Online Appendix

Consequences of Democratic Backsliding in Popular Culture:
Evidence from Blacklist in South Korea

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Democratization

Appendix A. Parallel Trend Assumption

One important theoretical underpinning of the difference-in-difference (DiD) literature is that the untreated ('control') are taken as a counterfactual of the treated (Abadie 2005). What would have happened to the treated without the treatment should be equivalent to what actually happened to the untreated during the post-treatment period. This assumption, commonly referred to as the 'parallel trend assumption' (PTA), enables a causal inference through comparing the treated and the control. Ultimately, however, this 'parallel trend' is not *testable* because one cannot observe the counterfactual and thus how close the control comes to it (Bilinski and Hatfield 2018).

One factor that could raise the confidence in the validity of PTA is the parallel trend observed in the pre-treatment period. What is implied here is the parallel treatment that existed during the pre-treatment period would have persisted in the post-treatment had no treatment ever occurred. Hence, researchers are often concerned about assuring that there is no pre-trend through visual examination of various plots. However, "the absence of a pre-trend is neither a necessary nor a sufficient condition for the absence of divergence in the counterfactual" (Kahn-Lang and Lang 2020, 618) because what would have happened in the absence of the treatment to the treated (the counterfactual) is by definition unobservable. Checking for pre-trend merely increases, not guarantees, the possibility that the parallel trend assumption holds with the data.

This limitation notwithstanding, the recent advancement of the literature has come up with creative and compelling ways to check the plausibility of PTA in applied research (See Roth et al. 2023 for an extensive review of the latest development). I apply some of them to the context of the present paper and test whether the benchmark estimates are vulnerable to the effect of any pre-trend.

A1. Excluding some of the run-up to the treatment

One way to critically question the validity of the parallel trend assumption holding in the benchmark model concerns an over-fitting based on exceptions. There could be a certain form of trend already developing before the treatment timing (i.e., pre-trend) that drives up the difference between the treated and control; or it simply appears so in visual examination. If the former, excluding some pre-treatment years that drive the pre-trend from the sample (thereby 'removing' the pre-trend) should significantly alter the average treatment effect on the treated (ATT). If the reported treatment effect was in fact the function of the pre-trend, that should disappear if it is 'de-trended.'

One plausible empirical scenario predicated on this suspicion would follow an inverted U-curve pre-trend: Traditionally, the would-be blacklisted were very mildly (if any) disadvantaged in the industry (see large confidence intervals in the pre-2000 period in Figure A1). But some underlying forces in the industry were altered in the 2000s and the would-be blacklisted were suddenly very heavily favored in the movie labor market (2006 and 2007, marked red in Figure A1). We could assume that these were 'odd' years and the industry conditions returned to the pre-2000 'normalcy,' the process of which overlaps with the treatment timing, 2012. In this hypothetical scenario, what transpired was set by the events of the mid-2000s and the benchmark DiD estimates erroneously capture the tail-end of this hypothetical long-term inverted-U curve trend.

