Democratic Backsliding and Financial Data Transparency

Byunghwan Son
George Mason University, bson3@gmu.edu
Chung-shik Moon
Chung-Ang University, sarim799@gmail.com

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Abstract

Extant literature on political regimes establishes that a backsliding government grows less interested than before in providing public goods. This tendency is consistent with the government's proclivity to cater to the particularistic interests of its narrow support coalition. While the empirical terrain on which this theoretical claim is tested is extensive and diverse, one area in which the backsliding literature has not shed light on is financial statistics transparency. Not only is a backsliding government unmotivated to invest in collecting and managing financial statistics which is a quintessential public good, it would strategically avoid releasing information about the financial market in fear of revealing their economic policies slanted towards their support base. Using a fixed-effect model as well as a staggered event study on a sample of 61 countries, we find evidence supportive of this expectation. The paper contributes to the burgeoning literature that highlights the multi-faceted negative consequences of democratic backsliding.

1   Introduction

Extant literature on political regimes establishes that a government undergoing democratic backsliding grows less inclined to prioritize public goods (Son and Bellinger, 2022; Wigley et al., 2020). The rationale is that democratic backsliding increases the government's proclivity to cater to the particularistic interests of its narrow support coalition. Quintessential public goods such as public health coverage tend to be under-supplied during backsliding, therefore.

While the empirical terrain on which this theoretical claim is tested is extensive and diverse (e.g., Nelson and Witko 2022; Szikra and Öktem 2022; Rüland 2021; Tschantret 2020), we note one area in which the backsliding literature has not shed light on–financial statistics transparency. By financial statistics transparency, we mean the degree to which a government makes publicly available the core statistics of the country's financial stability, creditworthiness, and indebtedness—the kind of information that might not be readily observable or inferable from ordinary macroeconomic indicators but is nonetheless critically reflective of the soundness of the financial market.

Collecting and making readily available financial data is an act of indisputable public good. Information about the financial status of a society, when made public, benefits market participants indiscriminately, helping them informed decisions (Goldstein and Yang, 2017). Likewise, it aids not just a select group of people but a much wider group of informed citizens to hold governments accountable for their economic policies (Hollyer, Rosendorff and Vreeland, 2018). In other words, without access to such data, only a handful of investors may avoid making risky decisions, serving private interests (Enderwick, 2005). Similarly, without knowing the financial status of the country, the society would not be able to reign in wrong-headed or reckless financial policies of the government. In addition, high levels of financial data transparency can indirectly bring about public goods such as low borrowing costs (Copelovitch, Gandrud and Hallerberg, 2018).

We suggest two ways in which a backsliding government would purposefully keep the transparency of financial statistics. First, a backsliding government may grow unmotivated to invest in collecting and managing financial statistics. Second, the government would also strategically avoid releasing information about how their economic policies are increasingly slanted towards their cronies, not the entire national economy. This way, the political leaders can minimize, or delay, the backlash from the social and (financial) market actors during an otherwise 'perilous' period of democratic backsliding. To put it more explicitly, we hypothesize: Hypothesis: A country experiencing democratic backsliding has a lower level of financial statistics transparency than those who are not experiencing backsliding.

2   Research Design

To demonstrate that this theoretical expectation holds empirically, we use a panel data set covering a sample of 61 developing and developed countries, from 1990 to 2013. The primary treatment variable is constructed using the V-dem's Episodes of REgime Transformation (ERT) data (Maerz et al., 2021). Autocratization here is defined as "substantial and sustained changes" (Maerz et al., 2021, 9) in the Electoral Democracy Index (Pemstein et al., 2021). A dummy variable taking the value of 1 when an autocratization episode is unfolding according to ERT, zero otherwise.

The outcome variable draws on the Financial Data Transparency (FDT) of Copelovitch, Gandrud and Hallerberg (2018). Through an unobserved latent variable approach, FDT "summarizes a country's likelihood of reporting in the World Bank's Global Financial Development Database" (Copelovitch, Gandrud and Hallerberg, 2018, 28). The data are based on thirteen different financial indicators related primarily to banks and asset positions of other financial actors. These data are not easily inferable from other macroeconomic indicators and can only be gleaned through deliberate government actions, consistent with our understanding of financial data disclosure as a public good provision.

