the rate of women living in urban areas. Details of the ALSWH design, recruitment methods and national representativeness of participants have been described elsewhere (Brown et al., 1998; Dobson et al., 2015; C. Lee et al., 2005). Potential participants in MatCH were all ALSWH participants born in 1973–78 who had not died, withdrawn from the study, asked not to be contacted about sub-studies, or had reported infertility. In 1996, when the women in the 1973–78 cohorts were recruited they were found to be largely representative of Australian women of the same age, with some over-representation of university-educated women (by about 5%) and under representation of immigrants from non-English speaking countries (by about 7%). Over successive surveys, these biases have increased and therefore need to be taken into account when the aim of any analyses are to generalise results to the Australian population (Dobson et al., 2015).

Data collected on the mothers

Women in this cohort were surveyed by postal questionnaires or online surveys in 1996, 2000, 2003, 2006, 2009, 2012 and 2015. The surveys included items about biological, psychological, social and lifestyle factors as well as physical and mental health and health service use. A summary of the constructs relevant to MatCH is shown in table 1.

Table 1: Sample of constructs and self-reported measures from ALSWH

Construct	Measures
Sexual health	Self-reported doctor diagnosis of sexually transmitted infections, visits to sexual health clinics.
Reproductive characteristics	Age at birth, inter-pregnancy interval, numbers of miscarriages, terminations, stillbirths, ectopic pregnancies and live births.
Fertility history	Diagnosis of endometriosis or polycystic ovary syndrome, use of fertility treatment, use of contraception.
Pregnancy and birth	For each pregnancy: gestational diabetes or hypertension, premature birth, infant birthweight, admission to special care.
Lifestyle factors	Height, weight, cigarette smoking, alcohol intake, use of illicit drugs, level of physical activity, sedentary behaviour.
Health service use	Visits to general practitioners and specialists; hospitalisation.
Childhood	Highest educational qualification and occupation of parents, experiences of abuse and adversity in childhood.
Adult socioeconomic position	Highest educational qualification, employment status, occupation history, income, ability to manage on income.
Family environment	Household composition, relationship with partner, experience of domestic violence, social support, life stressors, work–family conflict, family functioning.
Access to health services	Accessibility of services, remoteness from major cities.
Health related quality of life	Medical Outcomes Study Short Form-36 Health Survey (Version 1)[a].

Notes: [a] (Ware, Snow, Kosinski, Gandek, & Institute, 1993).

MatCH survey development

In 2014, focus groups and telephone interviews were undertaken with mothers of children aged 0–18 years to discuss the survey content, acceptable length and preferred modes of delivery, feelings about record linkage with external education databases and recommendations for encouraging participation by teenagers. For some of the proposed survey constructs, alternative instruments were trialled, with the most suitable chosen. The survey was pilot tested in 2015 with the 1973–78 pilot cohort, who regularly participate in survey development for the ALSWH. Women in this group who had reported at least one live birth were invited to complete surveys, either online or on paper, about their biological children under 13 years. Women were also asked to invite their older biological children (aged 13 to 18 years) to complete an online survey for themselves. There were separate surveys for each child according to age group (0–1 years, 2–4 years, 5–12 and 13–18 years).

As a result of the pilot testing it was decided to exclude teenagers as the recruitment strategy was unsuccessful (yielding a response rate of 8%; for more details see appendix A1). Consequently mothers were only asked to report on their three youngest children (it was estimated that less than 4% had more than three children aged under 13 years). Due to the ages of the children, the mothers completed all questions; that is, there was no child self-report. A tape measure was included in the mailed invitations as this had proved to increase participation and improve completion of the anthropometric measures. Based on maternal feedback about the burden of completing separate surveys for each child, the survey was re-formatted into a single multi-age instrument to make it easier to complete. Mothers who opted out of the survey were given the choice of consenting to data linkage only.

Selection of children

To be eligible for inclusion the children had to have birth mothers in the 1973–78 ALSWH cohort, be aged under 13 years on the date when the mother completed the MatCH survey and be currently living with their birth mother (at least part of the time).

Data collected on the children and families

The MatCH survey ran from August 2016 until May 2017. The measures used in the MatCH survey were designed to give a comprehensive snapshot of child health, development and wellbeing (table 2 and appendix A2). Individual measures were selected based on their scientific rigour, suitability for maternal report and length. Wherever possible, measures from other longitudinal studies were used in order to better enable comparisons.

MatCH is co-located with the ALSWH at the University of Queensland in Brisbane, Australia, with data collection procedures co-located with ALSWH at the University of Newcastle, Australia. Ethics approval was obtained from the University of Newcastle (reference number H-2014-0246) and The University of Queensland (reference number 2014001213) and women gave consent for themselves and their children.

Responses and participant characteristics

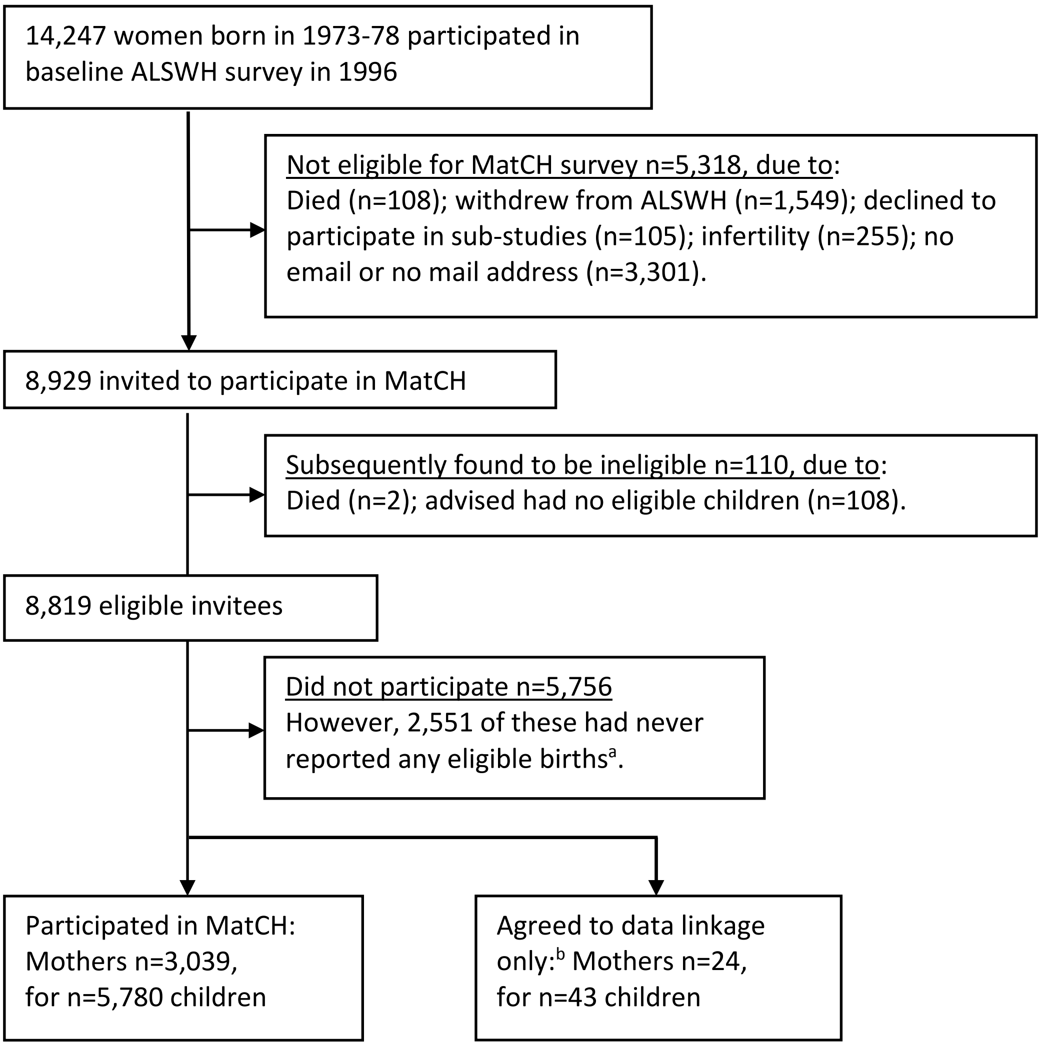
A total of 3,039 women responded to the MatCH survey invitation and provided information on 5,780 children (figure 1). The survey was completed online by 79% of respondents and on paper by 21%. A total of 2,229 mothers (73%) who completed the MatCH surveys consented to external record linkage and provided sufficient personal information to enable data linkage for 4,239 children. A further 24 women who did not complete the MatCH survey about their children did consent to their own survey data (which had details of their children's dates of birth and pregnancy histories) to be linked to external education record linkage about their children. The estimated response rate for the mothers was at least 34% (3,039/8,819) but could be up to 48% (3039/6268; see figure 1). As shown in figure 1, there were 2,551 women who did not respond to the invitation to MatCH for whom we had never ascertained any births. While it is possible that some eligible births had occurred in this group since their last ALSWH survey, many would have been ineligible (70% of the women in this group completed ALSWH survey 7, conducted in 2015–16, and a further 22% had completed at least one ALSWH survey since 2003). For the purposes of comparing MatCH survey participants and non-participants (table 3) we have included only women to have reported a child of eligible age for MatCH (N=6,268).

