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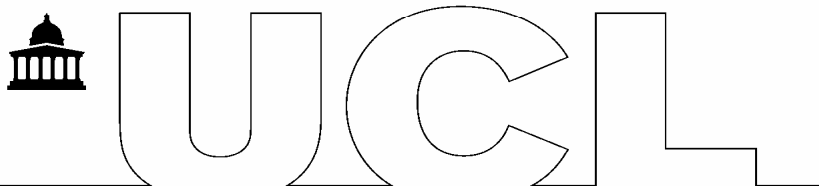

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Did Mass Privatisation Really Increase Post-Communist Male Mortality?

Christopher J Gerry[a] , Tomasz M Mickiewicz[b] , and Zlatko Nikoloski[c]

[a] University College London, UK, email: c.gerry@ucl.ac.uk
[b] University College London, UK
[c] University College London, UK

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Centre for Comparative Economics
UCL School of Slavonic and East European Studies
Gower Street, London, WC1E 6BT
Tel: +44 (0)20 7679 8519
Fax: +44 (0)20 7679 8777
Email: cce@ssees.ucl.ac.uk

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Christopher J Gerry*,

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UCL School of Slavonic and East European Studies, London, UK.
*****Corresponding author: c.gerry@ucl.ac.uk

A summary of this peer-reviewed paper with an associated web-appendix is now published in the *Lancet*, 375(9712):372 and is available at:
http://www.thelancet.com/journals/lancet/article/PIIS0140-6736(10)60158-4/fulltext

Please see the postscript below for further details.

Abstract

A recent article in the *Lancet*, by David Stuckler, Larry King and Martin McKee, investigated anew the fluctuations in adult male mortality rates that have come to characterise the so-called post-communist mortality crisis. Adopting a cross-country, time-series perspective the authors examined how the economic policy strategies of the 1990s impacted upon observed fluctuations in mortality. They conclude that the adoption of a strategy of rapid (mass) privatisation contributed to the adverse mortality trends. We subject that finding to closer scrutiny using the same data from which the Stuckler et al claim stems. We find that their claim that mass privatisation adversely affected male mortality trends in the post-Communist world does not stand up to closer examination. It is not supported empirically and is at odds with what we know about both transition in the post-communist world and about health trends over time in this region.

