THE LANCET

Supplementary appendix

Supplement to: Balaj M, York HW, Sripada K, et al. Parental education and inequalities in child mortality: a global systematic review and meta-analysis. *Lancet* 2021; published online June 10. http://dx.doi.org/10.1016/S0140-6736(21)00534-1.

Supplementary Appendix I

Supplementary Methods

1. Sample search string

The following keywords were provided to the research librarian by the review coordinator and adapted to each database:

> Infant mortality, neonatal mortality, child mortality, under-5 mortality, under-15 mortality, under-five mortality, under-fifteen mortality, under five mortality, under-fifteen mortality
>
> Educational status, educational attainment, maternal education, parent’s education, parental education, mother’s education, socio-economic status, socioeconomic status, parental socio-economic status, parent’s socio-economic status, mother’s socio-economic status, father’s socio-economic status, parent’s socioeconomic status, mother’s socioeconomic status, father’s socioeconomic status, maternal socioeconomic status, paternal socioeconomic status, parental socioeconomic status, social class.

Database: Web of Science

Date of the search: 04.02.2019

> TS=(“Infant mortality” or “neonatal mortality” or “child mortality” or “under-5 mortality” or “under-15 mortality” or” under-five mortality” or “under-fifteen mortality” or “under five mortality” or “under-fifteen mortality”)
>
> TS=(“Educational status” or “educational attainment” or “maternal education” or “parent* education” or “mother* education” or “socio-economic status” or “socioeconomic status” or “parent* socio-economic status” or “mother* socio-economic status” or “father* socio-economic status” or “parent* socioeconomic status” or “mother* socioeconomic status” or “father* socioeconomic status” or “maternal socioeconomic status” or “social class” or SES)

These are combined using the “Combine sets” function in Web of Science with the AND Boolean operator.

2. Equations

Equation 1. For interpretability purposes of the model coefficients, we standardized the DHS wealth variable to lie between 0 and 1 using the equation below:

$$I_{hh,i} = \frac{W_{hh,i} + \min(W_{hh})}{\max(W_{hh}) - \min(W_{hh})}$$

Where:

- $I_{hh,i}$ is the standardized wealth metric for household i, scaled to lie between 0 and 1, and
- W_{hh} is a vector of all wealth metrics measurements for a given survey, and
- $W_{hh,i}$ is the unstandardized wealth metric for household i.

Equation 2. For each survey-year-age-span, we ran a Cox proportional hazards model with random effects (frailty model), operationalized as follows:

$$\boldsymbol{ln\left(\frac{\lambda(t)}{\lambda_0(t)}\right)} \\ = \boldsymbol{\beta_1 * Maternal\ Education + \beta_2 * Partner's\ Education + \beta_3 * DHS\ Wealth\ Index} \\ \boldsymbol{+\ \beta_4 * Year\ of\ Birth +\ \beta_5 * Child\ Sex + \beta_6 * Maternal\ Stunting + \alpha_1 + \gamma_1}$$

Where:

- $\lambda(t)$ is the hazard function at time = t,
- $\lambda_0(t)$ is the baseline hazard function at time = t,
- *Maternal Education* is years of education of the mother,
- *Partner's Education* is years of education of the partner (usually the father),
- *DHS Wealth Index* is the continuous DHS wealth index (standardized to lie between 0 and 1),
- *Year of Birth* is the year of birth of the child centered by subtracting 1980,
- *Child Sex* is a binary indicator capturing whether the child is male or female, and
- α_1 is a normally distributed random effect on the first administrative unit,
- and γ_1 is a normally distributed nested random effect on the mother's birth cohort (in decades).

Resulting continuous relative risks corresponding to e^{β_1} or e^{β_2} for the relative risk of maternal or paternal education per one year of education were then used as data points in the meta-analysis.

Equation 3. For the meta-analysis, we included random intercepts for studies, so that multiple effect sizes from the same study did not drive the results unnecessarily. Outlier trimming was set at 10%. The model took the following form:

$$y = \left(X_{alt} - X_{ref}\right) \times (X_{cov}\beta_{cov} + \beta + u)$$

Where:

- y is the log(relative risk),
- X_{alt} and X_{ref} are midpoints for alternative and reference intervals for the exposed and unexposed groups measured in the relative risk effect size
- X_{cov} is a design matrix containing covariates we control for (wealth, urbanicity education, age of the mother, sex and age bin of the child)
- β_{cov} are covariate multipliers associated with X_{cov}
- β gives estimate of effect size (effect of unit of education on log relative risk).
- u is a study-specific random effect.

More information on this model is detailed in Supplementary Appendix II.

3. Displaying Nonstandard Data

For data display purposes, in some of the following figures we represent each effect size as the relative risk or log-space relative risk of under-5 mortality per year of maternal/paternal education. This is necessary as the included studies had inconsistent referent exposure categories and were thus incomparable when viewed in normal, unadjusted space (i.e., one study may report a relative risk with respect to 0 years of education while another may report a relative risk with respect to completed secondary education). This is concurrent with the above treatment of the data by the model if the final model is linear. The abbreviated method for normalizing this data for visualization purposes divides the effect size by the distance between the midpoints of the referent and alternate exposure windows as follows:

$$\frac{Log(RR)}{Year\ of\ Schooling} = \frac{Log(RR)}{(ref_{lower} + ref_{upper} + alt_{lower} + alt_{upper})/4}$$

4. Primary Analyses of DHS Data

As discussed in the main text, we ran independent analyses for n=114 unique DHS surveys across n=58 countries. For each survey, we ran independent models across different age ranges to assess the protective effects of parental education on child survival. We controlled for the most common confounding variables identified in the systematic review – sex of the child, partner’s educational attainment, and wealth –, avoiding variables that lie on the causal pathway when possible. Sensitivity analyses discussed below, however, show that incorporating additional the choices of confounders beyond these three did not have a large impact on our final estimates. These confounders included sex of the child, the partner’s educational attainment, and wealth.

The DHS wealth indicator, an asset-based indicator that is country- and survey- specific, was used as a proxy for household wealth. See Equation 1 above, for standardization of the DHS wealth variable between surveys.

Finally, we also included year of birth as a continuous fixed effect variable to control for agnostic gains in child survival due to the widespread economic development that occurred during this period that varied across countries. We also included nested random effects on level 1 administrative units and the mothers' birth cohort (in decades) to further absorb agnostic average changes in child mortality in due to changing location-based characteristics over time.

For each DHS survey, we ran a set of models which corresponded to the six major under-5 age intervals found in the systematic review. These models never pooled data from different locations or survey years unlike other analyses. The reasons for this are threefold. First, by limiting our reference frame to a single survey iteration, we allow comparability in effect size estimates across countries. If we instead pooled data across locations or within locations, several countries would benefit from effectively increased sample sizes resulting from multiple survey iterations while others would not. Second, by treating each survey iteration as a separate sample, we allow for comparisons within countries across time. Third, by effectively stratifying our dataset by survey iteration, we tacitly control for time-varying contextual factors such as political changes, foreign aid, and development that can change between surveys.
We ran the models identically over six different age groups, four of which are roughly analogous to the most common age bins found in the systematic literature, as discussed below.
For each survey-year-age-span, we ran a Cox proportional hazards model with random effects (frailty models), described in Equation 2 above. Resulting continuous relative risks corresponding to the relative risk of maternal or paternal education per one year of education were then used as data points in the meta-analysis.

5. Sensitivity Analysis I: Demographic and Health Survey Microdata Primary Analyses

As stated in the main paper, numerous studies were identified and excluded because they used Demographic and Health Survey (DHS) data in their analyses. We reasoned that, though our own primary analyses, we could standardize these studies to reduce noise due to variation in study design. However, there remained the question of what means was the best by which to analyze the data. As stated in the paper, we opted for survey-specific models as opposed to pooled models. These models were run separately for each survey and the effect sizes gleaned from them were used in our analysis. All models took the form of a mixed effects Cox proportional hazards model. The models operationalized maternal and paternal education as continuous variables. Included as fixed effects covariates were the DHS wealth index (standardized as explained in the main text), sex of the child, year of birth, and maternal stunting. In order to test the differential impact each of these has on the final effect sizes (the beta coefficients corresponding to maternal and paternal education), we conducted a sensitivity analysis that systematically measured their impact on the model. All models included maternal education and paternal education as fixed effects. We then tested nine other model specifications, one model that included all covariates, four that measured the impact of adding each covariate separately, four that measured the effect of removing each covariate from the fully controlled model. Only the beta coefficients for the main effects, maternal and paternal education, were of interest. See Supplementary Figure 15.

6. Sensitivity Analysis II: Systematic Review + DHS Synthesis and Meta-Analysis

The meta-analyses we undertook synthesized data from a systematic review and Demographic and Health Surveys microdata. Owing to the nature of these two data sources, we used a sensitivity analysis to see if any compositional biases in these data adversely affected our results. The meta-analytical models mentioned in the main text of this article were rerun using three data specifications: (1) the original data that combines both systematic review data and results from our own microdata analyses, (2) only the results from the DHS microdata analyses, and (3) only the input data identified by the systematic review. In our main text, we describe several sources that were identified in the systematic review that were the result of analyses of DHS data by other parties. As we describe in the main text, all three sensitivity analyses scenarios continued not to use those data. In the case of scenario 2, which only uses results from our analyses of DHS microdata, the metaanalysis is run with no covariates. This is because there is no need to adjust for study-level covariates because all studies in this meta-analytical task are already optimally controlled. A model specification that included these covariates would have no contrast in the covariate values across the rows of data, resulting in a failed model. To judge to comparability of these scenarios, we compare the same quantities displayed in Figure 4b of the main text, notably, the logged relative risk per each additional year of paternal or maternal education across multiple age ranges. See Supplementary Figure 16.

7. Supplementary Results, Tables & Figures

Supplementary Figure 1 shows the normalized effect sizes extracted from the systematic review and the midpoints of the exposure and referent categories for each extracted effect size as an approximation of the instantaneous slope of the relative risk curve implied by each extracted effect size. This is used as a means of examining the linearity of the dose-response relationship between parental education and under-5 mortality when superimposed with the aforementioned average effect sizes. While such a task is complicated by each datum having been approximated with a different set of confounders, visual inspection is still possible so as to determine the linearity and monotonicity of the fitted relative risk curve. Across all age intervals and both parent genders, linearity of the slopes of the effect sizes is apparent.

Additionally, Supplementary Figure 1 provides evidence for the monotonicity of the dose-response relationship between parental education and child mortality (i.e. the slope of the relative risk curve is negative across the entire exposure range). While some studies have shown significant payoffs of primary schooling compared to no schooling but have shown insignificant effects of secondary schooling as compared to primary schooling (citations pulled from systematic review), these results compel us to believe that there is, in aggregate, no evidence for a decreasing marginal utility of increased maternal or paternal education.

Supplementary Figures 3 through 14 display normalized relative risks per one year of parental education, shown for all study data separately by parent's sex, child's age, and systematic review or DHS data. Color indicates inlier/outlier. Full study titles are provided in the supplementary spreadsheet. Axis labels are intentionally small. These figures serve two purposes, one of which is to simply provide an overview of data availability and effect sizes. The second is to allow curious readers to cross reference specific studies to see how their measured effect sizes compare to other studies' effect sizes.

