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Mass Privatisation and the Post-Communist Mortality Crisis: Is There Really a Relationship?

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ABSTRACT

We reexamine the recent, well-publicized claim that "rapid mass privatisation [of state-owned enterprises]…was a crucial determinant of differences in adult mortality trends in post-communist countries" (Stuckler, King and McKee, 2009). Our analysis shows that the estimated correlation of privatization and mortality in country-level data is not robust to recomputing the mass-privatization measure, to assuming a short lag for economic policies to affect mortality, and to controlling for country-specific mortality trends. Further, in an analysis of the determinants of mortality in Russian regions, we find no evidence that privatization increased mortality during the early 1990s. Finally, we reanalyze the relationship between privatization and unemployment in postcommunist countries, showing that there is little support for the proposed mechanism by which privatization might have increased mortality.

JEL Classification Codes: I18, L33, P2, P31, O57

Key Words: privatization, mortality, health, shock therapy, unemployment, Eastern Europe, Former Soviet Union, Lancet

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1 Introduction

In the early 1990s, the postcommunist countries of Eastern Europe and the former Soviet Union initiated changes that would fundamentally transform their political and economic institutions. Coincident with these changes, many countries in the region experienced a dramatic increase in mortality, especially among working-age males. Various studies have investigated this mortality spike, but debate continues on the underlying causal mechanisms.[1]

In a recent, well-publicized contribution to this literature, David Stuckler, Lawrence King, and Martin McKee (henceforth SKM) argue in *The Lancet* that "rapid mass privatisation [of state-owned enterprises]... was a crucial determinant of differences in adult mortality trends in post-communist countries" (Stuckler, King and McKee, 2009, p. 1).[2] The evidence offered in support of this claim consists of country-level regressions of the adult male mortality rate on measures of enterprise privatization. The authors' primary interpretation is that rapid privatization increased unemployment and consequently illness, for which they offer support from country-level analysis of unemployment. The estimated effects of mass privatization on both mortality and unemployment are reported to be positive and statistically significant only in the former Soviet Union; no evident effects are reported among Central and East European countries.

The publication of SKM's study has reignited debate over the effects of economic reform. The provocative tone of the *Economist* leader on the topic—"Mass Murder and the Market"—reflects the stakes involved: should reformers be held responsible for millions of premature deaths?[3] Jeffrey Sachs, an architect of "shock therapy" in postcommunist countries, suggests that the study is a "confused polemic that will not withstand serious epidemiological scrutiny."[4] On the other side, Joseph Stiglitz argues that "[The] *Lancet* is right that Poland was an example of more gradual policies"—the study credits gradualism with reduced mortality—and reasserts his view that "'shock therapy' was a disastrous economic policy."[5]

Did mass privatization increase mortality in postcommunist countries? In this paper, we reexamine this relationship. We find that the estimated effect of privatization in cross-country data is not robust to recomputing the mass privatization measure using original source data, to assuming short lags between economic policies and changes in mortality rates, and to controlling for country-specific mortality trends. We also examine the relationship between privatization and mortality across Russian regions, finding no evidence that privatization was responsible for the large increase in Russian mortality during the early 1990s. Finally, we reanalyze the relationship between privatization and unemployment. Counter to the

[1] Cornia and Paniccià (2000), Shkolnikov et al. (2004), and Stillman (2006) provide extensive reviews. Recent studies that investigate the impact of particular policies and societal characteristics include Brainerd and Cutler (2005), Bobak et al. (2007), Treisman (2008), and Denisova (2009).

[2] All page numbers refer to the online version of SKM.

[3] "Mass Murder and the Market," *Economist*, January 22, 2009.

[4] "'Shock Therapy' Had No Adverse Effect on Life Expectancy in Eastern Europe," Jeffrey D. Sachs, letter to the *Financial Times* published January 19, 2009.

[5] "Stiglitz on Death and Privatization in the Eastern Bloc," *New York Times* Economix blog, January 16, 2009.

claims of SKM, there is no robust evidence that privatization increased unemployment in postcommunist countries.

2 Cross-Country Analysis

The results in SKM raise a number of questions concerning sample, data, definitions, and methods: Are the measures of privatization appropriate? Are any effects of privatization on mortality instantaneous? Are there no pre-existing trends in the data? How much can one conclude from a sample of only 15 countries (the FSU sample), and is country the right level of analysis? More generally, is the privatization coefficient identified, statistically distinguishable from other factors that may have affected mortality? And what is the causal mechanism that might link mortality with privatization? We address these questions through a series of related research designs.

We begin by reexamining the cross-country correlation between mortality and privatization. The evidence put forward by SKM is based on an unbalanced panel of 24 countries observed annually from 1989 to 2002, but a positive impact of privatization on mortality is estimated only for the 15 former republics of the Soviet Union (FSU), not for the 9 countries of Central and Eastern Europe (CEE). We therefore focus attention on the former group; results for CEE are available in Table A1 in the Appendix.

We first perform pure replications using the SKM regression specifications. The dependent variable in SKM and our replications is the natural log of the age-standardized mortality rate for males aged 15–59. SKM use two alternative measures of privatization in different specifications: a "mass privatization" indicator and the "average EBRD privatization index," the latter the average of two widely used measures of progress in privatization published in the annual *Transition Report* of the European Bank for Reconstruction and Development (e.g., EBRD, 2007). We discuss both measures further below. In all specifications, SKM include country fixed effects, and they control for various time-varying country characteristics: log income, price liberalization, foreign exchange/trade liberalization, democracy, war, population dependency, urbanization, and higher education.

Columns (1) and (2) of the first row of estimates in Table 1 present these replications. The results are very similar to those published in SKM. There is a positive estimated impact of privatization using both measures in the FSU sample. As in SKM, the estimated impact is zero in CEE (as shown in Table A1). Results for the control variables are not reported in SKM, so we suppress them here as well (they are available on request).

We next use original source data to reexamine the SKM privatization variables. (With the exception of these variables, we continue to use the SKM data in all specifications.) Beginning with the mass privatization indicator, the SKM definition is "a programme that transferred the ownership of at least 25% of large state-owned enterprises to the private sector in 2 years. . . 0 before mass privatisation, 1 thereafter," measured as "a jump from 1 to 3 on the EBRD large-scale privatisation index" (p. 2). The coding of the SKM variable is sometimes inconsistent with this definition, however, as when the rise from 1 to 3 took more than two years but the SKM variable is coded as 1. Furthermore, the SKM description of timing is ambiguous: at what point during the period the EBRD index is changing should

the indicator change from 0 to 1? The SKM variable is again inconsistent, but it seems most reasonable to code the mass privatization indicator as 1 from the year the index reaches 3. We use a recoded indicator that incorporates these two changes; details are available in Tables A2–A4.

Results for a regression with the recoded mass-privatization indicator are shown in column (3) of the first row of Table 1. The estimated effect on mortality is much smaller and only weakly significant. Further, as Table A1 shows, the estimated effect of privatization on mortality is now large and negative in Central and Eastern Europe. These results greatly undermine the case that enterprise privatization raised mortality in postcommunist countries. Giving SKM the benefit of the doubt, one could point out that designations of "mass privatization" are subjective, possibly differing among knowledgeable observers. SKM's description of their indicator might be incorrect or oversimplified. In any case, the results are clearly quite sensitive to the coding of this variable.