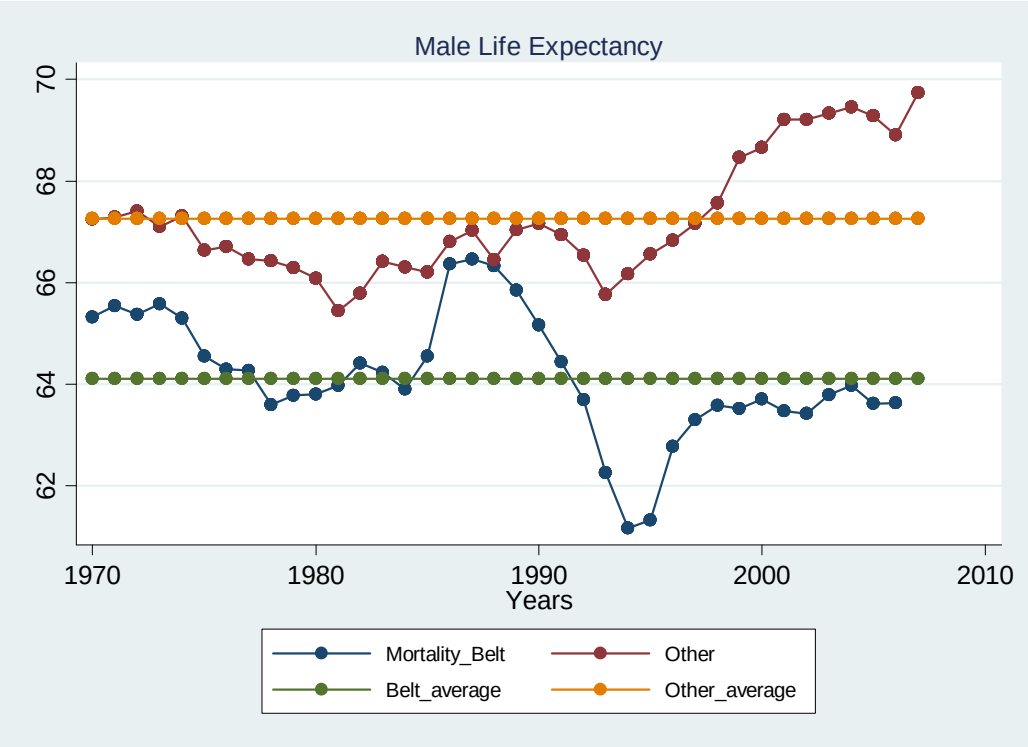
Postscript

A summary of this paper (unanimously recommended for publication by 4 anonymous Lancet referees) with an associated web-appendix is now published in the Lancet (375(9712):372) as a 'Correspondence' piece. It appears alongside work arriving at a similar conclusion by John Earle and Scott Gehlbach. In response, Stuckler et al have also been granted a correspondence piece with an associated web-appendix in which they respond to our findings. Unfortunately, the arguments put forward by Stuckler et al in their response to both our paper and that of Earle and Gehlbach are misrepresentative and muddled. On further examination, we learned from the Lancet Editorial Team that *their* response was ***not*** peer reviewed. While we respect the authors' right to reply, a necessary trademark of academic discussion is that the arguments, views and opinions therein are subject to collective objectivisation. This condition has clearly not been met. We have little interest in personal and even less in ideological exchange, far more in the objective pursuit of what the available data can teach us about the real world. Thus, rather than spend our time responding to their points case by case, we feel it suffice to reiterate the following: *The work of Gerry, Mickiewicz and Nikoloski and that of Earle and Gehlbach was developed independently and iteratively during 2009. It was subject to presentation at multiple academic fora, and most importantly, in both cases, was subject to the external peer review processes of the Lancet. That process of peer review resulted in strong, independent recommendations for publication on the basis that the claims of SKM had been shown not to be robust. The response from SKM, compiled in little over a month, has* ***not*** *been subject to any sort of peer review process by the journal in which it was published*.

1. Introduction[1]

During the 1990s the post-communist countries of Central and Eastern Europe, Central Asia and the Caucasus (CEE) played host to a decade of political, economic and social upheaval. Across parts of the region the 'transition' that followed the decline and ultimate implosion of the command economy system and political disintegration of the Soviet system also coincided with a demographic crisis prompted by steep increases in mortality rates. In Russia, the most extreme and well-documented case, life expectancy for males collapsed from 64.2 in 1989 to 58.3 in 1995. This health 'crisis' though was neither restricted to Russia nor was it restricted to the post-communist period. Right across the so-called 'mortality belt' – the western countries of the former Soviet Union[2] – life expectancy for both males and females fluctuated considerably *both* during the 1980s (a late Soviet period) and the 1990s (the 'transition' period), following a trend decline from the late 1960s. At the same time, despite being subjected to similar economic and political disruption at the start of transition, the post-communist countries of CEE bordering the mortality belt both from the west and from the east, suffered much less substantive decreases in their life expectancy during the 1990s (see figure 1).

Figure 1: Male Life Expectancy



Source: WHO Health for All Database, January 2009

Three things are clear from figure 1. Firstly, the countries of the mortality belt have had lower life expectancy for at least the last 3-4 decades. Secondly, there were substantial fluctuations in life expectancy from the mid-1980s through to the mid-1990s. Thirdly, in the mid-1990s life expectancy in both groups started to rise again, albeit more slowly in the mortality belt countries.[3] Stuckler, King and McKee,[4] writing in the *Lancet*, compile the CEE data for the decade starting in 1990 and using that data find that "mass privatisation programmes were associated with a

short-term increase in mortality rates in working-aged men"[5] and thus argue that the social costs borne by countries adopting rapid privatisation have been far greater.

While welcoming the attempt to examine the role of economic strategies in driving health outcomes, we don't share the view of Bobak and Marmot[6] that "Stuckler and colleagues argue convincingly that the speed of privatisation was an important determinant of mortality changes in the transition" nor that they adequately "tackle the difficulties of measurement and confounding". On the contrary, in this paper, we demonstrate (using their data) that the claim that mass privatisation adversely affected male mortality trends in the post-Communist world does not withstand closer scrutiny. It is not supported empirically and is at odds with what we know about both transition in the post-communist world and about health trends over time in this region.

2. Transition and mortality

Stuckler et al set themselves a difficult task: namely, that of isolating the effect on health of complex policy choices made in a turbulent socioeconomic and political environment, against the backdrop of fluctuating and often deteriorating health outcomes observed since the late 1960s. They are by no means the first to tread this path. As early as 1996, Jeffrey Sachs observed an inverse relationship between life expectancy and the speed of economic reform.[7] Elizabeth Brainerd examined the link between market reforms and mortality patterns and found the evidence to be more mixed.[8] In particular she observed that effective market reforms were associated with both increases (e.g. the Baltic republics) and decreases (e.g. Poland and Slovenia) in mortality rates. While noting that there is no obvious, clearly identifiable explanation for the mortality patterns, she also found that macroeconomic destabilisation (inflation rates), the crime rate, unemployment and economic growth were correlated with mortality rates in the early 1990s. In addition, the particularly strong correlation between deaths from cardiovascular disease and the inflation rate, the crime rate and the unemployment rate suggests that the uncertainity related stress experienced during the upheavals in countries which were less successful in their "transitional" economic policies may have been an important factor in the mortality crisis of the 1990s.[9,10,11]