Table 2: Selection of measures used in MatCH and applicable ages of the children[a]

Construct	Measures	Age range (years)		
		0–1	**2–4**	**5–12**
Child characteristics	Sex, current date, date of birth, multiple/pre-term birth.	✓	✓	✓
	Childcare.	✓	✓	✓
	School grade.	X	X	✓
Quality of life	Pediatric Quality of Life Inventory™	✓	✓	✓
Illness/disability	Longstanding conditions and symptoms, injury, infections, impact of child health on family.	✓	✓	✓
Health care	GP use/ satisfaction, health service use, alternative health practitioner use, immunisation status.	✓	✓	✓
Sleep	Brief Infant Sleep Questionnaire (BISQ).	✓	X	X
	Pediatric Sleep Problems Survey Instrument.	X	✓	✓
Social/emotional development	Short Temperament Scale for Toddlers.	✓	X	X
	Strengths and Difficulties Questionnaire (SDQ)	X	✓	✓
Parenting	Parental warmth and hostility.	✓	✓	X
	Alabama Parenting Scale – Short form.	X	X	✓
Physical activity	Measures against Australian PA guidelines for children.	✓	✓	✓
Home environment	Access to books, reading, play equipment/ facilities.	✓	✓	✓
	Environmental exposures in the child's bedroom (heating/cooling, mould/mildew, dust, floor covering) and in the home generally (renovations, cigarette smoke, pets, pesticides, cleaning practices).	✓	✓	✓
	Age/ number of other children in the household, whether birth father lives with the child	✓	✓	✓
Nutrition	Infant feeding, Children's Dietary Questionnaire (CDQ).	✓	✓	✓
Physical growth/ development	Pubertal Development Scale.	X	X	✓
	Anthropometric measurements.	✓	✓	✓
Language/motor development	Ages and Stages (ASQ 3)™	✓	✓	✓

Notes: [a] see appendix A2 for details of the measures used.

Figure 1: Participation in MatCH.



Notes: [a] Maternal status ascertained at the time of invitation. Women with no known eligible children received a modified invitation protocol, while known mothers received the full protocol.
[b] Mothers did not complete the MatCH survey but agreed to let the study team link information about them and their children (already collected during ALSWH) to external record linkage under MatCH.
(Detailed invitation protocols are provided in appendix A3).

Table 3 shows demographic and other information for mothers who participated in MatCH and known mothers who were invited but did not participate. Descriptively, mothers who participated in MatCH were more likely to have completed the most recent ALSWH survey, to have higher education, to be in full-time work and live in a major city. They were more likely to have never smoked, have healthy weight and report excellent or very good health. Multivariable analysis revealed the factors most predictive of participation (at p<0.001) were a university-level education versus year 12 or equivalent (Odds ratio and 95% confidence interval = 1.79 (1.50, 2.13)), working full time (1.59 (1.37, 1.87)) or part time (1.34 (1.16, 1.55)) versus not being in the labour force, being a current smoker (0.66 (0.54, 0.81)) versus a non-smoker, and being widowed (3.78 (1.18, 12.16)) or separated (0.63 (0.47, 0.84)) versus being married/in a defacto relationship (for details, see appendix table A4a; in addition, appendix table A4b contains a comparison of the differences between the 3,039 known mothers who participated in MatCH and the 2,551 women who had never reported births of children of eligible age for MatCH but who were invited and did not participate).

Most of the 5,780 children were aged 5–12 years (table 4). There were slightly more boys than girls, 4% were from multiple births and 6% had been born prematurely. Overall, around 16% of children were overweight or obese. More than 97% had been vaccinated and 44% had been breastfed for 6 months. Older children had fewer sleep problems but more longstanding health problems. Less than 10% had a communication or gross motor development delay. Less than 5% of children overall were reported to have moderate-severe longstanding health conditions: the most prevalent condition was asthma (overall 11%, but with children aged 5-12 years having a prevalence of 12.5%). Overall, 22% of children were reported to have moderate-severe longstanding health symptoms: the most prevalent of these were Eczema, dermatitis, skin allergy (19.2% overall, highest in children aged 2-4 years (22%)), respiratory allergy (15% overall, highest in 5-12 year olds (18%)), and anxiety (11% overall, highest in 5-12 year olds (13%)).

Most mothers reported being satisfied with their general practitioners (89.6%) with 6.8% neither satisfied nor dissatisfied and 3.5% dissatisfied. With regard to characteristics of the home environment, almost all of the households represented in the study had internet access at home, a private yard for children to play in and more than 30 books at home (table 5). Half had a pet dog or cat that lived indoors and less than 1% of households had cigarettes smoked indoors. Almost one in four dwellings had been renovated while the woman was pregnant or in the first 12 months of the child's life and one in three had ever used professional pest treatments.

Table 3. Comparison of MatCH survey participants and women with children of eligible age for MatCH who were invited but did not participate (data are from the most recent ALSWH survey the women completed).

	Participating mothers (48.5%) %	Non-participating mothers[a] (51.5%) %	Difference between groups, *p*
Total number (N)	3,039	3,229	
Age at invitation (mean, SD)	40.8 (1.4)	40.8 (1.4)	0.737
Last ALSWH survey completed:			
Surveys 1–6	5.7	38.6	
Survey 7	94.3	61.4	<0.001
Highest qualification			
Less than year 12 or equivalent	4.0	8.5	
Year 12 or equivalent	9.4	14.3	
Trade/apprenticeship/Cert/Dip	23.5	30.8	
University	63.0	46.4	<0.001
Marital status			
Never married	2.8	2.9	
Married/De facto	90.6	88.9	
Separated	2.8	5.1	
Divorced	3.4	3.0	
Widowed	0.4	0.1	<0.001
Live births reported[b]			
>3	8.3	10.2	
3	23.7	24.5	
2	49.2	46.9	
1	18.9	18.4	0.039
Employment status			
Not in labour force	15.7	23.2	
Employed part time	48.4	47.5	
Employed full time	36.0	29.3	<0.001
Smoking			
Never smoked	54.5	48.9	
Ex-smoker	37.8	38.7	
Current smoker	7.7	12.4	<0.001
Area of residence			
Major city	59.9	54.5	
Inner regional	25.3	28.6	
Outer regional	12.2	13.9	
Remote	2.5	2.9	<0.001
Illicit drug use[c]			
Never used	40.4	42.9	
Ever used	59.6	57.1	0.053
Alcohol use[d]			
Non-drinker	9.8	11.9	
Low risk	84.2	82.4	
High risk	6.0	5.6	0.025

Table 3. Continued.