Supplementary Table 1

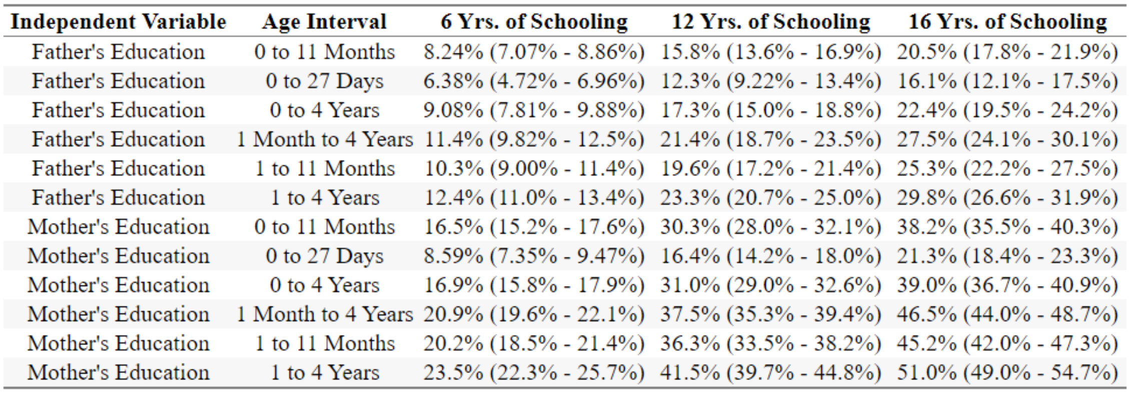
Survey characteristics for DHS data

Live Births and Deaths by Country

Location	Number of Surveys	Total Mothers	Total Live Births	Deaths 0-1 Month	Deaths 1-12 Months	Deaths 12-60 Months
Albania	2	11,725	28,528	298	305	69
Armenia	2	5,422	11,546	242	192	51
Azerbaijan	1	5,001	12,936	422	431	91
Bangladesh	4	51,301	151,308	8,936	3,360	3,390
Benin	3	27,467	114,279	4,408	4,060	4,651
Bolivia	1	10,906	41,882	2,232	1,595	1,100
Burkina Faso	2	15,332	67,133	3,268	3,972	4,538
Burundi	1	2,766	11,724	549	523	581
Cambodia	3	18,543	59,322	2,663	2,096	1,061
Cameroon	2	8,204	33,086	1,267	1,418	1,536
Chad	2	12,031	56,917	2,253	2,732	3,196
Colombia	2	51,697	142,927	2,548	1,651	717
Comoros	1	2,671	10,565	314	134	105
Congo (Brazzaville)	2	7,783	28,147	896	990	884
Côte d'Ivoire	1	2,949	12,121	664	516	404
DR Congo	1	3,083	13,157	567	788	617
Egypt	3	51,618	167,847	5,036	3,928	1,540
Ethiopia	3	23,130	97,912	5,302	3,981	3,861
Ghana	3	9,332	35,295	1,508	1,225	1,069
Guatemala	1	15,201	51,177	1,262	828	449
Guinea	3	9,125	40,741	2,221	2,141	2,192
Guyana	1	2,216	6,994	180	109	29
Haiti	3	12,505	45,058	1,718	1,537	1,302
Honduras	2	27,740	95,071	2,155	1,403	989
India	1	80,319	243,975	12,512	5,648	4,299
Jordan	3	17,156	69,717	1,064	519	175
Kenya	3	20,311	78,278	2,583	2,430	1,667
Kyrgyzstan	1	5,531	16,004	334	256	52
Lesotho	3	5,849	17,175	781	513	251
Liberia	1	4,676	19,865	993	1,684	755
Madagascar	2	10,459	39,250	1,335	1,429	1,144
Malawi	3	19,118	74,200	3,329	4,180	3,221
Maldives	2	7,368	23,043	630	269	152
Mali	2	14,157	65,340	4,056	3,836	4,683
Moldova	1	4,745	9,560	175	106	49
Morocco	1	8,433	31,656	1,497	864	359
Mozambique	2	17,698	67,869	3,717	4,413	2,712
Myanmar	1	7,533	22,274	1,065	666	400
Namibia	2	5,312	18,093	477	466	247
Nepal	2	11,948	38,862	2,186	1,129	921
Niger	2	7,524	36,547	1,603	2,382	2,648
Nigeria	4	61,537	279,227	14,008	14,194	17,757
Pakistan	2	7,924	34,780	1,785	788	385
Peru	5	77,609	237,818	6,440	4,579	3,043
Rwanda	3	10,962	43,913	1,935	1,904	2,188
Senegal	2	6,120	25,867	1,174	927	1,168
Sierra Leone	1	2,540	9,648	541	742	402
South Africa	1	949	2,611	78	59	24
São Tomé and PrÍncipe	1	1,937	7,402	149	267	161
Tajikistan	1	6,126	19,784	481	585	175
Tanzania	3	21,525	88,717	3,340	3,660	2,872
Timor-Leste	1	7,820	35,370	1,472	1,408	801
Togo	1	3,277	12,769	486	383	538
Turkey	2	8,750	24,889	686	503	158
Uganda	2	3,785	17,914	751	996	746
Zambia	1	4,710	19,783	804	1,068	796
Zimbabwe	2	11,729	35,889	950	837	566
eSwatini	1	2,211	8,712	232	352	167
Totals						
58	**114**	**875,396**	**3,112,474**	**124,558**	**103,957**	**90,104**

Supplementary Table 2

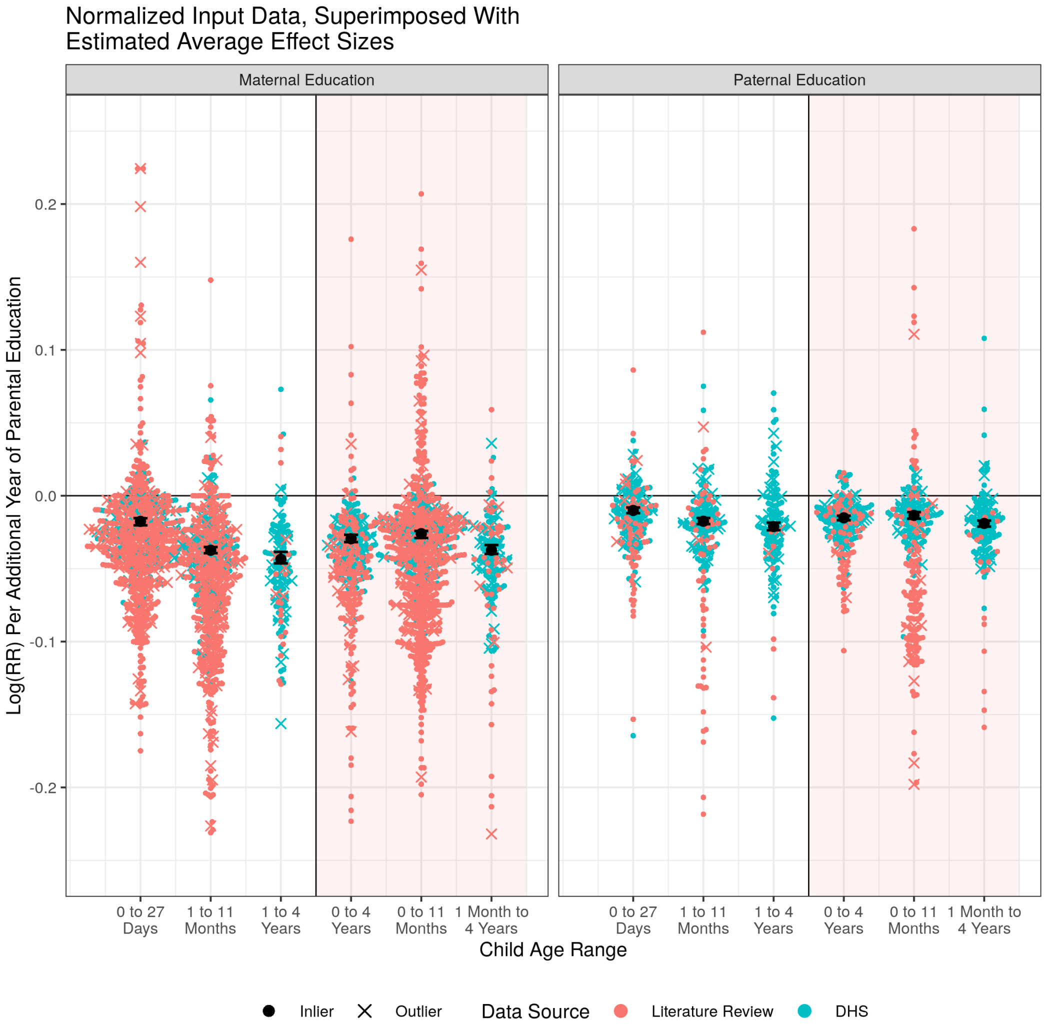
Percent reduction in childhood mortality across age intervals by parent’s gender (all effect sizes use a reference group of 0 years of parental education).



Independent Variable	Age Interval	6 Yrs. of Schooling	12 Yrs. of Schooling	16 Yrs. of Schooling
Father's Education	0 to 11 Months	8.24% (7.07% - 8.86%)	15.8% (13.6% - 16.9%)	20.5% (17.8% - 21.9%)
Father's Education	0 to 27 Days	6.38% (4.72% - 6.96%)	12.3% (9.22% - 13.4%)	16.1% (12.1% - 17.5%)
Father's Education	0 to 4 Years	9.08% (7.81% - 9.88%)	17.3% (15.0% - 18.8%)	22.4% (19.5% - 24.2%)
Father's Education	1 Month to 4 Years	11.4% (9.82% - 12.5%)	21.4% (18.7% - 23.5%)	27.5% (24.1% - 30.1%)
Father's Education	1 to 11 Months	10.3% (9.00% - 11.4%)	19.6% (17.2% - 21.4%)	25.3% (22.2% - 27.5%)
Father's Education	1 to 4 Years	12.4% (11.0% - 13.4%)	23.3% (20.7% - 25.0%)	29.8% (26.6% - 31.9%)
Mother's Education	0 to 11 Months	16.5% (15.2% - 17.6%)	30.3% (28.0% - 32.1%)	38.2% (35.5% - 40.3%)
Mother's Education	0 to 27 Days	8.59% (7.35% - 9.47%)	16.4% (14.2% - 18.0%)	21.3% (18.4% - 23.3%)
Mother's Education	0 to 4 Years	16.9% (15.8% - 17.9%)	31.0% (29.0% - 32.6%)	39.0% (36.7% - 40.9%)
Mother's Education	1 Month to 4 Years	20.9% (19.6% - 22.1%)	37.5% (35.3% - 39.4%)	46.5% (44.0% - 48.7%)
Mother's Education	1 to 11 Months	20.2% (18.5% - 21.4%)	36.3% (33.5% - 38.2%)	45.2% (42.0% - 47.3%)
Mother's Education	1 to 4 Years	23.5% (22.3% - 25.7%)	41.5% (39.7% - 44.8%)	51.0% (49.0% - 54.7%)