In more recent research, Brainerd and Cutler[12] review the literature and revisit the data, to explore four potential explanations: the collapse of the health care system[13]; individual lifestyles reflected by diet, smoking and alcohol intake[14]; material deprivation[15] and psychosocial stress.[16] They find that increased alcohol (and surrogate alcohol) use and the stress associated with socio-economic upheaval account for around half of the increase in mortality but that a large residual remains unexplained. McKee and Shkolnikov argue that premature male deaths stem from injuries and violence associated with alcohol consumption; and cardiovascular disease associated with diets high in fat and low in fruit and vegetables.[17] Correspondingly, Zatonski et al claim that the decline in deaths from cardiovascular disease in Poland since 1991 is related to a positive change in diet.[18] Most recently of all, Zaridze et al associate over half of male, 15-54 year old deaths, in three Russian cities with alcohol abuse.[19]

There is then an established literature examining the explanations for the mortality patterns in CEE and a cautious consensus that the observed mortality patterns are a function of alcohol, dietary related factors and stress associated with socio-economic upheaval. There is also some early evidence of a positive link between the aggregate indicators of progress in market reforms and better health outcomes. Stuckler et al, while not denying the potential importance of other factors, are the only authors who endeavour to link the transitional part of the mortality crisis to one very specific policy choice taken in the early 1990s.

3. Statistical analysis[20]

The stylised empirical facts

In their discussion, Stuckler et al provide a graph comparing life expectancy, between mass privatisers and non-mass privatisers, for the years 1991 – 2000 and claim this as further evidence of a causal relationship between choice of privatisation and mortality. That graph is effectively the right hand side section of our figure 1 except that 'mortality belt' is repackaged as 'mass privatisers'. Our figure 1 suggests that, at most, one could argue that those countries that happened to mass privatise are also those that in both historical and contemporary terms have suffered greater fluctuations in, and lower levels of, health. Notwithstanding this, it is instructive to look more closely at the basic temporal association between mass privatisation and mortality. Table 1 details the countries that Stuckler et al classify as mass privatisers, the year in which they are said to have started that process and the year in which their mortality levels peaked.

Table 1: Mass privatisation and male mortality

Country	Year of mass privatisation	Year of peak mortality
Armenia	1994	1993
Czech Republic	1992	1990
Georgia	1995	1993
Kazakhstan	1994	1996
Kyrgyz Republic	1994	1995
Latvia	1994	1994
Lithuania	1993	1994
Moldova	1994	1995
Romania	1995	1996
Russia	1992	1994
Ukraine	1995	1995

Interestingly, in 5 of the 11 countries, the year denoted as the start of mass privatisation either *coincides with or comes after* the peak in male mortality. Using Stuckler et al's data, Figure 2 shows that for both groups there was a steep increase in mortality followed by a decline. The peak in the mass privatisers mortality rate comes in 1994, by which time, only Lithuania, Russia[21] and the Czech Republic can be considered to have meaningfully commenced mass privatisation in a way that could plausibly show through in the mortality data, and in the latter case mortality had long since peaked.