We also use original source data to disaggregate the other SKM measure, the average EBRD privatization index, into its large-scale and small-scale components. The former refers to large industrial enterprises and the latter to small establishments in trade and services, farms, land, and housing. Yet all the article's arguments refer to large firms: for instance, "[t]he results would be more severe for employees of large-scale capital-intensive heavy industry and manufacturing enterprises..." (p. 2). The SKM emphasis on large privatization is also implicit in the use of only the EBRD large privatization index to construct the mass privatization indicator.

Replacing the SKM average with the large and small indices in separate regressions produces the results in columns (5) and (6) of Table 1. Interestingly, the estimated coefficient on each component is smaller than the coefficient on their average. Moreover, the estimated coefficient on small privatization is larger than that on large privatization, suggesting that both variables may be picking up some other aspect of transition that is associated with mortality.

Finally, we consider issues of regression specification. Many questions could be raised about the SKM specification, but we restrict attention to two: timing and trends. First, SKM assume that the impact of privatization on mortality is immediate, but it seems more likely that any impact would occur with a lag. Certainly this is the case if the causal mechanism is the one adduced in the article: privatized firms shed workers, who in turn become unemployed and unhealthy. Second, the SKM specification also assumes that trends in mortality are equal for all countries. As Figure 1 illustrates, however, adult male mortality trends are quite different across these countries.[6] Indeed, male mortality in most of the Soviet republics declined in the early 1980s, reaching a minimum around 1986–1987, and then began a steep rise, accelerating in some cases in the early 1990s, then declining and reverting to the post-

[6]The adult mortality data in the figure are drawn from the World Health Organization (2008), as they are available for a longer time period than the Unicef data used in SKM and the rest of this paper. We use the world standard population to weight mortality rates for ages 15–24, 25–34, 35–44, and 45–54. Thus, the age range is slightly different from the Unicef data, which include males up to age 59. But both the levels and trends in the data are very similar across the two sources for the years in which they are available in both.

1986 trend by the mid-1990s. The SKM comparison of mortality rates before and after mass privatization reflects these trends, which began well before the fall of communism (see also Stillman, 2006) and are statistically significant even within the SKM sample of years.[7] Given these questions about timing and trends, we therefore check the robustness of the results to lagging the privatization and other economic variables and to inclusion of country-specific linear time trends.

The specifications in the second and third rows of Table 1 lag the privatization and other economic variables by one and two years, respectively. Lagging by just one year substantially attenuates the original estimates and reduces their statistical significance. Lagging by two years further reduces the estimated coefficients and in four of five cases eliminates their statistical significance entirely. As shown in Table A1, with two-year lags three of the five CEE coefficients are statistically significant, but negative, implying that privatization lowered rather than raised mortality rates in these countries.[8]

The specification in the fourth row of Table 1 adds country-specific linear time trends. This small change substantially reduces both the magnitude and statistical significance of the estimated effect of privatization on mortality. Combining country-specific trends with one-year lags (the fifth row of Table 1) eliminates any statistically significant effect of privatization on mortality. Combining trends and two-year lags (the sixth row of Table 1) results in only negative coefficients, three of them statistically significant.

While the correct functional form for the privatization-mortality relationship is unknown, these results show that small, reasonable changes in variable measurement or specification yield substantially different conclusions on the magnitude and even sign of this relationship. We conclude that the positive estimated effect of privatization on mortality reported in SKM is not robust.

3 Privatization and Mortality in Russian Regions

We next turn to an alternative research design, examining the relationship between privatization and mortality across regions within Russia, perhaps the country with the best-known privatization program. This within-country approach has the advantage of holding constant many features of the economic, political, and social environment that could be correlated with privatization and mortality.[9] At the same time, we can exploit substantial variation across regions in the extent of privatization and in changes in mortality rates during the early transition period.

[7] An F-test on country-specific trends in a regression using data through 1993—the pre-privatization years in the data—produces a statistic significant at the 0.02 level. For an early suggestion that the results in SKM might not be robust to controlling for trends, see "Smertnost' v Rossii skvoz' prizmu privatizatsii," *Demoskop Weekly*, February 2–15, 2009.

[8] The tables report results from specifications that drop the first one and two observations for each country, respectively, when lagging by one and two years, but the estimates are very similar if we instead use original source data to back-fill variables.

[9] Ivaschenko (2005), Treisman (2008), and Walberg et al. (2009) employ a similar research design in investigations of mortality in Russian regions, but none explore the impact of privatization.

The Russian State Statistics Service (Rosstat) provides regional data on mortality. Unfortunately, however, the mortality rate for working-age men (defined as deaths per 100,000 men aged 16 to 60), the focus of SKM and most work on mortality in postcommunist countries, is not available for the years 1991–1993. Given that mass privatization in Russia was implemented between late 1992 and mid-1994, we therefore examine determinants of change in (the log of) the mortality rate for working-age men from 1990 to 1995, regressing this variable on measures of privatization and other regional characteristics; we obtain qualitatively similar results if we use the change from 1990 to 1994. We also consider changes in mortality rates for six major causes of death: infectious diseases; cancer; diseases of the circulatory, respiratory, and digestive systems, respectively; and "external" causes of death, including accidents, homicides, and suicides. Finally, as a check on these results, we estimate panel models where the dependent variable is (the log of) the mortality rate for the *general* population, which is available for all years during the period of interest. The pairwise correlation between change in mortality for working-age men and change in mortality for the general population is 0.88.

Figure 2 depicts change in mortality rates for working-age men from 1990 to 1995. Mortality rates increased in every region in Russia during this period. Dagestan experienced the smallest change, with an increase in mortality from 479 deaths per 100,000 working-age males in 1990 to 550 in 1995. The largest change was recorded in Sakhalin, where mortality increased from 758 deaths per 100,000 working-age males in 1990 to 1,729 in 1995. Many of the regions with the largest increases are concentrated in the northern part of European Russia, a historically more developed and urbanized area of the country.

To examine the relationship between privatization and change in mortality, we use two measures of employment in privatized firms. The first, provided by Rosstat, is the proportion of employment in firms with mixed state-private ownership. Because the state retained a residual share in nearly every firm privatized through mass privatization, this corresponds closely to privatized employment. (In contrast, fully private firms are in most cases *de novo* enterprises.) For our cross-section regressions, where the dependent variable is change in mortality from 1990 to 1995, we use data from 1995, the first year available.

We constructed the second measure, privatized manufacturing employment, from industrial-registry data on manufacturing enterprises collected by Rosstat and used in Brown, Earle and Gehlbach (2009) to estimate regional productivity effects of privatization. As summarized in that paper, these data are quite comprehensive, corresponding roughly to the "old" sector of manufacturing firms (and their successors) inherited from the Soviet system. For our cross-section regressions, we use ownership and employment data from 1994 to calculate the proportion of manufacturing employment in firms privatized to domestic owners. Both this and the Rosstat measure exhibit substantial variation, with standard deviations of 13% and 7%, respectively, versus means of 81% and 22%. Figure 3 depicts the geographic distribution of the first of the two measures.