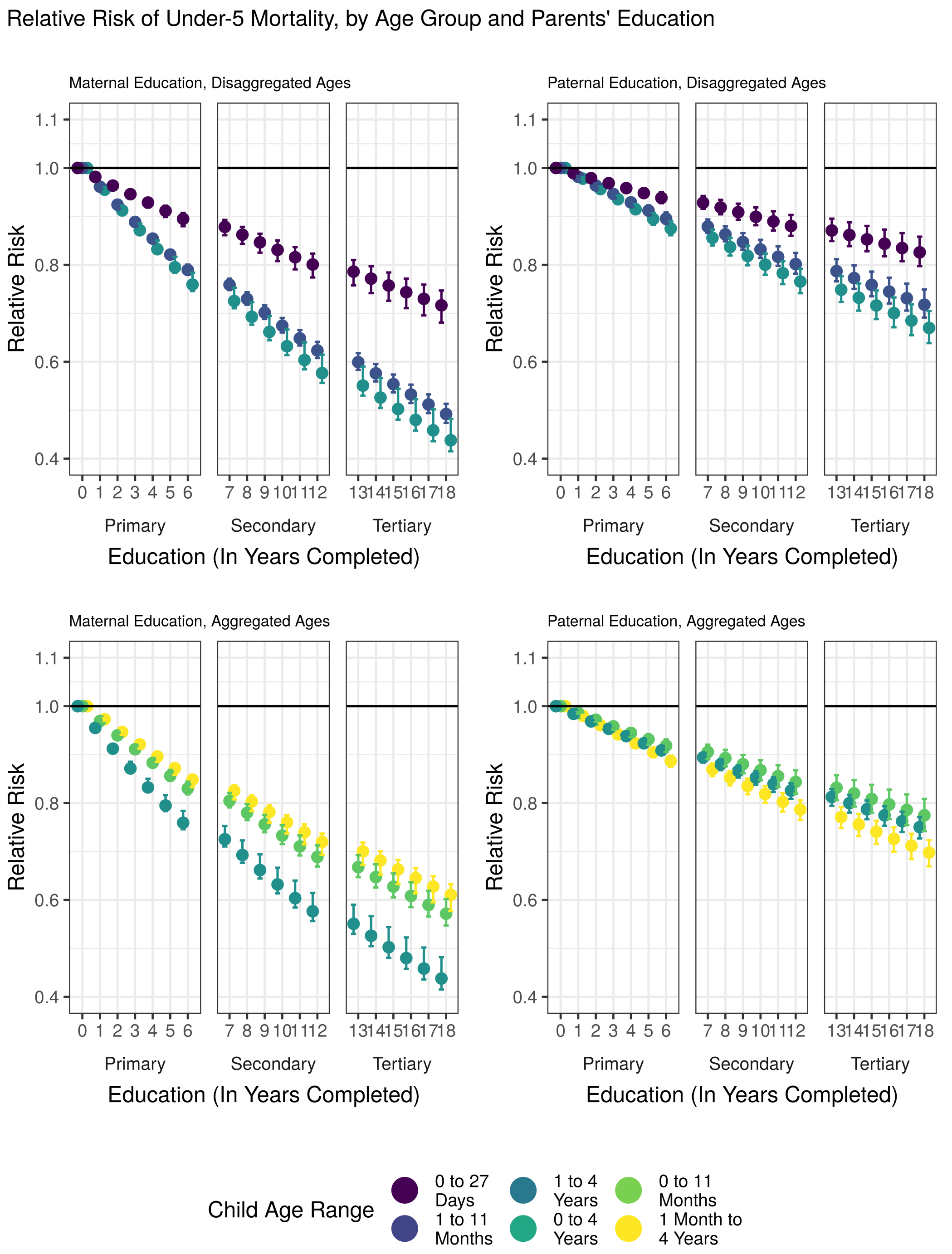
Supplementary Figure 1

Normalized input data, superimposed with estimated average effect sizes



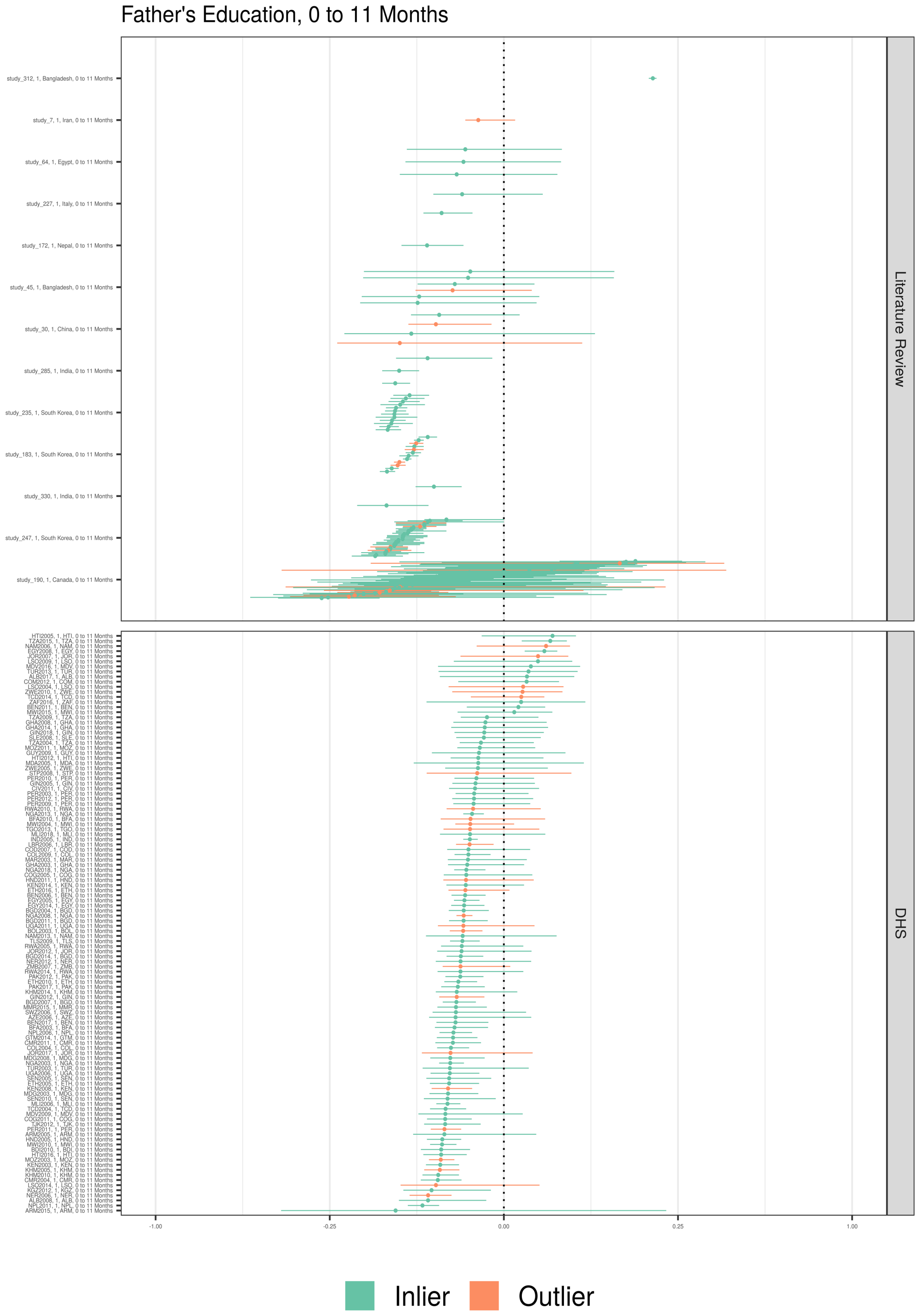
Supplementary Figure 2

Relative risk of under-5 mortality, by age group and parents' education

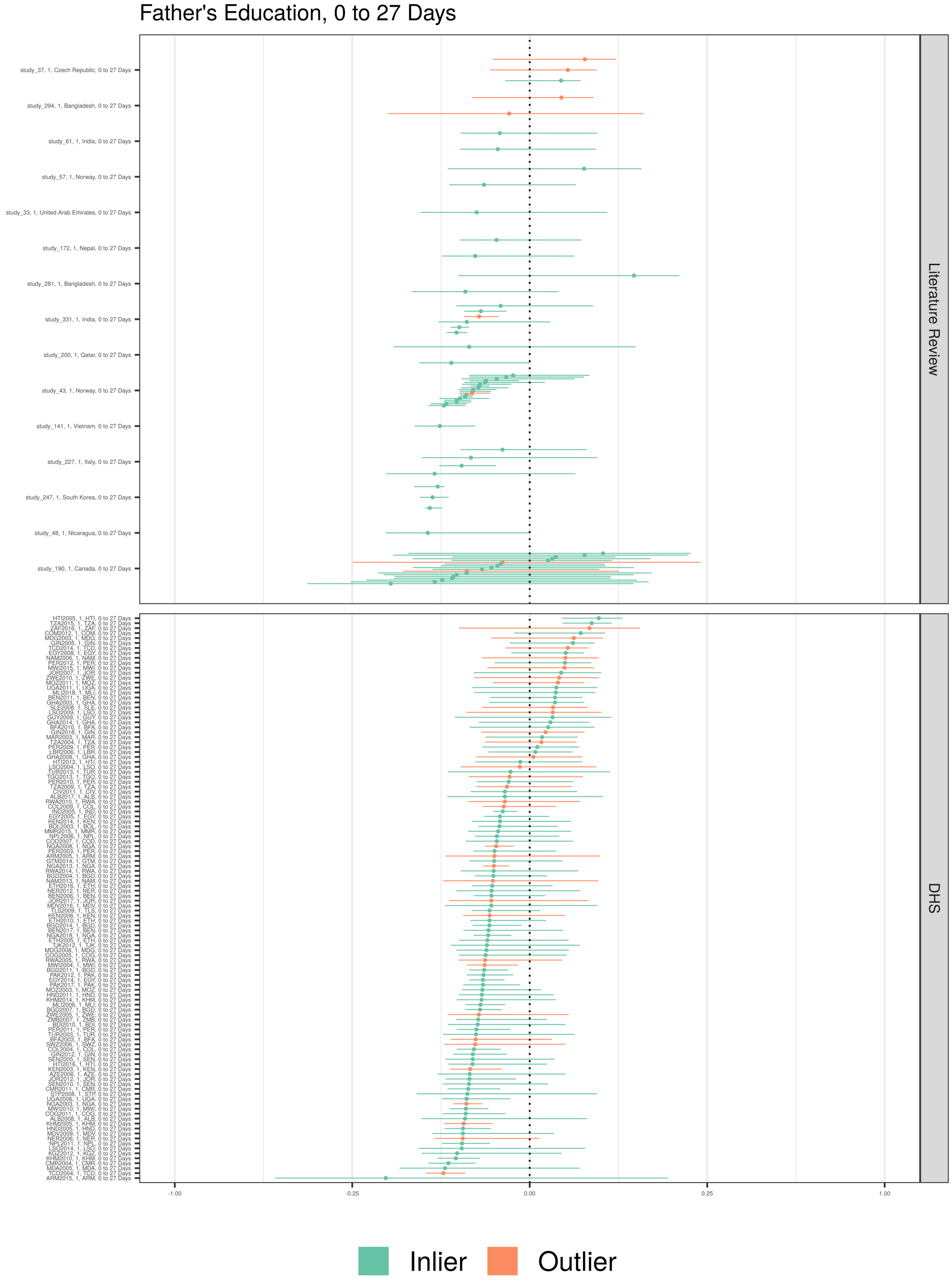


Supplementary Figure 3

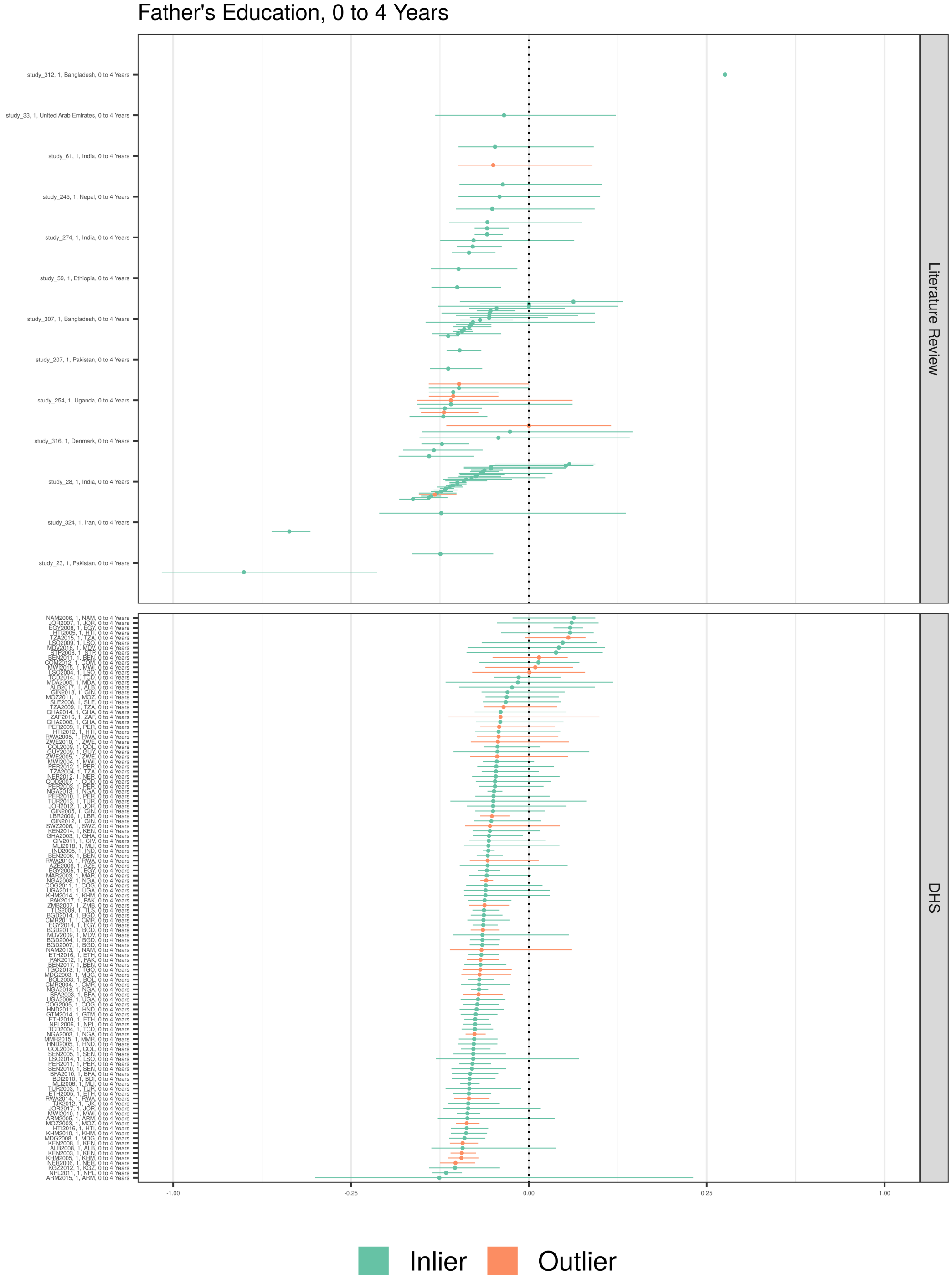
Normalized relative risks per one year of parental education, by age group, parent’s education, and data source.



