

Did mass privatisation really increase post-communist mortality?

David Stuckler and colleagues¹ have asserted that “mass privatisation programmes were associated with a short-term increase in mortality rates in working-aged men”. We examined their data carefully and explored the assumptions and intuition from which their claim stems. We demonstrate that the findings and methods of Stuckler and colleagues are erroneous.

Our argument was motivated by a simple stylised empirical fact that we found difficult to reconcile with the verdict of Stuckler and colleagues. That is, in countries that undertook it, rapid (mass) privatisation took place near, at, or after the end of a period of sustained increase in male mortality. Indeed, the data show that the health trends driving the association noted by Stuckler and colleagues pre-date the introduction of mass privatisation programmes in the post-communist world (figure 1 in webappendix) and specifically that the trends in mortality for privatisers and non-privatisers do not much differ (figure 2 in webappendix). The results claimed by Stuckler and colleagues stem from their ignoring the dynamic aspects of the key relationships under investigation.

In the section of our webappendix entitled “The econometrics”, we raise some concerns about the choice, classification, use, and coverage of variables. However, the key source of Stuckler and colleagues’ conclusion lies in their failure to understand the dynamic nature of the processes that underpin the observed mortality trends. In their estimates, Stuckler and colleagues pool the data and incorporate adjustments that allow for the error term to be correlated over both individual countries and time. However, this does not allow

for two important forms of temporal association that characterise the data being examined.

First, it does not allow for “true state dependence”. That is, if there is some factor (eg, disease stemming from some past exposure to pollution) causing people to die in a country in one year that is likely also to cause deaths the following year, then their estimates ignore it. Second, it does not allow for a dynamic effect to operate directly through the explanatory variables. For example, the Russian privatisation programme, announced in December, 1992, and completed in June, 1994, cannot plausibly be claimed to have affected mortality rates at all in 1992 and at most weakly in 1993.

In adopting a purely static approach to estimation, Stuckler and colleagues overlook both of these dynamic realities. In practical terms, if we think these factors are important, then a lagged effect, both of the mortality rates themselves and the explanatory variables, such as mass privatisation, needs to be accounted for in any empirical estimates (and in fact, once lagged effects are introduced, none of Stuckler and colleagues’ presented estimates are consistent). Dynamic panel-data analysis allows us to do this and is detailed in the webappendix.

So how do their findings change when these adjustments are made? In short, their controversial headline finding, of a positive association between mass privatisation and short-run increases in the male mortality rate, disappears. Column 6 of our table 2 (webappendix) augments their preferred results with a 1-year lag of the mass privatisation variable and finds *no association* between mass privatisation last year and mortality rates this year. Table 3 (webappendix) explores this in a dynamic panel setting. As expected, we confirm the presence of persistence, and find that across a range of different specifications, including the one

preferred by Stuckler and colleagues, there is simply no association between mass privatisation and male mortality. If anything, there may be some evidence of a *positive* link between market reforms and health outcomes and in our webappendix we indicate why this may be so.

There is then no evidence that mass privatisation in the post-communist world explains the observed fluctuations in male mortality. The timing of mass privatisation is not consistent with the claim, the historical trends in the region’s mortality patterns are not consistent with the claim, and the claim itself is not explored within the most appropriate statistical setting. So, the answer to the leading question is simple: “no”, there is no evidence that mass privatisation resulted in an increase in post-communist male mortality.

Aside from the immediate controversy that those claims caused (see, for example, the exchanges in the *Economist*, the *Financial Times*, or even the University of Oxford news release²), such conclusions matter: they matter for policy makers in emerging market economies; they matter for countries seeking to understand the health problems they face; and they matter for academics, practitioners, and policy makers alike as they too seek to understand the downstream effects of upstream social and economic choices.

We declare that we have no conflicts of interest.

*Christopher J Gerry,
Tomasz M Mickiewicz,
Zlatko Nikoloski
c.gerry@ucl.ac.uk

UCL School of Slavonic and East European Studies,
16 Taviton Street, London WC1H 0BW, UK

- 1 Stuckler D, King L, McKee M. Mass privatisation and the post-communist mortality crisis: a cross-national analysis. *Lancet* 2009; **373**: 399–407.
- 2 University of Oxford. Mass privatisation in the post-communist world may have cost up to one million lives. http://www.ox.ac.uk/media/news_releases_for_journalists/090115.html (accessed Jan 11, 2010).