In our cross-section analysis, we control for various regional characteristics that may be correlated with both changes in mortality and privatization outcomes. In addition to regressors similar to those in SKM, proportion Muslim (Heleniak, 2006) is included because regions with large Muslim populations may have been less affected by changes in the price and avail-

ability of alcohol, a leading explanation for changes in mortality rates (Leon et al., 1997; Brainerd and Cutler, 2005; Leon et al., 2007; Treisman, 2008; Zaridze et al., 2009). Mean January temperature is also included, as conditions may be different in inhospitable regions populated forcibly during the Stalinist era.

Table 2 reports results from OLS regressions of initial mortality and change in mortality on various regional characteristics, including our two privatization measures. For purposes of this paper, the primary finding is the uniform absence of any evidence that privatization increased mortality for working-age men. The point estimate of the privatization effect is in fact negative in every case, and it is statistically significant when privatization is defined as privatized manufacturing employment. This holds regardless of whether initial mortality is included among the regressors.

Table 3 presents regressions of the change in mortality rate by cause of death on our two privatization measures and the same regional characteristics used in columns (2) and (4) of Table 2; we obtain similar results from regressions where initial mortality is included as a regressor. Out of twelve regressions, the estimated effect of privatization on mortality is positive and close to significant at conventional levels ($p = 0.101$) only when the dependent variable is change in mortality from cancer and privatization is measured as privatized manufacturing employment. (When initial mortality is included as a regressor, the estimated coefficient on privatized manufacturing employment is 0.138, significant at $p = 0.053$.) Because cancer rates are unlikely to be affected in the short term by economic dislocation, any effect of privatization would more likely act through the withdrawal of medical care for cancer patients (e.g., if clinics had fewer resources for cancer treatment in regions with high privatization rates) than through increased risk of cancer. That said, there is no evidence of such an effect for other diseases, and variation in change in cancer mortality rates accounts for little of the variation in change in overall mortality rates.[10]

As a final exercise, we regress log mortality for the general population on various time-varying regional characteristics for a balanced panel covering the years 1991–2002.[11] Because we are interested in the impact of privatization on mortality, and not the share of employment in privatized firms per se (which may decrease over time as privatized firms downsize and new firms enter the market), we define our privatization variables to take values from the final year of mass privatization in all subsequent years. This practice is analogous to that in the cross-country regressions reported in SKM and above. We include region fixed effects in all regressions.

Table 4 reports results from these panel regressions. In a baseline specification similar to that in SKM, the estimated effect of privatization on mortality is positive for both privatization variables. However, as with the alternative cross-country specifications reported in Table 1, the estimated impact of privatization is attenuated when the economic variables (here, privatization and income) are lagged one year, and the point estimate reverses sign when

[10]Notzon et al. (1998) report that more than half the decline in Russian life expectancy in Russia during the 1990s can be attributed to cardiovascular diseases and external causes of death.

[11]Some variables used above are available only as a cross section; others are unavailable for 1990. Our qualitative results are very similar if we control for the regional vodka price, as in Treisman (2008); that variable is available from 1992.

these variables are lagged two years. Moreover, in four out of six specifications, the estimated effect of privatization is smaller (more negative) when region-specific trends are added to the equation. As with the cross-country results reported in the previous section, we conclude that the positive estimated correlation between privatization and mortality is not robust.

4 Privatization and Unemployment

The analysis so far focuses on the robustness—or lack thereof—of the privatization-mortality correlation in SKM. As a final check on the results, we consider the question of causality: how could privatization raise mortality? The main theory offered by SKM is that privatized firms reduce employment, with the resulting unemployment leading to worsened health and higher mortality. But is the first step in this logic valid—that is, does privatization systematically lead to substantial job loss?

SKM provide evidence on this point from regressions of the log of the registered male unemployment level on the same set of variables used in the mortality regressions. The reported coefficients on the mass privatization indicator and EBRD average privatization index are positive in the FSU, but not in CEE. We replicate that analysis, again checking for robustness to specifications that account for timing and trends.

The first two columns of the first row of Table 5 are pure replications of the SKM unemployment results, and the estimates are qualitatively similar. The results to the right in this row, however, show that the estimated effect of the recoded mass privatization indicator is negative, though statistically insignificant, and the average EBRD effect is due entirely to the small privatization index. The estimated large privatization effect is much smaller and statistically insignificant, which is entirely incompatible with the argument in SKM that "[t]he results would be more severe for employees of large-scale capital-intensive heavy industry and manufacturing enterprises..." (p. 2). Indeed, the retail and services sectors affected by small privatization were neglected under central planning and thus much more likely to grow after privatization. These results most likely reflect the coincidence of small privatization with the collapse of socialism and consequent rise of open unemployment in early transition.

The second row of Table 5 lags privatization and other economic variables by one year, which permits time for policy implementation to affect downsizing; the estimated effect of privatization on unemployment is substantially smaller than that in the baseline specification in all five cases. Adding country-specific trends to account for differences in trend unemployment growth, the estimated coefficients are all statistically insignificant, with magnitudes generally close to zero.

These results refer to country-level correlations, as in SKM. Analyses of such aggregated data always face problems from confounding influences, but there is a substantial body of relevant research that uses micro-level data with direct observations on firms with long time series before and after privatization. Perhaps the clearest example of such research is Brown, Earle and Telegdy (2009, henceforth BET), which analyzes data on nearly every manufacturing firm inherited from the socialist period in four major transition economies: Hungary, Romania, Russia, and Ukraine. While the data have the disadvantage of not covering all

the countries of the FSU and CEE, an important advantage is the possibility to directly observe ownership, employment, and other variables at the firm level. Firms are followed for up to 20 years, enabling BET to follow the path of employment and other variables for long periods both before and after privatization. The data also contain state-owned firms that are never privatized, which together with those that are not yet but eventually will be privatized can form a control group in examining the effect of privatization on employment within a particular industry and year. This ability to compare firms within industries and years—apples with apples, rather than apples with oranges—is a clear benefit of analyzing data at the level of the decision-maker rather than in the aggregate.

Analyzing these data with several statistical methods to control for possible biases due to selection of firms for privatization, BET find no evidence that privatization systematically lowers firm-level employment. As shown in Figure 4, the estimated effects of privatization to domestic owners are generally tiny, and where they are negative the magnitudes are almost always statistically indistinguishable from zero. The estimated effects of foreign privatization are almost always positive, large, and statistically significant, generally implying an approximate 10% expansion of employment following the foreign acquisition. The estimated foreign-privatization effect in Romania is the largest negative value, but it is statistically insignificantly different from zero. In Russia, the country with the most well-known mass privatization, the domestic privatization effect is positive. Analysis of the long time series in the data shows that the absence of negative employment effects of privatization is the consequence neither of delayed restructuring several years after privatization nor of pre-privatization downsizing, which is negligible in these economies.