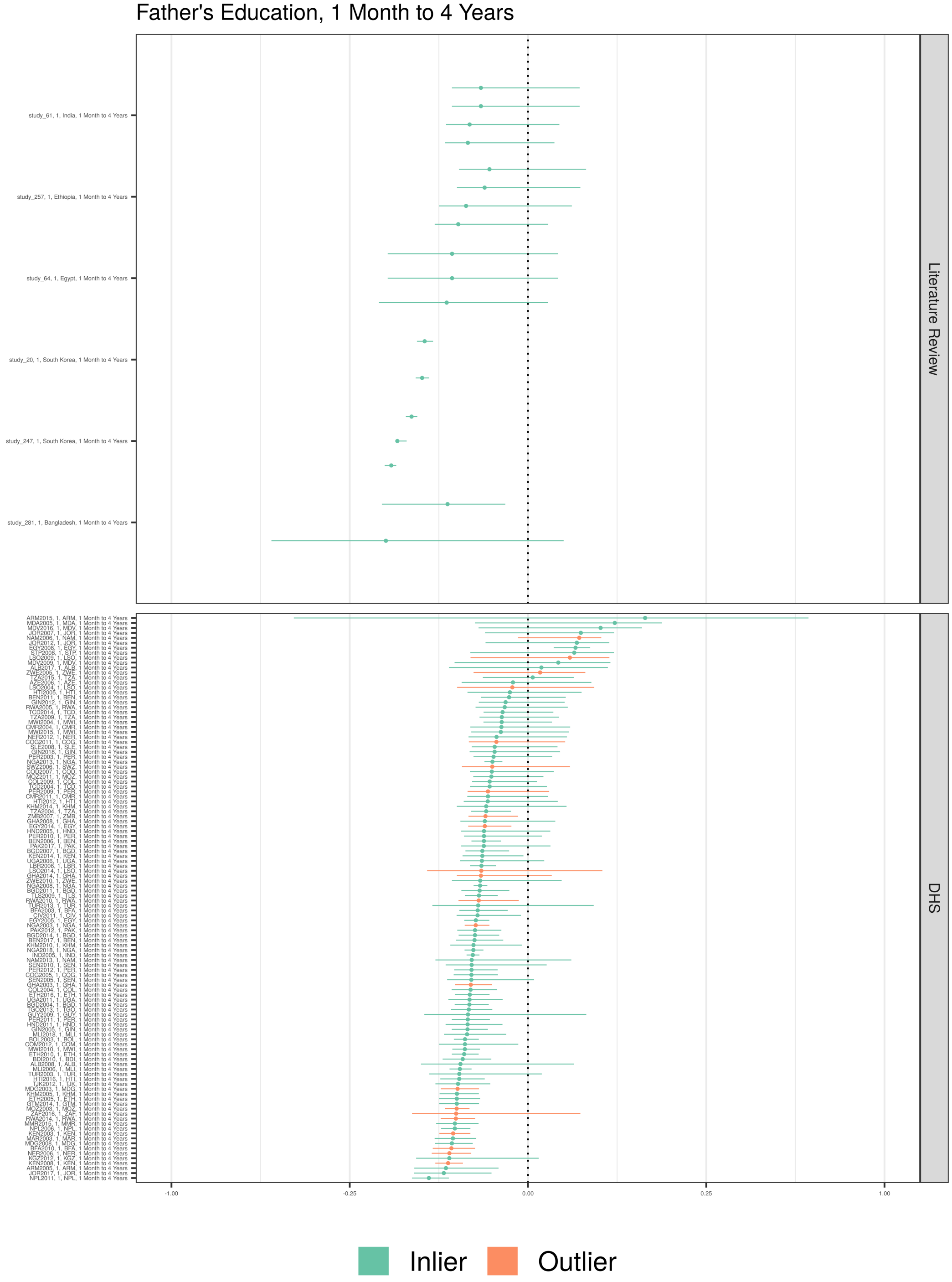
Supplementary Figure 4



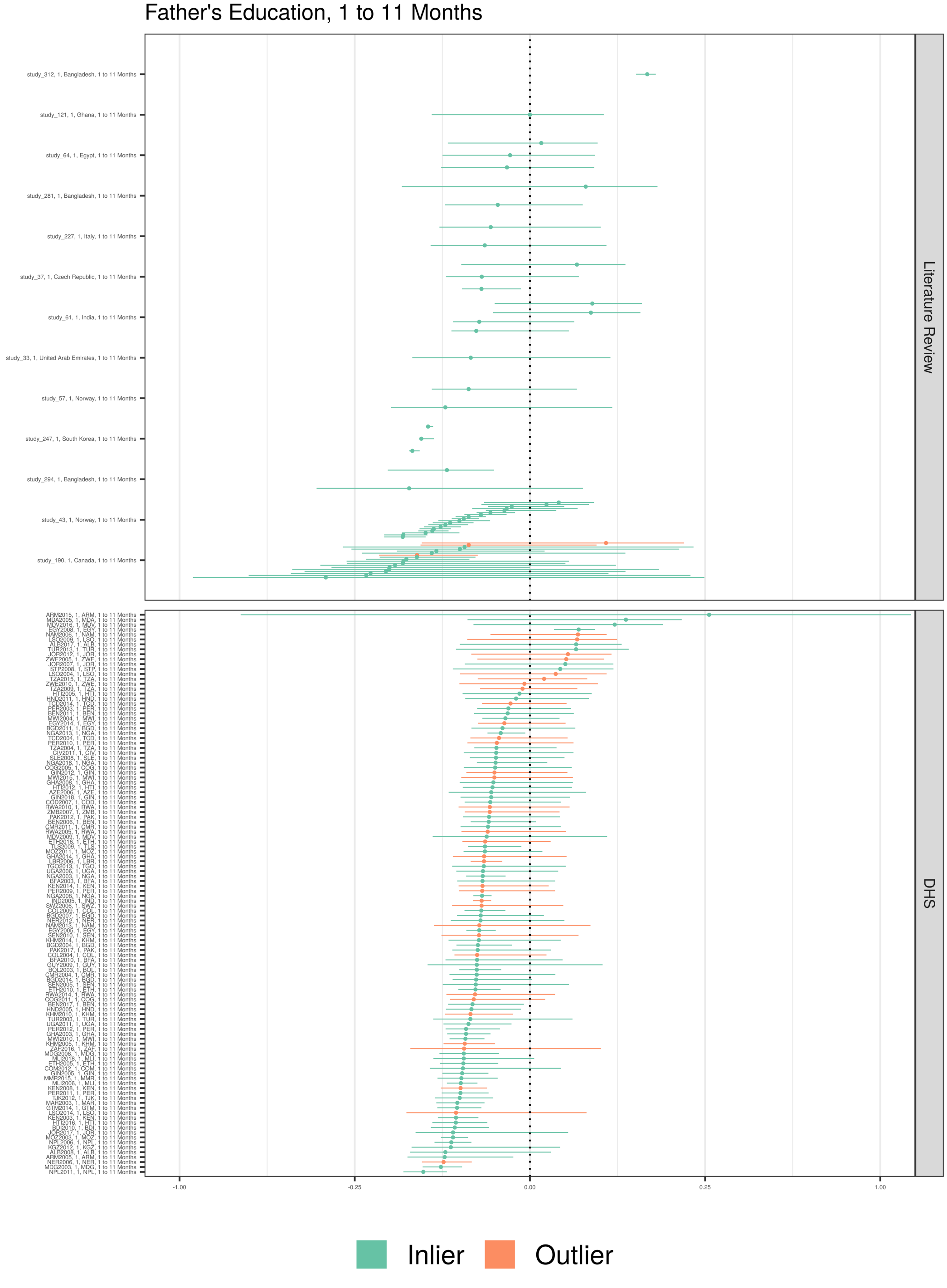
Supplementary Figure 5



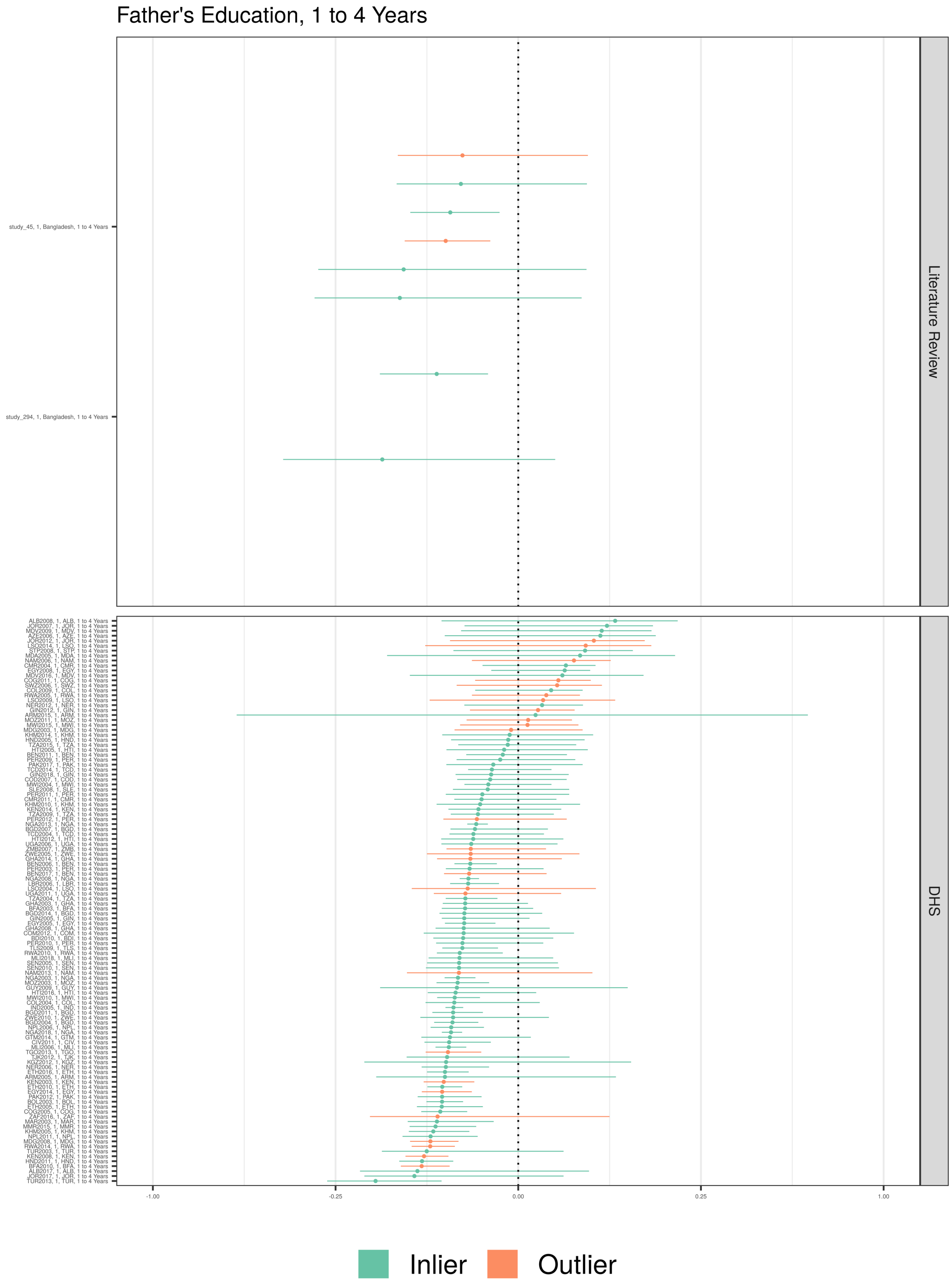
Supplementary Figure 6



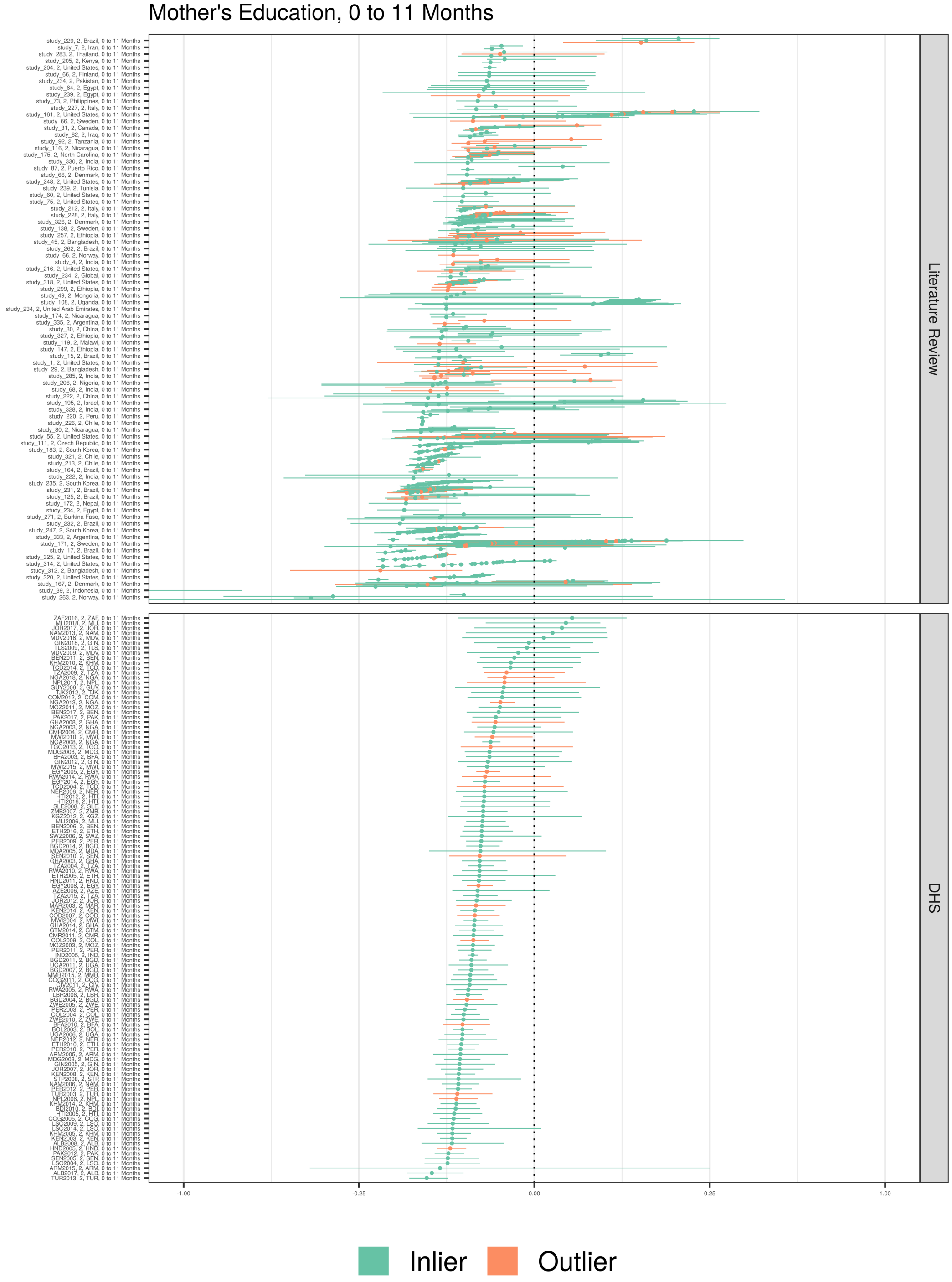
Supplementary Figure 7