Figure 2: Log male mortality rates

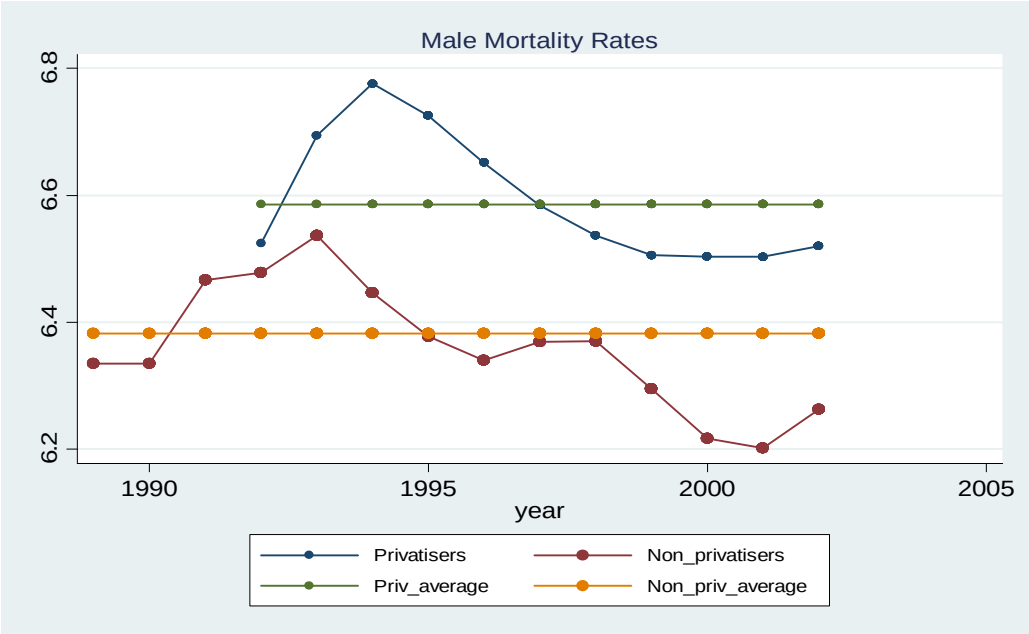
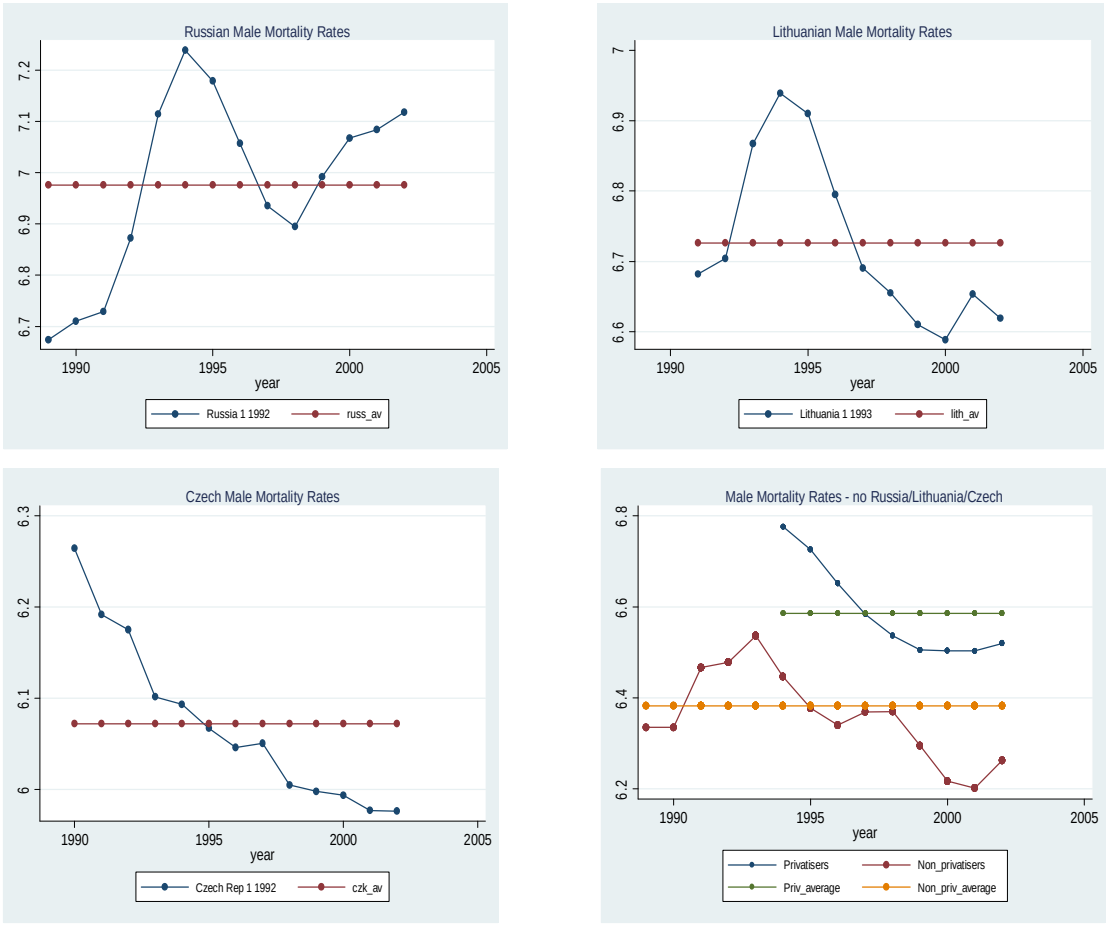


Figure 3 isolates the trends for each of these 3 countries (Russia, Lithuania and Czech Republic) for whom mortality peaked after mass privatisation and for the overall trend among mass privatisers once those 3 countries are removed. By the time that mass privatisation could have plausibly impacted upon mortality rates in Russia and Lithuania, mortality was coming towards the end of its steep rise, while in the Czech Republic it was already well into a steep decline and continued despite mass privatisation. Once these countries are removed from the aggregate analysis, Figure 2 paints an even clearer picture. That is, on average, countries embarking on mass privatisation in 1994 or later enjoyed an immediate decline in male mortality. Indeed, the stylised empirical fact is simple: in those countries that undertook it, mass privatisation either takes place near the end of, at the end of or after the end of a significant and well-established increase in male mortality. This is a reality that is very difficult to tally with the claims of Stuckler et al.