	Participating mothers (48.5%) %	Non-participating mothers[a] (51.5%) %	Difference between groups, *p*
Body mass index[e]			
Underweight	2.3	1.6	
Healthy	49.1	46.2	
Overweight	27.5	27.7	
Obese	21.1	24.4	0.005
Self-reported health			
Excellent/very good	62.2	55.7	
Good	30.2	34.3	
Fair/poor	7.6	10.0	<0.001

Notes: [a] excludes two women who had died, 108 who advised they had no eligible children, and 2,551 who had never reported any eligible births; [b] This includes all live births regardless of child's age; [c] illicit drugs – amphetamines, LSD, natural hallucinogens, tranquilisers, cocaine, ecstasy, inhalants, heroin or barbiturates; [d] low risk - ≤14 drinks/week, high risk – 15+ drinks/week; [e] World Health Organisation Body mass index (BMI) guidelines – underweight (<18.5 kg/m^2), healthy weight (18.5–24.9 kg/m^2), overweight (25–29.9 kg/m^2), obese (≥ 30 kg/m^2).

Table 4: Selected characteristics of the children

	Missing	Overall		< 2 years		2–4 years		5–12 years	
	N	N	%	N	%	N	%	N	%
Total number		5,780	100	329	5.7	1,051	18.2	4,400	76.1
Sex	6								
Male		2,990	51.8	173	52.6	571	54.5	2,246	51.1
Female		2,784	48.2	156	47.4	477	45.5	2,151	48.9
Birth									
Twin/triplet	256	218	4.0	13	4.1	38	3.8	167	4.0
Premature	121	349	6.2	15	4.6	74	7.2	260	6.0
Body Mass Index[a]	1220								
Underweight		491	11.6	-	-	92	11.5	399	11.6
Acceptable		3063	72.4	-	-	561	70.2	2503	72.9
Overweight		518	12.2	-	-	112	14.0	406	11.8
Obese		158	3.7	-	-	34	4.3	124	3.6
Current sleep problems	24	969	16.8	101	30.8	245	23.4	623	14.2
Vaccinated[b]	45	5,571	97.2	317	96.4	1,013	97.0	4,242	97.3
Exclusively breastfed to six months of age	125	2,498	44.2	143	44.4	441	42.6	1,914	44.6
Injury in past 12 months	43	982	17.1	23	7.1	163	17.1	797	18.2
Infection in past 12 months	170	2,963	52.8	162	50.0	684	67.4	2,118	49.6
Any moderate–severe longstanding conditions[c]	41	265	4.6	2	0.6	33	3.2	230	5.3
Any moderate–severe longstanding symptoms[c]	47	1,285	22.4	29	8.9	151	14.4	1105	25.4
Communication delay[d]	168	49	5.0	23	9.5	26	3.6	-	-
Gross motor delay[d]	178	86	8.9	33	13.6	53	7.4	-	-

Notes: [a] BMI only valid for children aged two years and over (N=5,451). In some cases BMI could not be calculated due to missing (N=1,184) or biologically implausible values (N=36). [b] Had received all recommended vaccinations for their age to date; [c] 'Longstanding' defined as '…something that has troubled your child over a long period of time, or is likely to affect your child over a long period of time' (e.g. longstanding conditions included asthma, heart problems and epilepsy; longstanding symptoms included dermatitis and food allergies); [d] Communication and Gross motor questions were only presented to online participants, and applicable to the younger age groups (N=1,140).

Table 5: Characteristics of the home environment and environmental exposures

	Missing N	Total N	%
Total number of households		3,039	100.0
Internet access at home	36	2,885	98.8
Private yard for kids to play in	44	2,939	98.1
Number of books at home	42		
None		0	0.0
1–10		34	1.1
11–20		91	3.0
21–30		153	5.1
More than 30		2,719	90.7
Cigarette smoking indoors	59	23	0.8
Pet (dog or cat) indoors	51	1,525	51.0
Use professional pest treatments	68	934	31.4
Mould in bedroom of at least one study child	49	195	6.5
Renovations, during pregnancy/first 12 months of life, of at least one study child	53	1,029	34.5

Conclusions

Multiple aspects of the family environment play a critical role in shaping child health and development and in determining health and social outcomes across the life course. This is reflected in policy and research priorities at the national and international level. The MatCH study has many strengths and can make a unique contribution to advancing this field of knowledge. The study includes the history of maternal and family health and social characteristics and takes a family-centred approach to understanding the varying factors that influence the health, development and health service use of the children within family units. MatCH builds on 20 years of prospectively collected background data on a large and nationally representative cohort of mothers, and includes families living in rural and remote areas. The inclusion of data linkage to national data sets on child development is a major strength and will enable monitoring of child development and health service use well into the future. Limitations to MatCH include the following. The response rate is estimated to be between 34% and 47.5%. Further, as MatCH was limited to women participants in the ALSWH with children aged under 13 years, the cohort of mothers was of restricted age range (25 to 43 years), which may introduce bias as socioeconomic, health and family characteristics of women who give birth at a young age are different from women who give birth at later ages, and the impact on their children's outcomes differ (Fall et al., 2015). The child data have only been collected at a single point in time, however further waves of data collection are planned dependent on funding and the use of data linkage mitigates this limitation to some extent. Only data on the three youngest siblings were examined, although data on birth order and the total number of children in the family are also available. Finally, the majority of data collected in MatCH is by self-report, although in a previous study of the same ALSWH participants agreement between self-reported perinatal outcomes and administrative records was found to be high (<87%) (Gresham et al., 2015). Further, potential biases from self-report may also be mitigated by the use of validated questionnaires and data linkage.

Overall, the MatCH study is uniquely placed to strengthen the evidence base on child health, development and health service use, and to inform policy. Findings from MatCH will support early identification of mothers who are most at risk of having children with poorer health and development outcomes, provide guidance for family-focused health care, and inform preventative and primary health care for Australian families.

Access to MatCH data and data from the ALSWH

The ALSWH data are available free of charge on request to bona fide researchers. The process is documented on the website [http://www.alswh.org.au/], which includes all the survey questionnaires, data books of frequency tables for all surveys, meta-data, conditions of data access and request forms. Restrictions are imposed by some of the human research ethics committees (both national and state-based) and some data custodians on where some of the linked data may be analysed. Currently, MatCH survey data are not available as they are still being cleaned, checked and tested. MatCH data, including weights to enable comparisons with the Australian population, will become available in the future with access through the same process as described above.

Acknowledgements

The project is funded by the National Health and Medical Research Council (NHMRC) project grant (APP1059550). GM is supported by an NHMRC Principal Research Fellowship (APP1121844). KDH is supported by an Australian Research Council Future Fellowship (FT130100637) and Honorary National Heart Foundation of Australia Future Leader Fellowship (100370). The authors are grateful to the Australian Government Department of Health for funding ALSWH and to the women and their children who provided the survey data.