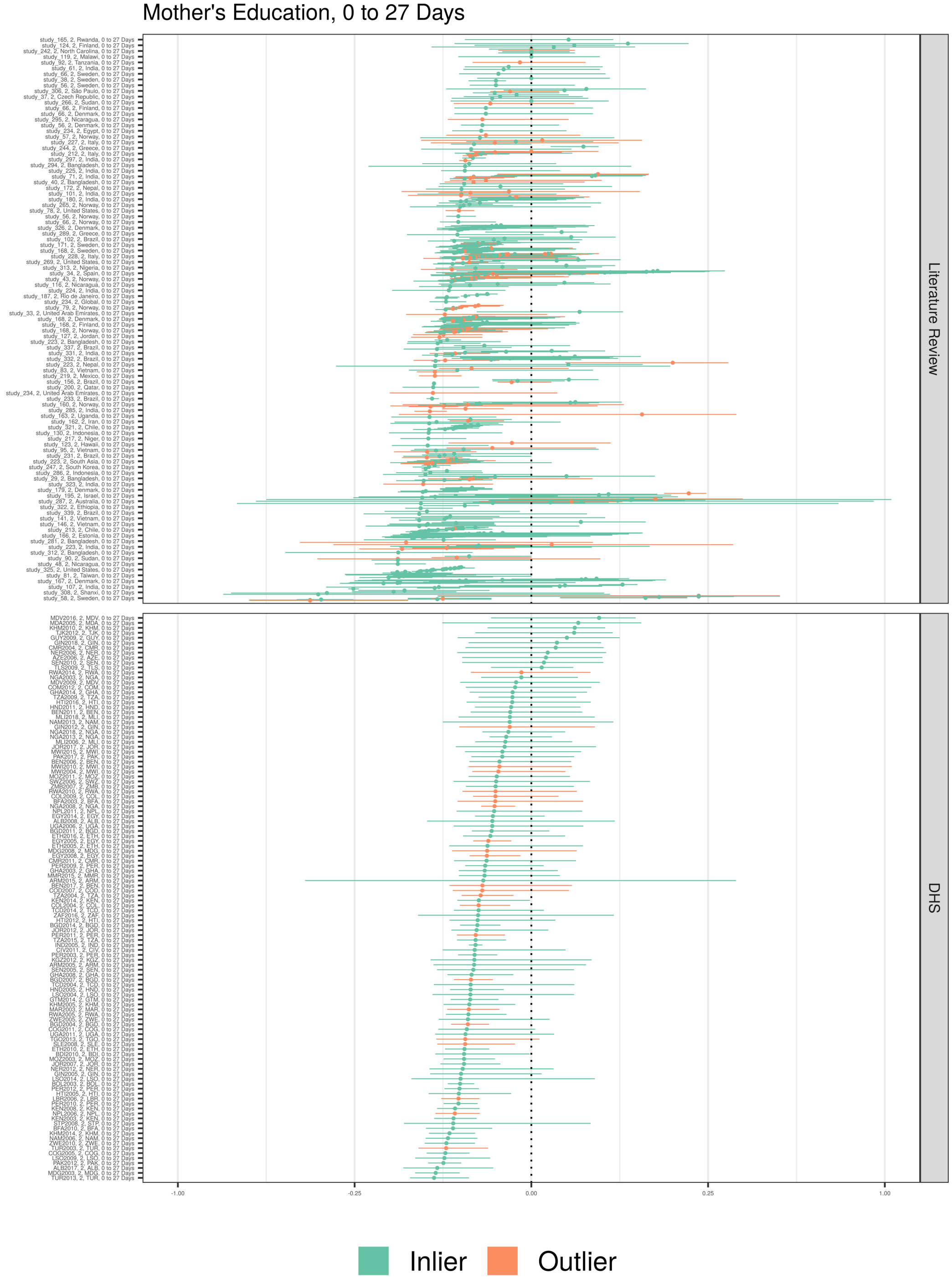
Supplementary Figure 8



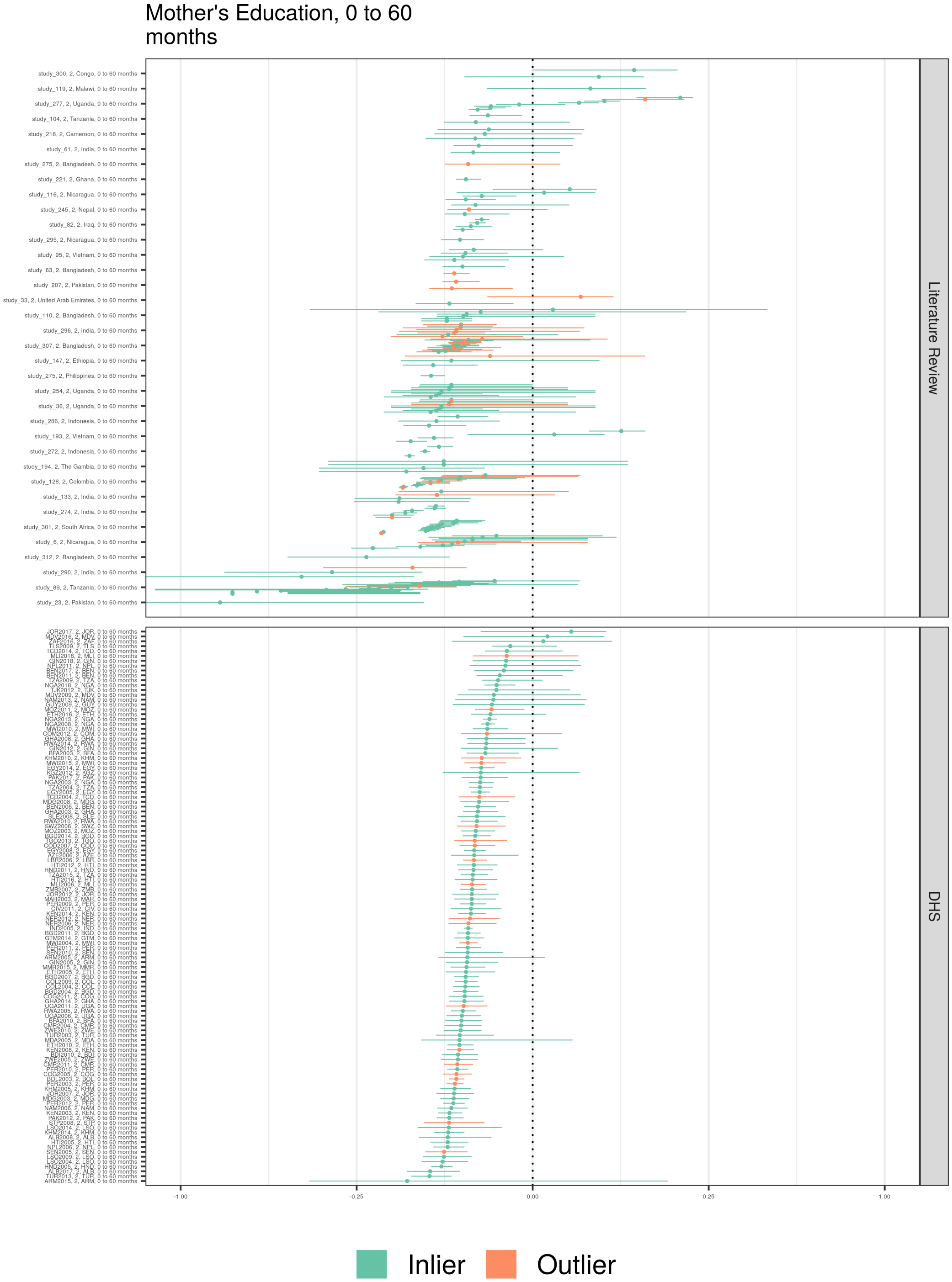
Supplementary Figure 9



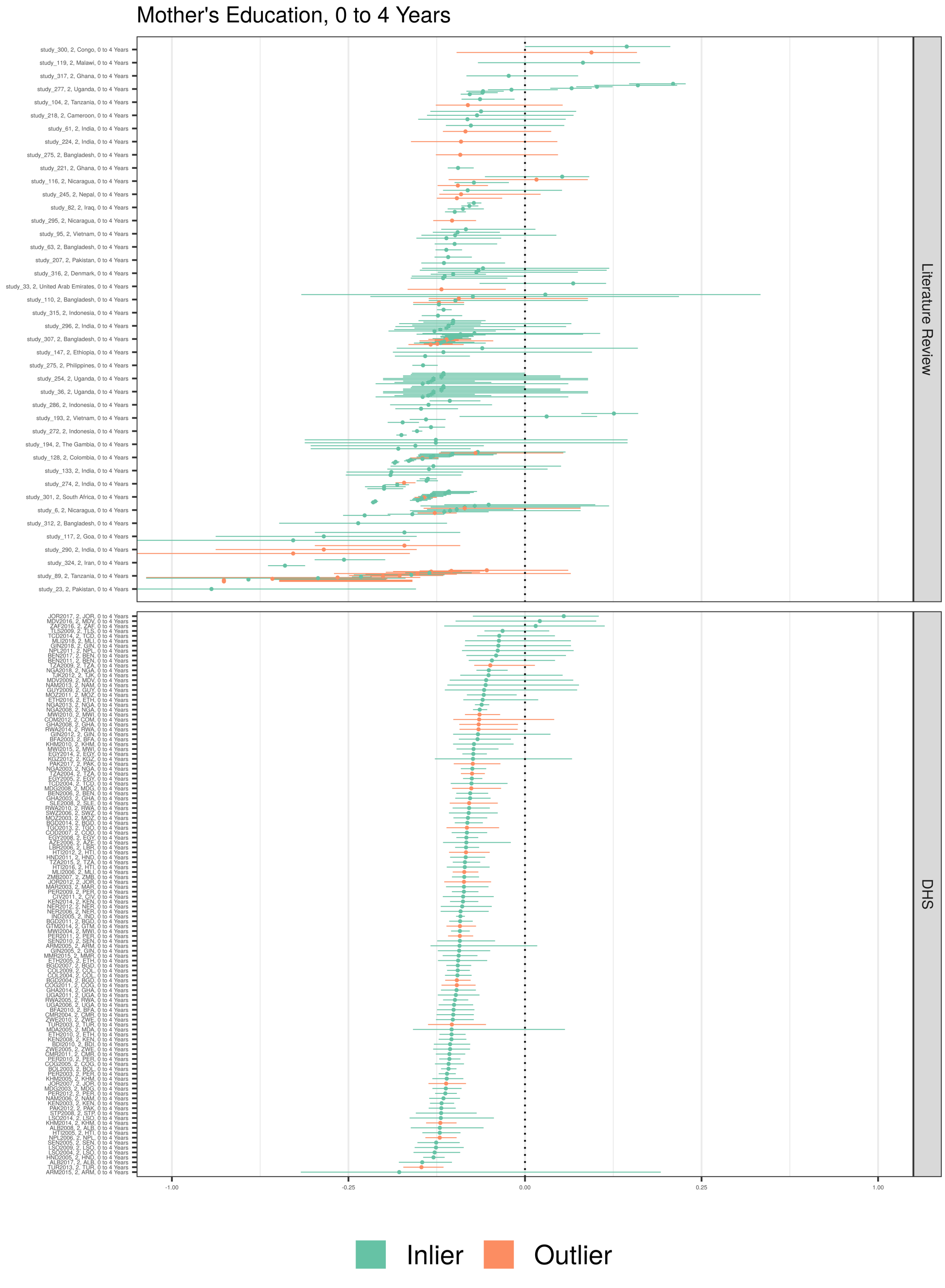
Supplementary Figure 10



Supplementary Figure 11

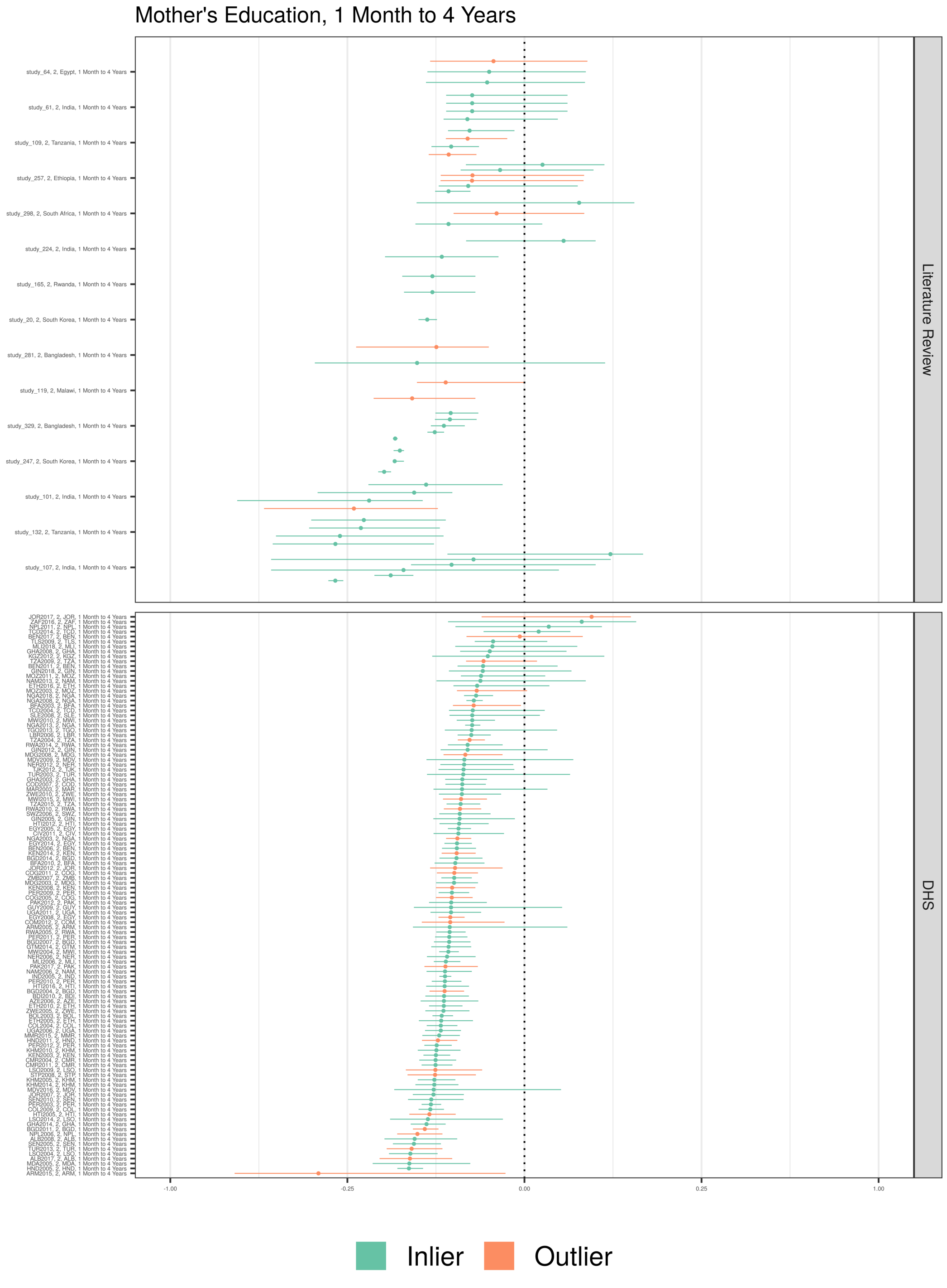