Figure 3: Log male mortality in Russia, Lithuania, Czech Republic and mass privatisers without Russia, Lithuania and Czech Republic



4. The Econometrics

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 mortality. We therefore devote a little time to discussing their data and approach to estimation. Stuckler et al present results of the following model, estimated through a pooled OLS technique, adjusted for the presence of both serial correlation and correlation between countries:

$$AMR_{it} = \alpha + \beta 1PRIV_{it} + \beta 2GDP_{it} + \beta 3LIB_{it} + \beta 4TRADE_{it} + \beta 5DEM_{it} + \beta 6DEP_{it} + \beta 7WAR_{it} + \beta 8URBAN + \beta 9EDUC_{it} + \mu i + \varepsilon it$$

The subscripts *i* and *t* refer to country and time; AMR refers to the log of the adult male mortality rate, PRIV is the privatisation measure (we use their dummy variable in this paper), GDP relates to log GDP per head in current $US, LIB is the EBRD price liberalisation index, TRADE is the EBRD foreign exchange and trade liberalisation index, DEM is the Freedom House democratisation index, DEP is the dependency ratio, WAR is their World Bank derived dummy for military conflict, URBAN is the percentage of population in urban settings and EDUC is the, World Bank sourced, percentage of population with tertiary education.

A number of the variables in their data are problematic. First, their proxy for the level of development (GDP per capita) is denominated in current US$ rather than international $ adjusted for purchasing power parity. This is a mistake – it is *essential* to use purchasing power parity $ measures in cross-country regressions that estimate health outcomes. Second, the choice of the EBRD variables (LIB and TRADE) is arbitrary and runs against the accepted norm in the transition literature[22]: it is well established there that due to serious multicollinearity between the reform indicators and the risk of ommitted variable bias the preferred approach either calls on the aggregate EBRD index or simply uses one of the sub-indices as a proxy for reforms. Indeed, in this case, using either LIB or TRADE produces a significant result, further suggesting that multicollinearity is a relevant point to consider in this case. [23] Third, the World Bank data they use to proxy for education is highly problematic and known to be of low quality. Indeed, choosing alternative World Bank education proxies directly reverses the observed direction of association.

Amplifying the effects of these data use choices, Stuckler et al seem to overlook the importance of the data coverage itself. Although the estimation techniques that they use to derive their results are able to handle unbalanced panel data, the assumption that the gaps in the data are random is still relied upon, and yet it is unlikely that this holds in reality. Examining their panel we observe that for 6 countries the first year observed is 1989, while for the remainder the first observation varies from 1990 to 1993. It is most unlikely, as our figure one suggests, that this unbalancing of the panel is without consequence.[24] Additionally, the main results discussed in their paper are based on a pooled OLS model and yet there is neither discussion nor testing of the appropriateness of pooling this data.[25]

Turning to the estimates themselves, Stuckler et al reduce to Webappendix 2 their discussion of other confounding explanations for the observed mortality patterns. While they appear to explore the role of health service variables, they neither discuss the problems surrounding these variables in the post-communist context nor do they give consideration to the consumption of demerit goods such as tobacco and alcohol. Indeed, as Leon et al note "Although factors such as nutrition and health services may be involved, the evidence is that substantial changes in alcohol consumption over the period could plausibly explain the main features of the mortality fluctuations observed".[26] Reliable aggregate data on health service characteristics and on consumption behaviour are difficult to identify but some attempt needs to be made to control for the factors that might explain the long run trends that pre-date the transition (and privatisation) period. Moreover, the authors' assertion that by using fixed effects models they control for "pre-existing societal characteristics and predispositions to higher mortality" is simply wrong. While the fixed effects of these variables are indeed netted out in their *effects* models, any time-varying or interactive effects are still swept into the error term.

This observation leads us to our most substantive criticism – their choice of estimator. Despite all the evidence and intuition suggesting that they should, they fail to treat mortality as a dynamic variable with the characteristic traits of persistence. Indeed, all of their presented estimates are static and ignore several likely reasons for correlation over time in the dependent variable. First, it can't be other than that mortality in *t* is influenced directly by mortality in *t-1* (true state dependence explanation). If, for example, mortality this year is caused by low vaccination rates 20 years ago then it follows that, for the same reason, mortality next year will be caused, in part, by the same factor. Second, mortality is affected directly, over time, through the explanatory variables (observed heterogeneity explanation). In the case of mass privatisation, for example, it is much more probable that mass privatisation last year would impact on mortality this year, rather than the impact being contemporaneous. Finally, if important variables are omitted, as explained above, then there could be a high individual fixed effect in each period that distorts the

results (unobserved heterogeneity explanation). None of their specifications are dynamic; while they use the Prais Winsten correction for serial correlation in one of their specifications, recognising that there is serial correlation in the error term, they fail to take into account that, if the process is dynamic, then none of their OLS, fixed effect, random effects, Prais-Winsten or first differenced estimates are consistent.[27] The second reason we take issue with their estimator also relates to the nature of the relationships they discuss. Despite explaining in their text that GDP per head is endogenous to health outcomes, they fail to attempt to attenuate the bias that this imparts upon all estimated coefficients and standard errors. Using an instrumental variables approach within a dynamic panel setting allows us to attenuate this endogeneity induced bias.