References

Aral, S. O. (2001). Sexually transmitted diseases: magnitude, determinants and consequences. *Int J STD AIDS*, *12*(4), 211–215. https://doi.org/10.1258/0956462011922814

Australia. Department of Health and Ageing. (2010). *National Women's Health Policy 2010*. Canberra: retrieved from http://www.health.gov.au/womenshealthpolicy

Australian Bureau of Statistics. (2015). 4159.0 *General social survey: Summary results, Australia 2014*. Canberra: retrieved from http://www.abs.gov.au/AUSSTATS/abs@.nsf/Lookup/4159.0Main+Features12014?OpenDocument

Australian Department of Health and Ageing. (2011). *Improving Primary Health Care for All Australians*. Canberra: retrieved from https://trove.nla.gov.au/version/94226599

Australian Institute of Health and Welfare. (2010). *Medical Labour Force 2008*. Canberra: retrieved from https://www.aihw.gov.au/reports/workforce/medical-labour-force-2008/contents/table-of-contents

Australian Institute of Health and Welfare. (2012). *A picture of Australia's children 2012*. Canberra: retrieved from https://www.aihw.gov.au/reports/children-youth/a-picture-of-australia-s-children-2012/contents/summary

Bradley, R. H., & Corwyn, R. F. (2002). Socioeconomic status and child development. *Annu Rev Psychol*, *53*, 371–399. https://doi.org/10.1146/annurev.psych.53.100901.135233

Brown, W., Bryson, L., Byles, J. E., Dobson, A. J., Lee, C., Mishra, G. D., & Schofield, M. (1998). Women's health Australia: Recruitment for a national longitudinal cohort study. *Women and Health*, *28*(1), 23–40. https://doi.org/10.1300/J013v28n01_03

Camarano, L., Alkon, A., Nachtigall, R. D., Schembri, M., Weiss, S., & Croughan, M. S. (2012). Preterm delivery and low birth weight in singleton pregnancies conceived by women with and without a history of infertility. *Fertil Steril*, *98*(3), 681–686 e681. https://doi.org/10.1016/j.fertnstert.2012.04.033

Cameron, A., Ball, K., Pearson, N., Lioret, S., Crawford, D., Campbell, K., Hesketh, K., & McNaughton, S. (2012). Socioeconomic variation in diet and activity-related behaviours of Australian children and adolescents aged 2–16 years. *Pediatric obesity*, *7*(4), 329–342. https://doi.org/10.1111/j.2047-6310.2012.00060.x

Chen, E., Matthews, K. A., & Boyce, W. T. (2002). Socioeconomic differences in children's health: how and why do these relationships change with age? *Psychol Bull*, *128*(2), 295–329. https://doi.org/10.1037/0033-2909.128.2.295

Dobson, A. J., Hockey, R., Brown, W. J., Byles, J. E., Loxton, D. J., McLaughlin, D., Tooth, L. R., & Mishra, G. D. (2015). Cohort Profile Update: Australian Longitudinal Study on Women's Health. *International Journal of Epidemiology*, *44*(5), 1547–1547f. https://doi.org/10.1093/ije/dyv110

Fall, C. H. D., Sachdev, H. S., Osmond, C., Restrepo-Mendez, M. C., Victora, C., Martorell, R., Stein, A. D., Sinha, S., Tandon, N., Adair, L., Bas, I., Norris, S., & Richter, L. M. (2015). Association between maternal age at childbirth and child and adult outcomes in the offspring: a prospective study in five low-income and middle-income countries (COHORTS collaboration). *The Lancet Global Health*, *3*(7), e366–e377. https://doi.org/10.1016/S2214-109X(15)00038-8

Filicori, M., Cognigni, G. E., Gamberini, E., Troilo, E., Parmegiani, L., & Bernardi, S. (2005). Impact of medically assisted fertility on preterm birth. *BJOG*, 11 Suppl, 113–117. https://doi.org/10.1111/j.1471-0528.2005.00598.x

Grant, K. L., Carpenter, D. O., Sly, L. J., & Sly, P. D. (2013). Environmental contributions to chronic non-communicable diseases: redefining environmental diseases. *J Environ Immunol Toxicol*, *1*, 51–52. https://doi.org/10.7178/jeit.11

Gresham, E., Forder, P., Chojenta, C. L., Byles, J. E., Loxton, D. J., & Hure, A. J. (2015). Agreement between self-reported perinatal outcomes and administrative data in New South Wales, Australia. *BMC Pregnancy and Childbirth*, *15*(1), 161. https://doi.org/10.1186/s12884-015-0597-x

Herbert, D. L., Lucke, J. C., & Dobson, A. J. (2012). Birth outcomes after spontaneous or assisted conception among infertile Australian women aged 28 to 36 years: a prospective, population-based study. *Fertil Steril*, *97*(3), 630–638. https://doi.org/10.1016/j.fertnstert.2011.12.033

Lawlor, D. A., Tooth, L., Lee, C., & Dobson, A. J. (2005). A comparison of the association between socioeconomic position and cardiovascular disease risk factors in three age cohorts of Australian women: findings from the Australian Longitudinal Study on Women's Health. *J Public Health (Oxf)*, *27*(4), 378–387. https://doi.org/10.1093/pubmed/fdi052

Lee, C., Dobson, A. J., Brown, W. J., Bryson, L., Byles, J., Warner-Smith, P., & Young, A. F. (2005). Cohort Profile: the Australian Longitudinal Study on Women's Health. *Int J Epidemiol*, *34*(5), 987–991. https://doi.org/10.1093/ije/dyi098

Lee, D. H., Steffes, M. W., Sjodin, A., Jones, R. S., Needham, L. L., & Jacobs, D. R. (2010). Low Dose of Some Persistent Organic Pollutants Predicts Type 2 Diabetes: A Nested Case-Control Study. *Environmental Health Perspectives*, *118*(9), 1235–1242. https://doi.org/10.1289/ehp.0901480

Loxton, D., Mooney, R., & Young, A. F. (2006). The psychological health of sole mothers in Australia. *The Medical Journal of Australia*, *184*(6), 265.

Loxton, D., Tooth, L., Harris, M. L., Forder, P. M., Dobson, A. J., Powers, J., Brown, W., Byles, J., & Mishra, G. D. (2017). Cohort Profile: The Australian Longitudinal Study on Women's Health (ALSWH) 1989–95 cohort. *International Journal of Epidemiology*, dyx133. https://doi.org/10.1093/ije/dyx133

Luby, J., Belden, A., Botteron, K., Marrus, N., Harms, M. P., Babb, C., Nishino, T., & Barch, D. (2013). The effects of poverty on childhood brain development: the mediating effect of caregiving and stressful life events. *JAMA Pediatr*, *167*(12), 1135–1142. https://doi.org/10.1001/jamapediatrics.2013.3139

Maggi, S., Irwin, L. J., Siddiqi, A., & Hertzman, C. (2010). The social determinants of early child development: an overview. *J Paediatr Child Health*, *46*(11), 627–635. https://doi.org/10.1111/j.1440-1754.2010.01817.x

Mishra, G. D., Anderson, D., Schoenaker, D. A., Adami, H. O., Avis, N. E., Brown, D., Bruinsma, F., Brunner, E., Cade, J. E., Crawford, S. L., Dobson, A. J., Elliott, J., Giles, G. G., Gold, E. B., Hayashi, K., Kuh, D., Lee, K. A., Lee, J. S., Melby, M. K., Mizunuma, H., Sievert, L. L., & Weiderpass, E. (2013). InterLACE: A New International Collaboration for a Life Course Approach to Women's Reproductive Health and Chronic Disease Events. *Maturitas*, *74*(3), 235–240. https://doi.org/10.1016/j.maturitas.2012.12.011