These empirical results strongly contradict the notion, frequently assumed but little investigated, that large job cuts follow privatization. Why is this assumption empirically incorrect? One possibility is that privatization simply matters very little for firm behavior: new private owners do not restructure and therefore do not lay off workers. BET investigate this possibility by decomposing the employment effects of privatization into two components, which we label "productivity" and "scale" effects. Holding the firm's scale—its level of production—constant, an increase in productivity tends to lower employment. Holding constant the level of productivity, an increase in scale tends to raise it. The empirical analysis of these mechanisms finds that privatization tends to raise both productivity and scale; results are displayed in Figure 4. Both effects are much larger in firms privatized to foreign investors, with 10–25% increases in productivity and 15–30% increases in scale. The dominance of the scale over the productivity effect implies the positive impact of privatization on employment that we observe.

Privatization to new domestic owners in Hungary and Romania also yields positive productivity and scale effects, but they are smaller (6–10%) than the corresponding foreign effects, and the productivity effects slightly dominate the scale effects, resulting in very small negative impacts of privatization on employment in these cases. The productivity and scale effects of domestic privatization are tiny in Ukraine. Domestic privatization in Russia is the outlier, with more substantial negative estimated effects on both productivity and scale, but the drop in productivity exceeds the fall in scale, resulting in a positive net employment

impact.[12]

Thus, the primary mechanism hypothesized in SKM is also not supported by analysis of data on firms, the level where decisions about employment and privatization take place. Unemployment may worsen health, but there is little evidence that postcommunist privatization caused unemployment to rise. Moreover, while involuntary turnover of workers may lead to poor health outcomes,[13] all available evidence suggests little impact of enterprise privatization in postcommunist societies on layoffs and other types of worker turnover.[14]

5 Conclusion

Did mass privatization increase mortality in postcommunist countries? A casual reader of the world's newspapers in January 2009 might be inclined to think so, as many international outlets reported the results of a *Lancet* study that claimed to find such an effect.[15] While the study is useful in drawing renewed attention to an important question, closer scrutiny shows that the data do not support the assertion that privatization was a "crucial determinant" of mortality in postcommunist countries. The correlations reported in the original article are simply not robust.

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[12]These productivity estimates are qualitatively similar to those reported in Brown, Earle and Telegdy (2006), including the finding of a negative impact of domestic privatization on productivity in Russia.

[13]Lazareva (2009), for example, finds poor health associated with occupational mobility in Russia.

[14]See, for example, Earle (1997) on Romania, Gerber (2002) and Brown and Earle (2003) on Russia, and Brown, Earle and Vakhitov (2006) on Ukraine.

[15]A short list would include the *New York Times*, the *Financial Times*, Agence-France Presse, and the *Economist*; see `http://www.upjohninst.org/mortality/discussion.html`.

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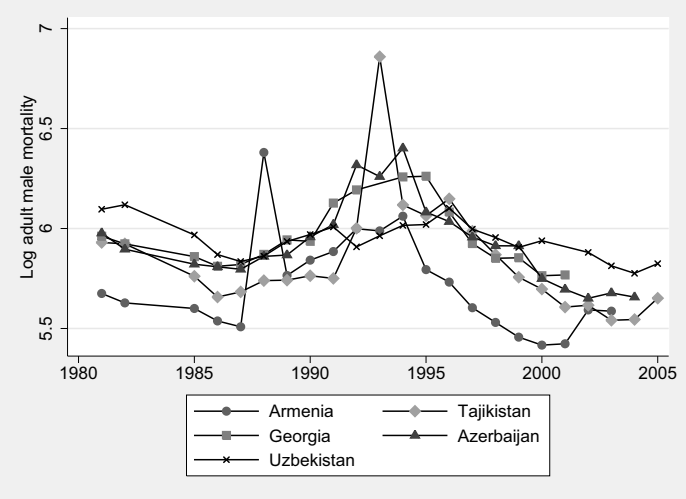
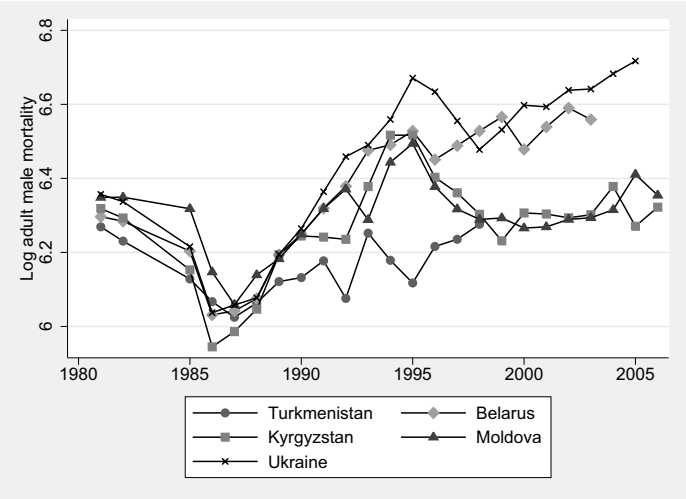
Table 1: Cross-Country Mortality Regressions on the SKM Sample of FSU Countries

	Mass Privatization	Average EBRD Privatization	Recoded Mass Privatization	EBRD Large Privatization	EBRD Small Privatization
	(1)	(2)	(3)	(4)	(5)
SKM specification	0.158***	0.099***	0.069*	0.073***	0.075***
	(0.042)	(0.022)	(0.040)	(0.016)	(0.023)
One-year lags	0.108**	0.064***	0.015	0.046***	0.049**
	(0.041)	(0.023)	(0.038)	(0.017)	(0.023)
Two-year lags	0.063*	0.014	−0.015	0.031	−0.006
	(0.037)	(0.025)	(0.043)	(0.023)	(0.021)
Country-specific trends	0.093**	0.069**	0.050	0.035	0.054*
	(0.038)	(0.031)	(0.048)	(0.024)	(0.029)
One-year lags & country-specific trends	0.034	0.036	−0.014	0.017	0.029
	(0.041)	(0.030)	(0.055)	(0.022)	(0.029)
Two-year lags & country-specific trends	−0.042	−0.047*	−0.113**	−0.006	−0.053**
	(0.034)	(0.028)	(0.056)	(0.024)	(0.024)

Notes: Each cell of the table reports the estimated effect of privatization on log working-age male mortality rate from a separate regression. Sample is 15 countries of the former Soviet Union, 177 country-years. With the exception of the privatization measures in Column (3)–(5), data are identical to those in SKM. Specifications are identical but for the specific changes noted in the table. Heteroskedasticity-robust standard errors in parentheses. Significance levels: *** = 0.01, ** = 0.05, *= 0.10.