See Online for webappendix

Submissions should be made via our electronic submission system at <http://ees.elsevier.com/thelancet/>



David Stuckler and colleagues¹ claim that mass privatisation of enterprises was “a crucial determinant of differences in adult mortality trends in post-communist countries”. We attempted to replicate their results and found that the relationship is not robust. Here we summarise our findings, which are expanded in a webappendix. Because Stuckler and colleagues do not find a positive correlation between privatisation and mortality in central and eastern Europe, but only in the former Soviet Union, we focus on the latter set of countries.

In our replication we carried out three simple checks. First, by examining the data used by Stuckler and colleagues, we found inconsistencies between the published description of their dummy variable measuring “implementation of mass privatisation”—one of two privatisation measures used in the paper—and the coding of this variable. We therefore created a new variable coded precisely as described in the article (“a jump from 1 to 3 on the EBRD large-scale privatisation index”), and we re-estimated Stuckler and colleagues’ model using this corrected measure.



Second, because an instantaneous effect of privatisation on mortality is implausible, we re-estimated the model assuming short lags (1 or 2 years) between policy changes and mortality. Third, we controlled for differences across countries in long-term mortality trends, a common statistical method (indeed, one used by Stuckler and colleagues in other work²).

The results, shown in the table, demonstrate that any one of these changes substantially weakens the positive correlation between privatisation and mortality reported by Stuckler and colleagues, and a combination of any two changes eliminates it entirely. Indeed, the estimated effect of privatisation on mortality is negative when assuming 2-year lags and controlling for trends. Although the correct functional form is unknown, one could as easily conclude that privatisation lowered as raised mortality in the former Soviet Union.

It bears emphasis that our attempt to replicate Stuckler and colleagues’ analysis uses the same data and general methods as in the original article. An important assumption of Stuckler and colleagues is that country-level data are appropriate for studying the relationship between mortality and privatisation, but it is difficult to control for confounding factors with aggregate data. In addition, therefore, we analysed data on Russian regions, but again the results do not support the hypothesis that privatisation raised mortality.

Our replication also follows Stuckler and colleagues’ focus on estimating correlations, mostly ignoring the question of causality. However, we do reanalyse the single potential channel of causation for which Stuckler and colleagues provide evidence, which is that privatisation led to increased mortality by raising unemployment. Counter to Stuckler and colleagues’ claim that “rapid privatisation of thousands of inefficient firms from the Soviet era would have cut many jobs”, but consistent with many micro-level studies of

post-communist employment,³ the results do not support the view that privatisation raised unemployment in postcommunist countries.

Stuckler and colleagues’ conclusions were accepted as facts by the world press, but closer scrutiny shows that the data do not support their assertion that privatisation was a “crucial determinant” of mortality in postcommunist countries. The correlations reported in the original article are simply not robust.

We declare that we have no conflicts of interest.

*John S Earle, Scott Gehlbach
earle@upjohn.org

Upjohn Institute for Employment Research and Central European University, Kalamazoo, MI 49007, USA (JSE); and University of Wisconsin, Madison, WI, USA (SG)

- 1 Stuckler D, King L, McKee M. Mass privatisation and the post-communist mortality crisis: a cross-national analysis. *Lancet* 2009; **373**: 399–407.
- 2 Stuckler D, Basu S, Suhrcke M, et al. The public health effect of economic crises and alternative policy responses in Europe: an empirical analysis. *Lancet* 2009; **374**: 315–23.
- 3 Earle JS. Mass privatisation and mortality. *Lancet* 2009; **373**: 1247.

Authors’ reply

We have watched with interest the increasing sophistication of attempts to discredit our paper, many at fora where we were not present, so we are grateful that we can finally respond. These criticisms have included misrepresentation of basic mortality data and a series of letters from leading advocates of privatisation that was, in turn, gratuitously offensive, epidemiologically uninformed, and factually wrong.