Morris, A. S., Robinson, L. R., Hays-Grudo, J., Claussen, A. H., Hartwig, S. A., & Treat, A. E. (2017). Targeting Parenting in Early Childhood: A Public Health Approach to Improve Outcomes for Children Living in Poverty. *Child Development*, *88*(2), 388–397. https://doi.org/10.1111/cdev.12743

Pearce, A., Lewis, H., & Law, C. (2013). The role of poverty in explaining health variations in 7-year-old children from different family structures: findings from the UK Millennium Cohort Study. *J Epidemiol Community Health*, *67*(2), 181–189. https://doi.org/10.1136/jech-2012-200970

Propper, C., Rigg, J., & Burgess, S. (2007). Child health: evidence on the roles of family income and maternal mental health from a UK birth cohort. *Health Econ*, *16*(11), 1245–1269. https://doi.org/10.1002/hec.1221

Qin, J., Liu, X., Sheng, X., Wang, H., & Gao, S. (2016). Assisted reproductive technology and the risk of pregnancy-related complications and adverse pregnancy outcomes in singleton pregnancies: a meta-analysis of cohort studies. *Fertility and Sterility*, *105*(1), 73–85.e76. https://doi.org/10.1016/j.fertnstert.2015.09.007

Sawyer, S. M., Afifi, R. A., Bearinger, L. H., Blakemore, S. J., Dick, B., Ezeh, A. C., & Patton, G. C. (2012). Adolescence: a foundation for future health. *Lancet*, 379(9826), 1630–1640. https://doi.org/10.1016/S0140-6736(12)60072-5

Schofield, D. J., Shrestha, R. N., & Callander, E. J. (2012). Access to general practitioner services amongst underserved Australians: a microsimulation study. *Hum Resour Health*, *10*(1), 1. https://doi.org/10.1186/1478-4491-10-1

Schoon, I., Sacker, A., & Bartley, M. (2003). Socio-economic adversity and psychosocial adjustment: a developmental-contextual perspective. *Soc Sci Med*, *57*(6), 1001–1015. https://doi.org/10.1016/S0277-9536(02)00475-6

Vimpani, G., Patton, G., & Hayes, A. (2004). The relevance of child and adolescent development for outcomes in education, health and life success, in *Children's Health and Development: New research directions for Australia*. Melbourne: retrieved from https://aifs.gov.au/publications/childrens-health-and-development

Ware, J. E., Snow, K. K., Kosinski, M., Gandek, B., & New England Medical Center Hospital. Health Institute. (1993). *SF-36 health survey: manual and interpretation guide*. Boston, Mass.: The Health Institute, New England Medical Center.

Appendix

A1: Overview of attempt to recruit teenage children (13–18 years)

In the MatCH Pilot study, mothers were gatekeepers for access to teens. Survey development and engagement activities that we conducted between 2012 and 2014 (including an online survey of 400 mothers, 80% of whom responded) showed that mothers were highly supportive of the proposed MatCH study. Focus groups with mothers subsequently revealed that incentives would be required for teen recruitment, however, we did not have a specific budget for this. For the Pilot study we offered teen prize draws (200 chances to win a $25 voucher). While similar incentives had recently proved to be highly effective in the online recruitment of a new cohort of 18–23 year old women to the ALSWH study, in that instance participants were drawn from the general public, rather than being already defined by their relationship to members of a previous cohort, as was the case for MatCH.

The MatCH recruitment strategy was designed with due regard to teen privacy: we had no means of directly contacting and following up potential teen participants. Rather, we asked the mother to pass on the online survey link. There were also ethical constraints about using unequal power relationships (in this case, parent–child) to induce participation. Therefore, we could not disclose to the mothers that their teen's survey was still not completed (however, in cases where we followed up the mother about her own survey, we added a general reminder about passing the invitation on to any teenaged children). Feedback from the telephone follow up of mothers revealed that they were frequently reluctant to pass the invitation on to their teens, or, if they did pass it on, to remind them about it.

The poor results to the Teen Pilot meant that we would have had to develop a completely new recruitment strategy for this group. However, there was a high risk that this would exhaust the remaining budget and timelines, so we decided to focus only on the younger children for the current survey wave. Teen participation may be re-visited in future waves, depending on available funding, the length of follow up, and the interests and capacity of the researchers.

A2: Details of the measurements and assessment scales used in the MatCH survey

Measurements and assessment scales	Details	Applicable ages
Ages and Stages Questionnaires Third Edition (ASQ-3)® Communication and Gross Motor subscales only	Squires, J., Bricker, D., & Potter, L. (1997). Revision of a parent-completed developmental screening tool: Ages and Stages Questionnaires. *Journal of Pediatric Psychology*, *22*(3), 313–328. http://www.brookespublishing.com/resource-center/screening-and-assessment/asq/asq-3/	1–66 months
Alabama Parenting Scale – Short form	Elgar, F. J., Waschbusch, D. A., Dadds, M. R., & Sigvaldason, N. (2007). Development and Validation of a Short Form of the Alabama Parenting Questionnaire. *Journal of Child and Family Studies*, *16*(2), 243–259. doi: 10.1007/s10826-006-9082-5	5–12 years
Body Mass Index in children	Cole, T. J., Flegal, K. M., Nicholls, D., & Jackson, A. A. (2007). Body mass index cut offs to define thinness in children and adolescents: international survey. *British Medical Journal*, *335*(7612), 194. doi: 10.1136/bmj.39238.399444.55	2–12 years
Brief Infant Sleep Questionnaire	Sadeh, A. (2004). A brief screening questionnaire for infant sleep problems: validation and findings for an Internet sample. *Pediatrics*, *113*(6), e570–e577.	0–1 years
Checklist of longstanding conditions and symptoms	Adapted from question used in the 2013 Primary Caregiver 3-Year Questionnaire, Growing up in Ireland Study[a]	0–12 years
Childcare	Adapted from unpublished question used in Deakin University 'HAPPY/INFANT' Studies[b]	0–12 years
Children's Dietary Questionnaire, Section B and C only	Adapted from: Magarey, A., Golley, R., Spurrier, N., Goodwin, E., & Ong, F. (2009). Reliability and validity of the Children's Dietary Questionnaire; a new tool to measure children's dietary patterns. *International Journal of Pediatric Obesity*, *4*(4), 257–265. doi: 10.3109/17477160902846161	0–12 years (individual items were asked for all children old enough to consume solid foods, but scores were only computed for children aged 2 years and over)
Health service utilisation:	Based on questions used in Longitudinal Study of Australian Children (LSAC)[c] and Growing up in New Zealand[d] studies.	0–12 years
Home environment:		0–12 years
- Availability/use of play equipment / facilities, including screen-based equipment	Based on questions used by Deakin University HAPPY and INFANT Studies[b]	