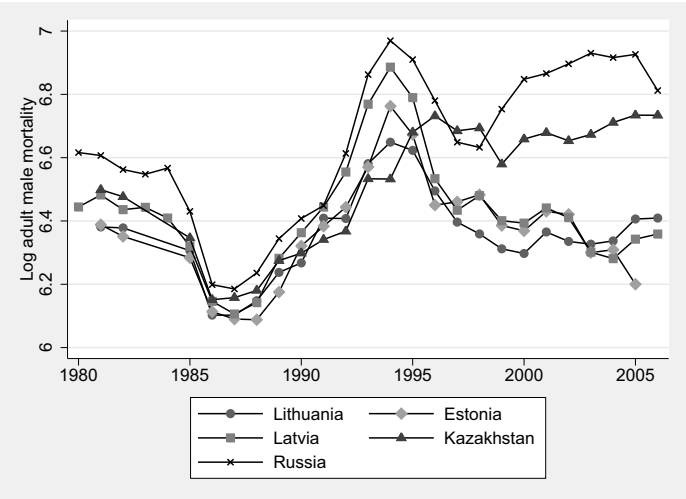


Figure 1: Mortality Trends in Former Soviet Union

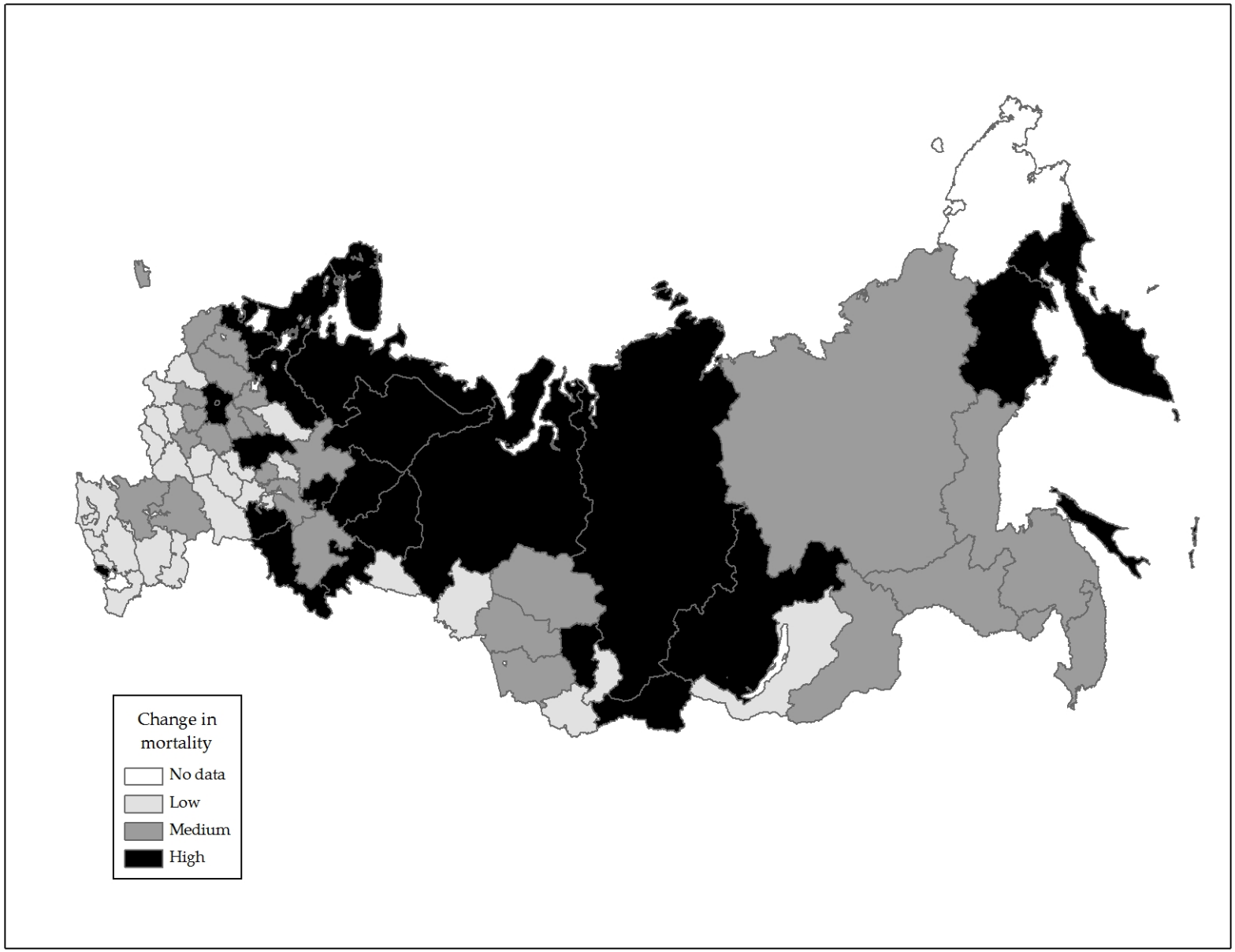


Figure 2: Change in Log Mortality Rate for Working-Age Males, 1990–1995

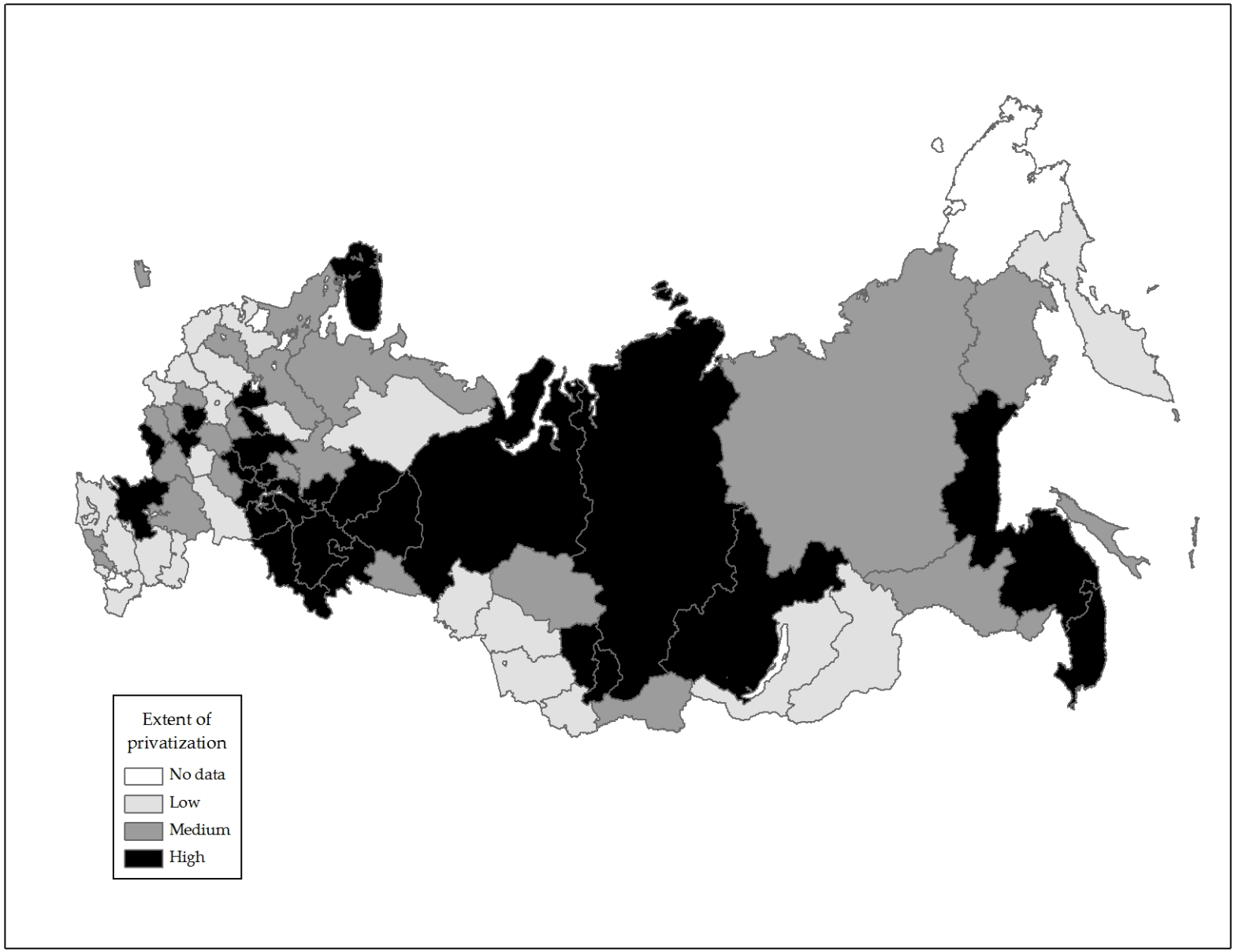


Figure 3: Share of Employment in Privatized Firms, 1995

Table 2: Determinants of Mortality in Russian Regions

	Initial mortality	Change in Mortality			
	(1)	(2)	(3)	(4)	(5)
Privatized employment		−0.130	−0.136		
		(0.169)	(0.169)		
Privatized manufacturing employment				−0.188*	−0.189*
				(0.099)	(0.104)
Log initial mortality			−0.035		0.012
			(0.140)		(0.140)
Log income	−0.058	0.004	0.002	−0.003	−0.002
	(0.065)	(0.082)	(0.081)	(0.080)	(0.080)
Population dependency	1.821**	−1.570**	−1.504*	−1.704**	−1.726**
	(0.798)	(0.776)	(0.793)	(0.764)	(0.812)
Urbanization	−0.080	0.639***	0.639***	0.640***	0.642***
	(0.109)	(0.128)	(0.130)	(0.086)	(0.082)
Higher education	−0.179	−0.859	−0.870	−1.006*	−1.006*
	(0.276)	(0.599)	(0.616)	(0.558)	(0.563)
Proportion Muslim	−0.586***	−0.100**	−0.12	−0.133***	−0.126
	(0.085)	(0.045)	(0.085)	(0.046)	(0.080)
Mean January temperature	−0.002	−0.002	−0.003	−0.001	−0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Constant	5.900***	0.822**	1.031	1.041***	0.973
	(0.364)	(0.349)	(0.921)	(0.365)	(0.875)
R-squared	0.569	0.628	0.628	0.647	0.647

Notes: OLS regressions. Dependent variable is log mortality rate for working-age men, 1990 (column 1); change in log mortality rate for working-age men, 1990 to 1995 (columns 2–5). Sample is 76 regions. Heteroskedasticity-robust standard errors in parentheses. Significance levels: *** = 0.01, ** = 0.05, * = 0.10.

Table 3: Determinants of Mortality in Russian Regions by Cause of Death

Panel A: Privatized employment

	Infectious diseases	Cancer	Circulatory system	Respiratory system	Digestive system	External causes
	(1)	(2)	(3)	(4)	(5)	(6)
Privatized employment	−0.872**	0.040	0.133	−0.506	−0.051	−0.511*
	(0.380)	(0.182)	(0.210)	(0.521)	(0.519)	(0.295)
Log income	0.000	0.125**	0.176***	−0.419*	0.047	−0.032
	(0.244)	(0.060)	(0.059)	(0.247)	(0.282)	(0.133)