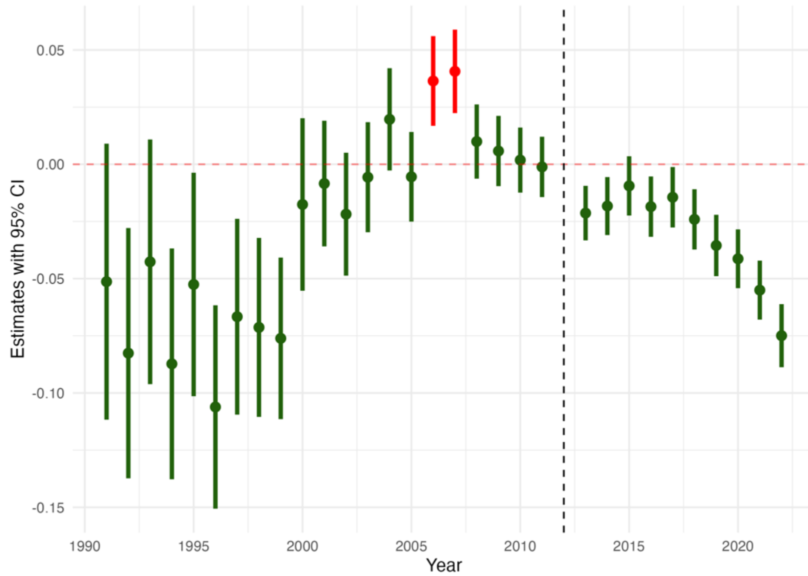

Figure A1. The benchmark event study plot. Recreated for reference. Dashed vertical line indicates year 2012 when the 'treatment' took place, the right hand-side of which indicates the post-treatment period.

A straightforward way to address this concern is to 'detrend' the pre-treatment period by eliminating the 'odd years' from the analysis, an approach roughly analogous to Liu, Wang, and Xu (2022). While the exact Placebo test they suggest was not directly applicable here because the size of the data set was simply too large, the intuition could be still borrowed. If the benchmark DiD estimates are really the function of the (inverted U-curve) pre-trend driven by the 'odd years,' they should change substantially (i.e., dramatically weaken or lose significance) when those years are dropped.

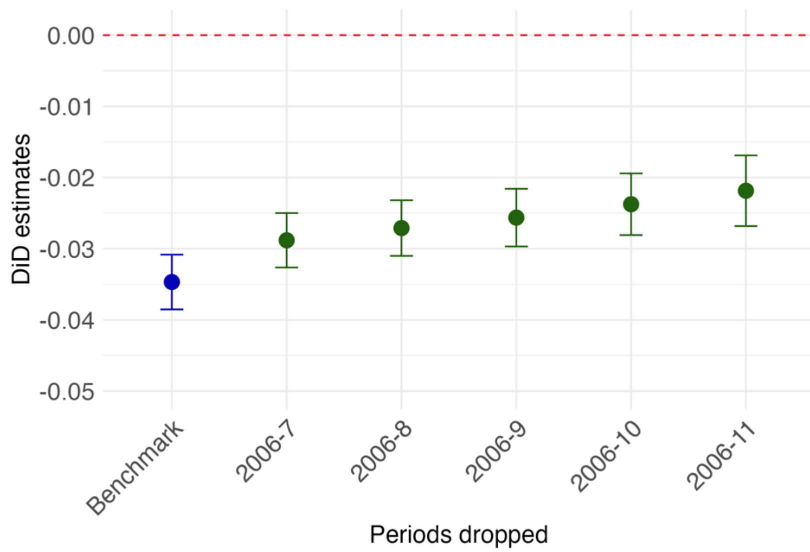

Figure A2. Dropping Odd years? Excluding pre-treatment years from the sample

Figure A2 reports the results when such an exclusion was applied. When the two odd years, 2006 and 2007 were dropped, the ATT (second from left) is not meaningfully different from the benchmark (leftmost). In case this does not de-trend the pre-treatment period, subsequent years were incrementally added to these 'exclusion years' (third through sixth in the figure). Again, the result does not meaningfully change. The effect appears gradually weaken perhaps because a large swath of observations close to the treatment are sliced out. The benchmark estimates, in other words, is unlikely driven by the pre-trend.

A2. How much trend can we allow?

Another approach to assess the validity of PTA does not involve identifying the existence of pre-trends, but rather focuses on the *degree* to which pre-trends can be allowed. To reiterate, in a strictly conservative sense, we cannot completely rule out the influence of pre-trend because the counterfactual parallel trend is simply not observable. Even if a trend is not visible in plots, an unobserved condition originating from the pre-treatment period could inflate the difference between the treated and control groups, independent of the treatment effect.

An alternative, a partial identification approach considers if there still remains a significant treatment effect when a certain amount of pre-trend is assumed to have persisted into the post-treatment period. ATT still significant after accounting for a substantial amount of the pre-trend effect can be considered robust to pre-trend. This approach allows the use of DiD even when pre-trend is assumed.