Measurements and assessment scales	Details	Applicable ages
- Books and reading	Questions used in the Longitudinal Study of Australian Children (LSAC)[c] and Growing up in New Zealand[d] Studies	
- Bedroom heating/cooling, ventilation	Questions used in Growing up in New Zealand Study[d] adapted for Australian environment.	
- Bedroom floor covering - Home renovations during gestation/ early life - Passive cigarette smoke - Pets in the home - Home pesticide use - Home cleaning practices	Selected items from a questionnaire under development by Queensland Children's Medical Research Institute (The questions are new, but are based on associations described in English, K., Healy, B., Jagals, P., & Sly, P. D. (2015). Assessing exposure of young children to common endocrine-disrupting chemicals in the home environment: a review and commentary of the questionnaire-based approach. *Reviews of Environmental Health*, *30*(1), 25-49. doi: 10.1515/reveh-2014-0069).	
- Other resident family members	New question	
Immunisation status	New question	0–12 years
Impact of child's health on child and family	New question	0–12 years
Infant feeding	Adapted from questions used in: • Feeding Queensland Babies. Newby, R., Brodribb, W., Ware, R. S., & Davies, P. S. (2014). Infant feeding knowledge, attitudes, and beliefs predict antenatal intention among first-time mothers in Queensland. *Breastfeeding Medicine*, *9*, 266–272. doi: 10.1089/bfm.2014.0012 • Australian National Infant Feeding Survey. Fein, S. B., Labiner-Wolfe, J., Shealy, K. R., Li, R., Chen, J., & Grummer-Strawn, L. M. (2008). Infant Feeding Practices Study II: study methods. *Pediatrics*, *122 Suppl 2*, S28–35. doi: 10.1542/peds.2008-1315c	0–12 years
Infections	Growing up in New Zealand Study[d]	0–12 years
Injury question	From question in WHO Health Behaviour in School Children Study. Scheidt, P. C., Harel, Y., Trumble, A. C., Jones, D. H., Overpeck, M. D., & Bijur, P. E. (1995). The epidemiology of nonfatal injuries among US children and youth. *American Journal of Public Health*, *85*(7), 932–938.	0–12 years
PACE+: adherence to Physical Activity guidelines	Question adapted for Australian Guidelines. Prochaska, J. J., Sallis, J. F., & Long, B. (2001). A physical activity screening measure for use with adolescents in primary care. *Archives of Pediatric and Adolescent Medicine*, *155*(5), 554–559.	1–12 years

Measurements and assessment scales	Details	Applicable ages
Parental warmth	Paterson, G., & Sanson, A. (1999). The Association of Behavioural Adjustment to Temperament, Parenting and Family Characteristics among 5-Year-Old Children. *Social Development*, *8*(3), 293–309. doi: 10.1111/1467-9507.00097	0–4 years
Parental hostility	Early Childhood Longitudinal Program[e] (as used in LSAC[c])	
Pediatric Sleep Problems Survey Instrument	Biggs, S. N., Kennedy, J. D., Martin, A. J., van den Heuvel, C. J., & Lushington, K. (2012). Psychometric properties of an omnibus sleep problems questionnaire for school-aged children. *Sleep Medicine*, *13*(4), 390-395. doi: 10.1016/j.sleep.2011.12.005	2–12 years
PedsQL[TM]: Infant/ Toddler/ Young child/ Child (Australian) versions	Varni, J. W., Seid, M., & Rode, C. A. (1999). The PedsQL (TM): Measurement model for the pediatric quality of life inventory. *Medical Care*, *37*(2), 126–139. http://pedsql.org/	0–12 years
Pubertal Development Scale	Carskadon, M. A., & Acebo, C. (1993). A self-administered rating scale for pubertal development. *Journal of Adolescent Health*, *14*(3), 190–195.	5–12 years
School grade	New question	4–12 years
Short Temperament Scale for Toddlers	Short version as used by the Longitudinal Study on Australian Children (LSAC) Fullard, W., McDevitt, S. C., & Carey, W. B. (1984). Assessing Temperament in One-to Three-Year-Old Children. *Journal of Pediatric Psychology*, *9*(2), 205–217. doi: 10.1093/jpepsy/9.2.205	0–1 years
Strengths and Difficulties Questionnaire (SDQ), 2–4, 4–10 years (Australian) versions	Goodman, R. (1994). A Modified Version of the Rutter Parent Questionnaire Including Extra Items on Children's Strengths: A Research Note. *Journal of Child Psychology and Psychiatry*, *35*(8), 1483–1494. doi: 10.1111/j.1469-7610.1994.tb01289.x http://www.sdqinfo.com/	2–12 years

[a] http://growingup.ie/index.php?id=83
[b] http://www.deakin.edu.au/ipan/our-research/other-projects
[c] http://www.growingupinaustralia.gov.au/
[d] http://www.growingup.co.nz/en.html
[e] https://nces.ed.gov/ecls/

A3: Details of the MatCH study invitation protocol and response rates

Table A3a: MatCH study invitation protocol

MatCH Study: Recruitment protocols		When sent[c]
A	**Full invitation protocol: Women with children of eligible age for MatCH[a] (N=6245)**	
A1	Mailed invitation	Batch 1: 23/8/2016
		Batch 2: 28/10/2016
A2	Email invitation	8 days after A1
A3	Email reminder with survey link	8 days after A2
A4	SMS reminder	8-10 days A3
A5	Mailed letter and paper survey	4-7 weeks A4
A6	Telephone follow up[d]	4 weeks after A5
A7	Email Invitation or paper survey re-sent (if requested	As soon as possible after A6
A8	Final SMS notification	3/05/2017
A9	Final email re data linkage-only option[d]	10/05/2017
B	**Modified invitation protocol: Women who had never reported births of children of eligible age for MatCH[b] (N=2684)**	
B1	Email invitation	Batch 1: 1/09/2016
		Batch 2: 2/11/2016
B2	Email reminder with survey link	8 days after B1
C	**Follow up of incomplete online surveys (N=1,499)**	
C1	First email reminder	2 hours after last survey login
C2	Second email reminder	8 days after C2
C3	SMS reminder	8 days after C3
C4	Telephone follow up	14 days after C4
C5	Email reminder or paper survey re-sent (if requested)	As soon as possible after C4
C6	Final SMS notification	3/05/2017

Notes: [a] Women in the ALSWH 1973–78 cohort who had reported at least one birth in the eligible age range in ALSWH surveys since 2003. [b] Women in the ALSWH 1973–78 cohort who had never reported any births in the eligible age range in ALSWH surveys since 2003. The protocol was modified to be sensitive to personal reproductive circumstances. [c] Invitations were initially issued to women who had completed follow up for ALSWH Survey 7. Batch 2 was issued to remaining potentially eligible women after the close of data collection for ALSWH Survey 7. [d] The 'data linkage only' option was offered on declining the survey online; on telephone follow up (where women indicated they did not have time to do the survey); and by email at the close of the survey (Protocol A9). Under this option women provided their child's personal details and consented to linkage of the child's health and education records with data held by ALSWH about the mother.

Table A3b: Response rate by study invitation protocol

	A Known mothers		B Women who had never reported births of children of eligible age for MatCH		A + B All women	
	N	%	N	%	N	%
ALSWH 1973–78 Cohort	6594	100.0	7653	100.0	14247	100.0
Not invited:	349	5.3	4969	64.9	5318	37.3
Deceased/withdrawn	163		1494		1657	
Other (declined substudies, infertile, lacking contact details[a])	186		3475		3661	
Invited:	6245	94.7	2684	35.1	8929	62.7
Advised not eligible	61		49		110	
Eligible invitees:	6184	100.0	2635	100.0	8819	100.0
Did not respond	3205	51.8	2551	96.8	5756	65.3
Data linkage only option	24	0.4	0	0.0	24	0.3
Survey participants	2955	47.8	84	3.2	3039	34.5

Notes: [a] For Protocol A, women could be invited if they had either a mailing address or a valid email address in the ALSWH Participant Database. For Protocol B, a valid email address was a prerequisite to invitation.

A4: Comparisons between MatCH participants and ALSWH participants who may or may not have been eligible for MatCH

Table A4a: Fully adjusted logistic regression analysis of the comparison between women with children of eligible age for MatCH who did (N=3,039) and did not (N=3,229) participate in MatCH (Odds ratios (OR), 95% Confidence interval (CI)).