Population dependency	−0.127	−1.301*	−0.920	−6.483***	0.646	−0.466
	(1.468)	(0.766)	(0.764)	(1.711)	(1.955)	(1.397)
Urbanization	1.102**	−0.081	0.295**	1.618***	1.149**	1.193***
	(0.441)	(0.097)	(0.119)	(0.424)	(0.521)	(0.226)
Higher education	−1.366	−0.426	−0.587	−0.905	−0.981	−1.383
	(1.129)	(0.326)	(0.358)	(1.409)	(1.235)	(1.213)
Proportion Muslim	0.059	−0.018	−0.048	−0.269*	−0.185	−0.168
	(0.211)	(0.099)	(0.058)	(0.148)	(0.208)	(0.127)
Mean January temperature	−0.011	0.000	−0.002	−0.010*	−0.003	−0.003
	(0.009)	(0.002)	(0.002)	(0.006)	(0.005)	(0.004)
Constant	0.238	0.745**	0.823**	2.124**	−0.124	0.113
	(0.675)	(0.372)	(0.346)	(0.806)	(0.862)	(0.686)
R-squared	0.233	0.333	0.514	0.641	0.236	0.452

Panel B: Privatized manufacturing employment

	Infectious diseases	Cancer	Circulatory system	Respiratory system	Digestive system	External causes
	(1)	(2)	(3)	(4)	(5)	(6)
Privatized manufacturing employment	−0.073	0.115	−0.019	−0.018	0.001	−0.287**
	(0.446)	(0.069)	(0.103)	(0.238)	(0.271)	(0.114)
Log income	0.032	0.131**	0.169***	−0.399	0.049	−0.029
	(0.268)	(0.060)	(0.062)	(0.244)	(0.285)	(0.133)
Population dependency	−0.356	−1.229	−0.902	−6.603***	0.636	−0.737
	(1.591)	(0.771)	(0.790)	(1.719)	(1.902)	(1.366)
Urbanization	0.764**	−0.098	0.355***	1.414***	1.128**	1.066***
	(0.342)	(0.074)	(0.095)	(0.336)	(0.431)	(0.161)
Higher education	−0.734	−0.303	−0.725**	−0.505	−0.937	−1.346
	(0.912)	(0.281)	(0.317)	(1.257)	(1.153)	(1.128)
Proportion Muslim	0.040	0.002	−0.050	−0.275	−0.185	−0.222*
	(0.256)	(0.091)	(0.065)	(0.171)	(0.228)	(0.122)
Mean January temperature	−0.009	−0.001	−0.003	−0.009*	−0.003	0.000
	(0.007)	(0.002)	(0.003)	(0.005)	(0.005)	(0.004)
Constant	0.423	0.616	0.828**	2.205**	−0.119	0.485
	(0.835)	(0.382)	(0.379)	(0.850)	(0.875)	(0.698)
R-squared	0.202	0.351	0.512	0.636	0.236	0.456

Notes: Dependent variable is change in log mortality rate for working-age men, 1990 to 1995, by cause of death. Sample is 76 regions. Heteroskedasticity-robust standard errors in parentheses. Significance levels: *** = 0.01, ** = 0.05, * = 0.10.