Unfortunately, these two letters continue on this path, with manipulation of data in ways that can be interpreted as owing more to the pursuit of preconceived beliefs than to a search for scientific truth. As Christopher Gerry and colleagues note in their webappendix, “Our goal here is not to establish per se what does cause mortality. Rather, we are concerned to demonstrate that there is no evidence in the data used by Stuckler et al that mass privatisation resulted in increased

For a fuller report, peer reviewed and unedited see <http://www.upjohninst.org/mortality/index.html>

See Online for webappendix

	Mass privatisation	Average EBRD privatisation	Recoded mass privatisation
Stuckler and colleagues’ specification	0.158 (p=0.000)	0.099 (p=0.000)	0.069 (p=0.086)
1-year lags	0.108 (p=0.010)	0.064 (p=0.006)	0.015 (p=0.690)
2-year lags	0.063 (p=0.085)	0.014 (p=0.583)	-0.015 (p=0.722)
Country-specific trends	0.093 (p=0.016)	0.069 (p=0.027)	0.050 (p=0.298)
1-year lags & country-specific trends	0.034 (p=0.408)	0.036 (p=0.234)	-0.014 (p=0.794)
2-year lags & country-specific trends	-0.042 (p=0.212)	-0.047 (p=0.091)	-0.113 (p=0.048)

Each cell of the table reports the estimated effect of privatisation on the log working-age male mortality rate from a separate regression. Privatisation is measured in three alternative ways: first column, as a dummy variable for mass privatisation coded by Stuckler and colleagues; second column, as the average of the European Bank for Reconstruction and Development (EBRD) indices for large-scale and small-scale privatisation; and third column, as a dummy variable for mass privatisation recoded precisely following the description in Stuckler and colleagues. With the exception of the privatisation measure in the third column, data are identical to those in Stuckler and colleagues. Specifications are identical but for the specific changes noted in the table. In parentheses, p values calculated from heteroskedasticity-robust standard errors.

Table: Cross-country mortality regressions on Stuckler and colleagues’ sample of countries in the former Soviet Union

mortality.” Given this, we are surprised that neither sees any need to declare a conflict of interest as authors on both letters have received funds from organisations supporting privatisation. In attempting to show that we are wrong, they employ biologically implausible assumptions, commit fundamental methodological errors, and thus generate findings that are inconsistent with published data. These measures meet established criteria for “data torture”,¹ as summarised in the panel and in our webappendix, where we address all of their points in detail.

John Earle and Scott Gehlbach, after replicating our findings, introduce three implausible and erroneous manipulations to our exposure variable, outcome data, and hypothesised mechanism.

First, as a result of misreading all three places in our paper where we describe our methods, they miscalculate our main explanatory variable, creating errors in the timing of exposure and misclassifying 36% of the cases of mass privatisation (overall 56.3% misclassification), thus failing to test our hypothesis.

Second, without a biological justification and neglecting evidence from this region that mortality peaks around the time of unemployment,² they dismiss the possibility of contemporaneous effects of mass privatisation on health. Indeed, given evidence that workers’ stress rose in anticipation of privatisation, adverse causal effects could have occurred in the period before privatisation.^{3,5} Nonetheless, Earle and Gehlbach use an ad hoc specification of lagged effects, a method which, according to one statistical textbook, “opens the researcher to the charge of data mining”.⁶

Third, in a situation where mortality rates were undergoing fluctuations that were unprecedented in a peacetime era,⁷ they remove each country’s mortality trend during the 1990s by adding 27 control variables (misreported as removing “long-term

Panel: Definitions of “data torture”¹ and selected examples of such from letters by Gerry and colleagues and by Earle and Gehlbach

Interpreting every result as confirming a major hypothesis

- Earle and Gehlbach state that, because the 2-year lag of mass privatisation has a negative coefficient, “one could as easily conclude that privatisation lowered as raised mortality in the former Soviet Union.” However, this finding is consistent with our theory, reflecting an artifactual rebound from the labour market shock of privatisation.
- Their suggestion that privatisation of already overstaffed and underproductive firms increased employment and decreased productivity is inconsistent with other authoritative sources such as the European Bank for Reconstruction and Development as well as our individual-level analyses (see webappendix).⁸

Lack of biological plausibility

- Neither group of authors offers a plausible alternative explanation. Both transform data without biological justification. Their disregard of immediate effects and invoking of 2-year lags is contrary to existing epidemiological evidence.^{2,9} Gerry and colleagues implausibly find that the only determinant of the post-communist mortality crisis is mortality in the preceding year.
- Earle and Gehlbach generate biologically implausible short-term associations between income and cancer. Gerry and colleagues generate an implausible lack of a short-term association between war and mortality.