Variables	OR (for participation)	95% CI
Age	0.99	0.95, 1.02
Education (Ref: Year 12 or equivalent)		
Less than year 12	0.82	0.62, 1.08
Trade/Certificate/Diploma	1.08	0.89, 1.29
University	1.79***	1.50, 2.13
Marital status (Ref: married/defacto)		
Divorced	1.29	0.95, 1.76
Never married	1.23	0.88, 1.72
Separated	0.63**	0.47, 0.84
Widowed	3.78*	1.18, 12.16
Labour force participation (Ref: Not in labour force)		
Full time	1.59***	1.37, 1.87
Part time	1.34***	1.16, 1.55
Smoking status (Ref: Never smoked)		
Current smoker	0.66***	0.54, 0.81
Ex-smoker	0.89	0.79, 1.01
Alcohol consumption (Ref: Low risk consumption)		
Non-drinker	0.89	0.75, 1.07
Risky Drinker	1.15	0.89, 1.41
Area of residence (Ref: Major city)		
Inner regional	0.92	0.81, 1.04
Outer regional	0.90	0.77, 1.07
Remote	0.88	0.63, 1.23
Self-rated health (Ref: Excellent/very good)		
Fair/Poor	0.90	0.74, 1.11
Good	0.89	0.79, 1.01
Body Mass Index (Ref: Healthy weight)		
Underweight	1.52*	1.02, 2.26
Overweight	1.00	0.88, 1.14
Obese	0.99	0.86, 1.39
Number of children (Ref: Two)		
One	0.94	0.81, 1.09
Three or more	0.97	0.85, 1.11

Overall logistic regression model Likelihood Ratio χ^2_{26} = 266.9, $p \leq 0.001$

*** $p \leq 0.001$; ** $p \leq 0.01$; * $p \leq 0.05$

Table A4b. Comparison of MatCH survey participants (N=3,039) and women who had never reported births of children of eligible age for MatCH[a] and who were invited but did not participate (N=2,551) (data are from the most recent ALSWH survey the women completed).

	Participating mothers	Women who had never reported births of eligible children but who were invited and did not participate	Difference between groups, *p*
Total number (N)	3,039	2,551	
Age at invitation (mean, SD)	40.8 (1.4)	40.9 (1.4)	0.008
Last ALSWH survey completed:			
Surveys 1–6	5.7	29.9	
Survey 7	94.3	70.1	<0.001
Highest qualification			
Less than year 12 or equivalent	4.0	7.3	
Year 12 or equivalent	9.4	13.0	
Trade/apprenticeship/Cert/Dip	23.5	29.7	
University	63.0	50.0	<0.001
Marital status			
Never married	2.8	34.7	
Married/De facto	90.6	55.6	
Separated	2.8	3.8	
Divorced	3.4	5.5	
Widowed	0.4	0.4	<0.001
Live births reported[b]			
>3	8.3	1.4	
3	23.7	6.7	
2	49.2	17.0	
1	18.9	9.1	
0	0	65.9	<0.001
Employment status			
Not in labour force	15.7	8.4	
Employed part time	48.4	22.0	
Employed full time	36.0	69.6	
Smoking			
Never smoked	54.5	50.2	
Ex-smoker	37.8	31.3	
Current smoker	7.7	18.5	<0.001
Area of residence			
Major city	59.9	59.8	
Inner regional	25.3	25.2	
Outer regional	12.2	12.6	
Remote	2.5	2.4	0.958
Illicit drug use[c]			
Never used	40.4	40.7	
Ever used	59.6	59.3	0.866
Alcohol use[d]			
Non-drinker	9.8	10.2	
Low risk	84.2	82.0	
High risk	6.0	7.8	0.026

Body mass index[e]			
Underweight	2.3	2.1	
Healthy	49.1	41.8	
Overweight	27.5	25.6	
Obese	21.1	30.5	<0.001
Self-reported health			
Excellent/very good	62.2	51.9	
Good	30.2	35.0	
Fair/poor	7.6	13.1	<0.0001

[a] these women may have had older children but had never reported any children in the eligible age range for MatCH, they were invited in case they had an eligible child who was not known to the ALSWH survey team ; [b] includes all live births regardless of child's age ; [c] illicit drugs – amphetamines, LSD, natural hallucinogens, tranquilisers, cocaine, ecstasy, inhalants, heroin or barbiturates; [d] low risk - ≤14 drinks/week, high risk - 15+ drinks/week; [e] World Health Organisation Body mass index (BMI) guidelines - underweight (<18.5 kg/m^2), healthy weight (18.5-24.9 kg/m^2), overweight (25-29.9 kg/m^2), obese (≥ 30 kg/m^2).

Table A4b shows that the women who had never reported births of children of eligible age for MatCH were more likely to be unmarried, to have never had children (or to have had them at an earlier age), to be working full time, and to have slightly poorer health indicators. This group also included a higher percentage of ALSWH survey participants who had not recently completed surveys.

Longitudinal and Life Course Studies 2018 Volume 9 Issue 3 Pp 376–378 ISSN 1757-9597

KEYNOTE LECTURE
The impact imperative

David Bell University of Stirling, UK

Based on a keynote presentation to the Society for Longitudinal and Life Course Studies conference at Stirling University, October 2017.

I recently attended a book launch at New Register House in Edinburgh, the magnificent home of the National Records of Scotland. The author was Michael Anderson, professor emeritus of Economic History at the University of Edinburgh. His book is a 480-page quantitative history of Scotland's population based on Scotland's censuses and vital events records from 1851 to 2011 (Anderson, 2018). It will be an invaluable reference for demographers for many years to come.

Unfortunately, this is the kind of scholarship that seems to me to have been sidelined by the Research Excellence (REF) process in the UK. The REF is the key mechanism that the funding councils in England, Scotland and Wales use to distribute research funding to higher education institutions. Its results will be important both for the financial health of an institution and, perhaps more importantly, for its reputation, which is a key driver of its ability to attract high-quality staff and students.

The REF assessment process is increasingly rewarding the 'impact' of research on external stakeholders, such as policymakers. More precisely, impact is intended to capture the "'reach and significance' of impacts on the economy, society and/or culture that were underpinned by excellent research"(REF02, 2011, p6). For the 2021 REF, impact will have a weight of 25% of the overall assessment.

Short, sharp pieces of research that have a "measurable" impact will tend to gain favour with university managers in their quest to maximise research income. However, as he has been retired since 2007, Michael is not bound by such narrow considerations.

There are parallels between the response that Michael's book would likely get from a REF panel and the work that supports many of the longitudinal studies that are the standard tools of the Society for Longitudinal and Life Course Studies: it is difficult to associate them with measurable impacts.

The difficulty of establishing impact with longitudinal studies was one of the themes I touched on in my keynote address to last year's SLLS conference. It continues to interest me, given that my colleagues and I have now completed a pilot study for a new longitudinal study of ageing in Scotland (Douglas, Rutherford & Bell, 2018).

A quick perusal of the impact prizes awarded by the Economic and Social Research Council (ESRC) in recent years suggests that winners tend to focus on research topics whose impacts are amenable to direct measurement over relatively short time periods. None of the winners in the last four years used any of the longitudinal studies in which the ESRC now invests around £20million per year.

The difficulties of establishing impact were highlighted in the recently published ESRC review of longitudinal studies. While broadly endorsing the value of continuing these investments, the independent, international review panel argued that "Impacts from these ESRC longitudinal data investments undoubtedly exist but are hard to pinpoint and quantify, in part because insights drawn from their use more typically contribute to 'conceptual' impact (or 'enlightenment') and thus act gradually to change the discourse, thinking, and common knowledge around an issue." (Davis-Kean et al., 2018, page 6). It may be that, as the review implies, impacts have occurred mainly over the long term, when studies have several waves under their belt. This may help with causal inference, but makes funders impatient.