Table 4: Determinants of Mortality in Russian Regions: Panel Regressions

Panel A: Privatized employment

	Baseline	One-year lags	Two-year lags	Region-specific trends	1-year lags & trends	2-year lags & trends
	(1)	(2)	(3)	(4)	(5)	(6)
Privatized employment	0.612***	0.107***	−0.285***	0.507***	−0.114**	−0.506***
	(0.039)	(0.030)	(0.027)	(0.072)	(0.058)	(0.048)
Log income	−0.071***	−0.141***	−0.052***	−0.059***	−0.132***	−0.041***
	(0.011)	(0.007)	(0.006)	(0.010)	(0.007)	(0.006)
Population dependency	−2.671***	−3.101***	−3.246***	−1.967***	−1.218**	−1.910***
	(0.213)	(0.218)	(0.245)	(0.594)	(0.537)	(0.573)
Urbanization	−0.523	0.037	0.346	−1.992***	−0.472	−0.280
	(0.334)	(0.301)	(0.375)	(0.388)	(0.422)	(0.504)

Panel B: Privatized manufacturing employment

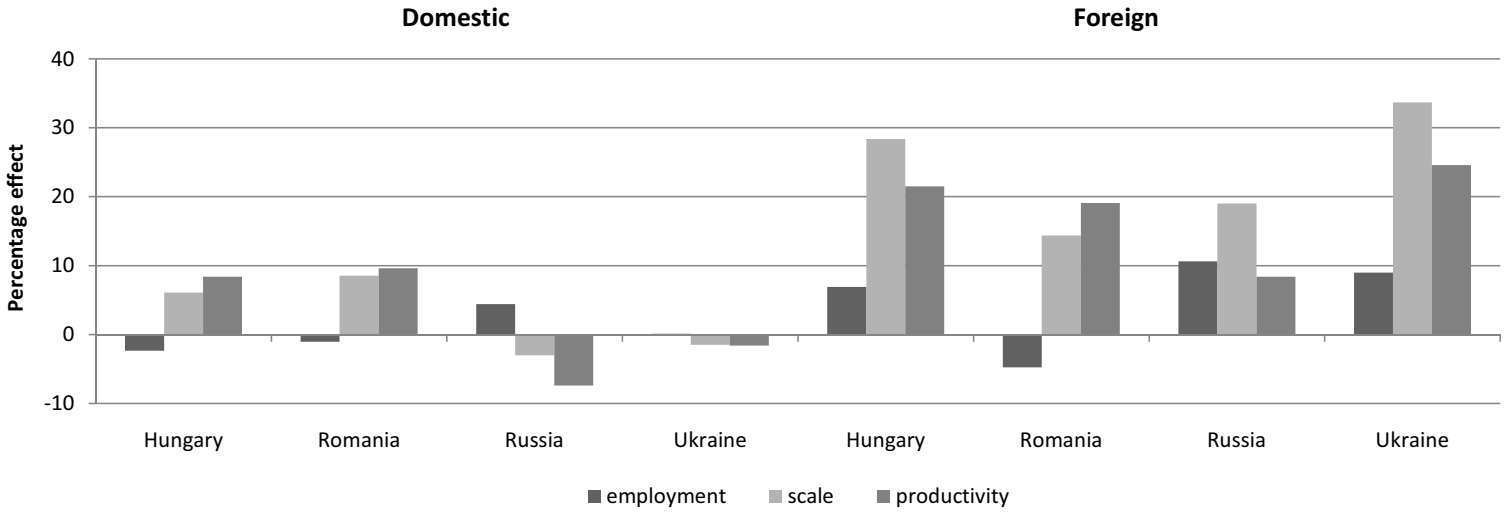
	Baseline	One-year lags	Two-year lags	Region-specific trends	1-year lags & trends	2-year lags & trends
	(1)	(2)	(3)	(4)	(5)	(6)
Privatized manufacturing employment	0.228***	0.067***	−0.092***	0.285***	0.078***	−0.154***
	(0.010)	(0.008)	(0.007)	(0.018)	(0.015)	(0.013)
Log income	−0.052***	−0.138***	−0.062***	−0.057***	−0.146***	−0.059***
	(0.009)	(0.006)	(0.005)	(0.008)	(0.007)	(0.005)
Population dependency	−2.373***	−2.777***	−3.265***	−4.855***	−3.755***	−1.397**
	(0.201)	(0.209)	(0.230)	(0.636)	(0.645)	(0.590)
Urbanization	0.057	0.152	0.371	−0.978***	−0.396	−0.413
	(0.304)	(0.295)	(0.368)	(0.332)	(0.367)	(0.499)

Notes: Dependent variable is log mortality rate for general population. Sample is a balanced panel of 76 regions, 1991–2002. Region fixed effects (all columns) and region-specific trends (columns 4–6) included. Privatized (manufacturing) employment and log income lagged, as indicated. Heteroskedasticity-robust standard errors in parentheses. Significance levels: *** = 0.01, ** = 0.05, * = 0.10.

Table 5: Cross-Country Unemployment Regressions on the SKM Sample of FSU Countries

	Mass Privatization	Average EBRD Privatization	Recoded Mass Privatization	EBRD Large Privatization	EBRD Small Privatization
	(1)	(2)	(3)	(4)	(5)
SKM specification	0.684***	0.579***	−0.073	0.203	0.594***
	(0.227)	(0.158)	(0.255)	(0.127)	(0.138)
One-year lag	0.568***	0.272*	−0.371	0.116	0.282**
	(0.211)	(0.142)	(0.234)	(0.117)	(0.112)
One-year lag & country-specific trends	0.300	0.080	−0.340	0.017	0.082
	(0.213)	(0.153)	(0.239)	(0.127)	(0.112)

Notes: Each cell of the table reports the estimated effect of privatization on log registered male unemployment level from a separate regression. Sample is 15 countries of the former Soviet Union, 177 country-years. With the exception of the privatization measures in Column (3)–(5), data are identical to those in SKM. Specifications are identical but for the specific changes noted in the table. Heteroskedasticity-robust standard errors in parentheses. Significance levels: *** = 0.01, ** = 0.05, *= 0.10.



Source: Brown, Earle and Telegdy (2009).