Lack of reporting of number of data comparisons made

- Earle and Gehlbach fail to present the full extent of comparisons made in trying to test their hypothesis that we are incorrect. In at least one case, they attempt to recode our war variable but fail to confirm their hypothesis (that we are wrong), but do not present it.

Dropping subjects without biological justification

- Earle and Gehlbach drop significant subjects without appropriate justification (ie, removing each country’s mortality experience over the period studied from the analysis of over 80% of the mortality variations we sought to explain), and Gerry and colleagues also remove subjects from the analysis (ie, using the lag of mortality rates). They propose no biological justification for these steps, nor do they justify the initial year chosen for estimating long-term mortality trends.
- Both groups effectively drop exposures and outcomes without justification. They fail to adhere to scientific conventions in estimating contemporaneous and lagged effects simultaneously in finite-distributed lag models. Instead, both rely on an ad hoc specification, failing to apply standard joint F tests or a Bayesian Information Criterion or a biological justification to determine the appropriate number of lags.

Inappropriate classification of exposure and disease

- Earle and Gehlbach use the wrong exposure variable (successful privatisation, all types; misclassifying >50% of the exposures and dropping 36% of the population cases of mass privatisation). Both Earle and Gehlbach and Gerry and colleagues erroneously remove the contemporary effect of privatisation from analysis, changing the study’s hypothesis and removing country mortality experiences (>70% of mortality data). Earle and Gehlbach also use an incorrect disease measure in their regional analysis (crude death rates).

See webappendix for more details and examples.

trends”), producing attenuation bias, and eliminating more than 80% of variations in mortality we sought to explain (see figure 1, webappendix). This is equivalent to removing patients with adverse cardiac outcomes from a randomised controlled trial of a cardiovascular drug without justification.

Gerry and colleagues, in addition to committing mistakes similar to those of Earle and Gehlbach, set up a straw man by falsely attributing to us the view that mass privatisation was the only cause of fluctuating

mortality when it clearly was not (as shown in more than 180 papers on post-communist mortality we have collectively coauthored).

First, our model explains the three cases they cite (Armenia, Georgia, and Czech Republic) for which there is an inconsistency in the timing of mortality peaks: Armenia was subject to a blockade creating widespread shortages of food and energy during its war with Azerbaijan; Georgia was struggling with a large influx of refugees and economic chaos after

See Online for webappendix

the conflict in South Ossetia; and the Czech Republic had very high levels of social capital mitigating rises in mortality from mass privatisation.

Second, they incorrectly state that Russia's mass privatisation programme was "announced in December, 1992, and completed in January, 1994." Those responsible for implementing the programme state, "Vouchers were issued in September 1992" and that the programme officially began in October, 1992, and was completed by July, 1994. Furthermore, many contemporaneous commentators describe the anxieties provoked in the preceding months by preparation for voucher issue.³⁻⁵

Third, in a region where the mortality fluctuations were driven predominantly by alcohol, they adjust for the preceding year's mortality experience as a potential source of confounding, which they implausibly suggest might result from "disease stemming from some past exposure to pollution." They neglect to point out how this manipulation, in combination with removing our hypothesised mechanism, introduces spurious second-order serial correlation and also makes well established relationships such as war as a cause of mortality disappear. This indicates a serious error in their model.

