Online supplementary materials

For: *Mines - the Local Wealth and Health Effects of Mining in Developing Countries*

December 3, 2014

Online appendices

Notes and definitions

Additional analysis

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Appendix A: Notes on data sources and processing

DHS surveys in sample

Albania (2009)
Angola (2007, 2011)
Bangladesh (2000, 2004, 2007, 2011)
Bolivia (2008)
Burkina Faso (1993, 1999, 2003, 2010)
Burundi (2010)
CAR (1995)
Cambodia (2000, 2005, 2010)
Cameroon (1991, 2004, 2011)
Colombia (2010)
DR Congo (2007)
Cte d'Ivoire (1994, 1999)
Dominican Republic (2007)
Egypt (1992, 1995, 2000, 2003, 2005, 2008)
Ethiopia (2011)
Ghana (1993, 1998, 2003, 2008)
Guinea (1999, 2005)
Guyana (2009)
Haiti (2000, 2006)
Indonesia (2003)
Jordan (2002, 2007)
Kenya (2003, 2009)
Lesotho (2004, 2009)
Liberia (1986, 2007, 2009, 2011)
Madagascar (1997, 2009, 2011)
Malawi (2000, 2004, 2010, 2012)
Mali (1996, 2001, 2006)
Moldova (2005)
Morocco (2004)
Mozambique (2009)
Namibia (2000, 2007)
Nepal (2001, 2006, 2011)
Niger (1992, 1998)
Nigeria (1990, 2003, 2008, 2010)
Peru (2000, 2005)
Philippines (2003, 2008)
Rwanda (2005, 2008, 2010)
Senegal (1993, 1997, 2005, 2009, 2011)
Sierra Leone (2008)
Swaziland (2007)
Tanzania (1999, 2004, 2008, 2010, 2012)
Togo (1988, 1998)
Uganda (2001, 2006, 2009, 2011)
Zambia (2007)
Zimbabwe (1999, 2006, 2011)

Definition of quarries

We exclude from the analysis all mines that are best characterized as quarries. As noted, this is because we posit that quarries are sufficiently different from mines in both their economic importance and as a source of emissions to warrant treatment as a separate type of entity. Because of their economic importance, we choose to include gemstone mines in our analysis; however, we exclude mines that produce semi-precious stones.

More precisely, we define as a quarry any location where exclusively any combination of the following materials (as defined in the USGS data) is being produced: abrasive, ball clay, bentonite, brick clay, bromine, calcium, cement rock, clay, diatomite, dolomite, feldspar, fire clay, flagstone, fluorine-fluorite, fullers earth, garnet, granite, gypsum-anhydrite, halite, kaolin, kyanite, limestone, magnesite, marble, mica, mineral pigments, olivine, peat, perlite, pumice, quartz, rock asphalt, salt, sand and gravel, semi-precious stones, silica, staurolite, stone, talc-soapstone, vermiculite, travertine, volcanic materials, wollastonite, zeolites.

World metals and minerals price data

Where available, we obtain metal and mineral prices from the World Bank's Global Economic Monitor commodities data (World Bank, 2013). We add additional price series from UNCTAD (2013) (manganese ore and tungsten), and the IMF (2013) Primary Commodity Prices (iron ore and yellowcake uranium oxide). Not all metals and minerals are traded in exchanges; for those where a market price is not easily observed, we obtain aggregated transaction-level price data. We use transaction-level data for minor platinum-group metals from Johnson Matthey, the metal traders (2013), and for all other metals and minerals not covered in the sources listed above, from the U.S. Geological Survey (Kelly and Matos, 2013). We omit diamond mines from our IV analysis, since it has been argued that "there are no internationally set prices for rough diamonds ... [and] the market prices for rough natural diamonds are almost constantly in a state of flux." (Natural Resources Canada, 2009) Prices are generally given either for units of processed metal, or units of metal content in ore. The exceptions are coal, iron ore, phosphate rock and potash, where prices are given for the raw product. We align price units with production units in our data accordingly. Where necessary, we deflate price data using the U.S. CPI published by the U.S. Bureau of Labor Statistics.

Further notes on the panel treatment definition

As noted in the main body of the paper, we do not impute mining activity in our production data, not even tacitly, by contrasting observations before and after an opening date. We do impute an absence of activity under the following restrictive conditions: we assume an absence of activity for five years prior to a mine opening date, if (i) the opening date is recorded clearly in the data, (ii) the recorded date is no more than three years earlier than the first year in which production data is available, and (iii) it is not the case that production data reflects an ambiguous start date. We consider the start of production to be ambiguous if production is reported as missing during the opening year, is known to have been zero in the year before, and is known to have been non-zero in the year after.

Further notes on the time-varying instrument

In the panel, we instrument for the current operating status of a mine using a weighted price index. Because mines typically extract several minerals, we define the price index as the world market price for each mineral produced in year $t - \tau$, weighted by the share of minerals in the previous year's production, at $t - \tau - 1$. To account for the large difference in price levels across minerals, we normalize price in the year 2005 to one. For years before the first year of production, we weight prices by the average production shares during the mine's subsequent production history; for years after the final year of production, we weight by the final year; for years in between production years, we weight by production shares in the most recent year of observed production.

Appendix B: Construction and interpretation of the asset index

To obtain a sound measure of wealth in the absence of data on consumption, expenditure, or income, we compute a standard index of asset and housing characteristics, as in Filmer and Pritchett (2001). Because our asset data includes many dummy variables, we follow Sahn and Stifel (2003) in using a factor index in our main specification, rather than the more well-known principal-component index; empirically, the differences are slight.