On the other hand, perhaps longitudinal studies are not good at recording impact as currently defined for REF purposes. This is not an easy task and rarely a top priority for investigators when studies are being designed. One reason that the ESRC (with the Medical Research Council (MRC)) established the Cohort and Longitudinal Studies Enhancement Resource (CLOSER) was to increase the impact of the U.K.'s longitudinal studies. Meanwhile, the household panel study, Understanding Society, has established a policy unit to increase direct interaction with the policy community. Such initiatives may help longitudinal studies increase their overall impact, but whether they help in relation to impact as defined by the REF remains to be seen.

My experience in setting up the Healthy Ageing In Scotland (HAGIS) study has brought home to me the huge fixed costs associated with establishing a new longitudinal study (Douglas et al., 2018). Almost all of my earlier career had been built on secondary data analysis, so the mechanics of survey design, sampling, interviewing, data collection and processing came as a bit of a shock. One innovation that we introduced was to base our sampling on an administrative data spine – the National Health Service Central Register – that has existed in Scotland since the early 1950s. The ESRC Longitudinal Studies Review argued that sampling from population data spines offered significant advantages over traditional sampling methods. However, getting ethical approval for this innovation from the Public Benefit and Privacy Panel took more than a year. And interviewing didn't work as initially planned. It required frequent interaction with our very committed survey company. And all of this effort was for a pilot survey that collected only 1000 responses.

These fixed costs have to be incurred before there can be any possibility of establishing impact. As a result, we have taken the view that we should not be shy about publishing findings that rely simply on cross-sectional evidence. Many policymakers simply wish to understand the context in which actors are choosing between alternative courses of action. And there, cross-sectional data can be of value, also helping to maintain stakeholder support for the study.

However, for the REF, impact studies have to be based on excellent research, meaning that they have to establish a direct link to the impact from high-quality, peer-reviewed, research publications. Since longitudinal studies are, almost by definition, multidisciplinary undertakings, the potential for impact may be spread across different assessment units (subjects) as defined in the REF. This will increase the scope for impact submissions, but may make it more difficult to establish a clear path from research to impact within a single disciplinary assessment unit. The difficulties of assessing interdisciplinary research were recognised by the Stern Review (Stern, 2016). Following its recommendations, the 2021 REF panels will have at least one member to oversee and participate in the assessment of interdisciplinary research. This may increase the probability that longitudinal studies have an enhanced role in the REF, but much will depend on the precise role given to the 'interdisciplinarity' panel members. And if impact continues to be assessed in a narrow sense, longitudinal studies may have to accommodate such considerations more widely – from survey design to dissemination. Nevertheless, it would be a matter of real regret if this process led to a weakening of the general insights that longitudinal studies – or a good book – can provide.

References

Anderson, M. (2018). *Scotland's Populations from the 1850s to Today*. Oxford: Oxford University Press. https://doi.org/10.1093/oso/9780198805830.001.0001

Davis-Kean, P., Chambers, R., Davidson, L. L., Kleinert, C., Ren, Q. and Tang, S. (2018). Longitudinal Studies Strategic Review: 2017 Report to the Economic and Social Research Council https://esrc.ukri.org/files/news-events-and-publications/publications/longitudinal-studies-strategic-review-2017 Accessed May 10 2018.

Douglas, E., Rutherford, A., Bell, D. (2018). Pilot study protocol to inform a future longitudinal study of ageing using linked administrative data: Healthy AGeing in Scotland (HAGIS). BMJ Open 2018, 8:e018802. https://doi.org/10.1136/bmjopen-2017-018802

Stern, N. (2016) Building on Success and Learning from Experience An Independent Review of the Research Excellence Framework. Department for Business, Energy & Industrial Strategy, London. https://www.gov.uk/government/publications/research-excellence-framework-review

AUTHOR GUIDELINES SUMMARY

Submission of Papers

All papers, written in the English language, should be submitted via the *LLCS* website as a Microsoft Word file. If there is a good reason why this is not possible, authors are requested to contact sjeal@slls.org.uk **before** submitting the paper. All subsequent processes involving the author are carried out electronically via the website.

Preparation of Texts

Length. The paper should normally be **approximately 5,000 words**, with longer papers also accepted up to a maximum of 7,000 words. This word count excludes tables, figures and bibliography.

Font and line spacing. Please use Calibri (or similar sans serif) font, 12pt, with 1.5 line spacing in all main text, single space in figure or table captions.

Page layout. All text should be justified to the left hand margin (all margins of at least 2.5cm) with no indents at the start of paragraphs and a line space separating paragraphs. All headings and sub-headings used within the body of the paper should also be left justified. Please do **NOT** use automatic text formatting.

Weblinks. To help our readers to look up cited references or other information available on the web, authors should ensure that all such references are activated.

DOIs. Do NOT include DOIs in the bibliography – they are added during editing as resolvable URLs.

Ensuring an anonymous (blind) review. Please submit papers with a full detailed title page. Once a paper has been submitted to *LLCS* via the website, it will be 'anonymised' by the removal of all author name(s) and institution names on the title page, and any identifying electronic document properties will also be removed. Authors do not need to remove their name(s) from the main text or references but any reference to their work or themselves should be expressed in the third person.

Abstract. The abstract (no subheads or paragraphs) should be no more than 250 words (not part of the main word count).

Keywords. These should be included just below the author list (minimum 3 maximum 10).

Abbreviations. Words to be abbreviated should be spelt out in full the first time they appear in the text with the abbreviations in brackets. Thereafter the abbreviation should be used.

References. Please use the APA 6th edition and refer to examples in the full Guidelines. *Authors not complying with these reference guidelines will be asked to make all the necessary alterations themselves, if their paper is accepted for publication*.

Notes. As a general rule, supplementary notes should be avoided, but if thought to be essential, they should *not* appear on the page as footnotes, but instead, be included as endnotes.

Supplementary material. Supplementary material may be uploaded with the submission, and if the paper is published, will be visible to the reader via a link on the RHS of the screen.

Submissions containing graphs, tables, illustrations or mathematics. All graphs, tables and illustrations should be embedded in the submitted text, and have clear, self- explanatory titles and captions. Mathematical expressions should be created in Word 2003 (or a compatible package) with equation editor 3.0, unless the author has good reason to use other software, in which case please contact sjeal@slls.org.uk. All biological measures should be reported in SI units, as appropriate, followed, in the text, by traditional units in parentheses.

Author citation. If the paper is accepted for publication, a version of the paper with all authors cited in full on the title page will be used. Only individuals who have contributed substantially to the production of the paper should be included.

Copy editing. All accepted manuscripts are subject to copy editing, with reference back to author with suggested edits and queries for response.

Proofs. The corresponding author will be asked to view a layout proof of the article on the website and respond with final amendments within three days.

(Full Author Guidelines at: http://www.llcsjournal.org/index.php/llcs/about/submissions#authorGuidelines**)**

Open Journal System

The ***LLCS*** journal is produced using the Open Journal System, which is part of the Public Knowledge Project. OJS is open source software made freely available to journals worldwide, for the purpose of making open access publishing a viable option for more journals. Already, more than 8,000 journals around the world use this software.

INTRODUCTION

PAPERS

STUDY PROFILE

KEYNOTE LECTURES

LLCS Journal can be accessed online at www.llcsjournal.org

Published by the Society for Longitudinal and Life Course Studies
info@slls.org.uk www.slls.org.uk