In our webappendix we detail many additional examples where Gerry and colleagues misrepresent our paper, fail to follow statistical reporting and analytical conventions, misreport their own findings, selectively emphasise findings that support their arguments (while ignoring those estimates that corroborate our findings), make factual errors (such as suggesting we did not split our sample into countries that did or did not belong to the Soviet Union when we actually did), and scatter pejorative asides in our direction (accusing us of failing to include pre-1993 data from Slovakia, a country that only came into existence that year). We show our basic findings are robust to all of their criticisms

(including controlling for long-term trends and lagged effects).

We retain confidence in our original findings that rapid privatisation had a significant role in the short-term rises in mortality among working-age men seen in this region.

MM has acted as an adviser to the World Bank, WHO, and European Bank for Reconstruction and Development, advising on health policy in several former communist countries.

*David Stuckler, Lawrence King,
Martin McKee
david.stuckler@chch.ox.ac.uk

Department of Sociology, University of Oxford, Oxford OX1 1DP, UK (DS); University of Cambridge, Cambridge, UK (LK); and London School of Hygiene and Tropical Medicine, London, UK (MM)

- 1 Mills J. Data torturing. *N Engl J Med* 1993; **329**: 1196-99.
- 2 Perlman F, Bobak M. Assessing the contribution of unstable employment to mortality in posttransition Russia: prospective individual-level analyses from the Russian Longitudinal Monitoring Survey. *Am J Public Health* 2009; **99**: 1818-25.
- 3 Aslund Å. How Russia became a market economy. Washington, DC: Brookings Institution, 1995.
- 4 Lieberman I, Kopf DJ. Privatization in transition economies: the ongoing story. Bingley: Emerald Group, 2007.
- 5 Kokh A. The selling of the Soviet Empire. New York: SPI Books, 1998.
- 6 Gujarati D. Basic econometrics. New York: McGraw-Hill, 1995.
- 7 Cornia GA, Paniccia R. The mortality crisis in transitional economies. Oxford: Oxford University Press, 2000.
- 8 European Bank for Reconstruction & Development. Transition report: ten years of transition. London: EBRD, 1999.
- 9 Leon DA, Saburova L, Tomkins S, et al. Hazardous alcohol drinking and premature mortality in Russia: a population based case-control study. *Lancet* 2007; **369**: 2001-09.

Addressing congenital causes of disability

The excellent Comment by Nora Groce and Jean-François Trani (Nov 28, p 1800)¹ highlights the systematic exclusion of people with disabilities from national and international priorities and programmes. Congenital diseases are a significant cause of disability, especially in developing countries, and account for a substantial proportion of deaths in children younger than 5 years²—a fact absent

from mortality statistics. WHO has now recognised the need to reduce these deaths if the Millennium Development Goals (MDGs) are to be achieved.³

Interventions targeted at congenital diseases can reduce the burden of disability. These can be effectively carried out with available methods such as micronutrient fortification, education on risks of advancing maternal age and of drugs and alcohol use in pregnancy, skilled birth attendance, assistive technologies, and community-based rehabilitation. These methods can be largely implemented at primary-care and community levels and as part of existing maternal, child, and general public health and social services. Moreover, rapid advances and increasing accessibility of biomedical technologies are already making a difference, particularly in more developed countries and can have a similar effect in developing countries.

Progress towards the MDGs will need political will, a deeper understanding of the epidemiology of child deaths, and the recognition that, with the declining burden of infectious diseases, congenital diseases are and will become increasingly important causes of childhood morbidity and mortality, especially in developing countries. If nations understand and get prepared to tackle these problems, then the MDGs may indeed be achieved. We still have 5 years, but we need to be quick.

We declare that we have no conflicts of interest.

*Luis Nacul, Sowmiya Moorthie,
Mukesh Kapila, Ron Zimmern
luis.nacul@phgfoundation.org

Foundation for Genomics and Population Health (PHG Foundation), Worts Causeway, Cambridge CB1 8RN, UK

- 1 Groce NE, Trani JF. Millennium development goals and people with disabilities. *Lancet* 2009; **374**: 1800-01.
- 2 Christianson A, Howson C, Modell B. Global report on birth defects: the hidden toll of dying and disabled children. New York: March of Dimes Birth Defects Foundation, 2006.
- 3 WHO. Birth defects: report by the Secretariat. Geneva: World Health Organization, 2009.

The printed journal includes an image merely for illustration