The index is based on the largest set of assets and housing characteristics available *within each survey round*. That is, the information used in the index varies between countries and survey years. We choose this approach because (i) the set of assets recorded varies greatly across survey years, so that working with the largest common set would discard a great deal of information, and because (ii) in our very heterogenous sample, defining impacts relative to the variation in wealth within a given country and year seems more appealing than defining them relative to global variation.

We include any asset for which data is available for 90% of those households for which at least one woman answered the women's questionnaire. Empirically, little changes if we strike a different balance between data availability and richness of information.

The maximum set of variables included is the following:

Housing characteristics

- Dummy indicating whether the household has: a kitchen, a chimney.
- Dummies recording whether the dwelling uses 'rudimentary' or 'finished' building materials (as opposed to the omitted category of 'natural' building materials) for floors, walls, and the roof. The categories are country-specific and intuitive. For instance, in the 1986 Liberia survey, 'natural' roof materials were thatch and grass; the 'rudimentary' material was sheet metal; and 'finished' materials were concrete, asphalt, or asbestos.

Assets

- Share of children of no more than 15 years of age in the household who have: blankets, shoes, clothes;
- Dummy for whether the household owns any number of each of the following items: phone landline, mobile phone; stove other than open fire, electricity connection, refrigerator; radio, TV; watch, bank account; bicycle, motorbike, car.

To illustrate how the index relates to ownership of individual assets, Table B shows factor loadings for those survey rounds used to illustrate results in the main body of the paper.

Table B

Asset index - examples of factor loadings

	Peru 2000				Burkina Faso 2010			
	Factor loading	Mean	SD	Factor x SD	Factor loading	Mean	SD	Factor x SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Assets and housing characteristics								
Landline	0.07	0.18	0.38	0.18	0.09	0.02	0.15	0.61
Mobile phone					0.08	0.64	0.48	0.17
Electricity	0.10	0.67	0.47	0.22	0.22	0.14	0.34	0.63
Refrigerator	0.10	0.31	0.46	0.23	0.13	0.04	0.20	0.68
Radio	0.03	0.86	0.35	0.09	0.05	0.72	0.45	0.10
TV	0.13	0.68	0.47	0.27	0.21	0.17	0.38	0.55
Watch					0.04	0.41	0.49	0.09
Bank account					0.12	0.14	0.34	0.35
Bicycle	0.02	0.24	0.43	0.04	0.00	0.88	0.33	0.01
Motorbike	0.02	0.04	0.20	0.09	0.10	0.39	0.49	0.20
Car	0.03	0.08	0.28	0.11	0.07	0.02	0.13	0.56
Floor rudimentary	-0.01	0.08	0.27	-0.04				
Floor finished	0.10	0.45	0.50	0.20	0.13	0.47	0.50	0.27
Roof rudimentary					-0.01	0.03	0.16	-0.09
Roof finished	0.30	0.38	0.49	0.62	0.11	0.58	0.49	0.23
Walls rudimentary	-0.48	0.64	0.48	-1.00	-0.06	0.22	0.41	-0.15
Walls finished					0.11	0.43	0.49	0.21
Asset index								
Mean	0.00				0.00			
Standard deviation	0.98				0.94			
Minimum	-1.26				-1.15			
Maximum	1.84				3.77			
Number of households	19,452				11,329			

Appendix C: Weighted cross-sectional results

In the main paper, we work throughout with unweighted regressions. This is (i) because due to sample size limitations, we cannot obtain the mine-by-mine coefficients needed for weighted estimates for all outcome variables, and (ii) because given the data structure, even the best weighted scheme is ultimately incorrect.

To gain some insight into how our unweighted estimates should be interpreted, we compare them to alternative weighting schemes in the case of the impact of mining on one outcome, namely asset ownership. The asset index is available for nearly all households in our sample. In the cross-section, we therefore have enough observations near many mines to estimate the effect of closeness separately, mine by mine. This offers an opportunity to compare our baseline unweighted estimates to estimates obtained by giving equal weight to each mine ('mine-weighted' estimates), and by computing a population-weighted average of mine-level coefficients ('population-weighted' estimates).

Table C shows four treatment effect estimates obtained by using different weighting approaches, alongside the baseline estimate. Column (1) replicates the unweighted baseline estimate shown in Table IV, and Column (2) shows that the unweighted estimate is very similar for the sub-sample of mines for which separate estimates can be generated. Similar results emerge from pooled estimation using naïve sampling weights - namely, the original sampling weights given in the DHS data, re-scaled to account for population and sample size in the different surveys that make up our pooled sample. (Column 3)

Coefficients given in Column (4) are averages of mine-wise estimates of Equation (1), pooled with *equal weights* for each mine-year, and with standard errors as described in Deaton (1997). This is nearly a consistent estimate of the mine-weighted effect, except for the fact that each mine-level estimate is obtained from an unweighted regression over households. Finally, Column (5) shows the average of mine-wise estimates of Equation (1), pooled using population weights for each mine-year. Because sampling is stratified at the cluster level, not the mine level, we construct mine-level weights from the sum of cluster-level weights. We then use these in pooling coefficients. The resulting coefficient is intended as an estimate of the population-weighted treatment effect. It is, however, inconsistent both because the mine-level regression is unweighted (as in Column 4), and also because the population weight generated from the aggregate of cluster weights is ultimately not correct.

Both the mine-weighted and population-weighted estimates (Columns 4 and 5) are larger than the unweighted estimates; they are highly significant despite being generated by an inefficient process. Thus, while they serve as a reminder that our unweighted baseline estimates are inherently somewhat vulnerable, the weighted results confirm that there are strong and significant positive wealth effects in mining communities. Finally, we note that weighting by population, rather than applying equal weight for each mine, increases the point estimate. *Prima facie*, this suggests that treatment effects will tend to be larger in larger communities at least among the sample of mines with dense enough population in the vicinity to allow for mine-level estimates.