With these core variables, a simplified functional form of our model can be written:

$$transparency_{it} = \beta_1 transparency_{it-1} + \beta_2 autocratization_{it-1} +$$
$$FE(unit, time) + trend + \mathbf{X}_{it} + \epsilon_{it}, \quad (1)$$

where i and t represent the unit (country) and time (year). *Transparency* and *autocratization* are the primary outcome and treatment variables, respectively. For the hypothesis put forth above to be supported, β_2 should be significant and negative. $\mathbf{X}$ represents a vector of covariates, which include macroeconomic indicators such as logarithms of GDP (*logGDP*), GDP per capita (*logGDPpc*), growth rates (*GDP growth rates*), and total government consumption (*govt consump/GDP*). Variables taking into account whether the country is currently under an IMF standby agreement (*IMF SBA*) and the total number of years under IMF SBA up until the previous year (*total IMF*), which can shed light on the cases where governments are required to disclose financial statistics regardless of their intentions. To take into consideration any country- and year-specific idiosyncrasies, thereby making the treated and untreated groups more comparable, the two-way fixed effects are also applied. Given various data availability initiatives of international organizations and technological advances, it is expected that transparency would improve globally over time. A trend variable (*year*) is

included in this regard. ϵ indicates the error term.

Finally, a lagged dependent variable (LDV, $transparency_{it-1}$) is also included. There are two straightforward reasons for this inclusion. First, it is to account for the underlying 'base level' transparency–the current year's policies on statistics tend to be the previous year's regardless of democratic backsliding. More importantly, LDV can alleviate the stationarity issue in the data. Our Im-Pesaran-Shin unit-root test (Im, Pesaran and Shin, 2003) using the residual estimates reveals that when the LDV is not included, the null hypothesis ('all panels contain unit roots') could not be rejected (p=0.9547). When the LDV was included the opposite was the case (p=0.000). Applying different permutations of lag structures and trends did not alter this stark difference.

3   Empirical Analysis

Table 1 presents the baseline and benchmark estimates. Model 3 presents the result of our preferred specification with the full set of control variables (benchmark). The autocratization variable is negative and statistically significant (p<0.042), lending support to the hypothesis. Figure 1 illustrates this significant treatment effect.

Model 1 presents the result of excluding all the control variables from, and retaining only methodologically necessary components (fixed effects and LDV) in, the model. The coefficient and standard error of autocratization variable that are nearly identical to those of Model 3 imply that the benchmark estimates are unlikely to be driven by a 'suppression effect' (Lenz and Sahn, 2021). Given that the decisions of suspending programs of financial statistics reporting can follow a more dynamic process, we estimate the model with an additional LDV (i.e., $transparency_{t-1}$). The result is not fundamentally altered. In fact, the level of significance of the autocratization variable becomes much higher when a second order LDV is included. We decided not to use this specification as our benchmark, however, because doing so loses many observations (51) while not improving the goodness-of-fit or predictive power of the model.