In the empirical context of the present paper, this implies a hypothetical situation where the blacklisted were disadvantaged in the movie labor market due to a factor other than Blacklist. Such a factor would be a confounder, in that it happens to gain influence on the outcome that is increasing between pre- and post-treatment periods (e.g., changing movie tastes of the public), thus appearing to affect both the treatment and outcome. This factor might have started wielding its influence before the treatment period (i.e., pre-trend) and continue to do so into the post-treatment period–likely increasingly so over time.

While this clearly violates PTA, it does not necessarily mean that Blacklist did not have any effect. If we could show that even after the effect of this hypothetical pre-trend confounder is accounted for, ATT still remains statistically significant in the expected direction, it would be safe to conclude that the DiD model at least partially identifies the true effect of Blacklist. This is a much more stringent approach than traditional ones focusing on pre-trend itself, in that it assumes a situation of a significant violation and examines if the reported effect still survives it.

Rambachan and Roth (2023) propose an innovative method based on this approach. They assume that the pre-treatment trend linearly persists, or even grows beyond the linear prediction, in the post-treatment period, which is reflected in a parameter '$\bar{M}$': "post-treatment violation of parallel trends between consecutive periods by M times the maximum pre-treatment violation of parallel trend" (p. 12). When $\bar{M} = 0$, therefore, one assumes only the linear persistence of the pre-trend (still a violation of PTA). By varying the value of $\bar{M}$, one can estimate how much pre-trend could be tolerated until the benchmark ATT remains significant in the expected direction. In other words, $\bar{M} > 0$ indicates a deviation away from the linear extension of the pre-trend, which is a greater degree of violating PTA.

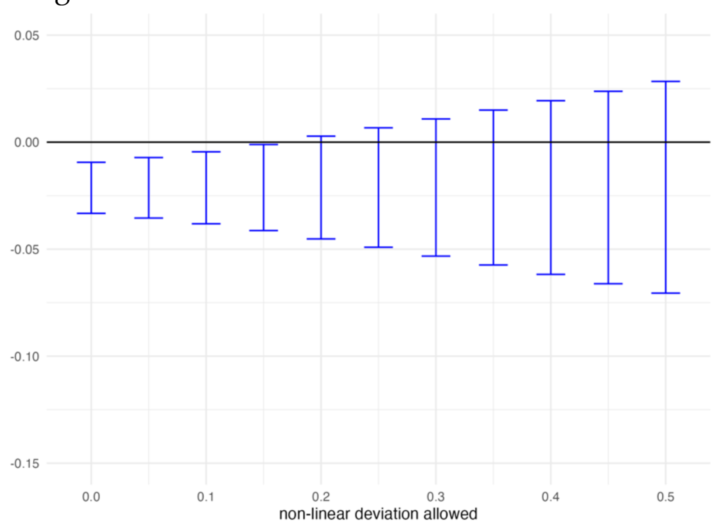

Figure A3. Sensitivity Analysis of Allowing for Pre-trends (Rambachan and Roth 2023). Y-axis represents the size of ATT at each value of $\bar{M}$, x-axis.

Figure A3 reports the result of the sensitive analysis where $\bar{M}$ is varied as suggested by Rambachan and Roth (2023). It suggests that the benchmark ATT is robust to allowing for a substantial amount of violation of PTA, be it a simple linear extension of the pre-trend (i.e., $\bar{M}$=0) or deviations from the linear pre-trend (i.e., $\bar{M}$>0). It indicates that up to 15% of further deviation from the maximum violation linearly presumed in the pre-treatment period, the ATT retains the 95% significance in the direction consistent with the benchmark. In other words, even after considering a fairly drastic amount of violation of PTA, the reported ATT is still recovered. Any (un)observed pre-trend effect notwithstanding, Blacklist has an identifiable effect on the movie labor market.