Table C
Weighted estimates of cross-sectional asset wealth effects

	All HHs				
	Benchmark: unweighted estimate	Unweighted estimate for mines included in mine-level estimation	Naïve weights, re-scaled to global sample	Equal mine-year weights	Mine-year sampling weights
	(1)	(2)	(3)	(4)	(5)
HH close to mine	0.105***	0.0914***	0.0963***	0.193***	0.295***
	(0.035)	(0.0104)	(0.0199)	(0.0345)	(0.0513)
Number of households	90,319	51,598	50,743		
Number of groups	1562	306	306	306	306

Notes. Columns (1-3) show estimates of equation (1), using mine-year fixed effects. Columns (4) and (5) show estimates obtained by computing equation (1) for observations near each mine separately, and then summing them as described in Appendix C. Controls include a quadratic in the household head's age, and an indicator for urban/rural status. Standard errors obtained following Deaton (1997). Significant at * 10%, ** 5%, *** 1%.

Appendix D: Measurement error in geolocation

Visual inspection in high-resolution satellite images available on Google Earth of the geolocations recorded for mines in the USGS and RMD data raises some doubts about the precision of the marker positions. At times, the impression is that the marker is at some distance from visible mine features. This visual quality check is far from perfect: images available online may not have been taken at a time when the mine was operational; underground mines and small mines may not be readily visible in satellite images at any time; in many other instances, mines are very large complexes, and there is little intuition as to where the correct marker location ought to be; and in a substantial number of cases, mines are geographically clustered, and it is impossible to identify individual operations without substantial research.

However, to assess potential measurement error concerns, we benchmark geolocations obtained from the RMD business intelligence data against those recorded in an additional, entirely independent dataset (*Mining Atlas*), for the common subset of mines. To implement this test, we manually match mines from the two datasets on their name, the minerals mined, and the country in which the mine is located. Where necessary, we consult additional information, such as company records, to confirm the merge. The merge would seem to be more reliable for large mines with unusual names producing some of the more rarely mined minerals in countries where there are few mines (for instance, the Langer Heinrich uranium mine in Namibia), but less accurate without substantial additional research for mines with common names producing common metals in countries with very many mines (consider the Santa Rosa polymetallic mine in Peru). Because of this concern, we make no attempt to benchmark locations in the very large USGS dataset used in the cross-section.

As noted in the main body of the paper, in our baseline data, we drop a handful of mines where RMD geolocations are obviously erroneous, or for which the RMD and Mining Atlas geolocations are 40km or more apart. Conditional on excluding these mines, the sampling cluster distances to the nearest mine generated by the two datasets are strongly correlated, with an apparent white noise error pattern. For mines that ever appear in our analysis, the mean absolute discrepancy in distance to the nearest mine is 4.7km (with a median of 2.5km). We conclude that we should expect some attenuation bias in our results.

With two noisy, but plausibly independent measures of distance in hand, we can in the cross-section use closeness to mines as measured by one data set to instrument for closeness to mines in the other, and hence, correct measurement error.[1] Table D shows results from this approach for the asset index. Column (1) mirrors the baseline cross-sectional result shown in Table IV, but obtained using using state-year fixed effects.[2] Columns (2) and (3) show results using closeness as measured by RMD geolocations and Mining Atlas geolocations, respectively, for clusters in the vicinity of those mines for which geolocation data is available from both sources. As is evident, the point estimates are very close to the benchmark, as well as to each other. Columns (4) and

[1] We use production information only from one source of geolocation data, and hence, cannot implement a similar approach in the panel.

[2] When we use mine-year effects, results are empirically very similar. However, allowing for mine-year effects necessitates a choice in the IV models of which dataset mine-year effects should be based on, and forcing a choice would seem to run counter to the spirit of instrumenting with one noisy measure for another.

(5) show IV estimates for the sub-sample of mines present in both datasets. As expected in the presence of measurement error, both point estimates are larger than the fixed effect estimates in columns (2) and (3), though they are not significantly different.

We further note that the ratio between the OLS and IV estimates is about 0.85 for the RMD data, and 0.61 using the Mining Atlas data. Asymptotically at least, we would therefore conclude that distance as measured using RMD geolocations is a less noisy measure of true distance than the measure derived from Mining Atlas geolocations (Filmer and Pritchett, 2001). This reassures us in our choice of RMD as the basic data source. We conclude that there is evidence of measurement error in mine locations, and of resulting attenuation bias. Bias is considerable, between 18% and 65% in the two specifications we estimate. However, our preferred RMD-based estimate exhibits the lower level of bias, and in any case, the corrected estimates are not so different from our baseline estimates as to substantially change our interpretation of wealth patterns in mining communities.

Table D
Robustness to measurement error in geolocation

	Asset index				
	Benchmark (RMD - full sample)	RMD - common sample	Mining Atlas - common sample	Using Mining Atlas to instrument for RMD	Using RMD to instrument for Mining Atlas
	(1)	(2)	(3)	(4)	(5)
HH close to mine	0.105***	0.0983***	0.0969***	0.116**	0.160**
	(0.0314)	(0.0306)	(0.0324)	(0.0553)	(0.0645)
Number of households	90,319	43,985	42,190	39,073	39,073
Number of groups	554	440	435	426	426
R-squared	0.152	0.181	0.176		
First stage IV coefficient				0.712***	0.665***
				(0.0506)	(0.0576)

Notes. The table shows estimates of equation (1) in columns (1-3), and additional IV estimates in columns (4-5). Column (1) mirrors the benchmark estimate given in Table 5. Columns (2) and (3) show estimates using measures of sampling cluster distance to the nearest mine as given by the two mine location datasets, respectively. The sample is restricted to those mines where locations are recorded in both datasets. (The number of groups is not equal, because some sampling clusters are within 20km of the nearest mine in one of the datasets, but not in the other.) Columns (4) and (5) show results obtained by instrumenting for closeness as measured in one of the datasets by closeness as measured by the other. Controls include a quadratic in the household head's age, and an indicator for rural/urban status. Standard errors are clustered at the state level. IV standard errors come from cluster bootstraps using 400 repetitions. Significant at * 10%, ** 5%, *** 1%.