Table 1: Basline and Benchmark Estimates

	(1) Basline	(2) Two LDVs	(3) Full
autocratization	-0.035**	-0.024**	-0.036**
	(0.017)	(0.009)	(0.018)
transparency$_{t-2}$		-0.506**	
		(0.047)	
transparency$_{t-1}$	0.982**	1.472**	0.983**
	(0.007)	(0.047)	(0.007)
Vdem$_{t-1}$			-0.030
			(0.075)
Under IMF SB program			0.016
			(0.015)
past IMF years			-0.000
			(0.003)
logGDP			0.048
			(0.030)
logGDPpc			-0.046
			(0.028)
GDP growth rates			-0.001
			(0.001)
govt consump/GDP (%)			0.001
			(0.002)
Constant	1.454	2.149**	1.361
	(0.997)	(0.947)	(1.577)
Observations	1399	1338	1288
R^2	0.990	0.993	0.990
AIC	-3256.867	-3588.870	-2909.637
country-FE	✓	✓	✓
year-FE	✓	✓	✓
trend	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$. OLS estimates. Standard errors clustered over countries.
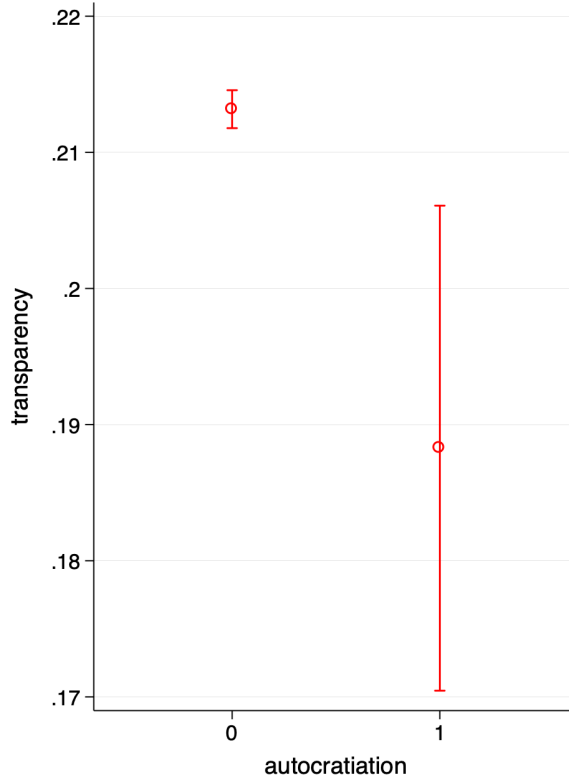
Figure 1: The Effect of Autocratization with 95% confidence intervals. Based on Model 3 of Table 1.

Table 2 presents the result of several robustness checks on the benchmark estimates. First, we test if what the benchmark estimates represent is really a consequence of political transitions, not necessarily autocratization by including a dummy variable for democratization episodes (also from the ERT data). Model 1 indicates that the benchmark effect of autocratization remains negative and significant, although a bit weakened. The democratization variable is not at all significant and positive, implying that what we document in Model 3 of 1 is unlikely to be the consequence of political transition.

Model 2 offers a nuanced understanding of the benchmark effect. As the interaction term between the autocratization variable and the LDV indicates, the past level of transparency can condition the effect of autocratization. Figure 2 illustrates this conditional effect. When much of financial data were unavailable in the previous year (left hand-side), the transparency-reducing effect of autocratization is simply insignificant.