Supplementary Figure 12

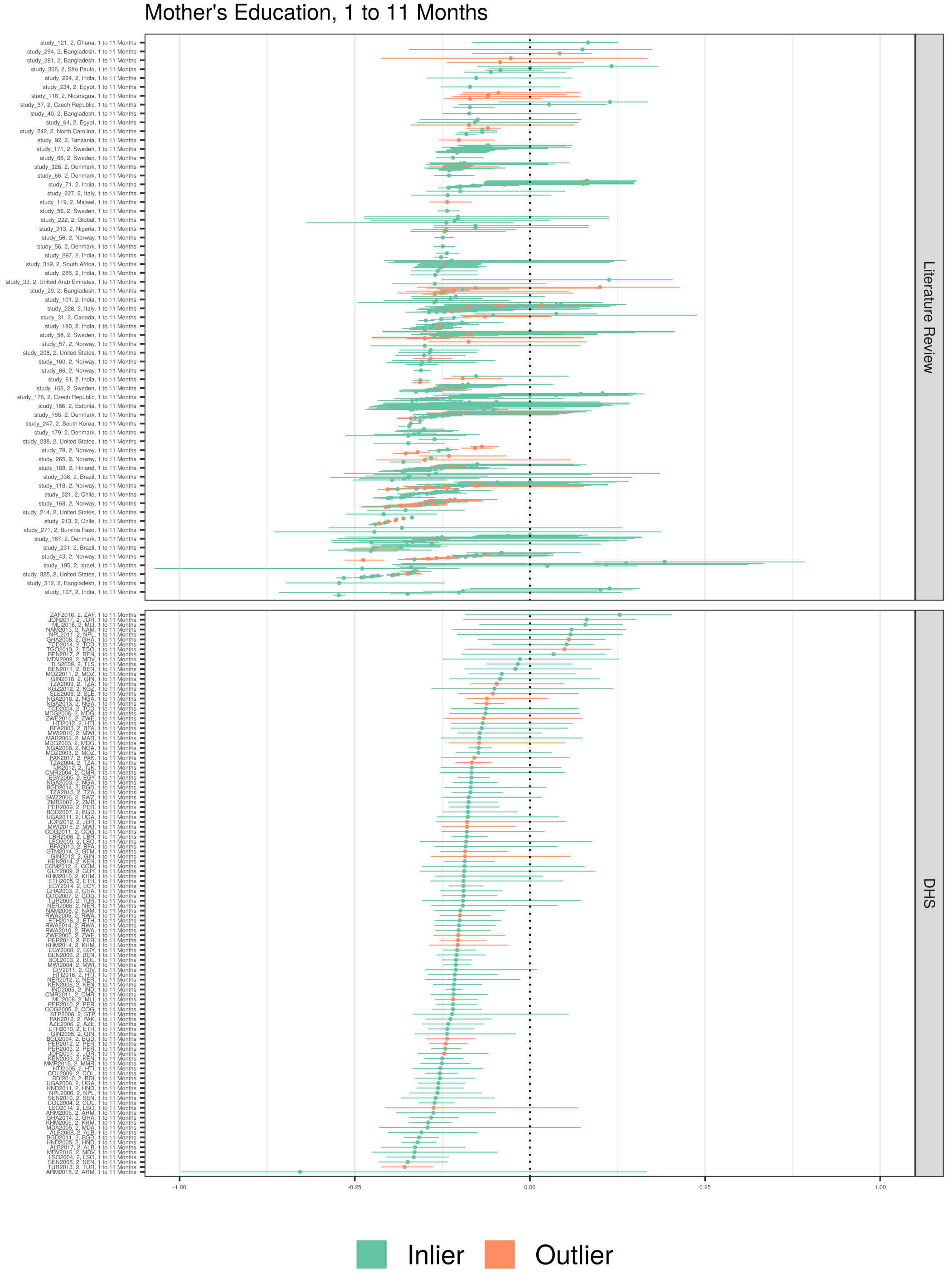


Mother's Education, 0 to 4 Years

Supplementary Figure 13

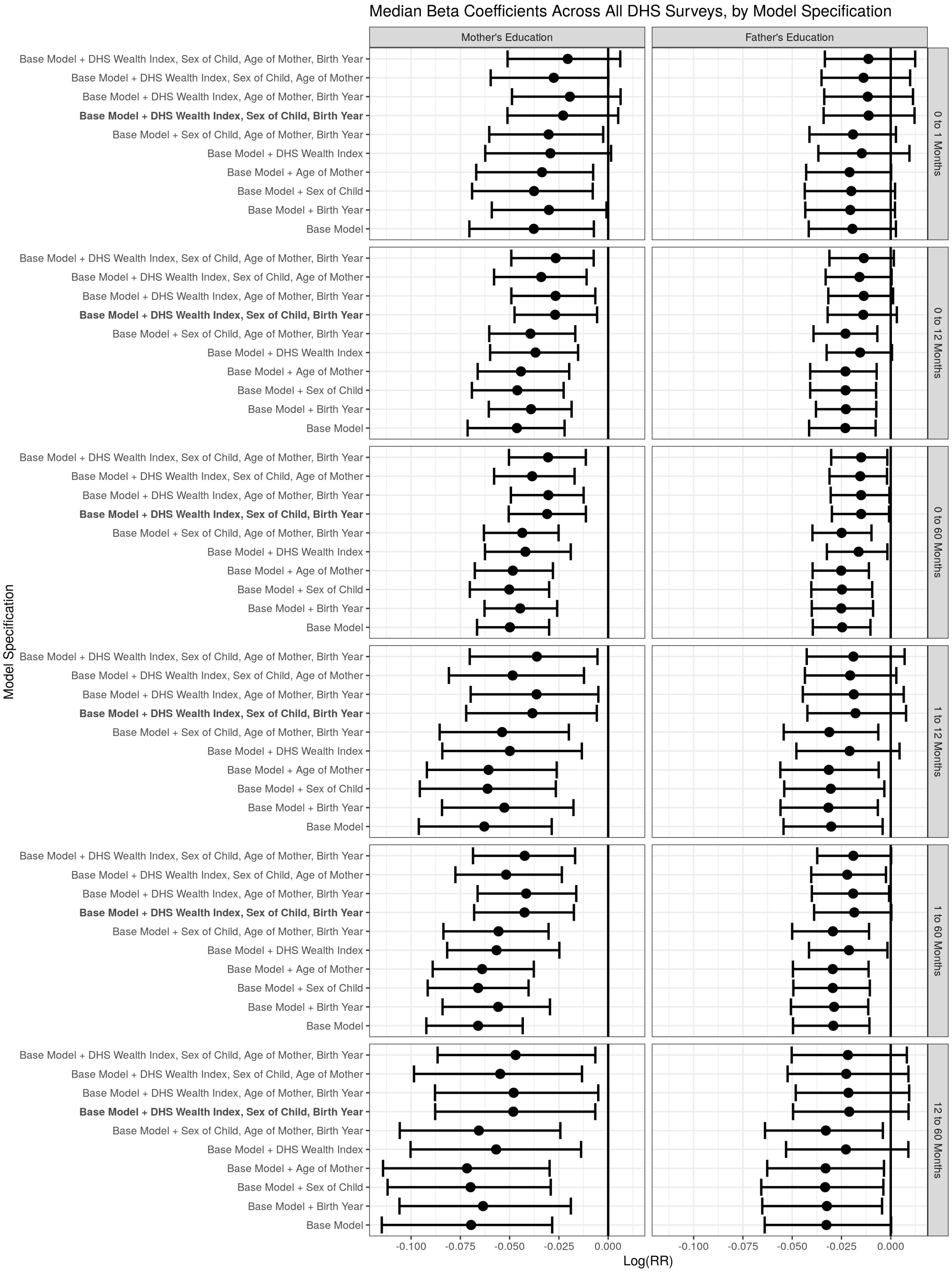


Supplementary Figure 14



Supplementary Figure 15

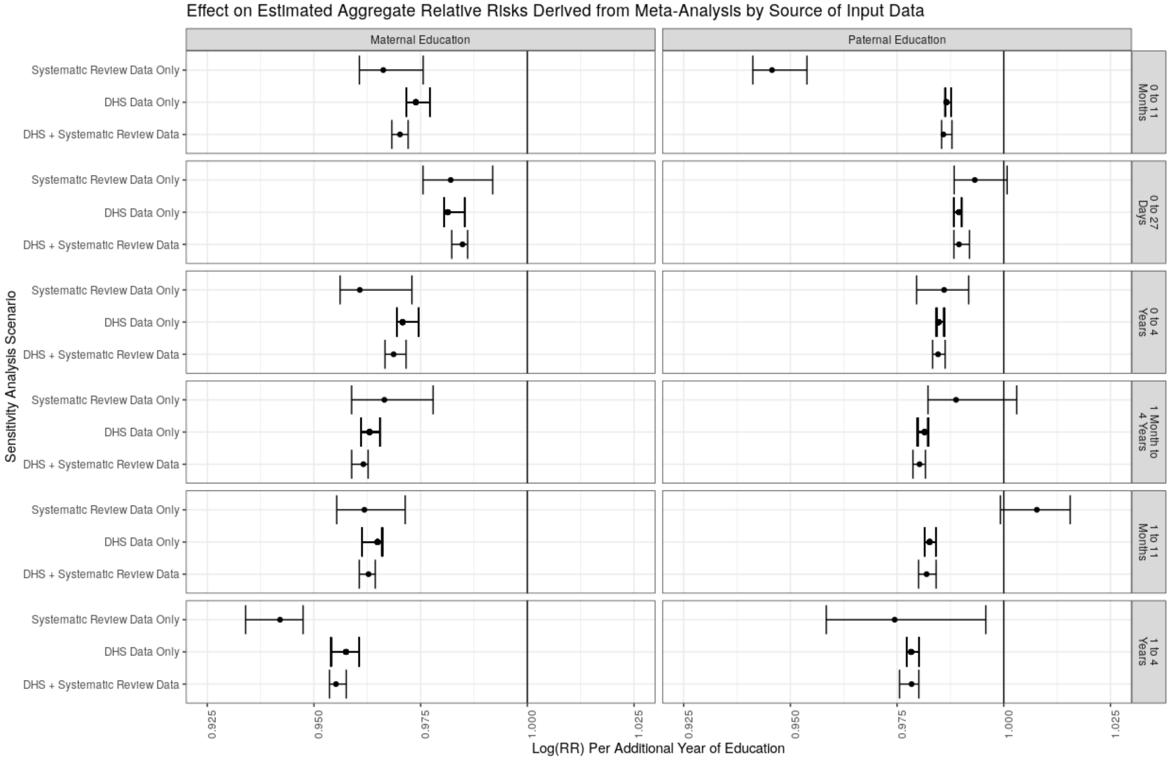
Sensitivity analysis of DHS Microdata Primary Analyses



Supplementary Figure 16

Sensitivity analysis of Systematic Review + DHS Synthesis and Meta-Analysis

'Log(RR) Per Additional Year of Parental Education' is identical to the beta value estimated by the model.