Additional results tables and figures

- Appendix E Functional consequences of lead exposure
- Appendix F Effects on causes of anemia other than lead exposure
- Appendix G Core results when disaggregating between mines and smelters
- Appendix H Effects on birth weight
- Appendix I Additional falsification tables - child health outcomes
- Appendix J Additional falsification tables - adult health outcomes
- Appendix K Health investment and insurance
- Appendix L Additional figures: distribution of mine-level treatment effects

Table E

Health impacts due to lead exposure and their functional consequences

	Health impacts on adults	Health impacts on children			
	Anemia	Anemia		Cognitive deficits	Stunting
Previously observed threshold PbB levels	≥ 50μg/dL	≥ 40μg/dL		No apparent threshold	≥ 10μg/dL
Descriptive impacts	Listlessness, reduced focus and ability to perform physical work.	Listlessness, reduced focus and ability to perform physical work.	Poor cognitive development secondary to anemia.	Poor cognitive development as a direct consequence of lead toxicity.	Lower adult height and body mass, adverse birth outcomes in stunted women. Poor cognitive development.
Estimated functional consequences in terms of productivity loss (%) in adulthood	5-17% (a)	2.5% (b)	1.2% (c)	1.6-13% (d)	54% (e)
Disability weights (% DALY per year)	0.5-5.8% (f)	0.5-5.8% (f)	2.4%	2.4-36% (g)	0.2-5.3% (h)
Direct impact used in imputing productivity loss	n/a	0.5-1.5σ decrement in performance on Bayley Scales of Infant Development (i)	1.73 IQ point decrease with 1g/dL decrease in Hgb (j)	0.3-2.7σ loss in IQ with a move from 2μg to 20μg PbB (k)	n/a
Sources	Horton (2003), Salomon et al. (2013)	Horton (2003), Salomon et al. (2013)	Stoltzfus et al. (2004), WHO (2004)	Lanphear (2005), Rau (2013), WHO (2004)	Dewey and Begum (2011), Hoddinott et al. (2011), Ricci et al. (2006), WHO (2004)

Notes. Threshold PbB levels from ATSDR (2007) and Lanphear (2005). (a) Range of direct productivity estimates from RCTs, as reported in Horton (2003). Estimates relate to the impact of iron-deficiency anemia; iron deficiency may impact productivity through channels orther than anemia. (b) Imputed productivity loss, as reported in Horton (2003). (c) Authors' calcuation of productivity loss due cognitive losses secondary to anemia. We assume mean Hgb levels in anemic and non-anemic children as observed in our sample. In addition, we follow Horton (2003) in assuming a correlation of 0.62 between childhood and adult IQ , and a wage decrease of about eight percent associated with a one-standard deviation decrement in adult IQ. (d) Authors' calculation of productivity loss due to a move from 2μg to 20μg PbB. Additional assumptions as in (c). (e) Direct productivity estimate from an RCT reported in Hoddinott (2011). (f) Range refers to weights for mild and moderate anemia. (g) Range refers to low weights for cognitive impairment secondary to anemia, and high weights for "mild metal retardation attributable to lead exposure". (h) Range refers to low weights for stunting secondary to protein-energy malnutrition, and high weights obtained by attributing a share of DALYs lost to diarrheal disease to the secondary effect on stuntintg. (i) Range of outcomes from RCTs that provided iron supplements. (j) Estimate from a meta-analysis of observational studies. (k) Lanphear et al. report a semi-log dose-response function; Rau et al. report a linear relationship. We transform them to directly compare the impact of moving from a background level of lead exposure to a level in between the thresholds previously associated with overt anemia or stunting. A standard deviation is assumed to be equivalent to 19 IQ points, as in Lanphear (2005).

Table F
Differential diagnosis - effect of closeness to mines on causes of anemia other than lead exposure

	Iron deficiency			Malaria		Worms
	Youngest child given meat or eggs	Youngest child given iron-rich vegetables	Iron pills during most recent pregnancy	Tested positive for malaria	No malaria drugs during most recent pregnancy	Worm pills during recent pregnancy
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Cross-section - full sample					
HH close to mine	-0.0224*	-0.0141	-0.00163	-0.0399	-0.0134	0.00848
	(0.0129)	(0.0161)	(0.00920)	(0.0274)	(0.0131)	(0.0251)
Number of observations	17,639	17,639	43,521	3,384	19,636	14,491
	Panel B: Cross-section - women's Hgb sample					
HH close to mine	-0.0134	-0.0341	-0.0156	-0.00253**	-0.00562	-0.0204
	(0.0261)	(0.0226)	(0.0181)	(0.000970)	(0.0187)	(0.0188)
Number of observations	6,223	6,223	12,678	1,615	7,125	6,478
	Panel C: Mine type DiD - full sample					
HH close to a 'heavy metal' mine	-0.0245	-0.00362	0.0190	-0.0142	0.00118	-0.00272
	(0.0307)	(0.0394)	(0.0273)	(0.0169)	(0.0271)	(0.0348)
Number of observations	17,647	17,647	37,387	1,879	18,566	13,755
	Panel D: Mine type DiD - women's Hgb sample					
HH close to a 'heavy metal' mine	-0.0131	0.0156	-0.000880	-0.00333***	-0.0358	0.0461*
	(0.0505)	(0.0501)	(0.0415)	(0.000197)	(0.0394)	(0.0270)
Number of observations	6,223	6,223	12,678	1,615	7,125	6,478
	Panel E: Panel - full sample					
HH close * mine operating in treatment period	-0.0533	-0.0342	0.0680		0.0104	-0.0207
	(0.0747)	(0.0678)	(0.0490)		(0.0757)	(0.0497)
Number of observations	3,893	3,893	7,529		4,012	2,452
	Panel F: Panel - women's Hgb sample					
HH close * mine operating in treatment period	-0.0238	0.0628	-0.0169		0.0498	-0.0699
	(0.0851)	(0.0431)	(0.0696)		(0.0997)	(0.0768)
Number of observations	1,648	1,648	3,039		1,758	1,115