Table 2: Robustness Check

	(1) Democratization	(2) Interaction	(3) PCSE
autocratization	-0.034*	-0.024*	-0.039**
	(0.019)	(0.012)	(0.018)
democratization	0.015		
	(0.019)		
transparency$_{t-1}$	0.983**	0.985**	
	(0.007)	(0.006)	
autoc $\times$ transp		-0.072*	
		(0.040)	
Vdem$_{t-1}$	-0.012	-0.047	0.255**
	(0.090)	(0.073)	(0.108)
Under IMF SB program	0.015	0.015	0.017
	(0.015)	(0.014)	(0.011)
past IMF years	-0.000	-0.000	0.037**
	(0.003)	(0.003)	(0.008)
logGDP	0.046	0.055*	0.289**
	(0.030)	(0.030)	(0.044)
logGDPpc	-0.044	-0.053*	-0.245**
	(0.028)	(0.029)	(0.041)
GDP growth rates	-0.001	-0.001	-0.000
	(0.001)	(0.001)	(0.001)
govt consump/GDP (%)	0.001	0.002	0.005**
	(0.002)	(0.002)	(0.002)
Constant	1.390	1.536	0.000
	(1.560)	(1.599)	(.)
Observations	1288	1288	1334
R^2	0.990	0.990	0.299
AIC	-2909.269	-2915.828	.
country-FE	✓	✓	✓
year-FE	✓	✓	✓
trend	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$. OLS estimates. Standard errors clustered over countries in the first two columns. Panel-corrected standard errors with a first-order autocorrelation disturbance is applied to the third column.
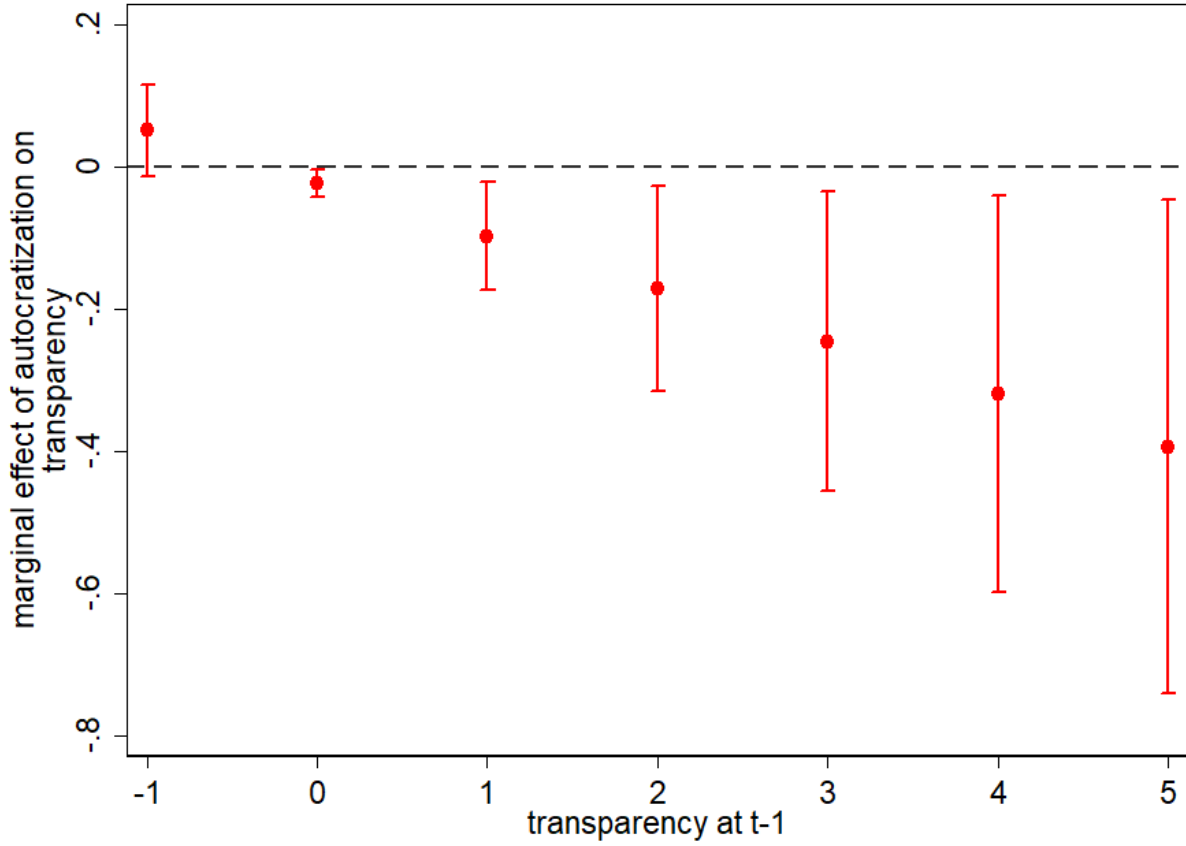
Figure 2: Conditional Effect of the Trasparency in the Previous Year with 90 % confidence intervals.

One implicit assumption made in our benchmark model is that the coefficient of the autocratization variable captures the treated (autocratizing countries) and untreated (non-autocratizing countries). One challenge to the validity of this assumption in our empirical context is that the treatment timing is not uniform across units and, more importantly, some of the units are 'never treated.' The fixed effect setup we use compares the treated and untreated any given time, quite often assuming the never-treated and yet-to-be-treated are the same untreated groups. This 'staggered' treatment design could be a source of problem in inferring the treatment effect (Baker, Larcker and Wang, 2021). Sun and Abraham (2021) proposes an alternative event study design that can address this problem. Figure 3 illustrates the result of using Sun-Abraham method while maintaining our specification. Given the small size of each treatment 'cohort' the confidence intervals are generally very large. Nonetheless, the figure offers a finding consistent with the benchmark. The effect of autocratization on the treated is significantly larger than that on the untreated in the run-up to the treatment. The treated lose this difference after the treatment, implying the effect of the autocratization on the treated is negative ('downward'). One possible reason why the to-be-treated have higher