5. Revisiting the results

We support the criticism presented above with a series of specifications, *using Stuckler et al's data*; the results are presented in Tables 2 and 3 below. Our intention is not to make claims regarding what does actually explain the observed mortality patterns. In that sense, we are less ambitious than Stuckler et al. Rather, using their data, we simply set out to demonstrate that the association they observe between mass privatisation and mortality is a spurious one. Table 2 examines the effects of correcting their specifications in a step-wise manner, while Table 3 demonstrates the effect of using a dynamic panel estimator which allows for endogeneity and time persistence in the dependent variable.

In Table 2, the first column replicates the key results obtained by Stuckler et al to which they refer in the main text of their paper.[28] Columns 2 and 3 show the results after using the correct measurements and reducing what we consider to be overspecification, while column 4 adds the initial mortality rate into the model. Columns 2-4 are important to the extent that they improve the fit of the model and indeed lower the number of deaths implied by the mass privatisation coefficient. They don't though challenge the main qualitative claim of Stuckler et al. To achieve the latter we recall our argument above that the impact of reforms on health inevitably comes with some delay. For example, a mass privatisation programme introduced in December 1992 and completed in June 1994 is unlikely to have had any effects on Russia's 1992 mortality rates (as implied by Stuckler et al's specifications). In general, programmes introduced mid-year are more likely to have effects in the following year.[29] Column 5 therefore introduces lags for those variables that cannot reasonably be argued to have an immediate impact on mortality rates. This column is a gesture towards a more dynamic framework and it immediately *removes the significance of mass privatisation* as an explanatory variable. Column 6 confirms this by using the column 2 specification amended only for a lag in the mass privatisation variable. That is, the inclusion of a simple lag in the key policy variable shows that mass privatisation last year does not have any statistical impact on male mortality rates this year.[30] Stuckler et al's headline story simply does not stand up to careful interrogation.

Table 2: Dependent variable, log male mortality rates, as in Stuckler et al (2009)

Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)
Dummy for mass privatisation	0.128*** (0.025)	0.114*** (0.026)	0.102*** (0.025)	0.102*** (0.025)		
lagged one year					0.028 (0.030)	0.052 (0.029)
Log GDP per head	-0.119*** (0.028)					
Log GDP per head (ppp)		-0.160* (0.066)	-0.198** (0.070)	-0.198** (0.070)		-0.241** (0.086)
lagged one year					-0.176* (0.079)	
EBRD Price liberalisation index	0.006 (0.013)	0.000 (0.013)	-0.008 (0.013)	-0.008 (0.013)		0.002 (0.014)
lagged one year					0.015 (0.013)	
EBRD Forex and trade lib index	0.009 (0.013)	0.005 (0.005)				0.017 (0.011)
Freedom House democracy indicator	-0.010 (0.006)	-0.009 (0.006)				-0.015* (0.006)
Military or ethnic conflict	0.224*** (0.049)	0.207*** (0.050)	0.200*** (0.051)	0.200*** (0.051)	0.223*** (0.050)	0.197*** (0.045)
Population dependency ratio	0.017*** (0.004)	0.017*** (0.005)	0.018*** (0.005)	0.018*** (0.005)	0.023*** (0.005)	0.016** (0.005)
% Pop with tertiary education	-0.003* (0.001)	-0.002 (0.001)				-0.002 (0.001)
% Pop in urban settings	0.032*** (0.009)	0.028** (0.009)	0.024* (0.011)	0.024 (0.011)*		0.028** (0.010)
Initial period mortality rate				0.818 (0.201)***	0.775*** (0.216)	
Rho	0.356	0.426	0.461	0.461	0.437	0.397
No of country-years	289	286	286	286	263	263
No of countries	24	24	24	24	24	24
Wald Chi squared	192023	91134	211535	459258	179017	17653

Notes: All estimations are pooled OLS with errors corrected for AR(1) serial correlation and heteroskedasticity and with country dummies included. Robust panel corrected standard errors in brackets; * p<0.05; ** p<0.01; *** p<0.001.
Column (1) refers to the main results of the Stuckler et al paper; (2) replaces their GDP variable with the correct version; (3) removes highly correlated democracy (with GDP), EBRD Forex indicator and education; (4) adds a control for initial conditions and (5) presents the same but with lagged explanatory variables; (6) presents (2) but with a lagged mass privatisation variable.

However, given the dynamic and potentially endogenous nature of the relationships we need to incorporate the process of dynamic adjustment explicitly in order to obtain consistent estimates, as explained above. For that reason we next adopt the Blundell and Bond General Methods of Moments approach ('system GMM').[31] This estimator achieves consistency, including for unbalanced panels, through adopting an instrumental variables technique in which the lags of the included variables provide the instruments. These estimates further highlight the deficiencies of an approach that ignores the mortality patterns prior to and during mass privatisation. In each of our dynamic specifications, the coefficient of the lagged dependent variable indicates a level of persistence hard to ignore without introducing bias.[32]

Column 1 of Table 3 presents the dynamic version of the Stuckler et al model and confirms that mass privatisation is not a significant determinant of male mortality. Columns 2 and 3 speculate on possible extensions of this. In particular, column 2 treats GDP as endogenous, introduces

initial conditions as predetermined and removes the variables identified in the earlier discussion as problematic. Column 3 repeats that exercise but, in the spirit of our table 2 findings, uses lagged independent variables to allow explicitly for the possibility that changes in key variables in the last period influence mortality. [33] In neither case is mass privatisation's contribution to mortality significantly different from zero.