8. List of articles included in systematic review

1) Adetunji JA. Infant mortality in Nigeria: effects of place of birth, mother's education and region of residence. Journal of Biosocial Science. 1994;26(4):469-77.
2) Adeyinka DA, Muhajarine N, Petrucka P, Isaac EW. Inequities in child survival in Nigerian communities during the Sustainable Development Goal era: insights from analysis of 2016/2017 Multiple Indicator Cluster Survey. BMC Public Health. 2020 Oct 27;20(1):1613.
3) Adlakha AL, Suchindran CM. Factors affecting infant and child mortality. Journal of Biosocial Science. 1985;17(4):481-96.
4) Agha A, Ajmal F, Iqbal A, White F. Father's support and literacy--factors associated with child mortality in Gambat, Sindh-Pakistan. Journal of the Pakistan Medical Association. 2010;60(2):81.
5) Aguilera X, Delgado I, Icaza G, Apablaza M, Villanueva L, Castillo-Laborde C. Under five and infant mortality in Chile (1990-2016): Trends, disparities, and causes of death. PLoS One. 2020 Sep 30;15(9):e0239974.
6) Akter T, Hoque DME, Chowdhury EK, Rahman M, Russell M, Arifeen S. Is there any association between parental education and child mortality? A study in a rural area of Bangladesh. Public health. 2015;129(12):1602-9.
7) Al Hosani, H.A., Brebner, J., Bener, A.B. & Norman, J.N. Study of mortality risk factors for children under age 5 in Abu Dhabi. Eastern Mediterranean health journal. 2003;9(3):333.
8) Alam N, Van Ginneken JK, Bosch AM. Infant mortality among twins and triplets in rural Bangladesh in 1975–2002. Tropical Medicine & International Health. 2007;12(12):1506-14.
9) Alam N. Teenage motherhood and infant mortality in Bangladesh: maternal age-dependent effect of parity one. Journal of biosocial science. 2000;32(2):229-36.
10) Aleman J, Liljestrand J, Peña R, Wall S, Persson L. Which Babies Die during the First Week? Gynecologic and obstetric investigation. 1997;43(2):112-5.
11) Alexander GR, Baruffi G, Mor JM, Kieffer E. Pregnancy outcomes among Whites and Filipinos: A paradoxical birth weight-neonatal mortality relationship. Am J Hum Biol. 1993;5(2):203-209.
12) Ali MM, Shah IH. Sanctions and childhood mortality in Iraq. The lancet. 2000;355(9218):1851-7.
13) Amin S. The effect of women's status on sex differentials in infant and child mortality in South Asia. Genus. 1990 Jul-Dec;46(3-4):55-69.
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9. MR-BRT Methods for Parental Education Meta-Analysis.

This Appendix details the statistical model and fitting procedure used in the analysis. The article [4] has a more complete mathematical specification of the model.

(1) Mixed-Effects Model

We consider the following basic nonlinear mixed effects model:

$$\begin{aligned} \boldsymbol{y}_i &= \mathbf{X}_i\boldsymbol{\beta} + \mathbf{Z}_i\boldsymbol{u}_i + \boldsymbol{\epsilon}_i \\ \boldsymbol{u}_i &\sim N(\mathbf{0}, \boldsymbol{\Gamma}), \quad \boldsymbol{\Gamma} = \operatorname{diag}(\boldsymbol{\gamma}), \quad \boldsymbol{\epsilon}_i \sim N(\mathbf{0}, \boldsymbol{\Lambda}), \end{aligned} \tag{1}$$

where $\boldsymbol{y}_i \in \mathbb{R}^{n_i}$ is the vector of observations from the ith study, $\boldsymbol{\epsilon}_i \in \mathbb{R}^{n_i}$ are measurement errors with given covariance $\boldsymbol{\Lambda}$, $\boldsymbol{u}_i \in \mathbb{R}^{k_\gamma}$ are independent random effects, and $\mathbf{Z}_i \in \mathbb{R}^{n_i \times k_\gamma}$ is a linear map, and $\boldsymbol{\beta}$ are regression coefficients. The models F_i may be nonlinear.

To fit $(\boldsymbol{\beta}, \boldsymbol{\gamma})$ we solve the marginal likelihood problem

$$\min_{\boldsymbol{\beta},\boldsymbol{\gamma}} f(\boldsymbol{\beta},\boldsymbol{\gamma}) := \sum_{i=1}^{m} \frac{1}{2}(\boldsymbol{y}_i - \mathbf{X}_i\boldsymbol{\beta})^\top(\mathbf{Z}_i\boldsymbol{\Gamma}\mathbf{Z}_i^\top + \boldsymbol{\Lambda}_i)^{-1}(\boldsymbol{y}_i - \mathbf{X}_i\boldsymbol{\beta}) + \frac{1}{2}\ln|\mathbf{Z}_i\boldsymbol{\Gamma}\mathbf{Z}_i^\top + \boldsymbol{\Lambda}_i|. \tag{2}$$

(2) Trimming outliers.

Least trimmed squares (LTS) is a robust estimator proposed by [2, 3] for the standard regression problem. Given the problem

$$\min_{\boldsymbol{\beta}} \sum_{i=1}^{n} \frac{1}{2}(y_i - \langle \mathbf{X}_i, \boldsymbol{\beta} \rangle)^2, \tag{3}$$

the LTS estimator minimizes the sum of *smallest* h residuals rather than all residuals. These estimators were initially introduced to develop linear regression estimators that have a high breakdown point (in this case 50%) and good statistical efficiency (in this case $n^{-1/2}$).[1] LTS estimators are robust against outliers, and arbitrarily large deviations that are trimmed do not affect the final $\hat{\boldsymbol{\beta}}$.

Rather than writing the objective in terms of order statistics, it is far simpler to extend the likelihood using an auxiliary variable $\mathbf{W}$:

$$\min_{\boldsymbol{\beta},\mathbf{W}} \sum_{i=1}^{n} w_i \left(\frac{1}{2}(y_i - \langle \mathbf{X}_i, \boldsymbol{\beta} \rangle)^2 \right) \quad \text{s.t.} \quad \mathbf{1}^\top\mathbf{W} = h, \quad \mathbf{0} \le \mathbf{W} \le \mathbf{1}. \tag{4}$$

The set

$$\Delta_h := \left\{ \mathbf{W} : \mathbf{1}^\top\mathbf{W} = h, \quad \mathbf{0} \le \mathbf{W} \le \mathbf{1} \right\} \tag{5}$$

is known as the *capped simplex*, since it is the intersection of the h-simplex with the unit box (see [1] for details). For a fixed $\boldsymbol{\beta}$, the optimal solution of (4) with respect to $\mathbf{W}$ assigns weight 1 to each of the smallest h residuals, and 0 to the rest. Problem (4) is solved *jointly* in $(\boldsymbol{\beta}, \mathbf{W})$, simultaneously finding the regression estimate and classifying the observations into inliers and outliers. This joint strategy

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[1] Breakdown refers to the percentage of outlying points which can be added to a dataset before the resulting M-estimator can change in an unbounded way. Here, outliers can affect both the outcomes and training data (features).

makes LTS different from post-hoc analysis, where a model a fit first with all data, and then outliers are detected using that estimate.

To explain how trimming enters the marginal likelihood problem, we focus on a single group term from the ML likelihood (2):

$$\left(\frac{1}{2}(\boldsymbol{y}_i - \mathbf{X}_i\boldsymbol{\beta})^\top(\mathbf{Z}_i\boldsymbol{\Gamma}^{-1}\mathbf{Z}_i^\top + \boldsymbol{\Lambda}_i)^{-1}(\boldsymbol{y}_i - \mathbf{X}_i\boldsymbol{\beta}) + \frac{1}{2}\ln|\mathbf{Z}_i\boldsymbol{\Gamma}^{-1}\mathbf{Z}_i^\top + \boldsymbol{\Lambda}_i|\right).$$

We introduce auxiliary variables $\mathbf{W}_i \in \mathbb{R}^{n_i}$, and define

$$\boldsymbol{r}_i := \boldsymbol{y}_i - \mathbf{X}_i\boldsymbol{\beta}, \quad \mathbf{W}_i := \operatorname{diag}(\mathbf{W}_i), \quad \sqrt{\mathbf{W}_i} := \operatorname{diag}(\sqrt{\mathbf{W}_i}).$$

We now form the objective

$$\frac{1}{2}\boldsymbol{r}_i^\top\sqrt{\mathbf{W}_i}\left(\sqrt{\mathbf{W}_i}\mathbf{Z}_i\boldsymbol{\Gamma}^{-1}\mathbf{Z}_i^\top\sqrt{\mathbf{W}_i} + \boldsymbol{\Lambda}_i^{\odot\mathbf{W}_i}\right)^{-1}\sqrt{\mathbf{W}_i}\boldsymbol{r}_i + \frac{1}{2}\ln\left|\sqrt{\mathbf{W}_i}\mathbf{Z}_i\boldsymbol{\Gamma}^{-1}\mathbf{Z}_i^\top\sqrt{\mathbf{W}_i} + \boldsymbol{\Lambda}_i^{\odot\mathbf{W}_i}\right|, \tag{6}$$

where $^\odot$ denotes the elementwise power operation:

$$\boldsymbol{\Lambda}_i^{\odot\mathbf{W}_i} := \begin{bmatrix} (\lambda_{1j})^{w_{i1}} & 0 & \dots & 0 \\ 0 & \ddots & \ddots & \vdots \\ 0 & \dots & 0 & (\lambda_{in_i})^{w_{in_i}}. \end{bmatrix} \tag{7}$$

When $w_{ij} = 1$, we recover the contribution of the ijth observation to the original likelihood. As $w_{ij} \downarrow 0$, The ijth contribution to the residual is correctly eliminated by $\sqrt{w_{ij}} \downarrow 0$. The jth row and column of $\sqrt{\mathbf{W}_i}\mathbf{Z}_i\boldsymbol{\Gamma}^{-1}\mathbf{Z}_i^\top\sqrt{\mathbf{W}_i}$ both go to 0, while the jth entry of $\boldsymbol{\Lambda}_i^{\odot\mathbf{W}_i}$ goes to 1, which effectively removes all impact of the jth point on the covariance matrix.

(3) Posterior Variance-Covariance Matrix

In the meta-regression setting, the model we consider is given by:

$$\boldsymbol{y} = \boldsymbol{X}\boldsymbol{\theta} + \boldsymbol{U}\boldsymbol{\gamma} + \boldsymbol{\epsilon}, \quad \boldsymbol{\epsilon} \sim N(0, \sigma^2\boldsymbol{I}),$$

and the estimate for $\boldsymbol{\theta}$ at the true value forγ is given by

$$\hat{\boldsymbol{\theta}} = (\boldsymbol{X}^T\boldsymbol{V}(\gamma)^{-1}\boldsymbol{X})^{-1}\boldsymbol{X}^T\boldsymbol{V}(\gamma)^{-1}\boldsymbol{y},$$

with its variance is computed by

$$\boldsymbol{V}(\hat{\boldsymbol{\theta}}) == (\boldsymbol{X}^T\boldsymbol{V}(\gamma)^{-1}\boldsymbol{X})^{-1}. \tag{8}$$

To obtain an estimate of this matrix we can replace $\boldsymbol{\gamma}$ by its estimate $\hat{\boldsymbol{\gamma}}$.

Sampling from this distribution, a single measurement $\hat{y}_i$ for given values of the design matrix $\boldsymbol{x}$ has variance

$$V(y_i) = \boldsymbol{x}^T(\boldsymbol{X}^T\boldsymbol{V}(\gamma)^{-1}\boldsymbol{X})^{-1}\boldsymbol{x}.$$

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