Notes. The table shows estimates of equations (1), (2), and (4), as indicated. Each equation is estimated separately for the entire sample, and for the sub-sample for which we observe womens' Hgb levels. In each case, we report only the coefficients of interest, as indicated. Fixed effects, time effects, and covariates are as in the preferred Hgb models reported in the main body of the paper. Standard errors are clustered at the mine level. Significant at * 10%, ** 5%, *** 1%.

Table G
Comparative effects on core outcomes near mines and smelters

	Asset index		Women's altitude-adjusted Hgb (g/dL)		Children's height for age score	
	Mines/legacies	Smelters	Mines/legacies	Smelters	Mines/legacies	Smelters
			A: Cross-section			
HH close to mine	0.109***	0.0765	-0.0948*	-0.0648	0.0822*	0.0910
	(0.0413)	(0.0666)	(0.0553)	(0.0691)	(0.0446)	(0.0762)
Number of observations	77,569	12,750	31,304	6,913	34,776	5,776
			B: Mine type DiD			
HH close to mine	0.111**	0.0294	-0.0502	0.121	0.0896*	-0.0147
	(0.0457)	(0.0960)	(0.0581)	(0.0953)	(0.0487)	(0.0837)
HH close to a 'heavy metal' mine	-0.0172	0.0870	-0.523**	-0.244**	-0.0556	0.241
	(0.102)	(0.119)	(0.224)	(0.121)	(0.128)	(0.156)
Number of observations	77,569	12,750	31,304	6,913	34,776	5,776
			C: Panel			
HH close to mine	-0.101	-0.110***	0.189	0.537***	0.195**	-0.0633
	(0.0978)	(0.00686)	(0.155)	(0.00895)	(0.0865)	(0.131)
Mine operating in treatment period	-0.0498	-0.110***	0.264*	0.222***	0.0535	-1.774***
	(0.0434)	(0.00353)	(0.144)	(0.0233)	(0.0604)	(0.240)
HH close * mine operating in treatment period	0.322***	0.183***	-0.159	-0.429***	-0.207**	0.178
	(0.107)	(0.0216)	(0.215)	(0.0552)	(0.0943)	(0.195)
Number of observations	15,688	6,891	6,680	3,165	8,726	2,903

Notes. The table shows estimates of equation (1) in the uppermost panel, estimates of equation (4) in the middle panel, and estimates of equation (2) in the bottom panel. Fixed effects and covariates are as in the preferred models in the main body of the paper. In columns (1), (3), and (5), the sample is limited to observations near mines or legacies, while the other columns, it is limited to observations near smelters. Standard errors are clustered at the mine/smelter level. Significant at * 10%, ** 5%, *** 1%.

Table H
Effects on birth weight (in grams) in children under five years of age

	Panel A: Cross-sectional results				
	Benchmark	Never-movers only	Effect near 'heavy metal' mines	Effect on infants near any mine	Effect on infants near 'heavy metal' mines
	(1)	(2)	(3)	(4)	(5)
HH close to mine	7.456	-8.686	0.411	0.711	-4.651
	(11.75)	(19.32)	(13.37)	(12.73)	(14.23)
HH close to a 'heavy metal' mine			16.50		24.27
			(31.54)		(32.68)
Child in infancy				-8.653	-9.879
				(7.081)	(7.857)
HH close and child in infancy				13.12	22.12
				(15.39)	(18.52)
Nearest mine (≤ 20km) is a 'heavy metal' mine, and child in infancy					6.222
					(17.04)
HH close to a 'heavy metal' mine, and child in infancy					-34.98
					(33.97)
Number of observations	38,165	13,651	36,978	36,978	36,978

	Panel B: Panel results			
	Mine-level	Mine-level	Mother-level	Mother-level
	(6)	(7)	(8)	(9)
HH close to mine	-19.23	-28.72		
	(32.89)	(31.87)		
Mine operating during pregnancy	60.32**		103.2*	
	(24.80)		(61.67)	
HH close and mine operating during pregnancy	15.29		-246.7*	
	(36.43)		(144.9)	
Mine operating in birth year		62.54***		51.55
		(21.99)		(47.89)
HH close and mine operating in birth year		28.09		80.48
		(34.60)		(135.6)
Number of observations	10,559	10,559	10,559	10,559

Notes: The table shows estimates of equations (1), (2), and (4), as indicated. Fixed effects, time effects, and covariates are as in the corresponding results reported for child growth outcomes in the main body of the paper. The dependent variable is the birth weight in grams. Standard errors are clustered at the mine level. Significant at * 10%, ** 5%, *** 1%.