The results presented in tables 2 and 3 highlight five key findings: first, mass privatisation does not explain the observed fluctuations in male mortality; second, the most likely explanation for Stuckler et al's claim is that they ignore the dynamics that must underpin any association between policy and mortality; third, the other variables in their specification are also unable to explain fluctuations in mortality; fourth, there is evidence, concordant with Sachs (1996), that liberalising reforms actually served to *reduce* mortality during transition;[34] and fifth, higher initial mortality levels were associated with higher increases, as suggested by Figure 2.

Table 3: Dynamic GMM Estimations.

Explanatory Variables	**(1)**	**(2)**	**(3)**
Lagged Dependent variable	0.882*** (0.070)	0.881*** (0.095)	0.918*** (0.074)
Dummy for mass privatisation	0.040 (0.027)	-0.002 (0.029)	
lagged one year			0.011 (0.031)
Log GDP per head (ppp)	-0.002 (0.016)	-0.049 (0.044)	
lagged one year			0.028 (0.031)
EBRD Price liberalisation index	-0.022 (0.014)	-0.027** (0.010)	
lagged one year			-0.026*** (0.007)
EBRD Forex and trade lib index	-0.028*** (0.009)		
Freedom House democracy indicator	0.002 (0.004)		
Military or ethnic conflict	-0.014 (0.024)	-0.010 (0.029)	
Population dependency ratio	0.001 (0.002)	-0.001 (0.002)	0.001 (0.002)
% Pop with tertiary education	0.002 (0.001)	0.003 (0.003)	
% Pop in urban settings	0.001 (0.002)		-0.000 (0.002)
Initial period mortality rate		0.083 (0.093	0.148* (0.085)
No of country-years			
No of countries	261	261	261
Wald Chi squared	24	24	24
Arellano-Bond test for AR(2): z	4317***	2130***	1085***
Hansen test: chi squared	0.528	0.462	0.399
	16.9	19.0	21.2

Notes: System GMM estimates, standard errors in brackets; * p<0.05; ** p<0.01; *** p<0.001.
Column (1) refers to the dynamic estimate of Stuckler et al's main results, using the correct GDP measure; (2) treats GDP as endogenous and introduces initial conditions; (3) explores the same specification with lags.

6. Conclusion

There is 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 regions mortality patterns are not consistent with the claim, and the claim itself does not stand up to econometric analysis. The contrary finding, reported by Stuckler et al, stems from their ignoring of the dynamics and persistence that necessarily underpins any such relationship. Even using their data (which as we indicated is problematic) and their specification, the inclusion of a simple one period lag, removes the association. Incorporating a more explicitly dynamic estimation, there is still no link between mortality and mass privatisation and moreover, there is some evidence that it was not privatisation but liberalising reforms that had an impact on health and that such reforms were actually associated with lower mortality.

However, we make no claim that the specification in column (3) of Table 3 provides ultimate proof about the factors that affected mortality in transition countries in the 1990s and early 2000s. Such a claim would require further research, richer data allowing for a more precise attempt to capture the behavioural elements of health determinants, and (preferably) corroboration with micro-level data. Relying on cross-country studies with just 24 units of observation renders any such claim highly speculative. However, for a group of countries that went through such a distinctive time pattern of health outcomes, socio-economic performance, and reforms, with economic and health results deteriorating over the late communist period and early transition period and then improving later on, albeit not everywhere, ignoring the dynamic aspects of the data comes at a seriously high cost in the form of mistaken conclusions. Yet 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.

Finally, we allow ourselves a little speculation consistent with our estimates. While privatisation could lead to a reduction in employment in privatised companies, one has to remember that some of those initial jobs were value-destroying[35] and arguably a much more important issue pertained to whether jobs were generated in the newly emerging private sector. This being so, the question of liberalisation and freedom of entry may well be a more important one than the question of privatisation, when trying to understand individual level health and socioeconomic outcomes.

References

[1] We thank conference and seminar participants from Tartu and Svetlana Makarova, Carmen Li and Liubov Borisova for constructive comments in drafting the paper and in particular Judith Shapiro for useful discussions and advice throughout. We are also grateful to the constructive and supportive suggestions of the Lancet's referees who unanimously supported this paper for publication.

[2] The mortality belt comprises of Belarus, Estonia, Latvia, Lithuania, Moldova, Russian Federation and Ukraine. The 'other' category (using Stuckler et al's data) therefore consists of Albania, Armenia, Azerbaijan, Bulgaria, Croatia, Czech Republic, Georgia, Hungary, Kazakhstan, Kyrgyz Republic, Macedonia, Poland, Romania, Slovakia, Tajikistan, Turkmenistan and Uzbekistan.