Figure 4: Decomposition of the Employment Effect of Privatization into Scale and Productivity Effects (Estimates with Firm-Specific Trends)

Table A1: Cross-Country Mortality Regressions on the SKM Sample of CEE Countries

	Mass Privatization	Average EBRD Privatization	Recoded Mass Privatization	EBRD Large Privatization	EBRD Small Privatization
	(1)	(2)	(3)	(4)	(5)
SKM specification	−0.005	−0.019	−0.140***	−0.025	−0.005
	(0.042)	(0.018)	(0.044)	(0.016)	(0.016)
One-year lags	−0.046	−0.028*	−0.082*	−0.022*	−0.021*
	(0.040)	(0.014)	(0.044)	(0.013)	(0.012)
Two-year lags	−0.060*	−0.032***	−0.053	−0.017	−0.032***
	(0.034)	(0.011)	(0.037)	(0.011)	(0.009)
Country-specific trends	−0.022	−0.024	−0.118***	−0.024*	−0.003
	(0.033)	(0.022)	(0.030)	(0.014)	(0.017)
One-year lags & country-specific trends	−0.053	−0.033**	−0.056	−0.019	−0.019
	(0.036)	(0.016)	(0.048)	(0.013)	(0.012)
Two-year lags & country-specific trends	−0.038	−0.033**	−0.023	−0.009	−0.031***
	(0.034)	(0.014)	(0.036)	(0.010)	(0.011)

Notes: Each cell of the table reports the estimated effect of privatization on log working-age male mortality rate from a separate regression. Sample is 9 countries in Central and Eastern Europe, 112 country-years. With the exception of the privatization measures in Column (3)–(5), data are identical to those in SKM. Specifications are identical but for the specific changes noted in the table. Heteroskedasticity-robust standard errors in parentheses. Significance levels: *** = 0.01, ** = 0.05, *= 0.10.

Table A2: Mass Privatization Indicator: Coding in SKM

	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002
Albania	0	0	0	0	0	0	0	0	0	0	0	0	0	
Armenia			0	0	0	1	1	1	1	1	1	1	1	1
Azerbaijan				0	0	0	0	0	0	0	0	0	0	0
Belarus			0	0	0	0	0	0	0	0	0	0	0	0
Bulgaria	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Croatia			0	0	0	0	0	0	0	0	0	0	0	0
Czech Republic		0	0	1	1	1	1	1	1	1	1	1	1	1
Estonia			0	0	0	0	0	0	0	0	0	0	0	0
Georgia			0	0	0	0	1	1	1	1	1	1	1	1
Hungary	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Kazakhstan			0	0	0	1	1	1	1	1	1	1	1	1
Kyrgyzstan			0	0	0	1	1	1	1	1	1	1	1	1
Latvia			0	0	0	1	1	1	1	1	1	1	1	1
Lithuania			0	0	1	1	1	1	1	1	1	1	1	1
Macedonia				0	0	0	0	0	0	0	0	0	0	0
Moldova			0	0	0	1	1	1	1	1	1	1	1	1
Poland	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Romania	0	0	0	0	0	0	1	1	1	1	1	1	1	1
Russia	0	0	0	1	1	1	1	1	1	1	1	1	1	1
Slovakia					0	0	0	0	0	0	0	0	0	0
Tajikistan			0	0	0	0	0	0	0	0	0	0	0	0
Turkmenistan			0	0	0	0	0	0	0	0	0	0	0	0
Ukraine			0	0	0	0	1	1	1	1	1	1	1	1
Uzbekistan			0	0	0	0	0	0	0	0	0	0	0	0

Table A3: Mass Privatization Indicator: Recoding Using Data from Original Source with Correction for Timing

	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002
Albania	0	0	0	0	0	0	0	0	0	0	0	0	0	
Armenia			0	0	0	0	0	1	1	1	1	1	1	1
Azerbaijan				0	0	0	0	0	0	0	0	0	0	0
Belarus			0	0	0	0	0	0	0	0	0	0	0	0
Bulgaria	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Croatia			0	0	0	0	0	0	0	0	0	0	0	0
Czech Republic		0	0	0	1	1	1	1	1	1	1	1	1	1
Estonia			0	0	0	1	1	1	1	1	1	1	1	1
Georgia			0	0	0	0	0	1	1	1	1	1	1	1
Hungary	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Kazakhstan			0	0	0	0	0	0	0	0	0	0	0	0
Kyrgyzstan			0	0	0	0	0	0	0	0	0	0	0	0
Latvia			0	0	0	0	0	0	0	0	0	0	0	0
Lithuania			0	0	1	1	1	1	1	1	1	1	1	1
Macedonia				0	0	0	0	0	0	0	0	0	0	0
Moldova			0	0	0	0	0	0	0	0	0	0	0	0
Poland	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Romania	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Russia	0	0	0	0	1	1	1	1	1	1	1	1	1	1
Slovakia					1	1	1	1	1	1	1	1	1	1
Tajikistan			0	0	0	0	0	0	0	0	0	0	0	0
Turkmenistan			0	0	0	0	0	0	0	0	0	0	0	0
Ukraine			0	0	0	0	0	0	0	0	0	0	0	0
Uzbekistan			0	0	0	0	0	0	0	0	0	0	0	0

Table A4: EBRD Large Privatization Index

	1989	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	2000	2001	2002
Albania	1	1	1	1	1	1	2	2.33	2.33	2.33	2.67	2.67	3	3
Armenia	1	1	1	1	1	1	2	3	3	3	3	3	3	3.33
Azerbaijan	1	1	1	1	1	1	1	1	2	2	1.67	1.67	2	2
Belarus	1	1	1	1	1.67	1.67	1.67	1	1	1	1	1	1	1
Bulgaria	1	1	1	1.67	2	2	2	2	3	3	3	3.67	3.67	3.67
Croatia	1	1	1	2	2	2	3	3	3	3	3	3	3	3
Czech Republic	1	1	1	2	3	4	4	4	4	4	4	4	4	4
Estonia	1	1	1	1	2	3	4	4	4	4	4	4	4	4
Georgia	1	1	1	1	1	1	2	3	3.33	3.33	3.33	3.33	3.33	3.33
Hungary	1	2	2	2	3	3	4	4	4	4	4	4	4	4
Kazakhstan	1	1	1	1	2	2	2	3	3	3	3	3	3	3
Kyrgyzstan	1	1	1	2	2	3	3	3	3	3	3	3	3	3
Latvia	1	1	1	2	2	2	2	3	3	3	3	3	3	3.33
Lithuania	1	1	1	2	3	3	3	3	3	3	3	3	3.33	3.67
Macedonia	1	1	1	1	2	2	2	3	3	3	3	3	3	3
Moldova	1	1	1	1	2	2	3	3	3	3	3	3	3	3
Poland	1	2	2	2	2	3	3	3	3.33	3.33	3.33	3.33	3.33	3.33
Romania	1	1	1.67	1.67	2	2	2	2.67	2.67	2.67	2.67	3	3.33	3.33
Russia	1	1	1	2	3	3	3	3	3.33	3.33	3.33	3.33	3.33	3.33
Slovakia	1	1	1	2	3	3	3	3	4	4	4	4	4	4
Tajikistan	1	1	1	1	1	1	2	2	2	2	2.33	2.33	2.33	2.33
Turkmenistan	1	1	1	1	1	1	1	1	2	1.67	1.67	1.67	1	1
Ukraine	1	1	1	1	1	1	2	2	2.33	2.33	2.33	2.67	3	3
Uzbekistan	1	1	1	1	1	2	2.67	2.67	2.67	2.67	2.67	2.67	2.67	2.67

Source: EBRD Transition Indicators (http://www.ebrd.com/country/sector/econo/stats/tic.xls).