Table I
Additional falsification tests - child health outcomes

	Infant mortality	Under-five mortality	Diarrhea	Cough	Fever
	(1)	(2)	(3)	(4)	(5)
			Panel A: Cross-section		
HH close to mine	-0.00246	-0.00305	0.0112*	0.00480	0.00191
	(0.00223)	(0.00270)	(0.00579)	(0.00963)	(0.00788)
Number of children	298,373	298,373	61,567	60,305	59,494
			Panel B: Cross-section - never-movers		
HH close to mine	-0.00711**	-0.00974***	0.00709	0.0292*	0.00898
	(0.00288)	(0.00369)	(0.0124)	(0.0159)	(0.0156)
Number of children	110,764	110,764	22,732	22,192	21,272
			Panel C: Cross-section - differential impact on infants		
HH close to mine and child in infancy			0.00756	-0.0143	-0.0109
			(0.0113)	(0.0107)	(0.0112)
Number of children			61,567	60,305	59,494
			Panel D: Mine-type DiD		
HH close to a 'heavy metal' mine	0.00147	0.00381	-0.00658	0.0434**	0.00128
	(0.00426)	(0.00548)	(0.0123)	(0.0208)	(0.0186)
Number of children	298,373	298,373	61,567	60,305	59,494
			Panel E: Mine-type DiD - differential impact on infants		
HH close to a 'heavy metal' mine * child in infancy			-0.0163	-0.0212	-0.0122
			(0.0240)	(0.0262)	(0.0234)
Number of children			61,567	60,305	59,494
			Panel F: Mine-level panel		
Exposure period	In utero	In utero	Survey year	Survey year	Survey year
Mine operating in exposure period * HH close	-0.00499	-0.00819	-0.00260	0.00392	-0.0234
	(0.00745)	(0.00864)	(0.0282)	(0.0299)	(0.0258)
Number of children	43,057	43,057	15,449	15,325	15,576
			Panel G: Mother-level panel		
Exposure period	In utero	In utero	Survey year	Survey year	Survey year
Mine operating in exposure period * HH close	-0.0108	-0.00514	-0.140	-0.0965	-0.114
	(0.0214)	(0.0248)	(0.125)	(0.0945)	(0.111)
Number of children	43,057	43,057	15,989	16,113	16,370

Notes. The first panel reports results from Equation (1); the following panel reports estimates from the same equation, with the sample restricted to never-movers; and the panel labled 'differential impact on infants' shows the coefficient on the interaction of the treatment in equation (1) with an indicator variable for whether a child was in infancy. The panel labeled 'mine-type DiD' reports estimates of equation (4); the following panel shows estimates of the same equation, with an additional interaction term of the DiD variable with an indicator for infancy. The mine-level and mother-level results are estimates of equations (2) and (3), respectively. All models include fixed effects and controls as in Table 13 in the main paper. Standard errors are clustered at the mine level. Significant at * 10%, ** 5%, *** 1%.

Table J
Additional falsification tests - adult health outcomes

	Ever miscarried	Female respondent very sick	Night blindness during pregnancy	Male respondent very sick
	(1)	(2)	(3)	(4)
Panel A: Cross-section				
HH close to a mine	0.00263	0.00328	0.00254	0.0120
	(0.00460)	(0.00527)	(0.0104)	(0.00977)
Observations	117,118	11,022	29,317	9,808
Panel B: Cross-section - never-movers				
HH close to a mine	-0.00433	0.0155*	0.00946	0.0149
	(0.00781)	(0.00880)	(0.0173)	(0.0131)
Observations	49,817	4,459	11,701	3,716
Panel C: Mine-type DiD				
HH close to a 'heavy metal' mine	-0.00377	-0.00286	0.0310	0.0129
	(0.0109)	(0.00898)	(0.0216)	(0.0154)
Observations	117,118	11,022	29,317	9,808

Notes. The first panel reports results from question (1); the panel labeled 'never-movers' reports estimates from the same equation, with the sample restricted to households that reported never having moved from their current place of residence. The panel labeled 'mine-type DiD' reports estimates of equation (4). All models were estimated using mine-year indicators as group fixed effects, and include controls for the the respondent's age and urban/rural status of the sampling cluster. Standard errors are clustered at the mine level. Significant at * 10%, ** 5%, *** 1%.

Table K.1
Effects on health insurance coverage

Panel A: Health insurance coverage of women

	Overall coverage			Employer-provided coverage		
	Cross-section		Panel	Cross-section		Panel
	All HHs	Never-movers		All HHs	Never-movers	
	(1)	(2)	(3)	(4)	(5)	(6)
HH close to mine	0.0282**	0.0518***	0.0260**	0.0206***	0.0276***	-0.0015
	(0.0127)	(0.0130)	(0.0125)	(0.00769)	(0.00789)	(0.0123)
Mine operating			0.191			0.0168**
			(0.12)			(0.00658)
Mine operating * HH close (DiD)			0.0571**			0.0255
			(0.028)			(0.0252)
Number of respondents	35,971	13,014	9,183	17,876	3,849	4,597
Number of groups	638	540	105	231	140	68
R-squared	0.003	0.005	0.021	0.002	0.004	0.008

Panel B: Health insurance coverage of men

	Overall coverage			Employer-provided coverage		
	Cross-section		Panel	Cross-section		Panel
	All HHs	Never-movers		All HHs	Never-movers	
	(7)	(8)	(9)	(10)	(11)	(12)
HH close to mine	0.00826	0.0287		0.00600	0.00995	
	(0.0225)	(0.0264)		(0.0133)	(0.0136)	
Mine operating						
Mine operating * HH close (DiD)			n/a			n/a
Number of respondents	6,978	1,601		6,148	994	
Number of groups	180	86		159	68	
R-squared	0.002	0.007		0.001	0.002	

Notes. The table reports estimates of equation (2) in columns (3) and (6), and estimates of equation (1), otherwise. Cross-sectional estimates use indicators for each mine-year pair as group fixed effects; panel estimates use mine-specific area effects, and country-year indicators as time effects. The panel cannot be estimated for the models in equations (9) and (12). All regressions control for a quadratic in the respondent's age, and an indicator for rural/urban status. Standard errors are clustered at the mine level. Significant at * 10%, ** 5%, *** 1%.