[3] The sharp increase in life expectancy in 1986 is generally attributed to the anti-alcohol campaign introduced in May 1985. See Shapiro Judith. The Russian Mortality Crisis and its causes, in *Russian Economic Reform in Jeopardy?* Aslund Anders, ed. London and New York: Pinter Publishers. 1995; 149-78; Shkolnikov Vladimir, Nemtsov, A. The Anti-alcohol campaign and variations in Russian mortality. In *Premature Death in the New Independent States*, eds. Bobadilla, J.L. Costello, C. A. And Mitchell, F. Pp. 238-252. National Academy Press, Washington, D.C.

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[5] *Ibid*. p. 404.

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[20] Unless otherwise stated all empirical evidence is based on the exact data used by Stucker et al (2009) and detailed therein. We are grateful to the authors for making their data available.

[21] Classifying Russia as a 1992 mass privatiser is also technically a mistake. While the mass privatisation programme was announced in December 1992 it only got meaningfully underway in 1993 and was completed in 1994.

[22] See: Elisabetta Falcetti, Tatiana Lysenko, Peter Sanfey. Reforms and Growth in Transition: Re-examining the evidence. *Journal of Comparative Economics*. 2006: 34(3): 421-445, and further discussion in Tomasz Mickiewicz. *Economic Transition in Central Europe and the Commonwealth of Independent States*. 2005 (Houndmills: Palgrave Macmillan), Chapter 10.

[23] There are other colinearities among their selected variables e.g. between GDP per capita and democracy (see their Webappendix 1) which, although to some extent inevitable in this kind of study, merit further comment.

[24] In addition, it is not clear why Slovakia is unobserved until 1993 or why Slovenia is missing completely or indeed why Russia is observed from 1989 but Ukraine is only observed from 1991. In none of these cases is it a matter of data availability.

[25] Our own tests with their data show that pooling Commonwealth of Independent States (CIS) and non-CIS data is not appropriate.

[26] Leon David, Chenet Laurent, Shkolnikov Vladimir, Zakharov S, Shapiro J, Rakhmanova S, Vassin S, McKee M. Huge Variation in Russian Mortality Rates 1984-1994: Artefact, Alcohol or what? *Lancet*. 1997; 350: 383-88.

[27] In their Webappendix 2 they report having conducted some dynamic panel estimates. However, on the one hand they do not present or discuss these results and on the other hand, these are not robustness checks – either the process is dynamic or it is not. As it turns out the data confirms our prior expectations that the process is dynamic and thus none of the results that they actually present are appropriate.

[28] See their Webappendix 2, column 5.

[29] Russia is the most spectacular but not the only mistake in the dataset on privatization used by Stuckler et al; see, for example, details on the Russian privatization programme timeline in Blasi J M, Kroumova and Krouse D. *Kremlin Capitalism. Privatising the Russian Economy*. 1997; Ithaca: Cornell University Press. Specifically, by shifting the privatization dates backwards Stuckler et al generate a stronger empirical association with the crisis. Nevertheless, though we believe that the way in which the privatisation variable is constructed adds to the problems in their analysis, we don't consider it to be the main error in their approach and so continue to use their adopted classifications.

[30] Using alternative lag lengths does not alter this observation.

[31] Blundell Robert W and Bond Stephen R. Initial Conditions and Moment Restrictions in Dynamic Panel Data Models', *Journal of Econometrics*. 1998; 87: 115-143.

[32] In our GMM estimates we follow Roodman (2006) in using lags 2 and deeper for the transformed equation and lag 1 for the levels equation. This is standard for endogenous variables, see Roodman David. How to do xtabond2: An Introduction to "Difference" and "System" GMM in Stata. *Center for Global Development. Working Paper Number 103*. 2006; December. We then check the validity of our results by limiting the lag structure and collapsing the number of instruments. The results are robust to different combinations and it is the latter presented in table 3. Our results are also robust to the Windmeijer (2005) twostep finite sample correction. See Windmeijer Frank. A finite sample correction for the variance of linear efficient two-step GMM estimators. *Journal of Econometrics*. 2005: 126(1):25-51.

[33] We also check these results using the Stuckler et al specification from column 1 but with the simple addition of the lagged version of the mass privatization variable. Our findings are confirmed.

[34] It makes no difference to the results whether we use price liberalization, trade liberalization or the EBRD aggregate index in these specifications.

[35] On evidence of widespread inefficient, energy-costly and value-destroying employment at the initial point of transition, see Hughes G and Hare P. Industrial Policy and Restructuring in Eastern Europe, *Oxford Review of Economic Policy*. 1992; 8: 82-104. See also Mickiewicz Tomasz. *Economic Transition in Central Europe and the Commonwealth of Independent States*. 2005; Houndmills: Palgrave Macmillan. See also Earle, John. Mass privatisation and mortality, *The Lancet*. 2009; 373 (9671): 1247-1247.