A3. Fake ('Placebo') Treatment

It is worth revisiting here one traditional sensitivity analysis with regard to PTA, Placebo treatment. The idea is that PTA might not hold if there is a certain underlying trend, by which DiD estimates are really driven. If a 'fake' treatment is applied and the result is still similar to the reported ATT, then it would the trend, not the treatment effect that is producing the significant DiD estimates. To implement this traditional method, I first excluded all the blacklisted in the sample such that nobody in the new sample appears in any of the lists. I then assigned individuals with even-number IDs (random numeric values uniquely identifying each individual) into the treatment group and others, the control group. This way the random treatment assignment is ensured and the result may hardly reflect anything but some underlying trends (and definitely not the original treatment effect, Blacklist). The DiD estimates in this case (with the same controls and fixed effects as the benchmark) are: coefficient = 0.005; standard error = 0.003. Not only is it statistically insignificant, it also is in the opposite direction of the benchmark DiD estimates, suggesting that the effect of an underlying trend violating PTA is unlikely.

A4. Staggered Treatment: the reason for trend

One remaining question, though, would be that if pre-trend, if any, did not affect the benchmark estimates, why we witness a seemingly inverted-U curve trend across the event study plots presented in the paper. One answer could be found in Sun and Abraham’s (2021) research. They suggest that in a staggered treatment assignment setting, earlier treatment might make it seem as though there was a pre-trend. There could be a handful of, though not many, victims in the industry who were affected by the Lee government’s blacklist operations before 2012. When indeed the staggered nature of the treatment is taken into account, almost the entire pre-trend period becomes statistically insignificant, with the exception of the two odd years (Figure 5 in the manuscript). The inverted-U curve visually disappears. The effect of Blacklist identified by the Sun and Abraham (2021) method is nonetheless consistent with the benchmark ATT, suggesting that the latter remains robust to a possible pre-trend. That is, the seeming pre-trend in the event study plots could be due to the staggered treatment, but correcting for this does not alter the primary result reported.

Appendix B. Additional Tables and Figures

Table B1. Excluding the 'Netflix Era' from the sample

	(1)	(2)	(3)
	Benchmark	Exclude:	Exclude:
Blacklisted × PostTreatment	-0.035***	-0.027***	-0.023***
	(0.004)	(0.004)	(0.004)
Number of Observations	1216866	1013329	732429
R^2	0.589	0.641	0.670
AIC	1316011.6	1116750.7	798742.7
BIC	2690484.6	2436874.8	1790006.8
Log.Lik.	-543578.783	-446772.327	-313205.329
Unit Fixed	✓	✓	✓
Year Fixed	✓	✓	✓
Role Fixed	✓	✓	✓

OLS estimates with standard errors in the parentheses, clustered over the
* p < 0.05, ** p < 0.01, *** p < 0.001

Table B2. Commercial Movies? Pre- and Post-1990 Debuts

	(1)	(2)
	pre-1990 debut	**post-1990 debut**
Blacklisted × PostTreatment	−0.123***	−0.013***
	(0.015)	(0.004)
Number of Observations	161915	1054951
R^2	0.693	0.469
AIC	265188.0	1111079.1
BIC	369763.8	2346749.3
Log.Lik.	−122130.984	−451430.548
Unit Fixed	✓	✓
Year Fixed	✓	✓
Role Fixed	✓	✓
Control	✓	✓

OLS estimates with standard errors clustered over unit. Only the key results are presented to save space. All models include unit, year, and role fixed effects.