Table K.2
Effects on access to health care

Panel A: Maternal and infant health care

	Delivery in improved setting			Vitamin A supplement		Time of first antenatal visit (months)		Number of antenatal visits		Post-natal check on mother		Post-natal check on child	
	Cross-section	Panel		Cross-section	Panel	Cross-section	Panel	Cross-section	Panel	Cross-section	Panel	Cross-section	Panel
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
HH close to mine	0.0265**	-0.0147		0.00711	-0.0398	0.00151	-0.0999	0.0141	-0.0162	0.00153	0.00630	0.00210	0.00332
	(0.0130)	(0.0290)		(0.0143)	(0.0352)	(0.0417)	(0.115)	(0.0599)	(0.160)	(0.0151)	(0.0479)	(0.0278)	(0.0552)
Mine operating		0.0588***	0.0410		0.00185		-0.159*		0.336**		-0.0104		-0.0200
		(0.0217)	(0.0346)		(0.0276)		(0.0897)		(0.131)		(0.0222)		(0.0354)
Mine operating * HH close (DiD)		0.0130	-0.0917**		0.0235		0.0413		0.182		0.0268		-0.0500
		(0.0319)	(0.0447)		(0.0363)		(0.129)		(0.195)		(0.0483)		(0.0584)
Number of respondents	62,410	15,796	15,796	37,235	9,818	44,767	11,854	43,900	11,143	34,129	7,926	14,410	3,127
Number of groups	1,506	256	12,945	1,236	226	1,498	255	1,498	253	996	193	746	157
R-squared	0.021	0.106	0.024	0.000	0.062	0.005	0.050	0.008	0.087	0.003	0.038	0.002	0.092

Notes. Columns labeled 'Cross-section' show estimates of equation (1), using indicators for each mine-year pair as group fixed effects. Column (3) shows estimates of equation (3), using indicators for each mother as group fixed effects, and country linear time trends. All other columns marked 'Panel' report estimates of equation (3) using mine-level area effects, and indicators for each country-year pair as time fixed effects. Standard errors are clustered at the mine level. Significant at * 10%, ** 5%, *** 1%.

Panel B: Reasons for not seeking medical attention

	No knowledge		No money		Distance		No provider		No drugs	
	Cross-section	Panel	Cross-section	Panel	Cross-section	Panel	Cross-section	Panel	Cross-section	Panel
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
HH close to mine	0.0187*	0.0351	-0.00576	-0.0208	0.00104	0.0211	0.00134	0.112	-0.0114	0.0542
	(0.0103)	(0.0307)	(0.0105)	(0.0439)	(0.0114)	(0.0510)	(0.0164)	(0.118)	(0.0150)	(0.0961)
Mine operating		0.00165		-0.0217		-0.00803		-0.180***		-0.191***
		(0.0240)		(0.0259)		(0.0308)		(0.0454)		(0.0411)
Mine operating * HH close (DiD)		-0.0394		-0.0232		-0.0168		-0.150		-0.0998
		(0.0372)		(0.0464)		(0.0520)		(0.128)		(0.105)
Number of respondents	56,860	19,242	100,811	27,219	100,792	27,212	48,796	13,583	49,044	13,598
Number of groups	929	127	1,307	189	1,307	189	727	143	731	144
R-squared	0.002	0.011	0.007	0.018	0.020	0.024	0.001	0.007	0.001	0.008

Notes. Columns labeled 'Cross-section' show estimates of equation (1), using indicators for each mine-year pair as group fixed effects. Columns marked 'Panel' report estimates of equation (2), using mine-level area effects, and indicators for each country-year pair as time fixed effects. Standard errors are clustered at the mine level. Significant at * 10%, ** 5%, *** 1%.