transparency than the untreated before the treatment might be that some of the untreated do not autocratize and maintain low levels of transparency all along, as indirectly implied in the left-hand side of Figure 2.
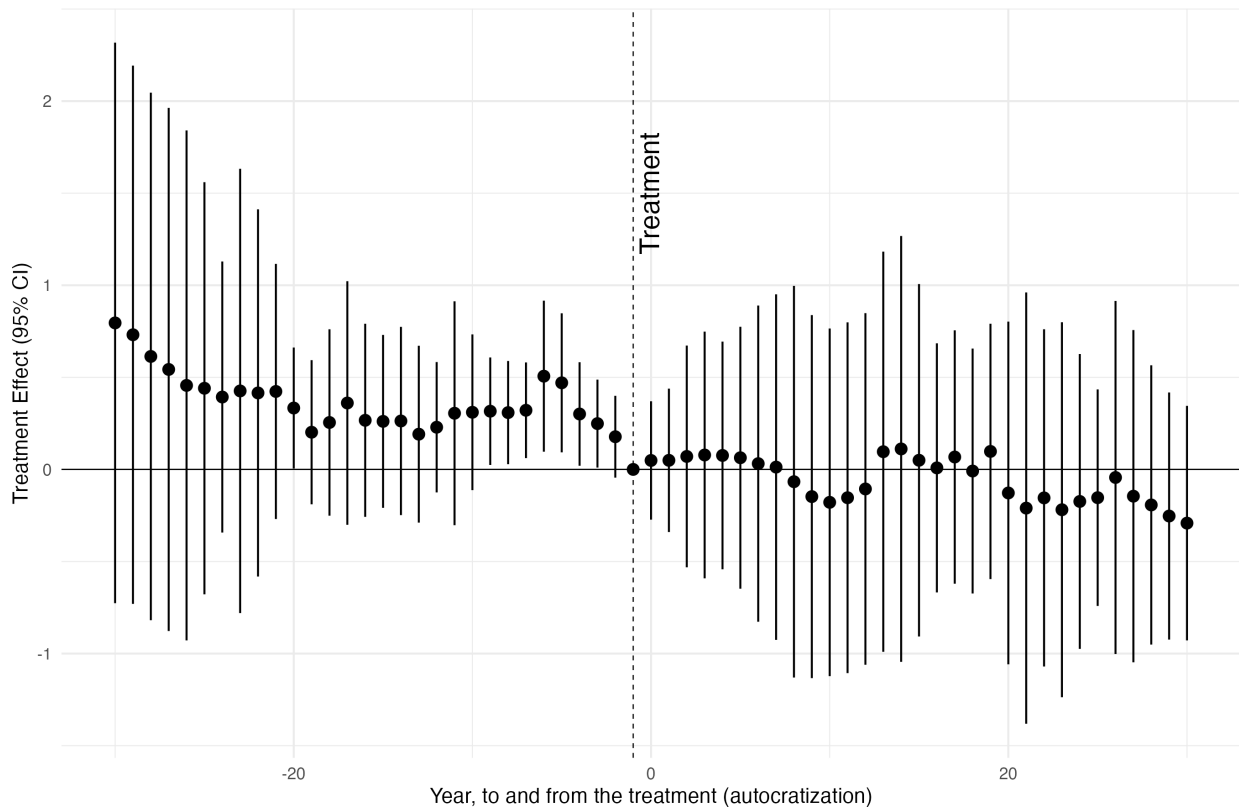

Figure 3: Sun-Abraham (2022) estimates of staggered treatment.

4 Conclusion

The paper makes two important contributions to the literature. First, it helps expand the horizon of the burgeoning studies that highlight the multifaceted negative consequences of democratic backsliding by highlighting the strategic choice of the government in the realm of finance. Second, it synthesizes two distinct bodies of literature, one on democratic backsliding and the other on information governance. While researchers have shown a great deal of interest in these two areas in recent years, they have seldom been bridged.

We acknowledge some limitations of the study. The sample used in the empirical analysis is limited to 61 countries and many of the developing countries are largely excluded. Similarly, the series ends in 2013, in effect not accounting for a period in which, arguably, a global wave of backsliding took place (Hellmeier et al., 2021). We plan to update the FDT to address

these issues.

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