Table B3: Full Results of Table 2

	(1)	(2)	(3)	(4)
	Baseline	Benchmark	Non-actor	Actor
Blacklisted × PostTreatment	−0.029***	−0.035***	−0.053***	−0.022***
	(0.004)	(0.004)	(0.005)	(0.005)
PostTreatment	−0.137***	0.440***	0.676***	−0.461***
	(0.011)	(0.019)	(0.083)	(0.023)
Blacklisted	−2.510***	−4.683***	−4.369***	0.508
	(0.131)	(0.217)	(0.441)	(5.919)
ln(number of movies + 1)		0.221***	0.220***	0.282***
		(0.005)	(0.006)	(0.010)
ln(years since debut + 1)		−0.575***	−0.580***	−0.334***
		(0.004)	(0.005)	(0.007)
Number of Observations	1216866	1216866	934276	282590
R^2	0.506	0.589	0.598	0.599
AIC	1539301.1	1316013.6	1096139.7	147119.7
BIC	2913762.1	2690498.6	2126867.7	429125.8
Log.Lik.	−655224.570	−543578.783	−460329.853	−46833.848
Unit Fixed	✓	✓	✓	✓
Year Fixed	✓	✓	✓	✓
Role Fixed		✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ OLS estimates with standard errors clustered over unit. Fixed effect results are abbreviated to spare space.

Table B4: Full Results for Table 3

	(1) Binary DV	(2) Logit	(3) Post-2000	(4) Double-SE
Blacklisted × PostTreatment	−0.016***		−0.038***	−0.035***
	(0.003)		(0.004)	(0.010)
Not-Blacklisted, PostTreatment		−0.262***		
		(0.018)		
Blacklisted, PostTreatment		−0.373***		
		(0.031)		
PostTreatment	0.797***		0.491***	0.440
	(0.006)		(0.007)	(2.544)
Blacklisted	1.638***		−3.479***	−4.689
	(0.098)		(0.066)	(12.717)
ln(number of movies + 1)	−0.023***	1.141***	0.237***	0.221***
	(0.003)	(0.008)	(0.006)	(0.079)
ln(years since debut + 1)	−0.424***	1.535***	−0.608***	−0.575***
	(0.002)	(0.014)	(0.004)	(0.068)
years unemployed		−83.359***		
		(0.012)		
years unemployed^2		6.346***		
		(0.001)		
years unemployed^3		−0.126***		
		(0.000)		
Number of Observations	1216866	1138929	1169833	1216866
R^2	0.535		0.597	0.589
AIC	471346.5	217198.0	1267451.0	1316013.6
BIC	1845831.6	1514478.1	2635294.5	2690498.6
Log.Lik.	−121245.268	0.000	−519475.511	−543578.783
Unit Fixed	✓	✓	✓	✓
Year Fixed	✓	✓	✓	✓
Role Fixed	✓	✓	✓	✓

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$ OLS (Models 1, 3, and 4) and logit estimates (Model 2). Standard errors are clustered over the units except for Model 4 where they are clustered over both unit and year.

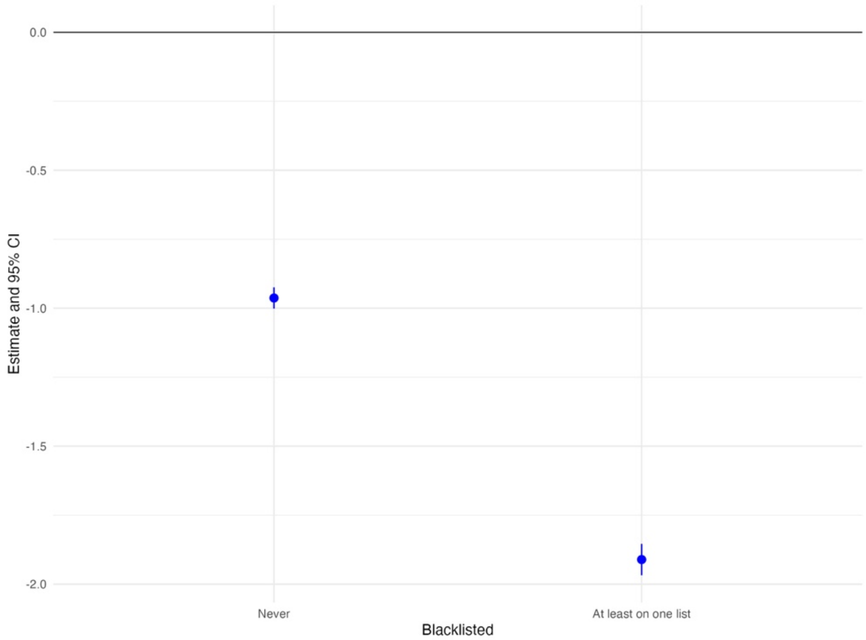

Figure B1. The Marginal Effect, logit estimates**.** Logit estimates from Model 2 of Table 3. The circles and bars indicate point estimates and 95% confidence intervals, respectively.