Appendix L: Distribution of mine-level treatment effects

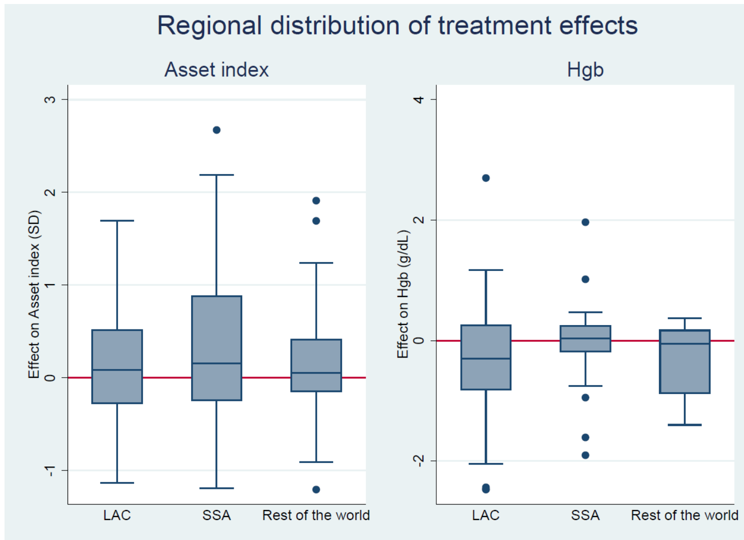

Figure M.1 - Regional distribution of treatment effects

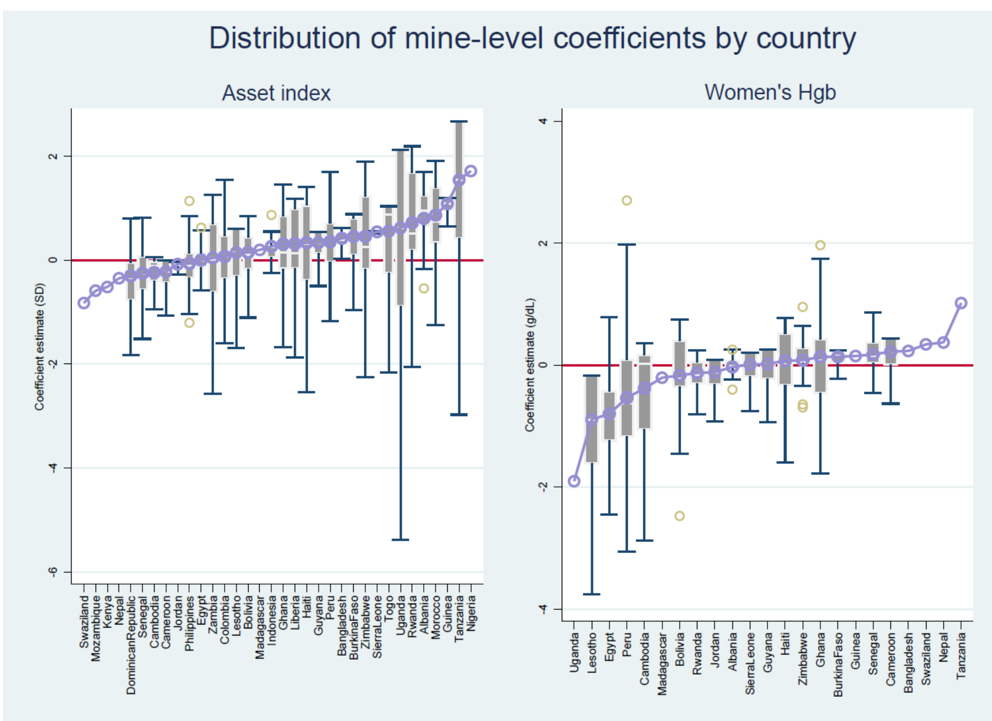

Figure M.2 - Distribution of mine-level coefficients by country

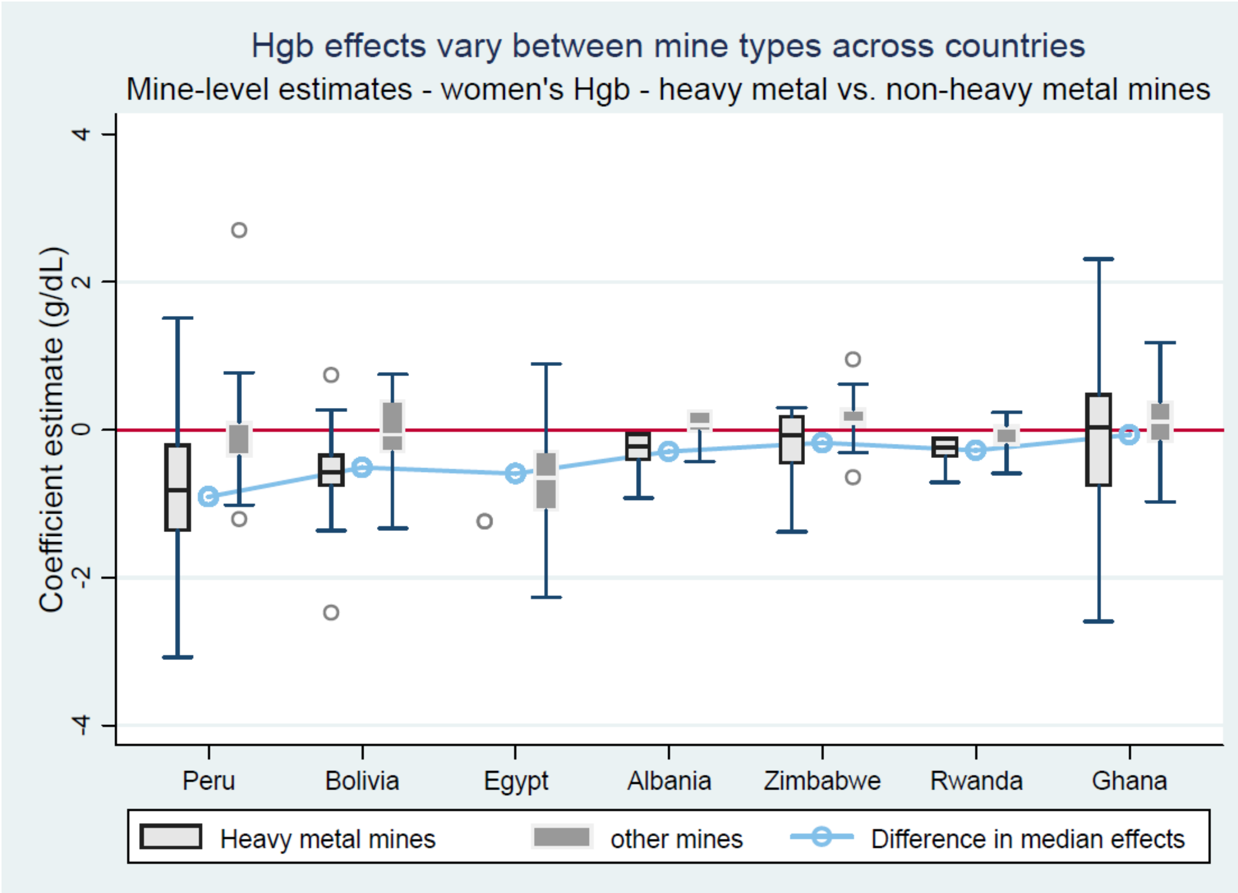

Figure M.3 - Distribution of mine type difference-in-difference estimates across countries

Note: The sample shown is limited to the sub-set of countries where mine-level estimates can be obtained for at least one mine where heavy metal pollution is expected, and one mine of a different type.

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