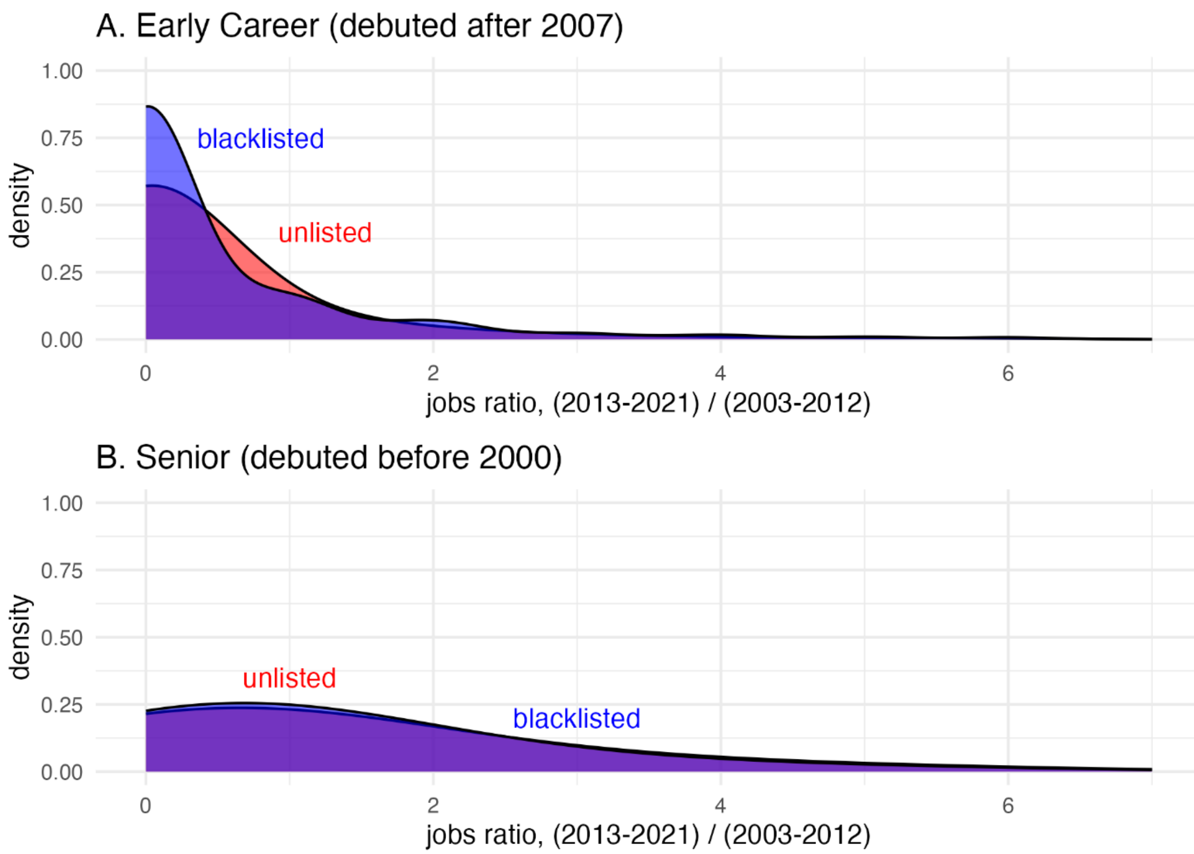

Figure B2. Early Career vs. Senior Movie Workers. Kernel density function estimates of the total number of jobs each group secured between 2021 and 2013, divided by that between 2012 and 2007.

Appendix Reference

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